

**VILLAGE OF DOWNERS GROVE
JOINT AD HOC COMMITTEE OF THE DOWNERS GROVE VILLAGE COUNCIL AND THE
DOWNERS GROVE LIBRARY BOARD OF TRUSTEES**

**Civic Center
Betty Cheever Council Chambers
850 Curtiss Street
Downers Grove, IL**

**April 3, 2025
7:00 PM**

AGENDA

- I. Call to Order**
- II. Roll Call**
- III. Approval of Minutes of the March 13, 2025 Meeting**
- IV. Review and Discussion of Information Requested at the March 13, 2025 Meeting**
- V. Discussion of Action Items/Next Steps**
- VI. Schedule for Future Meetings**
- VII. Public Comment**
- VIII. Adjourn**

**VILLAGE OF DOWNERS GROVE
JOINT AD HOC COMMITTEE OF THE DOWNERS GROVE VILLAGE
COUNCIL AND THE DOWNERS GROVE LIBRARY BOARD OF
TRUSTEES**

Civic Center
Betty Cheever Council Chambers
850 Curtiss Street
Downers Grove, IL

March 13, 2025
7:00 PM

I. Call to Order

II. Roll Call

Present: Village Commissioner Tully, Village Commissioner Sadowski-Fugitt, Library Trustee Khuntia, Library Trustee Williams

Absent: None.

Non-Voting Present: Village Manager Dave Fieldman, Village Attorney Enza Petrarca, Library Director Julie Milavec

III. Introduction of Committee Members

Commissioner Tully thanked everyone for attending. He asked each member to provide a brief self-introduction.

Trustee Williams said he has been a member of the Library Board of Trustees since late last year. He said he is glad to see everyone in attendance. He noted the EQDG One Book One Town event taking place during this meeting. He read an opening statement covering context, audience, the evaluation process and the scope of work of this Ad Hoc committee. He noted the Library Board of Trustees strongly disagrees with the referendum that was proposed, and the content of the Resolution created the Joint Ad Hoc Committee, particularly the “Whereas” clauses.

Trustee Khuntia shared she has been a Library trustee since 2019. She thanked the Village Council for correcting the mistake regarding her term. She acknowledged both Village and Library staff and thanked them for their work. She said she agrees with the comments provided by Trustee Williams and is interested to learn more and is excited to sit on a committee with the other members.

Commissioner Sadowski-Fugitt, Village Council member since 2019, said she was also not in favor of the referendum. She said she will try to be as impartial as she can and work as hard as she can on this committee.

Commissioner Tully said he appreciates that this meeting is occurring. He said he does not think this is a problem to be solved but rather a question to be answered. He said he hopes everyone can focus on the task at hand and work on getting information out to the community. He said there is one audience, the residents of the Village of Downers Grove, and that is who he hopes they are working to serve. He noted he is coming into this with an open mind and his own views on this topic have evolved over time. He said it is important to engage in discussion and dialogue rather than debate. He said he hopes this is a guiding principle for this committee. He spoke of a question in 1999 on whether or not the Village Council should expand from 5 to 7 members and said this was not residents saying the current council was ineffective rather it was a question of if more people could do better. He said he hopes they can explore what's possible.

IV. Charter/Role of the Committee

- **Resolution 2025-21, dated February 18, 2025**

Commissioner Tully said the charter for this committee is as set forth in Resolution 2025-21, A Resolution Establishing a Joint Ad Hoc Committee of the Downers Grove Village Council and the Downers Grove Library Board of Trustees. He reviewed the objectives listed in the resolution.

V. Discussion of Objectives

- **Review and research alternative methods for selection of Library Board of Trustee members (Resolution Section 3.1)**
- **Seek and consider appropriate expert information (Resolution Section 3.2)**

Commissioner Tully suggested taking up these two items. He opened the discussion to the committee of how they can take advantage of low-hanging fruit and use currently available information. He said getting some outside input would be very valuable.

Commissioner Sadowski-Fugitt said they have some of that information already. She said that because Downers Grove is a commission form of government, there cannot be an elected board like other surrounding communities. She said they will need to figure out what staff capacity is. She asked if the best use of time would be for staff to work hard to figure out low-hanging fruit or to hire someone right off the bat.

Trustee Khuntia echoed what Commissioner Sadowski-Fugitt said. She said she strongly believes in working smarter and thinks hiring an expert would be in the best interest of the committee.

Trustee Williams asked, regarding Resolution Section 3.1, for confirmation that the committee will be looking at pros and cons of both election process and current appointment process. Commissioner Sadowski-Fugitt said that is her understanding. Commissioner Tully said they do have the ability to ask the Village Council to give permission to perform additional duties and tasks. He said he looks at it like there are multiple ways to consider each option.

Trustee Williams said he does not understand Section 3.3. He said they have to consider what would be the effect of a modified appointment process. He said he thinks that if there are certain cons with the current appointment process it is important to consider during this committee's process.

Commissioner Tully said if looking at the pros and cons of an appointed Library Board, depending on one's viewpoint, one of the pros or cons could be the removal process. Trustee Williams spoke of the public asking for more transparency in the appointment process. Commissioner Tully said initially the focus should be on what, not how. Trustee Williams said this is just how the report will be framed. He said he is not trying to guide the direction rather he is trying to get an impartial, balanced final report.

Commissioner Tully said he understands where Trustee Williams is coming from. Trustee Williams noted he is just trying to get clarification on the resolution as it seems ambiguous to him. Commissioner Tully said the committee has the ability to get clarification. He said even if an expert is going to be hired, he thinks that process will take some time and during that process currently available information should be gathered.

Trustee Williams said he wants confirmation that an expert will be considered no matter how the upcoming election goes. He noted that the Library Board has found an expert from Northwestern University who is willing to work with the committee at a nominal cost. He expressed his belief that an expert being hired would be a good choice.

Commissioner Tully said he agreed that is something the committee should explore. He said getting all of the Village and Library information into one place would be beneficial to the committee and the community.

Trustee Williams asked about Commissioner Gilmartin and the questions he asked and if any of those questions and answers were privileged. He asked to what extent they can be shared to the community.

Village Attorney Enza Petrarca and Village Manager Dave Fieldman said all of the Village Council questions and answers have been published online.

Trustee Williams noted the Library Board of Trustees has their own data. Commissioner Tully said he thinks getting all of that information in one place would be beneficial to this committee as well as the public. He said even the best experts will come in and gather the already available information which is why he thinks gathering the information before hiring an expert would be a good idea. He said his understanding is that the committee would have to make a recommendation in order for money to be spent. Commissioner Tully said he wants to make sure everything is fully transparent. He asked if the committee can agree among themselves to ask respective staffs to pull together the already available information as well as information about the aforementioned expert.

Trustee Williams asked if the Council has prevalence data. Trustee Khuntia noted that Library Director Julie Milavec has that. Commissioner Tully asked if he is referring to trend information. Trustee Williams said his understanding is that many places are moving from elected boards to appointed boards. He said if they do not have that information, it is best that they start collecting it. Commissioner Sadowski-Fugitt said she knows they have the numbers but not sure about the trends. Trustee Williams said it is important to note the context of how each board ended up the way they currently are. Commissioner Tully noted home-rule status and how it plays into these decisions. Commissioner Tully said his understanding is that this is a Manager-Council form of government.

Village Attorney Enza Petrarca confirmed Downers Grove is a Managerial form of government, not a commission form of government.

Commissioner Tully said he thinks a lot of information will be gathered in the next few weeks to discuss at the next meeting. He asked if any other experts were looked into.

Trustee Williams said The Library Board will look into it.

Commissioner Tully said they need to check out multiple options, not necessarily lowest bidder but he wants to make sure everything is transparent in the selection process.

Commissioner Tully asked about the hybrid approach. Trustee Williams asked if there are any other communities that have this. He said if there are things that fall under option 3, he wants options to be shared. He said this research does not have to be confined to Illinois libraries. He said this is the what's possible section and it is part of the committee's mission to identify all of these options.

VI. Discussion of Deliverables

- **Deliver by September 30, 2025 a report summarizing the attributes of 1) an elected library board selection process, 2) an appointed library board selection process, and 3) any other alternative selection process (Resolution Section 3.3)**

VII. Discussion of Action Items/Next Steps

VIII. Discussion of Committee Rules re: procedure, meeting and Operation of the Committee and future meeting schedule

Commissioner Tully said the committee spoke about meeting every 2 weeks but there is no need to be only meeting on set days. All committee members agreed. Commissioner Tully asked if it makes sense to try and schedule a meeting three weeks from now. Trustee Williams said he would prefer not to meet on the same week of Library Board meetings. Commissioner Tully tentatively suggested April 3rd, same time and place. Commissioner Tully asked staff members for their input on the consolidation of information by the end of the week before the next meeting and if it was possible for staff.

Manager Fieldman recapped what is being asked of staff: repost existing Council questions and answers, research on structure and formats on libraries in the Chicago area with historical context and trends, commentary on form of government and how it relates to questions at hand, information on the expert mentioned earlier tonight and any other potential experts, and commentary on a hybrid approach. Library Director Julie Milavec noted trend data tends to be statewide because data comes from the Illinois State Library. Commissioner Tully asked for any general research or information the Library has available to be included. Commissioner Tully noted that he and Commissioner Sadowski-Fugitt will have additional questions in the meantime. Manager Fieldman asked for confirmation on the meeting date and time. Commissioner Tully said that is correct. Manager Fieldman noted that Attorney Petrarca will not be available on that date. Commissioner Tully said legal discussions can be held until a later meeting. Trustee Khuntia noted this is a tentative meeting. Manager Fieldman said staff will keep everyone informed on the process of gathering information.

Commissioner Tully brought up the establishment of a quorum and said it has been suggested to be a quorum of 4. He asked for a motion.

Commissioner Sadowski-Fugitt made a motion to approve the quorum for this committee as four members, Trustee Khuntia seconded. The motion passed unanimously by roll call vote.

IX. Public Comment

Commissioner Tully thanked everyone in attendance for being here. He asked for public comments to be kept to the focus of the Ad-Hoc committee's work.

Kylie Spahn, resident, spoke about the need to hire an expert. She reviewed the history of the Library and Village land use. She asked for a legal expert in land use agreements, an archivist, a financial consultant to be consulted and paid for by the Village.

Commissioner Tully noted that the financial aspect will be picked up by the Village. He also noted that verbal conversations would not be found in a FOIA request. He spoke of the land

use issue mentioned and expressed he is not sure how it impacts this committee but is interested in learning more.

Ed Pawlik, resident, suggested it would be helpful to know where the committee is going when gathering information in terms of efficiency. He spoke in favor of hiring an expert to do this work. He asked for something to protect and defend the Library during this process.

Commissioner Tully said he appreciates these comments but noted they want to be cognizant of those who are not as involved in this process and important to show to the community that the committee started out broad and worked to narrow down information.

Laura Kamedulski, resident, asked how the experts' work or opinions will be shared and if that will be in a report form or in attendance at a meeting.

Commissioner Tully said he thinks having someone come and testify at this point would be premature but if someone is hired, a report will be made publicly available.

Bruce Mabee, resident, said he likes that this is being looked at as more than just two options. He suggested a third objective as "alternative processes of public engagement" and spoke about how the public can be pulled into dialogue about this.

Commissioner Tully said the committee will emphasize that there is an option 3.

He thanked everyone for coming tonight.

X. Adjourn

A motion to adjourn the meeting was made by Trustee Khuntia and seconded by Commissioner Sadowski-Fugitt. The motion passed unanimously by voice vote and the meeting was adjourned at 8:17 p.m.

VILLAGE OF DOWNERS GROVE
Report For The Joint Ad Hoc Committee of the
Village Council and Library Board of Trustees Meeting
4/3/2025

SUBJECT:	SUBMITTED BY:
Items and Information Requested at the March 13, 2025 Meeting	David Fieldman Julie Milavec Village Manager Library Executive Director

The Joint Ad Hoc Committee of the Village Council and Library Board of Trustees held its first meeting on March 13, 2025. The Committee requested that staff provide the following information.

Publish the Responses to Village Council Questions

The Village Council questions and staff responses originally prepared in November and December of 2024 are attached to this staff report.

Provide Information About the Form and Structure of Other Libraries

The Committee requested information about the form and structure of other libraries within the State including supporting information such as history, context, and relevant trends. Further, the Committee asked for any information that the Village or Library staff already prepared.

Much of the requested information is included in the Illinois Secretary of State Library Heritage Project report. The report, along with highlights and summaries of pertinent sections, is attached to this staff report (“Illinois Libraries History”). This attachment also includes information on Illinois libraries that have transitioned to districts.

Using information provided by the Illinois State Library from the Illinois Public Library Annual Report, staff compiled a table listing all Village libraries in Illinois and the number of Trustee seats reported. Library Director Julie Milavec investigated those reporting six seats to determine if they were elected or appointed boards. The libraries with six seats are highlighted in the attached table (“Village Libraries in Illinois”).

Further, staff has compiled information about libraries similar to the Downers Grove Public Library. The table indicates the library type, board member terms and method of appointment (see attached “Peer Library Data”).

Staff has identified 11 scholarly research papers and newspaper articles which may be helpful to the Committee (see attached “Articles”).

Provide Information on the Village's Form of Government

The Committee requested information about the Village's form of government and how it relates to the Committee's work. The following are the questions and responses prepared in November and December that address the form of government:

1. Pursuant to what form of government is the Village of Downers Grove currently operating?

The Village is currently operating pursuant to the Managerial form of government, which was adopted in 1962. Prior to 1962, the Village operated pursuant to the Commission form of government, which was adopted by the Village in 1915.

2. How was the Managerial form of government created?

The Managerial form of government was approved by a referendum in a special election in 1962. The Village then passed an Ordinance adopting this form of government.

Provide Information on the Expert Identified by the Library and Other Experts for Consideration

The Library identified and held preliminary discussions with Northwestern University Law Professor Nadav Shoked. Information about Professor Shoked is available [here](#). The Joint Ad Hoc Committee may wish to consider working with the following people:

[Sheldon Nahmod](#), Professor Emeritus, Chicago-Kent College of Law.

[Christopher Berry](#), Professor, University of Chicago, Harris School of Public Policy

Joe Filapek, Associate Executive Director, Reaching Across Illinois Library System (RAILS) joe.filapek@railslibraries.org

Joe served on the Board of Library Trustees at Aurora Public Library during its conversion from an appointed to an elected board and has been both elected and appointed to that board. His work at RAILS includes support and training of library trustees.

[John Chrastka](#), Executive Director and Founder of EveryLibrary
john.chrastka@everylibrary.org

Library Journal just published EveryLibrary's analysis of library ballot measures in 2024. In addition to being a longtime library trustee, he is the former president of RAILS. John is a former partner in AssociaDirect, a Chicago-based consultancy focused on supporting associations in membership recruitment, conference, and governance activities.

Attachments

Compiled Council Questions About the Library, December 19, 2024

Illinois Libraries History

Village Libraries in Illinois

Peer Library Data

Articles

Compiled Council Questions About the Library December 19, 2024

- 1. Was there a referendum question about the form of the Downers Grove Public Library (DGPL) in the 1990s?**

A petition requesting the submission of a proposition for the conversion of the Downers Grove Public Library to a Public Library District was filed on August 13, 1993. There were several objections filed to that petition, and on August 31, 1993 the Downers Grove Municipal Officers Electoral Board sustained the objections. Ultimately, the question did not appear on the ballot.

- 2. Is there anything that precludes the Library Board of Trustees (LBOT) and the Village Council from meeting in an ad hoc fashion, or on a regular basis (i.e. annually, bi-annually), to discuss objectives or any matter that is important to either/both boards?**

No, there is nothing that precludes this.

- 3. Is there anything preventing the Village Council from publicly posting the names of the applicants for LBOT positions and having open/public interviews for the positions?**

No, nothing would prevent the Village Council from publicly posting the names of the applicants for LBOT positions and having open/public interviews of the positions.

- 4. Does the Village have a policy regarding the appointment of members to Boards and Commissions?**

Yes, see [Village Council Policy re: Appointments to Boards & Commissions](#)

- 5. Does the Mayor have the authority to make modifications to the process described in the policy?**

The attached Village Policy can only be modified by a majority of the Village Council. As noted in the response to Question 6, several different processes which comply with the policy have been used in the past several years.

- 6. In recent years, have Mayors used a modified appointment process? If so, what processes have been used?**

Over the past several years, a variety of applicant screening processes have been employed. All of them have been in conformance with the Council Policy.

Summary of Processes Used

Mayor - The Mayor handled the screening of applicants on his own without input from Village Council members.

Mayor Pro-Tem - The Mayor assigned the screening of applicants to the Mayor Pro Tem without input from Village Council members.

Mayor Short List - The Mayor produced a short list of persons to be considered for nomination for appointment to Boards and Commissions. The Mayor then assigned individual Council members to handle the recommendations for appointment to the Mayor.

Council Members Assigned to Boards & Commissions - The Mayor requested that Council members handle the screening of applicants for specific Boards and Commissions assigned to them. Council members made recommendations for nominations to the Mayor.

7. **Given that the appointments to the LBOT are the responsibility of the Village Council as a whole, does the policy on appointing members apply to the library appointment process?**

The [Policy](#) is applicable to appointments to the LBOT; see section 1(b).

8. **Do individual Council members have the authority to make modifications to the process described in the policy?**

See response to question 5, above.

9. **What services is the DGPL afforded as a component unit of the Village?**

The Village is currently providing the following services for the DGPL:

- Financial reporting and auditing
- Employee payroll processing
- Accounts Payable - Process all DGPL vendor checks, EFT's and wire transfers
- Bank account management and reconciliation
- Cash management and cash flow (Library cash is part of the Village's pooled cash investments)
- Bond issuances
- Employee health benefits administration (DGPL employees are on the Village's health benefit plan)
- Property tax levy administration
- Participation in the Village's IMRF pension plan
- Provide access to the Village's ERP system

The DGPL is not covered by the Village's self-insured property insurance and workers' compensation program.

10. Does the Village provide services free of charge to any government or similar agency?

The Village provides the following services at no cost:

- Information Technology services are provided to Downtown Management and the DGEDC
- Salt for snow and ice control is provided to Downtown Management and their member commercial properties
- The Village allows the Downers Grove Park District, DGEDC and Downtown Management (in addition to the DGPL) to participate in the employee health benefit program. Premiums are charged to all participants in an equal manner. The Village does not charge any additional administrative or management fees to the participating agencies

11. This is a list of the services that you provided that the Village currently provides the library. Does the Village provide these services to the Park District?

The Village is currently providing the following services for the DGPL:

- *Financial reporting and auditing*
- *Employee payroll processing*
- *Accounts Payable - Process all DGPL vendor checks, EFT's and wire transfers*
- *Bank account management and reconciliation*
- *Cash management and cash flow (Library cash is part of the Village's pooled cash investments)*
- *Bond issuances*
- *Employee health benefits administration (DGPL employees are on the Village's health benefit plan)*
- *Property tax levy administration*
- *Participation in the Village's IMRF pension plan*
- *Provide access to the Village's ERP system*

No, the Village does not provide all of these services to the Downers Grove Park District. The Village only provides employee health benefits administration for the Downers Grove Park District (DGPD employees are on the Village's health benefit plan).

12. If the LBOT are elected instead of appointed, can the Village Council choose to continue to provide these services?

Yes, if there is a change in the way DGPL Trustees are selected, the VoDG would continue to provide services to the DGPL in the same manner as the services are currently provided.

13. Does the DGPL pay for electricity?

Based on a review of the DGPL accounts payable, staff believes that the DGPL is not charged for use of electricity.

14. Does the DGPL have the ability to issue bonds? Does the Village have discretion to approve or disapprove the issuance of bonds?

The DGPL does not have the authority to issue bonds. It can only issue bonds through the Village. The bonds are Village of Downers Grove bonds, issued by the Village of Downers Grove. The Village has discretion on whether or not to approve the bond issuance.

15. Do DGPL employees participate in the Village pension plan?

Yes. The pension plan used for most Village employees is the Illinois Municipal Retirement Fund (IMRF). IMRF rules require the Village to allow DGPL employees to participate in the pension fund. However, the employer contributions to IMRF for DGPL employees is paid out of the DGPL's budget.

16. If so, what is a rough number or percentage of the total number of participants?

The DGPL has 56 IMRF participants, which equals 27.45% of the total number of participants in the Village's IMRF pension plan.

17. Is the DGPL provided any legal protections as a component unit of the Village?

The Village staff is not aware of any legal protections that are afforded to the DGPL as a component unit.

18. If the DGPL remains a component unit of the Village and has an elected board, are there any concerns about the DGPL being accountable to both the VoDG as a component unit and to the voters? Are there any operations, actions or areas of responsibility that would be affected by being a component unit with an elected board?

If the DGPL Board were to become elected rather than appointed, the rights and obligations of the DGPL would remain unchanged, and it would still be subject to the Illinois Local Library Act, which is the State statute that governs both elected and appointed library boards that are component units.

19. How many local libraries are there in Illinois compared to public library districts?

According to the Illinois State Library, there are currently 640 local libraries, 363 of which are library districts.

20. How many libraries have converted from a component unit to a district in the past several years?

The Illinois State Library does not keep a running total of conversions, but they do document when a conversion occurs. According to a phone call between staff and the Illinois State Library, on average there are about 1 or 2 conversions a year. The Pittsfield library is in the process of converting to a district.

21. How would the Village's and the DGPL's bond rating be affected if the DGPL were converted to a district?

The DGPL does not have a bond rating. As noted in the response above, any bonds issued for DGPL purposes are issued by the VoDG and subject to the Village's bond rating. Currently, there are no outstanding DGPL related bond issuances. Updated bond ratings are provided at the time of bond issuances or refinancings. The next rating for the VoDG will likely be with the planned 2026 issuance for water system improvements. If the DGPL is a district at that time, the VoDG rating is not expected to be affected. If the DGPL were to convert to a district and then issue bonds, the DGPL would receive its own bond rating.

22. If the DGPL adopts a resolution converting to a Public Library District is the Village Council obligated to approve the resolution?

No, the Village Council is not obligated to approve the conversion. Once the library board adopts a resolution calling for the conversion to a public library district, it may then request the Village to approve the conversion. The Village is not obligated to approve the conversion or even consider the conversion. However, in order for the conversion to be binding the Village must approve the conversion within the 60 day period after the date the resolution was adopted by the library, and the library would then have to petition the Circuit Court for an order setting forth all terms of the conversion.

23. Per State law, once a conversion to a public library district occurs, the DGPL would have 547 days to remain in the IMRF plan. What happens after 547 days?

If the DGPL were to convert into a separate district, it could elect to participate in IMRF as its own unit of local government. If it were to timely elect to participate in IMRF, it would be required to make the necessary employer contributions to the pension plan for its employees. If DGPL did not timely elect to participate in IMRF, then DGPL employees would cease to be considered IMRF participants.

24. How would the tax levy (specifically property tax caps, PTEL) for the DGPL be affected if it were converted to a district?

If the DGPL were converted to a library district it would have a maximum tax rate not to exceed the greater of 0.15% of the value of all the taxable property in the Village or the last rate levied by the Village for library purposes for the most recent year before the conversion and that initial rate would not be subject to any rate limitations or referendum requirements imposed by State law. That rate may thereafter be increased in accordance with State law.

25. If the DGPL becomes its own, separate district, would it be able to operate pursuant to the Village's home rule authority?

No, the Illinois Constitution only authorizes municipalities and counties to operate with home rule authority.

26. If the DGPL is converted to a district, how would this affect the costs for both the DGPL and the VoDG?

The cost impacts cannot be determined at this time as there are no specific proposed operational changes.

27. Do the following libraries have elected or appointed boards? Which of them are component units of a municipality?

Library	Municipal Gov't Form	Selection Process	Component Unit (Y/N)
Downers Grove	Managerial	Appointed	Yes
Westmont	Managerial	Elected	Yes
Woodridge	Strong Mayor	Elected	Yes
Lisle	N/A	Elected	No (District)
Helen Plum (Lombard)	N/A	Elected	No (District)
Clarendon Hills	Managerial	Elected	Yes
Hinsdale	Managerial	Elected	Yes
Indian Prairie (Darien)	N/A	Elected	No (District)

Generally speaking, community libraries are governed by either the Illinois Local Library Act (75 ILCS 5/1-0.1, *et seq.*) or the Public Library District Act of 1991 (75 ILCS 16/1-1, *et seq.*).

Pursuant to the Illinois Local Library Act, the board of trustees for local libraries situated in a town or village that is not operated under a commission form of government are elected. (75 ILCS 5/4-3). Conversely, the board of trustees for local libraries situated in a city or a village operating under a commission form of government are appointed by the corporate authorities. (75 ILCS 5/4-1, 75 ILCS 5/4-2). Pursuant to the Public Library District Act of 1991, library district trustees are elected. 75 ILCS 16/30-10.

It should be noted that libraries organized under the Illinois Local Library Act may convert to become a library district. In cases where a local library is converted to a library district, the initial trustees are appointed by the County Board and the initial trustees serve until the next scheduled regular election occurring more than 4 months after the initial trustee appoints where the trustees are then elected.

Each of the libraries referenced in the chart above have an elected board of trustees because they are either a local library situated in a municipality operating as a village without a commission form of government, or they are organized as a separate special district. Although Downers Grove has adopted the Managerial Form of government, it continued to appoint members of the library board as a result of retaining elements of the commission form of government.

28. Please explain the Aurora Library

In 2019, legislation was passed that authorized the City of Aurora to place a binding referendum on the ballot whereby city electors could vote to have an elected board of trustees for their public library instead of a board of trustees appointed by the corporate authorities of the City, but still remain a component unit. However, shortly after the passage of the legislation, the Aurora Public Library and the City of Aurora both passed resolutions that would authorize the conversion of the Aurora Public Library to a Public Library District. The Aurora Public Library then petitioned the Circuit Court of Kane County for a court order authorizing the conversion, and the Aurora Public Library became a separate, public library district in June of 2020. It appears that the adoption of the legislation prompted a discussion between the City and the library. The library board and the City Council ultimately decided it would be in the community's best interest to have the library convert to a library district, as opposed to holding a referendum.

29. Other than Aurora, has any special legislation and amendment to the Library Act been approved for any municipality?

No, staff is not aware of any special legislation and amendment to the Library Act.

30. How many municipalities operating under a Commission Form have a component unit library with an elected library board?

According to a [document on the Illinois Secretary of State website](#) (2012), there are 49 municipalities in Illinois operating under the Commission form. Of these, it appears that none have a component unit library with an elected board.

Since staff's response to this question was based on a review of the 49 municipalities included in the SOS document, staff cannot say with one hundred percent certainty that there are no Illinois municipalities operating in this manner. However, the result of this research is consistent with State law which states that municipalities operating under a Commission form of government appoint members to the Library Board.

31. When did the VoDG adopt the hybrid form of government of Manager form with underlying components of Commission form?

The Village adopted the Managerial form of government in 1962.

32. How was this form of government created?

The Managerial form of government was approved by a referendum in a special election in 1962. The Village then passed an Ordinance adopting this form of government.

33. What operations and actions of the Village are affected by the underlying Commission Form?

According to State law, "Upon the managerial form becoming effective in any city or village, that city or village and its officers shall be vested with all the rights, privileges, powers and immunities of the previous form [commission form] of government in force at the time such city or village adopted the managerial form, including the procedures for elections therein described, the officers therein named and the duties and liabilities therein set forth, except as modified by the statute".65 ILCS 5/5-1-2.

Staff has not been able to identify a complete list of operations and actions affected by the components of the underlying Commission form. As a home rule municipality, the operations and actions of the Village are primarily governed by the Village's Municipal Code. The Municipal Code does not specifically indicate which sections are related to the components of the underlying Commission form.

Illinois Libraries History

The Illinois State Library Heritage Project follows the history of the Illinois State Library and the development of libraries statewide:

https://www.ilsos.gov/departments/library/heritage_project/

The following sections of the Illinois State Library Heritage Project discuss key occurrences in Illinois library development, particularly impacting the creation and mergers of library districts, which answer the question of why there are so many library districts in Illinois.

Library Service Act (LSA) 1956

This federal grant program was designed to address the lack of public library service in rural areas. Its projects supported the development of library districts and regional libraries to serve rural areas.

The 7th-10th paragraphs of this section detail the initial Illinois Library Services Act projects:

https://www.ilsos.gov/departments/library/heritage_project/home/chapters/years-of-transition-the-1950s/the-problem-of-unserved-residents/

From the 5th paragraph on, this section details some of the specific LSA projects undertaken, including successes and failures:

https://www.ilsos.gov/departments/library/heritage_project/home/chapters/years-of-transition-the-1950s/library-services-act-projects/

This section details the renewal of LSA in 1961 and the projects extended under it:

https://www.ilsos.gov/departments/library/heritage_project/home/chapters/building-for-the-future-the-1960s/renewal-of-the-library-services-act/

Library Services and Construction Act (LSCA) 1964

This federal grant program expanded its parameters to address the lack of public library service in all areas and included the construction of libraries.

This section details the beginnings of LSCA:

https://www.ilsos.gov/departments/library/heritage_project/home/chapters/building-for-the-future-the-1960s/the-library-services-and-construction-act/

From the 4th paragraph on, this section details the Illinois State Library's use of LSCA funds for Project PLUS (Promoting Larger Units of Service), which created 110 library districts from 1972-1993:

https://www.ilsos.gov/departments/library/heritage_project/home/chapters/coming-of-age-the-1970s/other-declarations-in-meeting-the-challenge/

The first 4 paragraphs of this section detail Project LiMe (Library Mergers), encouraging small libraries to merge into larger units of service:

https://www.ilsos.gov/departments/library/heritage_project/home/chapters/maturity-and-leadership-the-1980s/larger-units-of-service/

Illinois Libraries Trends and Data

Libraries that transitioned to District Libraries:

- The former Winnetka Public Library converted to the [Winnetka-Northfield Public Library District](#) in 1974 to include Northfield in their tax-supported service area. (Also of note: while Kenilworth is not a part of the library district, the town is served through a contractual agreement.)
- The former Fox Lake Public Library was converted to the [Fox Lake Public Library District](#) in 1975 in order to serve areas outside the boundaries of the village.
- The [Maywood Public Library District](#) was created in 1991, by election to ensure future funding for the library.
- The Coulterville Public Library [became a district in 2023](#) to serve a larger area and is now the [Coulterville District Library](#).
- The [Geneseo Public Library](#) transitioned from a city to a township public library in 1888 in order to increase funding and increase its “power of usefulness.”
- The Grayslake Public Library became the [Grayslake Public Library District](#) in 1958 to include serving the surrounding towns: Round Lake, Round Lake Beach, Third Lake, Hainesville, and Highland Lake.
- The [Anne West Lindsey District Library](#) (rural library in southern IL) changed from a city to a district library in 2007 to include the towns: Carterville, Crainville, Cambria, and the unincorporated area found primarily within the boundaries of the Carterville Community School District.
- The [Barrington Area Library](#) moved from a village to a district library as a result of referendums passed in 1969 and 1970 to include the communities of Barrington, Barrington Hills, Deer Park, Lake Barrington, North Barrington, South Barrington, Tower Lakes, Algonquin, Fox River Grove, Hoffman Estates, Inverness, Kildeer, Lake Zurich, Palatine, Port Barrington, and some unincorporated areas.
- The [Algonquin Public Library](#) moved from a village library to a district library in 1962 to include Algonquin, Lake in the Hills, Haeger’s Bend, and portions of Cary as well.
- The [West Chicago Public Library District](#) passed a referendum in 1986 to convert the library from a municipal library, run by the city and paid for by municipal taxes, into a district library
- [Pittsfield Public Library](#) recently converted to a district via joint resolution.

The table below was provided by the Illinois State Library from the Illinois Public Library Annual Report, listing all Village libraries in Illinois and the number of Trustee seats reported. Library Director Julie Milavec investigated those reporting 6 seats to determine if they were elected or appointed boards. The libraries with 6 seats are highlighted.

	LIBRARY	CITY	TYPE	NUMBER OF SEATS
1	Addison Public Library	Addison	Village	7
2	Arlington Heights Memorial Library	Arlington Heights	Village	7
3	Bellwood Public Library	Bellwood	Village	7
4	Berkeley Public Library	Berkeley	Village	7
5	Bloomington Public Library	Bloomington	Village	7
6	Bluffs Public Library	Bluffs	Village	6
7	Bridgeview Public Library	Bridgeview	Village	7
8	Calumet Park Public Library	Calumet Park	Village	6
9	Carol Stream Public Library	Carol Stream	Village	7
10	Chicago Ridge Public Library	Chicago Ridge	Village	7
11	Clarendon Hills Public Library	Clarendon Hills	Village	7
12	Clifton Public Library	Clifton	Village	7
13	Coulterville Public Library	Coulterville	Village	6
14	Cutler Public Library	Cutler	Village	7
15	Deerfield Public Library	Deerfield	Village	7
16	Downers Grove Public Library	Downers Grove	Village	6
17	Elk Grove Village Public Library	Elk Grove Village	Village	7
18	Elmwood Park Public Library	Elmwood Park	Village	7
19	Evansville Public Library	Evansville	Village	7
20	Evergreen Park Public Library	Evergreen Park	Village	7
21	Flewelin Memorial Library	Shabbona	Village	7
22	Flossmoor Public Library	Flossmoor	Village	7
23	Forest Park Public Library	Forest Park	Village	6
24	Forreston Public Library	Forreston	Village	7
25	Forsyth Public Library	Forsyth	Village	7
26	Franklin Grove Public Library	Franklin Grove	Village	7
27	Glen Ellyn Public Library	Glen Ellyn	Village	7
28	Glencoe Public Library	Glencoe	Village	7
29	Glenview Public Library	Glenview	Village	7
30	Grant Park Public Library	Grant Park	Village	7
31	Hillside Public Library	Hillside	Village	7
32	Hinsdale Public Library	Hinsdale	Village	7
33	Homer Community Library	Homer	Village	7
34	Ira C. Reed Public Library	LaFayette	Village	
35	Itasca Community Library	Itasca	Village	7
36	Kirkland Public Library	Kirkland	Village	7
37	LaGrange Public Library	La Grange	Village	7
38	Lake Bluff Public Library	Lake Bluff	Village	7
39	Lansing Public Library	Lansing	Village	7
40	Brookfield Public Library	Brookfield	Village	7
41	Lyons Public Library	Lyons	Village	7
42	Melrose Park Public Library	Melrose Park	Village	7
43	Messenger Public Library of North Aurora	North Aurora	Village	7
44	Midlothian Public Library	Midlothian	Village	7
45	Mills & Petrie Memorial Library and Gymnasium	Ashton	Village	6
46	Morton Grove Public Library	Morton Grove	Village	7

47	Mount Morris Public Library	Mount Morris	Village	7
48	Mount Prospect Public Library	Mount Prospect	Village	7
49	New Baden Public Library	New Baden	Village	7
50	Northbrook Public Library	Northbrook	Village	7
51	Oak Brook Public Library	Oak Brook	Village	7
52	Oak Lawn Public Library	Oak Lawn	Village	7
53	Oak Park Public Library	Oak Park	Village	7
56	Ogden Rose Public Library	Ogden	Village	7
57	Olympia Fields Public Library	Olympia Fields	Village	7
58	Orland Park Public Library	Orland Park	Village	7
59	Palos Park Public Library	Palos Park	Village	6
60	Park Forest Public Library	Park Forest	Village	7
61	Patoka Public Library	Patoka	Village	6
62	Pawnee Public Library	Pawnee	Village	7
63	Peoria Heights Public Library	Peoria Heights	Village	7
64	Ramsey Public Library	Ramsey	Village	7
65	Rantoul Public Library	Rantoul	Village	7
66	River Forest Public Library	River Forest	Village	7
67	Riverside Public Library	Riverside	Village	7
68	Riverton Village Library	Riverton	Village	7
69	Schiller Park Public Library	Schiller Park	Village	7
70	Sheffield Public Library	Sheffield	Village	7
71	Sidney Community Library	Sidney	Village	7
72	Skokie Public Library	Skokie	Village	7
73	South Holland Public Library	South Holland	Village	7
74	South Pekin Public Library	South Pekin	Village	7
75	Thomas Ford Memorial Library	Western Springs	Village	7
76	Thornton Public Library	Thornton	Village	7
77	Tilden Public Library	Tilden	Village	6
78	Tinley Park Public Library	Tinley Park	Village	7
79	Vermont Public Library	Vermont	Village	7
80	Villa Park Public Library	Villa Park	Village	7
81	Village of Avon Public Library	Avon	Village	7
82	Wayne City Kissner Public Library	WAYNE CITY	Village	7
83	West Salem Public Library	West Salem	Village	7
84	Westchester Public Library	Westchester	Village	7
85	Westmont Public Library	Westmont	Village	7
86	Williamsville Public Library and Museum	Williamsville	Village	7
87	Winfield Public Library	Winfield	Village	7
88	Woodridge Public Library	Woodridge	Village	7

Nine libraries reported 6-member boards.

#6, 61, & 77 [Bluffs Public Library](#), [Patoka Public Library](#), and [Tilden Public Library](#) have not responded to questions about their board structure and do not list board information online.

8 [Calumet Park Public Library](#) self-reported 6 members due to a vacancy. Their elected board has 7 Trustees.

#13 Coulterville Public Library [became a district in 2023](#) in order to serve a larger area, now [Coulterville District Library](#).

#45 [Mills & Petrie Memorial Library and Gymnasium](#) appears to be organized under [75 ILCS 50](#) Village Library and Gymnasium Tax Act and is perhaps the only remaining library of this type in the state. They have not responded to questions about their board structure and currently list 5 Trustees on their website.

#16, 23, & 59 [Downers Grove Public Library](#), [Forest Park Public Library](#), and [Palos Park Public Library](#) are village libraries operating under the commission form of government for purposes of selecting board members.

Library (Community) Name	Library Type	<u>Community / service area size</u>	Board Members / Terms	# of memb ers	Method	Term
Downers Grove Public Library	Municipal Government (city, town or village)	50,247	Six members appointed to six-year terms	6	Appointed	6
Addison Public Library	Municipal Government (city, town or village)	35,702	Seven members elected to six-year terms	7	Elected	6
Algonquin Area Public Library District	Library District	40,809	Seven members elected to six-year terms	7	Elected	6
Arlington Heights Memorial Library	Municipal Government (city, town or village)	77,676	Seven members elected to six-year terms	7	Elected	6
Aurora Public Library District	Municipal Government (city, town or village)	180,542	Seven members elected to six-year terms	7	Elected	6
Barrington Public Library District	Library District	45,360	Seven members elected to #-year terms	7	Elected	?
Bartlett Public Library District	Library District	36,034	Seven members elected to four-year terms	7	Elected	4
Batavia Public Library District	Library District	26,256	Seven members elected to four-year terms	7	Elected	4
Berwyn Public Library	Municipal Government (city, town or village)	57,250	Nine members appointed to three-year terms	9	Appointed	3
Carol Stream Public Library	Municipal Government (city, town or village)	39,854	Seven members elected to four-year terms	7	Elected	4
Cook Memorial Public Library District (Libertyville)	Library District	61,297	Seven members elected for four-year terms	7	Elected	4
Crystal Lake Public Library	Municipal Government (city, town or village)	40,269	Nine members appointed for three-year terms	9	Appointed	3
Des Plaines Public Library	Municipal Government (city, town or village)	60,675	Nine members appointed to three-year terms	9	Appointed	3
Ela Area Public Library District (Lake Zurich)	Library District	36,366	Seven members elected to four-year terms	7	Elected	4
Elk Grove Village Public Library	Municipal Government (city, town or village)	22,205	Seven members elected for six-year terms	7	Elected	6
Elmhurst Public Library	Municipal Government (city, town or village)	46,571	Nine members appointed to three-year terms	9	Appointed	3
Fountaindale Public Library District (Bolingbrook)	Library District	67,049	Seven members elected to 6-year terms	7	Elected	6

Library (Community) Name	Library Type	<u>Community / service area size</u>	Board Members / Terms	# of memb ers	Method	Term
Downers Grove Public Library	Municipal Government (city, town or village)	50,247	Six members appointed to six- year terms	6	Appointed	6
Fox River Valley Public Library District	Library District	71,530	Seven members elected to four-year terms	7	Elected	4
Fremont Public Library District (Mundelein)	Library District	38,792	Seven members elected for four-year terms	7	Elected	4
Gail Borden Public Library District (Elgin)	Library District	149,907	Seven members elected to four-year terms	7	Elected	4
Geneva Public Library District	Library District	32,791	Seven members elected to four-year terms	7	Elected	4
Glen Ellyn Public Library	Municipal Government (city, town or village)	28,846	Seven members elected to #- year terms	7	Elected	2
Glencoe Public Library	Municipal Government (city, town or village)	8,849	Seven members elected to four-year terms	7	Elected	4
Glenside Public Library District (Glendale Heights)	Library District	35,094	Seven members elected to six-year terms	7	Elected	6
Glenview Public Library	Municipal Government (city, town or village)	48,705	Seven members elected to four-year terms	7	Elected	4
Grayslake Area Public Library District	Library District	27,919	Seven members elected to #- year terms	7	Elected	?
Helen M. Plum Memorial Public Library District (Lisle)	Library District	44,585	Seven members elected to four-year terms	7	Elected	4
Highland Park Public Library	Municipal Government (city, town or village)	30,176	Nine members appointed to five-year terms	9	Appointed	5
Hinsdale Public Library	Municipal Government (city, town or village)	17,395	Seven members elected to four-year terms	7	Elected	4
Indian Prairie Public Library District (Darien)	Library District	43,892	Seven members elected to four-year terms	7	Elected	4
Indian Trails Public Library District (Wheeling)	Library District	65,423	Seven members elected to four-year terms	7	Elected	4
Joliet Public Library	Municipal Government (city, town or village)	150,362	Nine members appointed to three-year terms	9	Appointed	3
La Grange Public Library	Municipal Government (city, town or village)	16,321	Seven members elected to four-year terms	7	Elected	4

Library (Community) Name	Library Type	<u>Community / service area size</u>	Board Members / Terms	# of memb ers	Method	Term
Downers Grove Public Library	Municipal Government (city, town or village)	50,247	Six members appointed to six-year terms	6	Appointed	6
Lake Villa Public Library District	Library District	39,809	Seven members elected for four-year terms	7	Elected	4
Lemont Public Library District	Library District	23,182	Seven members elected to four-year terms	7	Elected	4
Lisle Library District	Library District	30,281	Seven members elected to four-year terms	7	Elected	4
Mchenry Public Library District	Library District	41,852	Seven members elected for six-year terms	7	Elected	6
Naperville Public Library	Municipal Government (city, town or village)	149,540	Nine members appointed to three-year terms	9	Appointed	3
Niles-Maine District Library	Library District	59,181	Seven members elected to six-year terms	7	Elected	6
Northbrook Public Library	Municipal Government (city, town or village)	35,222	Seven members elected for four-year terms	7	Elected	4
Oak Lawn Public Library	Municipal Government (city, town or village)	58,362	Seven members elected for six-year terms	7	Elected	6
Oak Park Public Library	Municipal Government (city, town or village)	54,583	Seven members elected to four-year terms	7	Elected	4
Orland Park Public Library	Municipal Government (city, town or village)	58,703	Seven members elected to six-year terms	7	Elected	6
Oswego Public Library District	Library District	70,508	Seven members elected for four-year terms	7	Elected	4
Palatine Public Library District	Library District	89,395	Seven members elected for four-year terms	7	Elected	4
Park Ridge Public Library	Municipal Government (city, town or village)	38,810	Nine members appointed to three-year terms	9	Appointed	3
Plainfield Public Library District	Library District	79,063	Nine members elected to four-year terms	9	Elected	4
Poplar Creek Public Library District (Streamwood)	Library District	65,645	Seven members, elected for six-year terms, plus Student Trustee	7	Elected	6
Round Lake Area Public Library District	Library District	39,675	Seven members elected for four-year terms	7	Elected	4

Library (Community) Name	Library Type	<u>Community / service area size</u>	Board Members / Terms	# of members	Method	Term
Downers Grove Public Library	Municipal Government (city, town or village)	50,247	Six members appointed to six-year terms	6	Appointed	6
Schaumburg Township District Library	Library District	130,345	Seven members, elected for four-year terms, plus Student Advisory Trustees	7	Elected	4
Skokie Public Library	Municipal Government (city, town or village)	67,824	Seven members, elected for six-year terms	7	Elected	6
Saint Charles Public Library District	Library District	55,889	Seven members elected to six-year terms	7	Elected	6
Thomas Ford Memorial Library (Western Springs)	Municipal Government (city, town or village)	13,629	Seven members elected to four-year terms	7	Elected	4
Tinley Park Public Library	Municipal Government (city, town or village)	55,971	Seven members elected for six-year terms	7	Elected	6
Vernon Area Public Library District (Lincolnshire)	Library District	44,206	Seven members elected for six-year terms	7	Elected	6
Villa Park Public Library	Municipal Government (city, town or village)	22,263	Seven members elected to four-year terms	7	Elected	4
Warren-Newport Public Library District	Library District	66,477	Seven members elected for four-year terms	7	Elected	4
Warrenville Public Library District	Library District	14,096	Seven members elected to four-year terms	7	Elected	4
Wauconda Area Public Library District	Library District	28,941	Seven members elected for four-year terms	7	Elected	4
Westmont Public Library	Municipal Government (city, town or village)	24,429	Seven members elected to four-year terms	7	Elected	4
Wheaton Public Library	Municipal Government (city, town or village)	53,970	Nine members appointed to 3-year terms	9	Elected	3
White Oak Library District (Romeoville)	Library District	76,850	Seven members elected to six-year terms	7	Elected	6
Wilmette Public Library District	Library District	28,180	Seven members elected for four-year terms	7	Elected	4
Woodridge Public Library	Municipal Government (city, town or village)	34,158	Seven members elected to six-year terms	7	Elected	6

Summary of Articles

Staff has identified the following 11 scholarly research papers and newspaper articles which may be helpful to the Committee. Abstracts from each article are included below. The complete articles are also included.

Hessami, Zohal, "Accountability and Incentives of Appointed and Elected Officials," *The Review of Economics and Statistics* 100:1 (2018)

Eric Posner, G. Mitu Gulati & Stephen J. Choi, "Professionals or Politicians: The Uncertain Empirical Case for an Elected Rather than Appointed Judiciary" (John M. Olin Program in Law and Economics Working Paper No. 357, 2007).

Whalley, A., "Elected and Appointed Policy Makers: Evidence from City Treasurers," *Journal of Law and Economics* 56 (2013)

"Should School Board Members be Appointed or Elected?" *The Journal of Education* 49:8 (1899)

Besley, T., and S. Coate, "Elected Versus Appointed Regulators: Theory and Practice," *Journal of the European Economic Association* 1 (2003)

Nahmod, Sheldon H., "Reflections on Appointive Local Government Bodies and a Right to an Election," *Duquesne Law Review* 11:2 (1972)

Ferrer, Joshua, "To Elect or Appoint? Evidence from Local Election Administration," (2024)

Ferrer, Joshua, "How Partisan Is Local Election Administration?" Cambridge University Press, (2023)

[Furstenberg-Beckman, Hannah, Greg Degen, and Tova Wang. "Understanding the Role of Local Election Officials: How Local Autonomy Shapes U.S. Election Administration"](#)

[Rubin, Paul G. "Political Appointees vs. Elected Officials: Examining How the Selection Mechanism for State Governing Agency Board Members Influences Responsiveness to Stakeholders in Higher Education Policy-Making" *Education Policy Analysis Archives* 29:115 \(2021\)](#)

[Guillermo, Toral. "The benefits of patronage: How political appointments can enhance bureaucratic accountability and effectiveness" \(2021\)](#)

Hessami, Zohal, "Accountability and Incentives of Appointed and Elected Officials," *The Review of Economics and Statistics* 100:1 (2018), p. 51-64.

Political agency models suggest that elected public officials choose different policies than appointed officials do. This paper is the first (a) to apply a clean empirical design to study whether the selection rule has a causal effect on public officials' policy choices and (b) to investigate transmission channels. I exploit a unique setting in Germany, where a reform has created quasi-experimental variation in the selection rule for mayors. As the outcome variable, I use data on grant receipts for highly visible investment projects for which mayors must apply to the state government. Elected mayors attract 7% to 7.4% more grants in election years; for appointed mayors, there is no cycle. Using hand-collected data on mayor characteristics, I find suggestive evidence that although the selection of mayors changes following the reform studied, a likely reason for the observed cycle is that elected mayors have stronger electoral incentives.

Eric Posner, G. Mitu Gulati & Stephen J. Choi, "Professionals or Politicians: The Uncertain Empirical Case for an Elected Rather than Appointed Judiciary" (John M. Olin Program in Law and Economics Working Paper No. 357, 2007).

Although federal judges are appointed with life tenure, most state judges are elected for short terms. Conventional wisdom holds that appointed judges are superior to elected judges because appointed judges are less vulnerable to political pressure. However, there is little empirical evidence for this view. Using a dataset of state high court opinions, we construct objective measures for three aspects of judicial performance: effort, skill and independence. The measures permit a test of the relationship between performance and the four primary methods of state high court judge selection: partisan election, non-partisan election, merit plan, and appointment. The empirical results do not show appointed judges performing at a higher level than their elected counterparts. Appointed judges write higher quality opinions than elected judges do, but elected judges write many more opinions, and the evidence suggests that the large quantity difference makes up for the small quality difference. In addition, elected judges do not appear less independent than appointed judges. The results suggest that elected judges are more focused on providing service to the voters (that is, they behave like politicians), whereas appointed judges are more focused on their long-term legacy as creators of precedent (that is, they behave like professionals).

Whalley, A., "Elected and Appointed Policy Makers: Evidence from City Treasurers," *Journal of Law and Economics* 56 (2013), 39-81.

This paper investigates whether the method of selecting public officials affects policy making. I compare the policy choices of bureaucrat city treasurers and politician city treasurers, who are selected and held accountable in very different ways. The analysis draws on rich data from California to examine whether cities with appointed or elected city treasurers pay lower costs to borrow. The results demonstrate that having appointive treasurers reduces a city's cost of borrowing by 19–31 percent. Holding officials directly accountable to voters can result in lower levels of performance in complex policy areas.

“Should School Board Members be Appointed or Elected?” *The Journal of Education* 49:8 (1899), 117.

This article explores the debate over whether school board members should be appointed or elected. It examines the advantages and disadvantages of both methods, considering factors such as accountability, efficiency, political influence, and community representation. Proponents of elected school boards argue that they enhance democratic participation and ensure that board members are directly accountable to the public. On the other hand, supporters of appointed boards contend that this method allows for the selection of highly qualified individuals who can make decisions based on expertise rather than political considerations.

Besley, T., and S. Coate, “Elected Versus Appointed Regulators: Theory and Practice,” *Journal of the European Economic Association* 1 (2003), 1176-1206.

This paper contrasts direct election with political appointment of regulators. When regulators are appointed, regulatory policy becomes bundled with other policy issues the appointing politicians are responsible for. Because voters have only one vote to cast and regulatory issues are not salient for most voters, there are electoral incentives to respond to stakeholder interests. If regulators are elected, their stance on regulation is the only salient issue so that the electoral incentive is to run a pro-consumer candidate. Using panel data on regulatory outcomes from U.S. states, we find new evidence in favor of the idea that elected states are more pro-consumer in their regulatory policies.

Nahmod, Sheldon H., “Reflections on Appointive Local Government Bodies and a Right to an Election,” *Duquesne Law Review* 11:2 (1972), 119-132.

While few would dispute the importance in a representative democracy of the right to vote for government officials, there appears to be relatively little discussion of what might be termed an individual's constitutional right to an election.' Granted, the reapportionment cases and other voting rights cases direct attention to vote dilution and voters' qualifications, but they involve situations where elections have already been provided. Suppose, however, that elected officials of a properly apportioned municipality are permitted by statute to appoint members of a school board which will be exercising important governmental powers, such as the power to levy and collect taxes, issue bonds, and acquire property by condemnation. Might an argument be made that those school board members must constitutionally be elected by their constituents? What could be the constitutional source and judicial apportionment cases which prompted them. The following discussion is primarily intended to stimulate further thinking in this area. standards of such a right to an election? And, what might be the implications of this right?This article will explore these questions, beginning with the reapportionment cases which prompted them. The following discussion is primarily intended to stimulate further thinking in this area.

Ferrer, Joshua, “To Elect or Appoint? Evidence from Local Election Administration,” (2024)

Do elected or appointed officials produce better outcomes for their constituents? Elections should improve representation by providing a direct link to voters. However, some argue that citizens may have too little information to select good leaders and hold them accountable, especially at the local level. In order to assess these conflicting claims, I examine the performance of local election officials, an office that has come under immense strain to deliver democratic elections and for which selection method is a live policy debate. Using an original collection of election administration structures in 1,116 counties across 13 states and over 62 years, I leverage changes in selection method over time to credibly measure differences in the election outcomes produced by local election officials based on whether they were elected or appointed. I find that appointed officials out-perform their elected counterparts, increasing voter turnout by one to two percentage points and raising registration rates by one percentage point. Appointed officials appear to boost election administration resources, more actively communicate with voters, and reduce voter wait times. I present evidence that the quality of selection and sanctioning are higher for appointed officials, leading to better educated and more closely monitored agents. My findings speak to the challenges in designing local institutions that advance and protect democratic ideals.

Ferrer, Joshua, “How Partisan Is Local Election Administration?” Cambridge University Press, (2023).

In the United States, elections are often administered by directly elected local officials who run as members of a political party. Do these officials use their office to give their party an edge in elections? Using a newly collected dataset of nearly 5,900 clerk elections and a close-election regression discontinuity design, we compare counties that narrowly elect a Democratic election administrator to those that narrowly elect a Republican. We find that Democrats and Republicans serving similar counties oversee similar election results, turnout, and policies. We also find that reelection is not the primary moderating force on clerks. Instead, clerks may be more likely to agree on election policies across parties than the general public and selecting different election policies may only modestly affect outcomes. While we cannot rule out small effects that nevertheless tip close elections, our results imply that clerks are not typically and noticeably advantaging their preferred party.

[Furstenberg-Beckman, Hannah, Greg Degen, and Tova Wang, “Understanding the Role of Local Election Officials: How Local Autonomy Shapes U.S. Election Administration”](#)

This policy brief will examine the independence and discretionary powers of local election officials and offer a framework to better understand local autonomy in our electoral system. It will also describe the larger system within which the local election official operates and demonstrate how local power and voter-focused decision-making varies across the country. The brief will use illustrative examples of the exercise of autonomy by local election officials from past elections as well as examples of shifts in local discretionary powers from the recent wave of state legislative efforts that seek to restrict autonomy. It will also address the implications of

local autonomy for those with an interest in increasing voter access and promoting voter participation. This brief can be a resource for those seeking a better understanding of the possible levers of change in their own state or locality's electoral system.

[Rubin, Paul G. "Political Appointees vs. Elected Officials: Examining How the Selection Mechanism for State Governing Agency Board Members Influences Responsiveness to Stakeholders in Higher Education Policy-Making" *Education Policy Analysis Archives* 29:115 \(2021\)](#)

Through an exploratory comparative case study of two U.S. states (Georgia and Nevada), this study investigates how the selection mechanism to state higher education on governing agency boards influences the responsiveness of board members to stakeholders and their role in the policy-making process. Framed around the recent national policy agenda to improve postsecondary degree attainment and college completion, findings suggest that state agency board members in both states prioritized the opinions, insights, and goals of the state governor and governing agency staff, regardless of selection mechanism. However, for more localized issues and on the ground decision making, stakeholders formally involved in the day to day operation of higher education, such as administrators, faculty, and students, serve a larger role, though this influence can be mediated by the selection mechanism of board members.

[Guillermo, Toral. "The benefits of patronage: How political appointments can enhance bureaucratic accountability and effectiveness" \(2021\)](#)

The political appointment of bureaucrats is typically seen as a rent-seeking strategy that helps politicians sustain clientelistic networks and manipulate public administration to their advantage. I argue that political appointments can also increase bureaucratic accountability and effectiveness because they provide political and social connections between bureaucrats and politicians. These connections grant access to material and non-material resources, enhance monitoring, facilitate the application of sanctions and rewards, align priorities and incentives, and increase mutual trust. In certain conditions, political appointments can thus enhance bureaucrats' accountability and effectiveness in public service delivery. I test this theory using data on Brazilian municipal governments, leveraging two quasi-experiments, two original surveys of bureaucrats and politicians, and in-depth interviews. The findings challenge the traditional view that patronage is universally detrimental to development, and highlight how political appointments and connections can be leveraged to enhance public service delivery.

ACCOUNTABILITY AND INCENTIVES OF APPOINTED AND ELECTED PUBLIC OFFICIALS

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ACCOUNTABILITY AND INCENTIVES OF APPOINTED AND ELECTED PUBLIC OFFICIALS

Zohal Hessami*

Abstract—Political agency models suggest that elected public officials choose different policies than appointed officials do. This paper is the first (a) to apply a clean empirical design to study whether the selection rule has a causal effect on public officials' policy choices and (b) to investigate transmission channels. I exploit a unique setting in Germany, where a reform has created quasi-experimental variation in the selection rule for mayors. As the outcome variable, I use data on grant receipts for highly visible investment projects for which mayors must apply to the state government. Elected mayors attract 7% to 7.4% more grants in election years; for appointed mayors, there is no cycle. Using hand-collected data on mayor characteristics, I find suggestive evidence that although the selection of mayors changes following the reform studied, a likely reason for the observed cycle is that elected mayors have stronger electoral incentives.

I. Introduction

A central question in political economy is how to select public officials. Public officials may be appointed by an elected body, such as a legislature, or directly elected by voters. The way in which public officials are selected is likely to affect policy choices for two reasons. Based on political agency models, it is plausible that elected public officials face stronger incentives to perform well in office and have characteristics that make them more appealing to voters than appointed public officials (Besley, 2006; Persson & Tabellini, 2000).¹ This is the first study to explore whether there is a causal effect of the selection method for an important type of public official, local government executives, on policy choices and to investigate underlying mechanisms.

Previous literature on the selection method for local executives focuses on U.S. municipalities, where two main forms

of government exist: the council-manager system (the manager is appointed by the city council) and the mayor-council system (the mayor is elected by voters).² The majority of these studies use municipal spending in one or two U.S. states as the outcome variable. The results are mixed: some find that mayor-council cities spend more than council-manager cities (Booms, 1966; Lineberry & Fowler, 1967), while others reach the opposite conclusion (Clark, 1968; Coate & Knight, 2011; Sherbenou, 1961) or find no effect (MacDonald, 2008). A limited number of studies focus on other outcome variables. Levin and Tadelis (2010) find that privatizations take place more often in council-manager cities than in mayor-council cities. Vlaicu and Whalley (2016) are the first to consider political cycles: elected mayors hire more police officers in election years to please voters who value security. This is not the case in cities with appointed mayors. Enikolopov (2014) focuses on patronage as a tool for targeted redistribution and provides evidence that the number of public employees is larger in mayor-council cities than in council-manager cities, especially in election years.³

Although the literature on mayor-council and council-manager cities offers compelling results regarding the importance of the selection rule for policy choices, it has to confront a number of empirical difficulties. First, citizens in U.S. municipalities can choose their form of government in a local referendum that may give rise to reverse causality.⁴ A second issue is cotreatment: managers and mayors in the United States differ not only in the way they are selected. Managers are typically nonpartisan bureaucrats required to have a professional background in public administration. Mayors are professional politicians endowed with more formal powers to influence policy decisions than managers.⁵ A third difficulty is measurement error: official information on

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¹ This is also supported by empirical studies that show that policy choices are affected by selection and incentives of officeholders due to term limits (Alt, Bueno de Mesquita, & Rose, 2011; Besley & Case, 1995; List & Sturm, 2006) and the type of electoral rule (e.g., proportional versus plurality rule) that is applied (Funk & Gathmann, 2013; Gagliarducci, Nannicini, & Naticchioni, 2011; Persson and Tabellini, 2003).

² According to Enikolopov (2014), two-thirds of all U.S. cities can be classified into these two types. The remaining ones are cities with commission, town meeting, or representative town meeting forms of government, as well as counties with commission form of government.

³ Besides these studies on the form of local government, there is also a related literature on the selection method for officials in other areas of the public sector. Choi, Gulati, and Posner (2008) show that while elected judges write more opinions than appointed ones, their opinions tend to be of lower quality (see also Iaryczower, Lewis, & Shum, 2013, and Lim, 2013). Besley and Coate (2003) find that elected regulators choose more pro-consumer-friendly policies than appointed ones. Other public officials for whom selection rules have been explored are school superintendents (Partridge & Sass, 2011) and city treasurers (Whalley, 2013).

⁴ One notable attempt to address endogeneity is the use of precipitation shocks as an instrument for changes in the form of government (Vlaicu & Whalley, 2016).

⁵ The two main forms of government in the United States may also differ in other respects, as any detail can be changed in a local referendum. Recently, many council-manager cities have also selected a mayor who serves on the council but has less political influence than a mayor in a mayor-council city.

the form of government in U.S. cities is not available. The studies therefore rely on administrative surveys, which have been shown to provide diverging classifications for some municipalities, in turn leading to contradictory empirical results (Coate & Knight, 2011).⁶

In this paper, I exploit a quasi-experiment at the level of German municipalities to study the effect of the selection rule for mayors on their policy choices. Following a statewide referendum in 1991, municipalities in the German state of Hesse switched from mayor appointment to direct mayor elections between 1993 and 1998. The end of the term of the last appointed mayor varies across municipalities for exogenous historical reasons that are orthogonal to municipality characteristics. This randomization determines the timing of the switch in a particular municipality. At the same time, the reform did not change the balance of power and responsibilities between the mayor and the local council. Finally, official information on how mayors are selected is available for all municipalities in each year of the sample period.⁷

This paper makes three substantive contributions to the literature. First, the reliance on a natural experiment and the availability of administrative data on the mayor selection rule in a given municipality and year allow me to identify the causal effect of direct elections on mayors' policy choices. Specifically, these features allow me to circumvent the three problems in the existing literature on the form of government reviewed above (reverse causality, cotreatment, and measurement error). A second contribution of my study is that I use a new outcome variable to analyze policy choices. U.S. studies use municipal spending and public employment as the outcome variable. I use investment grant receipts for several reasons. First, a municipality may receive a grant for an investment project if and only if a detailed application

is prepared and submitted by its mayor. Therefore, grant receipts directly depend on mayors' effort and competence, and mayors can claim exclusive credit for grant receipts. This is supported by anecdotal evidence that mayors utilize their success in attracting investment grants in their electoral campaign via the local media.⁸ On the other hand, spending and police hiring are not under the direct control of mayors in my context. Second, investment grants are restricted to public projects that happen to be highly visible to voters (e.g., day care facilities, road construction). A third contribution is that I combine official election and fiscal data at the municipality level with hand-collected information on individual mayors. Data on mayor appointments (e.g., dates, identities of mayors, party affiliations) are not available from an official source. Similarly, there is no official information on the education level, occupational background, and age of elected or appointed mayors. This unique combination of data allows me to investigate transmission channels.

Political agency models provide a useful theoretical background to think about the effect of the selection rule for mayors on investment grant receipts. First, mayors may have stronger incentives to perform well when they are elected. Given that voters can oust them from office if they perform poorly, elected mayors may be less susceptible to moral hazard and exert more effort to attract transfers (Barro, 1973; Ferejohn, 1986). Direct elections may also incentivize mayors to signal their competence to voters prior to an election. Following Rogoff and Sibert (1988) and Rogoff (1990), such incentives would lead to political budget cycles in investment grants. Second, elected mayors may attract more transfers because they have different characteristics than appointed mayors do. If the preelectoral signaling as modeled in Rogoff and Sibert (1988) and Rogoff (1990) is effective, candidates elected into office may be more competent and therefore more efficacious in attracting additional grants even if incentives had not changed. More generally, the candidate pool may be endogenous to the selection method for the mayor's office, and voters may be more likely to select competent mayors than party officials (Besley, 2005).

In line with these theoretical considerations, I find that in an election year, elected mayors attract on average 7% to 7.4% more investment grants for their municipality from the state government. For appointed mayors, there is no cycle.⁹

⁶ While measurement error may seem like a minor issue at first sight, studies indicate that estimates for the relationship between the form of government and public spending depend decisively on the accuracy of classifications for the form of government. MacDonald (2008) and Coate and Knight (2011) use a similar sample and estimation techniques but reach different conclusions: MacDonald (2008) finds no differences, while Coate and Knight (2011) find that spending is higher in council-manager cities. Coate and Knight (2011) explain this as follows: "The differences appear to be driven by differences in the switching measure used. While MacDonald used a measure based upon comparison of reported government form across survey years, we use a more conservative measure based upon a reported change in form of government" (p. 105).

⁷ I am not the first to analyze reforms of mayor selection in Germany. Previous studies investigate the effect of presidentialism/parliamentarism or the use of the plurality/proportional rule on fiscal outcomes at the local level. Blume, Doering, and Voigt (2008) find that when mayors are elected, local spending and revenues decline in municipalities in Schleswig-Holstein and converge toward municipalities in Baden-Wuerttemberg, where mayors have always been elected. Since expenditures are aggregated at the state level, however, the observed effects cannot be disentangled from state-specific effects. Ade (2013) focuses on three German states and provides evidence that tax rates are lower and public spending is higher in cities with elected mayors. Egger, Koethenburger, and Smart (2007) analyze the introduction of mayor elections in Lower Saxony, which was accompanied by a large increase in the power and responsibilities of mayors. This reform therefore resembles a move from the U.S.-style council-manager system to the mayor-council system. The authors find that redistributive spending increases with mayor elections.

⁸ One example is an article in a local newspaper, "Mayor Election in Langen—Frieder Gebhardt Starts the Electoral Campaign," December 7, 2013. The mayor strategically mentions his success in attracting investment grants and lists several infrastructure projects: "When he looks back on his six years in office, he thinks: 'It is great what we managed to get done.' Examples are the successful town hall project, the reconstruction of the gymnasium . . . , the reconstruction of a child care facility and the construction of an additional childcare facility" (<http://www.op-online.de/lokales/nachrichten/langen/buergermeisterwahl-langen-frieder-gebhardt-spd-eroeffnet-wahlkampf-3259423.html>).

⁹ This paper also contributes to an emerging empirical literature on political budget cycles that are conditional on institutional features at the level of subnational governments (Aidt & Mooney, 2014; Aidt, Veiga, & Veiga, 2011; Akhmedov & Zhuravskaya, 2004; Sjahrir, Kis-Katos, & Schulze, 2013).

Using hand-collected data on mayor characteristics, I also present suggestive evidence for electoral incentives rather than selection as a more relevant explanation for the cycle in my setting. First, the estimate for the cycle in investment grants hardly changes when I control for mayor characteristics. Second, I show that for the subset of mayors who were in office in both regimes, the estimate is similar to the baseline estimate.

Three extensions to the baseline specification validate my main findings. First, I show that when only considering elections that take place late in a year, the increase in investment grants in election years is as high as 11%. Second, the last cohort of appointed mayors that faced a direct election for the first time was particularly motivated to attract transfers, presumably due to the expectation that voters prefer different types of mayors. In the year of the first direct elections, the increase in investment grant receipts amounts to about 10%. Third, I provide further evidence that electoral incentives are mainly responsible for the cycle by exploring the issue of divided government. Electoral incentives of mayors are likely lower under divided government because voters may be less likely to attribute investment grant receipts only to the mayor. A council dominated by opposition parties may try to claim credit for any additional grants. Conversely, the mayor may attempt to shift the blame to the council if she fails to attract them. In line with this argument, I observe a cycle only for the subset of elected mayors who do not face a divided government.¹⁰

The findings in this paper are interesting not only because they provide credible causal evidence that the selection method for public officials such as mayors affects policy choices (internal validity), but also because the Hessian setting has more general relevance (external validity). Many other German states (and even countries such as Italy) have recently replaced mayor appointments with mayor elections. From a policy perspective, it is important to know whether and how the switch has affected policy choices of mayors. However, other settings are not as amenable to a causal analysis of this question. The switch to directly elected mayors in other German states, for example, was typically accompanied by a change in mayors' competencies. By isolating the effect of the selection rule from other concurrent changes to the responsibilities of the mayor, the results in this paper allow us to better understand the specific implications of various reforms to this public office that typically happen simultaneously.

The remainder of this paper is structured as follows. Section II discusses the theoretical background. Section III provides information on the institutional background. Section IV describes the empirical strategy and the data. Section V reports the main estimation results. Section VI

presents the results for several extensions. Section VII concludes.

II. Theory

Political agency models suggest two mechanisms for an effect of selection rules on policy choices: electoral incentives and selection. This section reviews theoretical contributions on these mechanisms to derive a prediction on the effect of the selection rule for mayors on investment grant receipts.¹¹

In first-generation political agency models, pioneered by Barro (1973) and Ferejohn (1986), elections motivate officeholders to exert effort on behalf of voters. For example, in Ferejohn (1986) voters adopt a simple decision rule where they reelect an incumbent only if her policy choices ensure a level of utility that is above a certain threshold. The incumbent thus faces electoral incentives to exert sufficient effort to reach this level.

In second-generation agency models, such as Rogoff and Sibert (1988) and Rogoff (1990), voters prefer to reelect competent incumbents but cannot directly observe competence. They know, however, that competent incumbents are more capable of temporarily manipulating fiscal variables. Public officials thus have an incentive to create political budget cycles by choosing low tax rates or high public expenditures before an election to signal their competence. Rogoff and Sibert (1988) and Rogoff (1990) show that such preelectoral manipulation can result in separating equilibria where only competent officials are retained by voters. Consequently, elections may also affect policy choices by making it more likely that competent candidates are selected into office.¹²

In view of this literature, it is plausible that a switch from appointment to direct election leads to higher investment grant receipts. First, elected mayors arguably have stronger electoral incentives than appointed mayors. When mayors are elected, the link between mayors' policy choices and their probability of reelection is immediate. Thus, elected mayors face incentives to attract more transfers, in particular when an election is imminent. In contrast, voters can hold appointed mayors accountable only in an indirect way. When an appointed mayor performs poorly, voters can only attempt to oust him from office by voting in the next council elections for a party that is opposed to the mayor. Yet in council elections, voters cast their vote based on a broader

¹¹ Note that an older literature in industrial organization suggests an ambiguous prediction for this relationship. The argument there is that while elected officials are subject to a reelection constraint, appointed officials are subject to a comparable reappointment constraint. Thus, elected and appointed officials may face ultimately the same incentives and choose the same policies to please the median voter (Baron, 1988; Deno & Mehay, 1987; Laffont, 1996).

¹² That selection may affect policy choices is argued even more explicitly in citizen-candidate models on agency where voters elect candidates according to their characteristics because voters anticipate that candidates with certain characteristics are committed to specific policies (Osborne & Slivinski, 1996; Besley & Coate, 1997).

¹⁰ One may also suspect that municipalities received extra funds from the state tier when they switched to the new regime as compensation for reform-related expenses. However, it is unlikely that such additional resources would be channeled through the investment grants program. I am also not aware of any such grants having been allocated.

set of policy outcomes (see section IIIA for more details on the responsibilities of the council) and an appointed mayor may therefore have smaller incentives to attract grants.¹³ Second, the switch to direct elections may lead to an increase in investment grants through the selection of mayors. The introduction of mayor elections may induce a more competent breed of candidates to run for the mayor's office for the following reasons: the personal characteristics of candidates have become more salient, candidates are more exposed to the public in a personalized election campaign, and elected mayors are more likely to enjoy personal satisfaction from attaining office. In addition to the increase in average competence in the candidate pool, direct elections may increase the likelihood that more competent candidates are elected into office because voters might be more likely to choose such candidates than party elites (Besley, 2005). Given that elected mayors are on average more competent, it is possible that elected mayors are more effective in attracting transfers than appointed mayors are.

To summarize, elected mayors may attract more transfers than appointed mayors because elections give rise to stronger electoral incentives and a more competent selection of mayors. First-generation agency models predict that transfers will be higher over the entire term. This prediction also follows from second-generation models since elections may lead to the selection of more competent officeholders. Second-generation models also predict that grant receipts are particularly high in election years.¹⁴

III. Institutional Background

A. Municipalities, Mayors, and Local Public Finances

Hesse is among the more prosperous states in the German federation and has been ruled by both left-wing and right-wing governments. The population size of Hesse was 6.1 million at the end of 2012 (about 7.6% of the German population). Hesse consists of 421 regular municipalities and 5 exceptionally large municipalities that have a special status and therefore assume both municipal and county responsibilities.¹⁵

¹³ The policy choices of appointed mayors are more likely to be determined by nonelectoral career concerns (Alesina & Tabellini, 2007). For an appointed mayor who wants to retain her position, for example, (partisan) loyalty to the council majority may be more important than any success in attracting grants. Alternatively, for an appointed mayor whose career options include a possible switch to the private sector or a promotion to higher office, it may be more important to be perceived as a professional administrator who is able to ensure the smooth functioning of the administration rather than helping the council majority get reelected.

¹⁴ Note that in Rogoff-type agency models of the rational political budget cycle, electoral cycles are due to both selection and accountability effects. Yet evidence for an electoral cycle could also be consistent with an accountability-only agency model if voters are forgetful, as in Findley (2015).

¹⁵ Since these five municipalities (Darmstadt, Frankfurt, Kassel, Offenbach, and Wiesbaden) are not comparable to the 421 regular municipalities due to their special status, I exclude them from the empirical analysis.

Hessian municipalities provide their citizens with a number of goods and services, the provision of which is either voluntary or compulsory. Compulsory provision includes primary schooling, municipal day care services, and civil protection. Voluntary provision includes swimming pools, sports venues, and hospitals. Public provision of goods and services at the municipal level is financed with municipal taxes, user fees, and transfers. In 2013, Hessian municipalities had 15.5 billion euros at their disposal. General expenditures are financed through taxes and transfers, while user fees are charged for specific services. Transfers from the state tier make up a large fraction of municipal revenues. There are various transfer programs, which I describe in section IIIC.

The municipal code of Hesse (*Hessische Gemeindeordnung*)—the magistrate constitution—stipulates that the local council is in charge of key decisions and monitors the overall administration.¹⁶ The council can, however, hand over some decisions to the mayor. The mayor usually works full time and is supported by two individuals who work in an honorary capacity. Together they form the magistrate.¹⁷ The mayor is present at council meetings and is entitled to voice her opinion even though she does not have a vote.

B. The Transition from Mayor Appointments to Mayor Elections

The introduction of mayor elections in Hesse was a rather unexpected reform that was strategically placed on the agenda during an electoral campaign at the state level. At the end of the 1980s, the Christian-Democratic president of Hesse, Walter Wallmann, initiated parliamentary deliberations that culminated in a referendum on the introduction of mayor elections. This came as a surprise since this reform was not popular with the ruling conservative-liberal parties (CDU and FDP), but it was with left-wing parties. Political observers have interpreted Wallmann's move as a strategic maneuver in the run-up to the state election on January 20, 1991. It is therefore also no coincidence that the referendum took place on the same day as the state election (von Arnim, 2002). Yet, not only was the referendum unexpected; so was the popularity of this reform: 82% of voters supported the reform. The relevant laws were finally changed in May 1992. The reform was hence not based on considerations to motivate mayors to exert more effort in applying for state grants. It was simply a strategic maneuver to win the support of a broader group of voters for the state election.

For historical reasons, the introduction of mayor elections (and the start of the terms of the first elected mayors)

¹⁶ The council is elected from party lists under a single-district proportional election rule and is headed by an elected chairman, who presides over council meetings.

¹⁷ The size of the magistrate is determined by the municipality itself. Especially in large cities, there may be more than three magistrate members.

was spread out between 1993 and 1998.¹⁸ Two important sources of this variation are the initial asynchronous dates for mayor appointments in 1946 and 1947 and municipal merger reforms in the 1970s. According to the Hessian municipal code (§42, HGO), the appointment or election of a mayor has to take place three to six months before the incumbent steps down.¹⁹ Hence, some of the variation is due to the autonomy that municipalities have in choosing the date within this three-month window and the way in which the dates are shifted further and further apart over the decades. Another reason is that some mayors have not served a full term often for reasons beyond their control (death, sickness, retirement, or dismissal by the council).²⁰ After the last appointed mayor in a municipality had completed her six-year term, the first directly elected mayor took office.

The introduction of mayor elections has not affected the division of formal powers and responsibilities. The reform has, however, removed the dependence of the mayor on the local council. This independence also comes at a cost for the incumbent mayor. When running for reelection, the incumbent mayor can no longer rely on the entrenched majority of her party in the council.²¹

While the role of mayors is mostly limited to the execution of council decisions (see section IIIA), the reform of mayor selection is nevertheless likely to have affected policy outcomes in Hessian municipalities. As I describe in section IIIC, mayors are responsible for the preparation and submission of applications for state investment grants. They can submit these applications without permission by the council.²²

This paper focuses on Hessian municipalities for two reasons that make Hesse a compelling setting to study the effects

of the selection method for mayors on policy choices.²³ First, the powers and responsibilities of Hessian mayors were not changed by the reform. This is different in other German states that later introduced mayor elections. A second advantage is that the timing of the switch varies across Hessian municipalities for exogenous historical reasons. These two unique features allow for a clean identification.

C. Allocation of Investment Grants

The German constitution requires that citizens have some minimum living standard regardless of where they live.²⁴ Within each German state, this principle is reflected by a municipal equalization scheme (*Kommunaler Finanzausgleich*). These schemes require that the state government provides poorer municipalities and municipalities that face a higher burden of public provision of goods and services with the necessary financial means.

The first step in the municipal equalization scheme is the determination of the total amount of resources to be allocated, which varies from year to year. It equals 23% of the state government's total revenues from the income tax, the corporate tax, the sales tax, the wealth tax, the motor vehicle tax, the property acquisition tax, and the apportionment of the business tax, plus any additional resources that the state government may wish to add. In 2013, the state government allocated 3.8 billion euros by the municipal equalization scheme.

More than half of these resources are allocated to Hessian municipalities via general-purpose grants (*Schlüsselzuweisungen*), which are allocated according to specific rules that into account the fiscal need and the fiscal capacity of each municipality. The fiscal capacity measure reflects the hypothetical tax-raising potential of a municipality, while the fiscal need measure is a function of a municipality's population size.²⁵

Special-purpose grants (*besondere Finanzausweisungen*) differ from general-purpose grants since they are tied to a particular purpose (e.g., schooling, day care facilities,

¹⁸ The first mayor elections took place on May 2, 1993, in Alsfeld, Borken, Gudensberg, Lauterbach, Ottrau, and Waldems. The last municipality that held its first mayor election was Melsungen on November 8, 1998.

¹⁹ If a term ends prematurely for unforeseen reasons, it has to take place within four months.

²⁰ To give an example, the first mayor of Hanau (Karl Rehbein) was appointed twice by the local council but died after ten years in office, two years ahead of the end of his second term. The second mayor (Heinrich Fischer) served a full term of six years. The third mayor of Hanau (Herbert Dröse) was elected twice into office but retired in the middle of his second term. The fourth mayor of Hanau (Hans Martin) served two full terms. Afterward, Helmut Kuhn was in office for only about sixteen months before he was dismissed by the local council. Finally, Hans Martin was elected again into office two times before retiring three years ahead of the end of his last term.

²¹ Before the reform, the mayor was appointed by the local council via a two-thirds majority vote. The council was able to remove the appointed mayor from office if it wished to do so. Since the reform, the mayor is elected by the people. If no candidate receives an absolute majority of votes, a runoff election takes place with the two most successful candidates of the first round running against each other. Note that turnout in council elections has decreased from an average of 78% in the prereform period to 61% in the postreform period, which indicates that the introduction of mayor elections may have given rise to some degree of voter fatigue.

²² Given this particular role of mayors in Hesse, using other outcome variables such as municipal tax rates and spending or even nondiscretionary outcomes such as rule-based transfer receipts can serve to further validate my estimation results for investment grants via placebo tests. In online appendix B, I provide estimation results showing that none of the other fiscal outcomes are affected by the reform of mayor selection.

²³ The reform in Hesse had an impact on West German local politics beyond the boundaries of Hesse. Other German states where mayors had traditionally been appointed followed suit after the Hessian reform. Only in Bavaria and Baden-Wuerttemberg had mayors always been elected since World War II. Mayor elections were introduced in Rhineland-Palatinate in 1993, North-Rhine Westphalia and Saarland in 1994, Schleswig-Holstein in 1995, and Lower-Saxony in 1996.

²⁴ This is not explicitly stated in the constitution, but various provisions are usually interpreted accordingly. Specifically, Article 72, §2 of the Basic Law in its current wording states that the federal government may adopt legislation in a broad range of policy areas if such legislation is necessary to ensure similar living conditions (*gleichwertige Lebensverhältnisse*) in the federation. Article 106, §3.2 mentions similarity of living conditions (*Einheitlichkeit der Lebensverhältnisse*) as one goal the federal revenue-sharing scheme must strive to achieve.

²⁵ When fiscal need exceeds fiscal capacity, a certain fraction of this difference is compensated with a grant that is allocated to the municipality in question. Any shortfall of fiscal capacity below 80% of the fiscal need measure is completely equalized. When fiscal capacity exceeds fiscal need, a municipality nevertheless receives a minimum amount of general-purpose grants. This minimum is a function of population size.

nursery schooling, youth welfare services, social welfare, public transport, theaters, museums, libraries, music schools, culture, road construction, health resorts). The allocation of special-purpose grants in most cases follows a formula. For example, schooling transfers depend on the number of enrolled students, while road construction transfers are a positive function of the kilometers of roads that already exist within a municipality's boundaries.

Investment grants (*Investitionszuweisungen*) are tied not only to a specific purpose but even to a particular investment project. The following types of projects are covered by this grant scheme: hospitals, municipal drinking water systems, municipal (hazardous) waste disposal, public transport, municipal road construction, municipal child care facilities, municipal elderly care facilities, habitat protection, municipal energy-saving measures, business-related municipal infrastructure, village and city renewal, water protection, libraries, museums, music schools, sports facilities, and municipal facilities for the disabled. In 2010, the Hessian state government allocated 388 million euros as investment grants.

Investment grants also differ from general-purpose and special-purpose grants in a second important aspect: they are allocated via an application procedure (*Antrags- und Bewilligungsverfahren*).²⁶ The type of the project determines to which ministry the application has to be sent. For instance, the Ministry of Social Affairs is responsible for projects regarding elderly care and hospitals, while the Ministry of Economic Affairs, Traffic, and Land Renewal is responsible for projects that concern public transport or village renewal.

The procedure for the allocation of investment grants is as follows. First, in the second half of the preceding year, the state government disseminates a call for proposals for each type of project via the state government gazette (*Staatsanzeiger*) and the responsible state ministries. Each call for proposals provides information on the goals of the grant program, details on which kinds of projects can be financed, and a list of required documents to support the application.²⁷ The call for proposals closes with an application form that has to be completed and to which the supporting documents have to be attached. This includes extensive documentation that the project complies with a long list of funding guidelines.

Mayors are responsible for the preparation and submission of the grant application packages. The responsible ministry decides whether a project grant is approved based on whether the project fulfills the catalog of funding guidelines and whether all other important information has been provided. Generally, as long as the funding guidelines are fulfilled and there are enough resources available, municipalities are

awarded the grant. The decision is usually taken by the responsible ministry within a few weeks. In principle, it is even possible that in the same year, a municipality is awarded more than one grant by one of the ministries. The likelihood that a grant is awarded is generally higher at the beginning rather than the end of a year when, as is usually the case, all resources are depleted. Yet if an application fulfills all the criteria but no resources are left over, the grant will simply be awarded in the following year.²⁸

Mayors who seek to be reelected have several ways in which they can increase the likelihood that their municipality receives an investment grant. First, taking the trouble of preparing and submitting an application is a necessary condition for any grant receipt. Second, mayors should submit the applications early rather than late in the year. Third, mayors should ensure the quality of the application (whether the application is complete and fulfills all funding guidelines).

IV. Empirical Strategy and Data

A. Empirical Strategy

I investigate whether elected mayors attract more investment grants from the state government than appointed mayors do. As argued in section II, elected mayors may attract more grants because their electoral incentives are larger or because they are more likely to have characteristics that make them efficacious in writing grant applications.

My estimations are based on a difference-in-differences (DiD) approach. The reform divides the sample into pre- and posttreatment periods, as well as treatment and control groups. The identifying assumption for any DiD design is that the outcome in the treatment and control group would follow the same trend in the absence of treatment. This assumption is usually verified by comparing the pretreatment trends of the treatment and control groups.

Municipalities that introduced mayor elections in 1994 or later serve as the control group for the municipalities that were treated in 1993. The control group for the other annual cohorts is determined analogously. The identifying assumption for my empirical design is therefore that all six cohorts have parallel pretreatment trends in the outcome variable (log of investment transfers per capita), which is confirmed by figure 1. As argued before, the timing of the municipality-specific switch to mayor elections is due to exogenous historical factors that are orthogonal to investment grant receipts. This natural randomization makes it even more likely that the six cohorts are similar and provides a reasonable approximation for counterfactual posttreatment trends in the absence of treatment. The exogeneity in the timing of treatment is further examined in section IVC.

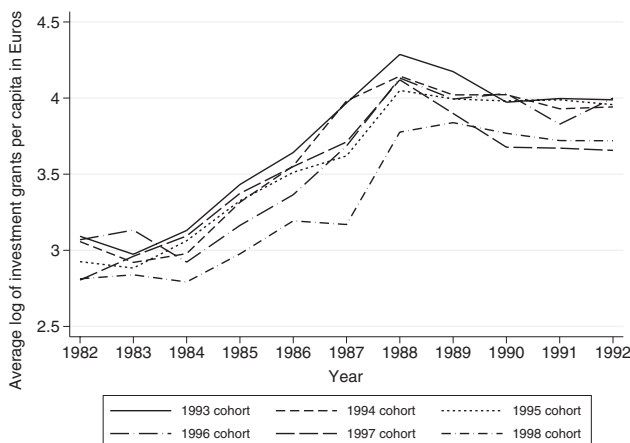
I implement the DiD approach using the dummy variable *ElectedMayor* that is 1 for a municipality as of the year

²⁶ These grants cover a large share but not the full cost of a project. The share of the project covered by the grant depends on the type of project and the type of costs.

²⁷ To give an example, in December 2012, a call for proposals published in the *Staatsanzeiger* concerned projects on energy-efficient refurbishments of municipal buildings (Hessisches Ministerium für Umwelt, Energie, Landwirtschaft und Verbraucherschutz, 2012). The application form was eight pages long, and at least seven supporting documents were required.

²⁸ If a large multiyear project such as the construction of a hospital is involved, the grant is paid in yearly installments conditional on the progress of the project.

FIGURE 1.—PARALLEL PRETREATMENT TRENDS IN THE OUTCOME VARIABLE ACROSS THE SIX REFORM COHORTS



The 421 municipalities are divided into six cohorts according to the year in which a mayor was first elected in a municipality. Each line in the figure represents averages in the log of investment transfers per capita for one of the six cohorts.

when voters have elected their mayor for the first time. The baseline model is

$$y_{it} = \beta \text{ElectedMayor}_{it} + \alpha_i + \mu_t + \mathbf{X}\gamma + \varepsilon_{it}, \quad (1)$$

where y is the log of investment transfers per capita in euros.²⁹ Subscript i refers to one of the 421 municipalities, and subscript t refers to a year in the sample period from 1982 to 2010. α_i are municipality fixed effects, and μ_t are time fixed effects. To obtain more precise estimates, I also include a number of controls in some of the estimations. The vector $\mathbf{X}$ includes demographic variables (log of population size, share of 0–14-year-olds, share of over-65-year-olds) as well as fiscal variables (log of rule-based transfers per capita, municipality-specific tax rate multipliers for the business tax rate and the tax rate for residential property).

While the specification in equation (1) implies that the effect is constant throughout a mayor's term, it is plausible that differences are particularly pronounced or significant in the years in which a mayor was selected, that is, in an appointment or election year (see the discussion in section II). My preferred specification therefore additionally includes a dummy variable that is 1 in each year where a mayor was either appointed or elected (*Election/AppointmentYear*) and interact this dummy with the *ElectedMayor* dummy. The estimation equation looks as follows:

²⁹ There are 77 observations with transfers equal to 0. I follow the standard approach in the literature and use $\log(\text{investment transfers per capita} + 1)$ for all observations. There is generally no consensus on how to deal with this problem. I have investigated the robustness of my results by instead excluding the observations where investment grant receipts are equal to 0. In these additional estimations (see table B.1 in online appendix B), the coefficient of the interaction term is slightly larger. As a second alternative, I have conducted estimations with a level-specification excluding outliers (available on request). The results are also qualitatively similar.

$$y_{it} = \beta \text{ElectedMayor}_{it} + \delta \text{Election/AppointmentYear}_{it} + \phi \text{ElectedMayor}_{it} \times \text{Election/AppointmentYear}_{it} + \mathbf{X}\gamma + \alpha_i + \mu_t + \varepsilon_{it}. \quad (2)$$

All hypothesis tests use heteroskedasticity-robust standard errors. Bertrand, Duflo, and Mullainathan (2004) show that autocorrelation is a substantial concern for the DiD approach. Therefore, in all of my estimations, the standard errors are clustered at the level of the 421 municipalities. Some estimations also include county-specific time trends to capture systematic variation over time in a more flexible manner.³⁰

While the above specifications allow me to test whether policy choices of mayors depend on the selection rule, any differences may be due to either incentive or selection effects. To discriminate between these two channels, I adopt two strategies. First, I add mayor characteristics as covariates to equation (2) and estimate the following model:

$$y_{it} = \beta \text{ElectedMayor}_{it} + \delta \text{Election/AppointmentYear}_{it} + \phi \text{ElectedMayor}_{it} \times \text{Election/AppointmentYear}_{it} + \mathbf{X}\gamma + \mathbf{Z}\omega + \alpha_i + \mu_t + \varepsilon_{it}, \quad (3)$$

where $\mathbf{Z}$ is a vector of mayor characteristics such as age, gender, or educational background. Any effect of direct elections on policy choices due to a different selection of mayors should be accounted for by these additional covariates. The estimates for the cycle can then be interpreted as the residual effect of larger electoral incentives.

As a second strategy, I restrict the sample used to estimate equation (2) to the set of mayors who were in office under both the appointment and the election system. As the underlying characteristics of these mayors have remained constant, any change in investment grant receipts can be plausibly ascribed to changes in electoral incentives. While a caveat to this approach is that this subsample of mayors is (self-)selected, these estimates can provide further suggestive evidence on the relative importance of electoral incentives and selection.³¹

B. Data

Official data. The source for official information on mayor elections and fiscal variables is the Statistical Office of Hesse. This includes a data set on mayor elections in all 421 municipalities since 1993. The data set provides information on the names, gender, and party affiliations of all candidates; the number of valid votes per candidate; and the election date. The Statistical Office also provides data on the

³⁰ In online appendix B, I present additional estimations with county-specific time dummies. The results are similar.

³¹ Specifically, even if the sample is (self-)selected, any observed differences in policy choices in this subset of mayors cannot be due to selection based on fundamental personal characteristics as gender, educational background, or prior professional career.

amount of investment grants per capita in euros that a municipality has received from the state government in a certain year. I use these data as the dependent variable in my estimations. Finally, I have obtained data from the Statistical Office on a number of fiscal and demographic variables. Summary statistics are provided in table A.1 in online appendix A.

Hand-collected data. The data provided by the Statistical Office are not sufficient to conduct the empirical analysis as described in section IVA. In particular, data on mayor appointments (names, gender, party affiliations of mayors, appointment dates) and mayor characteristics (dates of birth, education levels, and fields of occupation) are not available from an official source. I have hand-collected these two missing types of data from various Internet sources such as municipality websites, mayors' personal websites, and e-mail communication with the mayors or their administrative assistants and employees of the municipal archives. The data collection for this unique data set took place between September 2013 and March 2014. The hand-collected data cover 81.3% of the sample (9,926 out of 12,209 municipality-year observations).

Table A.3 in online appendix A collects descriptive evidence on changes in mayor characteristics over time. The *t*-tests provide evidence that there are some significant differences in mayor characteristics: elected mayors are more likely to have a degree from a higher education institution, they have studied other subjects at the university, they have a different professional background, they are more likely to be independent candidates, they are more often women, and they are older.³²

C. Exogenous Timing of First Mayor Elections

A closer look at the data suggests that the timing of the municipality-specific switch to mayor elections is indeed quasi-random. There are in particular three pieces of evidence. First, table 1 describes the timing of the introduction of mayor elections in the 421 municipalities between 1993 and 1998. The first mayor elections are distributed quite evenly over these six years as the quasi-experimental nature of the reform would suggest. Second, figure A.1 in online appendix A illustrates that the six reform cohorts are not geographically clustered.³³ Third, if this is a true natural experiment, the six reform cohorts should not differ in terms of predetermined characteristics. I have collected data on ten variables that describe the fiscal stance, the demographic structure, public employment, and the size of municipalities. For each characteristic, I calculate averages for the year 1992 and conduct two-group mean-comparison *t*-tests

³² I also find that direct elections have likely led to a different selection of mayors by studying incumbency effects. The incumbency advantage of mayors declines after the reform, which indicates that voters prefer different types of candidates than party elites do. These results are available on request.

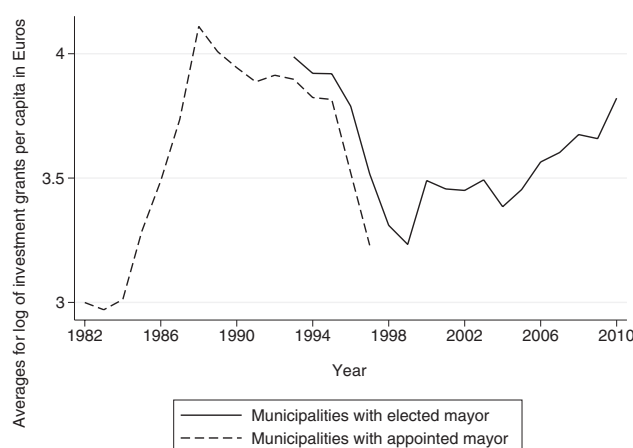
³³ This excludes imitation of neighboring municipalities that elected their mayor as one motive for self-selection into one of the six cohorts.

TABLE 1.—NUMBER AND SHARE OF APPOINTED AND ELECTED MAYORS IN OFFICE, 1992–1998

Year	Mayors Selected by Council Appointment		Mayors Selected by Direct Election	
	Number	Share	Number	Share
1992	421	100%	0	0%
1993	338	80.3%	83	19.7%
1994	253	60.1%	168	39.9%
1995	175	41.6%	246	58.4%
1996	90	21.4%	331	78.6%
1997	46	10.9%	375	89.1%
1998	0	0%	421	100%

The table summarizes how many appointed and elected mayors were in office between 1992 and 1998 (the period in which mayor elections were introduced). Increases in the number of mayors selected by direct election indicate the number of municipalities that introduced mayor elections in a given year.

FIGURE 2.—AVERAGE INVESTMENT TRANSFER RECEIPTS OVER TIME: MUNICIPALITIES WITH APPOINTED OR ELECTED MAYORS



The dashed line plots the evolution of the average log of investment transfer receipts in municipalities where an appointed mayor is in office in a particular year. The solid line plots the evolution of average log of investment transfer receipts in municipalities where an elected mayor is in office in a particular year.

for all possible pairs among the six cohorts. Table A.2 in online appendix A summarizes the results. While there are more significant differences than would be expected by pure chance, a large majority of the pairwise comparisons are insignificant. Moreover, there is no systematic pattern in the few significant *t*-tests, suggesting, for example, that later cohorts are systematically more or less likely to exhibit certain characteristics.

V. Results

A. Graphical Evidence

Figure 2 plots averages over time for the log of investment grants per capita received by two types of municipalities: those that still retain an appointed mayor (dashed line) and those that have introduced mayor elections (solid line). The dashed line covers the time period 1982 to 1997 and the solid line covers the time period from 1993 to 2010. There is substantial variation over time in grant receipts. One reason is, as pointed out in section IIIC, that the amount of resources available for the allocation of grants depends on

TABLE 2.—APPOINTED VERSUS ELECTED MAYORS AND INVESTMENT GRANTS FROM THE STATE TIER

	I	II	III
<i>Elected mayor</i>	0.039 (0.046)	0.040 (0.046)	0.038 (0.046)
Year fixed effects	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes
County-specific time trends	No	Yes	Yes
Control variables	No	No	Yes
Observations	12,209	12,209	12,209
Municipalities	421	421	421
R^2	0.122	0.139	0.143

The dependent variable is the log of investment transfers per capita. Standard errors in parentheses are robust to heteroskedasticity and clustered at the municipality level. Control variables include share of population over 65 years, share of population below 15 years, log of population size, log of rule-based transfers per capita, and business and property tax rate multipliers.

the tax revenues that the state government collects in a particular year, which in turn is highly dependent on the business cycle.

Independent of trends in average investment grants, during the phase-in period, the solid line is always above the dashed line: municipalities with elected mayors receive on average more investment grants than municipalities with appointed mayors do. Despite the fact that the composition of the two groups of municipalities changes over time as municipalities gradually elect a mayor for the first time, the distance between the two lines is fairly constant. This pattern additionally confirms the quasi-random timing in the switch to mayor elections, as discussed in section IVC.

B. Baseline Results

Table 2 collects the results for DiD regressions that relate a dummy for the mayor selection scheme to investment transfer receipts (see equation [1]). Model I includes only a dummy for the selection scheme. Model II adds county-specific time trends. Model III additionally includes control variables. The estimated coefficient for the *Elected Mayor* dummy is insignificant in all three models: there are no significant differences in grant receipts.

One reason that the coefficient is not significant is possibly that elected mayors attract higher transfers only when a mayor election is about to take place—in the election year itself. To explore this possibility, I compare transfer receipts in mayor election years with transfer receipts in mayor appointment years. Table 3 has the same structure as table 2. The only difference is that two additional variables are included: the *Election/Appointment Year* dummy and its interaction with the *Elected Mayor* dummy (see Equation [2]).

The estimated coefficient for the interaction term in all three models is significant at the 5% level. Transfer receipts are on average 7% to 7.4% higher in mayor election years compared to mayor appointment years. That is, I observe a cycle only in the new regime. In view of my results in table 2, this implies that the switch from appointments to elections has induced a shift in the timing of grant receipts

TABLE 3.—APPOINTED VERSUS ELECTED MAYORS AND ELECTORAL CYCLES IN INVESTMENT GRANTS FROM THE STATE TIER

	I	II	III
<i>Elected Mayor</i> × <i>Election/Appointment Year</i>	0.070** (0.035)	0.073** (0.035)	0.074** (0.035)
<i>Elected Mayor</i>	0.017 (0.048)	0.019 (0.048)	0.016 (0.048)
<i>Election/Appointment Year</i>	−0.026 (0.028)	−0.029 (0.028)	−0.030 (0.028)
Year fixed effects	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes
County-specific time trends	No	Yes	Yes
Control variables	No	No	Yes
Observations	12,209	12,209	12,209
Municipalities	421	421	421
R^2	0.123	0.139	0.144

The dependent variable is the log of investment transfers per capita. Standard errors in parentheses are robust to heteroskedasticity and clustered at the municipality level. Control variables include share of population over 65 years, share of population below 15 years, log of population size, log of rule-based transfers per capita, and business and property tax rate multipliers. Significant at *10%, **5%, and ***1%.

but has not led to higher grant receipts in general. The 7% to 7.4% difference amounts to about 42,300 to 44,700 euros for an average Hessian municipality with about 10,000 inhabitants.³⁴ This absolute increase is not overwhelming, but it is sufficiently large to for voters notice. Various additional projects can be realized with such amounts.³⁵

C. Electoral Incentives and Selection

The estimates reported in section VB suggest that while transfers do not differ significantly in nonelection/appointment years, they differ significantly in election/appointment years. While these results are consistent with the political agency framework, it remains unclear whether the cycle after the switch to direct elections emerges because of incentive or selection effects.

As discussed in section IVA, I pursue two strategies to explore this question. First, I report in table 4 regressions where I control for observable mayor characteristics as specified in equation (3). With this specification, I analyze whether the inclusion of mayor characteristics as covariates affects the size of the coefficient for the interaction term that captures differences in the electoral cycles. The data on mayor characteristics cover 398 of the 421 Hessian municipalities (i.e., the sample size is slightly smaller than in the baseline as it was not possible to collect data on mayor characteristics for the full sample of mayors).³⁶

³⁴ Average investment transfers per capita during the sample period were 60.39 euros. Hence, a 7% to 7.4% increase in transfers equals an absolute increase of 4.23 to 4.47 euros per capita.

³⁵ Examples: refurbishment of public day care facilities in Gross-Gerau (28,000 euros) and Biebertal (17,000 euros), city renewal measures in Grossenlöhder (19,000 euros). See <http://www.ruesselsheimer-echo.de/lokales/ruesselsheim/28-000-Euro-fuer-Kita;art57641,1544693>, <https://wirtschaft.hessen.de/presse/pressemitteilung/17000-euro-fuer-kita-bieber>, and <http://osthessen-news.de/n1121/kreis-fulda-336-000-euro-f-r-k-nftige-stadterneuerung-an-vier-kommunen.html>.

³⁶ A problem with the drop in sample size may be that this attrition is systematically related to the timing of the switch in a municipality. I have explored this issue and find that attrition is not concentrated in a particular cohort.

TABLE 4.—SELECTION VERSUS ELECTORAL INCENTIVES: MAYOR CHARACTERISTICS INCLUDED

	I	II	III	IV	V
<i>Elected Mayor</i> × <i>Election/Appointment Year</i>	0.064 (0.041)	0.064 (0.041)	0.061 (0.041)	0.061 (0.041)	0.065 (0.041)
<i>Elected Mayor</i>	0.007 (0.051)	0.005 (0.051)	0.005 (0.051)	0.006 (0.051)	0.009 (0.051)
<i>Election/Appointment Year</i>	−0.016 (0.036)	−0.017 (0.037)	−0.016 (0.037)	−0.016 (0.037)	−0.019 (0.037)
Education dummies	No	Yes	Yes	Yes	Yes
Occupation dummies	No	No	Yes	Yes	Yes
Female dummy and age	No	No	No	Yes	Yes
Party affiliation dummy	No	No	No	No	Yes
Observations	9,926	9,926	9,926	9,926	9,926
Municipalities	398	398	398	398	398
R^2	0.118	0.119	0.121	0.121	0.122

The dependent variable is the log of investment transfers per capita. Standard errors in parentheses are robust to heteroskedasticity and clustered at the municipality level. All estimations include year and municipality fixed effects, county-specific time trends, and control variables (share of population over 65 years, share of population below 15 years, log of population size, log of rule-based transfers per capita, and business and property tax rate multipliers). Table A.4 in online appendix A is a more extensive version of this table and reports coefficients and standard errors for each mayor characteristic.

TABLE 5.—SELECTION VERSUS ELECTORAL INCENTIVES: SUBSET OF MAYORS BOTH APPOINTED AND ELECTED DURING THEIR CAREER

	I	II	III
<i>Elected Mayor</i> × <i>Election/Appointment Year</i>	0.063 (0.068)	0.076 (0.067)	0.073 (0.067)
<i>Elected Mayor</i>	0.046 (0.098)	0.043 (0.098)	0.060 (0.094)
<i>Election/Appointment Year</i>	0.022 (0.051)	0.018 (0.050)	0.016 (0.050)
Year fixed effects	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes
County-specific time trends	No	Yes	Yes
Control variables	No	No	Yes
Observations	2,537	2,537	2,537
Municipalities	124	124	124
R^2	0.184	0.211	0.221

The dependent variable is the log of investment transfers per capita. Standard errors in parentheses are robust to heteroskedasticity and clustered at the municipality level. Control variables include share of population over 65 years, share of population below 15 years, log of population size, log of rule-based transfers per capita, and business and property tax rate multipliers.

I first rerun the baseline regression without any covariates but with the smaller sample as a benchmark. The results are collected in model I of table 4. The estimate in this model is 6.4%, slightly smaller than the baseline estimate of 7.0% to 7.4%, and insignificant. Models II to V successively add the education level, the professional background, the gender and age, and the party affiliation of mayors as covariates. The coefficients remain at the same order of magnitude as in model I. While the drop in the number of observations is problematic insofar as the coefficient becomes insignificant (p -value = 0.12), the results in table 4 overall provide suggestive evidence that the cycle in grant receipts for elected mayors is due to electoral incentives rather than selection.

The second strategy to discriminate between selection and incentive effects is to restrict the sample to mayors who were facing both an election and appointment while in office. This reduced sample covers 124 of 421 municipalities. By considering this subsample, I test whether the same person has responded differently to an imminent appointment or election. The estimates reported in table 5 are not significant given the smaller sample and the consequently larger

standard errors. Yet the coefficients have about the same magnitude as in the baseline estimations (7.3% instead of 7.4% in model III). These results provide additional suggestive evidence that the cycle in investment grants observed in the baseline regressions is more likely to be due to higher electoral incentives of elected mayors rather than changes in their characteristics.

VI. Robustness and Validation

In the following, I explore the robustness of the baseline results and validate further that the cycle observed in the baseline regressions is primarily due to larger electoral incentives.³⁷

A. Timing of the Election

As discussed in section IIIC, mayors typically submit the application and, if successful, receive the investment grant in the same year. Yet if an election is held early in the year, mayors likely have smaller incentives to submit an application as a grant may not be approved in time to help them win the election. The estimates in table 3 are thus possibly a lower bound: investment grant receipts in an election year may be higher when the election is held later in the year.

Models I to III in table 6 explore this issue. I redefine the election dummy (and consequently also the interaction with the dummy for direct elections) such that it is 0 if the election is held in January or February (model I), between January and April (model II), and between January and June (model III). Thus, I define only years where the election was held relatively late as election years (and treat election years where the election was held early as nonelection years).

The coefficient estimates in models I to III confirm that the baseline results are likely lower bounds. The estimates are positive, statistically significant, and of a larger magnitude

³⁷ I report four additional robustness tests (exclusion of nonpositive transfers, inclusion of lagged dependent variable, inclusion of county-specific time fixed effects, and placebo tests with other fiscal outcomes) in online appendix B.

TABLE 6.—TIMING OF ELECTIONS

	I	II	III	IV
	Only late elections			
	March–December	May–December	July–December	Preelection year
<i>Elected Mayor</i> × <i>Election/Appointment Year</i> – 1				0.029 (0.039)
<i>Elected Mayor</i> × <i>Election/Appointment Year</i>	0.087** (0.036)	0.093** (0.040)	0.108** (0.044)	0.079** (0.037)
<i>Elected Mayor</i>	0.008 (0.048)	–0.004 (0.048)	0.003 (0.050)	0.024 (0.050)
<i>Election/Appointment Year</i> – 1				–0.013 (0.032)
<i>Election/Appointment Year</i>	–0.033 (0.028)	–0.035 (0.030)	–0.040 (0.031)	–0.032 (0.030)
Year fixed effects	Yes	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes	Yes
County-specific time trends	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes
Observations	12,209	12,209	12,209	12,209
Municipalities	421	421	421	421
R ²	0.144	0.144	0.144	0.144

The dependent variable is the log of investment transfers per capita. Standard errors in parentheses are robust to heteroskedasticity and clustered at the municipality level. Control variables include share of population over 65 years, share of population below 15 years, log of population size, log of rule-based transfers per capita, and business and property tax rate multipliers. Significant at *10%, **5%, and ***1%.

than in the baseline regressions. Furthermore, the magnitude of the coefficients is consistently larger the later the considered elections are in the year. For elections held between July and December, I estimate an increase in grants of about 11%.

If an election is held early in the year, it is plausible that mayors are already trying to acquire additional grants in the preelection or appointment year. Not accounting for this may also lead me to underestimate the importance of the cycle. I hence investigate the timing of grant receipts further by exploring whether there are significant differences in electoral cycles across the two regimes one year ahead of an election or appointment. I define the dummy variable *Election/appointment year* – 1 and interact it with the *Elected Mayor* dummy.

Model IV of table 6 collects the estimation results. The estimates suggest that one year ahead of a mayor election, there are no significant differences in grant receipts compared to one year ahead of a mayor appointment. This generally suggests that elected mayors increase their efforts in attracting grants as close as possible to the election day. Successfully attracting investment grants in a preelection year may in fact be of little use for a mayor even for elections that are held early in the next year. Since the grants are awarded on a first-come, first-serve basis, waiting until the last few months of a preelection year likely decreases the chances that the grant will be approved in time (see section IIIC). On the other hand, successfully attracting grants too early in a preelection year may not make a difference in the election if voters have a short memory.

B. Anticipation Effects

One further consideration is that the last cohort of appointed mayors may have had either particularly small or particularly large incentives to perform well and to signal

their competence to voters before the first direct elections. On the one hand, the last cohort of appointed mayors may have expected to be ousted from office by voters no matter what because voters prefer a different type of mayor (e.g., a more charismatic type). Anticipating that no amount of investment grants would convince voters to retain them in office, they may have ceased to exert any effort to attract grants. I would underestimate the effect of the switch to direct elections on investment grant receipts if this type of anticipation effect is not accounted for (as these mayors were still in office when the first direct elections took place).

On the other hand, the last cohort of appointed mayors may have believed that there was some chance that they would be elected even if voters in principle would prefer a different type of mayor than party officials would. In this case, the last cohort of appointed mayors may have exerted even more effort to attract additional grants before the first direct elections to overcome their perceived disadvantage due to the change in the selection rule.

I explore this theoretically ambiguous issue in table 7 by distinguishing between the first mayor elections after the switch and all subsequent elections. I find that the increase in investment transfers is larger in the first than in subsequent election years (10.1% versus 6.5% in model III), suggesting that anticipation effects may have especially (but not exclusively) led the last cohort of appointed mayors to exert additional effort in the election year. These results further underline that electoral incentives matter for mayors' policy choices.

C. Divided Government

Another important extension to consider is the issue of divided government—the possibility that the council majority and the mayor belong to different ideological camps.

TABLE 7.—ANTICIPATION EFFECTS: FIRST VERSUS SUBSEQUENT ELECTIONS

	I	II	III
<i>Elected Mayor</i> × <i>Election Year</i> _{first elections}	0.097* (0.057)	0.099* (0.057)	0.101* (0.057)
<i>Elected Mayor</i> × <i>Election/</i> <i>Appointment Year</i> _{subsequent elections}	0.061 (0.038)	0.063* (0.038)	0.065* (0.038)
<i>Elected Mayor</i>	0.003 (0.059)	0.005 (0.059)	0.003 (0.059)
<i>Election/Appointment Year</i>	−0.025 (0.028)	−0.029 (0.028)	−0.029 (0.028)
Year fixed effects	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes
County-specific time trends	No	Yes	Yes
Control variables	No	No	Yes
Observations	12,209	12,209	12,209
Municipalities	421	421	421
R ²	0.123	0.139	0.144

The dependent variable is the log of investment transfers per capita. Standard errors are robust to heteroskedasticity and clustered at the municipality level (reported in parentheses). Control variables include share of population over 65 years, share of population below 15 years, log of population size, log of rule-based transfers per capita, and business and property tax rate multipliers. Significant at *10%, **5%, and ***1%.

Appointed mayors typically have by default the support of the council. While divided government was generally possible even before the switch (because council elections took place every four years and mayors were appointed every six years), any misalignment between the mayor's political camp and the council majority was temporary. After the introduction of mayor elections, divided government became both more likely and more persistent since the mayor could remain in office without the support of the council majority as long as she was reelected by the voters.

Existing evidence indicates that electoral accountability is lower under divided government (Lowry, Alt, & Ferree, 1998), presumably because voters find it difficult to accurately attribute responsibility for policy choices to the various branches of the government. In line with this, it is possible that elected mayors have electoral incentives to attract higher transfers in election years only when there is no divided government. The reason is that elected mayors may anticipate that a substantial fraction of voters attributes any failure to attract additional grants to the opposition council majority and mayors may therefore exert less effort. Alternatively, elected mayors may be concerned that an opposition council majority will claim some of the credit for additional grants.

Figure A.2 in online appendix A shows how the share of municipalities with divided governments has evolved over the sample period. As expected, there has been a discontinuous increase in the share of divided governments in 1993 from about 40% to more than 55%.³⁸ In this figure, divided government is defined as a misalignment between the political camp of the mayor and the political camp of the council majority. I consider three political camps: a left-wing camp consisting of the SPD, and Green Party, a right-wing camp consisting of the CDU and the FDP, and an independent

³⁸ Note that I do not know the party affiliation of all mayors, especially at the beginning of the sample period. I drop observations with missing information when constructing the figure.

camp consisting of all other parties.³⁹ A municipality is considered to be subject to divided government if a left-wing mayor faces a council with no left-wing majority, a right-wing mayor faces a council with no right-wing majority, and an independent mayor faces a council with a nonindependent majority.

As a next step, I investigate how the increased propensity for divided government matters for grant receipts. The hypothesis is that the switch to mayor elections led to larger electoral cycles only in municipalities that were not subject to divided government. I explore this hypothesis by estimating the baseline model for subsamples restricted to municipality-year pairs with and without divided governments, respectively. In models I to III in table 8, I reestimate the baseline models for the nondivided government subsample. I find a significant 12% increase in investment grants in election years, which is substantially larger than the 7% to 7.4% increase in the baseline regressions. In models IV to VI, I replicate the baseline regressions for the divided government subsample and find that the switch to direct elections did not lead to electoral cycles in this subset of municipalities. Overall, these additional results show that the strength of electoral incentives has a decisive influence on how the reform of the mayor selection rule affects investment grant receipts.

VII. Conclusion

A prominent theoretical literature on political agency suggests that the way in which public officials are selected matters for their policy choices. My study is the first to identify the causal effect of the selection method on mayors' policy choices and to differentiate between incentive and selection effects. In particular, I use the unique setting provided through a reform of mayor selection in a German state to investigate whether policy choices differ when mayors are appointed by a local council or directly elected by voters.

I find that elected mayors attract on average 7% to 7.4% more investment grants from the state tier in election years. Robustness tests indicate that this estimate is likely a lower bound. For appointed mayors, there is no cycle. I provide suggestive evidence that this cycle emerges because of stronger electoral incentives.

In view of my results, should mayors be elected or appointed? On the one hand, my results may imply that mayors should be elected. First, the finding that elected mayors exert more effort than appointed mayors to attract investment transfers in election years suggests that there may be some benefit for their municipalities. Second, the switch to

³⁹ It is reasonable to focus on political blocs rather than parties. First, almost all left-wing mayors are from the SPD, while almost all right-wing mayors are from the CDU. Second, while there are no explicit coalitions at the local level, it is plausible that the parties within the ideological camps cooperate when necessary. Note also that the figures on the incidence of divided government and the regression results are similar when I define divided governments based on individual parties rather than party blocs.

TABLE 8.—NONDIVIDED VERSUS DIVIDED GOVERNMENT

	I	II	III	IV	V	VI
	Nondivided Government			Divided Government		
<i>Elected Mayor</i> × <i>Election/Appointment Year</i>	0.125** (0.056)	0.122** (0.055)	0.122** (0.054)	−0.067 (0.065)	−0.062 (0.064)	−0.063 (0.063)
<i>Elected Mayor</i>	−0.038 (0.078)	−0.043 (0.080)	−0.070 (0.079)	0.038 (0.065)	0.048 (0.064)	0.055 (0.065)
<i>Election/Appointment Year</i>	−0.030 (0.045)	−0.030 (0.043)	−0.027 (0.043)	0.077 (0.061)	0.073 (0.059)	0.073 (0.059)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
County-specific time trends	No	Yes	Yes	No	Yes	Yes
Control variables	No	No	Yes	No	No	Yes
Observations	4,820	4,820	4,820	5,590	5,590	5,590
Municipalities	316	316	316	350	350	350
R ²	0.112	0.132	0.145	0.098	0.110	0.114

The dependent variable is the log of investment transfers per capita. Standard errors in parentheses are robust to heteroskedasticity and clustered at the municipality level. Control variables include share of population over 65 years, share of population below 15 years, log of population size, log of rule-based transfers per capita, and business and property tax rate multipliers. Significant at *10%, **5%, and ***1%.

direct elections, while distorting the timing of effort, may have improved the match between the investment needs of municipalities and transfer receipts. It is possible that elected mayors in municipalities that are most in need of investment grants attract more transfers because they expect particularly large electoral rewards.

On the other hand, my results may imply that mayors should be appointed. First, I find no significant differences in transfer receipts over the entire term, which suggests that elections do not cause elected mayors to exert more effort in general. One could argue that such a distortion in the timing of grant receipts may diminish welfare because citizens would prefer a steady flow of investment projects. Second, even if elections led to more grants in general (and not only in election years), it is not necessarily the case that writing grant applications is the best use of mayors’ time. Mayors could pursue other activities that would possibly be more beneficial but less visible to voters. Electoral incentives may thus lead public officials to reallocate their efforts from productive but invisible to less productive but visible tasks, and it is not clear that such a shift is necessarily welfare improving.

While the welfare implications are ambiguous in my context, the findings in this paper confirm that selection rules affect public officials’ incentives and policy choices. That elections ostensibly lead mayors to exert more effort suggests that direct elections may increase voters’ welfare provided that the local constitution is appropriately designed. Mayor elections are more likely to have positive welfare implications if there is a clear link between the policy domains over which mayors have authority and voters’ welfare.

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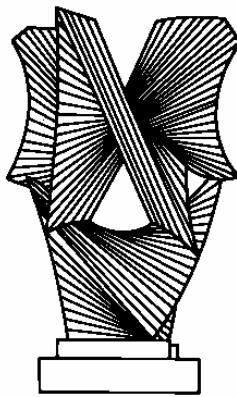
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Professionals or Politicians: The Uncertain Empirical Case for an Elected Rather than Appointed Judiciary

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August 21, 2007

Abstract. Although federal judges are appointed with life tenure, most state judges are elected for short terms. Conventional wisdom holds that appointed judges are superior to elected judges because appointed judges are less vulnerable to political pressure. However, there is little empirical evidence for this view. Using a dataset of state high court opinions, we construct objective measures for three aspects of judicial performance: effort, skill and independence. The measures permit a test of the relationship between performance and the four primary methods of state high court judge selection: partisan election, non-partisan election, merit plan, and appointment. The empirical results do not show appointed judges performing at a higher level than their elected counterparts. Appointed judges write higher quality opinions than elected judges do, but elected judges write many more opinions, and the evidence suggests that the large quantity difference makes up for the small quality difference. In addition, elected judges do not appear less independent than appointed judges. The results suggest that elected judges are more focused on providing service to the voters (that is, they behave like politicians), whereas appointed judges are more focused on their long-term legacy as creators of precedent (that is, they behave like professionals).

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“If the state has a problem with judicial impartiality, it is largely one the state brought upon itself by continuing the practice of popularly electing judges”.

Justice O’Connor, concurring in *Republican Party of Minn. v. White*, 536 U.S. 765, 792 (2002).

1. Introduction

Justice O’Connor’s backhanded put-down of Minnesota’s elected judiciary reflects the conventional wisdom among lawyers and scholars that judges should be appointed by elected officials or independent commissions, and should not be elected themselves (Geyh 2003; Tarr 2003). The conventional wisdom reflects a deeply rooted conviction that voters are too unsophisticated to evaluate judges and candidates for judicial office. When judges use campaign contributions to finance simple-minded television commercials, conflict of interest is layered on public confusion. However, this conviction is hardly self-evident. In a system that uses judicial appointments, nothing forces the appointing official to select judges on the basis of their legal ability; cronyism is very common. And, as the literature on voting shows, ordinary people use various strategies for evaluating candidates whose qualifications they do not fully understand. For example, they rely on party endorsements and newspaper editorials, and the give-and-take of the campaign.² And when many people participate in a decisionmaking process, aggregation of information occurs, which can produce more accurate results than when the decision is made by only one person.

The relative merits of appointment and electoral systems are an empirical question, but what exactly should be tested? Most empirical work on this topic focuses on judicial independence, the willingness of a judge to vote against the ideological interests of the party of the elected official who appointed her or of the party to which she belongs.

² Newspaper endorsements of judicial candidates are common.

However, independence captures only a part of the judge's role. Judges are supposed to be independent but not to be arbitrary: a judge who votes against her party may still make bad decisions. And an independent judge who is lazy will not resolve many cases, or will resolve them poorly. The measures of independence that have been used in the literature imply that the best judicial system would be one in which Democratic judges voted in favor of Republican interests and Republican judges voted in favor of Democratic interests. It is as though empirical studies of central banks focused exclusively on whether central banks made decisions that contradicted the expressed desires of the government, and not on whether their decisions were correct, as a matter of monetary policy. Central bank independence is important but a central bank that always decided the opposite of what the government sought would not necessarily be a good one. The same can be said about judges.

To test the conventional wisdom that appointed judges are better than elected judges, we use a tripartite definition of judicial quality—productivity, opinion-quality, and independence. Productivity refers to the number of opinions a judge writes in a particular time period such as a year. The more opinions a judge writes, the more disputes she has resolved—and dispute resolution is the chief function of the judge. Opinion-quality refers to the opinion's reasoning. Better-reasoned opinions explain to the parties why they won or lost, but much more important, they provide guidance to future judges who face similar cases, and to people and businesses who want to avoid litigation in the first place. And independence refers to the willingness of judges to follow the law rather than the interests of political parties.

The independent variable of interest is the selection method for high court judges. In 12 states, judges are appointed by governors (or, in few instances, legislatures). At the opposite extreme, judges in 9 states run for election—and reelection—as members of political parties. In between, there are two systems that combine partisan and non-partisan elements. In 16 states, merit commissions are used: typically, an independent commission provides nominees whom the governor may appoint, while a retention election is used at the end of a judge’s term (rather than a competitive election). In 13 states, non-partisan elections are held: the public votes but judges are not permitted to advertise themselves as members of particular political parties.

A small empirical literature has investigated the relationship between selection systems and judicial characteristics in the states. As noted above, the literature has focused on judicial independence and not other attributes of quality (Cann 2007; McLeod 2007; Shepherd 2007), and suggests that appointed judges are more independent than elected judges. Our tests of independence produce more complicated results and does not favor either system in a clear way. As for overall quality, again the literature assumes that appointed judges are better – albeit, with little attempt to measure quality (Cann 2007).³ We find that elected judges are more productive. And although appointed judges write opinions that are cited more often, the difference is small and outweighed by the productivity difference. In other words, in a given time period, the product of opinion number and citations-per-opinion is higher for elected judges than for appointed judges.

³ There are a handful of empirical studies that do attempt to construct quality measures so as to evaluate judicial performance. These studies use either surveys or measures of the educational qualifications of the judges as their dependent variables (Cann 2007; Glick and Emmert 1987; Canon 1972). Surveys may reflect the biases of the respondents as we discuss in the text. As for educational and professional qualifications, those look to be more appropriate as an independent variable rather than as a measure of judging quality. (As we discuss below, there are interesting differences overlooked in the literature: elected judges went to worse law schools but have stronger local ties than appointed judges do.)

After discussing our results, we attempt an explanation for why elected judges might differ in these ways from appointed judges. We argue that elected judges tend to be politicians while appointed judges tend to be professionals. Professionals care about their reputation among a national community of like-minded professionals, while politicians care about their reputation in the local community of lay voters and politicians. Appointed judges thus labor to write opinions that will be admired, while elected judges try to satisfy as many litigants as possible by dispensing quick but adequate justice. Although our evidence does not prove that elected judges are superior to appointed judges, it casts doubt on the conventional wisdom, and broadens the scholarly debate.

2. Theory

2.1 The Determinants of Judicial Quality

Beginning with the legal realists, scholars and even judges themselves have speculated about the motives of judges, and whether judges decide cases by applying the law in a neutral fashion or in a manner that reflects personal or political views, or both (e.g., Cross 2005; Newman 1984). Because American judges have the power to strike down laws, the early controversy about judges' motives led to a debate about the proper role of judges in the constitutional system. If judges are ideologically motivated, then their power to strike down laws sits uneasily with democratic commitments; if they are not, or if their ideological motives are constrained, then judicial review has many

attractive properties, including possibly the ability to prevent majorities from exploiting minorities or otherwise supporting bad law.⁴

In recent years, this debate has reemerged in the framework of an agency model, which treats the judiciary or individual judges as agents, and the public or particular elected officials as the principals (e.g., Maskin and Tirole 2004). Agency models warn that agents, unless properly selected, monitored, and rewarded, will not act in the interests of principals. In the context of the judiciary, political institutions need to be designed to ensure that people with preferences similar to that of the public are selected to be judges, and that judges be given the right incentives to decide cases according to the law.

With respect to selection, judges should be impartial and competent, rather than partisan, ideological, eccentric, or incompetent. What selection mechanism will ensure that good, rather than bad, people are selected to be judges and that they will remain good after they become judges? Elections might ensure that people with mainstream views become judges, but the electorate may not be able to evaluate a potential judge's legal ability and other technical qualifications, let alone whether she is misleading the electorate in terms of her future fidelity to what is best for the populace. Appointments might result in competent and politically mainstream judges; however, elected officials might prefer to use judgeships as patronage positions. Unhappiness with these two extremes has led, in many states, to reliance on commissions, combinations of appointments and elections, and other complex mechanisms.

⁴ The vast literature cannot be described here. Holmes (1988) traces the history of this debate. Bickel (1986) is the source of the modern debate in constitutional law. Croley (1995) brings the debate to bear on state courts.

With respect to incentives, judges should, in principle, face sanctions if they decide cases poorly and rewards if they decide cases well. However, if rewards and sanctions are used, someone must apply them, and if that person has political power, judges might be afraid to decide cases impartially. The federal system avoids this risk by giving judges lifetime tenure on good behavior, but the danger with such a system is that it allows judges to decide cases badly or in a partisan fashion, without fear of sanction. Most state systems attempt to constrain judges by forcing them to undergo reelection or a reappointment process, but the danger is that judges will decide cases in partisan fashion so as to avoid a partisan sanction.

Some history illustrates the tensions (Hanssen 2004). Legislatures dominated the judicial selection and retention process between 1790 and 1847 on account of colonial era suspicion of the executive and the judiciary, both of which were arms of the British; however, some states retained gubernatorial appointment systems. Corruption among the legislatures and the spread of Jacksonian democracy stimulated the growth of electoral judiciaries beginning around 1850. Progressive-era distrust of electorates led to adoption of nonpartisan judicial elections at the turn of the century, and then the rise of merit plans. Merit plans generally created commissions that appointed judges, and used retention elections to discipline them. Though ideological currents clearly played a role in this history, the changes can easily be put in the agency framework, as Hanssen (2004) depicts. Reliance on partisan mechanisms reflected fears about the incentives of governors and then legislators to select and reappoint the proper sort of judge; the shift toward nonpartisan mechanisms reflected concerns about the ability of the public to

evaluate judges.⁵ Today, most scholars reject electoral systems (e.g., Fordham Symposium 2007).

The empirical literature on judicial behavior has focused primarily on federal judges, and especially on the U.S. Supreme Court (George 1998). One vein of this literature establishes that judges' voting behavior reflects partisan or ideological preferences, at least to some extent. Judges who are Republicans or who are supported, at time of appointment, by conservative media, tend to vote in an ideologically conservative way; a corresponding bias characterizes Democrats (Segal and Spaeth 2002). This work confirms that unconstrained judges do not necessarily decide cases impartially and casts doubt on the assumption that appointment systems are necessarily best. Another vein in the literature shows that structural and institutional features influence and constrain judges' incentives to vote once on the bench. For example, perhaps because of whistleblower or group polarization effects, federal judges vote less ideologically when the panel is split by party than when it is not (Cross and Tiller 1998, Sunstein et al. 2004).

A smaller empirical literature on the state courts has come to similar results. Brace and various coauthors find correlations between voting and ideology, analogous to the federal court studies (e.g., Brace et al., 2001, Brace et al. 2006). Tabarrok and Helland (1999) find that tort awards are higher in electoral states than in non-electoral states. They argue that their results reflect the stronger incentives of elected judges to redistribute wealth from out-of-state corporations to in-state voters and to please the local trial bar (see also Helland and Tabarrok 2002). Hanssen (1999) finds more litigation in non-electoral states than in electoral states. He argues that high court judges in non-

⁵ Berkowitz and Clay (2006) find that legislative preferences with respect to judicial independence may be connected to the type of legal system (civil or common law) of the nation that originally settled the areas that became states.

electoral states have more independence, and therefore are under less pressure to decide cases consistently. Greater uncertainty about the law generates more litigation. Hanssen (2000) finds that state bureaucracies are larger in non-electoral states than in electoral states, which he attributes to defensive efforts by the agencies to protect themselves from less politically constrained judges. Pinello (1995) finds that appointed judges are more likely to favor criminal defendants than elected judges are. Besley and Payne (2006) find that employment discrimination claims are more numerous in electoral states than in non-electoral states, which they argue shows that elected judges are more likely to rule in favor of employees than appointed judges are. Berkowitz and Clay (2006) find that the quality of state courts, as measured by surveys of senior attorneys at wealthy companies, is positively correlated with nonpartisan judicial retention procedures. Shepherd (2007) focuses on the political party of “retention agents”—those people who decide whether a judge will be retained or not—and finds that judges (of whatever party) are more likely to vote in favor of traditional Republican interests when retention agents are Republicans, and are more likely to vote in favor of traditional Democratic interests when retention agents are Democrats. The effect is larger when the retention process is electoral than when it involves reappointment. An early study by Landes and Posner (1980) finds that citations (including federal and out-of-state citations) of state supreme court opinions are uncorrelated with selection system.

The literature, taken as a whole, provides evidence that selection and retention institutions influence judicial outcomes—by influencing who becomes a judge, or how judges decide cases, or both. The literature also confirms that judges are influenced by political factors. The literature largely skirts our topic—whether elected judges are,

overall, better or worse than appointed judges—but offers tantalizing hints. The Tabarrok and Helland studies imply that elected judges are better agents of their constituents than are appointed judges; the social problem they identify is due to the federal structure of the country and the overlapping jurisdictions of states. Hanssen's (1999) finding that electoral pressures force state judges to be more consistent implies that elected judges are better—more consistent opinions, all else equal, are better than inconsistent opinions. Berkowitz and Clay (2006) do use a measure of overall judicial quality based on the views of senior attorneys at wealthy companies, but, as they acknowledge (pp. 412–13), the views of these senior attorneys are hardly unbiased. Landes and Posner's (1980) study is closest to ours, but they do not look at productivity and independence, and their study has several methodological limitations.⁶ Our results are quite different from theirs.

Following the literature, we envision the relationship between the public and the judiciary as an agency relationship. The optimal selection mechanism minimizes agency costs. Judges expend unobservable effort to decide cases. Agency costs can take two forms: laziness (resulting in bad opinions or few opinions) and ideological self-indulgence (biased opinions). The optimal selection mechanism screens out judges with a strong preference for laziness or ideological self-indulgence and/or punishes judges who are observably lazy or ideologically self-indulgent.

⁶ We use a larger sample, different variable definitions, and many more control variables. Their study is mainly about the federal courts. And their data come from a different era, before the modern concern about excessive political competition among state court justices fueled by campaign donations (Carrington 1998).

2.1 Judicial Quality and Judicial Selection Mechanisms

2.1.1 Judicial Quality

We use three measures of judicial quality.

Productivity. Judges have some discretion over how many opinions they write. Judges who write slowly will write fewer opinions. Judges who are lazy and wish to avoid writing concurrences and dissents, will also write fewer opinions. We measure a judge's productivity using the total number of opinions she writes during our sample time period (1998-2000), including majority, concurring, and dissenting opinions.

Productivity is a measure of effort, but is also a measure of quality because, all else equal, more opinions settle a greater number of legal disputes and resolve more legal issues. However, not all else is equal. A judge who writes more opinions might devote less time to each opinion, so that quality suffers. So productivity is at best only a partial measure of quality.

Citations. We assume, consistently with the rapidly expanding literature on judicial citations, that citations are a measure of quality (e.g., Baker 2007; Cross and Lindquist 2006; Lott 2005; Landes, Lessig and Solimine 1998). Better opinions are cited more frequently than worse opinions. An opinion is cited frequently because it resolves a legal question or identifies some new legal problem or represents an advance in the law or simply clarifies doctrine. We focus on out-of-state citations because this allows us to control for the possibility that in-state citations reflect local legal customs or conventions.

To check for robustness, we further subdivide citations into federal, state, and law review citations.⁷

Independence. Judges have the duty to enforce the law impartially, without regard to the legally irrelevant characteristics of the litigants or the goals of political parties. We thus assume that better judges are more independent. Some studies (e.g., Shepherd 2007) measure independence by the propensity of a judge to vote against interests associated with her party—for example, a Republican judge voting in favor of an employee and against an employer. This measure of independence focuses exclusively on the vote in the case and does not take into account the direction of the opinion’s reasoning. It would, for example, code an opinion that decides a particular case against an employee, but creates precedent through its reasoning that would assist later employee suits, as Republican. And then there is the question of whether coding a vote against an employee and for the employer, regardless of the facts of the case, gets at the Republican/Democrat distinction at all. To avoid the foregoing pitfalls, we look directly at when judges decide to write opinions against judges of the same or opposite party. We posit that a judge who writes several dissents against majority opinions authored by judges of the same party (or majority opinions against dissents of same party judges) is more independent than a judge who rarely dissents and never against a judge of the same party.

Our measure of independence is imperfect, and we address its problems and alternatives subsequently. For now, we note two problems. First, the measure can be distorted by personal animosities. Personal animosities might cause judges to refuse to join opinions as often as they otherwise would (Choi and Gulati 2004). Second, there

⁷ The problems with citations studies have been rehearsed elsewhere and we will not repeat them here (for example, on the possibility of bias, see Abramowicz and Tiller 2005; Bhattacharya and Smyth 2004; Choi and Gulati 2007).

might be judges who are excessively partisan. These excessively partisan judges, because they view their co-partisan colleagues as not partisan enough, may end up dissenting a lot against the moderate judges from their own party. Such behavior – extremely partisan behavior – may then be interpreted as independence.

2.1.2 Selection Mechanisms

State judicial selection mechanisms can be divided in several ways. The literature has not arrived at a consistent methodology, and our approach differs from those of other researchers. Nonetheless, the approaches are roughly consistent, and we test ours for robustness. We divide judicial selection mechanisms into four categories (Table 1).

<<Insert Table 1 About Here>>

Governor or Legislative Appointment. In 12 states, judges are appointed by the governor or (in South Carolina and Virginia) the legislature. Gubernatorial appointments usually require the consent of the upper house of the legislature or the participation of a special commission such as an executive council. In most of these states, judges serve a term (ranging from 6 to 14 years) and then may be reappointed in the same manner. In Massachusetts, New Hampshire, and Rhode Island, judges enjoy lifetime or near-lifetime tenure.

Merit Plan. In 16 states, judges are nominated by a nonpartisan commission, and then appointed by the governor. Judges serve a term and then are subject to a retention election, where they run alone, and voters can either approve another term or vote against them. Terms vary but on the whole are less than those in appointment states.

Nonpartisan Election. In 13 states, judges run for election. Their political affiliations are not listed on the ballot, and so voters, unless specifically informed, do not know a candidate's political party. These judges serve a term and then may run for reelection. The terms range from 6 to 10 years.

Partisan Election. In 9 states, judges run for election as a member of a political party. They serve a term in the range of 6 to 10 years for the most part and then may run for reelection.

Readers might be skeptical about whether voters care much about judicial elections and use the elections as an opportunity to reward good judges and punish bad judges. Hall (2001) finds that only 8.3 percent of state supreme court judges seeking reelection between 1980–94 were defeated. Hall, nonetheless, reports a great deal of variation across time and selection systems. In partisan elections, judges during this period were defeated 18.8 percent of the time; in 1994, 36 percent of them were defeated. And judges' electoral success appears to hinge on their ideological similarity to voters. "The fact of the matter ... is that supreme court justices face competition that is, by two of three measures, equivalent if not higher to that for the U.S House" (Hall 2001, p. 319). (For further discussion, see Dudley 1997, Aspin 1999, and Geyh 2003).

We should note that each state has a unique system; the categorizations suppress a great deal of variation. For example, the governor of Massachusetts appoints nominees of a judicial selection commission while the governor of Maine makes appointments subject to confirmation by the Senate. Massachusetts judges serve until the age of 70; Maine judges have seven year terms, at the end of which they may be reappointed by the governor, again subject to Senate confirmation. These differences have led to different

coding practices in the literature, with some authors focusing on retention (e.g., Shepherd 2007) and others on selection procedures (e.g., Besley and Payne 2006); some using only two categories, others using multiple categories, and so forth.

Our categorization is similar to that of the other authors. The main concern is that if selection is relatively nonpartisan, and retention is relatively partisan, our selection variable will be misleading. Fortunately, it appears that the degree of partisanship tends to be the same at selection and retention decisions, and indeed tenure length is negatively correlated with the partisanship of the selection process (see Table 5, below).

2.2 Hypothesis

Conventional wisdom is that appointed judges are better than elected judges. If so, appointed judges should have higher productivity, citation numbers, and independence. The existing empirical literature provides some support for the hypothesis that appointed judges are more independent. Our analysis adds an analysis of productivity and citation numbers in addition to a new measure of independence to get at the underlying question of whether appointed judges are of higher quality than elected judges.

3. Data Description

3.1 The Dataset

We examine the decisions of all the judges of the highest court of every state for the years 1998, 1999, and 2000. The District of Columbia is excluded because of its anomalous position. Two states—Texas and Oklahoma—have two highest courts, one

with jurisdiction over civil appeals and the other with jurisdiction over criminal appeals. We, in effect, treat these courts as separate state courts: so we have 52 “states.”

Each court has a certain number of seats, but we count judges, rather than seats, so if turnover occurs, a court will have more judges than seats; and, if some seats are left empty, there could be fewer judges than seats. Our dataset contains 408 judges, about 8 per court. The average judge spent 2.65 of the 3 years in our sample period on the court. And each judge wrote on average about 67 opinions per year.

We organize the data in three ways. For productivity, we run judge-year level regressions. Each observation is a judge for a particular year; there are 1082 observations—that is, the product of 408 and 2.65. For citations, we run opinion-level regressions to allow us to assess the factors that lead to citations to specific majority opinions. There are 27,596 majority opinion observations in our dataset. For independence, we run judge-level regressions on data pooled from 1998 to 2000, and hence there are 408 observations. We assume that a judge’s independence does not change over the 1998 to 2000 time period and use pooled data to obtain as large a sample as possible of opposing opinions with which to construct our independence measure. In many of our regressions, the actual number of observations is lower as a result of inadequate data for variables of interest.

3.2 Measures of Judicial Quality

3.2.1 Productivity

Productivity is measured by total number of opinions written for any given year, including dissents and concurrences (Total Opinions). Our least productive judge wrote

two opinions in one year,⁸ while our most productive judge wrote 263 opinions in one year. The mean was 67 opinions per year. Table 2 provides productivity data, arranged by type of selection system.

<<Insert Table 2 About Here>>

A pattern is evident. Judges in *more* partisan systems are more productive than judges in less partisan systems. The difference between the mean level of Total Opinions for Election Partisan compared with Appointed judges is significant at the <1% level. Judges in more partisan systems also write greater numbers of dissents than judges in less partisan systems—an issue to which we will return. The difference between the mean Dissenting Opinions for Election Partisan compared with Appointed is significant at the <1% level.

3.2.2 Opinion Quality

Our primary quality variable is the number of out-of-state citations to a particular opinion by a particular judge (Outside State Citations). We also look at narrower measures—such as law review citations and outside federal court citations.

The best measure of quality is citation by an outside court—including another state court or outside federal court. Inside state and home federal court citations are driven to large extent by precedent. Looking at only outside citations allows us to examine those citations where judges have greater discretion to pick which opinions to cite. For the most part, judges are citing these outside opinions because they are helpful, not because the opinion has precedential force. Because of this discretion, an outside

⁸ Probably because the judge left office early in the year, entered office late in the year, was sick during the year, or had administrative duties.

circuit citation represents a better indication of which opinions judges deem of higher quality. Table 3 provides citation data, categorized by selection system.

<<Insert Table 3 About Here>>

The citation pattern reported in Table 3 is strong. Appointed and other judges subject to less partisan pressures have higher per-opinion outside citation levels than both non-partisan and partisan elected judges. The differences in the mean levels of Outside State Citations for non-partisan and partisan election judges compared with appointed judges are significant at the <1% level. This pattern carries through for law review citations as well (where the differences are again significant at the <1% level).

3.2.3 Independence

Our independence measure focuses on the tendency of judges to write opinions that disagree with co-partisans when the pool of judges provides opportunities to do so.⁹ We define an “opposing opinion” as either a majority opinion when a dissent exists, or a dissent when a majority exists. We assume that a judge exhibits independence when she writes an opposing opinion against a co-partisan.

We obtain a measure of the political party for each judge in our sample. We looked to three sources of information on party membership. First, we searched NEXIS and the Internet (using Google) for any news reports on the political affiliation of the each judge. Second, we also searched for information on political contributions at the opensecrets.org website. We used the political party of the donee candidate as a proxy for the political party of judges who contributed. Third, we used the party of the governor (if

⁹ The variable is defined as follows: $indep = (demopratio - opdistratio) * (republican = 1) + (repopratio - opdistratio) * (democrat = 1)$, where *demopratio* is the fraction of majority opinions in the state written by a Democrat (and similarly for *repopratio*), and *opdistratio* is the fraction of opposing opinions written by the judge in question against a Democrat.

any) who appointed the judge as a proxy for the judge's political party. In most of the cases where we had multiple sources of information on political party, the party was consistent across these sources. When our three sources reported different parties, we gave first priority to the party identified through our NEXIS and Internet searches and second to the party identified in the openSecrets.org database.

We define `Opposite_Party` as the number of opposing opinions written, by the judge of interest, against a judge of the opposite party divided by the number of opposing opinions written against a judge of either party from 1998 to 2000. Thus, this variable measures propensity to side with co-partisans. Not all opposing opinions are driven by the ideology of the opposing judges. A judge who dissents at random would dissent 70 percent of the time against an opposite party judge if the background pool of majority opinions consisted of 70 percent opposite party authored opinions. To take into account the background pool of opinions, we define `Opposite_Pool` as the total number of majority opinions authored by an opposite party judge divided by the total number of majority opinions authored by either an opposite or same party judge (not including the judge in question) from 1998 to 2000.

We define `Independence` as `Opposite_Pool` minus `Opposite_Party`. A more negative `Independence` score corresponds to a judge who writes opposing opinions against opposite party judges more frequently than the background pool of majority opinions authored by opposite party judges. Conversely, a more positive `Independence` score corresponds to an authoring judge who writes opposing opinions less frequently against opposite party judges compared with the background pool of opinions (and thus

more frequently against co-partisans). We treat a more positive Independence score as indicative of a more independent judge.¹⁰

Table 4 reports summary statistics on our Independence measure. Only judges for whom we could identify a political party were included in the analysis, and only judges from states that had a mixture of judges from different political parties were included.

<<Insert Table 4 About Here>>

Panel A of Table 4 reports the mean Independence level for judges associated with the different selection systems for high court judges. Note that unlike our productivity measures discussed above, Independence is based on pooled data from 1998–2000. We do not expect the Independence of a judge to vary by year, and pooling the data enables us to increase the amount of data used to generate the Independence measure for any particular judge. From Panel A, note that the mean Independence level was negative for judges associated with all four different selection systems. Judges tend to write opposing opinions more against opposite party judges relative to the background pool of majority opinions

Judges subject to partisan election have the highest independence. Judges subject to non-partisan election have the lowest independence. However, the differences between the mean Independence scores for non-partisan election judges and merit selection and appointed judges are not statistically significant. Only the difference between the mean

¹⁰ Consider, for example, a Republican judge sitting on a high court in a state where the other judges are split 50-50 between Republican and Democrat judges and the pool of majority opinions written by other judges corresponds to this 50-50 split. Suppose our Republican judge authors 10 dissents and 20 majority opinions where there is a dissenting opinion. And suppose that 5 of the 10 dissents are authored against a Democrat judge and 15 of the 20 majority opinions face a dissent from a Democrat judge. In this case, $\text{Opposite\_Party} = (5+15)/(10+20) = 2/3$. $\text{Opposite\_Pool} = 1/2$. Independence would then equal $1/2 - 2/3 = -1/6$. Because of the tendency of our Republican judge to write an opposing opinion more frequently against Democrats compared with the background pool of majority opinions, the Republican judge receives a negative Independence score.

Independence score for non-partisan election and partisan election judges is significant (at the 10 percent confidence level only), which is a bit puzzling.

The bottom line is that the data is consistent with the findings in the empirical literature that judicial opinions reflect partisan or ideological bias. However, the data does not support the received wisdom that appointed judges are more independent than electoral judges.

This partial result could reflect problems with our Independence measure. There are two main problems. First, consider the extreme case where all judges on a particular state high court are all of the same political party (say all Republican). In this case, our Independence measure will equal zero since `Opposite_Party` will equal `Opposite_Pool` (and both will equal zero since there are no Democrat-authored opinions). Our analysis in Table 4 excludes judges who come from states with no variation in political party among judges for this reason. But, by the same token, we lose data.

Second, even where all judges are not of the same political party in a state, if an imbalance exists, the range of the Independence variable will vary. Consider two Republican judges. One is in a state with 90 percent of the majority opinions written by Democrats and the other is in a state with 10 percent of the majority opinions written by Democrats. For the first, Independence can range from -0.1 to $+0.9$. For the second, Independence can range from -0.9 to $+0.1$. So the second judge could have a much more negative Independence score than the first judge simply because the range is shifted over.

To address these problems, we try a number of different approaches.

First, we create a version of the Independence variable that is less dependent on the background political makeup of a particular state court. `Independence_Indicator` is

defined as 1 if Independence is greater or equal to zero and 0 otherwise. The indicator variable addresses the range problem but also throws out information: it suggests judges subject to non-partisan elections are less independent than the other types, who are about the same. The last column of Panel A of Table 4 reports on the mean of Independence_Indicator. None of the differences in mean Independence_Indicator levels among the varying selection systems are statistically significant.

Second, we consider the possibility that the act of writing a dissenting opinion (even against opposite-partisan judges) can be a greater display of independence than writing a majority opinion (even against co-partisan dissenters). Judges who write dissents display independence in their willingness to write critically of the opinions of their colleagues. Thus, we treat dissenting activity as a possible proxy for independence. However, as Panel B of Table 4 shows, no significant differences exist in the Independence score of active dissenters compared with occasional dissenters for judges of any of the four selection systems.

Third, we consider the possibility that the different systems might have different effects on judges of varying political preferences. The bottom of Panel B shows that judges who make political contributions (Open Secrets = 1) tend to be less independent for the more partisan selection systems compared with judges who did not make any political contributions (Open Secrets = 0).

The bottom line is that appointed judges are not, overall, more independent than judges in partisan elected systems; they might even be less independent. We do find that, among judges who contribute to political campaigns, appointed judges are more

independent, but it is not clear that this signifies greater overall independence for the appointment system.

4. Multivariate Tests

We estimate a number of multivariate regression models to assess the relationship of several key independent variables, including the judicial selection mechanism, and our measures of judicial quality (used as the dependent variables in our models). We also include a number of common control variables in our models.

4.1 Selection Mechanisms

Our main interest is the relationship between the selection mechanism used by states to select and retain judges and our dependent variable measures of judicial productivity, quality, and independence. Table 1 describes the selection systems for the different states. In all the states, the selection mechanism long predates our data pool, and thus we need not worry about endogeneity—that states adopted new mechanisms in response to changes in judicial quality. Electoral systems can be traced back to the Jacksonian era. As we mentioned above, the switch to alternative systems generally occurred during the Progressive Era, and the process was more or less complete by the 1970s. More recent changes, with the exception of South Carolina, have been marginal (Hanssen 1999, Besley and Payne 2006, table 1).

To check for robustness, we also test the importance of judicial tenure data—the average tenure (meaning actual service, not *de jure* term length) of high court judges by state, as of the spring of 1997 (from Hanssen 1999, tble. 1) (Tenure). Note that the Tenure data does not come from our data set: we do not have information on the tenure of

the judges, many of whom are still in office. We provide summary statistics on the relationship between Tenure and the different judge selection mechanisms in Table 5.

<<Insert Table 5 About Here>>

Note from Table 5 that mean tenure rises as partisanship falls. We have two clusters—high partisanship and low tenure, and low partisanship and high tenure.

The advantage of the Tenure variable is that it allows us to compare the different selection mechanisms along a common metric: the length of time that a judge expects to remain in office. Only a few appointment states have lifetime tenure, so even the relatively apolitical mechanisms permit judges to be “punished”: elected officials or commissions unhappy with the performance of judges can refuse to reappoint them or support their reappointment. Thus, tenure indicates the vulnerability of the judge to later retention decisions more directly than do the selection mechanisms. Judges with longer tenure face less retention pressure while judges with shorter tenure face greater retention pressure. Tenure allows us to measure the real effects of the retention mechanisms while allowing us to rely less on de jure rules that might be evaded in local legal practice.

However, problems exist with the Tenure variable. If a judge does a bad job from the perspective of the decisionmaker for the retention decision, the judge may be sanctioned, in which case she ends up with a short tenure. Judges in equilibrium may respond to the threat of a frequent retention decision by catering to the preferences of the retention decisionmaker. Thus a judge with a long tenure could represent either a judge who rarely faces a retention decision or, alternatively, a judge who does face frequent retention decisions but who in equilibrium is adept at satisfying the preferences of the retention decisionmaker. The strong correlation between Tenure and state selection

systems that provide for longer (if not lifetime) tenure leads us to discount this latter possibility. Because of these potential problems with Tenure, we use Tenure only as a robustness check of our selection system results.

4.2 Control Variables

Our multivariate models of judicial productivity, quality, and independence include a common set of control variables. First, we include state level controls that attempt to capture differences among states that might account for judicial performance. Many of our state level controls relate to the state court system. We include measures for the average high court associate justice salary (Adjusted Associate Justice Salary) and the average partner salary in the state (Adjusted Partner Salary). The salary variables are adjusted for the cost of living for the metro area in which the high court is located in the state. Higher-paying states may attract higher-quality judges. Perhaps judges work harder if their salary will decline (or not rise much) if they are not retained. We include an indicator variable for whether the judges on the high court remained the same throughout our sample time period from 1998 to 2000 (Stable Court) and the size of the bench during the 1998 to 2000 period (Number of Active Judges on Bench). As a measure of resources available to high court judges, we include the average number of clerks per judge for the 1998 to 2000 period (Number of Clerks Per Judge) and an indicator variable for whether the clerks are tenured for at least one year (Long-Term Clerk). Judges may also act differently if facing a high workload, particularly if an intermediate appellate level court does not exist to help with the workload. We include the log of the number of trial cases in the state measured in 1998 ($\ln(\text{Number of Trial Cases in the State})$) and an indicator

variable for the presence of an intermediate appellate court (Intermediate Appellate Court).¹¹ The variables for the number of clerks and clerk tenure, the size of the bench, the number of trial cases, and the existence of an intermediate appellate court may all influence a judge's choice to devote time to any specific case.

Our state-level controls also include variables relating to the general characteristics of each state. Differences in overall state population ($\ln(\text{Population})$), gross state product ($\ln(\text{Gross State Product})$), and crime rates (Crime Rate) may lead to different mixes of cases and judicial responses to these cases. Because previous research suggests that state judges are influenced by judges in neighboring states (Harris 1985), and because larger neighboring states might produce different types of cases, we include a variable for the aggregate population of border states ($\ln(\text{Border Population})$). We also include a measure of the age of the state (State Age). Older states have longer judicial traditions and hence possibly a more sophisticated jurisprudence on which modern judges can draw. State Age controls for the possibility that modern judges are cited more often outside of the state just because they can draw on the older and more sophisticated jurisprudence of their particular state. Finally, we include a variable for citizen ideology based on election results in each district (from Berry et al. 1998) (Citizen Ideology Score). The background ideology of the citizens of a state may affect the preferences of high court judges.

¹¹ We also ran the regressions with a control variable for the ratio of cases resulting from mandatory appeal to the total number of cases (mandatory and discretionary), to try to control for the possibility that a court's ability to control its docket affects productivity and other dependent variables (Brace and Hall 1990). Another possible approach would have been to code for de jure rules governing appeals but these turn out to be extremely complicated and we could not determine an objective way to code them. We do not report the first regressions because of lack of confidence in the appropriateness of this control variable; in any event, it did not affect the statistical significance or (except trivially) the size of the coefficients for our main results, with a single small exception: in the independence regressions, the interaction of dissents and partisan elected judges loses its statistical significance.

Second, the multivariate models include judge level controls. We include an indicator variable for whether the judge was the chief judge of the high court (Chief Judge). A judge who is chief judge may have less time to author opinions. The chief judge may also command greater respect and receive greater numbers of citations as a result for her opinions. Alternatively, the chief may be able to assign herself the more important opinions and garner more citations that way (Langer 2003). We include the number of years between 1998 and the year in which the judge received her law degree (Post Law-School Experience) and the number of years the judge has been on the high court (Court Experience). More experienced judges may decide opinions with greater skill, leading to more citations. A variable for whether a judge retired in 2001 or earlier captures the possibility of end game problems (Retirement Close)—judges who retire soon have little to lose from deciding badly.

4.3 Productivity

Our first multivariate model focuses on judicial productivity. We estimate the following equation on judge year-level data using an ordinary least squares regression with robust standard errors:

$$\begin{aligned} \ln(\text{Total\_Opinions})_i = & \alpha + \beta_{1i}\text{Election\_Non-Partisan} + \beta_{2i}\text{Election\_Partisan} \\ & + \beta_{3i}\text{Merit\_Plan} + \sum \beta_{ji}\text{State Controls}_{ji} + \sum \beta_{ki}\text{Judge Controls}_{ki} \\ & + \text{Year Effects} + \varepsilon_i \end{aligned}$$

Our model relates the log of the total number of opinions authored by a judge in any given year to three indicator variables for Non-Partisan Election, Partisan Election, and Merit Plan states. The three indicator variables use Appointed states as the baseline. The regression model includes year-level State Controls and Judge Controls described

above (see Appendix for definitions). The model also includes year fixed effects. Table 6 reports results.

<<Insert Table 6 About Here>>

The results in Model 1 of Table 6 contradict the hypothesis that judges subject to more partisan pressure are less productive; the opposite is the case. The coefficients on Election Non-Partisan, Election Partisan, and Merit Plan are all positive and significant at the <1% level. Partisan-elected judges are the most productive, followed by merit-plan and non-partisan judges. Appointed judges (the omitted, baseline case) are the least productive.

Our control variables largely make sense and are interesting in their own right. More highly paid judges are more productive, taking into account the opportunity cost of forgoing a practicing lawyer's salary—though the magnitude is small. Judges on a stable court are more productive, probably because collegial norms are stronger on stable courts. Chief judges are less productive, perhaps because they have administrative duties. Judges with more judicial and legal experience are more productive; but judges become less productive as they approach retirement. Judges from states with a large population are more productive because they have more cases to decide, and, because of the greater activity and diversity of the population, more law to make. The presence of an intermediate appellate court, the number of trial cases in the state, and crime variables are either statistically insignificant or small.¹²

¹² As a robustness test, we re-estimate Model 1 of Table 6 using the log of the total number of majority opinions in any given year and, alternatively, the log of the total pages written for all opinions in any given year as dependent variables. Unreported, we obtain the same qualitative results as in Model 1 in both robustness models. Judges from Election Non-Partisan, Election Partisan, and Merit Plan states are more productive than Appointed judges (whether measured by total majority opinions or total pages written). We also re-estimate Model 1 with the addition of an indicator variable set equal to 1 if the judge was in an election year and 0 otherwise (Election Year). Unreported, the re-estimated model provides the same

We replace the three indicator variables for the state judge selection system with Tenure, our alternative measure of the pressure facing judges, focusing on the retention decision. Model 2 reports that the coefficient on Tenure is negative and significant at the <1% level. Judges who face frequent retention decisions (and presumably experience a shorter expected tenure), are significantly more productive than judges who have the luxury of a longer expected tenure period. These results are consistent with the Model 1 results.

Why are judges subject to electoral pressures more productive? Perhaps productivity is used by voters as a signal of judicial competence, or by intermediaries, such as newspaper editorialists, bar associations, and parties. Judges who write few opinions, then, will be vulnerable in reelection campaigns.¹³ This would also explain why productivity declines as retirement nears (Retirement Close), as reported in both Models 1 and 2.¹⁴

If elected judges respond to electoral pressures through increased productivity, one might predict that productivity would increase with political competition. To test this hypothesis, we use a standard measure in the political science literature. This measure takes a higher value when elected officials are more frequently replaced. To Models 1

qualitative results as in Model 1; the coefficient on Election Year is negative (indicating decreased productivity) but not statistically significant.

¹³ Anecdotal evidence supports the hypothesis that elected judges feel pressure to be productive. Productivity is mentioned in newspaper endorsements and judicial evaluation materials from time to time. A clear example can be found in the reelection campaign materials of a Texas intermediate appellate court judge, which include a table with productivity statistics for a group of judges:

For the fiscal year ending August 31, 2006, Third Court of Appeals Justice Bob Pemberton ranked #1 statewide among Texas' court of appeals judges in production of original appellate opinions on the merits. These results show that Justice Pemberton is the most productive appeals judge in Texas for original opinions and the Third Court of Appeals is the most productive of Texas' 14 courts of appeals.

http://www.bobpemberton.com/2006/09/20/appeals_opinion_productivity/.

¹⁴ We also ran the regressions using total pages written by a judge for a specific year as the dependent variable; the results were nearly the same, in most cases with somewhat higher coefficients and higher levels of statistical significance.

and 2 of Table 6 we add an indicator variable, High Political Competition, for states where the level of political competition is greater than the median level. We also add interaction terms between High Political Competition and the three indicator variables for the state judge selection system (Model 1) and Tenure (Model 2). Table 7 reports our results.

<<Insert Table 7 About Here>>

Model 1 of Table 7 reports that the coefficient on High Political Competition is positive and significant at the <1% level. In contrast, the coefficients on the interaction terms between High Political Competition and Election Non-Partisan and Election Partisan are negative and significant at the <1% level. Moreover, the sums of High Political Competition + Election Non-Partisan x High Political Competition and High Political Competition + Election Partisan x High Political Competition are both negative and significant at the 5% and <1% levels respectively. While judges from Election Non-Partisan and Election Partisan states are generally more productive, the amount of increased productivity diminishes in states with a large amount of political competition (but still remains positive compared with judges from Appointed states).¹⁵ Model 2 of Table 7, in contrast, reports no significant relationship between the level of productivity and High Political Competition interacted with Tenure. These results are puzzling, and suggest that the political competition measure is either not accurate or (more likely) does not reflect the pressures on judges per se.

¹⁵ The sums of Election Non-Partisan + High Political Competition + Election Non-Partisan x High Political Competition and Election Partisan + High Political Competition + Election Partisan x High Political Competition are both positive and significant at the <1% levels.

4.4 Citations

For outside citations, we estimate the following equation for each majority opinion using ordinary least squares and robust standard errors:

$$\begin{aligned} \ln(\text{Outside State Citations}_i) = & \alpha + \beta_{1i}\text{Election\_Non-Partisan} + \beta_{2i}\text{Election\_Partisan} \\ & + \beta_{3i}\text{Merit\_Plan} + \beta_{4i}\text{Number Dissents} + \beta_{5i}\text{West Key Pages} \\ & + \beta_{6i}\text{Opinion Length} + \sum \beta_{ji}\text{Subject Matter}_{ji} \\ & + \sum \beta_{ki}\text{State Controls}_{ki} + \sum \beta_{li}\text{Judge Controls}_{li} \\ & + \text{Year Effects} + \varepsilon_i \end{aligned}$$

The model relates the number of outside state citations (Outside State Citations) for any specific majority opinion with three indicator variables for Election Non-Partisan, Election Partisan, and Merit Plan states. The three indicator variables use Appointed states as the baseline. The model includes the number of dissents written against the majority opinion in question (Number of Dissents). A majority opinion with one or more dissents may deal with more novel issues of law and generate more citations as a result. The model includes the number of west key pages (West Key Pages) as a rough measure of the legal importance of the opinion. Similarly the model includes the length of the opinion (Opinion Length); longer opinions are more likely to contain analysis that other judges may cite compared with shorter opinions, all other things being equal. We also include subject matter fixed effects for twelve different subject matter categories, including Administrative, Attorney and Client, Capital Punishment, Church and State, Commercial, Criminal, Family, First Amendment, Labor, Property, Rights, and Torts. We use Other opinions as the baseline subject matter category.

We include both State Controls and Judge Controls, as described above, in the model. For the judge controls, instead of the Retirement Close indicator variable, we

include a series of indicator variables for whether retirement occurred prior to 2000 or in 2001, 2002, 2003, 2004, or 2005. Because the number of outside citations may drop after a judge retires, we use these indicator variables to capture more precisely when a judge retires. We also use year fixed effects. Table 8 reports the results from our multivariate outside citation model.

<<Insert Table 8 About Here>>

Our results are consistent with the hypothesis that judges subject to less partisan pressure write higher quality—more frequently cited—opinions. Model 1 reports that the coefficients on Election Non-Partisan, Election Partisan, and Merit Plan are negative and significant at the <1%, 5%, and <1% level respectively. Model 1 is consistent with the view that appointed judges write the best opinions. Also, majority opinions with dissents are cited more frequently, perhaps because the majority opinion makes new law. This reason might also explain why longer opinions are cited more often. Finally, commercial, torts, family law, and rights opinions stand out as those that are cited most frequently out of state; administrative law and capital punishment decisions are cited the least out of state. We do not have an explanation for this pattern.

Why would appointed judges write better opinions than elected judges? One possibility is that the quality of an opinion, unlike the number of opinions written, is not observable to the public, so elected judges do not have a strong incentive to write high-quality opinions. With less pressure to produce, appointed judges might prefer to advance their influence and professional reputation by writing good opinions. Another possibility

is that a system that selects for judges skilled at electioneering and politicking does not also necessarily select for judges skilled at authoring high quality legal opinions.¹⁶

To test the importance of high levels of in-state political competition, we add to Model 1 the High Political Competition variable as well as interaction terms between High Political Competition and the three indicator variables for the state judge selection system. Model 2 reports that the coefficient on Election Non-Partisan x High Political Competition is positive and significant at the <1% level. Greater political competition in-state correlates with increased quality opinions for Election Non-Partisan judges. This result is somewhat puzzling. On the other hand, the sum of Election Non-Partisan + High Political Competition + Election Non-Partisan x High Political Competition is not significantly different from zero. As well, the interaction term between High Political Competition and Election Partisan is not significantly different from zero—indicating that judges from Election Partisan states, unlike for Election Non-Partisan states, do not respond to increased political competition with higher quality opinions.

We replace the three indicator variables for the state judge selection system with Tenure, our alternative measure of the pressure facing judges, focusing on the retention decision. Model 3 reports that the coefficient on Tenure is positive but not significantly different from zero. On the other hand, the coefficient on High Political Competition is positive and significant at the 5% level. Greater levels of political competition correlate

¹⁶ As a robustness test, we re-estimate Model 1 of Table 8 using the log of one plus the number of law review citations to a majority opinion ($\ln(1+\text{Law Review Citations})$) as an alternative measure of opinion quality. Not reported, we obtain the same qualitative results as in Model 1. Judges from Election Partisan, Election Non-Partisan, and Merit Plan states produce opinions that are cited less by law reviews than judges from Appointed states. Moreover, the biggest drop off in the level of law review citations is for Election Non-Partisan opinions. We also re-estimate Model 1 with the addition of the Election Year indicator variable. Unreported, the re-estimated model provides the same qualitative results as in Model 1; the coefficient on Election Year is negative (indicating decreased opinion quality in an election year) but not statistically significant.

with higher opinion quality. Perhaps because judges in election states where there is more political competition do not compete as much based on productivity (but instead compete along other dimensions), they have more time to craft higher quality opinions.

4.5 Independence

4.5.1 Propensity to Dissent

To assess what factors correlate with a high propensity to write dissenting opinions, we estimate the following equation on judge-year level data using ordinary least squares and robust standard errors:

$$\begin{aligned} \ln(1+\text{Number Dissenting Opinions})_i = & \alpha + \beta_{1i}\text{Election\_Non-Partisan} \\ & + \beta_{2i}\text{Election\_Partisan} + \beta_{3i}\text{Merit\_Plan} + \sum \beta_{ji}\text{State Controls}_{ji} \\ & + \sum \beta_{ki}\text{Judge Controls}_{ki} + \text{Year Effects} + \varepsilon_i \end{aligned}$$

The regression model relates the number of dissenting opinions written in a given year ($\ln(1+\text{Number Dissenting Opinions})$) with three indicator variables for Non-Partisan Election, Partisan Election, and Merit Plan states. The indicator variables use Appointed states as the baseline. The model includes year-level versions of the state controls and judge controls as well as year fixed effects. Table 9 reports the model.

<<Insert Table 9 About Here>>

Model 1 of Table 9 indicates that elected judges, especially partisan-elected judges, dissent the most, with merit-plan judges in the middle. Appointed judges write the fewest dissenting opinions. If dissenting indicates independence, than elected judges are more independent than appointed judges are.

Model 1 of Table 9 also reports, however, that the presence of high political competition among elected officials in the state (High Political Competition) reduces the propensity to dissent for the judges in elected judge states. Note that the coefficients on the interaction terms between High Political Competition and Election Non-Partisan and Election Partisan are both negative and significant at the 10% and <1% levels respectively. The behavior of judges with respect to writing dissents in Election states is not independent of politics. Judges in states with a large amount of political competition, for example, may choose not to write as many dissents to avoid providing opponents with political targets. Such judges may instead use dissents without opinions to display their party loyalty while keeping their political profile low. By contrast, appointed judges are not affected by political competition at a statistically significant level.

Model 2 of Table 9 reports the results of our robustness test using Tenure as an alternative specification of how judges differ across the states with a focus on the retention decision. Note that the coefficient on Tenure in Model 2 is negative and significant at the <1% level. Judges who face the possibility of a short tenure (as in elected states) write more dissents. Similarly, the coefficient on High Political Competition is negative and significant at the <1% level and the coefficient on the interaction term between Tenure and High Political Competition is positive and significant at the <1% level (indicating that judges who face more frequent retention decisions are more affected by High Political Competition to reduce the number of dissenting opinions). More political competition in the state might cause judges who otherwise would compete based on writing dissenting opinions to focus on other aspects of competition.

As a robustness test, we re-estimate Models 1 and 2 of Table 9 with the addition of the Election Year indicator variable. Unreported, the re-estimated model provides the same qualitative results as in Models 1 and 2; the coefficients on Election Year are negative (indicating decreased productivity) but not statistically significant.

If dissenting indicates a willingness to express one's honest view, then our results suggest that elected judges are more likely to do this when political competition is low, while appointed judges are less deterred from dissenting when political competition is high.¹⁷ But overall, elected judges are more willing to dissent than appointed judges are, so if dissenting signifies independence, then elected judges are more independent than appointed judges are.

4.5.2 Propensity to Write Opinions Against Co-Partisans (Independence)

Given the shortcomings of focusing solely on the number of dissenting opinions as a measure of independence, we turn to our Independence measure. We estimate the following equation on pooled data from 1998 to 2000 using an ordinary least squares regression model with robust standard errors:

$$\begin{aligned} \text{Independence}_i = & \alpha + \beta_{1i}\text{Election\_Non-Partisan} + \beta_{2i}\text{Election\_Partisan} \\ & + \beta_{3i}\text{Merit\_Plan} + \sum \beta_{ji}\text{Subject Matter}_{ji} \\ & + \sum \beta_{ki}\text{State Controls}_{ki} + \sum \beta_{li}\text{Judge Controls}_{li} + \varepsilon_i \end{aligned}$$

The regression model relates our Independence measure (Independence) based on pooled 1998 to 2000 data for each judge with three indicator variables for Non-Partisan Election, Partisan Election, and Merit Plan states. The three variables use Appointed

¹⁷ We also find a positive correlation between dissent activity and the size of the bench (that is, the number of judges participating in cases during our period), which is consistent with Lindquist (2007), who finds the same result using federal appellate courts).

states as the baseline. The model includes pooled data version of the state controls and judge controls as described above. As a control for the subject matter composition of the pool of opinions, for each of the 12 subject matter categories (see Appendix for definitions) we compute the number of majority opinions that deal with the specific subject matter divided by the total number of majority opinions for the state in the 1998 to 2000 time period. We include this ratio for each subject matter in the model as pooled controls for subject matter in the state. Table 10 reports the results of our model.

<<Insert Table 10 About Here>>

Model 1 of Table 10 reports that none of the coefficients on the three indicator variables for the state judge selection systems are significantly different from zero. The coefficient on Number of Dissenting Opinions is, however, positive and significant at the 5% level. Those judges who write more dissents are also more independent under our Independence measure. In contrast, the coefficient on the interaction term between Number of Dissenting Opinions and Election Partisan is negative and significant at the <1% level. While judges who write more dissenting opinions generally receive a higher Independence score, this relationship does not hold true for judges in Election Partisan states.

We posit that more ideologically intense judges will tend to act less independently when faced with the possibility of an election. To test this, we add the Open Secrets measure of political contribution as a proxy for political intensity to Model 1. We also add interaction terms between Open Secrets and the three indicator variables for the state selection systems. Model 2 reports that the interaction terms between Open Secrets and Election Non-Partisan and Election Partisan are both negative (indicating less

independence) and significant at the 5% and <1% levels respectively. While more intensely political judges do not correlate significantly with a decreased independence score for Appointed and Merit Plan judges, we do see a negative relationship between political intensity and independence for the elected judges.

We lastly posit that elected judges from states with a high level of political competition will act differently than appointed judges with similarly high levels of political competition. We add the High Political Competition indicator variable to Model 2 as well as interaction terms between High Political Contribution and the three indicator variables for the state judge selection system and report the results in Model 3 of Table 10. From Model 3 note that the coefficient on the interaction term between Partisan Election and High Political Competition is positive and significant at the 5% level while the coefficients on all the other High Political Competition variables are insignificant. While Partisan Election state judges do display a difference in behavior depending on the underlying state political environment (in a way that Appointed and Merit Plan judges do not), we are not sure of why the level of Independence *increases* for Partisan Election judges in High Political Competition states. Perhaps elected judges in High Political Competition states compete other than through their opinions (and thus act less partisan in the decisions themselves).¹⁸

¹⁸ As a robustness test, we re-estimate Model 3 using Independence_Indicator as the dependent variable, set equal to 1 if Independence is greater or equal to 0 and set equal to 0 otherwise. Unreported, we obtain qualitatively similar results with some differences. The coefficient on Election Non-Partisan is now positive and significant at the 5% level, indicating the judges from Election Non-Partisan states are generally more independent under this measure than Appointed judges. On the other hand, the interaction terms between Election Non-Partisan and the Number of Dissenting Opinions and Open Secrets remain negative (and are significant at the 10% level), indicating that elected judges who write more dissents and are politically more active are less independent. Moreover, the coefficients on the High Political Competition variables are now all insignificant.

Our results are complicated and difficult to summarize but our overall sense is that elected judges are more sensitive to political competition than appointed judges are (suggesting that elected judges are less independent) but that elected judges are more likely to dissent (suggesting more independence), and that the two types of judges are otherwise roughly equally like to write against co-partisans (suggesting equal independence). Strongly partisan judges act in a more partisan way in electoral systems than in appointment systems, but it is not clear why this matters if independence for overall judicial activity is the same. Perhaps the clearest conclusion is that the conventional wisdom that appointed judges are more independent than elected judges is a simplification and probably an exaggeration.

4.6 Productivity Versus Quality

Given the ambiguity of our independence results, what's more important: productivity or quality? As a measure of the overall influence of a judge's opinions, we calculated the aggregate number of outside citations to the opinions written by each judge in one year (Aggregate Outside Citations). The average judge in a partisan election system in one year writes opinions cited (outside of the state and home federal circuit) in the aggregate 11.3 times, whereas the average judge in an appointment system writes opinions cited in the aggregate 15.0 times (difference significant at the <1% level). The numbers for non-partisan election systems and merit selection systems are 11.3 and 14.0, respectively. At a summary statistic level, the influence of appointed judges is not significantly diminished relative to elected judges despite the lower productivity on the part of appointed judges.

To provide a multivariate test controlling for state and judge characteristics, we re-estimate our judge productivity model (Model 1 of Table 6) on judge-year level data, replacing $\ln(\text{Total Opinions})$ with $\ln(\text{Aggregate Outside Citations})$ as the dependent variable. Unreported, the coefficients on Election Partisan, Election Non-Partisan and Merit are all positive and significant at <1%, 5%, and 10% respectively, using Appointed as the base case. Judges from partisan election states in particular correlate with significantly higher levels of aggregate outside citations compared with appointed judges. In contrast with the summary statistic comparison, the multivariate test is consistent with the view that the lack of productivity on the part of appointed judges diminishes the overall influence and quality of their total judicial output of opinions.

We note parenthetically that the quality-quantity tradeoff—that judges apparently write more opinions that are of lower quality, or fewer opinions of higher quality—is itself of intrinsic interest, and seems highly plausible, yet has not been noticed in the literature.

5. Explaining the Results

We summarize our results in Table 11.

<<Insert Table 11 About Here>>

Elected judges write more opinions and while their average cite counts per opinion are lower than those for appointed judges, they garner more citations overall. From the average litigant's perspective, elected judges provide more justice to more people—if one takes the receipt of a written explanation for the court's decision as a

component of justice. Appointed judges write fewer opinions, but those that are written tend to be higher quality opinions—they garner more citations. Fewer litigants are receiving this high quality justice though. Judges in the different systems exhibit similar levels of independence. What explains this?

One explanation draws on differences in incentives to write opinions. The multitasking model (Holmstrom and Milgrom 1991), shows that if an agent is given two objectives (say, quality and productivity), and the activities that further only one of those objectives can be measured and monitored by the principal, then the agent will shirk on the hard-to-measure objective and invest in achieving the other.

Judges in more partisan systems may write more opinions because raw productivity is observable. Judges who write few opinions, for example, can be easily criticized and, as we have seen, some judges use productivity as a campaign issue. Quality is hard to observe. So, judges who are concerned about retention will shirk on quality, while judges with security have no such incentive. Absent political pressure, the latter set of judges might concern themselves more with their long-term reputation among judicial colleagues and lawyers.

As for independence, the propensity to vote with or against co-partisans is also hard to observe. If so, elected judges would have no more incentive to refrain from acting independently than appointed judges do. So it is, in the end, not surprising that the independence levels of elected and appointed judges are not clearly different.

A problem with the multitasking theory is our finding that elected judges write fewer opinions as political competition increases. Perhaps they need to spend more time

campaigning or forging political connections. In any event, they must think that the productivity loss does not hurt them, or not much.

But this problem also suggests that another interpretation of the data might be superior. This brings us to selection. It might be that the different systems attract different types of people to judgeships: selection matters more than incentives to behave once in office. In particular, electoral systems would seem to attract *politicians*, while appointment systems are more likely to attract *professionals*. Politicians want to satisfy the voting public, and this might mean deciding cases expeditiously and in great number. Professionals are more concerned about their reputation among other lawyers and judges, and are more interested in delivering well-crafted opinions that these others will admire.

To test this possibility, we compare judges from different selection systems. We chose characteristics that might explain the differences in judicial behavior.

<<Insert Table 12 About Here>>

The most striking differences are as follows. Compared with Appointed judges, Elected judges make more campaign contributions; are paid less; are on less stable benches; have shorter tenures; are more likely to have gone to a law school in the state in which they sit; and are more likely to have gone to a lower-rank law school. They are, in short, more politically involved, more locally connected, more temporary, and less well-educated than appointed judges. They are more like politicians and less like professionals. Elected judges also have less court experience, but the difference with Appointed judges is not statistically significant.

One might think that politicians would be more concerned about party identification, and hence would be more likely to vote together than professionals are—

but this is not the case, as our independence measures show. One simple explanation for our results is that for most cases, one's vote has no political salience. The vast majority of cases are unanimous. It is more important even for politician-judges to decide many cases correctly than to agree with their co-partisans. Political identification matters as a proxy for how one's view affects how one evaluates a case. That is why judges do tend to vote with co-partisans. But this general tendency does not produce different independence outcomes in electoral and appointment systems because independence for all but a tiny number of cases is unobservable (and therefore not a focus of judges under either type of selection system).

The selection argument is more appealing than the incentives/multitasking argument because so much of judicial behavior is hard to observe. Judges may well believe that, as long as they avoid extremes of behavior, they can count on being reelected or reappointed. Ordinary people elect hard-working locals, while governors appoint reliable professionals who will not embarrass them.

In sum, a simple explanation for our results is that electoral judgeships attract and reward politically savvy people, while appointed judgeships attract more professionally able people. However, the politically savvy people might give the public what it wants—adequate rather than great opinions, issued in greater quantity and therefore (given the time constraint) average speed.

6. Conclusion

We began this project with the assumption that the data would demonstrate that appointed judges are better than elected judges. Our results persuade us that the story is

more complicated. It may be that elected judges are, indeed, superior to appointed judges. At a minimum, the conventional wisdom needs to be reexamined.

A full comparative evaluation of the systems would require more research. In particular, there has been much concern in recent years about the rise in the cost of election campaigns for supreme court justices, and some evidence that supreme court justices are more likely to vote in favor of contributors and their interests (Liptak and Roberts 2006, Goldberg et al. 2006).¹⁹ However, by the same token campaign expenditures enhance public awareness and indicate that judges face real political competition, which, for reasons we have given, might be a good rather than bad thing. In addition, one needs to take account of the hidden costs of appointment systems, namely, that they can serve patronage purposes.

¹⁹ Goldberg et al. (2006) identify Alabama, Illinois, Michigan, and Ohio as outliers—states where campaign contributions were much greater than in other states—as of 2000, the last year of our study. We checked the independence scores of judges in these states and found Alabama and Michigan had lower mean independence scores than the mean for all states, while Illinois and Ohio had higher scores. This is an interesting topic for future research, but as of now the data do not seem reliable and accessible enough to do a rigorous test.

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APPENDIX

Key Variable Definitions

Variable	Definition
Total Opinions	Total number of majority, concurring, and dissenting opinions authored by a particular judge in one year (ranging from 1998 to 2000)
Outside State Citations	Total number of citations from federal courts outside the circuit that includes the state in question and from courts in other states. Citations are measured in opinions authored up until January 1, 2007 (as tracked in the LEXIS Shepard's database).
Opposite_Party	The total number of opposing opinions written against an opposite party judge divided by the total number of opposing opinions written against either a judge of the opposite or same party as the judge in question for the 1998 to 2000 time period. Opposing opinions include dissents written against a majority opinion and majority opinions where a dissenting opinion exists.
Opposite_Pool	Total number of majority opinions written by the high court judges of the opposite political party (from the perspective of the judge in question) divided by the total number of majority opinions written by judges of both the same and opposite parties from 1998 to 2000.
Independence	Defined as Opposite_Pool minus Opposite_Party. A more negative Independence score occurs when Opposite_Pool < Opposite Party, indicating an increased tendency to write an opposing opinion against an opposite party judge. Conversely, a more positive Independence score indicates a decreased tendency to write an opposing opinion against an opposite party judge.
Election Non-Partisan	Indicator variable equal to 1 if the state uses a non-partisan election to select high court justices and 0 otherwise.
Election Partisan	Indicator variable equal to 1 if the state uses a partisan election to select high court justices and 0 otherwise.
Merit Plan	Indicator variable equal to 1 if the state follows the Missouri Merit Plan or a variant (including the Tennessee Plan) to select High Court justices and 0 otherwise.
Tenure	The average tenure of high court judges for the state in question, measured as of the spring of 1997 (from Hanssen 1999, tble. 1).
Political Competition	Folded Ranney index determined for 1995 to 1998 for the state in question (from Bibby and Holbrook 1999). The folded Ranney index ranges from 0.5-1, with higher numbers indicating greater competitiveness.
High Political Competition	Indicator Variable equal to 1 if the state in question has a Political Competition Score that is greater than the median for all states and 0 otherwise.

Number of Dissents	Indicator Variable equal to 1 if the judge authoring an opinion is Republican and 0 otherwise.
West Key Pages	Number of pages in an opinion associated with the West key pages section (as provided in the West reporter version of the opinion and tabulated on Westlaw).
Opinion Length	Number of pages from the start of the opinion to the end of the opinion as provided in the West reporter version of the opinion and tabulated on Westlaw. For majority opinions, we measured from where the authoring judge's actual opinion starts to the end of the majority opinion.
Open Secrets	Indicator Variable equal to 1 if the judge authoring the opinion in question has donated to a political candidate and 0 otherwise. Political contributions are tracked by www.opensecrets.org and include Federal Election Commission records of receipts from all individuals who contribute at least \$200 from 1992 to 2006.

APPENDIX

State-Level Variable Definitions

Variable	Definition
Adjusted Associate Justice Salary	For year-level data, the associate justice salary reported in the prior year for the state (so 1997 for 1998 judge-level data) divided by a cost of living adjustment for the year in question measured for the metro area in which the high court of the state is located. For pooled data, the associate justice salary reported in 1997 divided by the cost of living adjustment for 1998.
Adjusted Partner Salary	For year-level data, the average partner salary reported for the year in question for the state divided by a cost of living adjustment for the year in question measured for the metro area in which the high court of the state is located. For pooled data, the average partner salary in 1998 divided by the cost of living adjustment for 1998.
Stable Court	Indicator variable equal to 1 if the state high court justices stayed the same from 1998 to 2000 and 0 otherwise.
Number of Active Judges on Bench	Number of judges who were active at any time from 1998 to 2000 for the state in question.
Long-Term Clerk	Indicator variable equal to 1 if state clerks are tenured for more than one year and 0 if tenure is 1 year or less.
Number of Clerks Per Judge	Average number of clerks per judge in the 1998 to 2000 time period.
Number of Trial Cases in the State	Number of trial cases in the entire state in 1998.
Intermediate Appellate Court	Indicator Variable equal to 1 if the opinion is in opposition to the opinion of another judge in the same case and 0 otherwise. In the case of a dissenting opinion written by the judge in question, the opinion is treated as in active opposition to the majority opinion. In the case of a majority opinion by the judge in question, active opposition exists if the majority opinion is opposed by a dissenting opinion.
Population	For year level data, the population of the state in millions measured in the year prior to the year in question (so the population in 1997 if the data year is 1998). For pooled data, the population of the state in millions measured for 1997.
Gross State Product	Gross State Product (measured as of 1998).
Crime Index	For year level data, overall crime rate for the state (including property and violent crime) per 100,000 population from the FBI Crime Report for the year prior to the year in question. For pooled data, the overall crime rate measured for 1997.
Total Population in Border States	Total population of all bordering states of the state in question (measured as of 1997).
State Age	Age of the state. For year-level data this is defined as the difference between the year in question and the year of admission of the state into the United States. For pooled data, this is defined as the difference between 1998 and the year of admission of the state into

the United States.

Citizen Ideology Score	Measure of citizen ideology based on election results in each district, which are then used to compute a statewide average (ultimately based on interest group ratings of a given state's federal congressional delegation) (from Berry et al. 1998).
Misconduct	Number of misconduct removals of judges in the state in question from 1990-1997.
Impeached	Number of judicial impeachments in the state in question from 1990-1997.

APPENDIX

Judge-Level Variable Definitions

Variable	Definition
Chief Judge	For year-level data, indicator variable equal to 1 if the judge in question is the chief judge of the court in the year in question and 0 otherwise. For pooled data, indicator variable equal to 1 if the judge in question is the chief judge of the court for any year from 1998 to 2000 and 0 otherwise.
Court Experience	For year-level data, the difference between the year in question and the year the judge first joined the high court. For pooled data, the difference between 1998 and the year the judge first joined the high court (if the judge started on the court in 1998 or later court experience is set to 0).
Post-Law School Experience	The difference between 1998 and the year the judge graduated law school.
Retirement Close	Indicator variable equal to 1 if the judge in question retired from the bench in 2001 or earlier and 0 otherwise.
USNews Rank	U.S. News Ranking of the judge's law school (measured as of 2005). ²⁰
In-state Law School	Indicator variable equal to 1 if the judge attended a law school located in the state in question and 0 otherwise.

²⁰ These data were convenient; older data was either accessible or less

APPENDIX - continued
Subject Matter Categories

Variable	Definition
Administrative	Review of Agency/Government Decisionmaking (not in another subject matter category). Also includes Government Actions (e.g., State suit to comply with state statute that does not fit in other categories); private actions suing state actors for negligence, etc (unless the case involves prisoner rights which is included in the “Criminal” category of cases).
Attorney and Client	Attorney Misconduct; Attorney fees (unless fits in one of above categories); Disbarment; contempt of court order against attorney
Capital Punishment	Capital Punishment-related actions.
Church and State	Pledge of Allegiance; Funding for Private Religious Schools; Prayer in School; Ten Commandments
Commercial	Contracts; Insurance; Private arbitration; Creditor v. Debtor; Lessor-Lessee; Usury Laws; Franchise v. Franchisor; Employment Contractual Disputes; Corporate Law; Piercing the Corporate Veil; Tax; Bankruptcy; Enforcement of mechanics lien; Implied warrant of merchantability
Criminal	Sentencing Guidelines; Prisoners Rights; Murder; Rape; Drugs/Controlled Substances; Attorney-Client Privilege in Criminal Context; Grand Jury-related; Juvenile Criminals. Excludes Capital Punishment cases.
Family	Divorce; Adoption; Child Support; Probate/Inheritance
First Amendment	Employment issues (excluding employment contractual disputes); ERISA; National Labor Relations Board (NLRB); Occupational Safety and Health Act (OSHA); Fair Labor Standards Act (FLSA); Wrongful Discharge; Labor Management Relations Act (LMRA); Family and Medical Leave Act (FMLA); Employee Benefits; Worker’s Compensation claims; Retaliatory Discharge claims.
Labor	Employment issues (excluding (1) employment contractual disputes that are not Workers Comp or state administrative wage rate related—these go to “Commercial” and (2) excluding discrimination-type claims that fit in “Civil Rights”); ERISA; NLRB; Occupational Safety and Health Act (OSHA); Fair Labor Standards Act (FLSA); Wrongful Discharge; Labor Management Relations Act (LMRA); Family and Medical Leave Act (FMLA); Employee Benefits; Worker’s Compensation claims; Retaliatory Discharge claims; State Wage Rate Claims

Property	Takings claims; Zoning issues; Property rights; Property Licensing-Related or Permit-Related; Landlord-Tenant-Related.
Rights	Race Discrimination; Sex Discrimination; Affirmative Action; Civil Rights; Age Discrimination; Privacy; Handicap Discrimination; Abortion (Includes discrimination in employment context cases); Voting Rights-Voting Related
Torts	Federal Tort Related Act; Medical Malpractice; Products Liability; Wrongful Death; Libel; etc.
Other	All other cases.

Table 1

Appointed	Merit Selection	Non-Partisan Election	Partisan Election
Connecticut	Alaska	Georgia	Alabama
Delaware	Arizona	Idaho	Arkansas
Hawaii	Colorado	Kentucky	Illinois
Massachusetts	Iowa	Louisiana	Mississippi
Maine	Indiana	Michigan	North Carolina
New Hampshire	Kansas	Minnesota	New Mexico
New Jersey	Maryland	Montana	Pennsylvania
New York	Missouri	North Dakota	Texas
Rhode Island	Nebraska	Nevada	West Virginia
Vermont	Oklahoma	Ohio	
South Carolina	South Dakota	Oregon	
Virginia	Utah	Washington	
	Wyoming	Wisconsin	
	California		
	Florida		
	Tennessee		

Table 2: Productivity

	Number of Judge-Years	Mean Total Opinions Per Year	Mean Majority Opinions Per Judge Per Year	Mean Dissenting Opinions Per Judge Per Year
Partisan Election	229	31.3	19.4	8.0
Non-Partisan Election	277	27.5	17.9	6.9
Merit Selection	336	23.6	18.1	3.7
Appointed	232	20.9	16.7	3.0

Judge-years represent yearly data for each judge in our sample. Total Opinions includes majority, dissenting, and concurring opinions.

Table 3: Mean Citations Per Majority Opinion

	Number of Majority Opinions	Outside Federal Court	Other State Court	US Supreme Court	Outside State Citations	Law Review Citations
Partisan Election	4447	0.108	0.458	0.005	0.572	1.765
Non-Partisan Election	4952	0.103	0.603	0.007	0.712	1.856
Merit Selection	6079	0.134	0.631	0.009	0.774	1.840
Appointed	4007	0.211	0.657	0.006	0.872	2.273

Home Federal Court includes all citations from a federal district or circuit court located in the circuit that contains the state in question. Outside Federal Court includes all citations from a federal district or circuit court located in a circuit that does not contain the state in question. Other State Court includes all citations from state courts outside of the state in question. US Supreme Court includes all citations from the U.S. Supreme Court. Outside Citation is the sum of Outside Federal Court + Other State Court + US Supreme Court. All citations are from the LEXIS Shepard's database and are tracked up until January 1, 2007. Law Review Citations are for law reviews as tracked by the LEXIS Shepard's database (until January 1, 2007).

Table 4: Independence**Panel A**

	Number of Judges	Mean Independence	Independence_Indicator (1=Independent; 0=Partisan)
Partisan Election	83	-0.019	0.453
Non-Partisan Election	89	-0.066	0.345
Merit Selection	108	-0.030	0.433
Appointed	72	-0.027	0.435

Independence is defined as the Opposite_Party – Opposite_Pool. Opposite_Party is the number of opposing opinions written against a judge of the opposite party divided by the number of opposing opinions written against a judge of either the opposite or same party from 1998 to 2000. Opposite Pool is the total number of majority opinions authored by an opposite party judge divided by the total number of majority opinions authored by either an opposite or same party judge from 1998 to 2000. Independence_Indicator is defined as 1 if Independence is greater or equal to zero and 0 otherwise. Only judges for whom we could identify a political party were included in the analysis. Only judges from states that had a mixture of judges from different political parties were included.

Panel B

	Less than or Equal to Median Number of Authored Dissents	Greater than Median Number of Authored Dissents	p-value
Partisan Election	-0.021	-0.018	0.9063
Non-Partisan Election	-0.082	-0.055	0.5911
Merit Selection	-0.033	-0.025	0.7801
Appointed	-0.029	-0.023	0.9387

	Open Secrets=0	Open Secrets=1	p-value
Partisan Election	0.057	-0.052	0.0001
Non-Partisan Election	-0.032	-0.115	0.0893
Merit Selection	-0.028	-0.037	0.7817
Appointed	-0.044	0.067	0.1768

Table 5: Tenure Data

	Number of States	Mean	Std. Dev.	Min.	Max.
Partisan Election	10	6.8	2.4	3.5	11.1
Non-Partisan Election	13	7.0	1.5	4.6	9.1
Merit Selection	17	10.0	4.5	4	19.1
Appointed	12	9.2	3.1	5	14.9
Total	52	8.5	3.4	3.5	19.1

Tenure is defined as the average tenure of high court judges for the state in question, measured as of the spring of 1997 (from Hanssen 1999, tble. 1).

Table 6: Productivity

Dependent Variable	Model 1	Model 2
	ln(Total Opinions)	ln(Total Opinions)
Independent Variables		
Election Partisan	0.556** (6.290)	
Election Non-Partisan	0.478** (5.700)	
Merit Plan	0.426** (5.050)	
Tenure		-0.045** (-5.870)
Chief Judge	-0.173** (-2.800)	-0.190** (-3.030)
Court Experience	0.011* (2.440)	0.014** (2.950)
Post-Law School Experience	0.007* (2.250)	0.008* (2.430)
Retirement Close	-0.234** (-4.230)	-0.252** (-4.560)
Adjusted Associate Justice Salary	0.001** (3.100)	0.001** (4.750)
Adjusted Partner Salary	0.000 (-0.910)	0.000 (-0.920)
Stable Court	0.264** (4.120)	0.303** (4.780)
Number of Active Judges on Bench	0.000 (0.010)	0.045 (2.530)
Long-Term Clerk	-0.134** (-2.610)	-0.029 (-0.580)
Number of Clerks Per Judge	-0.019 (-0.430)	0.000 (0.010)
ln(Number of Trial Cases in the State)	0.010 (0.250)	-0.002 (-0.060)
Intermediate Appellate Court	0.075 (0.660)	0.097 (0.880)
ln(State Population)	0.835** (4.420)	1.051** (5.870)
ln(Total Population in Border States)	-0.057**	-0.046**

	(-5.080)	(-4.200)
Crime Index	0.000** (-5.050)	0.000** (-6.050)
State Age	0.005** (5.560)	0.002** (3.170)
ln(Gross State Product)	-0.941** (-5.490)	-1.155** (-7.070)
Citizen Ideology Score	0.002 (1.060)	-0.003 (-1.150)
Constant	12.249** (6.910)	15.025** (8.810)
Year Fixed Effects	Yes	Yes
N	1002	1002
R2	0.2004	0.2019

The t-statistics (in parentheses) are calculated using Huber-White robust standard errors. Variable definitions are in the Appendix.

⁺ Coefficient significant at the 10% level or less.

^{*} Coefficient significant at the 5% level or less.

^{**} Coefficient significant at less than the 1% level.

Table 7: Productivity and Political Competition

	Model 1	Model 2
Dependent Variable	ln(Total Opinions)	ln(Total Opinions)
Independent Variables		
Election Partisan	0.958** (7.410)	
Election Non-Partisan	0.739** (6.100)	
Merit Plan	0.533** (4.280)	
Tenure		−0.031* (−1.990)
High Political Competition	0.299** (2.750)	0.097 (0.540)
Election Partisan x High Political Competition	−0.743** (−4.490)	
Election Non-Partisan x High Political Competition	−0.554** (−3.570)	
Merit Plan x High Political Competition	−0.169 (−1.310)	
Tenure x High Political Competition		−0.016 (−0.870)
Constant	11.970** (6.200)	15.831** (8.340)
Year Fixed Effects	Yes	Yes
Judge Controls	Yes	Yes
State Controls	Yes	Yes
N	979	979
R2	0.2301	0.2055

The t-statistics (in parentheses) are calculated using Huber-White robust standard errors. Variable definitions are in the Appendix.

⁺ Coefficient significant at the 10% level or less.

^{*} Coefficient significant at the 5% level or less.

^{**} Coefficient significant at less than the 1% level.

Table 8: Citations

	Model 1	Model 2	Model 3
Dependent Variable	ln(Outside State Citations)	ln(Outside State Citations)	ln(Outside State Citations)
Independent Variables			
Election Partisan	−0.061** (−3.070)	−0.061* (−2.260)	
Election Non-Partisan	−0.047* (−2.580)	−0.089** (−3.690)	
Merit Plan	−0.081** (−4.100)	−0.109** (−4.140)	
Tenure			0.004 (1.140)
High Political Competition		−0.016 (−0.770)	0.084* (2.300)
Election Partisan x High Political Comp.		−0.008 (−0.260)	
Election Non-Partisan x High Political Comp.		0.131** (4.030)	
Merit Plan x High Political Comp.		0.051* (2.010)	
Tenure x High Political Comp.			−0.006 (−1.420)
Number of Dissents	0.060** (6.840)	0.060** (6.710)	0.062** (7.020)
West Key Pages	0.000 (0.450)	0.000 (0.180)	0.000 (0.380)
Opinion Length	0.034** (20.320)	0.034** (20.020)	0.034** (20.190)
Administrative	−0.077** (−3.000)	−0.075** (−2.870)	−0.077** (−2.940)
Attorney and Client	0.057+ (1.860)	0.049 (1.560)	0.051+ (1.650)
Capital Punishment	−0.112** (−3.400)	−0.116** (−3.530)	−0.115** (−3.500)
Church and State	0.183 (0.760)	0.175 (0.720)	0.179 (0.720)
Commercial	0.160** (6.330)	0.159** (6.230)	0.160** (6.250)
Criminal	0.034	0.026	0.028

	(1.470)	(1.100)	(1.170)
Family	0.077** (2.920)	0.070** (2.610)	0.070** (2.600)
First Amendment	0.126 (1.570)	0.152+ (1.780)	0.162+ (1.920)
Labor	-0.053* (-2.110)	-0.055* (-2.160)	-0.053* (-2.080)
Property	-0.005 (-0.180)	-0.006 (-0.210)	-0.002 (-0.090)
Rights	0.110** (2.650)	0.093* (2.180)	0.106* (2.510)
Torts	0.145** (5.660)	0.147** (5.670)	0.148** (5.670)
Constant	-3.031** (-8.530)	-2.578** (-6.520)	-3.067** (-8.190)
Year Fixed Effects	Yes	Yes	Yes
Court Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
N	18360	17726	17726
R2	0.1309	0.1342	0.1322

The t-statistics (in parentheses) are calculated using Huber-White robust standard errors. Variable definitions are in the Appendix.

+ Coefficient significant at the 10% level or less.

* Coefficient significant at the 5% level or less.

** Coefficient significant at less than the 1% level.

Table 9: Dissents

	Model 1	Model 2
Dependent Variable	ln(1+Dissent Opinions)	ln(1+Dissent Opinions)
Independent Variables		
Election Partisan	0.948** (6.190)	
Election Non-Partisan	0.759** (5.520)	
Merit Plan	0.005 (0.040)	
Tenure		-0.113** (-5.960)
High Political Competition	-0.041 (-0.310)	-0.959** (-4.220)
Election Partisan x High Political Competition	-0.599** (-2.680)	
Election Non-Partisan x High Political Competition	-0.410+ (-1.950)	
Merit Plan x High Political Competition	0.423* (2.520)	
Tenure x High Political Competition		0.087** (3.670)
Constant	3.402 (1.430)	4.407+ (1.770)
Year Fixed Effects	Yes	Yes
Court Controls	Yes	Yes
State Controls	Yes	Yes
N	979	979
R2	0.2500	0.2070

The t-statistics (in parentheses) are calculated using Huber-White robust standard errors. Variable definitions are in the Appendix.

+ Coefficient significant at the 10% level or less.

* Coefficient significant at the 5% level or less.

** Coefficient significant at less than the 1% level.

Table 10: Independence

	Model 1	Model 2	Model 3
Dependent Variable	Independence	Independence	Independence
Independent Variables			
Election Partisan	0.052 (0.810)	0.108 (1.630)	0.074 (0.900)
Election Non-Partisan	-0.068 (-1.100)	-0.019 (-0.310)	-0.114 (-1.350)
Merit Plan	-0.053 (-0.780)	-0.034 (-0.490)	-0.007 (-0.080)
Number of Dissenting Opinions	0.003* (2.300)	0.003* (2.320)	0.003* (2.350)
Number of Dissenting Opinions x Election Partisan	-0.006** (-3.310)	-0.005** (-2.790)	-0.005** (-2.640)
Number of Dissenting Opinions x Election Non-Partisan	0.001 (0.470)	0.002 (1.000)	0.002 (0.950)
Number of Dissenting Opinions x Merit Plan	-0.003 (-0.920)	-0.002 (-0.770)	-0.003 (-0.930)
Opensecrets		0.095 (1.450)	0.091 (1.310)
Opensecrets x Election Partisan		-0.198** (-2.700)	-0.197* (-2.580)
Opensecrets x Election Non-Partisan		-0.194* (-2.370)	-0.171* (-2.020)
Opensecrets x Merit Plan		-0.120 (-1.580)	-0.111 (-1.240)
High Political Comp.			-0.007 (-0.090)
Election Partisan x High Political Comp.			0.046 (0.370)
Election Non-Partisan x High Political Comp.			0.225* (2.000)
Merit Plan x High Political Comp.			-0.016 (-0.140)
Constant	2.894+ (1.890)	1.862 (1.220)	5.185* (2.420)
Subject Matter Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Judge Controls	Yes	Yes	Yes
N	327	327	318

R2	0.2037	0.2336	0.2584

The t-statistics (in parentheses) are calculated using Huber-White robust standard errors. Variable definitions are in the Appendix.

- ⁺ Coefficient significant at the 10% level or less.
- ^{*} Coefficient significant at the 5% level or less.
- ^{**} Coefficient significant at less than the 1% level.

Table 11: Selection Mechanisms and Quality

<i>Selection Mechanism</i>	<i>Tenure</i>	<i>Productivity</i>	<i>Independence</i>	<i>Quality</i>
Election Partisan	Lower	High	Unclear	Low
Election Non-Partisan	Lower	Middle	Unclear	Middle
Merit Plan	Higher	Middle	Unclear	Middle
Appointed	Higher	Low	Unclear	High

Table 12

	Mean Open secrets	Mean Salary Difference (Partner – Associate Justice Salary)	Mean Stable court	Mean Court Experience	Mean Post-Law School Experience
Election Partisan	0.67 ^a	1004 ^a	0.08 ^a	5.77	29.36 ^b
Election Non-Partisan	0.36	872	0.28	6.67	30.72
Merit Plan	0.22	972	0.19	8.47	32.03
Appointed	0.14	822	0.25	7.28	32.27

	Mean Retirement Close	Mean Tenure	Mean In-State Law School	Mean Active Judges	Mean US News Rank
Election Partisan	0.42 ^c	7.10 ^a	0.70 ^a	9.55 ^a	62.94 ^a
Election Non-Partisan	0.22	7.09	0.67	8.25	58.78
Merit Plan	0.23	10.13	0.70	7.65	61.40
Appointed	0.30	9.58	0.33	7.71	32.34

^at-test of difference in means for Election Partisan and Appointed Judge is significant at the <1% level.

^bt-test of difference in means for Election Partisan and Appointed Judge is significant at the 5% level.

^ct-test of difference in means for Election Partisan and Appointed Judge is significant at the 10% level.

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The Booth School of Business, University of Chicago

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Elected versus Appointed Policy Makers: Evidence from City Treasurers

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Abstract

This paper investigates whether the method of selecting public officials affects policy making. I compare the policy choices of bureaucrat city treasurers and politician city treasurers, who are selected and held accountable in very different ways. The analysis draws on rich data from California to examine whether cities with appointed or elected city treasurers pay lower costs to borrow. The results demonstrate that having appointive treasurers reduces a city's cost of borrowing by 19–31 percent. Holding officials directly accountable to voters can result in lower levels of performance in complex policy areas.

1. Introduction

Do political institutions affect policy making? The answer to the question depends crucially on whether policy outcomes are primarily due to the design of institutions (Besley 2006), capture by special interests (Grossman and Helpman 2001), or voter preferences (Downs 1957). Despite the central importance of policy making in political economics, our knowledge of the responsiveness of policy making to alternative political institutions remains limited.¹

In this paper, I compare the policy choices of bureaucrat city treasurers and politician city treasurers, who are selected and held accountable in very different ways. Bureaucrats and politicians face quite different incentives to exert effort. Politicians are directly accountable to voters and exert effort to gain reelection.

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¹ Besley and Case (2003) provide an excellent survey of the literature. Seminal contributions include Barro (1973) and Ferejohn (1986).

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In contrast, top-level bureaucrats are accountable to their professional peers and potential employers and exert effort to gain future job postings or professional recognition. Whether politicians or bureaucrats exert more effort or simply pander to voters and special interest groups depends crucially on the strength of the implicit incentives that reelection and career concerns provide. As the importance of these implicit incentives is unclear, the effect of delegating policy to a bureaucrat versus a politician remains unknown.²

I examine the effect of political versus bureaucratic control of policy in the case of city treasurers for several reasons. First, city treasurers' policy choices can be easily observed in the costs that cities pay to borrow. Debt management policy is a particularly compelling government service to examine, as costs are observable in the expenditure that cities pay to borrow, and attributes of public debt other than interest rates are likely to be less important to constituents than in other contexts.³ In addition, the complexity of debt management policy allows for a close match between theory and empirics, as recent theoretical contributions argue that bureaucrats have a comparative advantage in technical policy making (see, for example, Maskin and Tirole 2004; Alesina and Tabellini 2007, 2008).⁴

Second, the context of city treasurers seems to match typically considered accountability structures that emphasize effort and expertise effects very well. In terms of effort, elected and appointed treasurers likely face very different career concerns, as the effects of policy performance on reelection for the politician and promotion for the bureaucrat are likely to be quite different. These differences in effort crucially hinge on how well voters can infer talent from the policy performance of the politician and how the city treasurer profession infers talent from the policy performance of the appointed treasurer. In terms of expertise, the only requirements for a person to be an elected city treasurer is that he or she is a resident of the city and, usually, does not have a criminal conviction. In contrast, appointed city treasurers can be selected for their expertise in managing public debt and do not need to be a city resident. The context of city treasurers provides a rare opportunity to examine whether differences in the implicit incentives that bureaucrats and politicians face do indeed translate into meaningful differences in policy performance.

Finally, quasi-experimental variation in the treasurers' appointive status is

² A significant theoretical literature has addressed this question. See Besley and Coate (2003), Maskin and Tirole (2004), Alesina and Tabellini (2007, 2008), and Vlaicu and Whalley (2012). Much of the existing empirical work has focused on the related question of whether politicians are more likely than bureaucrats to choose policies that appeal to voters. See Besley and Coate (2003), Besley and Payne (2006), and Vlaicu and Whalley (2012). For recent evidence on career concerns for bureaucrats, see Boylan and Long (2005) and Phillips and Grace (2008).

³ There are, of course, many reasons for differences in the costs of borrowing across cities. The majority of the variation in borrowing costs across cities is due to differences in default risk. However, some of the differences may reflect differences in attributes of the debt that city residents value, such as variability in future interest rates.

⁴ While debt management is a complex policy task, it is unlikely to be so complex that highly competent public officials with specialized experience are unable to understand it, as Baye and Wright (2011) find for generalist judges in antitrust cases.

available. As many have argued that political institutions may be endogenously chosen to reflect the policy preferences of special interest groups or voters, disentangling the effects of a government's form from other unobserved factors may not be straightforward.⁵ However, as special interest groups and voters may face substantial costs in influencing the choice of political institutions, the endogeneity of institutions may not be a substantive concern in practice. The case of city treasurers provides a chance to examine whether addressing the endogeneity of political institutions substantively alters conclusions about institutional effects on policy making.

Before introducing the empirical approaches, it is worthwhile to explain how a city's borrowing costs measure debt management policy choices. The cost that a city pays to finance a debt issue is composed primarily of two factors: the perceived default risk of a city and the debt management policy choices of a city treasurer. The default risk of a city reflects any factors that predict default and thus require a premium from bond investors, from the size and diversity of the tax base to whether city expenditure policies are sustainable. Two debt management policies chosen by the treasurer also affect borrowing costs: debt refinancing and debt issuance. Reducing borrowing costs with debt refinancing policies requires active monitoring of public debt markets, as well as the expertise to evaluate likely changes in future market interest rates. Reducing borrowing costs with debt issuance policies, in contrast, requires the use of competitive debt sales that reduce payments to (potentially politically connected) financial intermediaries. The challenge for the empirical analysis is to control for a city's default risk so that the remaining differences in borrowing costs between cities with differing treasurer selection methods reflect debt management policy choices alone.

The central contribution of this paper is to estimate the effects of bureaucratic versus political control on policy making. Three alternative and complementary approaches are used to measure the appointive effect. First, I present an ordinary least squares (OLS) regression analysis to compare borrowing costs in cities with elected and with appointed city treasurers. By using richly detailed data for a large number of cities, I am able to control for a multitude of factors that affect default risk, and thus borrowing costs, which might also be related to the choice of institutions. To the extent that a city's economic, demographic, and fiscal policy characteristics are sufficient to capture the relationship between default risk and appointive city treasurer status, the OLS regressions will yield an estimate with a causal interpretation. Of course, as the choice of political institutions may be related to unobservable determinants of policy choice, the OLS results may be driven by the selective adoption of political institutions.

In my second approach, I implement a fixed-effects analysis. This approach

⁵ See Besley and Case (2003) and Acemoglu (2005) for discussion of the implications of endogenous institutions for estimating the causal effect of institutions. Aghion, Alesina, and Trebbi (2004) and Acemoglu, Ticchi, and Vindigni (2006) introduce theoretical models of the endogenous determination of institutions.

addresses concerns that political institutions in a city may just reflect unobserved time-invariant factors that also determine policy outcomes. For example, cities with strong public-sector unions may have stronger preferences for appointive officials. If the presence of public-sector unions also leads to higher levels of public spending, the default risk and thus borrowing costs in a city would be higher anyway. The fixed-effects analysis addresses this issue as long as the relevant factors do not change over time. To address the possibility of bias arising from changes in unobservable factors leading to changes in institutions, I implement a third approach.

My third approach is a regression discontinuity (RD) research design. The design takes advantage of the unique characteristics of California's local referendum process to isolate quasi-experimental variation in city treasurer selection methods. Many differences in city treasurer selection methods across cities are likely due to differences in unobserved city institutions, resident preferences, or special interest groups' strength that also likely affect borrowing costs. Cities in which residents vote to pass an appointive city treasurer referendum may well differ on both observable and unobservable dimensions from those that do not. However, cities in which an appointive treasurer referendum passes by a very narrow margin are likely to be quite similar on average to cities in which an appointive treasurer referendum fails by a very narrow margin. Taking advantage of this feature, I use an RD design to identify the causal effect of city treasurer selection methods on cities' borrowing costs. I apply this estimator to a newly available rich data set from cities in California that combines more than a decade's worth of information on local referenda with annual measures of cities' borrowing costs.⁶

My main results concern the appointive treasurer effect on a city's borrowing costs. I find that having an appointed rather than elected city treasurer reduces cities' borrowing costs by 19–31 percent in the OLS and fixed-effects analyses. The results of the RD analysis indicate that having an appointed rather than elected city treasurer reduces cities' borrowing costs by 20–22 percent. The similarity of the results across these approaches suggests that unobservable determinants of borrowing costs are not strongly related to the choice of political institutions. In addition, I find little evidence that either fiscal policy or other costs of borrowing are affected by the method of selection for the city treasurer.

My results provide clear evidence that bureaucratic control of cities' debt management policy reduces cities' borrowing costs. Caution is required, however, in attempting to generalize these results beyond my context, as cities' debt management is a highly technical policy. Nevertheless, finding that bureaucratic control of debt management policy reduces cities' borrowing costs is an important result that provides empirical support for recent theoretical work emphasizing the comparative advantage of bureaucratic control for technical policy making

⁶ Lee and Lemieux (2008) provide an excellent survey of the regression discontinuity (RD) research design.

(Maskin and Tirole 2004; Alesina and Tabellini 2007, 2008). Empirically, the results contrast with Besley and Coate (2003), who find that elected electricity rate regulators choose lower prices than do appointed regulators.⁷

The remainder of the paper is organized as follows: Section 2 describes the conceptual framework and context. Section 3 describes the empirical approach. Section 4 discusses the data and descriptive statistics. Section 5 presents the main results and examines evidence of differences between politician and bureaucrat treasurers in their debt restructuring and issuance policy choices. Section 6 concludes.

2. Conceptual Framework, City Treasurers, and Debt Management Policy

2.1. *Conceptual Framework*

2.1.1. Political Agency

Here I discuss a simple framework to understand the effects of cities' appointive treasurer status on the policy outcome, cities' borrowing costs. I discuss these effects in a simple agency model of treasurer performance based on Alesina and Tabellini (2007).⁸ In the model, there are two treasurer-supplied inputs that affect policy performance: effort and ability. The choice of treasurer selection methods affects the policy outcome through differences between bureaucrats and politicians in terms of effort—the effort effect—or in terms of expertise—the expertise effect.

In general, the sign of the appointive effect for debt management policy outcomes is ambiguous. It hinges crucially on the sign and magnitude of both the effort and the expertise effects of bureaucratic versus political control. In principle, either effect could go in either direction. If the reelection of a politician city treasurer depends crucially on government borrowing performance but future employers of a bureaucrat place little value on prior government borrowing performance, the effort effect would favor the politician. Alternatively, if constituents face significant difficulty in observing policy performance by politicians but future employers of bureaucrats highly value prior policy performance, the effort effect would favor the bureaucrat. The sign of the expertise effect is also ambiguous. For example, bureaucrats might be more skilled than politicians on average if bureaucrats are selected on the basis of prior policy performance or education. Alternatively, if city treasurer positions are allocated to bureaucrats on a patronage basis, bureaucrats may be less skilled.

The sign and magnitude of the effort and expertise effects may be influenced

⁷ There is a large literature on the effects of appointed and elected regulators that has generally found mixed results. See Besley and Coate (2003) for an excellent survey of the prior literature.

⁸ A simple formal political agency model that more fully illustrates the roles of incentives and selection in policy performance is presented in Appendix A.

by pressure from special interest groups. While the presence of a special interest group is not explicitly incorporated into the simple political agency framework, it is straightforward to think about special interest groups seeking to influence the effort and ability supplied by a treasurer. For example, financial intermediaries may make campaign contributions to elected treasurers or provide kickbacks to appointed treasurers to influence policy choices. Of course, whether special interest groups prefer an appointed or an elected city treasurer likely depends on the relative costs of capturing the treasurer under alternative political institutions.

2.1.2. City Treasurers

The context of city treasurers seems to match the conceptual framework of Alesina and Tabellini (2007) very well. Appointed and elected treasurers are likely to differ in the levels of expertise and effort they supply to the policy task. In terms of expertise, the only requirements for a person to be an elected city treasurer is that he or she is a resident of the city and, usually, does not have a criminal conviction. In contrast, appointed city treasurers can be selected for their expertise in managing public debt and do not need to be a city resident. This difference often results in appointed treasurers having higher levels of education (often an MBA or master of public policy degree) than elected treasurers.⁹

In terms of effort, elected and appointed treasurers likely face very different career concerns, as the effects of policy performance on reelection for the politician and promotion for the bureaucrat are likely to be quite different. These effort differences crucially hinge on how well voters can infer talent from the policy performance of the politician and how the city treasurer profession can infer talent from the policy performance of the appointed treasurer.

Anecdotal evidence suggests that the incentives to exert effort for an appointed treasurer may well be larger than for an elected treasurer. The city treasurer occupation is highly professionalized, with a number of active associations and well-developed notions of professional performance. For example, the code of ethics for the California Municipal Treasurers Association contains the clauses “[t]o promote principles of good government,” “[t]o be dedicated to the concepts of effective and efficient local government service being provided by elected and appointed Treasurers,” and “[t]o observe the profession’s technical standards and continually strive to improve the Treasurers’ level of competence” (California Municipal Treasurers Association 2001). Similar clauses are contained in the code of ethics for the Association of Public Treasurers of the United States and

⁹ I attempted to directly collect information on individual city treasurers’ education levels from cities under a California Public Records Act request. Unfortunately, many cities keep only very limited records on the educational qualifications of their treasurers, and the resulting data set is too incomplete to be useful.

Canada.¹⁰ Many job advertisements for appointed treasurer positions require evidence that best practices in public finance were used in prior positions. In contrast, elected city treasurers may have less incentive to exert effort in the face of a public that has little knowledge of debt management policy.

2.2. *Cities' Borrowing Costs and Debt Management Policy*

There are three sets of participants in public debt markets: city issuers, financial intermediaries, and investors. The debt-issuing process begins with the city deciding that a capital project (or other need) requires financing; financial intermediaries are then hired, the issue is then presented to the debt-rating agencies, and finally the issue is sold to investors.¹¹

2.2.1. *Cities' Borrowing Costs*

The cost that a city ultimately pays to finance a debt issue is composed of two factors: the interest rate that investors demand to hold the debt and the spread between the interest rate spot price that investors demand and what the city pays. The interest rate that investors require to hold the city's debt depends principally on the default risk of the city.¹² Aspects of the local economy and the city's fiscal policy affect default risk and are reflected in the city's debt rating. These factors range from the diversity of the tax base to whether the city's expenditure policies are sustainable. In addition, other aspects of a debt issue are often determined by the nature of the project to be funded, such as the term length of the issue, and the principal amount can affect borrowing costs. The spread that a city pays above the spot market price for a debt issue depends on two debt management policies: debt issuance and debt restructuring.

2.2.2. *Debt Issuance Policies*

Debt issuance policies primarily affect the spread that intermediaries receive. The key debt issuance policy choice is whether to sell the debt in a competitive auction, in which sealed bids are submitted by underwriters and the lowest bid is chosen, or as a negotiated sale with a single underwriter chosen in advance

¹⁰ The code of ethics for the Association of Public Treasurers of the United States and Canada states that "[a] member shall adhere to concepts of effective and efficient local government service being provided by elected and appointed Public Treasurers. A member shall conduct himself or herself at all times in a manner which serves the public interest and maintains the good reputation of the profession" and that "[a] member shall observe professional technical standards and continually strive to acquire knowledge and improve levels of competence in Treasury management" (Association of Public Treasurers of the United States and Canada 2005).

¹¹ For a more detailed discussion of the issuance laws, rules, and regulations that govern the process of public debt for cities in California, see California Debt Issuance and Advisory Commission (2006). For a more general discussion of public debt issuance and management by local governments, see Joseph (1994) and Leonard (2004). See Feldstein and Fabozzi (2008) for an excellent and very thorough overview of the municipal debt market.

¹² Municipal defaults are rare but not unheard of. There were over 2,000 defaults by cities and incorporated townships in the United States between 1939 and 1969 (Spiotto 2008). Historically, defaults have been due to provision of nonessential services, fraud and mismanagement, adverse local economic conditions, and natural or man-made disasters (Spiotto 2008).

of pricing. Competitive sales lead to lower borrowing costs for the issuer but likely a smaller spread for the underwriter.¹³ Levitt (2009) criticizes noncompetitive sales as pay-to-play rewards for the campaign finances provided to elected officials by financial intermediaries. Thus, the choice of competitive or noncompetitive sales methods is likely to be related to the influence of special interest groups. Other debt issuance policy choices include when to issue the debt and the characteristics of the debt issue, such as term length.

2.2.3. Debt Refinancing Policies

Debt refinancing policies are the second type of policies that can influence borrowing costs. A city's debt issue, much like conventional mortgages, can be refinanced. As market interest rates fluctuate according to many factors, refinancing an issue when it is worthwhile to do so can result in lower average borrowing costs on a city's debt. The potential benefit of refinancing a debt issue is expressed nicely in this municipal finance saying: "While doctors bury their mistakes, in municipal financing, they are refunded [refinanced]" (Spiotto 2008, p. 707). As Figure 1 demonstrates, there is significant variability in the market interest rate for municipal securities.¹⁴ Market interest rate fluctuations reflect predictable patterns of demand, such as the timing of issues from the U.S. Treasury and other large issuers, national holidays, and seasonal demand from investors. Unexpected changes in monetary or tax policies may also affect market interest rates.

A debt-refinancing policy of responding to favorable market conditions can significantly reduce average borrowing costs. For example, in a typical year a city treasurer who sells a revenue anticipation note on a week that is in the top 10 percent of the interest rate distribution will pay an interest rate that is 22 percent higher than one who sells the note on a week that is in the bottom 10 percent. Of course, refinancing a debt issue is not without transactions costs, however, these are relatively low. The Government Finance Officers Association of the United States and Canada (1995) recommends as a best practice that refinancing an issue is cost-effective if the net present value of the interest savings from refinancing are on the order of 3–5 percent.

3. Empirical Approaches

The analysis employs two complementary empirical approaches. I first conduct an OLS analysis that uses a large sample of cities and richly detailed data on cities' economic, population, and financial characteristics to control for default risk. While the OLS analysis does not address concerns about correlation between

¹³ The evidence is very clear on this difference in borrowing costs. See Robbins and Simonson (2007) and Simonson, Robbins, and Helgersen (2001), which show that competitive sales reduce borrowing costs for cities.

¹⁴ Figure 1 is derived from my calculations based on data from Global Financial Data's US Municipal Government Bond Yields database (<http://www.globalfinancialdata.com>).



Figure 1. Weekly municipal bond and note market interest rates, 1995–2006

unobservable contributions to the default risk and political institutions, it does use a large sample of cities to measure the effect. In contrast, the RD analysis exploits quasi-experimental variation in political institutions to effectively address concerns about institutional endogeneity. However, the RD analysis is necessarily conducted on the smaller sample of cities that put an appointive treasurer referendum to the voters during the sample period.¹⁵ Thus, one particularly attractive feature of the city treasurer context is that both approaches, which have quite different sets of assumptions, are possible and comparable.

3.1. Ordinary Least Squares Approach

My first approach is to estimate the appointive effect by OLS regression. The simple model I estimate can be expressed as

¹⁵ The lack of large numbers of changes in political institutions is well known in the literature. In fact, because important political institutions change so infrequently (and any changes are likely to be endogenous), prior researchers in this area have necessarily focused on either cross-sectional analysis alone or within-state analysis, with very few experiments. I present a few examples from recent studies: Besley and Case (2003) note that some of the major state-level changes in electoral rules in the United States from 1950 to the 1990s, such as literacy tests or poll taxes, affected only 15 percent of states. In addition, the highly influential study of Besley and Coate (2003) has only three states that changed selection systems (from 1960 to 1997) from which to estimate the effect of the electoral status of regulators on electricity prices. Many other careful and important studies are limited to the analysis of cross-sectional data alone; see Persson and Tabellini (2003) and Aghion, Alesina, and Trebbi (2004) for recent examples of this work.

$$Y_{it} = \delta \text{Treasurer Appointed}_{it} + \mathbf{X}_i \beta + \omega_t + \varepsilon_{it}, \quad (1)$$

where i indexes the city and t indexes the year. The term $\text{Treasurer Appointed}_{it}$ is a dummy variable that takes a value of one if the treasurer is appointed and zero otherwise. The term $\mathbf{X}_i$ is a vector of covariates from the 1990 decennial census in the city in which the referendum occurs, ω_t is a set of year fixed effects, and ε_{it} is the error term. The variables I control for in $\mathbf{X}_i$ to capture variation in the default risk across cities are Per Capita Income, Population, Percentage White, Percentage Black, Percentage Less than 17, Percentage Greater than 65, Percentage College Graduate, Percentage High School Graduate, Mayor-Council Form of Government, and Employment-Population Ratio.¹⁶ The conditional mean difference in policy outcomes between appointed and elected treasurers is reflected by δ . I cluster the standard errors at the city level to account for the fact that borrowing costs are correlated within a city and changes in appointive treasurer status are persistent (Bertrand, Duflo, and Mullainathan 2004).

The outcome variable for the main analysis, Y_{it} , measures the borrowing costs in a city in a given year by the average interest rate paid on the stock of a city's debt. It is important to be clear on how this measure of borrowing costs measures a city's debt management policy choices. The interest rate on the stock of a city's debt will be affected by either debt issuance or restructuring choices. For the analysis that examines changes in the treasurer position within a city, my analysis likely captures the short-run effect of a change in treasurer selection methods. This short-run effect may well differ from a longer-run effect, depending on which policy choices change with the treasurer selection method. Debt restructuring policies are likely to result in a larger short- than long-run effect, as an effective treasurer who inherits a stock of expensive debt from a less effective treasurer could reduce average interest rates quite quickly by refinancing the debt. Of course, if the OLS approach yields estimates similar to the models based on changes in the treasurer position in a city, the short- and long-run effects are unlikely to differ substantially.

While it is possible to control for many easily measured city characteristics in $\mathbf{X}_i$, there could be important unobserved components of borrowing costs that are correlated with political institutions. For example, cities may differ in their budgetary institutions, which can affect the level of debt and probability of default. Poterba (1994) provides evidence that budgetary institutions affect debt levels and the adjustment to fiscal shocks for U.S. states. Other factors such as the strategic use of debt by politicians (Alesina and Tabellini 1990) or high-spending city legislative institutions (Coate and Knight 2009) may lead to substantial debt accumulation and high borrowing costs. As the importance of the endogeneity of political institutions for policy making is unclear, whether the rich set of city measures used here can capture selection issues is an open

¹⁶ For descriptions of all variables, see Table A1.

question. I address these possibilities by examining both fixed-effects and RD models.

3.2. Fixed-Effects Approach

My second approach is to estimate the appointive effect by OLS regression with city fixed effects instead of time-invariant controls. The simple model I estimate can be expressed as

$$Y_{it} = \delta \text{Treasurer Appointed}_{it} + \omega_t + \sigma_i + \varepsilon_{it}, \quad (2)$$

where all the variables are defined as in equation (1) with the additional set of city fixed effects denoted σ_i . The fixed-effects specifications impose an identification assumption that differs from either the OLS or the RD model. The fixed-effects models will lead to a parameter with a causal interpretation if changes in the treasurer appointment status are unrelated to changes in unobservable determinants of a city's borrowing costs. While these specifications effectively address a broad class of selection problems related to time-invariant unobserved characteristics of cities, I next examine RD models to explore whether violations of the fixed-effects identification assumption could contaminate the fixed-effects results.

3.3. Regression Discontinuity Approach

To address potential concerns that the method of selection for treasurer may be endogenously related to other determinants of a city's borrowing costs, I also use an RD strategy to compare cities in which an appointive treasurer referendum barely passed with cities in which an appointive treasurer referendum barely failed. To implement my RD approach, I estimate models of the following form for each policy outcome, Y_{jt} :

$$Y_{jt} = \delta \text{Pass}_{jt} + \gamma(s_j) + X_{jt}\beta + \omega_t + \varepsilon_{jt}, \quad (3)$$

where j indexes the focal referenda and t indexes the year. The term Pass_{jt} is a dummy variable that takes a value of one in every year after the referendum passes and zero otherwise. The vote share is denoted s_j , ω_t is a set of year fixed effects, and ε_{jt} is the error term. Thus, the appointive effect, δ , is estimated controlling for the percentage voting in favor linearly. This simple model will yield an unbiased estimate of the appointive effect if the referendum outcome, Pass_j , was uncorrelated with the other determinants of policy, ε_{jt} , conditional on the vote share, s_{jt} .

Lee (2008) demonstrates that the RD strategy provides quasi-random variation in referendum outcomes, because for narrowly decided referenda, whether the referendum passes is likely to be determined by pure chance as long as there is some unpredictable component of the ultimate vote. The central identifying assumption for δ to estimate the causal effect of treasurer selection method on policy outcomes is that all relevant factors vary smoothly at the referendum passage threshold. We need this assumption for a city's policy outcomes after a

narrowly failing referendum to form an adequate counterfactual for those outcomes after a referendum narrowly passes. While this assumption is not testable directly, I examine whether a city's policy outcomes are smooth, conditional on the referendum vote share, in the years prior to those with referenda to assess its plausibility.¹⁷

To estimate equation (3), I use all the postreferendum data for a focal referendum in a given city. Observations are uniquely identified by the city and the date of the referendum. For the cities that have more than one referendum, the same calendar year observation is used more than once.¹⁸ To address this issue, I follow Cellini, Ferreira, and Rothstein (2010) and cluster the standard errors at the city level to account for the dependence created by multiple city-year observations and any serial correlation in the error term due to persistent debt management policy. I code the timing of the change in the selection method as occurring 2 years after the referendum, as the referendum to change the takes effect after the incumbent elected treasurer completes a 2-year term.

3.4. Appointive Treasurer Referenda

Historically, all general law cities in California began with elected treasurers. The state code allows for a general law city to make the treasurer position appointive if city residents vote for this change, and many cities have done so. Referenda for the appointment of city treasurers are placed on the ballot by city councils in accordance with procedures outlined in the California State Code.¹⁹ The exact text of the measure is given in the California state Code (Cal. Gov. Code sec. 36509) as "Shall the office of city treasurer be appointive?" with the words "yes" and "no" following the question. If more than 50 percent of the voters choose "yes," the referendum is carried. If the referendum is carried, the

¹⁷ The implementation of the RD strategy I focus on here uses all of the referenda but controls the variation coming from nonclose referenda via controls for the vote share. The RD design can be estimated parametrically or nonparametrically, focusing on only close elections or the larger sample of all elections. I follow a parametric approach using all referenda because it allows for straightforward hypothesis testing and precise estimates. See Lee and Lemieux (2008) for a detailed comparison of alternative approaches to estimating RD models.

¹⁸ There are four cities that put multiple referenda to the voters, two of which failed the first time and subsequently passed, in the sample period. I examine the robustness of my results to the exclusion of these cities as a sensitivity analysis.

¹⁹ Local ballot measures mostly concern local issues of land use, governance, and safety (Gordon 2004). As Gordon (2004) notes, California's local initiative and referendum process was enacted in 1911 in response to the perceived influence of special interest groups, in particular the railroads. Currently, the initiative process is available in all of California's 475 cities. All general law cities follow the procedures outlined in the state code for an initiative to become eligible for the ballot. While charter cities have the option of having their own specific set of requirements that do not directly contravene the state code, most follow the set of requirements outlined in the state code. About 17 percent of local initiatives cover local government structure and organization. The local initiative process is most common in large, growing, and economically diverse cities with large public sectors; however, local income and other local political institutions play only a small role. Local initiatives can appear on the ballot in state and local (concurrent) or local-only (nonconcurrent) elections. For an overview of the local initiative process in cities throughout the United States, see Matsusaka (2003).

elected position is filled with a candidate of the city council's choosing on the expiration of the current treasurer's term.

Whether a referendum that would be very close to passing does, indeed, barely pass (or, conversely, barely fail) is unlikely to be driven by changes in the performance of the local economy, fiscal policy, other city institutions, or pressure from special interest groups. Referenda may pass or fail for a variety of reasons. The work of Bowler and Donovan (1998), Gerber (1999), and Nicholson (2005) shows that many features of ballot measures unrelated to the actual policy issue being voted on can affect their passage. For example, the salience of an issue on the national or state agenda or cues in the question text can determine the passage of a ballot measure. Marginal referenda may pass or fail depending on the public's view of bureaucrats at the time of the election, how other issues in the election crowd in or crowd out attention to the measure, the views of the swing voters who turn out to vote, and so on.

Debates between advocates and opponents of appointive treasurer referenda emphasize the trade-off between direct accountability to the electorate and expertise in financial matters. For example, in recent votes on treasurer ballot measures in the California cities of Maywood and Colverdale, advocates assert that "[t]he benefit to the community [Maywood] to appoint the City Treasurer position and consolidate duties is that, in addition to the cost savings, it will ensure that the office of City Treasurer is filled by a qualified individual who can competently and accurately perform the duties required" (Smart Voter 2011) and that "[t]he only performance evaluation an elected City Treasurer receives is at the ballot box every four years. However, when we are talking about the City's [Maywood's] finances, we need people who are educated in accounting, budgeting, and financial management, and there needs to be oversight of their performance on a day-to-day basis" (Smart Voter 2010). In contrast, opponents argue, "Support a transparent, accessible government with integrity and accountability [in Maywood]. Absent our voice, we can't express our will" (Smart Voter 2011), and "This is a flawed idea. It is more important than ever that the voters [in Cloverdale] have a Treasurer who does not answer to the City Manager or the City Council. You need a representative who can bring issues to attention of the taxpayers without worrying about job security or political backlash" (Smart Voter 2010). Similar points are raised elsewhere. In the campaign for an appointive treasurer ballot in San Bernardino, appointive advocates argue that "San Bernardino can no longer afford to waste our scarce tax dollars on politicians who are focused on their political agenda instead of making our city better," but opponents counter, "Keep your right to vote. Vote no on measure C."²⁰ Other points made concern the costs of each system in terms of staffing requirements, as well as the existence of other checks and balances on policy

²⁰ Ballotpedia, San Bernardino Appointed Clerk, Treasurer, and Attorney, Measure C (November 2010) ([http://ballotpedia.org/wiki/index.php/San_Bernardino_Appointed_Clerk,_Treasurer_and_Attorney,_Measure_C_\(November_2010\)](http://ballotpedia.org/wiki/index.php/San_Bernardino_Appointed_Clerk,_Treasurer_and_Attorney,_Measure_C_(November_2010))).

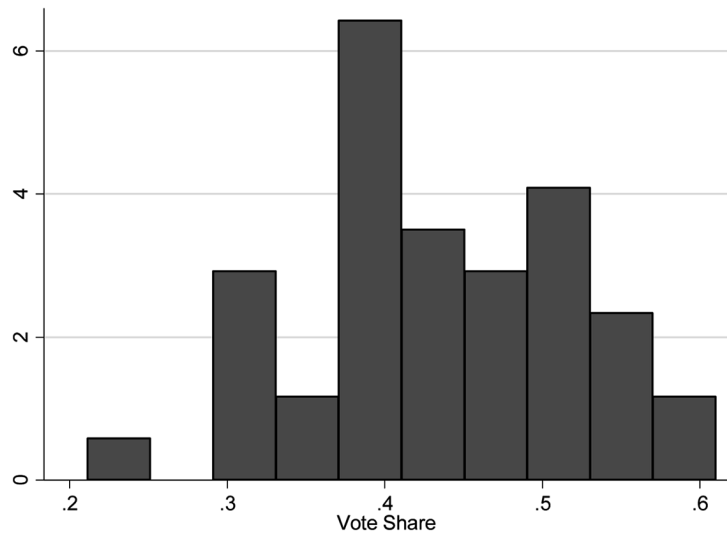


Figure 2. Appointive treasurer referendum vote distribution

makers’ behavior. There is little discussion of the performance of the incumbent treasurer.

I list the treasurer referenda in Table A2.²¹ During my sample period, there are 43 appointive treasurer referenda, occurring in over 20 percent of the general law cities in California. Eleven of the appointive treasurer referenda passed. Many referenda failed, likely because many Americans view bureaucrats negatively, as Wilson (1989) and others have noted. The distribution of the vote shares is plotted in Figure 2, which is derived from my calculations based on data from the California Elections Data Archive for 1995–2006.²² This distribution indicates that many of the referenda are quite close to the passing threshold, which is particularly useful for the RD estimation strategy to be compelling.

As my RD analysis focuses on the cities that have elected treasurers in 1992 and subsequently vote on a appointive treasurer referendum, a natural question to ask is whether these cities are representative of those with elected treasurers. In Table A3, I present descriptive statistics for cities with an elected treasurer in 1992 that do and do not subsequently vote on a referendum. By and large the cities are very similar, though the cities that vote on a referendum have somewhat lower levels of income per capita and lower education levels.

²¹ I discuss details of the exact data source and sample selection in Section 4.
²² Sacramento State University, Center for California Studies, California Elections Data Archive (http://www.csus.edu/calst/cal_studies/CEDA.html).

4. Data

The empirical analysis in this paper focuses on the effect of appointive treasurer referendum passage on borrowing costs. In general, data on vote shares and the question text for local referenda are not compiled by state agencies. Fortunately, the newly available California Elections Data Archive (CEDA) database compiled by the Center for California Studies in cooperation with the California secretary of state contains the key information required for this study. This database contains information on the date, content, and vote outcomes for all local initiatives and referenda appearing on the ballot in California cities from 1995 until 2006.²³ I obtain my measure of whether an appointive city treasurer referendum passed and the vote share from this source.

For each city, I match the referendum data to data on borrowing costs for fiscal years 1992–2008 obtained from California City Financial Transactions, 1991–Present (CCFT).²⁴ The CCFT is a uniquely detailed database with extensive coverage of many financial variables collected annually by the California state controller for each city. All 475 cities in California are required to file a report. More than 99 percent of cities file a financial transactions report in any given year.²⁵ I use variables for total interest expenditures, total outstanding debt, total expenditures, and total local source revenue, all based on the CCFT data. I construct my measure of cities' borrowing costs, the average interest rate paid on the stock of cities' debt, by dividing the total interest expenditure by total outstanding debt. Importantly, this interest rate measures the borrowing costs that cities actually pay, including both the gross margin that financial intermediaries charge as well as payments to investors. To match the data sources with one another, I code the passage of a referendum on the basis of the fiscal year as defined in the CCFT data.²⁶

Together, the data from CCFT and CEDA form the panel of California cities that I use in my central analysis. I match in data from two additional sources. First, I match in data from the 1990 decennial census to measure population, demographic, and economic characteristics of each city. Second, I match in data from the 1992 Census of Governments (U.S. Bureau of the Census 1994) on city government organization. I use this data set to measure the baseline method of selection of the city treasurer position.

I construct my analysis sample in the following way. I first use California cities that appear in the 1992 Census of Governments (U.S. Bureau of the Census

²³ As the treasurer selection methods change 2 years after the referendum is voted on, I am unable to use any of the post-2006 referenda. For years prior to 1995, I use the 1992 Census of Governments (U.S. Bureau of the Census 1994) data to measure Treasurer Appointed.

²⁴ RAND California Government Finance Statistics, California City Financial Transactions (CCFT), 1991–Present (<http://ca.rand.org/stats/govtfin/cityfinance.html>).

²⁵ California code (Cal. Gov. Code sec. 12463) directs the California state controller to annually compile and report to the public the financial transactions of all California cities.

²⁶ The city fiscal year runs from July to July, so, for example, I code referenda occurring between July 1995 and the end of June 1996 as occurring in the 1996 fiscal year. I use the 1996 fiscal year as the first year of the sample, as I do not have referendum data for the entire 1995 fiscal year.

1994) as my base population of cities, which is 456 cities. The few cities that incorporated after 1992 are not in the sample. I also drop charter cities, as the referendum process and question as specified applies only to general law cities, and charter cities are free to redefine the duties and role for the city treasurer.²⁷ This gives a sample of 361 cities. I then match these data to 1990 decennial census, CCFT, and CEDA data by city. I drop the cities without reported interest costs in at least three-quarters of the sample years, as a number of smaller cities do not have outstanding debt in every year. My final analysis sample contains 352 cities, with 43 appointive treasurer referenda taking place in 36 cities in the sample.

Table 1 presents descriptive statistics for all of the cities in my sample. Columns 1 and 2 show the means and standard deviations computed over all city-year observations dividing cities by method of selection for treasurer.²⁸ The comparison yields a number of interesting results. First, cities with elected treasurers pay higher borrowing costs. The borrowing costs in cities with elected treasurers are more than 25 percent (52 basis points) greater than those in cities with appointed treasurers. Cities that have elected and appointed treasurers are also somewhat different in other ways. Cities with elected treasurers tend to have more debt and are more likely to have directly elected mayors and clerks. Cities with elected treasurers also have lower levels of income per capita and a less educated population. These differences could well make cities with elected treasurers subject to more default risk, leading to higher borrowing costs regardless of their method of treasurer selection. Thus, the descriptive statistics indicate that cities with appointed treasurers pay lower borrowing costs but differ on key observable measures that are likely to affect borrowing costs independently of their method of treasurer selection.

5. Results

5.1. Main Results

Table 2 presents OLS estimates of δ from fitting various version of equation (1). We can see from column 1 that cities with appointed city treasurers pay less to borrow. The result in column 2 shows that the estimate changes little when the baseline variables measuring economic and demographic characteristics of cities are included. As city fiscal policy is likely related to borrowing costs and may be related to the choice of an appointed city treasurer, the specification for the result in column 3 adds controls for city spending, debt, and local taxes.

²⁷ General law cities follow the local government procedures and policies in the California State Code, whereas charter cities may adopt any policies and procedures that do not directly contradict state law.

²⁸ The method of selection for treasurer in a given year is defined from the baseline method-of-selection variable and any subsequent passing appointive treasurer referenda.

Table 1
Descriptive Statistics by Appointive City Treasurer Status

	Appointed (1)	Elected (2)	(1) – (2) (3)
Outcome: Interest Rate Paid on Total Outstanding Debt	1.68 (2.25)	2.20 (2.43)	–3.64 [.000]
City finances (2000 \$):			
Total Expenditures per Capita	1,218 (1,700)	1,443 (2,231)	–1.27 [.206]
Total Outstanding Debt per Capita	1,032 (1,850)	1,441 (2,804)	–2.09 [.037]
Total Local Source Revenue per Capita	605 (1,136)	633 (1,043)	–.26 [.793]
City government characteristics:			
Mayor-Council Form of Government	.08 (.28)	.24 (.43)	–4.06 [.000]
Clerk Appointed	.93 (.25)	.13 (.33)	27.06 [.000]
Full-Time Elected Officials	.04 (.19)	.02 (.14)	1.00 [.319]
City economic characteristics:			
Population	30,314 (34,092)	26,917 (30,726)	.99 [.324]
Income per Capita (1990 \$)	19,986 (14,352)	14,757 (7,685)	4.37 [.000]
Percentage College Graduate	.25 (.18)	.17 (.12)	4.67 [.000]
Employment-Population Ratio	.46 (.09)	.44 (.08)	2.55 [.011]
Observations	3,290	2,694	

Source. Calculations are based on data from the California City Financial Transactions, 1991–Present database for 1992–2008, the California Elections Data Archive for 1995–2006, the 1990 decennial census, and the 1992 Census of Governments (U.S. Bureau of the Census 1994).

Note. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments (U.S. Bureau of the Census 1994) and positive interest expenses in each year. The unit of observation is city-year. Columns 1 and 2 present means, with standard deviations in parentheses. Column 3 presents *t*-statistics for a test of differences, with *p*-values in brackets.

The results change little with the inclusion of these fiscal policy controls. The appointive effect remains negative and statistically significant.

The results in Table 2 also reveal that the magnitude of the appointive effect changes little across the alternative specifications. Relative to the baseline level of borrowing costs, the estimated reductions range from 19 to 27 percent. Thus, the appointive effects in Table 2 also appear to be economically significant. As interest payments in cities with elected treasurers' account for nearly \$43 per capita, the median estimates imply that having appointive treasurers reduces annual interest expenditures by more than \$8 per capita, or \$230,000 for the city in total. If we make the plausible assumption that there are no general equilibrium effects of appointive treasurer status, we can get a sense of the overall effects that the OLS estimates imply. The results imply that if all 155 cities with

Table 2
The Effect of Appointive City Treasurers on a City’s Borrowing Costs

	(1)	(2)	(3)
Treasurer Appointed	-.52** (.14)	-.40** (.15)	-.37* (.14)
1990 Decennial census controls	No	Yes	Yes
Fiscal policy controls	No	No	Yes

Source. Calculations are based on data from the California City Financial Transactions, 1991–Present database for 1992–2008, the California Elections Data Archive for 1995–2006, the 1992 Census of Governments (U.S. Bureau of the Census 1994), and the 1990 decennial census.

Note. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments (U.S. Bureau of the Census 1994) reporting interest expenses for at least 14 years. The dependent variable is Interest Rate Paid on Total Outstanding Debt. Year fixed effects are included and city fixed effects are excluded for all regressions. The unit of observation is city-year. Each column presents the results from one regression specification of equation (1). Values are coefficients, with standard errors clustered at the city level in parentheses. Observations = 5,907, cities = 352, and baseline dependent variable mean = 1.94 (SD = 3.26).

* Significant at the 5% level.
** Significant at the 1% level.

elected treasurers in the baseline period were to switch to appointed city treasurers, total annual borrowing expenditures would fall by over \$35 million.

Before turning to the other estimates, I conduct one further exercise to examine whether the OLS specifications in Table 2 are likely to suffer from omitted-variable bias. To shed light on this question, I estimate OLS models similar to those in Table 2 but add Clerk Appointed and Treasurer Appointed, and those results are shown in Table 3. As cities that have appointed treasurers also often have appointed clerks, any unobservable determinants of the adoption of appointive officials are likely common to both offices. However, because city clerks are not involved in debt management policy, we would expect that, conditional on the control variables, there is no appointive effect for city clerks. By continuing to include Treasurer Appointed, we can see how similar the estimated coefficients for different positions—with and without responsibility for debt management policy—actually are.

These results show that the city clerk effect is never statistically significant. Moreover, the point estimates on Clerk Appointed are all positive and near zero. While the point estimates are quite different, the statistical tests on the different coefficients on Treasurer Appointed and Clerk Appointed do not have sufficient power to reject the possibility that they are the same. Thus, the appointive clerk effect estimates suggest that the OLS estimates of the appointive treasurer effect is not likely to be confounded by omitted-variable bias. Of course, as these results are only suggestive, further investigation is warranted.

I next turn to a city fixed-effects specification using equation (2). This model is identified from variations in the political institutions within a city, and thus all time-invariant city characteristics are effectively controlled for. The result in column 1 of Table 4 reveals a negative and statistically significant relationship

Table 3
Comparing the Effects of Appointive City Treasurers and
Clerks on a City's Borrowing Costs

	(1)	(2)	(3)
Treasurer Appointed	-.51* (.26)	-.45 ⁺ (.24)	-.43 ⁺ (.24)
Clerk Appointed	.00 (.25)	.07 (.24)	.08 (.24)
H_0 : Treasurer Appointed = Clerk Appointed (F -Statistic)	1.10	1.29	1.25
p -Value	[.2953]	[.3562]	[.265]
1990 Decennial census controls	No	Yes	Yes
Fiscal policy controls	No	No	Yes

Source. Calculations are based on data from the California City Financial Transactions, 1991–Present database for 1992–2008, the California Elections Data Archive for 1995–2006, the 1992 Census of Governments (U.S. Bureau of the Census 1994), and the 1990 decennial census.

Note. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments (U.S. Bureau of the Census 1994) reporting interest expenses for at least 14 years. The dependent variable is Interest Rate Paid on Total Outstanding Debt. The unit of observation is city-year. Year fixed effects are included and city fixed effects are excluded for all regressions. Each column presents the results from one regression specification of equation (1). Values are coefficients, with standard errors clustered at the city level in parentheses. Observations = 5,907, cities = 352, and baseline dependent variable mean = 1.94 (SD = 3.26).

⁺ Significant at the 10% level.

* Significant at the 5% level.

between having an appointed city treasurer and a city's borrowing costs. In column 2, the results echo those in Table 3, as the Treasurer Appointed coefficient is negative and statistically significant at the 10 percent level and the Appointed Clerk coefficient is positive and statistically insignificant. Again, the hypothesis test does not have sufficient power to rule out the possibility that the clerk and treasurer coefficient estimates are the same, even though the point estimates differ substantially.

I present the RD estimates in Table 5. I show the estimates of δ from fitting two versions of equation (2), with and without baseline 1990 census controls. In column 1, I present the RD estimates with a vote share control only. This estimate indicates that appointed city treasurers do indeed cause a city's borrowing costs to be lower. Again, the effect appears to be both statistically and economically significant. The passage of an appointive treasurer referendum reduces borrowing costs by 22 percent of the baseline. Similarly, the result in column 2 shows that adding 1990 census controls does little to alter the statistical significance of the effect.²⁹ In addition, the magnitude of the effect remains above 20 percent of the baseline. The baseline RD result in column 1 of Table 5 is

²⁹ The value of adding time-invariant census controls is to improve precision of the estimates. One potential concern that would violate the RD design is that the covariates change discretely at the ballot measure passage margin. Fortunately, the results in Table A4 show that none of the covariates change discretely at the ballot measure passage margin.

Table 4
The Effect of Appointive City Treasurers on a City’s Borrowing
Costs: City Fixed Effects

	(1)	(2)
Treasurer Appointed	−.60* (.27)	−.65+ (.35)
Clerk Appointed		.11 (.45)
H_0 : Treasurer Appointed = Clerk Appointed (F -Statistic)		1.14
p -Value		[.2863]

Source. Calculations are based on data from the California City Financial Transactions, 1991–Present database for 1992–2008, the California Elections Data Archive for 1995–2006, the 1992 Census of Governments (U.S. Bureau of the Census 1994), and the 1990 decennial census.

Note. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments (U.S. Bureau of the Census 1994), interest expenses reported for 14 years, and an appointive treasurer referendum voted on. The dependent variable is Interest Rate Paid on Total Outstanding Debt. Year fixed effects and city fixed effects are included for both regressions; fiscal policy controls and 1990 decennial census controls are excluded. The unit of observation is referendum-year. The base regression specification is equation (2). Values are coefficients, with standard errors clustered at the city level in parentheses. Observations = 5,907, cities = 352, and baseline dependent variable mean = 1.94 (SD = 3.26).

+ Significant at the 10% level.

* Significant at the 5% level.

depicted visually in Figure 3.³⁰ The top graph clearly shows the positive vote share-interest rate gradient and the discontinuous drop in borrowing costs that occurs at the referendum passage threshold.

One potential concern with use of annual data in the RD analysis is that clustering the standard errors at the city level may not be sufficient to address the within-city correlation generated by using multiple observations from a city. To address this concern, I also implement the more conservative method of estimating the models on a cross-sectional postreferendum sample and present the results in Table A5.³¹ If the clustering procedure is effectively addressing the inference issues, I would expect to obtain very similar results. This is what we see in Table A5, where the point estimates and standard errors are very similar to those in Table 5 and are depicted in Figure 3B.

The fact that the OLS, fixed-effects, and RD estimates are quite comparable is interesting. This finding is particularly notable, as the different analyses address institutional endogeneity in very different ways and are necessarily conducted

³⁰ Figure 3 is derived from my calculations based on data from the CCFT for 1992–2008 and the California Elections Data Archive (CEDA) for 1995–2006. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments (U.S. Bureau of the Census 1994), positive interest expenses in 14 years, and an appointive treasurer referendum put to voters in a prior year. The figure plots Interest Rate Paid on Total Outstanding Debt by the margin of victory in the appointive treasurer referendum. In both graphs, the line plots the predicted values of a regression of the interest rate on a referendum pass indicator and a linear trend referendum vote share. In the Figure 3B, the data points are sized to indicate the number of postreferendum observations used to construct the postreferendum mean and weight the regression estimates.

³¹ I follow Angrist and Pischke (2009) and Angrist and Lavy (2009) in weighting the estimates by group size to account for the fact that some cities have more postballot observations than others.

Table 5
The Effect of Appointive City Treasurers on a City's Borrowing Costs: Regression Discontinuity Estimates

	(1)	(2)
Appointive Treasurer Referendum Pass	-.72* (.31)	-.66* (.28)
1990 Decennial census controls	No	Yes

Source. Calculations are based on data from the California City Financial Transactions, 1991–Present database for 1992–2008, the California Elections Data Archive for 1995–2006, the 1992 Census of Governments (U.S. Bureau of the Census 1994), and the 1990 decennial census.

Note. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments (U.S. Bureau of the Census 1994), reported interest expenses for 14 years, and an appointive treasurer referendum voted on. The regression discontinuity dependent variable is Interest Rate Paid on Total Outstanding Debt. Year fixed effects are included for both regressions. The unit of observation is referendum-year. Each column presents the results from one regression specification of equation (3). The models also control for a linear trend in the vote share. Values are coefficients, with standard errors clustered at the city level in parentheses. Observations = 295, cities = 36, and baseline dependent variable mean = 3.28 (SD = 4.27).

* Significant at the 5% level.

on quite different samples of cities. The similarity of the estimates provides reassurance that the RD estimates are not local to only the set of cities that put an appointive treasurer referendum to the voters. Furthermore, the similarity of the estimates across the approaches also suggests that any endogeneity of treasurer selection method adoption has little meaningful effect on the coefficient estimates. Even though the estimates appear to be very similar, I next present a series of robustness checks to probe the validity of the RD estimates.

5.2. Robustness Tests

While the passage of the referendum by a small margin is unlikely to be affected by significant differences in the effectiveness of a given treasurer, city fiscal policy, or other institutional characteristics, one might still be concerned that the narrow passage of an appointive treasurer referendum may be related to discontinuous changes in other key confounding variables. I next estimate a number of models to address these potential concerns.

5.2.1. Cities' Policy Outcomes Prior to Referendum Voting

One advantage of having panel data on cities' policy outcomes is that I can test for a discontinuous jump in city borrowing costs at the referendum passage threshold before the referendum is actually voted on. If the narrow passage of a city treasurer referendum simply reflects a discontinuity in unobservable determinants of a city's borrowing costs, the RD identification strategy would be threatened.

I estimate versions of equation (3) using only prereferendum data on cities' borrowing costs. The results shown in Table 6 indicate that no statistically sig-

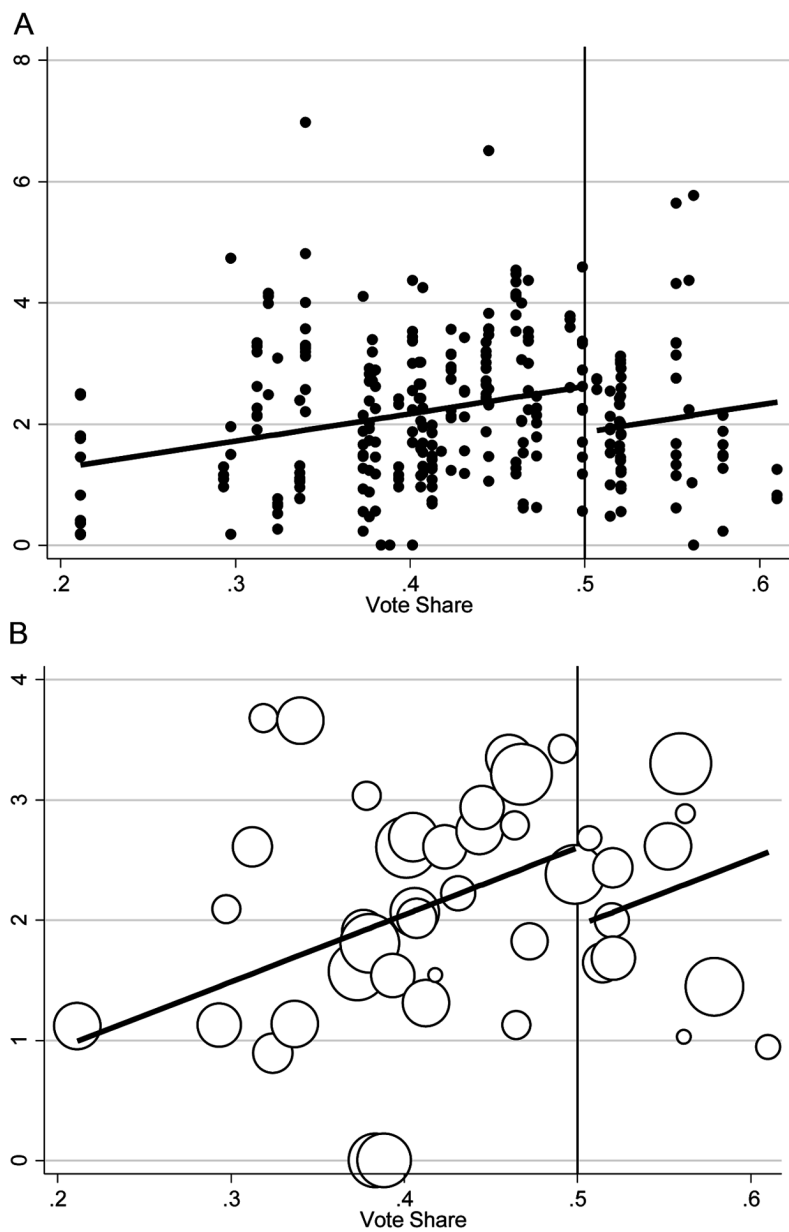


Figure 3. Appointive treasurer effect, annual (A) and cross-sectional (B) data

Table 6
The Effect of Appointive City Treasurers on a City's
Borrowing Costs: Robustness Tests

	(1)	(2)
Interest Rates Prior To Referendum: Future		
Appointive Treasurer Referendum Pass ^a	-.08 (.81)	.01 (.68)
With 45% vote share discontinuity: ^b		
Appointive Treasurer Referendum Pass	-.81* (.34)	-.74** (.27)
Vote share greater than 45%	.31 (.38)	.54 (.27)
With 55% vote share discontinuity: ^b		
Appointive Treasurer Referendum Pass	-.69* (.32)	-.75* (.33)
Vote share greater than 55%	-.09 (.40)	.26 (.31)
Comparison of appointive treasurer referendum and appointive clerk referendum: ^c		
Appointive Treasurer Referendum Pass	-.48 (.50)	-1.09+ (.58)
Appointive Clerk Referendum Pass	-.06 (.48)	.65 (.69)
H_0 : Appointive Treasurer Referendum Pass = Appointive Clerk Referendum Pass	.23	2.04
SD	[.636]	[.164]
1990 Decennial census controls	No	Yes

Source. Calculations are based on data from the California City Financial Transactions, 1991–Present database for 1992–2008, the California Elections Data Archive for 1995–2006, the 1992 Census of Governments (U.S. Bureau of the Census 1994), and the 1990 decennial census.

Note. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments (U.S. Bureau of the Census 1994), positive interest expenses in each year, and an appointive treasurer referendum put to voters in a future year. The dependent variable is Interest Rate Paid on Total Outstanding Debt. Year fixed effects are included for both regressions. The models also control for a linear trend in the vote share. The unit of observation is referendum-year. Each column presents the results from one regression specification of equation (3). Values are coefficients, with standard errors clustered at the city level in parentheses.

^a Observations = 429, cities = 36, and baseline dependent variable mean = 3.10 (SD = 3.37).

^b Observations = 295, cities = 36, and baseline dependent variable mean = 3.28 (SD = 4.27).

^c Observations = 224, cities = 29, and baseline dependent variable mean = 2.88 (SD = 3.69).

+ Significant at the 10% level.

* Significant at the 5% level.

** Significant at the 1% level.

nificant discontinuity exists in borrowing costs at the threshold of passage of the appointive referendum prior to the vote. Moreover, the point estimates are very close to zero with the sign being specification dependent. The relationship between prior interest rates and the future referendum vote share and passage is shown in Figure 4.³² Figure 4A shows clearly the lack of a significant discon-

³² The cross-sectional counterpart to Table 6 is Table A6, and the cross-sectional counterpart to Figure 4A is Figure 4B. Figure 4 is derived from my calculations based on data from the CCFT for

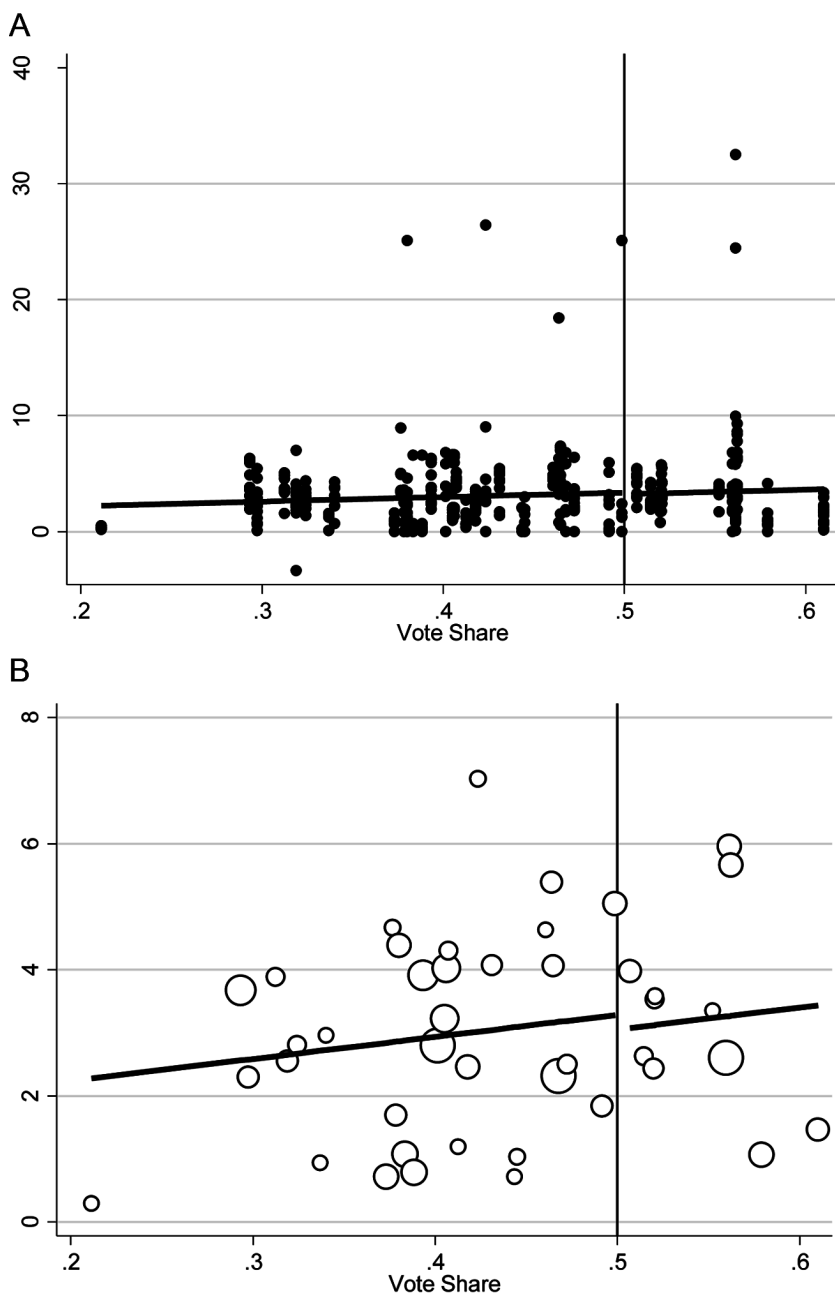


Figure 4. Appointive treasurer effect prior to appointive treasurer measure vote, annual (A) and cross-sectional (B) data.

tinuity in interest rates prior to voting on the referendum. The fact that there is little evidence of a significant discontinuity in the main outcome prior to the referendum lends credence to the identification assumption.

5.2.2. Other Discontinuities

While the lack of a preexisting discontinuity seems to rule out significant violations of the smoothness assumption inherent in an RD approach, it is also possible that cities' interest rates change discontinuously at natural vote share discontinuities after the vote. To rule out this concern, I add controls for two other nearby vote share discontinuities to test for similar effects when the treasurer position does not become appointed. Results for models with 45 percent and 55 percent discontinuities added are reported in Table 6. Little evidence for similar discontinuities at these vote shares lends further credence to the RD approach.

5.2.3. Other City Institutional Changes

The next threat to identification I consider is that the narrow passage of a treasurer referendum is tied to other institutional changes or reforms that might affect a city's cost of borrowing. As noted by Acemoglu and Robinson (2005), institutional changes are often bundled together. As a city council decides whether to place an appointive treasurer referendum before voters, this decision may be correlated with changes to other institutions that might also affect the costs of borrowing, such as city budgetary institutions. In this case, the estimated appointive treasurer effect may simply reflect other correlated changes in city government budgetary institutions rather than changes in the method of selection of the city treasurer.

To shed light on this issue and as a falsification check, I again examine whether the narrow passage of an appointive clerk referendum has a effect similar to that for narrow passage of an appointive treasurer referendum. I present the results of the RD models including the appointed clerk variables in Table 6.³³ I then estimate an RD model similar to equation (3) in which the referendum pass and vote share variables from the appointive clerk referendum are added to the

1992–2008 and the CEDA for 1995–2006. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments (U.S. Bureau of the Census 1994), positive interest expenses in 14 years, and an appointive treasurer referendum put to voters in a future year. Interest Rate Paid on Total Outstanding Debt is plotted by the margin of victory in the appointive treasurer referendum. The line plots the predicted values of a regression of the interest rate on a future referendum pass indicator and a linear trend referendum vote share. In Figure 4B, the data points are sized to indicate the number of prereferendum observations used to construct the prereferendum mean and weight the regression estimates.

³³ The sample for this analysis is somewhat smaller, as I examine only cities where the treasurer and clerk positions were both elected in 1992.

model.³⁴ The results reveal little statistically significant effect of appointed clerks on a city's borrowing costs. The fact that the point estimates for this model are positive provides further reassurance that the appointive city treasurer effect shown in Table 5 is not simply due to other unobservable institutional changes.

5.2.4. Alternative Specifications and Samples

I examine whether the RD results are robust across alternative samples and specifications and present the evidence in Table 7. I first consider whether the results differ in the sample of cities by closeness of the referenda's passage or failure. The central identification assumption in my RD analysis is that borrowing costs in cities that narrowly fail to pass a referendum form a valid counterfactual for those that narrowly do pass a referendum. Thus far, I have used all the referenda available to conduct my analysis and relied on the vote share control to smooth other unobserved determinants of borrowing costs. It is natural to ask whether the appointive effect is similar in the smaller sample of cities in which referenda barely pass and barely fail. I next estimate my models on various samples of cities where referenda had narrow margins of victory for different bandwidths in the vote share. There is a key trade-off inherent in bandwidth selection. When compared with broad bandwidths, narrow bandwidths are more likely to satisfy the identification assumption and lead to less biased estimates but with less data are less precisely estimated. Wider bandwidths are less likely to satisfy the identification assumption and lead to more biased estimates but are more precisely estimated as they use more data. The results in Table 7 seem to reflect this trade-off. The point estimates generally move toward zero as bandwidth increases, while the standard errors fall. Thus, while all the point estimates are negative, only one is statistically significant at the 5 percent level.³⁵

I next examine whether the results are robust to excluding extreme values of the borrowing costs and cities with multiple referenda from the sample. In column 1, the effects remain statistically significant at the 10 percent level for each of the smaller samples. In column 2, while the estimates are not statistically significant when the census controls are added and the cities with multiple referenda are dropped, they remain statistically significant when the outliers are dropped. In addition, as the point estimates in Table 7 are generally very close to those in Table 5, any differences in statistical significance are likely due to a loss of precision related to the smaller sample rather than substantively different estimates.

³⁴ A number of cities either put an appointive treasurer referendum and an appointive clerk referendum to the voters in the same election or put a single referendum question to the voters that would change both positions to become appointed. To the extent that appointive clerk and treasurer referenda are voted on jointly, we would expect that the passage of a treasurer or clerk referendum would have the same measured impact. However, as the list of treasurer referenda in Table A2 shows, in practice only a few of the appointive treasurer referenda are also appointive clerk referenda.

³⁵ The minimum number of clusters necessary for asymptotic distributions to be appropriate is still under discussion in the literature. Recent Monte Carlo evidence from Hansen (2007) indicates that asymptotic distributions for clustered standard errors perform well with as few as 10 clusters.

Table 7
The Effect of Appointive City Treasurers on a City's Borrowing Costs:
Regression Discontinuity Estimates with Alternative
Specifications and Samples

Appointive Treasurer Referendum Pass	(1)	(2)
15% Margin of victory sample: ^a	-.12 (.24)	-.24 (.16)
10% Margin of victory sample: ^a	-.32 (.23)	-.30* (.14)
5% Margin of victory sample: ^a	-.64 (.36)	-.13 (.17)
Sample excluding cities with multiple referenda: ^a	-.76 ⁺ (.37)	-.56 (.38)
Sample excluding interest rate outliers: ^a	-.77** (.31)	-.58* (.28)
1990 Decennial census controls	No	Yes

Source. Calculations are based on data from the California City Financial Transactions, 1991–Present database for 1992–2008, the California Elections Data Archive for 1995–2006, the 1992 Census of Governments (U.S. Bureau of the Census 1994), and the 1990 decennial census.

Note. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments (U.S. Bureau of the Census 1994), positive interest expenses in 14 years, and an appointive treasurer referendum voted on. The dependent variable is Interest Rate Paid on Total Outstanding Debt. Year fixed effects are included for both regressions.

^a Observations = 234, cities = 29, and baseline dependent variable mean = 3.11 (SD = 2.26).

^b Observations = 185, cities = 24, and baseline dependent variable mean = 3.86 (SD = 4.80).

^c Observations = 83, cities = 12, and baseline dependent variable mean = 3.11 (SD = 2.26).

^d Observations = 210, cities = 30, and baseline dependent variable mean = 3.40 (SD = 4.65).

^e Observations = 284, cities = 35, and baseline dependent variable mean = 3.72 (SD = 2.13).

⁺ Significant at the 10% level.

* Significant at the 5% level.

** Significant at the 1% level.

5.3. Other Policy Outcomes

5.3.1. Fiscal Policy Outcomes

The results thus far have indicated that appointed city treasurers implement debt management policies that result in lower borrowing costs. I next turn to the question of whether the treasurer selection method also indirectly affects fiscal policy. These effects are of interest for two reasons. First, it is interesting to know whether reducing the number of elected officials in a city affects fiscal policy. Of course, as the city treasurer is not part of the city council, we might expect the effects to be less significant than Berry and Gersen (2009) find for electoral density in general. Second, voters might hold treasurers accountable by observing the indirect effects of treasurers' policy choices through changes in fiscal policy rather than debt management policy per se. For this form of indirect accountability (with appropriate attribution) to be effective, fiscal policy would need to respond to changes in debt management policy.

In Table 8, I present OLS and RD estimates of the appointive treasurer effect for fiscal policy. We see that there is little evidence of an effect on total expenditures, which likely indicates that the reduction in borrowing costs is spent on

Table 8
The Effect of Appointive City Treasurers on City Fiscal Policy

	OLS (1)	RD (2)	RD (3)
Total Expenditures per Capita:			
Treasurer Appointed	−163.41 (164.02)		
Appointive Treasurer Referendum Pass		108.23 (259.21)	181.05 (234.60)
Observations	5,940	295	295
Cities	352	36	36
Baseline dependent variable mean	894	900	900
SD	[678]	[361]	[361]
Total Local Source Revenue per Capita:			
Treasurer Appointed	−24.72 (114.65)		
Appointive Treasurer Referendum Pass		51.64 (89.54)	80.10 (75.31)
Observations	5,940	295	295
Cities	352	36	36
Baseline dependent variable mean	468	374	374
SD	[642]	[158]	[158]
Total Outstanding Debt per Capita:			
Treasurer Appointed	−362.97 (180.17)		
Appointive Treasurer Referendum Pass		−505.18 (568.13)	−210.76 (562.62)
Observations	5,940	295	295
Cities	352	36	36
Baseline dependent variable mean	843	745	745
SD	[1,734]	[731]	[731]
1990 decennial census controls	Yes	No	Yes
Sample	All cities	Referendum cities	Referendum cities

Source. Calculations are based on data from the California City Financial Transactions, 1991–Present database for 1992–2008, the California Elections Data Archive for 1995–2006, the 1992 Census of Governments (U.S. Bureau of the Census 1994), and the 1990 decennial census.

Note. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments (U.S. Bureau of the Census 1994), positive interest expenses in each year, and an appointive treasurer referendum put to voters in a future year. Year fixed effects are included for all regressions. The models also control for a linear trend in the vote share. The unit of observation is referendum-year. Values are coefficients, with standard errors clustered at the city level in parentheses.

other public goods or tax revenue reduction. For locally generated revenue, the estimates are not statistically significant. The lack of a clear negative effect suggests that the reduction in borrowing costs is not passed on to taxpayers. For the stock of total outstanding debt, the results show little consistent evidence of an appointive city treasurer effect.

5.3.2. Debt Issuance and Management Costs

I next turn to the question of whether other costs related to debt management are related to the method of selection for treasurer, as interest rates are not the

only cost cities pay to borrow. Whether other costs of debt issuance respond to the method of selection is important for understanding the effect of the method of selection on total borrowing costs.

I consider two additional costs. I first examine whether cities with appointed treasurers spend more or less on per-issue fees to private-sector debt issuance consultants that form an important component of transactions costs. The effect could plausibly go in either direction. Effective city treasurers may be able to substitute their effort and expertise for that of a consultant, reducing city expenditures on advisory services. In contrast, effective city treasurers may be effective because they select higher quality financial advisors whose services may be more costly. It is also possible that private-sector debt consultants are able to extract rents from ineffective city treasurers who are not aware of the services that they really need or the price that they should be willing to pay. In addition, rents could be provided to advisors if appointed or elected city treasurers are captured by financial intermediaries.

In Table 9, I present the results of estimating OLS and RD models with the outcome for debt issuance consultant expenditures, similar to equations (1) and (3), respectively. The results clearly show little statistically significant effect of appointive treasurer referendum passage on the expenditure on debt consultants regardless of the precise specification.³⁶

Second, I examine whether the cost of government administration in the city treasurer's department increases with an appointed city treasurer. Department costs might change if the appointive city treasurer effect is due to hiring more or better quality staff in the finance department or spending more on the city treasurer position itself. In Table 9, I present the results of estimating OLS and RD models with the outcome for total expenditures on general government administration, similar to equations (1) and (3), respectively. The results are mixed. The OLS and unconditional RD estimates are not statistically significant. However, the conditional RD estimate indicates that appointive city treasurers do increase general government administration expenditures. The results in Table 9 do not provide consistent evidence of a strong appointive city treasurer effect on the cost of government administration.

6. Conclusion

This paper has documented that there are meaningful effects from bureaucratic control of cities' debt policy: assigning the debt task to a bureaucrat reduces cities' borrowing costs. The results demonstrate that appointive treasurers reduce a city's cost of borrowing. The appointive effect is also economically significant. The results imply that if all cities in California with elected treasurers were to

³⁶ Unfortunately, the data used in this study do not provide information on the identity of the financial advisor, so I am unable to explore the nature and impact of connections between advisors and treasurers in more detail.

Table 9
The Effect of Appointive City Treasurers on Other Borrowing Costs

	OLS (1)	RD (2)	RD (3)
Expenditures on Debt Consultants per Capita:			
Treasurer Appointed	−8.22 (10.84)		
Appointive Treasurer Referendum Pass		−3.85 (49.11)	−14.53 (44.59)
Observations	5,984	301	301
Cities	352	36	36
Baseline dependent variable mean	20.70	29.62	29.62
SD	[128.71]	[91.43]	[91.43]
Expenditures on Management and Support			
Department per Capita:			
Treasurer Appointed	−.26 (22.16)		
Appointive Treasurer Referendum Pass		45.36 (34.86)	77.03* (31.39)
Observations	5,940	295	295
Cities	352	36	36
Baseline dependent variable mean	91.64	73.91	73.91
SD	[117.90]	[48.37]	[48.37]
1990 decennial census controls	Yes	No	Yes
Sample	All cities	Referendum cities	Referendum cities

Source. Calculations are based on data from the California City Financial Transactions, 1991–Present database for 1992–2008, the California Elections Data Archive for 1995–2006, the 1992 Census of Governments (U.S. Bureau of the Census 1994), and the 1990 decennial census.

Note. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments (U.S. Bureau of the Census 1994), positive interest expenses in each year, and an appointive treasurer referendum put to voters in a future year. Year fixed effects are included for all regressions. The RD models also control for a linear trend in the vote share. The unit of observation is referendum-year. Values are coefficients, with standard errors clustered at the city level in parentheses.

* Statistically significant at the 5% level.

appoint them, total borrowing expenditures would be reduced by a figure on the order of \$40 million per year. Thus, this paper has demonstrated that alternative political institutions do indeed cause meaningful differences in policy making.

Importantly, the results from the two empirical approaches are very similar. The fact that the linear regression and the RD approaches yield such similar results indicates that institutional choices are not strongly related to unobservable determinants of policy making in the context of city treasurers. Of course, the endogenous choice of institutions may be a more substantial concern in other contexts, especially where policy making has a strong distributional component. A more definite conclusion awaits further evidence from other contexts.

There are several promising directions for future research. First, it would be interesting to examine whether similar differences in policy making are found for less technical policy choices. As the theoretical arguments noted in this paper indicate that politicians likely have a comparative advantage in less technical

policy areas, it would be interesting to examine empirical evidence on whether this is the case. Second, as highly detailed data on policy makers' expertise are unavailable for city treasurers, it would be interesting to examine how differences in expertise affect policy choices and performance in a context in which such measures are available.

The results of this study also have broader implications for the organization of public good provision. They suggest, for example, that the division of policy-making tasks typical in many advanced democracies, with appointed officials conducting the more technical monetary and regulatory policy and elected officials conducting the less technical fiscal policy, is appropriate. Efforts to improve governance in developing countries may well be enhanced by emulating the division of policy-making tasks in advanced democracies.

Appendix A

A Simple Model of Performance by Bureaucrat and Politician Treasurers

To guide the empirical analysis, here I outline a simple model of the effect on debt management policy of assigning the city treasurer task to a bureaucrat or politician. The model applies the agency framework developed by Alesina and Tabellini (2007) to the context of city treasurers and debt management policy.

The performance of a city treasurer is measured by the interest rate that a city pays on outstanding debt. City residents prefer to pay lower interest rates so that their tax burden is reduced or other public services that they value are provided. The policy outcome—the interest rate paid on a city's debt, i —is observable but not contractible, so society cannot write the optimal performance contract with the city treasurer to achieve the first-best effort level. Society can, however, choose whether to allocate the city treasurer task to a bureaucrat or politician.³⁷

The effort and ability of a city treasurer affect the interest rate that a city pays to borrow. In addition, the effort and ability of a treasurer determine borrowing costs through information collection and utilization in debt policy making, as well as through improved financial management and the hiring of appropriate financial advisors. Borrowing costs of a city are also determined by the exogenous default risk of a city. Default risk is determined by the level and stability of the tax base, as well as the level of debt and public spending of a city.

A1. Environment

Consider the policy outcome, i , the interest rate paid on a city's debt, which depends on the effort level chosen by the treasurer, a , and the treasurer's ability,

³⁷ It is also assumed that the participation constraint for both types of policy makers is satisfied but need not bind. Alesina and Tabellini (2007) note that since an optimal contract is not being considered, policy makers could be earning additional rents in equilibrium.

θ , and the exogenous default risk of a city, D . Citizens prefer to have less spent on debt interest and obtain utility from policy performance according to $u = U(i) = -i$. Policy performance is expressed in the following way:

$$i = D - \theta - a,$$

where θ is the random ability of the treasurer to execute the policy task and is uniformly distributed over $[\bar{\theta} - (x/2), \bar{\theta} + (x/2)]$. The interest rate on a city's debt falls with the ability and effort supplied by the treasurer and increases with the default risk of the city. The complexity of the debt management task is reflected by x , and the more complex the task, the greater the uncertainty that policy makers face about their ability to execute the task. Treasurers decide how much effort, a , to supply by maximizing their utility,

$$V(a) = R(a) - C(a),$$

where $R(a)$ is the reward function for effort and $C(a)$ is the cost of effort. The reward function for effort differs between politician treasurers, $R^P(a)$, whose goal is to be reelected, and bureaucrat treasurers, $R^B(a)$, whose goal is to increase future employers' perception of their talent, θ . The cost of effort function is the same for both types of treasurers, $C(a) = a^2$.

The timing of events is that first the policy is assigned to either a politician or a bureaucrat treasurer. Next, the treasurer chooses effort, a , before knowing his or her ability, θ . Finally, nature chooses θ , and policy outcomes are observed, and the rewards are paid.

A2. The Bureaucrat Treasurer

The reward function for bureaucrat treasurers is based on expected talent conditional on performance and the market value of ability. The reward function for bureaucrat treasurers is $R^B(a) = \alpha E^U(E^C(\theta|i))$, where E^U denotes the unconditional expectations over the random variable i and E^C denotes the conditional expectations over θ conditional on the realization of performance, i . The parameter α captures the valuation bureaucrats place on their performance either because of the value that current or future employers place on performance or because of the intrinsic motivation that they have for perceived performance. Denoting the public's perception of effort as a_e^B , we have $R^B(a) = \alpha E^U(-i - a_e^B + D) = \alpha E^U(\theta + a - a_e^B)$.

I can now compute the equilibrium level of effort for bureaucrat treasurers. Taking the first-order condition of $V^B(a)$ with respect to effort and then imposing the equilibrium condition $a = a_e$, I obtain the equilibrium effort for bureaucrat treasurers, $a^B = \alpha/2$. Thus, the equilibrium interest rate paid on a city's debt when the treasurer is a bureaucrat is given by

$$i^B = D - \bar{\theta}^B - \frac{\alpha}{2}, \quad (A1)$$

where D is the default risk of the city, $\bar{\theta}^B$ is the average ability of bureaucrat treasurers, and $\alpha/2$ is the equilibrium effort of bureaucrat treasurers.

A3. The Politician Treasurer

The reward function for politician treasurers is based on the value of the office and probability of reelection, so $R^P(a) = \beta \Pr(u \geq W)$, where $\Pr(u \geq W)$ is the probability of reelection. The parameter β reflects how valuable the office is to politicians because of ego rents that they receive. Politicians are reelected when their constituents' utility, $u = -i$, under the politician is greater than the utility from the policy performance of the average politician exerting equilibrium effort, so that $W = u(i^P) = -D + \bar{\theta} + a_e^P$. Thus, $R^P(a) = \beta[1 - \Pr(\theta \geq W - a + D)] = \beta[1 - \Pr(\theta \geq \bar{\theta} + a_e^P - a)]$. Using the uniform distribution of ability, we obtain $R^P(a) = \beta[(\bar{\theta} + a_e^P - a)/x]$. Like bureaucrats, politicians also choose effort before observing their talent.

We can now compute the equilibrium level of effort for politician treasurers. Taking the first-order condition of $V^P(a)$ with respect to effort and then imposing the equilibrium condition, $a = a_e^P$, we obtain the equilibrium effort for elected treasurers, $a^P = \beta/2x$. Thus, the equilibrium effort for politician treasurers increases with the value of the office, β , and falls with the complexity of the debt management task, x . The equilibrium interest rate paid on a city's debt when treasurers are politicians is then given by

$$i^P = D - \bar{\theta}^P - \frac{\beta}{2x}, \quad (\text{A2})$$

where D is the default risk of the city, $\bar{\theta}^P$ is the average ability of politician treasurers, and $\beta/2x$ is the equilibrium effort of politician treasurers.

A4. Appointive Effect

I can obtain an expression for the appointive effect for a city by taking the difference between equations (A1) and (A2). Doing so I obtain

$$\Delta i = i^B - i^P = -\left(\bar{\theta}^B - \bar{\theta}^P + \frac{\alpha}{2} - \frac{\beta}{2x}\right). \quad (\text{A3})$$

Equation (A3) demonstrates that the sign of the appointive effect is ambiguous. The appointive effect depends crucially on the sign and magnitude of both the expertise effect, $\bar{\theta}^B - \bar{\theta}^P$, and the effort effect, $(\alpha/2) - (\beta/2x)$, of bureaucratic versus political control. The central goal of the empirical analysis is to establish the sign and magnitude of Δi .

Table A1
Definitions of Variables and Controls

Variable or Control	Definition and Source
City finances:	
Interest Rate Paid on Total Outstanding Debt	Ratio of total interest expenses divided by the total outstanding debt in 2000 \$ (California City Financial Transactions, 1991–Present [CCFT])
Total Outstanding Debt per Capita	Total stock per capita in 2000 \$ of outstanding debt issued by the city; (CCFT)
Total Expenditures per Capita	Total expenditures per capita in 2000 \$ to deliver city services (CCFT)
Total Local Source Revenue per Capita	Total city revenue net of transfers from the federal, state, and county governments in 2000 \$ (CCFT)
Expenditures on Debt Consultants per Capita	Total per capita expenditures in 2000 \$ on debt consultants by the city (CCFT)
Expenditures on Management and Support Department per Capita	Total expenditures in 2000 \$ on government administration in the city's finance department per capita (CCFT)
City government characteristics:	
Mayor-Council Form of Government	Equals one if the city has a directly elected mayor and zero otherwise (U.S. Bureau of the Census 1994)
Treasurer Appointed	Equals one if the treasurer is appointed in city i in year t and zero otherwise (U.S. Bureau of the Census 1994)
Clerk Appointed	Equals one if the clerk is appointed in city i in year t and zero otherwise (U.S. Bureau of the Census 1994)
Elected City Clerk	Equals one if the clerk is elected in city i in year t and zero otherwise (U.S. Bureau of the Census 1994)
Pass	Equals one in every year after the referendum passes and zero otherwise (California Elections Data Archive [CEDA])
Appointive Treasurer Referendum Pass	Equals one if a local appointive treasurer referendum passed in city i before year t and zero otherwise (CEDA)
Future Appointive Treasurer Referendum Pass	Equals one if a local appointive treasurer referendum passed in a future year in city i in year t and zero otherwise (CEDA)

Appointive Clerk Referendum Pass Full-Time Elected Officials	Equals one if a local appointive clerk referendum passed in city i before year t and zero otherwise (CEDA) Equals one if the city's elected officials serve in a full-time capacity and zero otherwise (U.S. Bureau of the Census 1994)
City demographics:	City's population as estimated annually by the California Department of Finance (1990 decennial census)
Population	Per capita income in the city (1990 decennial census)
Income per Capita	Percentage of the population identified as white (1990 decennial census)
Percentage White	Percentage of the population identified as black (1990 decennial census)
Percentage Black	Percentage of the population identified as less than 17 years old (1990 decennial census)
Percentage Less than 17	Percentage of the population identified as more than 65 years old (1990 decennial census)
Percentage Greater than 65	Percentage of the population with a college degree (1990 decennial census)
Percentage College Graduate	Percentage of the population that graduated from high school (1990 decennial census)
Percentage High School Graduate	Ratio of employment to population (1990 decennial census)
Employment-Population Ratio	Population, Income per Capita, Percentage White, Percentage Black, Percentage Less than 17, Percentage Greater than 65, Percentage College Graduate, Percentage High School Graduate, Mayor-Council Form of Government, and Employment-Population Ratio
1990 Decennial census controls	Annual measures of Total Expenditures per Capita, Total Outstanding Debt per Capita, and Total Local Source Revenue per Capita
Fiscal policy controls	

Table A2
California Appointive City Treasurer Referenda

Date	City	Vote Yes	Vote No	Total Votes	Percentage in Favor	Referendum Pass	Joint Referendum with Appointive Clerk
November 7, 1995	San Jacinto	1,110	1,116	2,226	50	No	No
November 7, 1995	Ontario	1,836	6,846	8,682	21	No	No
March 26, 1996	Dunsmuir	214	415	629	34	No	No
March 26, 1996	Reedley	1,701	1,379	3,080	55	Yes	No
March 26, 1996	Ukiah	1,651	1,933	3,584	46	No	No
March 26, 1996	Orange	8,730	12,428	21,158	41	No	No
March 26, 1996	San Clemente	3,495	6,874	10,369	34	No	No
March 26, 1996	Brea	3,092	3,878	6,970	44	No	No
November 5, 1996	Fillmore	1,351	1,838	3,189	42	No	No
November 5, 1996	Live Oak	559	514	1,073	52	Yes	Yes
November 5, 1996	Livermore	8,557	14,374	22,931	37	No	No
November 5, 1996	Auburn	1,951	3,226	5,177	38	No	No
November 5, 1996	Waterford	602	897	1,499	40	No	No
November 5, 1996	Santa Paula	2,850	3,553	6,403	45	No	Yes
June 2, 1998	Dorris	102	164	266	38	No	No
November 3, 1998	San Juan Bautista	166	346	512	32	No	No
November 3, 1998	Paso Robles	2,386	3,486	5,872	41	No	No
November 3, 1998	Gonzales	474	447	921	51	Yes	No
November 3, 1998	Benicia	2,700	5,943	8,643	31	No	No
November 3, 1998	Arroyo Grande	2,486	3,615	6,101	41	No	Yes

June 8, 1999	Manteca	1,967	1,812	3,779	52	Yes	No
November 2, 1999	Livermore	7,842	5,698	13,540	58	Yes	No
November 2, 1999	San Jacinto	915	1,491	2,406	38	No	No
March 7, 2000	Waterford	544	619	1,163	47	No	No
March 7, 2000	Santa Maria	6,348	7,086	13,434	47	No	Yes
November 7, 2000	Tehachapi	889	1,173	2,062	43	No	No
November 7, 2000	Dorris	108	170	278	39	No	No
November 7, 2000	Woodland	7,715	7,123	14,838	52	Yes	No
November 7, 2000	Portola	298	459	757	39	No	No
November 5, 2002	Turlock	5,881	6,077	11,958	49	No	No
November 5, 2002	Signal Hill	685	1,125	1,810	38	No	No
November 5, 2002	Portola	162	390	552	29	No	No
November 5, 2002	Barstow	1,028	2,427	3,455	30	No	No
November 5, 2002	Firebaugh	265	306	571	46	No	No
November 5, 2002	Paso Robles	2,606	3,824	6,430	41	No	No
November 5, 2002	Wasco	952	1,096	2,048	46	No	No
January 28, 2003	South Gate	2,537	5,409	7,946	32	No	No
November 4, 2003	Fontana	1,931	1,234	3,165	61	Yes	No
March 2, 2004	Calistoga	607	590	1,197	51	Yes	Yes
November 2, 2004	Gustine	815	634	1,449	56	Yes	No
November 2, 2004	Waterford	990	779	1,769	56	Yes	No
November 8, 2005	Pittsburgh	4,319	6,016	10,335	42	No	No
November 9, 2005	Rocklin	8,322	6,502	14,824	56	Yes	No

Source. The local elections data for 1995–2006 are from the California Elections Data Archive (http://www.csus.edu/calst/cal_studies/CEDA.html).

Table A3
Descriptive Statistics by Appointive City Treasurer Referendum Status, 1992

	Voted On (1)	Not Voted On (2)	(1) – (2) (3)
Outcome:			
Interest Rate Paid on Total Outstanding Debt	3.37 (4.44)	1.94 (2.34)	1.57 [.118]
City finances (2000 \$):			
Total Outstanding Debt per Capita	775 (768)	913 (1,550)	–.98 [.328]
Total Expenditures per Capita	917 (373)	1,005 (771)	–1.08 [.284]
Total Local Source Revenue per Capita	383 (164)	514 (666)	–1.77 [.080]
City government characteristics:			
Mayor-Council Form of Government	.19 (.40)	.25 (.44)	–.38 [.704]
Elected City Clerk	.08 (.28)	.12 (.33)	–.50 [.617]
Full-Time Elected Officials	0 (0)	.02 (.15)	–1.00 [.320]
City economic characteristics:			
Population	28,669 (32,296)	26,368 (30,316)	.07 [.942]
Income per Capita (1990 \$)	12,981 (4,464)	15,167 (8,306)	–2.17 [.032]
Percentage College Graduate	.14 (.09)	.18 (.13)	–1.96 [.052]
Employment-Population Ratio	.43 (.08)	.44 (.09)	–.95 [.343]
Observations	36	126	

Source. Calculations are based on data from the California City Financial Transactions, 1991–Present database for 1992–2008, the California Elections Data Archive for 1995–2006, the 1992 Census of Governments (U.S. Bureau of the Census 1994), and the 1990 decennial census.

Note. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments (U.S. Bureau of the Census 1994) and positive interest expenses in each year. The unit of observation is city. Columns 1 and 2 present means, with standard deviations in parentheses. Column 3 presents the *t*-statistics for a test of differences, with *p*-values in brackets.

Table A4
Referendum Passage and Control Variables, 1992

	Baseline (1)	RD (2)
Population	38,668 [32,296]	13,629 (16,526)
Income per Capita	12,981 [4,464]	1,369 (2,387)
Percentage White	.75 [.19]	.00 (.10)
Percentage Black	.03 [.04]	.00 (.02)
Percentage Less than 17	.28 [.05]	-.01 (.02)
Percentage Greater than 65	.12 [.05]	-.02 (.03)
Percentage College Graduate	.14 [.09]	.01 (.05)
Percentage High School Graduate	.69 [.15]	.03 (.08)
Mayor-Council Form of Government	.19 [.40]	-.03 (.22)
Employment-Population Ratio	.43 [.08]	-.02 (.04)
Observations	36	43

Source. Calculations are based on data from the California Elections Data Archive for 1995–2006, the 1992 Census of Governments (U.S. Bureau of the Census 1994), and the 1990 decennial census.

Note. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments (U.S. Bureau of the Census 1994), positive interest expenses in 14 years, and an appointive treasurer referendum put to voters in a future year. The unit of observation is city in column 1 and referendum in column 2. Column 1 presents means, with standard deviations in brackets. Column 2 reports the referendum pass coefficients for regression discontinuity (RD) specification (3), with standard errors in parentheses.

Table A5
Regression Discontinuity Estimates

	(1)	(2)
Appointive Treasurer Referendum Pass	-.72* (.32)	-.71* (.34)
1990 Decennial census controls	No	Yes

Source. Calculations are based on a regression discontinuity analysis with data from the California City Financial Transactions, 1991–Present database for 1992–2008, the California Elections Data Archive for 1995–2006, the 1992 Census of Governments (U.S. Bureau of the Census 1994), and the 1990 decennial census.

Note. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments (U.S. Bureau of the Census 1994), positive interest expenses in 14 years, and an appointive treasurer referendum voted on. The dependent variable is Interest Rate Paid on Total Outstanding Debt. Year fixed effects are included for both regressions. The models also control for a linear trend in the vote share. The unit of observation is referendum. Values are coefficients, with standard errors in parentheses. Observations = 43, cities = 36, and baseline dependent variable mean = 3.28 (SD = 4.27).

* Significant at the 5% level.

Table A6
Regression Discontinuity Estimates and Smoothness Tests

	(1)	(2)
Interest rates prior to referendum: ^a		
Future Appointive Treasurer Referendum Pass	−.13 (.78)	−.12 (.79)
With 45% vote share discontinuity: ^b		
Appointive Treasurer Referendum Pass	−.82* (.36)	−.78* (.33)
Vote share greater than 45%	.38 (.42)	.56 (.38)
With 55% vote share discontinuity: ^b		
Appointive Treasurer Referendum Pass	−.68* (.31)	−.80* (.37)
Vote share greater than 55%	−.10 (.44)	.29 (.37)
Comparison of appointive treasurer referendum to appointive clerk referendum: ^c		
Appointive Treasurer Referendum Pass	−.37 (.48)	−1.12 (.73)
Appointive Clerk Referendum Pass	−.10 (.45)	.68 (.91)
1990 Decennial census controls	No	Yes

Source. Calculations are based on a regression discontinuity analysis with data from the California City Financial Transactions, 1991–Present database for 1992–2008, the California Elections Data Archive for 1995–2006, the 1992 Census of Governments (U.S. Bureau of the Census 1994), and the 1990 decennial census.

Note. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments (U.S. Bureau of the Census 1994), positive interest expenses in each year, and an appointive treasurer referendum put to voters in a future year. The dependent variable is Interest Rate Paid on Total Outstanding Debt. Year fixed effects are included for both regressions. The unit of observation is referendum. Each column presents the results from one regression specification, weighted by the number of postreferendum observations used to construct the group mean. Values are coefficients, with standard errors in parentheses.

^a Observations = 43, cities = 36, and baseline dependent variable mean = 2.84 (SD = 2.06).

^b Observations = 43, cities = 36, and baseline dependent variable mean = 3.28 (SD = 4.27).

^c Observations = 32, cities = 29, and baseline dependent variable mean = 3.57 (SD = 4.72).

* Significant at the 5% level.

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SHOULD SCHOOL BOARD MEMBERS BE APPOINTED OR ELECTED?

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SHOULD SCHOOL BOARD MEMBERS BE APPOINTED OR ELECTED?

BY A CHICAGO EDUCATOR.

It is generally conceded that personal considerations of a political nature too often become a factor in determining the choice of school board members, and inevitably the same influence is felt in the choice of a superintendent, and frequently in the choice of principals and teachers. Any measure which can reduce this evil to a minimum must be regarded as a benefit to the schools. The question is whether there is liable to be more of this political influence exerted when the school board is appointed by the mayor or when elected by a popular vote. That is, will the mayor of a city be less apt to permit his political aspirations to override his desire to protect the schools than will the voters in a popular political election?

Experience seems to have proven that mayors are more apt to place the interests of the schools above party preference than is the average voter at a general election. Superintendents elected by a partisan school board are compelled to recognize that fact in their administration of the schools, or else expose themselves to the early displeasure of their employers and a humiliating dismissal. Politics in the school board makes politics in the superintendent's office. When an exciting political election determines the choice of school board members, the issue of such election pretty clearly indicates the tenure of office of the superintendent. This is a serious fault, and it should be the purpose of all who wish the good of the schools to divorce politics as far as possible from school boards and school administration. It is useless to contend against politics in the appointment and removal of teachers as long as politics is a chief element in the selection of school board members.

SCHOOLS IN HAWAII.

The value of education is fully appreciated in the Hawaiian islands. In Hawaii, we are told, the schools are first considered in money appropriations, and public improvements and many other things have to stand aside for them. The lowest of the foreign-born population want an education, and the wealthy assist every movement for higher and better schools. A native is seldom found who cannot read and write Hawaiian and many know English.

Education is compulsory and free. It is under the direction of an executive department with the minister of public instruction, who is a member of the cabinet, at its head. In 1897, \$415,272.93 were expended on the schools, and last year the amount reached fully half a million dollars.

The latest statistics on the schools of the islands show that there are 131 public schools, English; one public school taught in Hawaiian, and 60 private schools. In all there are 205 male teachers and 302 female teachers; 8,017 male pupils and 6,505 female pupils; making 192 schools, 507 teachers, and 14,522 pupils.

The average number of school days per year is 200; total cost of education per pupil, \$21.17 (in United States, \$18.92); total cost of education per capita of population, \$2.06; average monthly salaries of male teachers, \$74.55; average monthly salaries of female teachers, \$55.18.

The government does not yet maintain kindergartens, but nearly a dozen are conducted in Honolulu, on the same plans as those in this country, by members of the Ladies' Free Kindergarten Association.

There is a night school in Honolulu where foreigners past the school age can learn written and spoken English. The Y. M. C. A. has six classes, largely attended every night. The Chinese are educated in two schools, one for boys, the other for girls, maintained by the government. There are also a great many private schools, both secular and religious. A large number of the teachers are from the United States, and the latest American ideas have been introduced.

One of the best schools is Oahu College, which is non-sectarian and for both boys and girls of any nationality. A graduate is fitted to enter the freshman class of Harvard, Yale, or Vassar. The college

is supported by private endowments. One dollar a week is charged for tuition, and board, rooms, etc., are furnished at cost. The school year is thirty-eight weeks.

The faculty is made up almost wholly of graduates of colleges in Eastern United States. The buildings are beautifully situated. The grounds cover over 300 acres, containing two ponds and a great variety of tropical fruit trees and foliage. There are ball grounds, tennis courts, and a large swimming tank, and plans are being made for a gymnasium.

Honolulu also has a normal school, a school of higher pedagogy, and a summer school for teachers, which was conducted last summer by Colonel Parker of the Chicago normal school.

As a part of the United States, these islands must extend their system of education to the more remote parts, but we could not ask a more devoted and progressive spirit than they have shown in the past.

THE TEACHER WITHOUT A LIBRARY.

BY SUPERINTENDENT A. B. COLE, PLAINVILLE, MASS.

Teachers who are not able to make use of the public library in the preparation of their work in nature may often receive material assistance from their pupils. If literature and nature are to be correlated, the teacher should keep a note book in which to place the titles of the various poems and books which she finds recommended in her educational journals and helps. By carefully watching the book reviews of her papers she will be able to obtain an understanding of the manner of treatment and scope of the books noted down. When one is found which suits her needs it can be purchased, and the nucleus of a private library formed. To obtain the poems it is well to place short lists of titles and the authors, if known, on the board, and have the pupils search their books at home for them. When one is found, the teacher should make an entry in her note-book of the pupil finding the poem, and the book and the page on which it was found. Soon she will find herself fully equipped to carry on a broad and exhaustive study. Children enjoy the searching, and by it many are more easily interested in the work than they could possibly be without such incentives.

THE PROFESSOR IS IMMORTAL.

BY HENRY SABIN.

You cannot kill the professor, and it is no use to try. We made the attempt, and, for the time being, with good results. It was when, long years ago, we entered upon our work as superintendent of schools in an Iowa city. We were at work in the office, marking out the term's work for each teacher, when the door opened and there entered a gentleman whom we at once knew to be of the book-agent type. "Is the professor in?" with a gentlemanly bow and hat in hand. "No sir," we answered; "The professor is dead." "Dead! ah, indeed, I am sorry to hear it. When did he die?" "Last week at a teachers' meeting, and I helped kill him. In fact, I struck the murderous blow which finished his career in these schools, I hope for all time." It was some time before he took it all in. The effect of our resistance to the term "professor" was so effectual with teachers and pupils, and even the citizens, that to this day, when we revisit the old town and some one refers to us as "the professor," we know that he is a late comer and "does not know Joseph." However, we are sorry that the disease has broken out in many new spots, and we have abandoned all hopes of killing the professor. There are so many men who like the term, and who glory in it, that we fear "the professor" is to be immortal.—Midland Schools.

It pays to be frank with parents. Principal George P. Hitchcock of Fitchburg, Mass., was troubled that pupils in the high school gave so much attention to minstrel shows, plays, etc. He sent a circular letter to about eighty parents asking their opinion in regard to it. Out of fifty replies forty-eight answered that they were decidedly opposed to anything that would in the least detract from the time that should be given to studies.

LOOKING ACROSS THE PACIFIC.

WALT WHITMAN'S PROPHECY OF OUR COLONIAL POSSESSIONS.

To the Editor of the Sun—Sir: The growing popularity of Walt Whitman, both in England and America, is a sure index of his abiding place in the republic of letters. His voice is lifted to a pitch for the ages to hear, and his words will yet sound to the ends of the world.

But we Americans should especially prize him as the largest and most spiritual interpreter of our institutions. I never understood my country or loved it for its mission until I read and reread "Leaves of Grass," one ode to democracy from first to last.

Whitman is more than a poet; he is a seer and prophet. He reads what is to be and writes it, in Bible style, in a language splendid as the optimism that possessed his entire being. No American boy or girl should fail to read him. I venture to inclose a selection from his poem entitled "A Broadway Pageant," written about 1861, on the occasion of the visit to New York city of two Envoys Extraordinary from China, who received on Broadway a great ovation from the thousands there gathered. It begins:—

Over the Western sea hither from Nippon come,
Courteous, the swart-cheek'd two-sworded Envoys,
Leaning back in their open barouches, bareheaded, im-
passive,
Ride to-day through Manhattan.
Libertad! I do not know whether others beheld what I
behold
In the procession, along with the nobles of Nippon, the
criand bearers,
Bringing up the rear, hovering above, around, or in the
ranks marching,
But I will sing you a song of what I behold, Libertad.

Then he proceeds to outline the real and spiritual meaning of this visit of the Orient to the Occident, "the Asiatic continent, the murky night-morning of wonder and fable inscrutable," as he terms it, and now at last inspecting our Western world. For behind these two Envoys he sees the eyes of all the population of China, Malaysia, Tibet, and Polynesia looking at us, and he says:—

I am seized by them, and friendly held by them,
Till as here them all I chant Libertad! for themselves
and for you.

Then he proceeds to utter this remarkable prophecy, which the commissioners of Paris have made a possible reality close at hand. He seems to be looking out over the Pacific and sees our future in the islands and lands beyond, our new "imperialism":—

I chant the world on my Western sea,
I chant copious the islands beyond, thick as stars in the
sky;
I chant the new empire grander than ever before, as in a
vision it comes to me;
I chant America the mistress, I chant a greater supre-
macy;
I chant projected a thousand blooming cities yet in time
on those groups of sea islands;
My sailships and steamships threading the archipelagoes,
My Stars and Stripes fluttering in the wind;
Commerce opening, the sleep of ages having done its
work, races reborn, refreshed,
Lives, works resumed—the object I know not—but the
old, the Asiatic renewed as it must be,
Commencing from this day surrounded by the world.

All of this, written nearly forty years ago, I call a most remarkable "vision"; indeed, a real prophecy, truly Biblical in the light of this glorious hour of our new empire beyond. I commend this and all else Whitman has written to those who see not as Caleb and Joshua saw, but who fear the inhabitants of those lands and the giant problems there. We are well able to go up and possess what God hath given us, "not because of our own righteousness, but for the wickedness of their peoples." It is this courage and vision that Whitman had, and that we all need to have.

And so, in the words of this grand old bard, our real American Homer, we can ask:—

Were the children straying westward so long? so wide
the tramping?
Were the precedent dim ages debouching westward from
Paradise so long?
Were the centuries steadily footing that way, all the while
unknown, for you, America, for reasons?
But they are justified, they are accomplished, they shall
now be turned the other way also, to travel toward
you thence,
They shall now also march obediently eastward for your
sake, Libertad!

The Good Gray Poet saw all that we see, and beyond—the day of Christ, "when men shall worship him, every one from his place, even all the isles of the heathen."

Seneca Falls.

Rev. Arthur Copeland.



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Elected Versus Appointed Regulators: Theory and Evidence

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ELECTED VERSUS APPOINTED REGULATORS: THEORY AND EVIDENCE

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London School of Economics

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Abstract

This paper contrasts direct election with political appointment of regulators. When regulators are appointed, regulatory policy becomes bundled with other policy issues the appointing politicians are responsible for. Because voters have only one vote to cast and regulatory issues are not salient for most voters, there are electoral incentives to respond to stakeholder interests. If regulators are elected, their stance on regulation is the only salient issue so that the electoral incentive is to run a pro-consumer candidate. Using panel data on regulatory outcomes from U.S. states, we find new evidence in favor of the idea that elected states are more pro-consumer in their regulatory policies. (JEL: H1, K2)

1. Introduction

A key issue in political economy concerns the accountability structures put in place to select public officials. While the principle that legislators are to be elected is now a defining feature of modern democracies, there are some offices where a plurality of selection methods survive. A key example is the case of regulators. Typically, heads of regulatory agencies are appointed by politicians, creating an insulating layer between citizens and regulators. However, a number of U.S. states have injected a degree of populism into the regulatory process by requiring that the heads of their independent regulatory commissions be directly elected.¹

For students of political economy the existence of these two methods of regulator selection raises two key questions. First, can we develop a satisfactory

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1. The regulation of public utilities in the United States is undertaken by state level public utility commissions. Each commission is run by a group of "commissioners," assisted by a professional staff. In states where commissioners are not directly elected, they are typically appointed by the state governor. Phillips (1988) Chapter 4 provides a good discussion of the institutional details. Insurance regulation has a similar structure with several states also electing their insurance commissioners.

theoretical understanding of the likely differences between regimes? Second, does the data from the U.S. states yield robust lessons that square with the theory? This paper contributes to answering both of these questions. It provides the first fully developed theoretical explanation of why direct election should lead to more consumer-orientated policies. It then uses a new panel data set on electricity rates from the U.S. states to investigate the validity of this theoretical prediction.

On any first encounter with the idea of popular election of regulators, one is drawn to the proposition that consumer interests might be served more intently by elected regulators since they are more likely to have their eye on the ballot box. Indeed, a number of contributions to the literature begin with the observation that this is self-evident. However compelling this claim might seem, further thought reveals it to be inconsistent with the view that representative democracy yields median policy outcomes. Since those who appoint the regulators are themselves elected, then they would surely have as much interest in promoting consumer interests as directly elected regulators? Hence, we might expect either regime to track the median voter's wishes on regulatory policy and authors including Baron (1988, 1995) and Laffont (1996) have modeled things this way.

The key to our explanation is recognizing that when regulators are appointed, regulation becomes bundled with other issues. While the likely importance of bundling has been mentioned by numerous authors,² the theoretical argument has not previously been developed. We study a model in which a majority of the population are consumers and a minority are stakeholders in a regulated industry. Political parties are organized along some dimension other than regulation—in our specific model this is public spending. When regulators are appointed, parties may be tempted to field candidates who would appoint pro-stakeholder regulators to further their interests in the public spending dimension. We give two conditions for this to be the case. Either, stakeholders view regulation as the main salient issue and will deviate to vote for whichever candidate promises them a better regulatory outcome or there is an organized pro-stakeholder special interest whose campaign support can be used to attract impressionable voters. The result is a sort of “regulatory capture” (Stigler 1971) that emerges endogenously through the electoral process because of diffuse costs and concentrated benefits. By contrast, if regulators are elected, their stance on regulation is the only salient issue so that the electoral incentive is to run a pro-consumer candidate.

The empirical analysis begins by looking at the long-run (conditional) mean electricity prices for three types of tariff (residential, commercial, and industrial) for a panel of forty states that did not change their regulatory regime between 1960 and 1997. We find that residential prices are significantly lower in states that elect their regulators—the point estimates amount

2. See, for example, Hagerman and Ratchford (1978), Boyes and McDowell (1989), Navarro (1982), Smart (1994), and Fields, Klein, and Sfridis (1997).

to around \$60 per year for the average household at 1992 prices.³ We also show that states with elected regulators are less likely to pass through cost changes into prices. Both of these are consistent with the idea that elected regulators are more pro-consumer in their outlook.

The remainder of the paper is organized as follows. The next section explains how the paper relates to the literature. Section 3 presents the model. Section 4 explains why direct elections are more likely to produce pro-consumer regulators than appointed regimes. Section 5 discusses the robustness of the theoretical argument to modeling assumptions. Section 6 draws out the empirical implications of the theory and Section 7 presents the empirical work. A brief conclusion is contained in Section 8. The Appendix contains the proof of the main proposition as well as a detailed description of the data and sources that we use.

2. Related Literature

The paper contributes to two literatures. As a contribution to political economics, it adds to the growing body of work investigating theoretically and empirically the implications of constitutional differences. There is now a sizeable literature on this from the U.S. states (see Besley and Case 2003) and across countries (Persson and Tabellini 2003). Interest in this area is explained by the fact that constitutional rules are a key policy lever. While the analysis is developed for the case of regulators, it has implications for any public office in which officials might reasonably be appointed or elected. This includes judges, school boards, or even directors of public broadcasting corporations!

The theoretical approach taken here can be seen as part of the post-Downsian agenda to modeling political economy issues. The Downsian approach of parties who commit to policy platforms up front and care solely about winning, has a hard time generating insights about constitutional differences and, more generally, in generating predictions in multidimensional policy settings. The modeling approach builds on two distinct strands of work: (1) the citizen-candidate approach to political competition due to Osborne and Slivinski (1996) and Besley and Coate (1997); and (2) voting models that allow for “noise” voters that can be swayed by campaign advertising as in Baron (1994) and Grossman and Helpman (1996).

The paper also contributes to the empirical literature on regulation. There is a large body of work investigating the difference between elected and appointed regulatory regimes empirically using data from the U.S. states. The earliest studies include Berry (1979), Boyes and McDowell (1989), Costello (1984), Crain and McCormick (1984), Harris and Navarro (1983), Navarro (1982), and

3. The conditioning variables, detailed later, are year-fixed effects, a state-specific fossil fuel cost index, and a number of time varying demographic and economic variables.

Primeaux and Mann (1986), each of which looks at the evidence from a different perspective. Some of these contributions looked at rate setting, while others have studied broader indicators of how favorable is the regulatory climate within a state.⁴ Costello's (1984) review of the early evidence concludes that "In summary, it probably makes little difference to the average ratepayer whether a PUC is elected or appointed."

More recent evidence suggests that elected regulators do produce more pro-consumer policies than those that are appointed. Formby, Mishra, and Thistle (1995) find this in their examination of electric utility bond ratings. Using data from 1979–1983 on a selection of investor-owned utilities, they find that election of public utility regulators has a negative effect on bond ratings, consistent with a squeeze on margins due to more pro-consumer choices. Fields, Klein, and Sfiridis (1997) find evidence that elected insurance regulators are more pro-consumer. They report that the market value of life insurance companies doing business in California declined sharply following the passage of Proposition 103, which changed the method of selection of the insurance commissioner from appointment to election. Using data from 1985, Smart (1994) reports that telephone rates are lower in states that elect their public utility regulators.⁵

Our empirical analysis significantly strengthens this more recent evidence that elected regulators favor consumers. In contrast to previous studies of electricity prices, we use panel data. This allows us to look at the long-run conditional mean differences in prices rather than at a single cross-section. Moreover, we use measures of production costs that have typically been omitted from previous studies. In addition, we exploit a very different source of identification in the tests based on the pass through of costs into prices where we interact the regulatory regime and a time-varying variable (production costs). This gets away from identification based purely on cross-sectional differences and hence minimizes concerns about the correlation between the regulatory regime and other sources of long-run heterogeneity among the states.

3. The Model

We develop the simplest possible model to illustrate why elected regulators are likely to be more pro-consumer than appointed regulators. To incorporate the idea of policy bundling, there are two issues: public spending and regulation.

4. In a influential paper, Joskow (1974) has argued, that price regulation is ultimately the tool that influences the rate of return that the industry enjoys.

5. To explain the difference between this and the older studies note that the three studies conducted in the 1990s were looking either at different kinds of regulation (Fields, Klein, and Sfiridis 1997 and Smart 1994) or at very different outcome measure (Formby, Mishra, and Thistle 1995).

Policies are selected by policymakers chosen by the voters in elections. Candidates in these elections are put forward by two policy-motivated parties.

Our basic notion is that there are two types of voters with respect to regulatory issues. One group are stakeholders in the regulated firms. These could be owners of capital, or workers if they are able to capture some of the rent. Stakeholders want higher profits and hence prefer higher prices. The remainder of voters have no financial stake in the regulated industry and always prefer low prices. However, getting those prices is less important to them than having their preferred public spending outcome.

3.1 Government Policies

The government has two functions: choosing the level of public spending and regulating a monopoly. On public spending, citizens have two preference types indexed by $k \in \{L, R\}$ where L denotes “left wing” and R “right wing.” A citizen of spending preference type k obtains a net benefit $b(g; k)$ from public spending level g , where $b(\cdot; k)$ is single peaked with interior maximum $g^*(k)$. Left-wingers have a higher demand for spending, so that $g^*(L)$ exceeds $g^*(R)$.

The monopoly produces good x with cost function $c(x)$, where $c(\cdot)$ is increasing and twice continuously differentiable. The price of good x , denoted p , is determined by the government and the monopoly is required to meet all consumer demand at this price, subject to it not making a loss.⁶ Each citizen has an identical demand function for good x , denoted $x(p)$. Letting N denote the size of the population, aggregate demand is $X(p) = Nx(p)$. The regulated firm’s profits are $\pi(p) = pX(p) - c(X(p))$ and these are equally shared by a group of stakeholders. Thus, with respect to their relationship to the regulated monopoly, there are two types of citizens—“consumers” who only consume the good and “stakeholders” who have some interest in the firm’s profits. We index these two types by $t \in \{C, S\}$.

The fraction of citizens of type (k, t) is denoted γ_t^k . We let $\gamma^k = \gamma_C^k + \gamma_S^k$ denote the fraction of the population with public good preference k and $\gamma_t = \gamma_t^L + \gamma_t^R$ the fraction with relationship to the monopoly t . We assume throughout that stakeholders are a minority group in the sense that γ_S is smaller than either γ^L or γ^R .

6. In the case of electricity, discussed later, the assumption of a price setting regulator seems a reasonable approximation, although things are more complicated than this in practice. As Joskow (1974) observes “The *statutes* establishing most regulatory agencies are usually quite vague. Regulatory agencies are normally mandated to insure that rates charged by regulated firms are “reasonable and nondiscriminatory” and that service of the “good quality” is maintained” page 296 (emphasis original). In practice rates should be set to allow stockholders a fair rate of return. However, Joskow (1974) notes that “Contrary to the popular view, *it does not* appear that regulatory agencies have been concerned with regulating returns per se. The primary concern of regulatory commissions has been to keep *nominal prices from increasing*.” page 298 (emphasis original).

Let $r(p, t)$ denote the monopoly-related payoff that a citizen of type $t \in \{C, S\}$ gets at price p . A consumer's payoff is his consumer surplus, while a stakeholder's payoff includes his profit share. Thus, $r(p, C) = \int_0^p x(p)dp - px(p)$ and $r(p, S) = r(p, C) + (1/\gamma_S)\pi(p)$. Let $p^*(t)$ denote the optimal price from the perspective of a type t ; i.e., that which maximizes $r(p, t)$ subject to the constraint of nonnegative profits.⁷ The optimal consumer price is the lowest price at which the regulated firm does not make a loss, implying that $p^*(C)$ equals average cost at output $X(p^*(C))$. The optimal stakeholder price also takes into account the firm's profits. We assume that it exceeds the optimal consumer price in which case it satisfies the first-order condition $p^*(S) = c'(X(p^*(S)))/\{1 - (1 - \gamma_S)/\varepsilon(p^*(S))\}$ where $\varepsilon(p)$ is the price elasticity of demand.⁸

Public spending is assumed to be more important to consumers than regulation in the sense that for both public spending types $k \in \{L, R\}$, $\Delta r(C)$ is less than $\Delta b(k)$ where $\Delta r(C) = r(p^*(C), C) - r(p^*(S), C)$ and $\Delta b(k) = b(g^*(k), k) - b(g^*(-k), k)$.⁹ This means that it is more important to consumers to get their preferred level of public spending than their preferred regulatory outcome. Public spending is therefore the more salient issue for consumers. However, for the stakeholders, we assume that regulation is the more salient issue in the sense that for both public spending types $k \in \{L, R\}$, $\Delta r(S)$ exceeds $\Delta b(k)$ where $\Delta r(S) = r(p^*(S), S) - r(p^*(C), S)$. This reflects the fact that the regulated price directly impacts the livelihood of the stakeholders.

3.2 Policy Determination

The level of public spending is chosen by an elected governor and the regulated price by a regulator. We compare two methods of regulator selection: appointment and election. Under the former, the governor appoints the regulator. Under the latter, the regulator is directly elected along with the governor. Under both regimes, the governor and regulator are citizens and hence characterized by their types (k, t) .

Candidates in gubernatorial and regulator elections are selected by two political parties, denoted L and R . Each party is comprised of member citizens bound together by their views on public spending. Thus, all members of Party L are left-wingers and all members of Party R are right-wingers. Both parties contain a mixture of stakeholders and consumers. Parties chose the candidate

7. We assume that there exists a price p that will yield positive profits.

8. There is no general guarantee that $p^*(S) > p^*(C)$. A sufficient condition is that

$$1 - \gamma_S > \varepsilon(p^*(C)) \left[1 - \frac{c'(X(p^*(C)))}{p^*(C)} \right].$$

Since $p^*(C) = [c(X(p^*(C)))/X(p^*(C))]$, this condition is satisfied if the monopoly's average costs are increasing at output level $X(p^*(C))$. If its average costs are decreasing at $X(p^*(C))$ the condition requires either that demand be relatively inelastic or that the fraction of stakeholders in the population is small.

9. The notation $-k$ refers to the opposite type to k . For example, $-k = R$ when $k = L$.

that a majority of their members prefer and we assume that the majority of each party's members are consumers.¹⁰

If the regulator is appointed, each party selects a candidate for the gubernatorial race. There are four possible types of candidate: (L, C) , (R, C) , (L, S) , and (R, S) .¹¹ No ex ante policy commitments are possible, so that, when in office, a type (k, t) candidate chooses a public good level $g^*(k)$ and appoints a regulator who shares his type t . This regulator then selects a price $p^*(t)$.

If the regulator is elected, each party selects two candidates: one for the gubernatorial race and one for the regulator race. While candidates are still characterized by their types (k, t) , only one dimension of their type is relevant for their policymaking roles. Thus, if elected governor, a type (k, t) candidate chooses a public good level $g^*(k)$ and, if elected regulator, a type (k, t) candidate chooses a price $p^*(t)$.¹²

There are two types of voters.¹³ A fraction μ are rational voters who anticipate the policy outcomes each candidate would deliver and vote for the candidate whose election would produce their highest policy payoff. Thus, if the regulator is appointed, a rational voter of type (k, t) who is faced with gubernatorial candidates of types (k_L, t_L) and (k_R, t_R) will vote for Party L 's candidate if $b(g^*(k_L), k) + r(p^*(t_L), t)$ exceeds $b(g^*(k_R), k) + r(p^*(t_R), t)$. Rational voters indifferent between two candidates abstain.

The remaining fraction of voters are noise voters. In each election, a fraction η of these vote for Party L 's candidate. Here, η is the realization of a random variable with support $[0, 1]$ and cumulative distribution function $H(\eta)$. If the regulator is elected, there is a separate (uncorrelated) draw of η for each election. The idea is that noise voters respond to nonpolicy relevant features of candidates such as their looks, sense of humor, etc. We assume that H is symmetric, which implies that noise voters are unbiased in the sense that the probability that a fraction less than η vote for Party L 's candidate equals the probability that a fraction less than η vote for Party R 's candidate.

Noise voters make the election outcomes probabilistic. To illustrate, con-

10. We have in mind a primary process by which parties select candidates. One could alternatively assume that party members select candidates via some type of bargaining process whose outcome maximizes a weighted sum of party members' utilities.

11. We do not require that a party must select a candidate from the ranks of its members. However, under the assumptions on preferences we make, parties have no incentive to select from outside their membership in equilibrium.

12. The idea of regulators choosing regulatory policy to maximize their own personal gain from the regulated industry should not be taken too literally. Indeed, there are laws that prevent regulators having any direct stake in the industry that they are regulating. The reality that we are trying to capture is that some regulators are more sympathetic to industry and others to consumers. This may be because of ideology, past associations with the industry or consumer groups, or future career concerns. Assuming that regulators can be either consumers or stakeholders and that these individuals maximize their selfish utility allows us to introduce different types of regulators in the simplest possible way.

13. This kind of approach is common in the electoral competition literature following Baron (1994) and Grossman and Helpman (1996).

sider an election in which the difference between the fraction of citizens obtaining a higher utility from the policy choices generated by Party L 's candidate and the fraction obtaining a higher utility from Party R 's candidate is ω . Since μ is the fraction of rational voters and η the fraction of noise voters who vote for Party L 's candidate, Party L 's candidate will win if $\mu\omega + (1 - \mu)\eta$ exceeds $(1 - \mu)(1 - \eta)$ or, equivalently, if η exceeds $[-\mu\omega/2(1 - \mu)] + 1/2$. The probability that Party L 's candidate will win is thus

$$\psi(\omega) = \begin{cases} 0 & \text{if } \omega \leq \frac{-(1 - \mu)}{\mu} \\ 1 - H\left(\frac{-\mu\omega}{2(1 - \mu)} + \frac{1}{2}\right) & \text{if } \omega \in \left(\frac{-(1 - \mu)}{\mu}, \frac{1 - \mu}{\mu}\right) \\ 1 & \text{if } \omega \geq \frac{1 - \mu}{\mu} \end{cases}$$

Parties are assumed to correctly calculate the election probabilities associated with different candidate pairs and take them into account when choosing candidates. We assume that the fraction of noise voters in the population is sufficiently high so that $|\gamma^L - \gamma^R|$ is less than $(1 - \mu)/\mu$. This assumption implies that $\psi(\gamma^L - \gamma^R)$ is positive but less than one, meaning that in an election between a left-winger and a right-winger in which public spending were the only issue, both candidates would win with positive probability.

Any election gives rise to a game between the two parties. Each party's pure-strategy set is the set of possible candidate types $\{L, R\} \times \{C, S\}$ and a strategy is a probability distribution over this set giving the probability that the party selects each type of candidate. Each party's payoff from any strategy pair is determined by the probability its candidate wins and its objective function. An equilibrium of the game is a pair of strategies, one for each party, that are mutual best responses. Given that each party's pure-strategy set is finite, we can be sure that an equilibrium exists.

4. Analysis

4.1 The Basic Model

Elected Regulators: If the regulator is elected, each party has effectively two (pure) strategies for the regulator election: run a pro-consumer candidate or a pro-stakeholder candidate. Rational voters vote on the basis of a candidate's regulatory stance and hence a pro-consumer candidate has an electoral advantage over a pro-stakeholder candidate. Since the majority of members of both parties prefer pro-consumer regulators, both parties select such candidates and the party affiliation of the winning candidate is determined by noise voters. This yields:

PROPOSITION 1: *If elected, the regulator will be pro-consumer.*

Appointed Regulators: If the regulator is appointed, the type of the regulator is determined by the regulatory stance of the winning gubernatorial candidate. Thus gubernatorial candidates' preferences over both public spending and regulation are relevant for voters. However, consumers prefer the candidate who shares their public spending preferences irrespective of his stance on regulation. Stakeholders, on the other hand, prefer the candidate who is pro-stakeholder irrespective of his public spending preferences. Assuming that they offer candidates with differing public spending preferences, this provides parties with electoral incentives to run pro-stakeholder candidates.

To see this, suppose that Party L is selecting a left-winger, Party R a right-winger and that both are pro-consumer. If Party L deviates by selecting a candidate who would select a pro-stakeholder regulator it will attract the support of all the stakeholders in Party R 's base. This will raise the probability that its candidate will win and implement its preferred spending level. This gain will offset the reduced probability of its preferred regulatory outcome if spending is relatively more important. On the other hand, if Party R were running a candidate who would select a pro-stakeholder regulator then Party L will lose the stakeholders in its base unless it does the same.

The argument relies crucially on the assumption that the parties offer candidates with differing public spending preferences. If this were not the case, then the only issue in the election would be regulation and the majority of votes would go to the candidate who would select a pro-consumer regulator. Parties may choose to offer candidates with different public spending preferences because they are purely policy motivated and a higher probability of winning has no intrinsic value. What is important is that public spending is sufficiently important relative to regulation that they would prefer a smaller probability of getting their preferred public spending outcome to a higher probability of getting their preferred regulatory outcome.

The following assumption embodies conditions under which equilibrium involves both parties selecting candidates who share the public spending preferences of their members, but who are pro-stakeholder.

ASSUMPTION 1: *For $k \in \{L, R\}$*

- (i) $\psi(\gamma^k - \gamma^{-k})\Delta b(k) > \psi(\gamma_C - \gamma_S)\Delta r(C)$,
- (ii) $(\psi(\gamma^k - \gamma^{-k}) - \psi(\gamma_C^k - \gamma_S^k - \gamma^{-k}))\Delta b(k) > \psi(\gamma_C^k - \gamma_S^k - \gamma^{-k})\Delta r(C)$,

and

- (iii) $(\psi(\gamma^k + \gamma_S^{-k} - \gamma_C^{-k}) - \psi(\gamma^k - \gamma^{-k}))\Delta b(k) > \psi(\gamma^k + \gamma_S^{-k} - \gamma_C^{-k})\Delta r(C)$.

Parts (i) and (ii) imply that both parties selecting pro-stakeholder candidates

who share their public spending preferences is an equilibrium. Part (i) ensures that neither party has an incentive to deviate to running a pro-consumer candidate with the opposing party's public spending preferences. It requires that each party's lost chance of getting its preferred level of public spending is more costly than the benefits from the improved chance of getting its preferred regulatory outcome. Part (ii) ensures that neither party wishes to deviate to a pro-consumer candidate. It requires that the electoral penalty stemming from the loss of each party's stakeholder constituency is prohibitive. Part (iii) guarantees that both parties selecting pro-stakeholder candidates is the only equilibrium. It ensures that, when they are fielding candidates with different public spending preferences, both parties have an incentive to select a pro-stakeholder candidate if the other party were to field a pro-consumer candidate. It requires that the electoral gain associated with attracting the other party's stakeholder constituency is large. Thus we have:

PROPOSITION 2: *Under Assumption 1, if appointed, the regulator will be pro-stakeholder.*

This proposition contains the basic insight into why electing rather than appointing regulators can produce more pro-consumer outcomes.¹⁴ If the regulator's type is determined in a gubernatorial election, regulatory policy is bundled with other issues. Regulatory policy is salient only for voters who wish to secure a high price in the regulated industry. This means that parties can gain electorally by running candidates with pro-stakeholder regulatory attitudes. These electoral gains matter more to parties when they care more intensely about attaining their preferred public spending outcome.

The conditions of Assumption 1 ensure that the regulatory outcome is captured by stakeholders as predicted by Stigler (1971) among others. However, the logic here is due to a combination of issue bundling coupled with concentrated benefits and diffuse costs. The latter is not sufficient on its own to generate capture—it is only when there are other issues in the election that are salient to voters that the power of stakeholders will be felt. By unbundling the issues through direct elections, the scope for regulatory capture is diminished.

Substantively, Assumption 1 requires three conditions to be satisfied. First, regulation must be a nonsalient issue for consumers. Second, in terms of the core issues, the population must be reasonably evenly divided between the two political parties. Third, the fraction of stakeholders in each party's base must be nonnegligible. Of these three conditions, perhaps the most restrictive is the third.

14. Given Proposition 1, Proposition 2 is more than is needed to establish the general result that elected regulators are more likely to be pro-consumer. Indeed, it is possible to establish this result under weaker conditions. For example, if only parts (i) and (iii) of Assumption 1 are satisfied, then, if appointed, the regulator will be pro-stakeholder with positive probability. Part (i) implies that in any equilibrium with appointed regulators in which a pro-consumer regulator is selected with probability one, each party's gubernatorial candidate must reflect its member's public spending preferences and be pro-consumer. Part (iii) implies that this cannot be an equilibrium.

However, the next subsection shows that the argument goes through when the currency of political influence is money rather than votes.

4.2 Campaign Contributions

In this subsection, we allow for the regulated firm to provide campaign contributions to pro-stakeholder candidates. Candidates can use these contributions to “buy” the votes of noise voters and thereby enhance their electoral chances. Parties rationally anticipate these contributions when selecting candidates and they give rise to the same incentives as having an intense stakeholder minority.

To focus cleanly on the role of campaign contributions, we assume that all voters regard public spending to be the salient issue—thus stakeholders exercise no political influence through the ballot box. Formally, this says that for $k \in \{L, R\}$: $\Delta r(S)$ is less than $\Delta b(k)$. To understand the effect of contributions, consider an election in which the difference between the campaign expenditures of the two parties’ candidates is z . If z is positive, Party L ’s candidate is outspending R ’s and vice versa. Then we assume that the fraction of noise voters voting for Party L ’s candidate, η , is a random variable with support $[0, 1]$ and cumulative distribution function $H(\eta; z)$. The function H is assumed to be twice continuously differentiable and to satisfy the condition that for all η and z , $H(\eta; z)$ is decreasing in z .

To ensure that noise voters remain unbiased, we restrict $H(\eta; z)$ to be symmetric in the sense that for all η and z , $H(\eta, z) = 1 - H(1 - \eta, -z)$. This implies that the probability that Party L ’s candidate gets a fraction of noise voters less than η when he outspends Party R ’s candidate by an amount z equals the probability that Party R ’s candidate gets a fraction of noise voters less than η when he outspends Party L ’s candidate by the same amount. We also assume that for all η and z greater than zero, the derivative $H_z(\eta; z)$ is increasing, implying diminishing returns to outspending an opponent.

To investigate the firm’s optimal contribution, consider an election in which the identity of the regulator is to be determined and let ω be the difference between the fraction of citizens obtaining a higher utility from Party L ’s candidate and the fraction obtaining a higher utility from Party R ’s candidate. If both candidates have the same regulatory stance, the monopoly will make no campaign contributions. However, if Party L ’s candidate is pro-stakeholder and Party R ’s pro-consumer, then the monopoly may contribute to Party L ’s candidate. Generalizing the earlier analysis, let $\hat{\psi}(\omega, z)$ be the probability that Party L ’s candidate wins when the difference between the two candidate’s campaign expenditures is z .¹⁵ Then the monopoly’s optimal contribution to Party L ’s candidate is

$$z^*(\omega) = \arg \max \{ \hat{\psi}(\omega, z) \pi(p^*(S)) - z : z \geq 0 \}.$$

15. Following the earlier logic, $\hat{\psi}(\omega, z) = 0$ if $\omega \leq [-(1 - \mu)/\mu]$, $\hat{\psi}(\omega, z) = 1$ if $\omega \geq (1 - \mu)/\mu$ and $\hat{\psi}(\omega, z) = 1 - H\{[-\mu\omega/2(1 - \mu)] + 1/2, z\}$ otherwise.

If Party R 's candidate is pro-stakeholder and Party L 's pro-consumer, the monopoly will contribute $z^*(-\omega)$ to Party R 's candidate implying that Party L 's candidate would win with probability $\hat{\psi}(\omega, -z^*(-\omega))$.

Turning to candidate selection, the parties anticipate the firm's lobbying behavior and incorporate this into the probabilities they assign to their candidates winning. Any election gives rise to a game as before, except that the election probabilities associated with different candidate pairs now incorporate the monopoly's lobbying behavior. Parties' strategies, strategy sets, and the definition of an equilibrium are all unchanged.

If the regulator is elected, the analysis is essentially the same as in the last section. Each party will field a pro-consumer candidate. While contributions may mean that a pro-stakeholder candidate is at less of an electoral disadvantage, they will not induce parties to distort their candidate choice. This is because the only gain to winning the regulator election is to control regulation. Hence, the conclusion of Proposition 1 remains valid.

If the regulator is appointed, then campaign contributions serve the same purpose as the intense stakeholder minority in the previous section, inducing the parties to put up pro-stakeholder candidates. If the two parties select candidates with differing public spending preferences, a party loses no votes from rational voters if it runs a pro-stakeholder candidate. However, if its opponent is running a pro-consumer candidate, it attracts campaign contributions that allow it to buy the votes of noise voters. On the other hand, if its opponent is running a pro-stakeholder, it stops the flow of contributions to the opposing candidate. Either way, there is an electoral incentive to run a pro-stakeholder candidate.

The equivalent of Assumption 1 in terms of campaign contributions is:

ASSUMPTION 1': For $k \in \{L, R\}$

- (i) $\psi(\gamma^k - \gamma^{-k})\Delta b(k) > \hat{\psi}(\gamma_C - \gamma_S; -z^*(\gamma_S - \gamma_C))\Delta r(C),$
- (ii) $(\psi(\gamma^k - \gamma^{-k}) - \hat{\psi}(\gamma^k - \gamma^{-k}; -z^*(\gamma^{-k} - \gamma^k)))\Delta b(k) > \hat{\psi}(\gamma^k - \gamma^{-k}; -z^*(\gamma^{-k} - \gamma^k))\Delta r(C),$

and

- (iii) $(\hat{\psi}(\gamma^k - \gamma^{-k}; z^*(\gamma^k - \gamma^{-k})) - \psi(\gamma^k - \gamma^{-k}))\Delta b(k) > \hat{\psi}(\gamma^k - \gamma^{-k}; z^*(\gamma^k - \gamma^{-k}))\Delta r(C).$

Under this assumption, the unique equilibrium has each party giving into the firm by running a gubernatorial candidate who would select a pro-stakeholder regulator and the conclusion of Proposition 2 remains valid.¹⁶

Two points should be noted about the argument. First, in the equilibrium in which both parties select pro-stakeholder candidates, the firm does not make

16. The proof is identical to that of Proposition 2 and hence is omitted.

contributions. It has a significant impact on the policy outcome without making any campaign contributions! Its power stems from the credible threat to support the other party's candidate if it puts up a candidate who would select a pro-consumer regulator. Second, issue bundling remains essential to get the difference between elected and appointed regimes. It creates the ability of the firm to move regulatory policy in a nonmajoritarian direction. Thus, the analysis presented here makes precise what structural features are needed and justifies building a model where the microfoundations of the political process are specified.

5. Robustness to Alternative Assumptions

This section briefly discusses how our argument would hold up under different modeling assumptions. Our main purpose is to argue that the basic conclusions and the logic that we develop are more general than our specific model might suggest.

5.1 Downsian Parties

Suppose that, instead of being policy motivated, parties were Downsian, caring only about winning elections.¹⁷ Proposition 1 remains valid in this case because, in a regulator election, a pro-consumer candidate has an advantage over a pro-stakeholder candidate. However, the equilibrium underlying Proposition 2 would not be an equilibrium with Downsian parties. If, say, $\gamma^L \geq \gamma^R$, Party *R* could raise its probability of winning by selecting a left-wing gubernatorial candidate who would select a pro-consumer regulator. If $\gamma_C^L \geq \gamma^R + \gamma_S^L$ then the unique equilibrium would be for both parties to select pro-consumer, left-wingers and there would be no difference between the two regimes. However, if this inequality were not satisfied, then a pro-stakeholder, right-winger would win with a probability of greater than $\frac{1}{2}$ against a pro-consumer left-winger and no equilibrium in pure strategies would exist. The mixed strategy equilibrium would involve parties selecting pro-stakeholder candidates with positive probability, so that the conclusion that appointing would be more likely to produce pro-stakeholder regulators would be valid in this case.

Similar remarks apply to the case with Downsian parties and campaign contributions. Proposition 1 remains valid provided that the amount of contributions given to a pro-stakeholder candidate is not sufficient to offset the unpopularity of his position among rational voters. With appointed regulators,

17. We maintain the assumption that parties compete by selecting candidates. Under the usual Downsian assumption that parties compete by selecting policy platforms (i.e., (g, p) pairs), the model would be intractable without introducing some noise in rational voters' voting behavior.

the equilibrium would either involve the two parties selecting pro-consumer, left-wingers or would be a mixed strategy equilibrium if contributions were sufficient to make a pro-stakeholder, right-winger win with probability of greater than $\frac{1}{2}$ against a pro-consumer left-winger.

5.2 Candidate Entry

Suppose that independent candidates can enter the regulator and gubernatorial elections. Whether a pro-stakeholder independent candidate would wish to enter the regulator election if both parties selected pro-consumer candidates, would depend on how rational consumer voters behaved. If they rallied behind one of the pro-consumer candidates, the pro-stakeholder's odds of winning would be very low, perhaps so low as to make entry unattractive. However, if rational consumer voters split between the two pro-consumer candidates, then the pro-stakeholder's odds of winning might be good enough to justify entry. Nonetheless, if parties anticipated this entry, then one of them would have an incentive not to run a candidate which would mean that a pro-consumer regulator would be elected with a probability of at least $\psi(\gamma_C - \gamma_S)$. While this probability is not one, it is significantly greater than $\frac{1}{2}$ implying that the conclusion of Proposition 1 would only be marginally weakened.

Turning to gubernatorial elections, one might suppose that an independent pro-consumer candidate would enter to compete with the two parties' candidates. However, this neglects important issues inherent in elections with three or more candidates. Consider the entry decision of a type (L, C) independent candidate, assuming that the two parties are selecting candidates of types (L, S) and (R, S) , respectively. If rational voters vote sincerely, entry by a type (L, C) independent would simply split the left-wing vote and significantly enhance the probability of the right-wing candidate winning. If rational voters are strategic, left-wingers will be reluctant to switch to the entrant for fear of wasting their vote. It follows that a type (L, C) independent is likely either to increase the probability of the right-wing candidate winning or to have no effect. Either way, such a candidate has little or no incentive to enter. Thus, entry of independent candidates will not impact Proposition 2. Accordingly, entry does not disturb the basic conclusion that elected regulators are more likely to be pro-consumer.

5.3 Alternative Views of Lobbying

The assumptions we are making about lobbying are important for the argument. It is key that money enters the picture as campaign contributions rather than bribes. If regulated firms can bribe regulators after they have been selected by

offering promises of future employment and other types of bribes to regulators in exchange for policy favors (as in Laffont and Tirole 1993, Grossman and Helpman 1994, and Besley and Coate 2001), the ability of the regulator selection mechanism to mute the regulated firm's influence is more limited. Even the staunchest pro-consumer regulator may find it difficult not to be swayed by the prospect of significant personal gain.

More interestingly, it is also important that the lobby chooses its contributions after the parties have selected their candidates. If it moves before parties, as in Grossman and Helpman (1996), this might disturb the conclusions of Proposition 1. This simple change in timing endows the interest group with the ability to commit. With regulator elections, the lobby could offer to support a party's gubernatorial candidate if the party were to run a pro-stakeholder candidate in the regulator election. This may induce both parties to run such candidates resulting in the elected regulator being pro-stakeholder.

Fortunately, there are still good reasons to believe that regulator elections are more likely to produce pro-consumer regulators even with this alternative specification of lobby behavior. First, if both parties were bribed into selecting pro-stakeholder candidates for the regulator election, an independent pro-consumer candidate would have an incentive to enter and challenge them. As argued before, this is not the case with appointed regulators when both parties select pro-stakeholder gubernatorial candidates. Second, the cost to the lobby of bribing parties to select pro-stakeholder candidates in the regulator election, is higher than the cost of bribing them to select pro-stakeholder candidates in the gubernatorial election. This is because, in the former situation, each Party k gives up the chance of a pro-consumer regulatory outcome with probability $\psi(\gamma_C - \gamma_S)$ as opposed to $\psi(\gamma^k - \gamma^{-k})$ in the latter.

5.4 Pro-stakeholder Parties

What would happen if stakeholders were an important constituency in one party and this led that party to favor a pro-stakeholder regulator? Such a party would select a pro-stakeholder candidate in a regulator election. Assuming that $\psi(\gamma_S - \gamma_C)$ is positive, there would be a positive probability that this candidate would be elected meaning that Proposition 1 no longer applies. However, the forces leading to the selection of pro-stakeholder candidates in the appointed regime would be strengthened by a party having stakeholder preferences. For, in such circumstances, there would be no policy compromise involved in running such a candidate. Thus, the conclusion that appointing would be more likely to produce pro-stakeholder regulators remains valid.

6. Empirical Implications

The basic idea of the theory is that in multidimensional political competition, under reasonable conditions, electing regulators will lead to more pro-consumer outcomes. This was embodied in Propositions 1 and 2 above. The first empirical implication refers to price levels. We record this as:

PREDICTION 1 (PRICE LEVELS): *States with elected regulators will have lower electricity prices.*

Our second test is motivated by the theoretical observation that prices set by more pro-consumer regulators should be less responsive to cost shocks. To see this, suppose (following standard practice) that regulators choose prices to maximize $Nr(p, C) + \lambda\pi(p)$ subject to the constraint that $\pi(p) \geq 0$ for some weight λ . The degree to which a regulator is “pro-consumer” is then measured by the relative weight he/she places on consumer relative to producer surplus. Our model represents a special case of this general formulation, with the weight a regulator puts on the profits of the regulated firm determined by his/her connection to the firm.

Further suppose that the regulated firm’s cost function is $c(x) = F + c \cdot x$ and let $p^*(c, \lambda)$ be a type λ regulator’s optimal price given the marginal cost c . Assuming that $\pi(p^*(c, \lambda)) > 0$, we have that

$$p^*(c, \lambda) = \frac{c}{1 - \frac{\left(1 - \frac{1}{\lambda}\right)}{\varepsilon}},$$

where ε is the price elasticity of demand. Assuming that consumer demand functions are of the constant elasticity form, it is clear that $p^*_c(c, \lambda) < p^*_c(c, \lambda')$ for $\lambda < \lambda'$.¹⁸ This result is consonant with Joskow (1974)’s description of the regulatory process that emphasizes the dynamics of rate increases in response to costs to shocks. Pro-consumer regulators should be less willing to respond to applications for rate increases by utilities in the face of cost shocks.¹⁹ Putting this together with the insights from Propositions 1–3 yields:

18. This also holds for the boundary where $\pi(p^*(c, \lambda)) = 0 < \pi(p^*(c, \lambda'))$ as long as $\varepsilon \leq 1$ which is the empirically relevant case for electricity demand. To see this, observe that $p^*(c, \lambda)$ satisfies the price equals average cost equation $p^* = c + F/Nx(p^*)$. This equation implies that $\partial p^*(c, \lambda)/\partial c = 1/\{1 - [\varepsilon(p^*(c, \lambda) - c)/p^*(c, \lambda)]\}$. Thus, using the expression for $p^*(c, \lambda')$ given in the text and noting that $[1 - 1/\lambda']/\varepsilon = [(p^*(c, \lambda') - c)/p^*(c, \lambda')]$, we have that $[\partial p^*(c, \lambda)/\partial c] < [\partial p^*(c, \lambda')/\partial c]$ if $[\varepsilon(p^*(c, \lambda) - c)/p^*(c, \lambda)] < [(p^*(c, \lambda') - c)/p^*(c, \lambda')]$. Since $p^*(c, \lambda) < p^*(c, \lambda')$, a sufficient condition for the result is that $\varepsilon \leq 1$.

19. To investigate the changing importance of regulator influence, it is possible to check whether the gap between appointers and electors widened between 1970 and 1980. In a simple difference-in-difference analysis we found that the average residential price per kilowatt hour between elected and appointed states was 0.3 cents in the 1960s and increased to 1 cent in the 1970s. This widening

PREDICTION 2 (PASS-THROUGH): *States with elected regulators will pass cost increases into price levels less than appointed regulators.*

The remainder of the paper takes a look at the evidence on these predictions.

7. Evidence

Our data come from the EEI and give prices of electricity in three segments of the market—residential, commercial, and industrial from 1960 to 1997 for the forty-eight continental U.S. states.²⁰ We first test whether prices are lower in those states that elect their public utility commissioners and then examine whether prices are less sensitive to cost shocks in elected states.

7.1 Price Levels

At the beginning of our period (1960), thirteen states elected their utility commissioners, falling to ten by the end (1997).²¹ This general trend masks the fact that six states switched their method of selecting regulators.²² Our analysis focuses chiefly on rate setting decisions by public utility regulators and concentrates on the forty-two states whose appointment method remained constant over the period.

We summarize some background information on the characteristics of the three categories of states (appointers, electors, and switchers) in Table 1. States that elect their utility commissioners tend to be smaller and poorer than states that appoint them. They are also more likely to have a Democrat as a governor. However, the states are similar in terms of demographic structure as measured by the proportion of children and their population aged over 65. There is no significant difference between the states that appoint and elect their utility commissioners in terms of the proportion of electricity produced from fossil fuels (around 70 percent for both kinds of states).

Table 1 also provides information on the raw means of the nominal prices of electricity (denoted in cents per kilowatt hour) across the three

gap is statistically significant at conventional levels. There are similar changes (also significant) for commercial and industrial prices.

20. The data are available at (http://econ.lse.ac.uk/staff/tbesley/index_own.html/data).

21. The data Appendix gives all the variations observed in the data. Appendix Table 1 gives a list of states in each category.

22. Florida switched from electing to appointing in 1981; Iowa switched to an election system for 1962 and 1963 only; Minnesota has the most colorful history, using an election system 1960–1971, appointing from 1972–1975, electing from 1976–1977 and appointing ever since; South Carolina switched to an election system in 1996; Texas switched from an election to an appointment system in 1977; Tennessee switched to appointing in 1996.

TABLE 1. TABLE OF MEANS

	States that appoint public utility commissioners	States that elect public utility commissioners	States that switched between 1960 and 1997
Residential price per* kilowatt hour (cents)	5.10 (2.93)	4.39 (2.19)	4.54 (235)
Commercial price per kilowatt hour (cents)	4.65 (2.58)	4.25 (2.11)	4.27 (1.97)
Industrial price per kilowatt hour (cents)	3.15 (2.16)	2.71 (1.60)	2.80 (1.63)
Income per capita* (1982 dollars)	10,746 (2,518)	9,177 (2,059)	9,857 (2,294)
State population (thousands)	4,939 (5,301)	2,468 (1,583)	6,239 (4,406)
Proportion (percent) Aged 5–17	22.25 (3.61)	23.36 (3.43)	22.41 (3.76)
Proportion (%) Aged 65 and over	10.87 (2.07)	11.19 (2.00)	11.74 (2.89)
Proportion (percent) Fossil fuels	70.89 (31.95)	67.82 (26.11)	79.18 (17.52)
Proportion (percent) who elect a Democratic governor*	56.77 (49.57)	73.33 (44.30)	63.33 (48.32)
Number of states	32	10	6

Notes: Standard deviation in parentheses. See the data Appendix for source and definitions of variables. A “*” denotes statistically significant difference between states that appoint and select at percent significance level. The data on power interruptions is available only for 1984–1997. Proportion who elect a Democrat is percentage of years in which a Democrat holds the Governor’s chair.

types of states.²³ If states with elected regulators have more pro-consumer regulators, we would expect them to have lower prices. The results give an immediate suggestion that electors have lower prices than appointers—the difference is statistically significant at 5 percent for residential rates and at 10 percent for commercial and industrial rates. Figure 1 illustrates one of these key findings graphically—plotting the mean residential price per kilowatt hour for states that appoint (marpk) and that elect (merpk). This finding is only suggestive—there are a host of reasons why prices may differ that are not controlled for in the means.

One important influence on prices are cost variations over space and time. However, to assemble a measure of costs is not straightforward given the variety of production methods used. In reviewing the available technologies, Turvey and Anderson (1977) contrast the low marginal cost technologies of hydro and nuclear with higher marginal cost technologies of fossil fuels. (The three main fossil fuel sources are gas, coal, and oil.) Hydro generation and nuclear power tend, on the whole, to have higher fixed costs. It is much easier to get series that capture the prices of fossil fuels which have experienced dramatic price changes. This dramatic increase in costs in the 1970s is apparent from Figure 2,

23. We conduct the analysis in terms of nominal prices. Similar results are obtained when prices are deflated using the consumer price index.

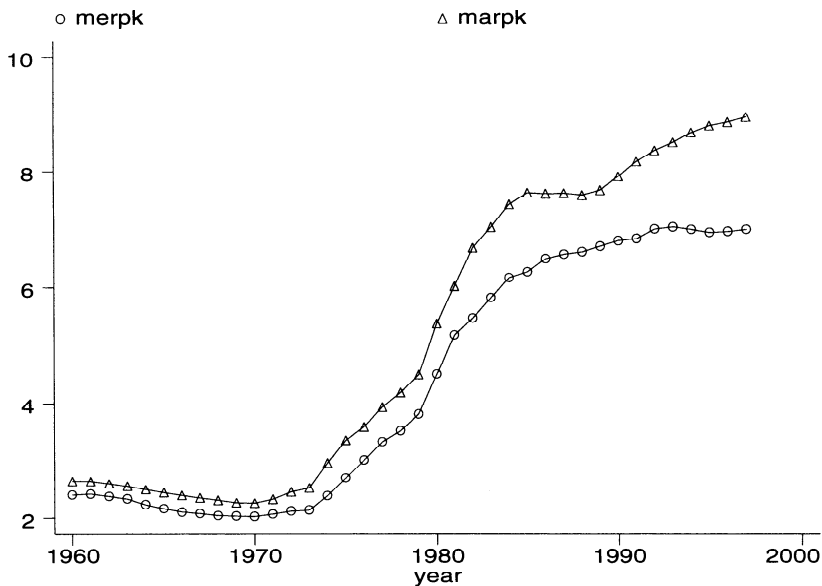


FIGURE 1. Mean Residential Prices over Time

Note: merpk is the annual average residential price of electricity per kilowatt hour for states that elected their public utility commissioners and marpk is the annual average residential price of electricity per kilowatt hour for states that appoint their public utility commissioners.

which graphs a composite fossil fuel price index (gas, coal, and oil) over time per BTU (the variable labeled *ffcomp*). The turbulent period from 1969 to the mid 1980s here is evident.

To measure cross state susceptibility to shocks, we focus on the fossil fuel component. States will have very different susceptibilities to these costs on account of their varying production structures. Our cost variable is constructed by multiplying the share (in total production) of electricity produced using each type of fossil fuel measured in BTUs weighted by a price series obtained from Energy Information Agency's Annual Energy Review. Figure 2 displays our series on the residential price of electricity (*mrpk*) and a state specific fossil fuel cost index per BTU (*mcost*)—both of these are annual averages for all forty-eight states. As expected, residential price increased along with costs in the 1970s. However, it keeps rising after costs declined in the late 1980s.²⁴

Table 2 looks at difference in the mean prices between electing and appointing states after controlling for (fossil fuel) costs, common macroshocks

24. However, the picture is somewhat different when looking at real rather than nominal prices. The 1960s saw falling real prices while prices increased in real terms in the 1980s. Real prices have been declining since.

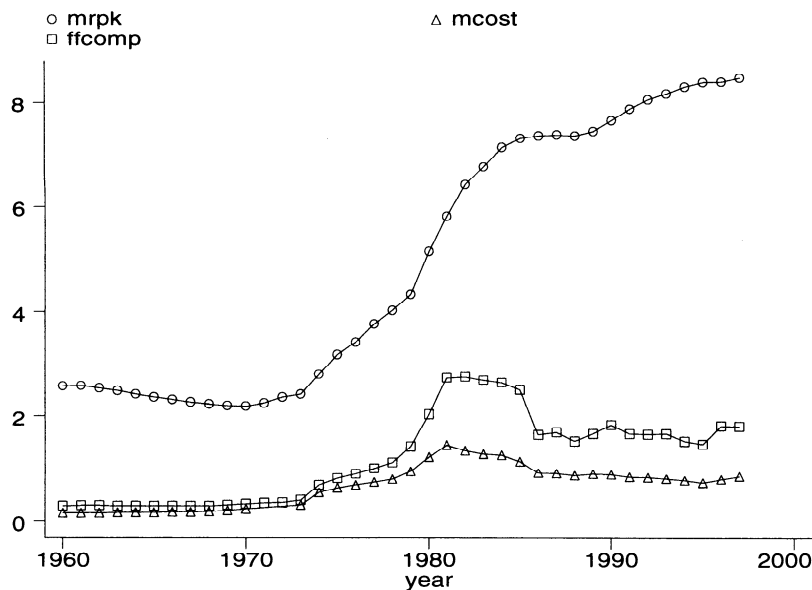


FIGURE 2. Fossil Fuel Price Index and Fuel Cost

Note: ffcomp is a fossil fuel price index, mcost is the annual average fossil fuel cost and mrpk is the annual average residential price of electricity per kilowatt hour.

and state-specific economic and demographic variables.²⁵ The results confirm the idea that price levels are lower in states that elect their public utility commissioners. However, the difference is only significant at a 5 percent level for residential prices. (Our cost variable (not reported) is strongly significant in all regressions.) To put this difference into perspective, at the mean household consumption of 10,000 kilowatt hours per annum, it implies a difference of around \$60 per annum on an average household’s electricity bills.

The results so far do not use data on the states that switched. However, it is interesting to look at how prices behaved before and after the transition between electing and appointing. The two most interesting cases are Florida and Texas both of whom switched roughly halfway through our data period. Here, we use the rank of these states’ electricity prices as the left-hand side variable—a purely ordinal

25. To be precise, we run a “standard” panel data regression on data from the forty-four states that did not switch their method of regulator selection between 1960 and 1997. This is of the form:

$$p_{st} = \alpha_s + \beta_t + \gamma c_{st} + \phi x_{st} + \varepsilon_{st}$$

where p_{st} is the average price per kilowatt hour for state s in year t ; α_s is a state-fixed effect, β_t are year dummy variables that pick up macroshocks and common changes in federal policy; c_{st} is our fossil fuel cost index in state s at time t ; x_{st} is a vector of state specific, time varying shocks (state income per capita, state income per capita squared, state population, state population squared, proportion aged 5–17 and proportion aged over 65). We then save the estimated fixed effects and run a regression of these on a dummy variable, δ_s , that is equal to one if the state elects its regulator. The coefficient θ is reported in Table 2.

TABLE 2. MEAN DIFFERENCES IN PRICES CONTROLLING FOR YEAR EFFECTS, COSTS, AND ECONOMIC CONTROLS

	(1) Residential	(2) Commercial	(3) Industrial
State elects public utility commissioner	−0.73 (2.17)	−0.45 (1.45)	−0.43 (1.57)
Implied percent price reduction in states that elect their public utility commissioners	−14.78	−9.96	−14.43

Notes: Absolute values of *t*-statistic is in parentheses. Footnote 25 in the text explains the exact method of estimation used to generate this coefficient. All regressions use robust standard errors with robust standard. They are run only on the forty-two states that did not switch their method of selection over the period 1960–1997. See the data Appendix for sources and definitions of variables.

measure of the residential electricity prices. Figure 3 and 4 graph these ranks before and after switching from election to appointment. In each case, moving from electing to appointing increases the state’s rank. The notes to the figures also give the results of regressing the rank on the change, confirming that in both cases, the move from election to appointment was associated with higher electricity prices. While concerns about endogeneity of the switching preclude reading too much into this, it is supportive of the basic findings above.

The results on price levels consistently support Prediction 1. Moreover, the result is most pronounced for residential rates. This finding holds up even after controlling for economic and demographic controls and state-specific fossil fuel

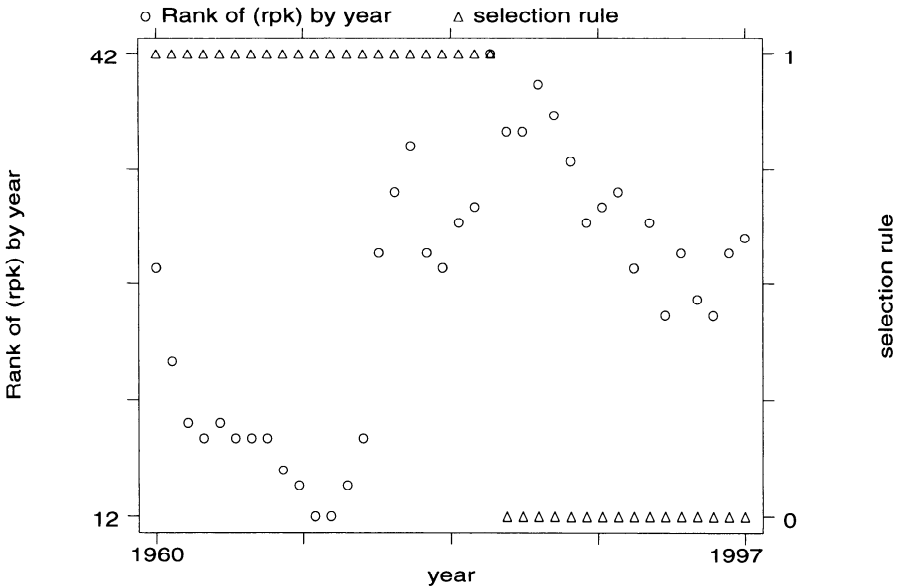


FIGURE 3. Effect of Switching from Electing to Appointing on Rank of Electricity Price in Florida (Rank = 31.62 − (8.98)*elect: *t*-value 3.74)

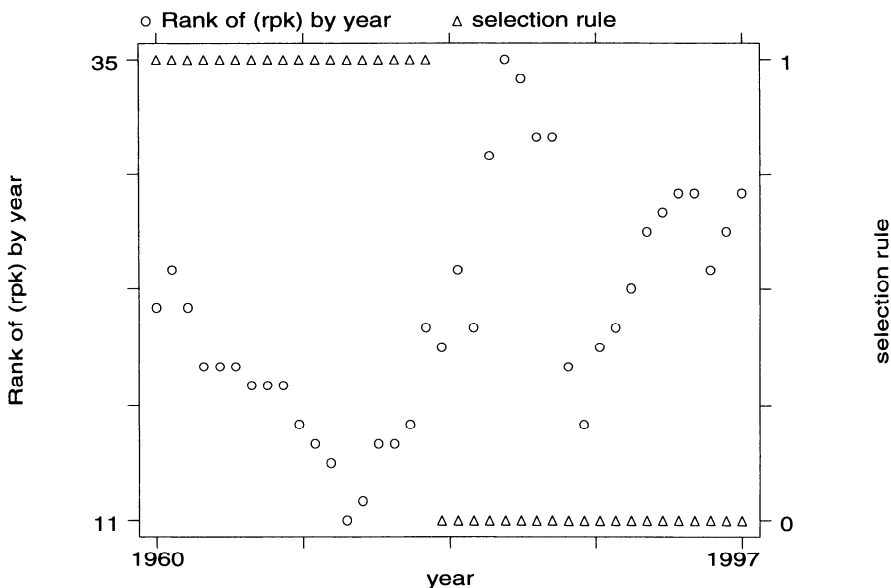


FIGURE 4. Effect of Switching from Electing to Appointing on Rank of Electricity Price in Texas ($\text{Rank} = 25.60 - (8.16) \cdot \text{elect}$; t -value 5.62)
Note: Select = 1 if the state elects its utility commissioners. The rank is the cross-sectional rank in the distribution of residential prices of the state in each year.

costs. This contrasts with the previous literature which has reached mixed conclusions about whether election produces more pro-consumer outcomes. Most of the previous evidence comes from looking at cross-sectional evidence from specific years. When we run separate cross-sectional regressions for each year of our data (including economic controls and the state-specific cost index), we found that electing regulators has a negative and significant effect only for the period 1975–1980. Otherwise, it is not significantly different from zero. The fact that we obtain sharper results on price level differences from panel data comes mainly from the fact that we are able to control for time-varying regressors. The negative coefficient on electing regulators holds up in the panel when it is included directly in a regression that also includes time varying controls for costs, economic, and demographic variables along with year-fixed effects.²⁶

While consistent with the theory, there remains the possibility that the regulatory regime is selected nonrandomly by a process that is related to price determination. In light of this, it would be incautious to interpret the finding as a causal effect. Regulatory regimes do not change often enough in our data period to exploit such time-series variation in any satisfactory way. Moreover,

26. It does not hold up once state-fixed effects are included. However, this is not surprising given the small amount of time series variation in the mode of regulator selection.

to do so would require a model of regulatory regime change. It is clear that understanding the factors that drive the choice of a regulatory regime provides an important agenda for future research.

7.2 Pass-Through

We test Prediction 2 above, by running panel regressions of the form:

$$p_{st} = \alpha_s + \beta_t + \gamma_1 c_{st} + \gamma_2 \delta_s c_{st} + \phi x_{st} + \varepsilon_{st}$$

where p_{st} is the average price per kilowatt hour for state s in year t ; α_s are state-fixed effects proxying for long-run differences in states' production and distribution systems due to climate etc; β_t are year dummy variables that pick up macroshocks and common changes in federal policy; x_{st} is a vector of state-specific, time-varying shocks (state income per capita, state income per capita squared, state population, state population squared, proportion aged 5–17 and proportion aged over 65); and δ_s is a dummy variable equal to one if the state elects its regulator and zero otherwise. To control crudely for differences in production structures across states, we also include in the x_{st} vector the fraction of fuel generated from fossil fuel sources. The variable c_{st} is our fossil fuel cost index in state s at time t . The key implication to be tested is that $\gamma_2 < 0$; i.e., prices should respond less to cost shocks when states elect their regulators. In all case, we report robust standard errors which allow for clustering by census region.

These results identify the difference between appointed and elected regulators from an interaction with a time-varying regressor (production costs). By controlling for state-fixed effects, they minimize concerns about the correlation between the regulatory regime and other sources of long-run heterogeneity among the states that drive the selection of the regulatory regime.

The basic results are given in Table 3, again for the forty-two states that did not switch between electing and appointing over the sample period. Columns (1)–(3) give results in each of three categories of provision where differences between states and over time are controlled for solely with state, year effects, and the fraction of energy produced from fossil fuels. The key observation is that the coefficient on costs interacted with whether a state elects its regulators is everywhere negative. This is robust to including a number of economic and demographic controls as shown in Columns (4)–(6). These results suggest that the pass-through coefficient for the elected states is roughly one half the coefficient for appointed states in Columns (1)–(3). The gap between electing and appointing states is somewhat smaller in Columns (4)–(6) with the largest difference being for residential prices.

TABLE 3. RESULTS ON PASS-THROUGH

	(1)	(2)	(3)	(4)	(5)	(6)
	Residential	Commercial	Industrial	Residential	Commercial	Industrial
Fossil fuel cost	0.87 (9.20)	0.81 (5.40)	0.88 (7.15)	0.75 (5.28)	0.70 (3.79)	0.78 (5.72)
Elect* fossil fuel cost	-0.40 (1.80)	-0.29 (1.43)	-0.40 (2.14)	-0.26 (2.21)	-0.12 (2.40)	-0.21 (2.60)
Fossil fuel share	-3.02 (5.18)	-2.46 (6.31)	-2.04 (3.33)	-2.32 (8.61)	-1.94 (9.13)	-1.44 (5.40)
Economic controls	No	No	No	Yes	Yes	Yes
State effects	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,487	1,487	1,487	1,446	1,446	1,446
R ²	0.94	0.94	0.93	0.96	0.95	0.95

Notes: See the data Appendix for sources and definitions of variables. These regressions are run on the sample of forty-two continental states that did not switch between electing and appointing their regulators during the period 1960–1997. All regressions use robust standard errors allowing for clustering by census region. Absolute values of *t*-statistics are in parentheses. Other controls are state income per capita, state income per capita squared, state population, state population squared, proportion aged over 65, proportion aged 5–17.

7.3 Robustness

We have subjected the results to a variety of robustness checks. The results on pass through of costs are robust to including the full sample of states.²⁷ We also experimented with various controls for differences in production structure across states, including the share of noninvestor owned production and production from nuclear sources. While this does lead to a more restricted sample of years, the results on pass through remain robust. The results are also robust to including an indication of whether the state had an automatic fuel adjustment clause.²⁸

The data suggest three distinct “regimes” over the period that we study. The 1960s saw stagnant prices, followed by a period of rising prices, peaking in the mid 1980s after which nominal input prices drifted downwards. Joskow (1974) observes that the influence of regulators on prices is likely to be much more important in an environment where input prices are rising, since rate reviews are most likely in such periods. According to this argument, regulator influence should become more important in the post-1969 period when fossil fuel prices increased dramatically. Following Joskow (1974), we look to see whether the effect of regulatory institutions are most apparent in the middle period. The results suggest that the middle period is the most important for explaining the overall results. Joskow (1974) also suggests the possibility that there could be

27. However, there is a positive coefficient on a state that elects its regulators—this being identified purely off the time series variation in six states. This may suggest that changing the method of selection may be bound up with the pricing process for these states.

28. We were only able to obtain data on this for the years 1973–1995 excluding (1978 and 1990).

asymmetries between periods of cost increases and decreases. We therefore allowed for the cost pass through to vary between cases where there had been increases and decreases in costs. We find in favor of Joskow's idea that cost increases are passed on more readily than cost decreases. The difference between electing and appointing states, however, remains except for industrial prices and is more pronounced for cost increases.

We also interacted our cost variables with other state characteristics (e.g., income and population) that Table 1 reveals are different in the electing and appointing states. This did not disturb our basic result that states that elect put up prices less in response to fossil fuel price shocks. The analysis does not exploit other dimensions of state regulatory policy such as the length of terms served by commissioners, the level of payment to regulators and the sources of funding for regulatory commissions. When we included such variables in the analysis, no consistent pattern in relation to pricing decisions emerged. However, clearly there is further work to be done on these other aspects of regulatory decisions.²⁹ Finally, we considered whether the results are robust to disaggregating the cost variable into oil, gas, and coal costs. Here we found that the pass-through results are driven mainly by oil and gas cost changes.

8. Conclusion

This paper has explored the theoretical and empirical basis for the claim that electing public utility commissioners will yield regulatory policies that are more pro-consumer. Making sense of this theoretically requires a model in which outcomes can diverge from what a majority of citizens would like to see. The theoretical explanation of this here rests on issue bundling—the fact that regulatory policy becomes bundled with other issues when regulators are appointed. This can lead to a regulatory policy that favors industry stakeholders as political parties see this as a way of enhancing their political agenda on nonregulatory issues. This idea has wider significance for the political economics literature. Its implications for citizens' initiatives are discussed in Besley and Coate (2000).

The results on price levels and pass-through both add credence to the theoretical approach. In each case, the evidence is consistent with the view that directly electing regulators yields more pro-consumer outcomes. Even if this

29. It is interesting to note that Navarro (1982) finds that states that elect their regulators show a more unfavorable regulatory climate according to his rankings of states gleaned from a number of commercial organizations. This is consistent with our findings although it is possible that states with elected commissioners also have stronger regulatory institutions. However, looking at the measures from Norton (1985), the only states that elected commissioners in his sample were classified as weakly regulated (Norton 1985, Table 1). (See also Costello 1984, Table 7.) Gormley (1981) observes that consumer movements are much more likely to be active in states where the public utility commissioner is appointed.

conclusion is accepted, there is still a question of who is paying for the lower electricity prices achieved. In the theoretical model, lower prices simply shift rents from stakeholders to consumers. In this case, the welfare conclusions depend upon the relative weights that are placed on the payoffs of these two groups. Since (under Assumption 1) the probability distribution over public spending is the same under both regimes, electing utility commissioners is welfare enhancing if and only if this rent transfer is desirable. However, it is likely that lower prices have effects on other decisions, particularly the decision to invest. Future work might usefully investigate the relationship between service quality and regulatory regime.³⁰

Appendix

Proof of Proposition 2

To prove the proposition, we show that if Assumption 1 is satisfied, the unique equilibrium involves Party *L* selecting a type (*L*, *S*) candidate and Party *R* selecting a type (*R*, *S*) candidate. The strategy of proof will be to show that the game is solvable by iterated strict dominance.

Step 1: It is easy to show that for Party *L* the pure strategy (*R*, *S*) is strictly dominated by the strategy (*L*, *C*) when Party *R*'s set of pure strategies is {(*L*, *C*), (*L*, *S*), (*R*, *C*), (*R*, *S*)}. It is also the case that for Party *L* the pure strategy (*R*, *C*) is strictly dominated by the strategy (*L*, *S*). To see this, suppose first that Party *R* selects (*L*, *C*). Then the expected payoff of Party *L*'s majority member if it chooses (*R*, *C*) would be:

$$\psi(\gamma^L - \gamma^R)b(g^*(L), L) + (1 - \psi(\gamma^L - \gamma^R))b(g^*(R), L) + r(p^*(C), C).$$

If it chooses (*L*, *S*) the payoff of its majority member would be:

$$b(g^*(L), L) + \psi(\gamma_C - \gamma_S)r(p^*(C), C) + (1 - \psi(\gamma_C - \gamma_S))r(p^*(S), C).$$

Subtracting the former from the latter, the difference is positive by part (i) of Assumption 1.

Next suppose that Party *R* selects (*R*, *C*). Then the expected payoff of Party *L*'s majority member if it chooses (*R*, *C*) would be:

$$b(g^*(R), L) + r(p^*(C), C).$$

30. An earlier version of this paper contained some preliminary findings suggesting that service reliability is lower in states that elect their regulators.

If it chooses (L, S) the payoff of its majority member would be:

$$\begin{aligned} &\psi(\gamma^L + \gamma_S^R - \gamma_C^R)[b(g^*(L), L) + r(p^*(S), C)] \\ &\quad + [1 - \psi(\gamma^L + \gamma_S^R - \gamma_C^R)][b(g^*(R), L) + r(p^*(C), C)]. \end{aligned}$$

Subtracting the former from the latter, the difference is positive since $\Delta b(L) > \Delta r(C)$ by assumption.

Now suppose that Party R selects (R, S) . Then the expected payoff of Party L 's majority member if it chooses (R, C) would be:

$$b(g^*(R), L) + \psi(\gamma_C - \gamma_S)r(p^*(C), C) + (1 - \psi(\gamma_C - \gamma_S))r(p^*(S), C).$$

If it choose (L, S) the payoff of its majority member would be:

$$\psi(\gamma^L - \gamma^R)b(g^*(L), L) + (1 - \psi(\gamma^L - \gamma^R))b(g^*(R), L) + r(p^*(S), C).$$

Subtracting the former from the latter, the difference is positive by part (i) of Assumption 1.

Finally, suppose that Party R selects (L, S) . Then the expected payoff of Party L 's majority member if it chooses (R, C) would be:

$$\begin{aligned} &\psi(\gamma^L + \gamma_S^R - \gamma_C^R)[b(g^*(L), L) + r(p^*(S), C)] \\ &\quad + [1 - \psi(\gamma^L + \gamma_S^R - \gamma_C^R)][b(g^*(R), L) + r(p^*(C), C)]. \end{aligned}$$

If it chooses (L, S) the payoff of its majority member would be:

$$b(g^*(L), L) + r(p^*(S), C).$$

Subtracting the former from the latter, the difference is positive since $\Delta b(L) > \Delta r(C)$ by assumption.

Similarly, for Party R the pure strategy (L, S) is strictly dominated by the strategy (R, C) and the pure strategy (L, C) is strictly dominated by the strategy (R, S) when Party L 's set of pure strategies is $\{(L, C), (L, S), (R, C), (R, S)\}$.

Step 2: We claim that for Party L the pure strategy (L, C) is strictly dominated by the strategy (L, S) when Party R 's set of pure strategies is $\{(R, C), (R, S)\}$. Suppose first that Party R selects (R, C) . Then the expected payoff of Party L 's majority member if it chooses (L, C) would be:

$$\psi(\gamma^L - \gamma^R)b(g^*(L), L) + (1 - \psi(\gamma^L - \gamma^R))b(g^*(R), L) + r(p^*(C), C).$$

If it chooses (L, S) the payoff of its majority member would be:

$$\begin{aligned} &\psi(\gamma^L + \gamma_S^R - \gamma_C^R)[b(g^*(L), L) + r(p^*(S), C)] \\ &\quad + [1 - \psi(\gamma^L + \gamma_S^R - \gamma_C^R)][b(g^*(R), L) + r(p^*(C), C)]. \end{aligned}$$

Subtracting the former from the latter, the difference is positive by part (iii) of Assumption 1. Next suppose that Party *R* selects (*R*, *S*). Then the expected payoff of Party *L*'s majority member if it chooses (*L*, *C*) would be:

$$\psi(\gamma_C^L - \gamma_S^R - \gamma^R)[b(g^*(L), L) + r(p^*(C), C)] \\ + (1 - \psi(\gamma_C^L - \gamma_S^R - \gamma^R))[b(g^*(R), L) + r(p^*(S), C)].$$

If it chooses (*L*, *S*) the payoff of its majority member would be:

$$\psi(\gamma^L - \gamma^R)b(g^*(L), L) + (1 - \psi(\gamma^L - \gamma^R))b(g^*(R), L) + r(p^*(S), C).$$

Subtracting the former from the latter, the difference is positive by part (ii) of Assumption 1.

Similarly, for Party *R* the pure strategy (*R*, *C*) is strictly dominated by the strategy (*R*, *S*) when Party *L*'s set of pure strategies is {(*L*, *C*), (*L*, *S*)}.

Thus, the game is solvable by iterated strict dominance and the solution is the strategy pair {(*L*, *S*), (*R*, *S*)}. This is the unique equilibrium. ■

APPENDIX TABLE A.1. LIST OF STATES

Elect: (10)	AZ, GA, LA, MS, MT, ND, NE, OK, SD
Appoint: (32)	AK, AR, CA, CO, CT, DE, ID, IL, IN, KS, KY, MA, MD, ME, MI, MO, NC, NH, NJ, NM, NV, NY, OH, OR, PA, RI, VT, WA, WI, WY, WV
Switch: (6)	FL, IA, MN, SC, TN, TX

Notes: Florida switched from elect to appoint in 1981. Iowa switched to an election system for 1962 and 1963 only. Minnesota used election 1960–1971, appointing 1972–1975 and electing from 1976–1977 and appointment since 1978. South Carolina switched to an election system in 1996. Texas switched to an appointment system in 1977. Tennessee switched to an appointment system in 1996.

Data

1. Data for electric prices, electricity generation, and fuel prices are directly collected or calculated from the EEI yearbooks.
1960–1992: Historical Statistics of the Electric Utility Industry, 1995, EEI, Washington, D.C.
1993–1997: Statistical Yearbook of the Electric Utility Industry, 1993–1997, EEI, Washington, D.C.
EEI refers to the source of data for its yearbooks to various places including U.S. Department of Energy, Energy Information Administration, Federal Power Commission, and Federal Energy Regulatory Commission.
a) Electric Prices for Residential, Commercial and Industrial Sectors: EEI reports annual revenues (in dollar terms) and sales (in kilowatt-hours) of total electric utility industry by state and class of service. The prices are calculated from the revenues and sales in terms of dollars per kilowatt-

hour. Besides the three sectors that are reported here, there are four other sectors categorized in the EEI yearbooks: street and highway lighting, other public authorities, railroads and railways, and interdepartmental. The three sectors take more than 95 percent of the revenues and sales throughout the years.

- b) Electric Generation and Sources of Energy for Electric Generation: EEI reports two kinds of breakdown of electric generation: (1) by type of prime mover driving the generator and (2) by energy source. The totals from each different breakdown are consistent. We have used the second breakdown here, and it consists of coal, fuel oil, gas, nuclear fuel, and hydro. There is one other source of energy that is reported to EEI is “other,” which includes generation by geothermal, wood, waste, wind, and solar. The generation by “other” is within 1–3 percent of total and affect only a small number of states. Generation by “hydro” was initially reported in the first category, type of prime mover, but from 1984 onwards, it was reported in both categories. Our data for “hydro” for 1960–1983 are from the first category. EEI consistently reported that for 1960–1983 the total generation in the second category is smaller than the one in the first category by the amount of “hydro.” As mentioned earlier, because of the “other” the total generation is not equal to the sum of the generation by different sources in a few states. All values less than 500,000 kilowatt-hours are recorded as zero, as they are reported blank in EEI data.
2. Data on prices of fossil fuels reported in kilowatt hours came from the Energy Information Administration, Annual Energy Review, 1998, Table 3.1 and denoted in dollars per British Thermal Unit (BTU) available at <http://www.eia.doe.gov/emeu/aer/finance.html>. To construct the fossil fuel cost index for state i in year t , let s_{jit} be the share of energy source j in state i in year t and let p_{jt} be the price per BTU. Then the cost index $c_{it} = \sum_j s_{jit} p_{jt}$.
3. Data for commissioners are from the state yearbooks.
1960–1997: The Book of the States, 1960–1997, Council of State Governments, Lexington, Kentucky. There are seven methods of selecting commissioners in our data. (The proportion of observations in each category are given in parentheses.)
 - i. Direct election (26.13 percent)
 - ii. Appointed by Governor (19.12 percent)
 - iii. Appointed by Governor with confirmation by the Senate (45.88 percent)
 - iv. Appointed by Governor with confirmation by executive council (2.85 percent)
 - v. Appointed by Governor with approval by legislature (2.20 percent)
 - vi. Selected by general assembly (0.66 percent)
 - vii. Selected by Legislature (3.07 percent)

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Reflections on Appointive Local Government Bodies and a Right to an Election

*Sheldon H. Nahmod**

While few would dispute the importance in a representative democracy of the right to vote for government officials, there appears to be relatively little discussion of what might be termed an individual's constitutional right to an election.¹ Granted, the reapportionment cases² and other voting rights cases³ direct attention to vote dilution and voters' qualifications, but they involve situations where elections have already been provided. Suppose, however, that elected officials of a properly apportioned municipality are permitted by statute to appoint members of a school board which will be exercising important governmental powers, such as the power to levy and collect taxes, issue bonds, and acquire property by condemnation.⁴ Might an argument be made that those school board members must constitutionally be elected by their constituents? What could be the constitutional source and judicial standards of such a right to an election? And, what might be the implications of this right?

This article will explore these questions, beginning with the reap-

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1. See, e.g., *Sailors v. Board of Educ.*, 387 U.S. 105, 109-10 (1967): "We need not decide at the present time whether a state may constitute a local legislative body through the appointive rather than the elective process." See also F. MICHELMAN & T. SANDALOW, *MATERIALS ON GOVERNMENT IN URBAN AREAS* 180, 913 (1970) [hereinafter cited as MICHELMAN & SANDALOW].

2. See, e.g., *Reynolds v. Sims*, 377 U.S. 533 (1964); *Baker v. Carr*, 369 U.S. 186 (1962).

3. See, e.g., *Kramer v. Union Free School Dist.*, 395 U.S. 621 (1969).

4. The Supreme Court has characterized powers of this sort as significant for reapportionment purposes because they are general and have considerable impact throughout the geographical area in which they are exercised. *Hadley v. Junior College Dist.*, 397 U.S. 50, 53-54 (1970).

portionment cases which prompted them. The following discussion is primarily intended to stimulate further thinking in this area.

THE REAPPORTIONMENT CASES

Although the reapportionment cases are not in themselves authority for a constitutional right to an election, an analysis of several of the more recent cases is helpful in speculating about such a right. One such case, *Avery v. Midland County*,⁵ the first Supreme Court decision applying "one man, one vote" to local government, involved the Midland County (Texas) Commissioners Court, four of whose five members were elected from districts of widely varying populations. This body, *inter alia*, set a county-wide tax rate, equalized assessments, issued bonds, and prepared and adopted a budget for allocating the county's funds. Utilizing a "power-impact" test, the Court found that the commissioners court had the power "to make a large number of decisions having a broad range of impacts on all the citizens of the county,"⁶ and characterized these as "general governmental powers" over the entire county. On this basis, the Court held unconstitutional the statutory election scheme.

This decision was not unexpected. Referring to predictions that a decision like *Avery* was likely, and even desirable, Justice Clark was heartened by "the possibility of educated forecasts of Supreme Court trends even without computerization."⁷ In contrast, the 1970 decision in *Hadley v. Junior College District*⁸ was less predictable. *Hadley* involved the validity of Missouri legislation authorizing the formation of junior college districts geographically composed of smaller school districts. Voters in each school district elected the trustees of the junior college school district according to a formula that was not based proportionately on population. In holding the scheme invalid, the Court applied "one man, one vote" to the election of the trustees. Hence, *Hadley* was a significant extension of *Avery* in several respects. First, *Hadley* involved a governmental unit in which the voters of each school district had voluntarily decided by referendum to participate while the scheme in *Avery* both set up a county commissioners court

5. 390 U.S. 474 (1968).

6. *Id.* at 483.

7. *Symposium—One Man-One Vote and Local Government*, 36 GEO. WASH. L. REV. 689 (1968).

8. 397 U.S. 50 (1970).

and determined the number of court members to be elected by the voters of each district. In addition, *Hadley* involved a special purpose unit. The elected officials exercised their powers, which included tax levy and collection, bond issuance, school district annexation and eminent domain, for a specific and limited educational purpose. In *Avery*, however, the commissioners court could exercise powers for purposes affecting almost every county resident in many areas of his life; it decided "whether industry should be solicited, roads improved, recreational facilities built and land set aside for school."⁹

Had the Court in *Hadley* so chosen, it could have recognized a distinction, for reapportionment purposes, between elections of officials who may exercise their powers for broad governmental purposes and elections of those whose powers may be exercised only for a specific purpose, such as education. Justice Harlan, dissenting, suggested that the justification for such a distinction could have been couched in terms of the arguably greater degree of flexibility necessary for the effective functioning of special purpose units.¹⁰ Yet, this distinction would not be persuasive under a power-impact test, for it may well be that the powers and impact of a special purpose unit on its constituents are as important as those of a general purpose unit. For example, the Court pointed out in *Brown v. Board of Education*¹¹ that providing education is crucial in a society which seems to offer its best primarily to those with a quality education. This view of the importance of education is also implicit in *Hadley* where the Court emphasized that "[e]ducation has traditionally been a vital government function"¹²

For the purpose of articulating a *rationale* for a right to an election, the *Hadley* Court's decision *not* to take into account the purpose of a particular governmental unit in applying "one man, one vote" seems significant. After pointing out the difficulty in distinguishing between various elections,¹³ the Court indicated that the best, and perhaps the only, way of determining the importance of choosing a particular governmental official" is the "decision of the State to select that official by popular vote."¹⁴ This could signal that the *Hadley* Court was moving away from applying a power-impact test which would assess a local unit's

9. 390 U.S. at 483.

10. 397 U.S. at 59.

11. 347 U.S. 483 (1954).

12. 397 U.S. at 56.

13. *Id.* at 55.

14. *Id.*

powers and their impact upon the unit's constituents before applying "one man, one vote." Justice Harlan apparently saw such a movement because he expressed concern that *Hadley* "forebodes, if indeed the case does not decide, that the rule is to be applied to every elective public body, no matter what its nature."¹⁵ Whether this is a correct reading of *Hadley* is uncertain, especially in view of the *Hadley* Court's apparent reliance on the power-impact test first set out in *Avery*.¹⁶ What is clear from *Hadley* is considerable, if not conclusive, reliance upon the state's decision to hold popular elections.

However, should the concern for participation in an election process depend upon the prior existence of an election? For example, appointing the school board members in the hypothetical given at the outset could deny to affected citizens a meaningful voice in the selection process. This result is not justified by suggesting that the complaining citizen's recourse should be to the properly apportioned municipality or legislature where he might exert political pressure on an equal footing with others. For this suggestion to citizens could, arguably, also justify the election from malapportioned districts of local officials to a special purpose unit where the municipality and legislature are properly apportioned. The holdings in *Avery* and *Hadley* indicate, however, that it does not. Consequently, something more seems at stake in the *Avery* and *Hadley* cases than recourse to a higher, politically accountable general purpose governmental unit.

What is at stake was aptly characterized by the Court in *Avery* when it stated: "[I]nstitutions of local government have always been a major aspect of our system, and their responsible and responsive operation is today of increasing importance to the quality of life of more and more of our citizens."¹⁷ In view of a citizen's interest in "responsible and responsive" local units,¹⁸ perhaps the existence of elections ought not

15. *Id.* at 60.

16. It is also worth noting that the majority opinion in *Hadley* stated:

It is of course possible that there might be some case in which a state elects certain functionaries whose duties are so far removed from normal governmental activities and so disproportionately affect different groups that a popular election in compliance with ["one man, one vote"] might not be required

Id. at 56.

17. 390 U.S. at 481.

18. This is not to suggest, of course, that direct elections guarantee responsible and responsive government. The point is that an election provides at least the opportunity to have some say in selecting the persons whose decisions will affect citizens. This will often assure some citizen input into the kinds of decisions made. The extent of their "say" and "input" beyond individual votes is uncertain, however, in view of *Whitcomb v. Chavis*, 403 U.S. 124 (1971). There the Court held that ghetto residents who claimed that Indiana's

to depend *exclusively* on a decision of the legislature or the municipality to have them. For if the interest in participatory democracy is so strong as to mandate under the Constitution the *equal* opportunity to participate, then this same interest could perhaps serve as the basis for a requirement that an election be held in certain circumstances, thereby providing the *opportunity itself* to participate. Conceivably, even officials of some special purpose units ought to be directly accountable to their constituents—those upon whom the impact of the unit's decisions will fall—regardless of whether it is the state or the municipality which has not provided for an election.

This argument for serious consideration of a right to an election appears at first blush to have been rejected by the Supreme Court in *Sailors v. Board of Education*.¹⁹ There, local school boards that were popularly elected in turn appointed all five members of the county school board. This scheme was upheld against a challenge to the county board based on its non-compliance with "one man, one vote." The Court seemed to indicate that "one man, one vote" simply did not apply where the state had not provided for an election.

Sailors, however, is distinguishable from the hypothetical school board situation which involves the exercise of powers of a legislative character. In *Sailors*, the Court concluded that the local county board officials exercised powers of a non-legislative character. Surprisingly, these supposedly non-legislative powers included the preparation of an annual budget, the levy of taxes, and the power to transfer areas from one school district to another. The *Sailors* characterization of such powers as non-legislative now appears questionable in view of the county commissioners court's similar powers in *Avery*. Still, a court could distinguish the hypothetical school board from the county school board in *Sailors* because the latter case, correctly or not, placed great reliance upon the asserted non-legislative character of the county board. Furthermore, the Court in *Sailors* emphasized that it was not deciding "whether a State may constitute a local legislative [as distinguished from a non-legislative] body through the appointive rather than the elective process."²⁰ It thereby avoided the issue of a right to an election which the hypothetical and this discussion are intended to raise.

multimember districts prevented their effective representation in the legislature had not met their substantial burden of showing inadequate representation of their interests.

19. 387 U.S. 105 (1967); cf. *Dusch v. Davis*, 387 U.S. 112 (1967).

20. *Id.* at 109-10.

THE RIGHT AND ITS CONSTITUTIONAL BASIS

If one concludes from the foregoing discussion that there are reasons for the existence of a right to an election, its constitutional basis must then be identified. The due process clause is one possibility. It has been intimated, for example, that a legislature should not be permitted to vest governmental authority over matters concededly local in persons not locally elected, because due process might require the direct participation of affected citizens voting in the selection of that governing body.²¹ However, unlike the extent of the federal judiciary's experience with the equal protection clause in a voting context, there has been little, if any, such experience with the due process clause. Consequently, there would be relatively few judicial standards to use in determining whether a right to an election has been violated.²²

If the right were to be based on the equal protection clause, the problem of judicial standards and experience, as will be seen, is somewhat diminished. Unlike the due process clause, the equal protection clause would require some kind of state classification; generally speaking, equal protection questions only arise where the state has first classified persons for the purpose of receiving benefits or incurring detriments. In the *Avery* and *Hadley* cases, for example, it was the state's discriminatory treatment of voters who resided in different areas that was held to violate equal protection. The hypothetical school board scheme would appear to involve no discrimination between voters since there is no provision for an election.

The right to an election, however, could conceivably be grounded on a theory, akin to substantive due process, of "minimum equal protection," an approach which has been discussed in the context of a "right to an education."²³ This approach would require the state to eradicate differences, based on wealth differential, in access to certain rights and benefits, such as public education, and is analytically derived from Su-

21. MICHELMAN & SANDALOW, *supra* note 1, at 180. See also Herget, *The Impact of the Fourteenth Amendment on the Structure of Metropolitan and Regional Governments*, 23 HASTINGS L.J. 763, 781 (1972) [hereinafter cited as Herget].

22. Nevertheless, there are situations discussed later, see notes 36-45 *infra*, where the due process clause might be the best vehicle for a right to an election. See note 36 *infra*. The equal protection clause is emphasized here because the power-impact test was articulated in connection with it.

The guaranty clause of the Federal Constitution has been determined by the Supreme Court to be nonjusticiable. *Baker v. Carr*, 369 U.S. 186, 223-26 (1962). Hence, its possible applicability to this discussion is not considered. But see R. DIXON, *DEMOCRATIC REPRESENTATION* 136 (1968).

23. Michelman, *The Supreme Court, 1968 Term—Forward: On Protecting the Poor Through the Fourteenth Amendment*, 83 HARV. L. REV. 7 (1969).

Appointive Local Government Bodies

preme Court decisions involving criminal process and voting rights. But there is considerable doubt whether a right to an education, standing alone and apart from racial discrimination, involves an interest significant enough to require the state to make *some* educational offering under a minimum equal protection theory. Even recent school financing cases such as *Serrano v. Priest*,²⁴ while they characterize education as a fundamental interest, are not authority for a minimum equal protection approach because they involved discrimination by the state.

However, after the reapportionment cases, no such doubt exists as to the propriety of characterizing voting rights generally as a fundamental interest. The right to an election, apparently on firmer "fundamental interest" ground than the right to an education, might be a more appropriate vehicle for a minimum equal protection approach. In addition, the difficulty in elaborating what constitutes this "minimum" is greater for the right to an education than it is for the right to an election. "Education," much less "minimum" education, has not been consistently or coherently defined even by educators, while an "election," though it can vary in its details, is in essence a reasonably well articulated democratic process. Assuming therefore that a court is sympathetic to a minimum equal protection approach, there could conceivably be less judicial reluctance in ordering an "election" than an "education." However, the Supreme Court's implicit rejection of a minimum equal protection approach in *Dandridge v. Williams*,²⁵ a welfare case, should prevent a court from adopting it even in a voting context.

A different approach would involve more traditional equal protection considerations. If in our hypothetical scheme *other* municipalities within the state were permitted to *elect* their school board officials it could be argued, based on *Griffin v. County School Board*,²⁶ that the voters within the hypothetical school district are denied equal protection. Read very broadly, *Griffin* might hold that territorial discrimination by the state must meet the test of the equal protection clause.²⁷ At least where voting rights are endangered, a denial of equal protection might exist where a legislature did not require elections for a particular

24. 5 Cal. 3d 584, 487 P.2d 1241, 96 Cal. Rptr. 601 (1971).

25. 397 U.S. 471 (1970).

26. 377 U.S. 218 (1964).

27. Cf. *McGowan v. Maryland*, 366 U.S. 420 (1961). See generally Horowitz & Neitring, *Equal Protection Aspects in Public Education and Public Assistance Programs from Place to Place Within a State*, 15 U.C.L.A.L. Rev. 787, 796 (1968).

school board while requiring them for all other school boards. It might also exist where a municipality itself, under a local option statute, in fact exercised local option and decided not to elect its school board. In *Reynolds v. Sims*,²⁸ the Court held invalid a legislature reapportionment scheme which had been approved by the voters in the state. This would indicate that if the right to an election were accorded constitutional status similar to the constitutional right in the reapportionment cases, it could not be abridged by a vote of the residents in a municipality. Such a conclusion would also be consistent with the result in *Hadley* where the Court held invalid a statutory apportionment scheme for the election of junior college trustees even though each of the member school districts had decided by voter referendum to participate voluntarily.

Using an equal protection approach in this context, a court could justify its intervention on the ground that voting rights are a "fundamental interest" which is to be protected by *active judicial review*.²⁹ In reapportionment cases, the citizen's interest has been characterized as the equality in distribution of political power stemming from the right to vote for a legislative representative.³⁰ As noted earlier, these decisions have repeatedly emphasized that this interest is clearly "fundamental," requiring that any state impairment of this interest be justified by a compelling state interest. In addition to the reapportionment cases, *active judicial review* is apparent from decisions which have held unconstitutional, as a violation of equal protection, certain property qualifications for voting in both general and school board elections.³¹ Territorial discrimination of the sort involved in the hypothetical situation, which implicates voting rights, could similarly require for its justification that the state show a compelling state interest and not merely a rational basis.

THE RIGHT AND JUDICIAL STANDARDS

Closely related to the discussion of a constitutional basis is the question of the judicial standards that could be used in determining the

28. 377 U.S. 533 (1964).

29. See *Developments in the Law—Equal Protection*, 82 HARV. L. REV. 1065, 1124-33 (1969).

30. Averbach, *The Reapportionment Cases: One Person, One Vote—One Vote, One Value*, 1964 SUP. CT. REV. 1, 22; *Developments in the Law—Equal Protection*, 82 HARV. L. REV. 1065, 1129 (1969).

31. See, e.g., *Harper v. Virginia Bd. of Elections*, 383 U.S. 663 (1966).

existence of a denial of the right to an election. Here, too, the right to an election, as contrasted with the right to an education, benefits from the Supreme Court's recent pronouncements on the "one man, one vote" rule and local government. The power-impact test used in *Avery* and *Hadley* could similarly be used to determine when officials of local general and special purpose units must be elected. As applied to the right to an election, this test would weigh the exercise of the officials' powers and their actual and potential impact in determining whether to make those officials directly accountable to their constituents.

In this connection, several considerations must be briefly noted. First, the power-impact test could be used for the right to an election even if the Court, with respect to apportionment, is moving towards exclusive reliance on the state's decision to have an election. Second, this test, because it has been used by the Court in only a few cases, provides uncertain guidance as to the factors important in asserting a right to an election. Uncertainty also results from the Court's inconsistent characterizations of similar powers in *Sailors* and *Avery*. It is true that *Hadley* answered in the affirmative the question whether, under a power-impact test, "one man, one vote" could be applied to a special purpose unit. Nevertheless, at this stage the relative importance of various powers, such as ad valorem taxation and annexation, is unclear in apportionment cases and also, therefore, for purposes of asserting a right to an election. Courts would have to make these determinations on a case by case basis.

Judicial recognition of a right to an election would, of course, further involve courts in the "political thicket," which Justice Frankfurter warned against. Still, a court following this approach would not tell a legislature or municipality what powers it must assign to local officials. This approach would only require an election where the powers assigned, under a power-impact test, warrant making local officials directly accountable to their constituents. Thus, a court that found a violation of the right to an election in the hypothetical scheme could give the legislature or municipality the opportunity to formulate the details for the election, which might include the number of officials to be elected, their qualifications, the timing of the first election, and the frequency of subsequent elections. Such a judicial procedure would be consistent with that used in the school desegregation decisions.

THE RIGHT AND THE NON-DELEGATION DOCTRINE

The judicial procedure to be used in protecting the right to an election raises the question of the differences in effect between the successful assertion of this right and the non-delegation doctrine. The non-delegation doctrine, while effectively dead in the federal system, is apparently very much alive in the states.³² This doctrine nominally prohibits a state from delegating so-called legislative power to non-representative persons or bodies who may act arbitrarily. The most common judicial approach to delegation involves an inquiry into the existence of legislative "standards" which limit the discretion exercised by the subordinate government.³³ Given standards, the assumption is that the delegated power is so circumscribed that when exercised, it is in effect the legislature which is acting. But where the court does not discover such standards, its response has been to declare the attempted delegation void. Consequently, until the standards are supplied by the legislature, the delegate has no authority to act in the particular standardless context.

In response to such a judicial decision, a legislature might provide standards under which the delegate body would act. Alternatively, it could provide that the officials constituting the delegate body are to be elected. Either response would probably save the delegate body from another successful non-delegation attack.³⁴ It is in this connection, however, that the non-delegation doctrine is distinguishable in its effect from a right to an election. If a legislature only provides standards, the delegation of legislative power would be upheld for non-delegation doctrine purposes, but, given a right to an election, might still be invalid in the absence of an election. Consequently, the right to an election may in some circumstances render invalid an otherwise valid delegation.

THE RIGHT AND ITS IMPLICATIONS

It is reasonable to assume that the officials of most general purpose local governmental units such as counties and municipalities are already selected by popular election. If the right to an election were limited to such general purpose units, its practical significance there-

32. See generally 1 K. DAVIS, ADMINISTRATIVE LAW TREATISE §§ 2.01-16 (1958).

33. *Id.*

34. See Herget *supra* note 21, at 772-73.

Appointive Local Government Bodies

fore would in all likelihood be minimal. The Court's decision in *Hadley*, however, undercuts such a limitation, for it suggests a substantial interest in the accountability of even special purpose unit officials to voters who will be affected by the exercise of their powers. In view of the presumably numerous and diverse situations where officials of special purpose units are not elected,³⁵ the effect on these situations of recognizing a right to an election which uses a power-impact test could be considerable.

For example,³⁶ the right to an election could be applied to metropolitan special purpose units that are the products of collaboration among different governmental units. *Hadley* applied "one man, one vote" to a consolidated junior college district which voters, notwithstanding malapportionment, had decided by referendum to form. Similarly, governmental units desiring to cooperate in the creation of a metropolitan special purpose unit could be required to provide an election if the special purpose unit were given powers with wide-ranging impact on its constituents. Under this view, an election could have been mandated in *Hadley*, for example, had there been no election provided for the trustees.

More generally, the right to an election might be applicable to metropolitan councils of government.³⁷ In a discussion of these COG's,³⁸ it was suggested that "a metropolitan planning body, in order to be able to deal with problems of efficiency, equity, and responsibility, must exist more or less independently of existing units,"³⁹ and that "[d]irect elections of metro organization members would, of course, be the most powerful means of altering this perception [of a consensus courtesy practice]." ⁴⁰ Yet concern was expressed that people would probably not take such elections seriously. It is true that the writers' solution—that

35. See generally Herget, *supra* note 21, at 767, 786-88 (Port of New York Authority, Chicago Transit Authority, Association of Bay Area Governments, etc.).

36. The applicability of a right to an election to the following examples might be better based on the due process clause than on the equal protection clause because, unlike the hypothetical school board, there might be no state classification involved. See notes 21-31 *supra*. In that event, perhaps the power-impact test used in the reapportionment cases might be incorporated for use through the due process clause.

37. See generally Comus, *The Council of Governments Approach to Governmental Fragmentation*, 22 VAND. L. REV. 811 (1969).

38. MICHELMAN & SANDALOW, *supra* note 1, at 815-23. Another commentator refers to regional governments of this sort as posing "one of the most perplexing problems to be faced under one-man, one-vote." He suggests that the real question is whether the delegates represent the people in their local units or the local unit as a political organization. Herget, *supra* note 21, at 789.

39. MICHELMAN & SANDALOW, *supra* note 1, at 820.

40. *Id.* at 821.

state legislators elected from districts within the metropolitan area would be the "legislative core of the council, and act as its governing body"⁴¹—might comply with the requirement of an election for purposes of this discussion. Nevertheless, the solution rests on two assumptions which might appear questionable. The first is that state legislators could competently carry out both their state and metro functions at one and the same time. And second, that voters would not take a direct election seriously. As the Supreme Court said in *Hadley*, "what might be a vital election to one voter might well be a routine one to another."⁴² For these reasons, the better approach might be direct elections.

An election requirement in these circumstances might answer the objections of another commentator⁴³ who argues against the application of "one man, one vote" to metropolitan government. He suggests that municipalities, faced with reapportionment if they form a metropolitan government, will respond by "the old devices of appointed, administratively-oriented, special districts and authorities."⁴⁴ These, he contends, are not at all effective for regional governance. However, because municipal officials could not, merely by appointing, evade an election requirement imposed on such special purpose units under a power-impact test, this might provide a stimulus toward the politically accountable metropolitan government that the commentator favors.⁴⁵

It might be argued that public control through voting of most special purpose units is to be avoided. First, public control would make it less likely that a unit's projects will be desirable and efficient for financial purposes. Second, the most capable people would not want to run for elections. These arguments, however, are misconceived in connection with the right to an election. Accountability through elections does not in itself make a unit's projects inefficient and unattractive from an investor's viewpoint. Rather, accountability helps to guard

41. *Id.*

42. 397 U.S. at 55. Compare, however, *Abate v. Mundt*, 403 U.S. 182 (1971), in which the Court upheld an apportionment scheme for a county legislature which departed by over ten percent from mathematical equality. The Court stressed that the scheme, whereby county supervisors held their offices by virtue of their election or town supervisors, reflected over a century of county and town coordination.

43. Jones, *Metropolitan Detente: Is It Politically and Constitutionally Possible*, 36 GEO. WASH. L. REV. 741 (1968).

44. *Id.* at 743.

45. The right to an election could conceivably apply to federally funded municipal housing authorities which, while without the power to tax, may have the power to condemn and to issue bonds. See, e.g., *City of Paterson v. Housing Authority*, 96 N.J. Super. 394, 233 A.2d 98 (Super. Ct. 1967).

against projects which, while efficient and financially attractive, are not responsive to the needs of the public. It is true that the quality of special purpose unit officials, whether appointed or elected, often depends upon the character and power of the predominant political parties. Still, an election requirement might open up the possibility of higher quality persons running as independents. It could also improve the quality of those chosen by the established political parties to run for election.⁴⁶

CONCLUSION

This tentative inquiry into the right to an election was prompted in part by the reapportionment cases which reflect the special attention given to the citizen's voice in selecting those governmental officials whose decisions will affect him. This attention led to consideration of the possible constitutional basis and judicial standards of such a right, as well as some discussion of its implications. Admittedly, the costs of further judicial intervention in this highly sensitive political area might be excessive and even prohibitive.⁴⁷ Furthermore, it is difficult to assess satisfactorily the practical significance of such a right. One might guess that the existence of a right to an election would, because of political considerations, often discourage state and local governments from creating special purpose units with adequate powers to meet special needs. If this meant that these needs would go unattended, then the effect of a right to an election would be undesirable. On the other hand, it is also possible that special needs would generate enough public pressure to force the state or municipality to take some kind of action, despite political considerations. This could result either in new metropolitan or regional governments or special purpose units which would then be politically accountable to the citizens affected by their decisions.

46. This might be overly optimistic. Also, it must be admitted that voters may become confused by the multiplicity of elections and hence not vote knowledgeably.

47. Such costs might include judicial imposition of a particular political theory upon local governments, undercutting flexibility and attempts to experiment, a reluctance on the part of local governments to comply with a decision to have an election and consequent evasions, and finally the need to proceed on a case by case basis with the resultant uncertainty.

Similar arguments have been made against the applicability of "one-man, one-vote" to local government generally, but the Court in *Avery* apparently discounted such fears. On the other hand, in both *Avery* and *Hadley* the Court emphasized what is apparently a need for flexibility in applying "one-man, one-vote" to local government. This emphasis is reflected in *Abate v. Mundt*, 403 U.S. 182 (1971), where the Court upheld a county's apportionment scheme, given its particular history and effect, despite a population variance in excess of ten percent.

While the latter is by no means inevitable, it is the preferable result. If an individual's constitutional right to an election were successfully asserted,⁴⁸ it could perhaps serve no better purpose.

48. *But see* Wallis v. Blue, 263 F. Supp. 965 (N.D. Ga. 1967), where an argument was unsuccessfully made against a state constitutional provision for grand jury selection of county school board officials who had the power to levy a county-wide property tax. The court stated: "There is no federal requirement intended to compel a state to elect any of its officers or agents through popular vote of the people." *Id.* at 969. *See also* Latham v. Board of Educ., 31 Ill. 2d 178, 201 N.E.2d 111 (1964), holding, *inter alia*, that Chicago residents had no inherent right to elect members of the Chicago Board of Education. However, the appointed Chicago Board of Education does not possess the power to levy and collect taxes. Furthermore, the decision on this point appears to be based on the state constitution.

To Elect or Appoint? Evidence from Local Election Administration.*

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Abstract

Do elected or appointed officials produce better outcomes for their constituents? Elections should improve representation by providing a direct link to voters. However, some argue that citizens may have too little information to select good leaders and hold them accountable, especially at the local level. In order to assess these conflicting claims, I examine the performance of local election officials, an office that has come under immense strain to deliver democratic elections and for which selection method is a live policy debate. Using an original collection of election administration structures in 1,116 counties across 13 states and over 62 years, I leverage changes in selection method over time to credibly measure differences in the election outcomes produced by local election officials based on whether they were elected or appointed. I find that appointed officials out-perform their elected counterparts, increasing voter turnout by one to two percentage points and raising registration rates by one percentage point. Appointed officials appear to boost election administration resources, more actively communicate with voters, and reduce voter wait times. I present evidence that the quality of selection and sanctioning are higher for appointed officials, leading to better educated and more closely monitored agents. My findings speak to the challenges in designing local institutions that advance and protect democratic ideals.

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1 Introduction

One of the challenging aspects of designing democracies is deciding which public officials to directly elect and which to appoint. America’s founders ratified a Constitution that relied almost exclusively on appointments. James Madison justified the indirect selection of the president via the Electoral College by reasoning that “A small number of persons, selected by their fellow-citizens from the general mass, will be most likely to possess the information and discernment requisite to such complicated investigations” (Madison 1788). In the 19th century, Jacksonian reformers expanded the practice of directly electing public officials to include senators, judges, state executives, and a multitude of county and municipal offices, whereas by the early 20th century, Progressive reformers sought to return many of these positions to appointments.

Scholars disagree about whether elections or appointments produce better outcomes for constituents. Elections should improve representation by providing a direct link between voters and their agents (Besley 2006; Ferraz and Finan 2011). However, the mass public may not have sufficient information compared to political elites, leading to the selection of less qualified officials and weaker accountability once in office (Gailmard and Jenkins 2009). Elections’ agency problems are likely strongest in local politics, where expertise is hardest to find and the public is least aware of their agents’ activities (Whalley 2013).

In this paper, I assess conflicting claims over selection method by studying the consequences of appointing vs. electing the people responsible for running elections. Unlike any other Western democracy, the U.S. delegates election administration duties to over 8,000 local officials who handle the minutiae of elections: registering voters, hiring poll workers, locating polling places, mailing ballots, tallying votes, and certifying results (Hale, Montjoy, and Brown 2015). These administrators have endured intense scrutiny in recent years, especially when former President Trump alleged the 2020 presidential election was stolen. Some officials were pressured to refuse to certify the election results,¹ and many have re-

¹<https://www.nytimes.com/2020/11/17/us/politics/michigan-certify-election-results.html>

ceived threats of violence over baseless accusations of malfeasance.² Some are elected and some are appointed (Kimball, Kropf, and Battles 2006)—a balance that is tipping more heavily towards appointments in recent decades while also becoming increasingly contested politically (Ferrer and Geyn 2023). Harris County, the third most populous county in the country, was forced by the Texas state legislature to switch its chief election official from an appointed to an elected position in 2023.³ Georgia’s state government considered taking over the administration of its most populous county after the legislature passed legislation in 2021 empowering it to do so.⁴ And Miami-Dade is being forced to return to an elected election supervisor after the approval of a potentially misleading referendum in 2018.⁵

Over 300 jurisdictions—nearly 1 out of 4 counties across 13 U.S. states that comprise 40% of the country’s population—have switched from electing to appointing their chief local election official since 1960. I leverage an exhaustive original collection of clerk selection methods spanning 1,116 counties and 28 federal elections to provide the strongest evidence to date for whether elected or appointed officials produce better outcomes for their constituents.⁶ Within-jurisdiction variation in selection method over time allows me to identify a precise effect on differences in election outcomes.

I use measures of voter participation such as turnout and registration rates as my primary outcome. Voter participation is one of the few reliable measures of election quality available over a large span of time. It is also an important one, frequently used in election quality indices such as the MIT Election Performance Index and the Varieties of Democracy Project. More than two-thirds of election officials consider increasing participation a central component of their job,⁷ as does their chief professional organization, the National Associa-

²<https://www.nytimes.com/2022/09/06/us/politics/midterms-elections-threats-security.html>

³<https://www.nytimes.com/2023/05/28/us/texas-voting-laws-harris-county.html>

⁴<https://georgiarecorder.com/2021/08/18/panel-begins-review-of-fulton-elections-ahead-of-potential-state-takeover/>

⁵<https://www.miamiherald.com/news/politics-government/election/article215034905.html>

⁶I occasionally refer to local election officials as clerks in shorthand. While clerks are the most common county election officials, there is wide variation in the position title across states and counties.

⁷https://evic.reed.edu/wp-content/uploads/2021/04/leo2020_codebook.pdf

tion of Election Officials.⁸ Finally, local election officials likely have the ability to influence participation rates given their far-ranging duties and discretion over administrative decisions (Burden et al. 2013; Kimball and Kropf 2006). Election administrator decisions over communication strategies (Merivaki and Suttman-Lea 2023), election expenditures (Grose 2022), and polling places (Yoder 2018) have all been shown to affect participation, as well as their indirect ability to shape voter wait times (Pettigrew 2017).

I find that when counties switch from electing to appointing their local election official, voter turnout in presidential elections increases by between 1 and 2 percentage points and registration rates increase by 1 percentage point. These findings are robust to a variety of different estimators; hold across multiple states, offices, years, and reform mechanisms; are largest in less populous jurisdictions; and do not differ by jurisdiction partisanship or come at the expense of increased partisan manipulation of elections. They are also substantively significant. A 2 percentage point boost to voter turnout in federal elections is equivalent to or larger than the effect of universal vote-by-mail (Thompson et al. 2020), automatic voter registration (McGhee, Hill, and Romero 2021), 10 additional days of early voting (Kaplan and Yuan 2020), or a door-to-door canvassing campaign (Green, McGrath, and Aronow 2013). I find suggestive evidence that part of the effect may be due to greater resource provision, contributing to additional staff, more active communication with constituents, and shorter wait times at the polls.

Through a series of mechanism tests, I show that the quality of selection and sanctioning is higher for appointed clerks than elected clerks. Appointed officials are more likely to hold a college degree and elected administrators rarely face competition at the polls. I identify three factors likely contributing to better sanctioning of appointed administrators: voters know little about their local election official, the differences between elected and appointed clerks are largest in areas lacking a local newspaper, and appointed officials have higher turnover rates.

⁸<https://www.electioncenter.org/about-us.php>

My findings speak to the challenges in designing local institutions that advance and protect democratic ideals. In the midst of unprecedented threats to that democracy, declining trust in elections (Stewart 2021), and partisan moves to shape election administration (Ferrer and Geyn 2023), this paper also informs ongoing debates over who should run elections in the U.S.

2 Selecting Public Officials

The United States is exceptional in the number of public officials we elect. By one count, there are approximately 520,000 elected officials in the country, with 96% of these politicians elected at the local level (Lawless 2012). I consider why we might expect appointing officials to be preferable to electing officials and the findings of prior scholarship on selection method.

2.1 Why Might Appointed Officials Produce Better Outcomes for Their Constituents?

According to political economy theories of governance, elections improve representation by allowing voters to select higher-quality politicians and ensuring their accountability to the electorate through the sanctioning mechanism of reelection (Besley 2006; Besley and Case 2003; Besley and Coate 2003; Fearon 1999). In some empirical contexts, it appears that elections do achieve these goals, producing officials who are more competent than the constituents they represent (Dal Bó et al. 2017), who work harder when they have the incentive of being reelected (Alt, Bueno de Mesquita, and Rose 2011; Christensen and Ejdebyr 2018; Ferraz and Finan 2011; Fourniaies and Hall 2022), and who better represent voters (Besley and Coate 2003). In other contexts, however, elections may have unintended consequences, lowering the quality of the pool of candidates, creating weak accountability mechanisms, and producing adverse incentives (Sances 2016; Whalley 2013).

First, elections alter the pool of candidates by selecting for those willing to run for office (Anzia and Berry 2011; Hall 2019). The skills that make someone a good politician may not align closely with the factors that make someone a good public official. If this is the case, then the election process itself may select out higher-quality candidates, simply due to the barriers to entry. Elected candidates typically must live within the jurisdiction they are elected to, whereas appointed administrators can be chosen from a broader geographic pool. Additionally, technological advancements and population growth have led many local public duties to require greater expertise, including election administration (Hale, Montjoy, and Brown 2015). Local elections are rarely contested (Burden and Snyder 2021; Lappie and Marschall 2018; Marschall and Lappie 2018). In the 2020 general election, 78% of all county-level races went uncontested,⁹ and half of all elections for partisan office went uncontested in 2022.¹⁰ Whereas long tenures and few challengers could be a sign of voter contentment with the officeholder, it could alternatively mean a breakdown of the accountability mechanism that is essential to ensuring good performance (Besley 2006). If only one candidate is willing to run, this severely limits the ability of voters to select the highest quality candidate and punish them once in office.

Second, low-information and low-salience environments can prevent voters from using elections to effectively monitor officials and sanction them for poor performance (Ashworth and Bueno de Mesquita 2008; Berry and Howell 2007; Besley 2006; Lim and Snyder 2010; Rogers 2023). In theory, elections should provide voters with a more direct accountability mechanism than appointments (Burden et al. 2013). In the absence of sufficient information, however, voters may be unable to select good candidates in the first place, distinguish between highly and poorly performing election officials, or select on quality rather than ideology or other characteristics (Franchino and Zucchini 2015). Local media has been on the

⁹<https://organizations.ballotready.org/research/nothing-to-lose-uncontested-races-in-2020-and-their-implications>

¹⁰<https://www.nytimes.com/2024/09/04/us/missouri-uncontested-races-elections.html?smid=nytcore-android-share>. See also <https://www.civicpulse.org/post/how-many-local-elections-are-uncontested>.

decline over the past few decades (Martin and McCrain 2019) and has increasingly devoted less attention to local politics (Lockhart 2021). This has led to less informed citizens and less competitive local races (Rubado and Jennings 2020). The large number of elected positions may cause voter fatigue and high ballot roll-off, with not many voters making it all the way to the bottom of the ballot where local offices are typically found (Augenblick and Nicholson 2015). Voters might be especially poor judges of performance in complex and technical policy areas (Whalley 2013). The voter information gap for election officials is particularly acute, as election officials have a portfolio of non-election responsibilities and unintuitive titles that dilute the ability of voters to effectively monitor and sanction their performance.¹¹ If public officials are acting rationally, we should expect them to shirk their duties in these circumstances, since their principals (the voters) lack the information necessary to effectively monitor and sanction them. The information-poor environment voters face contrasts with the richer information environment that appointed officials' principals possess. Appointments for election administrators are typically made by boards of local elites and can include county officers, local party chairs, judges, and county supervisors. Many of these officers are elected and thus have a high degree of familiarity with the election process.

Finally, elections can create adverse incentives for officeholders to make politically motivated decisions that are normatively undesirable (Canes-Wrone, Herron, and Shotts 2001). Electing rather than appointing assessors in New York exacerbates economic inequalities (Sances 2016), electing rather than appointing city managers skews economic policies towards the wealthy (Lubell, Feiock, and De La Cruz 2009), and electing rather than appointing municipal assessors in California leads to the adoption of more costly policies and higher borrowing costs (Whalley 2013). If a majority of the voting electorate prefers political outcomes achieved by reducing participation, then elected officials could be incentivized to concentrate costs on certain voters or discourage voting across the board. Likewise, elected officials have won office with the present electorate, so they might be disinclined to pursue

¹¹Examples include probate judge in Alabama and Georgia; auditor in Iowa, South Dakota and Washington; and tax assessor in Texas.

actions to expand the electorate. Appointments do not remove this possibility, but they may counterintuitively insulate officials from the pressures of responsiveness in ways that lead to socially desirable outcomes.

2.2 Prior Scholarship on Selection Method

A number of studies have examined the differences between elected and appointed public officials in federal, state, and local contexts. Elected officials tend to be more responsive to their constituents (Gailmard and Jenkins 2009), but participation gaps could cause responsiveness to skew policy outcomes in ways that benefit the wealthy and whites (Hajnal and Trounstein 2014; Lubell, Feiock, and De La Cruz 2009; Sances 2016) and lead to more punitive judicial outcomes (Gordon and Huber 2007; Huber and Gordon 2004). Additionally, appointing local officials has been found to improve policy outcomes in some cases. In a study of California treasurers, Whalley (2013) finds that municipalities that switched from elected to appointed treasurers enjoyed lower borrowing costs. He concludes that voters may be poorly equipped to judge performance, especially in complex policy areas.

A cross-sectional study of Wisconsin election officials found that elected clerks produce higher turnout, although it relied on the assumption that elected and appointed clerks were assigned as-if randomly in the state (Burden et al. 2013). The authors theorized that appointed officials are more insulated from public opinion than elected clerks, and thus pursue their own personal goals or the goals of the county officials who appoint them rather than the goals of the public. Since voters prefer that clerks make voting convenient whereas the appointing officials prefer minimizing costs, appointed clerks should oversee elections with lower turnout.

3 Data and Methods

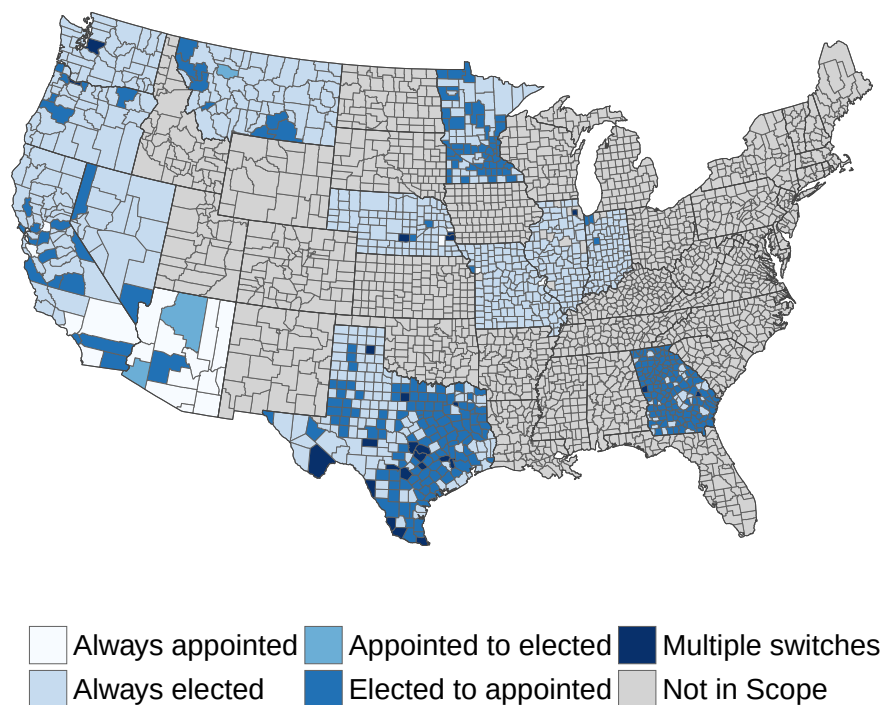
3.1 Measuring the Selection Method of Local Election Officials

I construct original panel data on the selection method of local election officials in 13 states from 1960 to 2022. In total, my dataset covers 62 years of election administration structures for 1,116 counties, encompassing over 30,000 county-federal election observations.

My sample consists of every state in the United States with at least one county-level change between appointing and electing clerks since 1960. These 13 states are a subset of the 42 states in the country where elections are primarily administered at the county level (Ferrer and Geyn 2023) and combined cover over 40% of the nation’s population. The states included are Arizona, California, Georgia, Illinois, Indiana, Minnesota, Missouri, Montana, Nebraska, Nevada, Oregon, Texas, and Washington.¹² Figure A.1 in the online appendix shows the selection method of election officials in all county-administered jurisdictions across the United States. Table A.1 shows that counties in the dataset are similar to those that are excluded. Figure 1 shows which counties enter into the dataset as well as whether they are always appointed, always elected, switch from elections to appointments, switch from appointments to elections, or have undergone multiple changes in selection method. The vast majority of counties that have switched since 1960 have moved from electing to appointing their clerks. In fact, 99.1% of counties switching their selection method have adopted appointments, and 93% of all singular switches have been in the direction of appointments. Four states in particular stand out for the number of switches: California, Georgia, Minnesota, and Texas. Figure 2 shows when each switch in selection method occurred. Counties have changed their clerk selection method in a staggered fashion over many decades, with switches accelerating since 2000. Table A.1.3 in the online appendix details the specific election authority used

¹²In states with multiple election authorities, I use the selection method for the authority with primary responsibility for administering elections on Election Day, as defined by Ferrer and Geyn (2023). I exclude five counties in Illinois and one in Missouri with nested municipal-level election administration.

Figure 1: **Local Election Administration Selection Methods, 1960-2022.** This graph displays over time change in the selection method of county election officials across all states with county-level administration where at least one change has occurred since 1960.

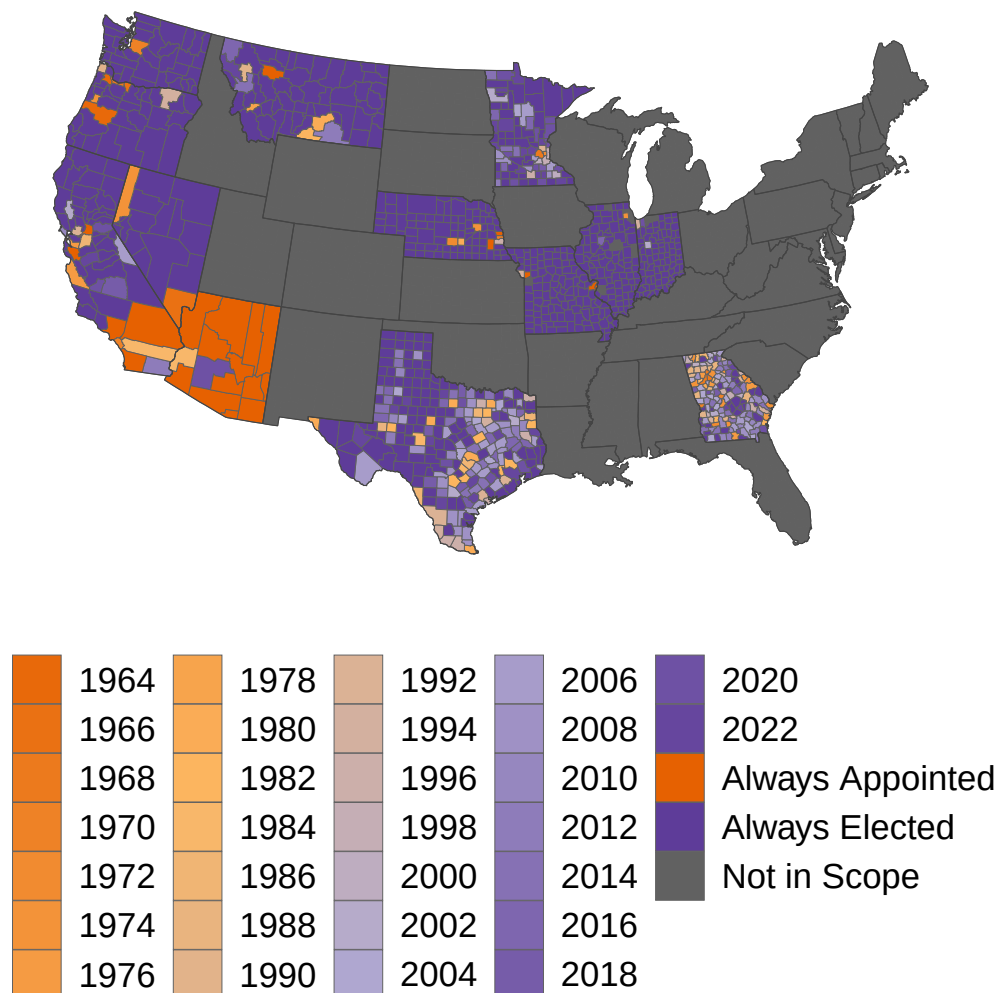


for each state, as was well as the number of counties falling into each clerk selection method category and the first and last year a change occurred.

Finally, Figure 3 graphs the extraordinary shift in selection method over time across these 13 states. The percentage of counties that appoint their election officials has grown from 2% in 1960 to 33% in 2022. The balance continues to shift towards appointments, with the trend accelerating over the past two decades. Election official selection method is a live, ongoing, and at times contentious policy debate.

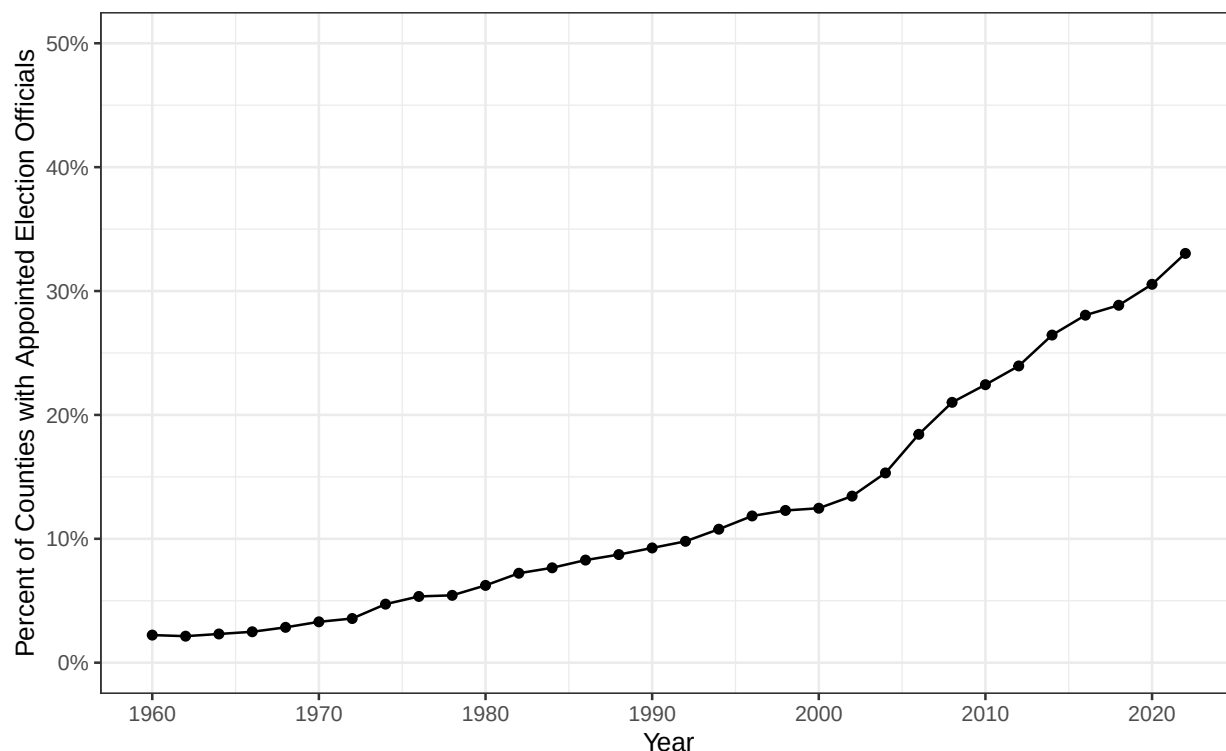
The mechanism and character of the changes vary widely across states. Most or all of the changes in California, Oregon, and Washington are due to the implementation of home rule charters that tended to make wholesale changes to local governance. Minnesota, Montana, and Texas devolve the power to switch selection methods to their counties, whereas California and Georgia typically require the passage of state legislation to enable a change.

Figure 2: **Local Election Administration Selection Cohorts, 1960-2022.** This graph displays the year county-level switches occurred between electing and appointing local election officials since 1960. In most cases, this switch is from electing to appointing the local election official. In counties where multiple switches occurred, the year of the first switch is reflected.



Some counties in California, Minnesota, Montana, and Washington hold binding referendums to initiate the reform, and several Midwestern states have population thresholds at which appointing their election official becomes possible or mandatory. In most cases, the switch

Figure 3: **Growth in Appointed Election Officials Across 13 States, 1960-2022.** This graph displays over time change in the selection method of county election officials across the 13 states with county-level administration where at least one change has occurred since 1960. In total, these states have 1,123 counties.



in selection method was not accompanied by any other substantive change to election policy or resource provision. For instance, in Georgia a state legislator that represents the affected county introduces a law to the legislature transferring election administration authority from the elected probate judge to an appointed board of elections that then selects an elections director. In Texas, the county commissioners enact the transfer of authority from an elected clerk or tax assessor to an appointed elections administrator. Minnesota presents a particularly minimal case of change, with county officials agreeing to a switch from election to appointment of the county auditor. In some cases, this does not even result in a change in leadership. I conduct a series of robustness tests isolating the effects of reform independent of other substantive policy changes.

Reasons counties state for making the switch include difficulty finding qualified candidates for office,¹³ a desire to professionalize the job,¹⁴ increasing efficiency and streamlining services,¹⁵ creating a dedicated position for election administration,¹⁶ or simply following in the footsteps of other counties in the state.¹⁷ Who receives appointing authority also varies, as well as whether they appoint an individual or a board (Ferrer and Geyn 2023).

Local election officials are entrusted with broad statutory authority to conduct elections (Ferrer, Geyn, and Thompson 2023). For instance, probate judges in Georgia determine precinct divisions, handle nomination petitions of candidates, publish notices and advertisements of elections, select and equip polling places, purchase and maintain election equipment, conduct early in-person voting, appoint and train poll officers, inspect the conduct of elections, receive and certify election results, prepare a budget estimate and appropriations request, conduct hearings to determine the eligibility of candidates, and administer photo ID provisions. Most clerks also handle registration administration and voter list maintenance duties, although these responsibilities are divided in Arizona, Georgia, and parts of Texas.

I use a combination of sources in order to identify the selection method of election officials across the dataset, including state legislative databases, home rule charters, newspaper archives, web scraped internet archives, Blue Book directories, public records requests, and correspondence with state and local election officials.

3.2 Data

I use presidential and midterm participation rates as my primary outcome measure. I focus on turnout and registration rates for four reasons: local election officials have the ability

¹³<https://www.fairmontsentinel.com/news/local-news/2023/07/19/faribault-county-looks-to-appoint-auditor-treasurer/>

¹⁴<https://www.houstonchronicle.com/opinion/editorials/article/harris-county-elections-legislation-hudspeth-18552129.php>

¹⁵<https://maplelakemessenger.com/2020/12/wright-county-considers-changing-auditor-treasurer-from-elected-to-appointed/>

¹⁶<https://www.timesrecordnews.com/story/news/local/2023/06/05/wichita-county-to-hire-election-administrator/70289429007/>

¹⁷https://www.unionrecorder.com/news/commissioners-discuss-possibly-creating-a-board-of-elections/article_43508cfc-6718-11ee-a035-13c8d8908b19.html

to influence participation levels, they view increasing participation as part of the job, voter participation is a key component of election quality metrics and the ultimate outcome of election quality, and I have access to high-quality data on participation rates. First, election officials typically have far-ranging duties and a significant degree of discretion in carrying out these duties (Kimball and Kropf 2006). Some studies have found that clerks of different parties influence turnout rates (Bassi, Morton, and Trounstone 2009; Burden et al. 2013; but see Ferrer, Geyn, and Thompson 2023). Second, according to the 2020 EVIC Survey of Local Election Officials, over 67% of local election officials agree that encouraging voter turnout is part of their job, compared with fewer than 10% who disagree. This is reflected in the National Association of Election Officials, which lists increasing participation as one of the main considerations for election officials.¹⁸ Third, participation rates are widely viewed as a key measure of election quality. MIT’s Election Performance Index uses both voter turnout and voter registration in comparing election administration performance across states,¹⁹ and Varieties of Democracy (V-Dem) uses electoral participation as one of its indicators of democratic health.²⁰ Voter turnout can be considered the ultimate effect of the quality of election administration. If voters have a poor voting experience, or are not readily or proactively provisioned with the information necessary to vote, then they are less likely to participate. Finally, high-quality data for turnout exists at the county level and is available going back many decades. This is not true of any other indicator of election quality, including voter confidence, voter wait times, number of polling places, and constituent communication.

Data on county-level vote totals is from Congressional Quarterly and David Leip’s U.S. Election Atlas and spans 1968 to 2022.²¹ I use data on registration totals from Leip’s Election Atlas. This covers presidential elections from 1996 and gubernatorial elections from 2004. I measure voting age population using estimates from the National Cancer Institute’s

¹⁸<https://www.electioncenter.org/about-us.php>

¹⁹<https://elections.mit.edu/#/data/map>

²⁰<https://www.v-dem.net/static/website/img/refs/codebookv12.pdf>

²¹I exclude Loving county, Texas from the analysis because its population is too small to reliably estimate participation rates.

Surveillance, Epidemiology, and End Results Program.²² I measure registration rate by dividing total registrants by the voting age population.

I assemble a set of county-level indicators of election administration policy using the U.S. Election Assistance Commission’s Election Administration and Voting Surveys (EAVS) from 2004 to 2022, including the number of polling places per 1,000 people, provisional ballot rate, provisional ballot rejection rate, absentee ballot rejection rate, and the registration removal rate. Following Ferrer, Geyn, and Thompson (2023) and Pettigrew (2017), I use data from the Congressional Election Study to measure the share of voters who had to wait at the polls for certain lengths of time. This is available for general elections in 2006, 2008, and 2012–2018. I also use election official communication data provided by Thessalia Merivaki and Mara Suttman-Lea.

I probe mechanisms using data on election administration expenditures from Mohr et al. (2018), data on the prior experience of local election officials from the 2020 EVIC Survey of Local Election Officials,²³ data on local newspapers from Gentzkow, Shapiro, and Sinkinson (2014) and Sean Ewing, and data on voter knowledge of election officials from an original survey.

3.3 Design

It is difficult to estimate the effect of local election administrator selection because counties that appoint officials likely differ from those that elect officials for a host of reasons beyond the selection method of the election official and in ways that are likely to affect participation rates. Table A.3 in the online appendix shows some of the differences between counties that appoint their clerk vs. those that elect their clerk. For instance, populous, dense, and racially diverse counties are all more likely to appoint their election officials than sparsely

²²This data includes some voting-age residents who may be ineligible to vote due to citizenship status or criminal record. While this may make some estimates noisier, it is unlikely to introduce bias since few people decide where to live based solely on the selection method of a county’s local election official. The data I use is available at <https://seer.cancer.gov/popdata/>. It is available from 1970 to 2020. I extrapolate the estimates to 1968 and to 2022.

²³<https://evic.reed.edu/leo-survey-summary/>

populated, rural, and mostly white counties (Ferrer and Geyn 2023). They also tend to have lower participation rates (Leighley and Nagler 2017). Similarly, counties in Western states tend to elect their officials and also tend to have higher turnout rates than counties in other regions (Springer 2014). Given these correlations, a simple cross-sectional analysis of counties would result in a relationship between appointed officials and lower turnout—but this would not be evidence that appointing officials *causes* lower turnout. Even if all of these obvious differences are controlled for, there are likely unobservable factors that make counties different in ways that happen to correlate both with their participation rate and the selection method of their clerk.

I overcome this issue with a difference-in-differences research design. I leverage county-level changes in clerk method across 13 states to credibly measure the effects of a switch on participation. The design compares the change in turnout when a county switches from electing to appointing its election official to the change in turnout in other counties in the same state that continue electing clerks. So long as year-to-year differences in turnout are commonly experienced across a state and not indirectly related to switches in clerk selection method, I can be confident that an observed difference in turnout in the counties that switch to appointed clerks is due to the selection method itself.

I estimate the regression $Y_{it} = \alpha_i + \delta_t + \beta \text{Appointed}_{it} + \epsilon_{it}$, where Y_{it} is a measure of voter turnout or registration in county i at election year t , α_i and δ_t are county and year fixed effects, respectively, and Appointed_{it} is a dummy variable taking 1 when counties appoint their local election official and 0 when counties elect their local election official. β is the causal effect of an appointed election official on voter turnout.

The causal interpretation of the difference-in-differences design rests on the parallel trends assumption. This means that counties that switch to appointed clerks are on similar turnout trajectories to those that do not switch, prior to the reform. It is possible to imagine that counties that switch to appointed officials are growing at a more rapid rate than those that stay with elected officials, and that turnout is trending down as a result. In this case,

appointed officials might be viewed as a way to professionalize the county’s election administration. Similarly, selection method might become a partisan issue. If more Democratic counties start to adopt appointed clerks, and Democrats reduce or increase their turnout relative to Republicans, then this would also result in the appearance of a causal relationship between appointments and turnout that was spurious. Table A.4 reveals differences in population, participation rates, and demographics between counties that switch to appointed officials and counties that stay with elected officials.

All regressions include at the minimum Year by State fixed effects. This ensures that comparisons are only made between counties in the same state, addressing the possibility that states may be on different turnout trajectories. I further address parallel trending concerns by incorporating two additional sets of interacted fixed effects: Year by State by Democratic vote share and Year by State by Population fixed effects. The former compares within-county over time change to other counties with similar partisan makeup, whereas the latter compares within-county overtime change to other counties with similar populations. These account for the possibility that counties that switch their election administration may also happen to shift either population or partisan trends in ways that are systematically related to turnout. Democratic vote share and population are divided into quartiles for each state, allowing the grouping cut points to vary by state, and measured pretreatment.²⁴

Even with these interacted fixed effects, it is still possible there are unobserved confounders. I conduct a generalized synthetic control balancing exercise to ensure that counties that switch are only compared to those that do not with similar pretreatment turnout trajectories and randomization inference to investigate the likelihood of getting the observed results given the structure of the data.

²⁴I use the 1960 census for population and the 1968 presidential election for Democratic vote share. Democratic vote share is measured as votes for the top-ticket Democratic candidate divided by votes for the top-ticket Democratic and Republican candidates.

4 Results

In this section, I present evidence that appointing clerks results in increased turnout and registration rates. I then validate this finding with a range of alternative estimators, conduct a placebo analysis using registration rates, distinguish between the effects of selection method and partisanship, show the effects are largest in less populous jurisdictions, and examine whether appointed officials benefit their principals’ party or if the effect differs by jurisdiction partisanship.

4.1 Appointing Election Officials Increases Voter Participation

Table 1 displays the results of a two-way fixed effects regression estimating the effects of appointing a local election official on citizen participation. Columns 1 through 3 estimate the effects on votes per voting-age resident and columns 4 through 6 estimate the effects on registrants per voting-age resident. Both are measured as proportions out of 1. The coefficients are the average percentage point difference in turnout and registration rates when counties switch from elected to appointed clerks. All six regressions include, at minimum, county and year by state fixed effects.

Column 1 shows that counties switching from directly elected to appointed election officials see an average increase in voter turnout of 1.8 percentage points in even-year general elections, compared with counties that do not switch. The point estimate is precisely estimated, allowing us to confidently rule out effects smaller than 1.2 percentage points. It is also substantively meaningful. The effect size for turnout in even-year general elections is on par or larger than those generated by the most significant modern policy interventions designed to boost voter participation. It is equivalent to implementing universal vote-by-mail (Thompson et al. 2020) or adding 10 days of early voting (Kaplan and Yuan 2020). It is also double the turnout boost caused by implementing automatic voter registration (McGhee, Hill, and Romero 2021). This effect size is also significant compared to get-out-the-vote

Table 1: **Appointing Local Election Officials Increases Citizen Participation (Even-Year General Elections, 1968-2022)**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.018 (0.003)	0.016 (0.003)	0.014 (0.003)	0.009 (0.004)	0.009 (0.004)	0.008 (0.004)
Counties	1116	1116	1116	942	942	942
Elections	28	28	28	13	13	13
Observations	31146	31146	31146	12216	12216	12216
Outcome Mean	0.50	0.50	0.50	0.84	0.84	0.84
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	Yes	No	No
Year x State x Dem vs FEs	No	Yes	No	No	Yes	No
Year x State x Pop FEs	No	No	Yes	No	No	Yes

Robust standard errors clustered by county in parentheses. Voter turnout and registration rate are measured as proportions out of 1. The number of observations is smaller in columns 4-6 because Arizona and Georgia are excluded and because turnout data is available from 1968 but registration data is only available from 1996.

interventions. It is twice the average turnout effect of door-to-door canvassing, three times that of a direct mailing, and five times that of a phone call campaign (Green, McGrath, and Aronow 2013).

I introduce year by state by Democratic vote share fixed effects to alleviate this concern in column 2 to alleviate the concern that counties with similar partisan compositions were on the same participation trajectory prior to their shift in selection method. The result is similar under this estimation strategy. The inclusion of year by state by population fixed effects in column 3 makes comparisons between counties of comparable sizes within the same state and yields analogous results.

Appointed election administrators also appear to oversee elections with higher registration rates. Arizona and Georgia are excluded from these specifications because registration duties are always undertaken by appointed registration boards. The coefficients range from 0.8 to 1 percentage point in magnitude, and a null of no difference can be confidently ruled out in all three estimators. An event study plot of the effect of appointment on registration,

shown in Section A.3.3, reveals some evidence of pre-trending, so this effect should be viewed with caution.

These estimates provide strong evidence that appointed clerks increase voter participation, relative to their directly elected counterparts. Regressions excluding midterm contests are found in Section A.2.1 and yield substantively similar findings. Table A.6 in the online appendix shows the results are also robust to the use of different criteria in constructing the panel data of election official selection methods.

The results hold in multiple states, across multiple offices, for multiple reform mechanisms, and over multiple years and date ranges. In Table A.7 in the online appendix, I show that switching to an appointed election administrator increases voter turnout in three of the four states with at least 10 counties experiencing switches (Georgia, Minnesota, and Texas), and is imprecisely estimated in the fourth case (California). Table A.8 in the online appendix shows that switching from elected probate judges, auditors, and clerks to appointments increases voter turnout. In Table A.9 in the online appendix, I show that both county- and state-initiated reform mechanisms lead to a boost in turnout and that the findings are robust to excluding the few cases where the change was packaged with unrelated reforms. This alleviates concerns that the boost to turnout is an artifact of the way the reform in selection method was initiated. I also run a series of Callaway and Sant’Anna (2021) regressions in Section A.2.5 to estimate the dynamic effects of switching from an elected to an appointed election official. The positive effects of appointments on voter turnout appear over time and across multiple county cohorts and time periods.²⁵ Finally, it is possible that low rates of turnout among African-Americans in the South due to the lingering effects of repressive Jim Crow restrictions confound the results. I show in Table A.11 in the online appendix that the results hold using only more recent elections, with some attenuation in effect magnitude.

²⁵This provides evidence that the positive effects of appointment are not simply due to a novelty or Hawthorne-style effect in the immediate aftermath of a change. I am not able to reliably estimate the effects of switching from appointed to elected clerks due to the small number of counties that have switched in this direction.

4.2 Validating the Effect of Appointing Election Officials on Voter Turnout

In this section, I validate my main finding that appointed local election officials produce higher voter turnout than directly elected officials. I utilize alternative difference-in-difference estimators and employ a generalized synthetic control balancing method which relaxes the assumptions needed for causal inference. These estimators show the results to be robust to a range of specifications.

4.2.1 Validating the Staggered Rollout Design

Recent scholarship has identified potential problems with the standard two-way fixed effects estimator when used in staggered adoption designs (Baker, Larcker, and Wang 2022; Borusyak, Jaravel, and Spiess 2021; de Chaisemartin and D’Haultfoeulle 2020; Callaway and Sant’Anna 2021). These issues stem from heterogeneous treatment effects. If treatment effects vary across time or units, the estimate will be biased due to the assignment of negative weights to some comparison groups.

To validate my main findings, I test a range of alternative specifications in Table A.12 in the online appendix, including removing counties that switch from appointments to elections, removing counties that use appointments throughout the dataset, and using stacked difference-in-difference estimators. All specifications result in precisely estimated effects on turnout between 2.1 and 3.5 percentage points. In Section A.3.2, I employ the de Chaisemartin and D’Haultfoeulle (2020) estimator and the Callaway and Sant’Anna (2021) dynamic effects estimator for states with at least 10 treated counties. The results are consistent with those shown in Section A.2.3.

4.2.2 Generalized Synthetic Control

An underlying concern of the difference-in-difference estimation strategy is that treated and control units do not look like one another. If the places that switch from electing to appoint-

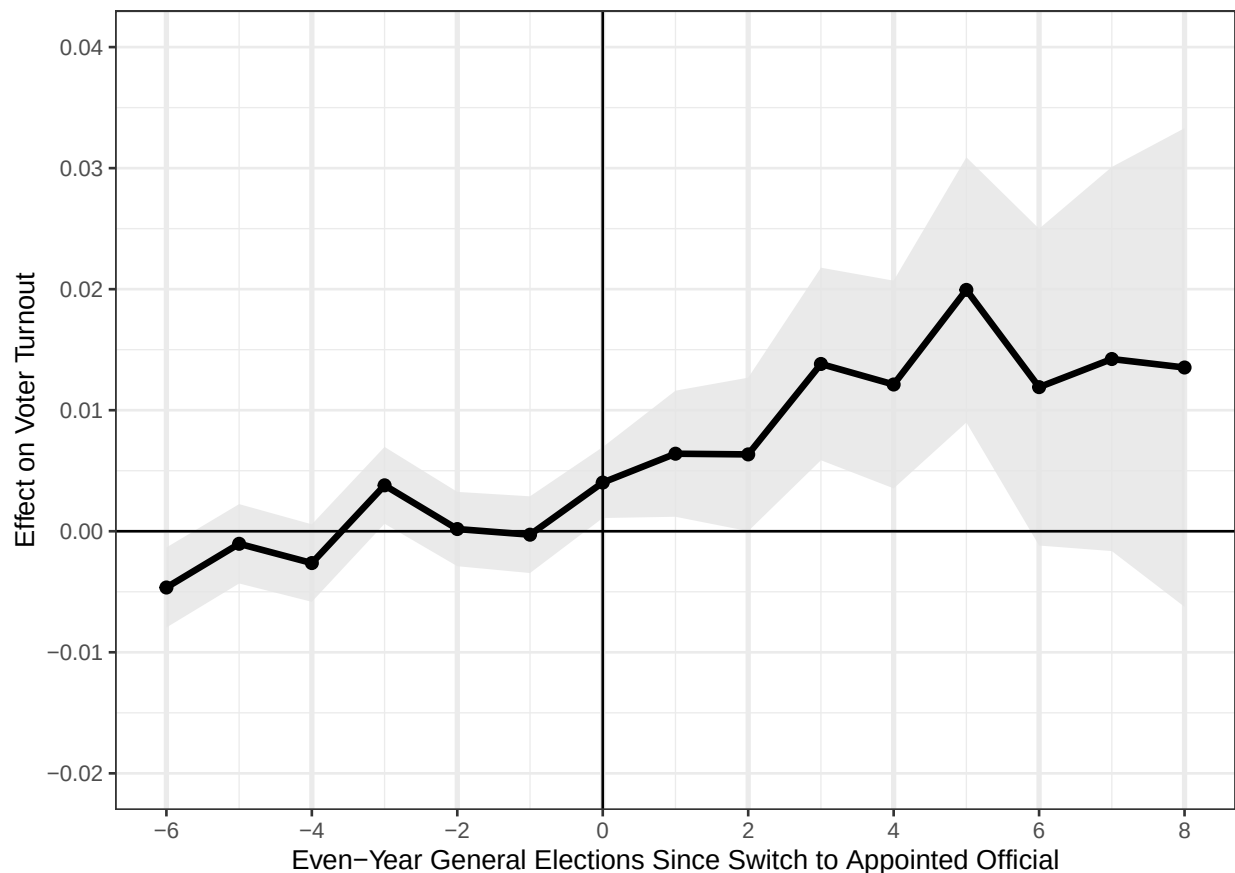
ing election officials are fundamentally different from those that remain elected on some unobserved characteristics, then this will undermine the causal validity of the regression specification. Figure A.5 in the online appendix investigates the validity of the parallel trends assumption using the Dube et al. (2022) local projections event studies estimator. It shows evidence that places that adopt appointments may be on different trajectories prior to reform.

I overcome this concern through the generalized synthetic control method. This estimation strategy rebalances the data sample by comparing treated and untreated units with similar pre-treatment voter turnout history. Figure 4 displays output from a Xu (2017) generalized synthetic control estimation. The line in the left-hand side of the figure is close to 0, showing that the strategy successfully compares treated and control counties with similar pre-treatment turnout trajectories. It becomes positive in the right-hand side of the figure and is statistically distinguishable from 0. This provides additional evidence that appointed election officials administer elections with higher turnout than their elected counterparts. As shown in Table A.18 in the online appendix, it produces a precisely estimated effect size of 0.8% on voter turnout, lower than the estimates shown in Table A.5 but still substantial for participation in federal general elections. In Section A.2.5 in the online appendix, I explore two possibilities for an increasing effect magnitude over time: delayed effects due to selection method reform triggering turnover, and a secular trend of declining availability in local news.

4.2.3 Randomization Inference

Randomization inference can be used to derive an alternative estimate of the likelihood of finding an effect as large or larger than the one observed by chance. I employ two different randomization permutations: in Figure A.9 in the online appendix, I randomly permute both which counties are treated and when they are treated, and in Figure 5, I randomly permute when treated counties receive treatment. Counties that switch from appointed to elected and counties that switch selection method multiple times are excluded. 1,000 permutations

Figure 4: **Estimated ATT of Generalized Synthetic Control.** This graph displays a generalized synthetic control method of the two-way fixed effects regression estimating the effect of appointing local election officials on even-year general election voter turnout. The specification includes two-way additive county and year fixed effects, automated cross-validation to identify the optimal number of factors, and a parametric bootstrap with 1000 samples. The black line is a dynamic estimated ATT effect of appointing an election official on turnout and the band is a 95% confidence interval.

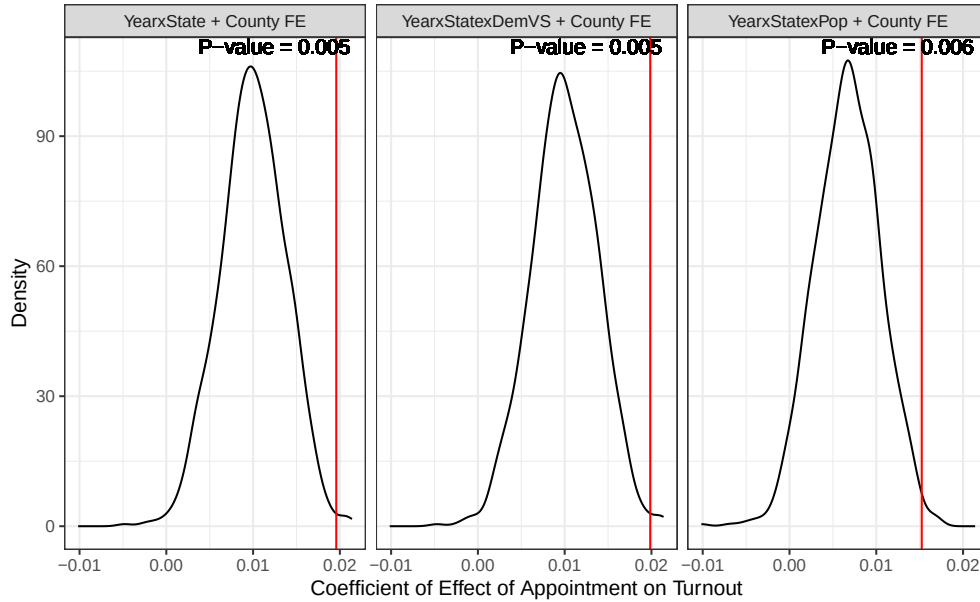


are computed for each exercise. The three regressions shown in Table 1, columns 1-3 are replicated with the permuted data and the coefficient stored for each permutation. Finally, the actual coefficient derived is compared with the distribution of permuted coefficients. The p-value is the number of randomized coefficients that are greater than or equal to the actual effect size divided by the total number of iterations.

Figure A.9 shows that the likelihood of observing the actual result or a more extreme effect is close to 0, given randomized treatment and treatment timing and assuming the true effect is null. The more demanding inferential test is when the counties that switch

to appointments are preserved, but when they switch is scrambled. Figure 5 shows that random treatment timing of the treated units typically results in a positive relationship between appointments and voter turnout. This aligns with the evidence of pre-trending shown in Figure A.5 and corrected for by the generalized synthetic control method in Figure 4. However, it is still extremely unlikely to get an observed effect as large as was actually observed—only in about 1 out of 500 simulations does the effect size reach 2%. This provides additional validation that appointing election officials increases voter turnout.

Figure 5: Randomization Inference for Table 1, Columns 1-3 - Timing of Treated Counties. This graph displays the output of randomization inference for the main effects of appointed local election officials on voter turnout. Which counties receive treatment is preserved, but when they first switch to appointments is randomly permuted. The black distribution shows the resulting coefficients of 1,000 iterations. The red solid vertical line is the actual coefficient observed, and the p-value is the share of coefficients that are equal to or larger than the one estimated in the respective specification in Table 1.



4.3 Appointing Election Officials Boosts Registration Rates More when Their Duties Specifically Include Registration

I run a placebo test examining whether switching to appointed officials increases registration rates more in states where their duties specifically include registration. The results, found in Appendix A.3.6, show suggestive evidence that counties experience a larger boost in registration rates when the official directly in charge of registration duties switches from an elected to an appointed position.

4.4 Selection Method, Not Partisanship, Explains the Results

Are the observed effects the result of a switch from elected to appointed clerks, or are they due to the switch from an openly partisan office to an ostensibly nonpartisan position? The results in Table A.5 present a bundled treatment of both selection method and partisanship. The partisan nature of elected office could lead clerks to act in ways that differ from their nonpartisan appointed counterparts—for instance, by attempting to alter turnout to advantage co-partisans. Georgia, Montana, and Washington’s history of county-level changes between elected partisan, elected nonpartisan, and appointed election officials provides an opportunity to disentangle the effects of selection method and partisanship. Table A.20 provides strong evidence that selection method, and not the partisan nature of the office, drive the main results on voter turnout.

4.5 Selection Method Effects Are Largest in Small Jurisdictions

Previous research suggests that the population of a jurisdiction is a defining feature in how its elections are run (Burden et al. 2012; Kimball and Baybeck 2013). The vast majority of election jurisdictions serve a small number of people, with 94% of jurisdictions serving less than one-third of the population and the median jurisdiction serving only 2,000 individuals (Kimball and Baybeck 2013). In counties where local election officials have fewer deputies,

the actions of the chief official could have a greater impact on participation rates. Table 2 displays the results of difference-in-difference regressions testing the magnitude of the difference in effect between less and more populous counties. A “small county” is defined as ranking in the bottom half in population compared to other counties within the same state. The top row is the effect of switching to appointed election officials for populous counties, and the bottom row is the additional effect of switching to appointments for relatively less populous counties. It is apparent that the effects are largest in small counties. Appointed election officials in less populous jurisdictions produce turnout rates that are between 2.0 and 2.2 percentage points higher than their elected counterparts, compared with 0.7 to 0.9 percentage points higher in more populous jurisdictions. A similar pattern is found with registration rates, with effect sizes in smaller counties double those found in large counties.

Table 2: Appointing Local Election Officials Increases Citizen Participation Especially in Small Counties (Even-Year General Elections, 1968-2020)

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.009 (0.003)	0.007 (0.003)	0.009 (0.003)	0.005 (0.004)	0.005 (0.004)	0.006 (0.005)
Appointed X Small County	0.013 (0.007)	0.013 (0.007)	0.013 (0.007)	0.006 (0.008)	0.005 (0.008)	0.003 (0.008)
Counties	1114	1114	1114	941	941	941
Elections	28	28	28	13	13	13
Observations	31104	31104	31104	12203	12203	12203
Outcome Mean	0.50	0.50	0.50	0.84	0.84	0.84
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State x Small FEs	Yes	No	No	Yes	No	No
Year x State x Dem vs x Small FEs	No	Yes	No	No	Yes	No
Year x State x Pop FEs	No	No	Yes	No	No	Yes

Small counties rank in the bottom half in population compared to other counties within the same state. Robust standard errors clustered by county in parentheses. The number of observations is smaller in columns 3-4 because Arizona and Georgia are excluded and because turnout data is available from 1968 but registration data is only available from 1996.

4.6 No Evidence That Appointed Local Election Officials Benefit Their Principals' Party Or That The Effect Is Different in Democratic Vs. Republican-Leaning Jurisdictions

I test whether appointed officials benefit the party of their supporters by compiling original data on counties in two states where partisan county commissioners appoint the local election official: Arizona and Pennsylvania. I use a difference-in-differences specification, testing what happens when the majority party of the election official's appointing body changes. The results, shown in Table A.21, reveal little evidence that election officials appointed by a newly Democratic body shift vote share or registrants in a Democratic direction. The regressions are precisely estimated, and no estimated effect is larger than three-tenths of a percentage point shift in a direction benefiting Democrats.

I also test whether the effect of appointments on participation is shared equally across jurisdictions, regardless of partisan lean, or concentrates in jurisdictions of a certain partisan balance. I show in Table A.22 that the benefits of switching to appointed election officials are similar across Democratic- and Republican-leaning jurisdictions.

5 Why Does Appointing Election Officials Increase Voter Participation?

What do appointed election officials do differently from elected officials that increases voter turnout and registration rates for their constituents? I show that appointed officials obtain additional election administration resources. Beyond this, I cannot definitively tell how appointed officials increase participation. However, I provide suggestive evidence that appointing election officials leads to higher election official salaries, a larger workforce, more robust communication with voters, and lower wait times, all consistent with activities that could increase participation. However, some findings are inconsistent with expectations. I

fail to find any differences between appointed and elected officials in polling places per 1,000 residents, share of provisional ballots cast, share of provisional or absentee ballots rejected, and share of registrants removed from the list.

5.1 Appointed Election Officials Obtain More Resources

Sufficiently funding elections is essential to ensuring high quality administration (Mohr et al. 2019, 2020; Kropf et al. 2020; McGowan et al. 2021). Previous scholarship has shown that increasing election administration resources can boost voter turnout (Grose 2022; but see Lal and Thompson 2024). Burden et al. (2013) argue that appointed officials are *less* able to advocate for more resources than their elected counterparts and therefore administer elections with fewer resources. However, Taylor, Swint, and Reilly (2024) find that appointed boards of election in Georgia spend 45% more on election administration than elected probate judges. Appointed officials might have better relationships with their principals and thus more sway over election funding. If the quality of selection is higher for appointed officials, they might be more proactive in securing additional resources. Alternatively, they might be more responsive to the interests of cost-conscious voters, as elected judges are more attentive voter’s desires to be efficient (Choi, Gulati, and Posner 2010). It is also possible that in smaller jurisdictions, switching to a dedicated appointed local election official increases the amount of full-time equivalent (FTE) employees who work in election administration.²⁶

I use jurisdiction election administration expenditure data from Mohr et al. (2018). This dataset includes estimated yearly expenditures for each county in Arizona, California, Georgia, Minnesota, Missouri, Nebraska, and Nevada starting from as early as 2002. This en-

²⁶Appointed officials’ sole job is to effectively administer elections. In comparison, most directly elected local election officials in the U.S. undertake additional responsibilities beyond election administration. County clerks have a variety of non-election duties such as maintaining legislative/judicial records and recording vital documents. Other offices, such as tax assessors (used in South Dakota and some Texas counties) and probate judges (used in Alabama and Georgia) have more substantial non-election duties. This resource difference should only exist in the least populous counties, where sometimes only a single official administers elections. According to the 2020 EVIC Survey of Local Election Officials, 34 percent of jurisdictions have no full-time election administrators and 17 percent have exactly one FTE (<https://evic.reed.edu/leo-survey-summary/>). In all other jurisdictions, switching to an appointed official should not directly increase the amount of FTEs.

ables the use of a difference-in-differences regression design to test the effect of switching to appointed election officials on election expenditures. Following Taylor, Swint, and Reilly (2024), I use the natural log of total election expenditures per registered voter as my dependent variable. Table 3 displays the results (an event study plot, found in Section A.3.3, shows no evidence of pre-trending). The first three specifications test the overall effect of appointments on election expenditures and the latter three test whether less populous counties enjoy a larger boost in resources than more populous counties. All point estimates are large and statistically distinguishable from zero. The coefficient in column 1 means that when counties switch to an appointed election official, their election expenditures per registered voter increases by 28 percentage points on average. We can confidently rule out effects of less than 8 percentage points. The average county spends \$9.50 per registered voter on administering elections in even years, and appointed officials secure an additional \$2.66 per registrant.

The effects are, if anything, smaller in less populous jurisdictions. This means they are likely driven by the actions of the local election official rather than a result of creating an additional FTE election administration position. I further investigate the reason for this effect by examining whether appointed officials were more likely to apply for the Center for Tech and Civic Life’s 2020 COVID grants to election administrators. Using data on grant applications from Lal and Thompson (2024) and nationwide cross-sectional selection method data from Ferrer and Geyn (2023), I find in Table A.23 that appointed officials were 7 percentage points more likely to apply for the grants compared to elected officials in counties within the same state, even after controlling for a range of factors including population, partisanship, median income, urbanicity, non-Hispanic white share, and COVID severity. This provides suggestive evidence that the effect is due to a quality difference between elected and appointed officials rather than elected officials simply being more attentive to the desire of voters to minimize costs. Finally, I show in Table A.24 that increased election expenditures may lead to additional voter turnout, especially in smaller jurisdictions. A

Table 3: **Appointing Local Election Officials Increases Election Expenditures (Even-Year General Elections, 2004-2016)**

	Ln(Total Election Expenditures Per Registered Voter)					
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.280 (0.100)	0.270 (0.091)	0.310 (0.098)	0.323 (0.110)	0.367 (0.110)	0.357 (0.109)
Appointed X Small County				-0.100 (0.224)	-0.204 (0.209)	-0.122 (0.215)
Counties	434	434	434	432	432	432
Elections	6	6	6	6	6	6
Observations	1929	1929	1929	1920	1920	1920
Outcome Mean	2.25	2.25	2.25	2.25	2.25	2.25
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	No	No	No
Year x State x Dem vs FEs	No	Yes	No	No	No	No
Year x State x Pop FEs	No	No	Yes	No	No	Yes
Year x State x Small FEs	No	No	No	Yes	No	No
Year x State x Dem vs x Small FEs	No	No	No	No	Yes	No

Small counties rank in the bottom half in population compared to other counties within the same state. Robust standard errors clustered by county in parentheses. Data is from Mohr et al. (2018) and is available for Arizona, California, Georgia, Minnesota, Missouri, Nebraska, and Nevada. Elections are the average number of elections included for each state, rounded down to the nearest interger. Expenditure data is normalized to 2020 dollars.

doubling of expenditures per registrant increases voter turnout by 0.4 percentage points on average and by 0.7 percentage points in less populous counties. This is in line with previous findings linking election expenditures with higher turnout (Kropf and Pope 2020).

5.2 What Administrative Policies Do Appointed Election Officials Pursue Differently?

Given that appointed election officials obtain more resources, what might they do with these resources that could lead to higher participation? Using data from the 2020 EVIC Survey of Local Election Officials, I find suggestive evidence in Section A.5.1 that appointed officials serving in similarly populous jurisdictions within the same state make \$5,000 more

than elected officials and hire an additional 0.6 FTEs on average, although both results are imprecise.

Election officials could use additional funding to improve voter outreach. Clerks have significant discretion in their communication with voters. They can pursue a proactive strategy of providing additional information to the public and accurately responding to constituent questions. Or, they can provide the legally required minimum amount of information. More active election official communication strategies has been shown to increase the share of registered voters (Merivaki and Suttman-Lea 2023), improve voter confidence (Suttman-Lea and Merivaki 2023), and reduce the number of mail ballots that are rejected (Suttman-Lea and Merivaki 2022). In an audit study of election officials, White, Nathan, and Faller (2015) found that elected officials were 16% less responsive and 12% less accurate in their responses than appointed officials. Figure A.26 in the online appendix uses data from Thesalia Merivaki and Mara Suttman-Lea to test whether appointed officials are more likely to maintain official social media accounts than elected officials serving in similar jurisdictions. I find that appointed officials are twice as likely to have a Twitter account as elected officials, although I do not find differences in the usage of other platforms.

I use EAVS data to explore the possibility that more voter outreach reduces the usage of provisional ballots and the rejection of provisional and absentee ballots or additional resources leads appointed officials to open more polling places. The results, found in Section A.4.3, do not allow me to rule out that appointed and elected administrators run elections with similar provisional ballot usage, provisional rejection rates, and absentee ballot rejection rates, as well as numbers of polling places per 1,000 residents and registration removal rates.

Additional resources could be employed to improve the Election Day experience for voters in a number of additional ways, including hiring more poll workers, providing them with better pay and more rigorous trainings, and better provisioning polling places with poll booths and voting machines. While I cannot directly test these mechanisms, I use the 2020 CES to examine whether voter wait times decrease when counties switch to appointed adminis-

tration. I employ difference-in-difference regressions with county and year fixed effects and individual controls for gender, race, age, education, and party identification. The results are shown in Figure 4. While the regressions are relatively imprecise, the coefficients are all negative and the effect sizes are large in relation to the outcome means. Switching to appointed election officials reduces the percentage of voters that wait at least 10 minutes in line by 8 percentage points, an effect size of 0.3, and reduces the percentage of voters waiting in line for 30 minutes or more by 2 percentage points. Longer wait times have been found to depress future voter turnout, making this one plausible factor explaining why appointed officials boost participation (Pettigrew 2021).

Table 4: **Appointed Local Election Officials May Decrease Voter Wait Times (Even-Year General Elections, 2008-2022)**

	Any Wait (1)	> 10 min (2)	> 30 min (3)	> 1 hr (4)
Appointed	-0.006 (0.074)	-0.078 (0.049)	-0.020 (0.028)	-0.000 (0.013)
Counties	745	745	745	745
Elections	6	6	6	6
Observations	15902	15902	15902	15902
Outcome Mean	0.50	0.23	0.09	0.03
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Individual controls are gender, race, age, education, and party identification.

6 Why Do Appointed Election Officials Outperform Elected Officials?

I explore two sets of mechanisms that could lead appointed election officials to produce better outcomes for constituents than elected officials: that the quality of selection is higher for appointed officials, and that the quality of sanctioning is higher for appointed officials.

For the former, I examine differences in education between elected and appointed clerks and the low contestation rates of clerk elections. For the latter, I investigate the information voters know about their election official, differences in turnover rates, and triple difference-in-difference estimates comparing the effect of selection method based on the presence of a local newspaper.

6.1 The Quality of Selection Is Higher For Appointed Election Officials

Are appointed local election officials more equipped for their job than elected administrators? This could be due to some failure in elections that prevent voters from selecting the most qualified individuals—because of a limited pool of viable candidates, lack of contested elections, aversion of experienced or well-educated administrators to elections, or the absence of high-quality information. It could also be due to geographic restrictions imposed by elections. In order for voters to choose quality candidates, they need to run in the first place. But voters rarely have a choice in election administrator at the ballot box. Ferrer, Geyn, and Thompson (2023) find that only 23% of general election races for local election official feature a contest between a Democrat and a Republican, and only 12% of all contests result in a race with a margin of victory of less than 20 percentage points. Previous research shows that low contestation rates is a problem across local offices (Burden and Snyder 2021; Lappie and Marschall 2018; Marschall and Lappie 2018; Thompson 2020; Yntiso 2022).

I use the 2020 EVIC Survey of Local Election Officials to examine whether elected and appointed officials possess different levels of education, a common indicator of the quality of public officials (Dal Bó et al. 2017). Table 5 tests differences in education between elected and appointed officials. All specifications include state fixed effects and both log population and log population squared controls to ensure that comparisons are only made between appointed and elected officials who oversee elections in similarly sized jurisdictions within

the same state. Any differences that arise are likely due to the selection method itself rather than inherent differences in the places that elect and appoint clerks.

Table 5: Appointed Local Election Officials Possess More Education Than Elected Officials

	Edu (1)	Degree (2)	Any College (3)	Edu (4)	Degree (5)	Any College (6)
Appointed	0.336 (0.209)	0.160 (0.115)	0.113 (0.044)	0.578 (0.232)	0.148 (0.089)	0.116 (0.056)
Appointed X Small County				-0.385 (0.316)	0.087 (0.168)	0.009 (0.100)
States	44	44	44	38	38	38
Observations	581	581	581	581	581	581
Outcome Mean	2.86	0.58	0.88	2.86	0.58	0.88
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Log Pop	Yes	Yes	Yes	Yes	Yes	Yes
Log Pop squared	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered by state in parentheses. Data is from the 2020 EVIC Survey of Local Election Officials and is filtered to only include chief local election officials. County is imputed from zip code to calculate population controls. Observations are weighted to be representative of the population of local election officials. Columns 1 and 4 measure educational attainment on a 5-point scale: high school, some college, college, some graduate school, and graduate school. Columns 2 and 5 measure whether the official possess a college degree, and columns 3 and 6 measure any college education.

Appointed officials appear to possess more formal education than elected officials. Appointed officials are 16 percentage points more likely to hold a college degree than elected officials (column 2) and are 11 percentage points more likely to receive any college education (column 3), an effect statistically distinguishable from 0. Columns 4–6 test whether the difference in education between elected and appointed officials is larger in less populous jurisdictions. This should be the case if the quality difference is due to a limited pool of candidates or geographic restrictions, rather than the absence of adequate voter information or inherent aspects of elections that turn away more educated professionals. There is little evidence that the effect varies across less and more populous jurisdictions.

Table A.27 in the online appendix tests a number of additional indicators of quality between elected and appointed officials using the 2020 EVIC survey. I find that appointed

officials tend to possess less previous experience in election administration, may hold more professional memberships, are likelier to have served elsewhere and in a greater number of previous jurisdictions, are less likely to be over the age of 65 years, make approximately 10% more in salary than elected officials, and recruit an additional 0.5 FTE. These findings are in line with a recent survey of municipal clerks in New England which found that elected clerks are older, less educated, longer-tenured, and have less institutional capacity than appointed clerks (Marsh et al. 2024). I take this as evidence that appointed officials possess less election administration experience but are more professionalized than their elected counterparts.

6.2 The Quality of Sanctioning Is Higher For Appointed Election Officials

In this section, I present evidence that voters do not know much about their local election officials, that the effects of switching to appointments on voter turnout are largest in jurisdictions without the continuous presence of a local newspaper, and that appointed officials have higher turnover rates.

6.2.1 Voters Know Little About Their Local Election Official

I fielded a survey of 3,200 U.S. adults to test respondent knowledge of their local election official. The survey hypotheses and analysis was preregistered on OSF,²⁷ and technical details are provided in Section A.5.2 in the online appendix. I compiled a complete list of currently serving chief local election officials from government websites and linked respondents to their current election official using zip code. Correcting for the frequency of guesses, only 17.2% of respondents were able to correctly identify the title of their chief election official. Fewer than 10% of respondents knew whether their election official was elected or appointed. And only 7.9% of respondents correctly identified their election official out of a list of five names.

²⁷osf.io/k7hq2

If the vast majority of voters do not know the position responsible for administering their elections, whether it appears on their ballot, or the person in charge of running elections in their community, it is unlikely that voters are able to adequately monitor the performance of this official and sanction them for mediocre performance. This is in contrast to the local elites in charge of appointing election officials. By their very nature, all principals know who the election official is and are likely to have a better idea of the quality of their work.

6.2.2 The Performance Gap Between Elected And Appointed Officials Is Largest In Jurisdictions That Lack a Local Newspaper

If appointed officials perform better than elected officials because they are better monitored, then the difference in performance should be smaller in jurisdictions where voters have greater access to information about local politics. Previous scholarship has established a causal effect between the presence of a local newspaper and increased turnout in federal elections (Gentzkow, Shapiro, and Sinkinson 2014), increased electoral competition in local races (Rubado and Jennings 2020), and a stronger incumbency advantage (Lockhart 2021). Is the performance gap between appointed and elected clerks larger when the county lacks a local newspaper, thus depriving voters of the information necessary to hold the public official accountable?

I test the effects of the presence or lack of a local daily newspaper on the relationship between selection method and voter turnout using a triple difference-in-differences design and a combination of two datasets: (Gentzkow, Shapiro, and Sinkinson 2014), which contains newspaper data from 1960 to 2004, and data from Sean Ewing that updates this data through 2020. I sort counties into two categories: those that have continuously had at least one newspaper headquartered in its boundaries within the study period, and those that have not. Table 6 displays the results of this analysis. The first row shows the effect of counties switching from elected to appointed election officials when they lack the continuous presence of at least one local newspaper. Column 2 is the additive effect on switching for counties that

have a local newspaper presence. Nearly the entirety of the positive benefits to appointing election officials lie in counties that lack local news coverage. An alternative specification, introducing over-time variability in the presence of a local daily newspaper, shows results consistent with Table 6 and is found in Section A.5.3.

Table 6: Consistent Presence of a Daily Local Newspaper Attenuates the Effect of Appointing Local Election Officials on Citizen Participation (Even-Year General Elections, 1968-2020)

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.025 (0.005)	0.024 (0.005)	0.021 (0.005)	0.011 (0.006)	0.011 (0.007)	0.007 (0.007)
Appointed X Newspaper	-0.021 (0.007)	-0.020 (0.007)	-0.013 (0.007)	0.001 (0.009)	0.002 (0.009)	0.007 (0.010)
Counties	979	979	979	824	824	824
Elections	14	14	14	6	6	6
Observations	13661	13661	13661	5751	5751	5751
Outcome Mean	0.58	0.58	0.58	0.85	0.85	0.85
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State x Newspaper FEs	Yes	No	No	Yes	No	No
Year x State x Dem vs x Newspaper FEs	No	Yes	No	No	Yes	No
Year x State x Pop x Newspaper FEs	No	No	Yes	No	No	Yes

Robust standard errors clustered by county in parentheses. All counties that switch between having and not having a daily newspaper over the period of analysis are dropped. The number of observations is smaller in columns 4-6 because Arizona and Georgia are excluded and because turnout data is available from 1968 but registration data is only available from 1996.

6.2.3 Appointed Election Officials Have Higher Turnover Rates Than Elected Officials

If appointed local election officials are monitored and sanctioned more than elected officials, then they should have shorter tenures in general. I test this using an original panel of the names and service tenures of chief local election officials across jurisdictions spanning 2000 to 2022, collected mainly from state and local administrative archives (Ferrer and Thompson 2024). Table 7 shows the results and Table A.7 in the appendix tests for pre-trending.

Table 7: **Appointed Local Elections Officials Turnover At Higher Rates Than Elected Officials (2004-2022)**

	Election Official Turnover (1)
Appointed	0.049 (0.025)
Counties	1113
Elections	3
Observations	10881
Outcome Mean	0.18
County FEs	Yes
Year x State FEs	Yes
Robust standard errors clustered by county in parentheses.	

Column 1 shows the effect of a switch in a county from elections to appointments on turnover of the election official. Because this switch causes turnover in most cases, I impute missing dependent variable values for the year each jurisdiction moves into treatment. It appears that appointed officials leave the position at higher rates than elected officials. Switching to an appointed official increases the probability of turnover over a 2-year period by 4.9 percentage points. Considering the average 2-year turnover rate of election officials in the dataset is 17%, this is a fairly substantial effect.

7 Conclusion

Across America’s history, democracy-minded reformers have tinkered with the selection method of government offices in an attempt to improve the accountability and performance of its public servants. In recent years, this practice has spread to local clerks, who are facing unprecedented attacks from former President Trump and his supporters and immense pressure to deliver free and fair elections. States are increasingly shaping the administrative structures of local jurisdictions for seemingly partisan ends, affecting who controls elections

for millions of Americans. These decisions could have significant consequences for the quality of elections and the timely and accurate certification of election results—something that came close to not happening in the 2020 presidential election.²⁸ Yet we have lacked the ability to effectively adjudicate between selection methods.

Using original data from 13 states, spanning 1,116 counties across 62 years, I show that when counties switch from electing to appointing their clerks voter participation rates increase substantially. The boost to voter turnout is on par with the most effective convenience reforms designed to raise participation such as implementing universal vote-by-mail (Thompson et al. 2020) and automatic voter registration (McGhee, Hill, and Romero 2021). It is several times the effect of get-out-the-vote interventions such as door-to-door knocking, mailings, and phone calls (Green, McGrath, and Aronow 2013). The findings are robust to alternate specifications including general synthetic control and randomization inference; hold across multiple time periods, states, offices, and reform mechanisms; and do not come at the expense of increased partisan manipulation of election results. Appointed officials appear to boost local expenditures on election administration, hire additional staff, increase communication with voters, and reduce voter wait times. I show evidence for stronger selection and sanctioning mechanisms to explain these effects. Appointed officials are more educated and more professionalized than elected officials. Most voters cannot identify their local election official from a list of names, appointed officials outperform elected officials most in jurisdictions with the least availability of local news, and appointed clerks have higher turnover rates.

These findings add to a growing literature on the limits of elections in ensuring accountable officeholders (Ashworth 2012; Rogers 2023). Elections are designed to achieve accountability between officeholders and the public. When voters have access to high-quality information, can make a choice between multiple candidates, and are able to effectively sanction an officeholder who shirks their duty, agents will be incentivized to perform their best in order

²⁸<https://www.politico.com/news/2020/11/17/wayne-county-michigan-election-certification-437181>

to win another term in office. However, if voters do not have access to adequate information or a sufficient choice on election day, there is little they can do to demand accountability from elected officials. The findings are in line with studies that have found that appointing other local offices, such as municipal assessors, treasurers, and managers, leads to preferable policy outcomes (Hajnal and Trounstein 2014; Sances 2016; Whalley 2013). Elections for local offices can counter-intuitively fail to ensure accountability or create adverse accountability effects that have undesirable policy consequences. This is especially true considering information environments at the local level continue to deteriorate (Lockhart 2021; Martin and McCrain 2019), the tasks demanded of local officials grow more complex (Hale, Montjoy, and Brown 2015), and contestation rates remain low. In short, knowledge, information, and expertise matter—and sometimes democracy works best when it does not let voters make all the decisions.

It is worth noting that appointing public officials does not guarantee desirable outcomes and that elections play an important role in the democratic process. In the 1960s, counties in the South eliminated elected offices in the wake of the Voting Rights Act for the express purpose of maintaining white power (Komisarchik 2018). The politicization of appointing authorities is emerging as a concern once again. For instance, several recently enacted bills in Georgia have created highly partisan election boards, including some filled with election deniers.²⁹ However, my results suggest that over a long period of time and across several states, appointed election officials have produced better outcomes for their constituents than elected officials.

Future work should consider other instances where elections fail to achieve their intended effects, with a goal of uncovering under what broader conditions appointed public produce better outcomes for their constituents. This analysis suggests that the information environment, competitiveness conditions, and technical requirements of the office shape the selection method trade-off. We also need better measures of accountability outcomes for public officials

²⁹<https://www.washingtonpost.com/nation/2022/03/14/georgia-elections-fraud-purge/>

(Carreri and Payson 2021). Scholars should work to distinguish between public responsiveness and conflicts in principals' goals. Are appointments beneficial only when the desires of voters and elites align? How often do they diverge, and what factors make preference convergence more likely? Measuring which issues and to what degree elites and voters have differing preferences could go a long way to clarifying the contexts where appointments are preferable to elections.

These findings also inform an ongoing public debate over the best form of election administration in the United States. Jurisdictions across the country continue to actively consider changes to how they select their local election officials. At a time when America's democracy has come under immense strain, it is more important than ever that the stewards of the democratic process are up to the task of administering our elections.

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Online Appendix

Intended for online publication only.

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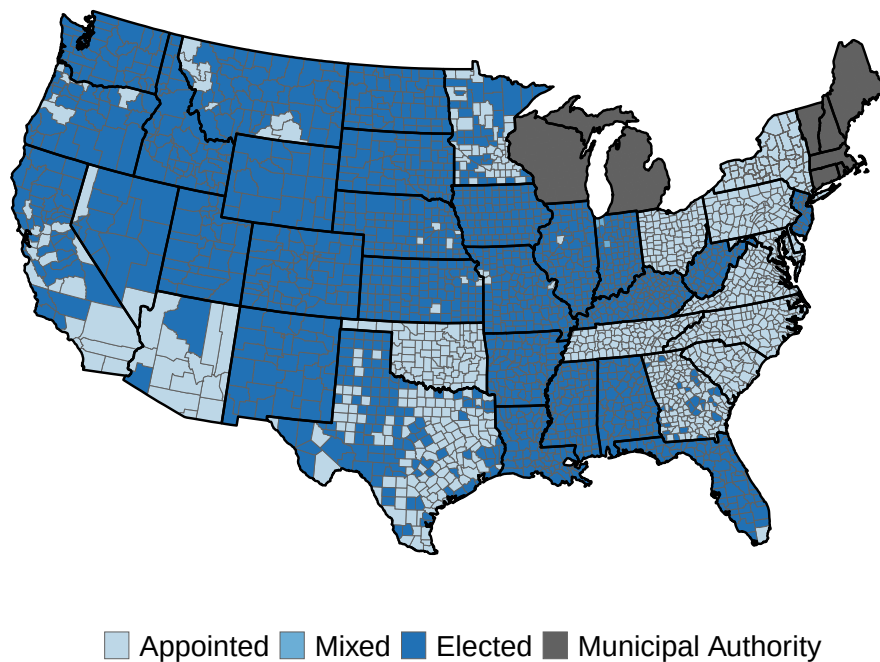
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A.1 Descriptive Appendices

A.1.1 Local Election Official Selection Method Map

Figure A.1 displays the current selection method of each main election authority for every jurisdiction in the United States where elections are administered at the county level.

Figure A.1: **Local Election Official Selection Method by County.** This map displays the selection method of the central election authority for each county in the United States where elections are administered at the county-level, as of 2022. In counties where municipal jurisdictions have separate administrators, the selection method for the county official is reflected. Data is from Ferrer and Geyn (2023). All election jurisdictions in Alaska use appointed officials and all counties in Hawaii use elected officials.



A.1.2 Descriptive Comparison of the Data Sample

Table A.1 compares counties within my sample of 13 states to counties in the 29 states that administer elections at the county level but that have not experienced any changes in selection method since 1960. I use population, racial/ethnic demographics, and region designation from the 2020 census and Democratic presidential vote share, voter turnout, and voter registration from Leip’s Election Atlas for the 2020 presidential election. Selection method data for the out-of-sample comparison is from Ferrer and Geyn (2023). Selection method for the in-sample data reflect administration for the 2020 general election.

Overall, there are few major differences between in-sample and out-of-sample counties. Counties within the sample are slightly more populous, less Democratic, and have larger Hispanic populations than counties not in the sample. The sample consists of more Western and Midwestern states and no Northeastern states. Finally, counties in the sample are somewhat less likely to appoint their local election officials.

Table A.1: **Description of Counties In and Not In Sample**

	In Sample (1)	Not In Sample (2)
Population (Thousands)	113.42 (449.41)	99.25 (243.57)
Dem Pres Vote Share	0.31 (0.16)	0.35 (0.16)
Voter Turnout	0.63 (0.11)	0.64 (0.10)
Voter Registration	0.86 (0.10)	0.90 (0.11)
Share Non-Hispanic White	0.74 (0.21)	0.77 (0.19)
Share Black	0.076 (0.12)	0.102 (0.15)
Share Hispanic	0.14 (0.18)	0.07 (0.09)
Northeast	0.00	0.11
Midwest	0.42	0.28
South	0.39	0.50
West	0.19	0.11
Share Appointed	0.32	0.42
Num Counties	1117	2016

Standard deviations are reported in parentheses below group means. Counties for the 8 states with municipal-level election administration (CT, MA, ME, MI, NH, RI, VT, WI) are not included in the out-of-sample descriptive characteristics.

A.1.3 Local Election Official Selection Method Changes by State

Table A.2 displays additional data on the elected and appointed local election entities used in the analysis for each state, as well as counts of the number of counties in each state, the number always appointed, the number always elected, the number switching from elected to appointed, the number switching from appointed to elected, and the number undergoing multiple switches. These counts are a tabular form of Figure 1. The table also includes the first and last year a clerk selection method switched in each state. All of this data is in reference to the years of analysis, 1960 to 2022. Three columns are of particular importance: elected to appointed, appointed to elected, and multiple switchers. The counties falling in these three categories within each state power the difference-in-difference analysis. As shown in the table, the number of counties shifting to appointments far exceeds the number switching to elections. Ignoring those switching multiple times, 333 counties have switched to appointing their election official since 1960, compared with 3 counties that switched to electing theirs. In other words, 99.1% of all switches in selection method have been from elections to appointments. When counting each switch separately (including counties with multiple switches), 93% of all switches in selection method have been in the direction of appointments.

Table A.2: Local Election Official Selection Methods by State.

State	Elected Entity	Appointed Entity	Counties	Always Appointed	Always Elected	To Appointed	To Elected	Multiple Switches	First Switch	Last Switch
AZ	Election Administrator	Recorder	15	12	0	1	2	0	1997	2020
CA	Clerk	Registrar of Voters / Clerk	58	6	38	14	0	0	1970	2022
GA	Probate Judge	Board of Elections and Registration	159	0	28	129	0	2	1968	2022
IL	Clerk	Election Commission	102	0	93	0	0	1	1974	2016
IN	Clerk	Board of Election and Registration	92	0	89	3	0	0	1994	2020
MN	Auditor	Auditor	87	0	39	48	0	0	1968	2022
MO	Clerk	Election Commission / Director of Elections	115	3	110	1	0	0	1993	1994
MT	Clerk and Recorder	Election Administrator / Clerk and Recorder	56	0	47	8	1	0	1977	2022
NE	Clerk	Election Commissioner	93	2	86	2	0	3	1969	1996
NV	Clerk	Registrar of Voters	17	0	15	2	0	0	1966	1974
OR	Clerk	Elections Manager/Director	36	0	29	6	0	1	1964	1994
TX	Clerk / Tax Assessor	Elections Administrator	254	0	118	119	0	17	1980	2022
WA	Auditor	Elections Director	39	0	38	0	0	1	1969	2009
Totals	-	-	1123	23	730	333	3	23	-	-

Only primary local election authorities are listed under elected and appointed entities—those responsible for the majority of election duties in each county. In states with multiple primary election authorities, they are listed in order by frequency. Always appointed and always elected refer to counties that have maintained the same election official selection method since 1960. Multiple switches refers to counties that have both switched from elected to appointed and from appointed to elected. Not all county switch rows add up to the total number of counties in each state because some counties are excluded from analysis (i.e., those with municipal-level authorities in Illinois and Missouri).

A.1.4 Descriptive Comparison of Counties that Appoint vs. Elect Their Local Election Official

Table A.3 compares appointed and elected counties across the United States using the same data sources described in Section A.1.2 (Ferrer and Geyn 2023). Appointed counties are more than twice as populous on average as elected counties. They are also more Democratic, more racially diverse, and more likely to be located in the Northeast and the South. Appointed counties have slightly lower voter turnout (62% vs. 63%) and voter registration rates (86% vs. 89%) than elected counties. This underscores the importance of using a credible research design to estimate causal effects from observational data.

Table A.3: **Description of Appointed and Elected Counties**

	Appointed (1)	Elected (2)
Population (Thousands)	164.89 (465.11)	63.44 (221.15)
Dem Pres Vote Share	0.37 (0.16)	0.30 (0.15)
Voter Turnout	0.62 (0.10)	0.63 (0.10)
Voter Registration	0.86 (0.09)	0.89 (0.11)
Share Non-Hispanic White	0.70 (0.21)	0.78 (0.19)
Share Black	0.13 (0.15)	0.08 (0.14)
Share Hispanic	0.10 (0.14)	0.10 (0.14)
Northeast	0.13	0.00
Midwest	0.13	0.41
South	0.67	0.39
West	0.07	0.20
Num Counties	1092	1816

Standard deviations are reported in parentheses below group means. Counties for the 8 states with municipal-level election administration (CT, MA, ME, MI, NH, RI, VT, WI) are not included.

A.1.5 Descriptive Comparison of Counties that Switched from Elected to Appointed vs. Always Elected Their Local Election Official

Table A.4 compares “control” counties in the sample—those that always elect their local election officials—to “treated” counties that switch from electing to appointing their election official. Counties that switch from elections to appointments are on average 3.5 times more populous than those that stay elected. They are also more Democratic, tend to have lower turnout and registration rates, are much more racially and ethnically diverse, and are mostly found in South and to a lesser degree the Midwest.

Table A.4: **Description of Elected To Appointed and Always Elected Counties**

	Elected to Appointed (1)	Always Elected (2)
Population (Thousdands)	173.28 (499.67)	51.51 (122.30)
Dem Pres Vote Share	0.35 (0.16)	0.28 (0.14)
Voter Turnout	0.60 (0.12)	0.64 (0.10)
Voter Registration	0.85 (0.11)	0.87 (0.09)
Share Non-Hispanic White	0.63 (0.22)	0.80 (0.18)
Share Black	0.15 (0.16)	0.04 (0.08)
Share Hispanic	0.18 (0.21)	0.12 (0.16)
Northeast	0.00	0.00
Midwest	0.15	0.57
South	0.76	0.21
West	0.09	0.23
Num Counties	358	730

Standard deviations are reported in parentheses below group means. Counties for the 8 states with municipal-level election administration (CT, MA, ME, MI, NH, RI, VT, WI) are not included.

A.2 Robustness Tests

A.2.1 Participation Effects Excluding Midterm Races

Table A.5 displays the results of a two-way fixed effects regression estimating the effects of directly electing a local election official on voter participation. These regressions only include data from presidential elections. The results are similar to those displayed in Table 1 in the main analysis, albeit slightly less precise.

Table A.5: **Appointing Local Election Officials Increases Citizen Participation (Presidential Elections, 1968-2020)**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.020 (0.004)	0.018 (0.004)	0.016 (0.004)	0.010 (0.004)	0.010 (0.005)	0.008 (0.005)
Counties	1116	1116	1116	942	942	942
Elections	14	14	14	6	7	6
Observations	15571	15571	15571	6577	6577	6577
Outcome Mean	0.57	0.57	0.57	0.85	0.85	0.85
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	Yes	No	No
Year x State x Dem vote share FEs	No	Yes	No	No	Yes	No
Year x State x Population FEs	No	No	Yes	No	No	Yes

Robust standard errors clustered by county in parentheses. Voter turnout and registration rate are measured as proportions out of 1. The number of observations is smaller in columns 4-6 because Arizona and Georgia are excluded and because turnout data is available from 1968 but registration data is only available from 1996.

A.2.2 Participation Effects with Alternative Administrative Data

Conflicts arose between administrative and web scrapped data in Texas and the main results included some data imputations for missing cells. Table A.6 shows that the main finding that appointed election officials increase voter participation is robust to alternative coding decisions privileging documents provided by the Texas Secretary of State over archived Secretary of State web pages and removing all data imputations.

Table A.6: **Appointing Local Election Officials Increases Citizen Participation (Even-Year General Elections, 1968-2020, Public Information Act Preferred)**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.018 (0.003)	0.016 (0.003)	0.014 (0.003)	0.009 (0.004)	0.008 (0.004)	0.008 (0.004)
Counties	1116	1116	1116	942	942	942
Elections	28	28	28	13	13	13
Observations	31123	31123	31123	12213	12213	12213
Outcome Mean	0.50	0.50	0.50	0.84	0.84	0.84
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	Yes	No	No
Year x State x Dem vote share FEs	No	Yes	No	No	Yes	No
Year x State x Population FEs	No	No	Yes	No	No	Yes

Robust standard errors clustered by county in parentheses. The number of observations is smaller in columns 4-6 because Arizona and Georgia are excluded and because turnout data is available from 1968 but registration data is available from 1996.

A.2.3 Exploring State and Office Heterogeneity

This section shows evidence that the main result is generalizable across states and offices. Table A.7 estimates the effects of appointing election officials on voter participation separately for each of the four states with at least 10 counties that have changed their election official selection method since 1960. Those states are California, Georgia, Minnesota, and Texas. The results reveal precisely estimated and substantively meaningful effects for Georgia, Minnesota, and Texas. The magnitude of the effect on turnout is greater in Georgia and Minnesota than in Texas. The point estimate for CA is negative, although it is imprecisely estimated.

Table A.7: Appointing Local Election Officials Increases Voter Turnout in Multiple States (Even-Year General Elections, 1968-2020)

	Voter Turnout			
	(1)	(2)	(3)	(4)
Appointed	-0.008 (0.012)	0.022 (0.005)	0.027 (0.008)	0.016 (0.005)
Counties	58	159	87	253
Elections	28	28	28	28
Observations	1624	4452	2436	7084
Outcome Mean	0.49	0.40	0.64	0.43
State	CA	GA	MN	TX
County FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. States are included if at least 10 counties have switched between electing and appointing their local election official since 1960.

I also examine whether the effect holds across different statutory offices. Most directly elected election officials across the United States are county clerks. In my sample of 13 states, all elected election officials in Illinois, Indiana, Montana, Missouri, Nevada, and Oregon are clerks or hold clerk duties in addition to other titles. The same is true of almost all elected election officials in California and Texas. All elected election officials in Arizona are recorders,

which I group with clerks in this analysis due to their similar roles.³⁰ A few Texas counties use elected tax assessors as their election official. Auditor is also a fairly common position. All elected election officials in Minnesota and Washington are auditors, as well as a small number of counties in California. Finally, probate judges are the elected election officials in Georgia. Table A.8 shows that participation increases when appointed officials (the omitted category) replace elected auditors, clerks, and probate judges. The increase is larger when probate judges and auditors are replaced, and somewhat smaller when clerks are replaced. The point estimate for tax assessors is negative but imprecisely estimated, as it relies on a relatively small set of observations.

Table A.8: Switching from Elected Auditors, Clerks, and Probate Judges to Appointed Officials Increases Citizen Participation (Even-Year General Elections, 1968-2020)

	Turnout (1)	Registration (2)
Tax Assessor	-0.008 (0.012)	0.008 (0.012)
Auditor	0.022 (0.007)	0.047 (0.008)
Clerk	0.014 (0.005)	0.008 (0.005)
Probate Judge	0.022 (0.005)	0.006 (0.009)
Counties	1116	1116
Elections	28	13
Observations	31146	14478
Outcome Mean	0.50	0.82
County FEs	Yes	Yes
Year x State FEs	Yes	Yes

Robust standard errors clustered by county in parentheses. Point estimates are reversed for clarity, and thus show the effect of switching from each elected position to an appointed office on participation.

³⁰District & county clerks, found in smaller Texas counties, are also pooled with clerks for parsimony.

A.2.4 Results by Clerk Selection Method Reform Mechanism

One threat to causal inference is that reforms caused by some specific mechanism—state legislature, county legislature, and/or county referendum—are not exogenous to an increase in citizen participation. This seems most likely for referenda. Perhaps initial voter participation in a referendum that caused a change in clerk selection method spurs more turnout in future elections due to increased political efficacy. Or, perhaps the places with stronger cultures of direct democracy are more likely to have a referendum on the matter. Another scenario is that counties with local backing in the change are more likely to equip their newly appointed clerk with the tools to succeed or choose reform at the moment when it is most needed, compared with places where the state legislature initiates the reform. In Table A.9, I run regressions separating counties that have experienced a reform into three categories according to the reform initiator: county legislature, county referendum, and state legislature. Each regression also includes all counties that did not experience a move into or out of treatment throughout the dataset (“always elected” and “always appointed”).

Table A.9: **Appointing Local Election Officials Increases Citizen Participation Across Reform Mechanisms**

	Voter Turnout		
	(1)	(2)	(3)
Appointed	0.014 (0.004)	0.003 (0.015)	0.023 (0.005)
Counties	916	763	885
Elections	28	28	28
Observations	25621	21350	24766
Outcome Mean	0.51	0.53	0.51
Initiator	County Leg	County Referendum	State Leg
County FEs	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses.

The results show that both county and state legislature-initiated reform mechanisms lead to a boost in turnout. Counties whose legislatures decide to switch from elected to appointed

clerks see 1.4 percentage points higher turnout in future presidential elections, on average. The effect is almost double—2.3 percentage points—when states initiate the reform. The result for county referendums is slightly positive but is imprecisely estimated. In short, the results hold across multiple reform mechanisms.

One related concern is that the reforms to clerk selection method that were initiated as part of a county charter suffer from similar endogeneity issues. The bundled treatment nature of these cases could also mean that the turnout effects are due to other changes in county governance that happened to coincide with the change to selection method. Table A.10 removes counties that changed their clerk selection method along with other amendments to their county charter. The results are similar to the main results shown in Table A.5. Virtually all other reforms concerned only the clerk selection method itself or, in rare cases, a reorganization of a few county departments, and thus the turnout effects cannot be attributed to other state or local policy changes.

Table A.10: Finding that Appointing Local Election Officials Increases Citizen Participation is Robust to Removing County Charter Changes

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.018 (0.003)	0.016 (0.003)	0.014 (0.003)	0.010 (0.004)	0.010 (0.004)	0.008 (0.004)
Counties	1108	1108	1108	934	934	934
Elections	28	28	28	12	12	12
Observations	30922	30922	30922	12112	12112	12112
Outcome Mean	0.50	0.50	0.50	0.84	0.84	0.84
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	Yes	No	No
Year x State x Dem vote share FEs	No	Yes	No	No	Yes	No
Year x State x Population FEs	No	No	Yes	No	No	Yes

Robust standard errors clustered by county in parentheses. Voter turnout and registration rate are measured as proportions out of 1. Counties that switched the selection method of clerk as part of a package of reforms to their county charter are removed.

A.2.5 Examining Dynamic, Group, and Time Period Effects of Appointing Election Officials

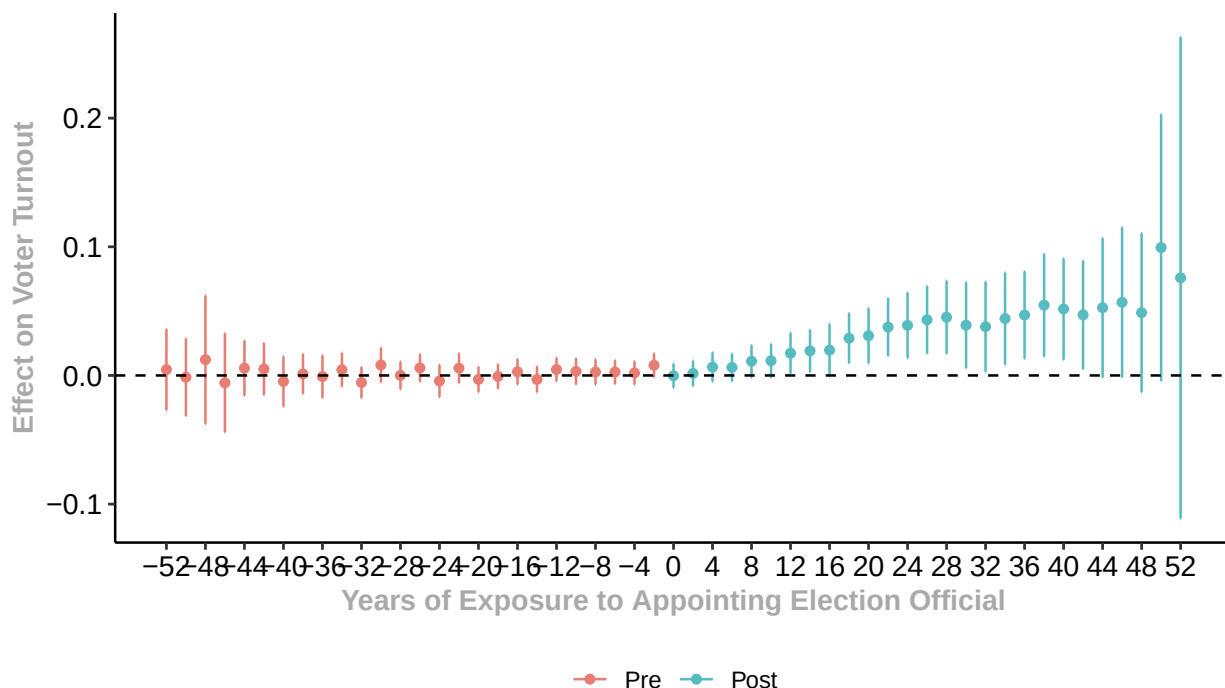
I use specifications from the Callaway and Sant’Anna (2021) estimator to examine dynamic, cohort, and time period effects of switching from elected to appointed clerks on presidential voter turnout. State dummies are used as covariates in these estimates to correct for state-specific trending in voter turnout. Dynamic effects are visualized in Figure A.2, cohort effects are visualized in Figure A.3, and time period effects are visualized in Figure A.4.

As seen in Figure 4 in the main analysis, there appears to be increasing effects on voter turnout over time for counties that switch to appointed administrators, relative to counties with elected officials. There are two potential explanations for this: appointed officials increasingly outperform elected officials as their tenure lengthens, or the value of appointed officials over elected ones has grown over time. In the former scenario, institutional learning effects and start-up costs of switching selection methods mean appointed officials need the practice of administering a few elections to realize their full potential compared to elected officials. Recent work has found that voter wait times may increase after the turnover of a local election official, although turnout rates do not dip when a change of leadership takes place (Ferrer and Thompson 2024). In the latter scenario, the declining ability of voters to adequately select and sanction elected officials combined with the increasing technical demands of the job and growing recruitment problems create a bigger gap between elected and appointed officials over time.³¹ The evidence I present regarding the differential effect of selection method by the presence of a local newspaper in Table 6 supports this theory.

Figure A.2 shows a fairly large increase in the effect size on turnout several elections far after the initial switch, to about 5 percentage points. This estimator accounts for heterogeneous treatment effects but does not correct for pre-trending so should be interpreted cautiously. What is more plausible is the increase in effect size shown in the generalized

³¹<https://www.inquirer.com/politics/election/spl/pennsylvania-election-2020-officials-retiring-nightmare-20201221.html>

Figure A.2: **Average Effect of Appointed Election Officials on Voter Turnout by Length of Exposure to Appointing.** Year 0 is the even-year general election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an election official on voter turnout, at x years of exposure since first selecting the official via appointment and with state dummy covariates. The lines above and below each point represent 95-percent confidence intervals. Red points indicate pre-treatment effects, blue points indicate treatment effects. Estimates are from the Callaway and Sant'Anna (2021) estimator for dynamic two-way fixed effects designs, which corrects for bias due to heterogeneous treatment effects.



synthetic control (Figure 4 in the main analysis), which is an approximately one additional percentage point boost in turnout three elections after the switch to appointments.

Figure A.3 displays cohort treatment effects of the Callaway and Sant'Anna (2021) estimator. Although the estimates are noisy, they suggest that earlier adopters of appointed election officials have experienced stronger overall treatment effects than more recent adopters. Figure A.4 displays time period effects of switching to appointing election officials. The greater effect of earlier adopters appears to be mostly due to long-term accumulation rather than a diminishing instantaneous effect over time.

Figure A.3: **Average Effect of Appointed Election Officials on Voter Turnout by Cohort Group.** Each point is an estimate of the average group effect of appointing election officials on voter turnout for counties that switch in the given cohort year, with state dummy covariates. The lines above and below each point represent 95-percent confidence intervals. Estimates are from the Callaway and Sant'Anna (2021) estimator for dynamic two-way fixed effects designs, which corrects for bias due to heterogeneous treatment effects.

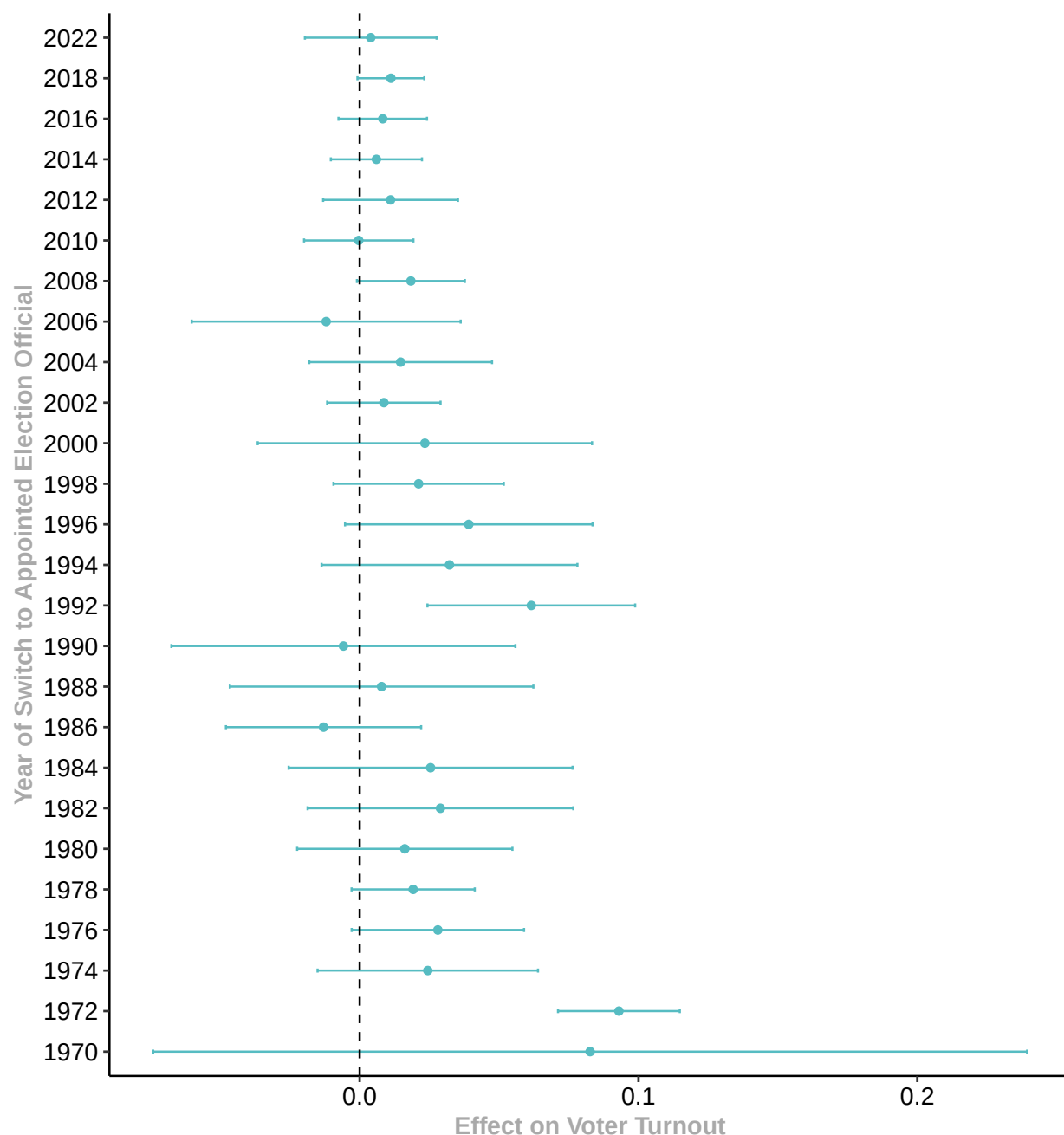
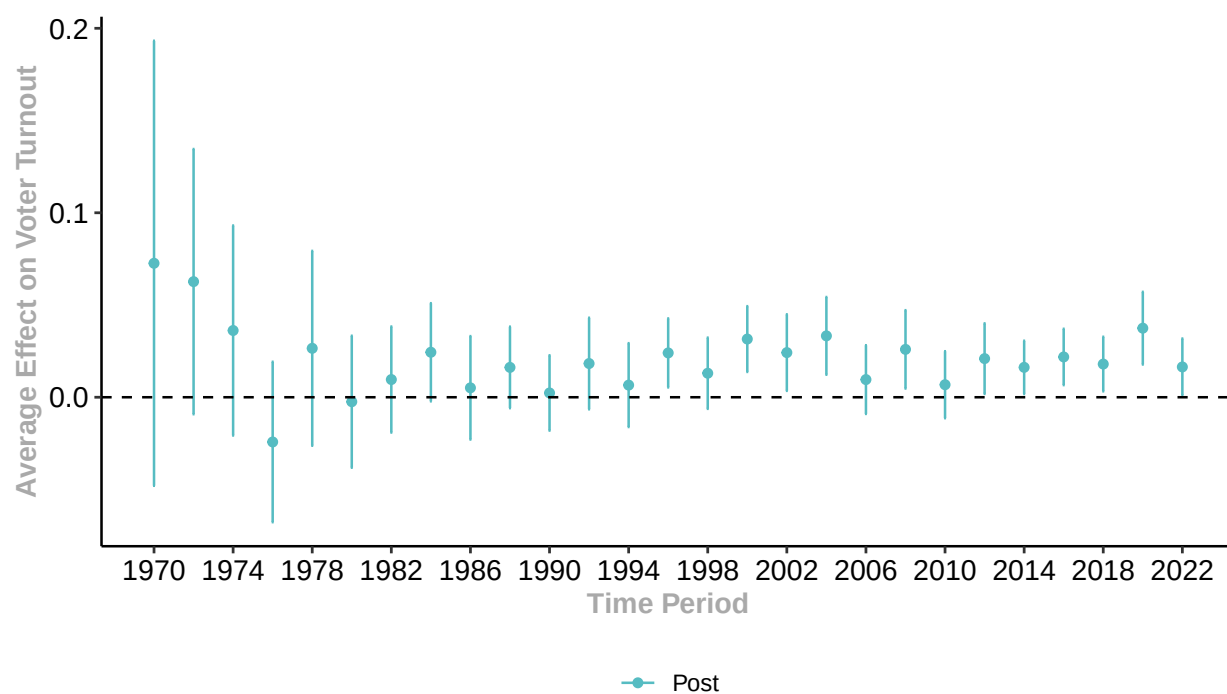


Figure A.4: **Average Effect of Appointed Election Officials on Voter Turnout by Time Period.** Each point is an estimate of the average time period effect of appointing election officials on voter turnout, with state dummy covariates. The lines above and below each point represent 95-percent confidence intervals. Estimates are from the Callaway and Sant'Anna (2021) estimator for dynamic two-way fixed effects designs, which corrects for bias due to heterogeneous treatment effects.



A.2.6 Are the Results an Artifact of the Jim Crow South?

One concern is that registration and turnout rates of African-Americans in Southern states were artificially low in the earlier periods of the dataset due to the lingering effects of racially targeted barriers to the ballot box. Even though the Voting Rights Act passed in 1965, African-American registration rates in the South continued to trail behind those of white voters until many decades later (Fraga 2018). For instance, African American and white registration rates in Louisiana did not achieve parity until 2000 (Keele, Cubbison, and White 2021). If counties that switch to appointments are more likely to have large African-American populations (Komisarchik 2018), then the inclusion of these earlier years in the dataset could confound the relationship between appointments and voter turnout.

Table A.11 displays three truncated cuts of the data: starting with the 1980 presidential election, the 1992 presidential election, and the 2000 presidential election. The main analysis displayed in Table 1 relies on turnout data beginning with the 1968 presidential election. Because registration data is only available from 1996, I focus on voter turnout here. The point estimates do grow similar when older data is discarded. However, in all specifications the point estimates are substantively large and statistically distinguishable from zero. In the most restrictive analysis, which only uses data from 2000 onwards, counties that switch to appointed clerks are estimated to boost turnout by half a percentage point.

Table A.11: **Finding that Appointing Local Election Officials Increases Citizen Participation is Robust to Alternative Year Cutoffs**

	Voter Turnout			
	(1)	(2)	(3)	(4)
Appointed	0.013 (0.003)	0.012 (0.003)	0.006 (0.002)	0.005 (0.002)
Counties	1116	1116	1116	1116
Elections	11	8	6	4
Observations	24481	18926	13365	7800
Outcome Mean	0.50	0.50	0.51	0.50
Year Cutoff	1980	1990	2000	2010
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Year cutoff indicates the first even-year general election included in the analysis.

A.3 Validation Exercises

A.3.1 Validating the Staggered Rollout Design with Alternative Estimators

Table A.12 displays results from additional estimators designed to help overcome the identification issues of the staggered adoption two-way fixed effects design. All estimators include county and year by state fixed effects. Column 1 is the same specification found in column 1 of Table 1. Column 2 excludes counties that switch from appointed to elected clerks, as they can be a source of bias. In the third specification, counties that are always “treated”—in this case, those that use appointments from the beginning of data availability—are excluded to avoid problematic comparisons in the estimation. The last two columns show the results of stacked difference-in-difference estimates (Cengiz et al. 2019). The point estimates are consistent and precisely estimated across all specifications.

I employ the (Imai, Kim, and Wang 2023) strategy of matching treated and control units. The key advantage of this procedure is that it allows me to match both on pre-treatment voter turnout trajectory and exact match on state, state by pre-treatment population, or state by Democratic vote share. I only include counties that either are elected throughout the dataset and those that start elected and switch to appointed. I do not include counties with multiple switches between elections and appointment. I match on eight elections of pre-treatment data, use the mahalanobis refinement method, and allow up to 10 control units to match with each treated unit. Table A.13 shows the results of this exercise.

The procedure produces 147 matches, leaving the estimates somewhat imprecise. However, the point estimates are in line with those found in Table 1.

Table A.12: **Finding that Appointing Local Election Officials Increases Citizen Participation Is Robust to Alternative Estimators (Even-Year General Elections, 1968-2020)**

	Voter Turnout				
	(1)	(2)	(3)	(4)	(5)
Appointed	0.018 (0.003)	0.020 (0.003)	0.020 (0.003)	0.028 (0.005)	0.024 (0.004)
Counties	1116	1085	1062	1062	853
Elections (avg)	28	28	28	28	28
Observations	31146	30366	29735	560421	41691
Outcome Mean	0.50	0.51	0.51	0.53	0.59
County FEs	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes	Yes
App to Elect Excluded	No	Yes	Yes	Yes	Yes
Always Treated Excluded	No	No	Yes	Yes	Yes
Stacked DiD	No	No	No	Yes	Yes
Shortened Event Window	No	No	No	No	Yes

Robust standard errors clustered by county in parentheses. Column 1 is identical to the specification shown in column 1 of Table 1. Column 2 excludes 28 counties that switch from appointing to electing their clerks. Column 3 additionally excludes counties that have not elected their clerk since 1966. Column 4 implements a stacked difference-in-difference regression following the procedure described by Cengiz et al. 2019. Column 5 additionally shortens the event window for each county to within 8 years before its switch and within 16 years after its switch.

Table A.13: **Finding that Appointing Local Election Officials Increases Voter Turnout Is Robust to Imai et al. 2024 Matching Estimator (Even-Year General Elections, 1968-2020)**

	Voter Turnout		
	(1)	(2)	(3)
Appointed	0.015 (0.017)	0.015 (0.017)	0.015 (0.018)
Matches	147	147	147
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Pre-treatment voter turnout matching	Yes	Yes	Yes
State exact matching	Yes	No	No
State x Dem vote share exact matching	No	Yes	No
State x Population exact matching	No	No	Yes

Bootstrapped standard errors with a degree-of-freedom adjustment in parentheses. All regressions use a pooled estimator that averages over the first 8 elections after treatment and matches over 8 elections prior to treatment. Matching is done using mahalanobis distance

A.3.2 Validating the Staggered Rollout Design with State-Specific Estimates

I run the de Chaisemartin and D’Haultfoeuille (2020) and the Callaway and Sant’Anna (2021) estimators separately for each state with at least 10 counties that have switched their election official selection method since 1960. The results are displayed in Tables A.14, A.15, A.16, and A.17. The de Chaisemartin and D’Haultfoeuille (2020) estimator employs dynamic effects with placebos. The Callaway and Sant’Anna (2021) estimator employs dynamic effects after aggregating counties into cohorts that begin treatment at the same time. This estimator is very similar to the stacked difference-in-differences estimator displayed in column 4 of Table A.12. First, always treated units are removed from the dataset (i.e., counties that have appointed their election officials since at least 1960). This eliminates a handful of counties that were extremely early adopters of appointed election administrators. Next, each county’s time period of first treatment is identified. The counties that switch from appointment to election are assigned to treatment even after their switch. Finally, those counties that are never treated (i.e., have always had elected election officials since 1960) are separated out as the “true control” by which each cohort can be compared with. Doing so avoids negative weights, thereby addressing the problems introduced by heterogeneous treatment and timing effects.

The point estimates produced by these analyses are generally in line with the main findings. All estimators for Georgia, Minnesota, and Texas return positive point estimates and are precisely estimated. The point estimates for California are slightly negative but are statistically indistinguishable from a null effect.

Table A.14: **Main Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Alternate Specifications - California**

	Voter Turnout		
	Two-Way FEs (1)	de Chaisemartin and D'Haultfoeuille (2)	Callaway and Sant'Anna (3)
Appointed	-0.008 (0.012)	-0.001 (0.002)	-0.006 (0.011)
Counties	58	58	52
Elections	28	28	28
Observations	1624	1291	1664
Outcome Mean	0.49	0.49	0.49
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses.

Table A.15: **Main Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Alternate Specifications - Georgia**

	Voter Turnout		
	Two-Way FEs (1)	de Chaisemartin and D'Haultfoeuille (2)	Callaway and Sant'Anna (3)
Appointed	0.022 (0.005)	0.006 (0.004)	0.050 (0.011)
Counties	159	159	155
Elections	28	28	28
Outcome Mean	0.40	0.40	0.40
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses.

Table A.16: Main Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Alternate Specifications - Minnesota

	Voter Turnout		
	Two-Way FEs (1)	de Chaisemartin and D'Haultfoeuille (2)	Callaway and Sant'Anna (3)
Appointed	0.027 (0.008)	0.011 (0.005)	0.066 (0.007)
Counties	87	87	86
Elections	28	28	28
Observations	2436	1704	2784
Outcome Mean	0.64	0.64	0.64
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses.

Table A.17: Main Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Alternate Specifications - Texas

	Voter Turnout		
	Two-Way FEs (1)	de Chaisemartin and D'Haultfoeuille (2)	Callaway and Sant'Anna (3)
Appointed	0.016 (0.005)	0.001 (0.005)	0.021 (0.009)
Counties	253	253	236
Elections	28	28	28
Outcome Mean	0.43	0.43	0.43
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses.

A.3.3 Testing the Parallel Trends Assumption with Event Studies Estimators

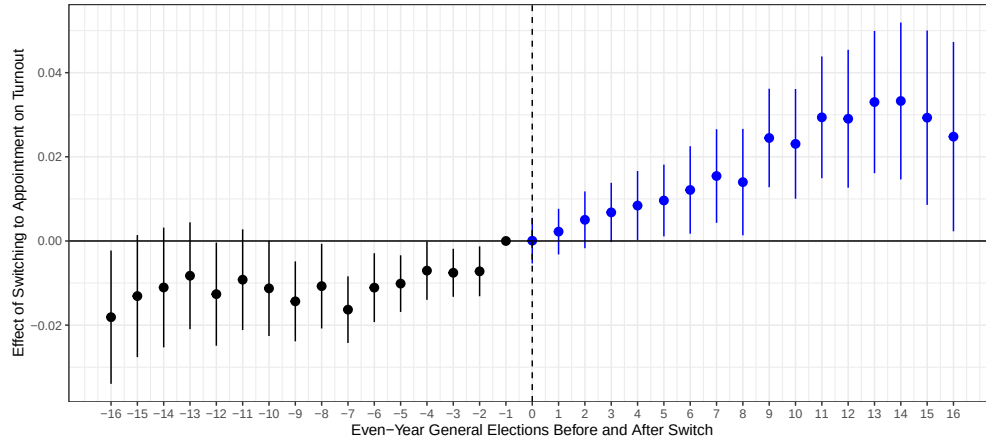
I investigate the validity of the parallel trends assumption for difference-in-difference specifications using the Dube et al. (2022) local projections event studies estimator. I make a series of pooled two-period two-group comparisons and estimate period-by-period effects, eliminating biases due to heterogeneous treatment effects. However, biases due to parallel trending remain a possibility. Figure A.5 plots the results for voter turnout. The x-axis marks the even-year general elections before and after a switch in local administration, with 0 marking the first election under an appointed clerk. Each point estimate is the difference in the change in turnout from the previous election of counties with appointed election officials rather than elected ones, at x federal elections before or after each county's actual switch. Negative coefficients in the left half of the graph suggest pre-trending. In other words, counties that switched to appointing clerks may already have been on a trajectory of higher turnout. The estimated effect becomes positive 1 even-year federal election after adoption of appointments. There also appears to be an increasing effect on turnout after counties switch their method of clerk selection, which I explore in Section A.2.5.

I examine the parallel trends assumption for the test on registration rates in A.6. There is some evidence of parallel trending, though a positive effect first appears in the first election after counties adopt appointments. Due to the more limited span of the registration data, I am unable to employ a general synthetic control design. Therefore, the main results for registration rates should be viewed with some degree of caution.

I examine the parallel trends assumption for the test on turnover rates in A.7. Point 0 shows an extremely large positive effect on turnover because in the vast majority of cases, switching to an appointed elections official forced turnover. Besides this expected aberration, I find little evidence of pre-trending.

Finally, Figure A.8 examines the validity of the parallel trends assumption for the effect of appointments on expenditures. I find no evidence of pre-trending and imprecisely estimated

Figure A.5: **Dube et al. (2022) Local Projections Difference-in-Differences Estimate of Effect of Appointing an Election Official on Voter Turnout.** Year 0 is the even-year general election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an election official on voter turnout, at x federal elections of exposure since first selecting the official via appointment. The bar lines above and below each point represent 95-percent confidence intervals. Estimates use the Dube et al. (2022) local projections difference-in-differences estimator for dynamic heterogeneous-robust difference-in-difference designs, which corrects for bias due to heterogeneity in year and county treatment effects.



but positive coefficients after a county switches to appointments. Additionally, the increase in expenditures is not instantaneous with the switch in selection method (period 0), but rather begins in the election after this switch. This is an indication that the increase in expenditures is caused by the appointed election official rather than some confounding factor causing both the selection method to change and election expenditures to increase.

Figure A.6: **Dube et al. (2022) Local Projections Difference-in-Differences Estimate of Effect of Appointing an Election Official on Registration.** Year 0 is the even-year general election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an election official on voter registration, at x federal elections of exposure since first selecting the official via appointment. The bar lines above and below each point represent 95-percent confidence intervals. Estimates use the Dube et al. (2022) local projections difference-in-differences estimator for dynamic heterogeneous-robust difference-in-difference designs, which corrects for bias due to heterogeneity in year and county treatment effects.

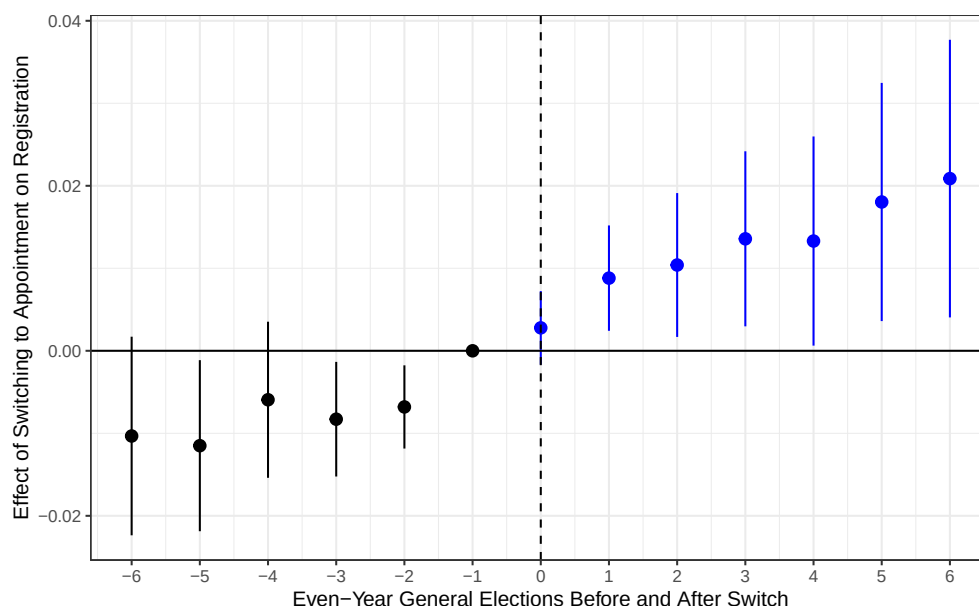


Figure A.7: **Dube et al. (2022) Local Projections Difference-in-Differences Estimate of Effect of Appointing an Election Official on Turnover.** Year 0 is the even-year general election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an election official on the election official turnover rate over a two-year period, at x federal elections of exposure since first selecting the official via appointment. The bar lines above and below each point represent 95-percent confidence intervals. Estimates use the Dube et al. (2022) local projections difference-in-differences estimator for dynamic heterogeneous-robust difference-in-difference designs, which corrects for bias due to heterogeneity in year and county treatment effects.

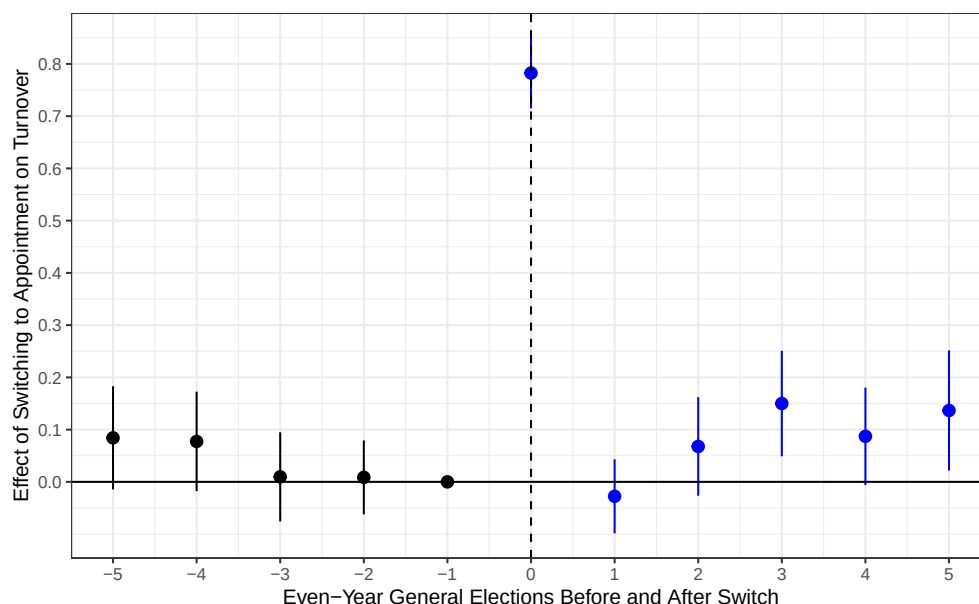
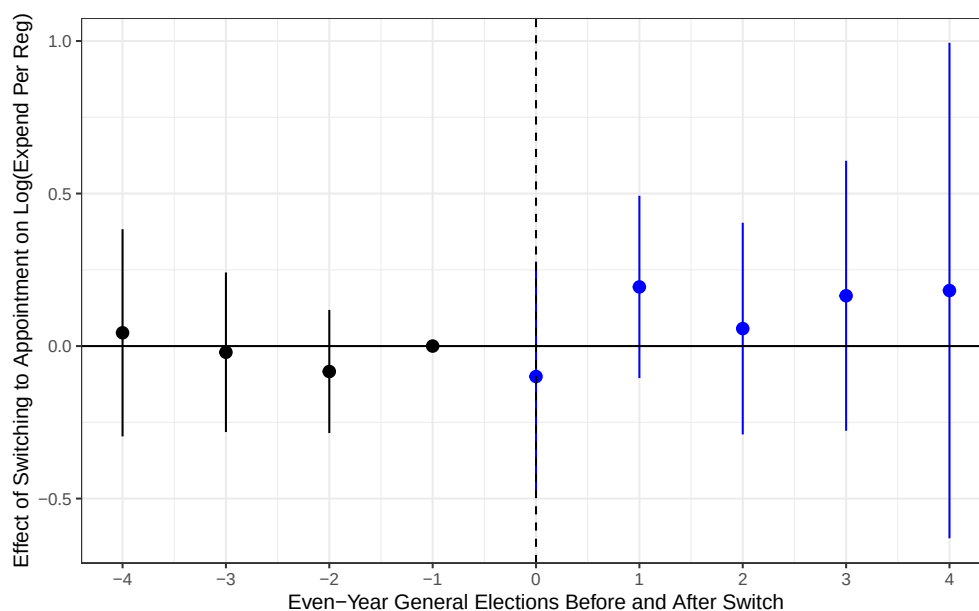


Figure A.8: **Dube et al. (2022) Local Projections Difference-in-Differences Estimate of Effect of Appointing an Election Official on Log(Expenditures Per Registrant)**. Year 0 is the even-year general election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an election official on logged election expenditures per registered voters, at x elections of exposure since first selecting the official via appointment. The bar lines above and below each point represent 95-percent confidence intervals. Estimates use the Dube et al. (2022) local projections difference-in-differences estimator for dynamic heterogeneous-robust difference-in-difference designs, which corrects for bias due to heterogeneity in year and county treatment effects.



A.3.4 Generalized Synthetic Control Regression Output

Table A.18 displays regression output from the Xu (2017) generalized synthetic control estimator, comparing treatment and control counties with similar pretreatment turnout histories. This method relies on strictly fewer assumptions than the difference-in-differences estimator and allows for a relaxation of the parallel trends assumption. The point estimate in Table A.18 is 0.8%. This is smaller and less precisely estimated than those found in Table A.5 in the main analysis, but it is still a substantively significant effect for even-year general elections.

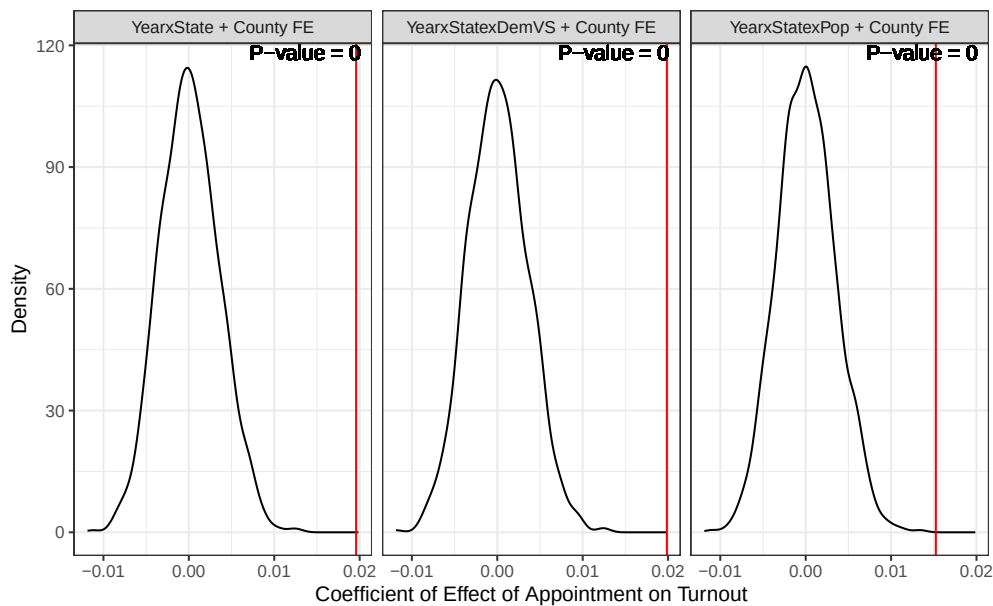
Table A.18: Main Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Generalized Synthetic Control Estimator

	Voter Turnout (1)
Appointed	0.008 (0.006)
Counties	1042
Elections	28
Observations	29176
Outcome Mean	0.50
Generalized synthetic control method matches treated and control counties on pretreatment voter turnout.	

A.3.5 Randomization Inference Additional Output

Figure A.9 shows the distribution of point estimates of the effect of appointments on voter turnout which counties switch to appointed local election officials and when they switch is randomly permuted. This procedure shows that it is extremely unlikely to observe an effect of appointments on voter turnout as large or larger than that observed by chance alone.

Figure A.9: **Randomization Inference for Table 1, Columns 1-3 - Treatment and Timing.** This graph displays the output of randomization inference for the main effects of appointed local election officials on voter turnout. Both which counties are treated and when counties are treated are randomly permuted. The black distribution shows the resulting coefficients of 1,000 iterations. The red solid vertical line is the actual coefficient observed, and the p-value is the share of coefficients that are equal to or larger than the one estimated in the respective specification in Table 1.



A.3.6 Appointing Election Officials Boosts Registration Rates More when Their Duties Specifically Include Registration

In most states, the switch from elected to appointed election officials involves both registration administration and voting administration duties. In Arizona and Georgia, the shift only impacts voting administration; registration duties are primarily carried out by separate appointed officials. It is possible that election administrators in these states impact registration rates by referring individuals to registration officials or providing a better overall voting experience. However, if appointed officials outperform their elected counterparts, we should expect to see a larger effect on registration rates when the official directly in charge of registration duties switches from elected to appointed. Table A.19 displays the results of this placebo test. The first four columns individually test registration rates for the four states with at least 10 counties that have switched between electing and appointing their local election official: California, Georgia, Minnesota, and Texas. We should observe greater effects of appointed administration on registration rates in California, Minnesota, and Texas than in Georgia. The point estimate is smallest for Georgia, although the magnitude of the effect is similar across Georgia, Minnesota, and Texas.

Column 5 pools results across states and uses a triple difference-in-differences design to test whether the effects of switching to appointed election officials on registration rates are smaller in states with separate registration systems. The effect on switching to appointed election officials on registration rates in counties where the registrar is always appointed is roughly half that found in counties where the reform switched registration duties from an elected to an appointed official. Overall, the evidence is suggestive that counties experience a larger boost to registration rates when the official directly in charge of registration duties switches from an elected to an appointed position.

Table A.19: **Appointing Election Officials Boosts Registration Rates More when Their Duties Specifically Include Registration (Even-Year General Elections, 1996-2020)**

	Registration Rate				
	(1)	(2)	(3)	(4)	(5)
Appointed	0.016 (0.012)	0.006 (0.009)	0.007 (0.005)	0.009 (0.005)	0.009 (0.004)
Appointed X Separate Reg					-0.004 (0.010)
Counties	58	159	87	253	1116
Elections	13	13	13	13	13
Observations	754	2067	1131	3289	14478
Outcome Mean	0.68	0.71	0.85	0.81	0.82
State	CA	GA	MN	TX	Pooled
County FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	No
Year x State x Sep Reg FEs	No	No	No	No	Yes
Reg Switch	Yes	No	Yes	Yes	—

Robust standard errors clustered by county in parentheses. Individual regressions are run on states with at least 10 treated counties. “Sep Reg” is short for a separate registration dummy.

A.3.7 Selection Method, Not Partisanship, Explains the Results

Georgia, Missouri, and Texas’s long histories of race-based disenfranchisement, the strong association between race and partisanship (Abramowitz and McCoy 2019; Carmines and Stimson 1989), and the present efforts of Republican politicians to increase barriers to the ballot box all contribute to the possibility that adverse policy responsiveness rather than quality differences could explain the divergence between appointed and elected election officials. I distinguish between the effects of selection method and partisanship by utilizing changes in Georgia, Montana, and Washington counties between partisan elections, nonpartisan elections, and appointments of election officials. Table A.20 displays estimates of voter turnout separating out the effects of appointments and partisan elections, with the omitted category elected nonpartisan elections. The results provide strong evidence that elections themselves, and not the partisan nature of the office, drive the main results on voter turnout. All of the estimated positive effect on turnout is observed for a switch from elected to appointed administration, whereas the effect of switching between partisan and nonpartisan administration is negative and indistinguishable from zero.

Table A.20: **Appointments, Rather than Partisanship, Drive the Effects on Voter Turnout (Even-Year General Elections, 1968-2020)**

	Voter Turnout		
	(1)	(2)	(3)
Appointed	0.015 (0.004)	0.014 (0.004)	0.011 (0.004)
Partisan Elected	-0.005 (0.005)	-0.004 (0.005)	-0.004 (0.005)
Counties	1116	1116	1116
Elections	28	28	28
Observations	31122	31122	31122
Outcome Mean	0.50	0.50	0.50
County FEs	Yes	Yes	Yes
Year x State FEs	Yes	No	No
Year x State x Dem vs FEs	No	Yes	No
Year x State x Pop FEs	No	No	Yes

Robust standard errors clustered by county in parentheses. The omitted category is selection through non-partisan elections.

A.3.8 Appointed Local Election Officials Do Not Benefit Their Principals’ Party

If the quality of selection and sanctioning of local election officials is indeed higher for those that are appointed, this leads to the possibility that appointed clerks might be selected and/or more successfully pursue strategies that benefit a certain political party over another. This would significantly alter the normative implications of the paper’s findings. Appointments could lead to better-administered elections and higher voter participation. But they could also lead to officials who try to skew election results in their party’s favor. Recent scholarship has found that Democratic and Republican clerks administer elections in similar ways and produce similar partisan outcomes and voter turnout (Ferrer, Geyn, and Thompson 2023). Shepherd et al. (2021) find no evidence that the party of appointed clerks in North Carolina shapes their decisions on polling place allocation. Here I examine whether appointed local election officials act in ways that benefit the majority party of their principals.

To test whether appointed officials benefit the party of their appointers, I examine two states where election officials are appointed by county officials who run in partisan elections: Arizona and Pennsylvania. In Arizona, the Board of Supervisors appoints the election official, whereas in most Pennsylvania counties the County Commissioners have this authority. I collect original data on the majority party of each clerk’s appointers between 2000 and 2022, using a combination of the American local government elections database (de Benedictis-Kessner et al. 2023), data from de Benedictis-Kessner and Warshaw (2020), and archival web searches. I combine this data with Democratic presidential and gubernatorial vote share from David Leip and Democratic share of registrants from administrative records in each state.

Table A.21 shows difference-in-differences regressions testing the effect of a switch to a Democratic party controlled appointing body on three outcomes: Democratic presidential 2-party vote share (columns 1-2), Democratic gubernatorial 2-party vote share (columns 3-4), and share of registrants that are Democrats (columns 5-6). All specifications include state-

by-year fixed effects to account for differential partisan trending in each state. The even columns also include county linear time trends, since counties that switch from Republican to Democratic local leadership are likely trending in a Democratic direction. This means that the outcomes are tested in excess of the underlying partisan trend specific to each county. There are concerns that including unit linear time trends in a two-way fixed effects analysis might absorb potential treatment effects, biasing the analysis downward (Wolfers 2006). However, in this dataset many counties switch back and forth between Democratic and Republican control, making this less of a concern .

Table A.21: **Appointed Local Election Officials Do Not Clearly Benefit The Party That Appoints Them (County-Level Shifts in AZ and PA, 2000-2022)**

	Dem pres vote share		Dem gov vote share		Dem reg share	
	(1)	(2)	(3)	(4)	(5)	(6)
Dem Appointer	0.023 (0.007)	-0.001 (0.003)	0.017 (0.006)	0.003 (0.007)	0.027 (0.008)	0.003 (0.003)
Counties	80	80	80	80	80	80
Elections	6	6	6	6	12	12
Observations	472	472	471	471	943	943
Outcome Mean	0.41	0.41	0.44	0.44	0.40	0.40
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes	Yes	Yes
County Time Trend	No	Yes	No	Yes	No	Yes

Robust standard errors clustered by county in parentheses. Columns 1–4 use Leip’s Atlas of U.S. elections. Columns 5-6 use an original data collection from each state’s administrative records. Dem Appointer means that the appointing authority of the local election official has a Democratic majority.

Two pictures emerge from these results depending on if the county time trend is included. Columns 1, 3, and 5 indicate that switching from a Republican-controlled appointing body to a Democratic-controlled body increases Democratic presidential vote share by 2.3 percentage points, increases Democratic gubernatorial vote share by 1.7 percentage points, and increases Democratic share of registrants by 2.7 percentage points. However, the odd columns show this to be an artifact of pre-trending: counties that start electing Democrats majorities to their county legislature or a Democrat to their county chief executive also become more

favorable to Democratic state and national candidates. The largest effect size including unit-specific linear time trends (columns 2, 4, and 6) is three-tenths of a boost in Democratic gubernatorial vote share and Democratic share of registrants, but both fall well within a 95% confidence interval. The coefficient for Democratic presidential vote share is slightly negative. In sum, the evidence is consistent with no effect of appointed election officials benefiting their principals' majority party.

A.3.9 Appointed Local Election Officials Increase Participation Similarly in Democratic and Republican Counties

If conservative-leaning voters prefer less turnout and elected officials are more beholden to the voters, switching to appointed officials could increase participation more in Republican-leaning jurisdictions. On the other hand, if appointed officials are more responsive to voters because they are better monitored and sanctioned, we might expect the opposite effect: that appointed officials increase voter turnout *less* in Republican-leaning jurisdictions. These effects could cancel each other out.

Table A.22 examines whether switching from an elected to an appointed election official leads to a larger boost in participation in jurisdictions that are more Democratic. “Democratic” is measured as being in the top half of a state’s Democratic vote shares for the 1968 presidential election, the last pre-treatment election year. The top row shows the effect of switching to appointed election officials in more Republican-leaning jurisdictions. The bottom row shows the additive effect of switching to an appointed election official in Democratic-leaning counties.

Column 1 shows that voter turnout increases by 1.6 percentage points, on average, when a Republican-leaning county switches to an appointed election official. When the county is Democratic-leaning, the effect is 1.9 percentage points. The difference in effect sizes is statistically indistinguishable from zero. When comparing differences within counties of similar size, the estimated effect of appointing election officials on turnout in more Democratic jurisdictions is only 0.1 percentage points larger than the effect in Republican-leaning jurisdictions. Columns 3 and 4 show that the effect on increased registration rates are actually smaller in Democratic-leaning jurisdictions, although again the difference is not statistically distinguishable. In summary, both Democratic- and Republican-leaning counties see similar increases in voter turnout when switching to appointed election officials.

Table A.22: **Appointing Local Election Officials Has A Similar Effect In Democratic- and Republican-Leaning Counties (Even-Year General Elections, 1968-2020)**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.016 (0.004)	0.015 (0.004)	0.013 (0.004)	0.016 (0.005)	0.015 (0.005)	0.015 (0.005)
Appointed X Democratic County	0.003 (0.006)	0.003 (0.006)	0.001 (0.006)	-0.012 (0.007)	-0.012 (0.007)	-0.012 (0.008)
Counties	1861	1861	1861	1625	1625	1625
Elections	28	28	28	13	13	13
Observations	30964	30964	30964	12138	12138	12138
Outcome Mean	0.51	0.51	0.51	0.84	0.84	0.84
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State x Dem FEs	Yes	No	No	Yes	No	No
Year x State x Dem vs FEs	No	Yes	No	No	Yes	No
Year x State x Pop x Dem FEs	No	No	Yes	No	No	Yes

Democratic counties rank in the top half in pre-treatment presidential Democratic vote share compared to other counties within the same state. Robust standard errors clustered by county in parentheses. The number of observations is smaller in columns 3-4 because Arizona and Georgia are excluded and because turnout data is available from 1968 but registration data is only available from 1996.

A.4 Mechanism Tests for Why Appointed Officials Increase Voter Participation

A.4.1 Appointed Election Officials Were More Likely To Apply For Private Grant Funding

Table A.23 tests whether appointed election officials were more likely to apply for the Center for Tech and Civic Life’s (CTCL) COVID-19 Response Grant program in September 2020.³² I obtain data on CTCL applications as well as population, income, metro, non-Hispanic white share, COVID death rate, social distancing share, and National Association of Counties membership covariate data from Lal and Thompson (2024) and follow Lal and Thompson’s specification strategy. I combine this data with 2020 election official selection methods across all counties ((Ferrer and Geyn 2023) and employ state fixed effects. In total, the data covers 37 states and over 2,600 counties.

A bivariate specification is shown in column 1, comparing the likelihood that appointed and elected counties within the same state applied for the CTCL grant. Counties with appointed election officials were 21 percentage points more likely to apply for the grant than counties that elect their election official. Column 2 controls for lagged Democratic presidential vote share to account for the fact that the grant was viewed with suspicion among some Republicans.³³ This attenuates the effect size to 16 percentage points. Column 3 adds controls for logged county population and logged county median income. Column 4 adds an indicator for urban and suburban counties and a control for the share of the county that is non-Hispanic white. Column 5 adds controls for COVID death rate and the share of respondents to the Nationscape survey who reported always complying with recommended social distancing in Fall of 2020. Column 6 adds an indicator for county membership in the National Association of Counties. The effect size is consistent at 7 percentage points across

³²<https://www.techandciviclelife.org/10-facts-about-ctcl-grants/>

³³<https://apnews.com/article/elections-facebook-mark-zuckerberg-d034c4c1f5a9fa3fb02aa9898493c708>

Table A.23: **Appointed Election Officials Were More Likely to Apply For CTCL Funding In 2020**

	Applied for CTCL Funding					
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.210 (0.031)	0.164 (0.034)	0.075 (0.034)	0.072 (0.032)	0.073 (0.032)	0.073 (0.032)
Lag dem vote share		0.532 (0.090)	0.331 (0.082)	0.450 (0.102)	0.432 (0.099)	0.431 (0.100)
Log(Population)			0.059 (0.007)	0.055 (0.007)	0.054 (0.007)	0.053 (0.007)
Log(Median income)			0.096 (0.038)	0.052 (0.043)	0.056 (0.042)	0.055 (0.042)
Metro				0.032 (0.019)	0.032 (0.019)	0.032 (0.019)
Non-Hisp white share				0.162 (0.117)	0.133 (0.123)	0.131 (0.124)
COVID death rate					-0.008 (0.021)	-0.008 (0.021)
Social distancing share					-0.023 (0.030)	-0.024 (0.030)
NACo						0.013 (0.021)
Observations	2644	2644	2644	2644	2589	2589
Outcome Mean	0.36	0.36	0.36	0.36	0.36	0.36
State FEs	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered by state in parentheses. Center for Tech and Civil Life grant and covariate data is from Lal and Thompson (2024). Population is the voting-age population. Median Income is median household income measured with the 5-year ACS ending in 2019. Metro is an indicator for urban and suburban counties based on the Census nine-value urban–rural continuum. Non-Hisp White Share is the share of residents who are classified as non-Hispanic White in the 2020 census. COVID death rate is the number of deaths per 1,000 residents prior to September 1, 2020. Social Distancing Share is the share of Nationscape respondents in the county who report always complying with recommended social distancing in the early fall of 2020. NACo is an indicator for county membership in the National Association of Counties.

columns 3 through 6. This shows that appointed officials were more likely to take advantage of this alternative source of funding than their elected counterparts in similar counties.

A.4.2 Additional Expenditures on Election Administration May Boost Voter Turnout

Table A.24 presents the results of difference-in-difference regressions testing the effects of increased election expenditures on voter turnout. The first three specifications test the overall effects of an increase in expenditures on turnout and columns 4 through 6 test the additional effect of expenditures in small jurisdictions. The point estimates can be interpreted as the percentage change to voter turnout due to a doubling of election expenditures per registered voter. Column 1 shows that a doubling of election expenditures increases voter turnout by 0.4 percentage points. The effect appears concentrated in small counties.

Table A.24: **Additional Election Expenditures Increases Voter Turnout (Even-Year General Elections, 2004-2016)**

	Voter Turnout)					
	(1)	(2)	(3)	(4)	(5)	(6)
Ln expend per reg	0.004 (0.002)	0.004 (0.002)	0.002 (0.002)	-0.001 (0.002)	-0.003 (0.003)	-0.001 (0.003)
Ln expend per reg X Small County				0.006 (0.004)	0.008 (0.005)	0.005 (0.004)
Counties	434	434	434	432	432	432
Elections	6	6	6	6	6	6
Observations	1929	1929	1929	1920	1920	1920
Outcome Mean	0.50	0.50	0.50	0.50	0.50	0.50
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	No	No	No
Year x State x Dem vs FEs	No	Yes	No	No	No	No
Year x State x Pop FEs	No	No	Yes	No	No	Yes
Year x State x Small FEs	No	No	No	Yes	No	No
Year x State x Dem vs x Small FEs	No	No	No	No	Yes	No

Ln expend per reg is the natural log of total yearly election expenditures per registered voter. Small counties rank in the bottom half in population compared to other counties within the same state. Robust standard errors clustered by county in parentheses. Data is from Mohr et al. (2018) and is available for Arizona, California, Georgia, Minnesota, Missouri, Nebraska, and Nevada. Elections are the average number of elections included for each state, rounded down to the nearest interger. Expenditure data is normalized to 2020 dollars.

A.4.3 Using EAVS Data to Examine Administrative Outcomes

Following Ferrer, Geyn, and Thompson (2023), I use the U.S. Election Commission’s Elections And Voting Surveys to examine a number of election administration outcomes. I combine all past available surveys and extensively clean the data to correct for data irregularities and errors in the raw data (Stewart 2018). Table A.25 displays the results of a two-way fixed effects regression of appointing election officials on the following county-level variables: number of polling places per 1,000 residents, provisional votes share, provisional rejection rate, absentee rejection rate, and registration removal rate. The point estimates for polling places, provisional rejection rates, and registration removal rates are all consistent with a positive effect on voter turnout, but the effect sizes are small and the coefficient for absentee rejection rate is in the opposite direction. In short, there is no strong indication that appointed officials site more polling places, run elections with fewer provisional ballots or fewer rejected absentee and provisional ballots, or remove more registrants from the voter roll.

Table A.25: **Comparison of Administrative Outcomes Between Appointed and Directly Elected Local Election Officials (Even-Year General, 2000-2022)**

	Polling Places (1)	Prov Share (2)	Prov Rejection (3)	Absentee Rejection (4)	Reg Removal (5)
Appointed	0.059 (0.065)	0.000 (0.001)	-0.024 (0.024)	0.004 (0.004)	-0.003 (0.002)
Counties	1037	1112	1013	1112	1112
Elections (avg)	6	7	7	9	9
Observations	7346	7736	6101	9809	9174
Outcome Mean	1.225	0.006	0.497	0.023	0.100
County FEs	Yes	Yes	Yes	Yes	Yes
Year x State	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Columns 1 through 5 use EAVS survey data from the US Election Assistance Commission. Column 1 measures the number of polling places per 1,000 residents, column 2 the share of votes cast provisionally, column 3 the share of provisional ballots rejected, column 4 the share of absentee ballots rejected, and column 5 the share of registrants removed from the list.

A.4.4 Appointed Election Officials May Pursue More Constituent Communication

More active election official communication strategies has been shown to increase the share of registered voters (Merivaki and Suttman-Lea 2023), improve voter confidence (Suttman-Lea and Merivaki 2023), and reduce the number of mail ballots that are rejected (Suttman-Lea and Merivaki 2022). Figure A.26 uses data provided by Thessalia Merivaki and Mara Suttman-Lea to examine whether appointed local election officials are more likely to have official social media accounts than elected officials. Appointed officials serving jurisdictions in the same state and with similar populations as elected officials are more likely to have social media accounts, although the results are imprecisely estimated. Appointed officials are twice as likely to have Twitter/X social media accounts as elected officials serving similar jurisdictions.

Table A.26: **Appointed Local Election Officials May Be More Likely To Maintain Official Social Media Accounts**

	Has social media (1)	Has FB (2)	Has X (3)	Has Insta (4)	Has Tiktok (5)
Appointed	0.007 (0.022)	0.008 (0.028)	0.037 (0.017)	0.002 (0.014)	0.002 (0.010)
Counties	13	13	13	13	13
States	13	13	13	13	13
Observations	1115	1115	1115	1115	1115
Outcome Mean	0.336	0.296	0.072	0.030	0.006
County FEs	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Election official social media data is provided by Thessalia Merivaki and Mara Suttman-Lea.

A.5 Mechanism Tests for Why Appointed Officials Produce Better Outcomes Than Elected Officials

A.5.1 Differences in the Experience, Age, and Professionalization of Appointed And Elected Local Election Officials

In the main text, I use the 2020 EVIC Survey of Local Election Officials to show that appointed clerks possess more formal education than elected clerks serving in similarly sized jurisdictions within the same state. Table A.27 shows the results of additional indicators of official quality. Column 1 tests whether appointed clerks possess greater previous experience in election administration than elected officials. I find that appointed officials actually possess 1.5 fewer years of election administration experience upon assuming their current position in the field. In column 2, I show that appointed officials are a member of marginally more professional election administration organizations than elected officials, but the difference is small and cannot be confidently distinguished from 0. Column 3 shows that appointed officials are slightly more likely than elected officials to have served as an election official in other jurisdictions. Among those who have served in elsewhere, appointed officials are much more likely to have served in multiple other jurisdictions (column 4). Appointed officials are half as likely as elected officials to be 65 years of age or older (column 5), and also make on average \$5,000 more a year than elected officials in the same state serving jurisdictions of a similar size. While this effect is statistically indistinguishable from zero, it represents an 8% salary premium. In column 6, I find that appointed officials hire an additional 0.6 FTEs, approximately 10% more than elected officials, although we cannot rule out that the finding arose by chance.

In sum, appointed officials are on average more educated and more professionalized than elected officials. However, they possess less election administration experience. This is

Table A.27: **Appointed and Elected Local Election Officials Possess Less Experience in Elections But Are More Professionalized**

	Previous Experience (1)	Professional Memberships (2)	Served Elsewhere (3)	Number Served (4)	Age >65+ (5)	Salary (6)	FTEs (7)
Appointed	-1.584 (0.745)	0.039 (0.085)	0.028 (0.043)	0.423 (0.233)	-0.152 (0.063)	5.077 (5.052)	0.565 (0.888)
States	44	44	44	28	44	44	44
Observations	587	699	664	97	584	556	669
Outcome Mean	7.40	1.17	0.15	1.71	0.16	59.74	5.87
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Log Pop	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Log Pop squared	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered by state in parentheses. Data is from the 2020 EVIC Survey of Local Election Officials and is filtered to only include chief local election officials. County is imputed from zip code to calculate population controls. Observations are weighted to be representative of the population of local election officials. Column 1 measures years of previous experience in election administration, calculated by subtracting current tenure length from total experience working in the field. Column 2 sums the number of professional memberships among the following four organizations: state association of local election officials, regional and/or local association of election officials, the Election Center (National Association of Election Officials), and the International Association of Government Officials (iGO). Column 3 measures whether clerks have served as election officials in other jurisdictions and column 4 measures the total number of other jurisdictions served in. Column 5 is a binary for whether the election official is over the age of 65 or not. Column 6 measures salary in dollars, which is derived by taking the midpoint values of salary ranges. Column 7 measures full-time equivalents, which is derived by taking the midpoint values of FTE ranges.

potentially an artifact of higher turnover rates among appointed officials, which is examined in Section 6.2.3.

A.5.2 Voter Knowledge Survey Technical Appendix

I fielded the UCLA Representation Survey, a large-scale nationwide survey conducted between April 29 and May 5, 2024 using ResearchCloud Connect. I collected responses from 3,200 participants comprising a representative sample of Americans with over-samples of Blacks, Hispanics, and Asians. The survey received approval from the UCLA IRB Review Board prior to fielding. I employ post-stratification weights of sex, region, age, education, race/ethnicity, and the interaction of race and education using census data to ensure the sample is representative of the nationwide adult population. In addition to the knowledge questions analyzed in the paper, the survey included basic demographic and political questions and three experimental components related to voters' attitudes towards local election officials.

I collected nationwide cross-sectional data on the institutional position, selection method, and name of every chief local election official. (Ferrer and Geyn 2023; Ferrer and Thompson 2024; Ferrer, Thompson, and Orey 2024) I matched participants with their current election official based on the zip code they provide earlier in the survey. For zip codes that span multiple counties, the county with the majority of the zip code's area was chosen. While it is true that approximately 20% of zip codes cross county lines, in most cases the vast majority of the zip code lies in one county. I was unable to match respondents living in jurisdictions with municipal-administered election administration because zip code was the smallest geography provided by respondents. This excluded approximately 6% of the population.

A.5.3 Local Newspaper Analysis

Table 6 in the main analysis examined the differences in the effect of appointments on citizen participation based on whether jurisdictions continuously had a local newspaper between 1968 and 2020. Table A.28 allows counties to switch in and out of having a local newspaper. The results are in line with those found in Table 6.

Table A.28: Presence of a Daily Local Newspaper Attenuates the Effect of Appointing Local Election Officials on Citizen Participation (Even-Year General Elections, 1968-2020)

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.026 (0.005)	0.024 (0.005)	0.020 (0.005)	0.011 (0.006)	0.011 (0.007)	0.006 (0.006)
Appointed X Newspaper	-0.021 (0.006)	-0.021 (0.007)	-0.014 (0.007)	0.000 (0.008)	0.001 (0.009)	0.007 (0.009)
Counties	1243	1243	1243	1011	1011	1011
Elections	14	14	14	7	7	7
Observations	15571	15571	15571	6577	6577	6577
Outcome Mean	0.57	0.57	0.57	0.85	0.85	0.85
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State x Newspaper FEs	Yes	No	No	Yes	No	No
Year x State x Dem vs x Newspaper FEs	No	Yes	No	No	Yes	No
Year x State x Pop x Newspaper FEs	No	No	Yes	No	No	Yes

Robust standard errors clustered by county in parentheses. The number of observations is smaller in columns 4-6 because Arizona and Georgia are excluded and because turnout data is available from 1968 but registration data is only available from 1996.

How Partisan Is Local Election Administration?

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In the United States, elections are often administered by directly elected local officials who run as members of a political party. Do these officials use their office to give their party an edge in elections? Using a newly collected dataset of nearly 5,900 clerk elections and a close-election regression discontinuity design, we compare counties that narrowly elect a Democratic election administrator to those that narrowly elect a Republican. We find that Democrats and Republicans serving similar counties oversee similar election results, turnout, and policies. We also find that reelection is not the primary moderating force on clerks. Instead, clerks may be more likely to agree on election policies across parties than the general public and selecting different election policies may only modestly affect outcomes. While we cannot rule out small effects that nevertheless tip close elections, our results imply that clerks are not typically and noticeably advantaging their preferred party.

INTRODUCTION

In much of the United States, elections are administered by partisan elected officials rather than nonpartisan bureaucrats. This sets the United States apart from other advanced democracies and leads many experts to worry that election officials give their party an unfair advantage. When asked whether election officials are impartial, election experts rank the United States 31 out of 34 OECD countries, ahead of only Hungary, Mexico, and Turkey (Norris and Grömping 2019). Many members of the public are also worried about the American way of conducting elections. According to an ABC News/Ipsos Poll conducted in 2021, 41% of Americans are not so confident or not at all confident in the integrity of the U.S. electoral system.¹ In the fall of 2020, Gallup reported that the share of people who were confident in the accuracy of U.S. elections matched its all-time low.² These widespread concerns about election integrity raise an important empirical question: do partisan local election officials give their party an advantage?

Political economy models of elections disagree about whether directly elected local election officials will advantage their party. Candidates improve their chances of winning by moderating their positions and may therefore run elections in a similar manner (Downs 1957). On the other hand, relatively extreme candidates are more likely to run than moderates, and this may result in distinctive Republican and Democratic ways of administering elections that tend to benefit co-partisans (Besley and Coate 1997; Osborne and Slivinski 1996). These standard models may do a poor job of describing the considerations of people running to be a local election official. For example, the set of qualified candidates may hold relatively similar views on election administration regardless of their party affiliation (Manion et al. 2021).

Sorting out how much of an advantage clerks give their party is difficult.³ Democratic clerks are more likely to serve in places where more people vote for Democrats for president, congress, and statewide office. But this does not tell us that clerks advantage their party; voters may simply prefer candidates from the same party in many offices.

We overcome this problem using a close-election regression discontinuity design, comparing Democratic presidential vote share in counties that narrowly elected a Democratic clerk to those that narrowly elected a Republican clerk. To do so, we build an original dataset of 5,880 clerk elections in 1,313 counties from 1998 to 2018. This design ensures that the differences we observe arise from who administers elections rather than pre-existing differences in citizen

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¹ <https://abcnews.go.com/Politics/americans-faith-election-integrity-drops-poll/story?id=82069876>.

² <https://news.gallup.com/poll/321665/confidence-accuracy-election-matches-record-low.aspx>.

³ We occasionally refer to local election officials as clerks. This is shorthand. In some counties, the local election official is called the election administrator or supervisor of elections. In other counties, the elections officer has additional duties unrelated to elections and their title is auditor, finance officer, probate judge, or tax assessor.

preferences or local conditions. Using election results as our primary outcome also allows us to evaluate the downstream consequences of partisan clerk elections rather than infer them from changes in policy.

Despite widespread concern that partisan election officials advantage their party, we find that Democratic and Republican election officials oversee similar election outcomes when serving comparable counties. We estimate that partisan clerks give their party an advantage of less than 0.4 percentage points. Three of our four estimators can detect an effect of 1.7 percentage points or smaller with 80% power. While our year-by-year estimates are noisier, we find that the effect on Democratic vote share is similar in every presidential election from 2004 to 2020. We also present evidence that even clerks who win in a landslide do not noticeably advantage their party and that Democratic and Republican clerks from comparable counties oversee elections with similar turnout and policies.

Why do elected clerks not advantage their party? We provide evidence that clerks do not advantage their party even when they no longer face reelection, suggesting that the reelection incentive is not the primary moderating force on clerks. Clerks who are most able to independently affect statewide outcomes also do not advantage their party, suggesting that collective action problems may not be the main reason clerks fail to advantage their party. Instead, we explain our main findings by pointing to existing research that suggests clerks are more likely than the general public to agree on election administration issues across parties and that election administration may only modestly affect electoral outcomes.

While we find that Democratic and Republican election officials oversee elections with similar outcomes, we cannot rule out small differences between Democratic and Republican officials that could determine very close elections. We also cannot rule out rare but very large effects. If a few election officials dramatically change the outcomes of elections they oversee, the effect in those counties would make up a small share of the average effect and be drowned out by the many officials who do not advantage their party. Still, we find that the average effect of replacing a Republican local election official with a Democrat is small, suggesting that most local election officials are not meaningfully biasing elections in their party's favor. Additionally, our results pertain only to county election officials in past elections. It is possible that partisan election officials at the state level or future county officials are able to bias elections in their party's favor. Finally, our analysis does not imply that electing partisan officials is the best way to select local election officials. Nonpartisan appointed officials may perform better than partisan elected officials (Ferrer 2022).

PARTISAN ADVANTAGE IN LOCAL ELECTION ADMINISTRATION

Should we expect clerks to advantage their party? Canonical theories of electoral competition reveal that

candidates whose policies more closely resemble the median voter's preferred policy are more likely to win reelection, which leads politicians from both parties to implement similar policies (Downs 1957; Fearon 1999). This reelection incentive is especially powerful for executives with meaningful discretion, like governor or mayor, who are especially likely to produce similar outcomes across parties because they make unilateral choices that directly affect their constituents' lives (Mayhew 1974). The role of clerk has many of these qualities: the elected official has considerable discretion over local election administration and citizens directly observe their performance when they vote or communicate with the office (Burden et al. 2013). However, elected partisan clerks must raise money for their campaign and win a partisan primary. These additional steps mean that candidates have to satisfy donors and primary voters who may prefer candidates that administer elections in their preferred way or even promise to tilt the scales in their party's direction (Ansolahehere, Snyder, and Stewart 2001; Brady, Han, and Pope 2007; Burden 2004). This incentive to shift policy away from the median voter's position may be especially strong in places where an overwhelming majority of citizens favor one party.

Citizen-candidate models point out that candidates with moderate policy preferences are unlikely to run if elections are costly because these potential candidates will often be nearly indifferent between the other candidates running (Besley and Coate 1997; Osborne and Slivinski 1996). Candidates with more extreme policy positions will have relatively more reason to run. This is especially true when the office confers few benefits and running is costly.⁴ Elected county clerks often receive modest pay (Adona et al. 2019), and running for office requires campaigning which many citizens might view as costly. Given these conditions, we would expect only committed partisans to run for clerk and then implement different policies across parties.

There are three potential countervailing forces within the citizen-candidate model leading clerks to not advantage their party. First, people with experience in election administration may have less polarized election policy views across parties than the public and elections may select for people with experience (Manion et al. 2021; Thompson 2020). Second, Democratic and Republican clerks may want to implement different policies, but if they were to do so they would not be able to noticeably influence turnout or partisan vote share (e.g., Gronke et al. 2008; Thompson et al. 2020). Third, clerks may face costs for changing policies that are only worth bearing if they can influence who wins. This creates a collective action problem: each clerk wants to help their party win but shirks to avoid bearing the costs because they are not pivotal by themselves. This collective action problem is not present when the costs of influencing elections are low and the likelihood of influencing election outcomes is high.

⁴ See Hall (2019) for further discussion of these models and tests of their implications in legislative elections.

Empirical research directly testing whether U.S. local election officials favor their party, which we review in Table A.1 in the Supplementary Material, is mixed. While some studies find that Democratic and Republican officials implement different policies and other studies find they do not, no study has a research design that can fully account for differences in the places that elect Democratic and Republican clerks that might lead to different policies regardless of which party controls the clerk's office.

The risk of partisan election administration is not limited to the United States. While everyone agrees that election administrators ought to ensure "free and fair elections" (Hall 2018), it is difficult to completely insulate election administration from partisan actors (James 2012). Central election management bodies are most effective when they are independent of the executive (López-Pintor 2000), but in practice partisan actors are involved in virtually every system (Massicotte, Blais, and Yoshinaka 2004). One notable example of partisan election administration comes from Ukraine, where party control of election management committees boosts that party's vote totals by a few percentage points (Herron 2020). While outright fraud is certainly a factor in many places (Alvarez, Hall, and Hyde 2008), practices that amount to a "soft perversion of the process" are even more common, such as appointing biased poll workers (Alvarez and Hall 2006) and filtering out candidates from the opposing party (Szakonyi 2022). Independent election monitors may curtail election day fraud and violence (Asunka et al. 2019), but they may simply shift fraudulent practices to earlier in the process (Daxecker 2014).

THE ROLE OF LOCAL ELECTION OFFICIALS

Across the United States, thousands of local election officials play a central role in the administration of elections. Clerk responsibilities include registering voters, maintaining an up-to-date list of registered voters, hiring and training poll workers, selecting poll locations, printing ballots, acquiring and maintaining election equipment, running early and absentee voting, educating and communicating with voters, overseeing election day, tabulating the votes cast, handling provisional ballots, and certifying election results (Kimball and Kropf 2006). They also usually have the authority to hire staff and influence department funding levels.

Clerks administer elections within the bounds of complex and frequently changing federal, state, and local laws. They work in concert with a range of other officials to successfully conduct elections. Clerks typically serve at the county level, though in 10 mostly Northeastern states important responsibilities are carried out at the municipal level.

Building on the work of Kimball and Kropf (2006), we conduct a review of state and local election laws. Table A.2 in the Supplementary Material shows a simplified division of states into tiers based on how much authority is vested in a single partisan elected official. We identify 32 states that contain at least some

jurisdictions with a partisan elected official tasked with election responsibilities. In many of these states, partisan elected officials share responsibilities with other local officials or with boards. In 21 of these 32 states, partisan elected clerks are the sole or primary election administrators. Our main analysis focuses on partisan elected officials in these 21 states.⁵

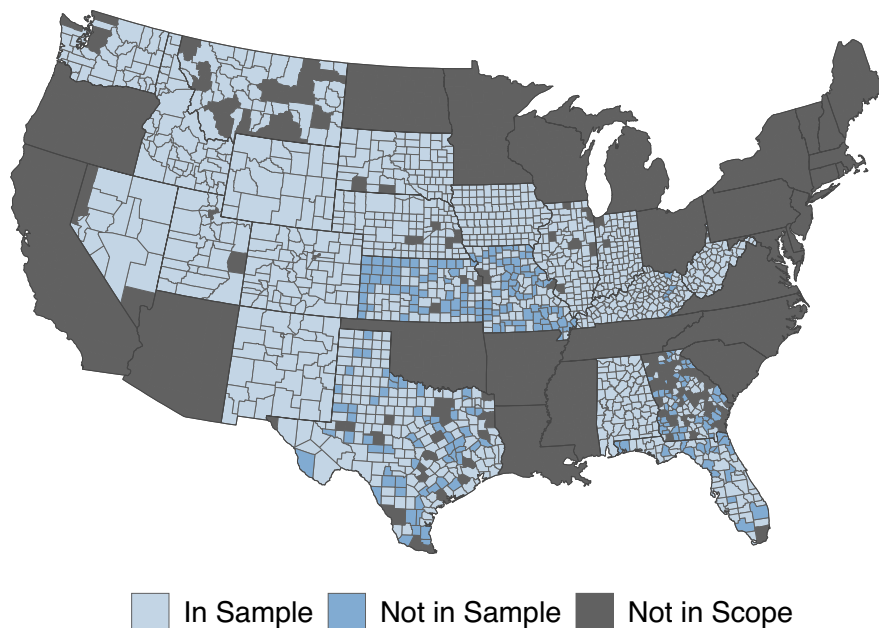
Even among states that delegate considerable election administration authority to a partisan elected official, there are significant differences in clerks' responsibilities and discretion. We describe this variation in Table A.3 in the Supplementary Material. For example, county clerks in Nevada have complete authority to register voters, maintain the registration list, site polling places, conduct early voting, and purchase voting equipment. They also have some discretion in recruiting poll workers and are not subject to any statewide training requirements. In contrast, probate judges in Alabama do not register voters or maintain registration lists. They are constrained by state law in recruiting poll workers, and both site polling places and select voting equipment in conjunction with the county commission.

Overall, most of the 21 states give registration and voting administration duties to the same partisan elected official. Most also entrust registration list maintenance and voting equipment decisions to this official. Partisan elected officials choose polling places in 14 states and administer early voting in 13 states, but are usually limited in their ability to hire poll workers, with most states requiring bipartisan appointments.

Clerks could plausibly affect election results with formal or informal practices. Using formal authority, clerks could attempt to increase participation and shift the composition of the electorate by siting many polling places in populated and accessible locations, providing extensive early voting options, ensuring that no eligible voters are removed from the voter roll, purchasing easy-to-use and reliable voting equipment, adequately resourcing polling locations with ballots and poll workers, and showing leniency in their acceptance of provisional and vote-by-mail ballots. Alternatively, officials might minimize participation and alter the composition of the electorate by siting polling places in inconvenient locations, providing limited early voting options, regularly purging voters from the rolls, maintaining old and difficult-to-use voting equipment, inadequately sourcing polling locations, and rejecting borderline provisional and vote-by-mail ballots.

Clerks might also undertake informal practices to reduce voter costs or do only what the law requires. Officials can conduct voter outreach campaigns, advertise how and where to register, maintain an active social media presence, and engage in extensive constituent communication. Alternatively, they could take none of these actions. Local election officials can engage in targeted practices by attempting to increase

⁵ In Table A.9 in the Supplementary Material, we run a robustness check using the 14 states where virtually all duties are delegated to a single partisan elected official.

FIGURE 1. Map of Counties Included in Original Data on the Elections of Partisan Local Election Officials

Note: Out of 1,582 counties that elect a partisan election official, 1,313 appear in our dataset at least once. Alaska and Hawaii do not have elected partisan election officials. “Not in Scope” indicates jurisdictions that did not elect partisan local election officials between 1998 and 2018.

participation among co-partisans and reduce participation among citizens from the opposing party. Finally, officials could take illegal actions at the risk of litigation. These include siting fewer polling places than the statutory minimum,⁶ following procedures that infringe upon the Voting Rights Act, and engaging in vote manipulation.

By estimating the effect of partisan election administrators on Democratic presidential vote share, we measure the sum total effect of all actions election officials take to influence elections.

STUDYING PARTISAN CONTROL OF LOCAL ELECTION OFFICES

In this section, we first describe our data including original data on the elections of local election officials, county-level election results and turnout for presidential and statewide offices from 2000 to 2020, and county-level administrative data on the number and location of polling places, the number of registered voters, the number of provisional ballots, and survey-reported wait times. Next, we discuss our close-election regression discontinuity design and how we improve the precision of our estimates by first predicting outcomes.

New Data on the Elections of Partisan Local Election Officials

We gather an original dataset of 5,880 elections of partisan local election officials in 1,313 counties and 21 states held between 1998 and 2018. We collect these results in three steps. First, we scrape state election websites for all county-level results. Next, we visit county election websites for results not available from states. Finally, we contact counties directly to request results not available on their websites.

Figure 1 shows the counties for which we have at least some data in light blue. Counties with partisan elected election officials where we are unable to find any election data are in dark blue. We use dark gray to denote counties where municipalities run elections, boards share responsibilities for elections, or election officials are appointed or nonpartisan. In Table A.4 in the Supplementary Material, we present descriptive statistics for the counties in and out of our sample, as well as out of scope counties. Missing counties tend to be less populous, located in the South, and have larger Black and Hispanic populations.⁷

Notably, the correlation between Democratic presidential vote share and Democratic clerk vote share is very low. Among counties in our sample that election elect local election officials on a presidential year cycle,

⁶ <https://www.theguardian.com/us-news/2020/mar/02/texas-polling-sites-closures-voting>.

⁷ Counties with fewer than one hundred residents are excluded from analysis due to data estimation limitations. This excludes Loving County, Texas.

Democratic presidential vote share correlates with lagged Democratic presidential vote share with a coefficient of 0.89. By contrast, Democratic clerk vote share correlates with same-year Democratic presidential vote share with a correlation coefficient of 0.32. Figure A.1 in the Supplementary Material captures this pattern.

County-Level Election Results and Voter Participation

We obtain county-level presidential election results for 1996 to 2020 from Dave Leip's Election Atlas.⁸ We also compile data on every regularly scheduled governor election from 1994 to 2017 and every regularly scheduled U.S. senate election from 1994 to 2020 from Leip's Atlas, as well as the number of votes counted in the race for the highest federal office on the ballot—either representative, senator, or president.⁹

We measure turnout as the share of voting age residents who cast valid ballots for the highest office. Voting age population is measured using estimates from the National Cancer Institutes's Surveillance, Epidemiology, and End Results Program.¹⁰

County-Level Data on Election Administration

We assemble a set of indicators on how elections have been run over time and across counties using the Election Administration and Voting Survey (EAVS) from the U.S. Election Assistance Commission.¹¹ We use this survey to measure the following for each federal general election in every county: the number of polling places, provisional ballots cast, provisional ballots rejected, absentee ballots rejected, and the number of registrants removed from the voter roll. We use Dave Leip's Election Atlas to measure the number of registered voters in each county and the share of registered voters listed as members of the Democratic party.

Additionally, we follow Pettigrew (2017) in using the Cooperative Congressional Election Study to measure voter wait times.¹² We compute the share of voters who had to wait at the polls for more than 30 minutes for each federal general election between 2006 and 2018, except for 2010 when the CCES did not ask about wait times. We also use data from Chen et al. (2022) who measure wait times by tracking cell phone locations.

⁸ <https://uselectionatlas.org/>.

⁹ Due to irregularities in the number of ballots cast in some counties, we use the number of votes in the race for the highest federal office as our measure of turnout.

¹⁰ Note that some voting-age residents may be ineligible to vote due to citizenship status or criminal record. These data do not allow us to remove these individuals. While this may make some of our estimates slightly noisier, it should not bias our estimates since it is highly unlikely anyone would decide where to live based solely on the outcome of close elections for the local election official. The data we use are available at <https://seer.cancer.gov/popdata/>.

¹¹ <https://www.eac.gov/research-and-data/datasets-codebooks-and-surveys>.

¹² <https://cces.gov.harvard.edu/data>.

Empirical Strategy: Regression Discontinuity Design

We estimate the advantage election officials give their co-partisans using a regression discontinuity design, fitting regression equations of the form:

$$Y_{ct+k} = \mu + \tau Dem_{ct} + f(M_{ct}) + \epsilon_{ct+k},$$

where Y_{ct+k} is Democratic presidential vote share in elections held k years after the election official was elected in county c , year t . Dem_{ct} is a dummy variable indicating a Democratic local election official winning the election. $f(M_{ct})$ is a flexible function of the margin M_{ct} by which the Democratic local election official won (i.e., the share of the two-party vote they received minus 0.5). M_{ct} ranges from -0.5 to 0.5 and is positive for a Democratic win, negative for a Republican win, and zero in an exact tie. We interpret τ as the average effect of electing a Democratic rather than Republican local election official in counties where the election was an exact tie. In other words, it is the effect of electing the next most likely or marginal Democrat to be a local election official rather than a Republican.

In our turnout and policy analyses, when each clerk election determines control of the office for multiple observations of the outcome, we cluster standard errors by clerk election (Abadie et al. 2017).

Our close-election regression discontinuity design ensures that, when we compare counties that elect a Republican to those that elect a Democrat, both sets of counties have a similar average partisan makeup, state political environment, preferences over election administration, and population, in addition to any other fixed and time-varying county factors. Our regressions identify the average effect of electing a Democratic rather than Republican election official in places with tied elections when the only thing that changes sharply at that point is which candidate was elected (Cattaneo, Idrobo, and Titiunik 2019; Imbens and Lemieux 2008; Lee and Lemieux 2010).¹³ We evaluate the plausibility of this assumption by comparing pre-election county-level characteristics in counties that narrowly elected Democratic officials to those that narrowly elected Republicans. We are most interested in the comparison of turnout and Democratic presidential vote share from before the local election official was elected because these are our primary outcomes of interest, and because they tend to correlate highly within a county over time. In Section A.6 in the Supplementary Material, we show that counties where a Democratic election official narrowly won are similar to counties where a Republican narrowly won on a large number of pre-treatment characteristics, including the lagged Democratic presidential vote share and lagged turnout. In Section A.6.2 in the Supplementary Material, we also show that Democrats and Republicans win close races at similar rates in counties

¹³ While this assumption has been disputed in a small number of particular cases (Caughey and Sekhon 2011), it holds under the majority of cases studied (Eggers et al. 2015).

controlled by Democrats at the time of the election and those controlled by Republicans.¹⁴ These results serve as evidence to support our claim that the only difference between a district that narrowly elects a Democrat and a district that narrowly elects a Republican is the partisanship of the elected clerk.

Our intention is to estimate the effect of replacing a marginal Republican with a marginal Democrat, which is identified under the assumptions we mention above. Our design does not identify the effect of a candidate changing the party they associate with or the effect of replacing a typical Republican with a typical Democrat (Hall 2019, Chapter 2; Marshall 2021).

We present results using a variety of regression specifications because of the bias-variance tradeoff that must be resolved in every regression discontinuity analysis. If the functional form of the running variable is not flexible enough, it can induce bias, mistaking a smooth curve in the outcome for a discontinuity. On the other hand, less flexible specifications that use more data and fewer degrees of freedom make the estimate more precise. Presenting multiple specifications ensures the robustness of our results across different functional forms of the relationship between Democratic election official vote share and our outcomes. Following Cattaneo, Idrobo, and Titiunik (2019), our primary specification is a local linear regression using triangular kernel weights and the automated bandwidth selection procedure described in Calonico, Cattaneo, and Titiunik (2014).

Improving Precision by First Predicting Outcomes

One of the main challenges we face when estimating the advantage clerks give their party is statistical precision. Estimating discontinuities is difficult—across many applications, the common estimators produce large standard errors and do not have sufficient power to detect substantively interesting effects (Stommes, Aro-now, and Sävje 2021).

We improve the precision of our estimates using a three-step procedure building on the recommendations of Lee and Lemieux (2010).¹⁵

1. Using leave-one-out cross-validation, we select a regression specification that best predicts Democratic presidential vote share from lagged Democratic presidential vote share.¹⁶ We use the full dataset for this exercise, not just the counties with competitive elections for their local election official. This procedure selects a prediction equation with state-year-specific coefficients on the lag and state-year-specific intercepts.

2. We compute the difference between predicted and observed Democratic vote share using the best-performing specification.
3. We use the residual from step 2 as the outcome in a standard regression discontinuity estimator.¹⁷

We use this procedure to improve our power for our main findings and for studying voter turnout and election policies.

We conduct power analyses to evaluate whether this more precise estimator is powerful enough to detect substantively meaningful effects. We report the minimum effect detectable 80% of the time with a one-sided *t*-test at a 5% significance level (i.e., $\alpha = 0.05$ and $\beta = 0.20$). We discuss our approach to calculating power in Section A.5 in the Supplementary Material.

As we report in Table 1, our main estimators have a minimum detectable effect of Democratic election officials on Democratic presidential vote share of between 1.2 percentage points and 2.3 percentage points. That means our design has sufficient power to detect effects on partisan vote share that are about as large as running 50 television ads (Sides, Vavreck, and Warshaw 2021; Spenkuch and Toniatti 2018) or 15% as large as the effect of nominating a moderate candidate (Hall 2015). Our minimum detectable effect is also approximately half the size of the effect of Democratic local election officials on the Democratic share of turnout reported in previous research (Bassi, Morton, and Trounstein 2009). In Table 2, we report that our estimators have minimum detectable effects of Democratic election officials on turnout of between 1.0 percentage points and 1.1 percentage points. Our minimum detectable effect on turnout is less than half the size of a large TV advertising campaign in a presidential election (Green and Vavreck 2008).

CLERKS DO NOT MEANINGFULLY ADVANTAGE THEIR PARTY

Descriptive Graphical Evidence Suggests Clerks Do Not Advantage Their Party

First, we show descriptive graphical evidence that presidential candidates from the clerk's party perform no better than expected based on historical election results. Figure 2 captures this result. In the top panel, we plot the regression of Democratic presidential vote share for each county-year on Democratic vote share in the previous presidential election. Counties with a Democratic clerk are colored blue and counties with a Republican clerk are colored red. We fit separate locally weighted regressions for counties with Democratic and Republican clerks.

Counties that vote overwhelmingly for Democratic presidents are also likely to elect Democrats to run

¹⁴ This is a version of the standard McCrary (2008) sorting test.

¹⁵ For a more recent discussion of this estimator, see Noack, Olma, and Rothe (2021). We discuss how this estimator compares with the estimator in Calonico et al. (2019) in Section A.4 in the Supplementary Material.

¹⁶ We discuss the candidate prediction equations and their performance in Section A.4 in the Supplementary Material.

¹⁷ See Lee and Lemieux (2010) for further discussion of why it is not necessary to residualize the running variable.

their elections. We can see this by noticing that the upper-right quadrant of the plot is made up almost entirely of blue Ds and the bottom-left portion of the plot is primarily composed of red Rs.

Nevertheless, this plot suggests that local election officials are not giving their party a large electoral advantage. We can see this by noticing that the lines are nearly identical. Conditional on being elected in counties with similar historical Democratic vote shares, Democratic and Republican local election officials oversee similar elections. If clerks were advantaging their party and continuing to seek new advantages every cycle, we would expect the blue line to be higher than the red line. That is, Democratic presidential candidates would perform better in counties with Democratic clerks than with Republican clerks after accounting for the normal two-party presidential vote in that county. This figure provides us little reason to suspect that clerks are giving their party a substantial advantage in presidential elections.

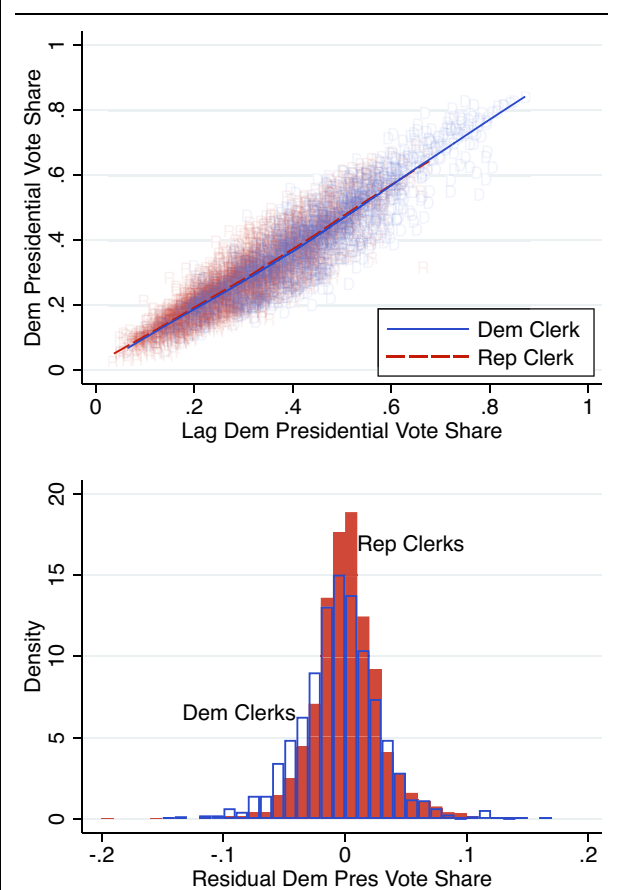
The bottom panel of Figure 2 plots histograms of the residual of predicted Democratic presidential vote share for counties with Democratic and Republican clerks.¹⁸ The histograms overlap substantially, although the histogram for Democrats is shifted slightly to the left and has a modestly wider dispersion.¹⁹ If clerks were advantaging their party and continuing to seek new advantages each cycle, we would expect the central tendency of the distribution of blue residuals to be shifted to the right of the central tendency of the red residuals indicating that Democratic presidential candidates perform better in counties with Democratic clerks than with Republican clerks after accounting for the expected presidential vote in that county. This implies that Democratic clerks oversee elections that are getting worse, on average, for Democratic presidential candidates.

One important weakness of these plots is that the party of the clerk is often the same in the previous presidential election. If partisan control of the clerk's office is constant over time and not increasing as the party holds the clerk's office, this plot would tend to understate the effect partisan control of the clerk's office on election results. We address this concern in the next section by using a regression discontinuity design which compares places with Democratic and Republican clerks that had an equal likelihood of having a Democratic clerk during the previous presidential election.

Regression Discontinuity Plot Suggests Clerks Do Not Advantage Their Party

Figure 3 captures our main result: local election officials do not improve their party's vote share in presidential

FIGURE 2. Democratic and Republican Election Officials Conduct Elections with Similar Results

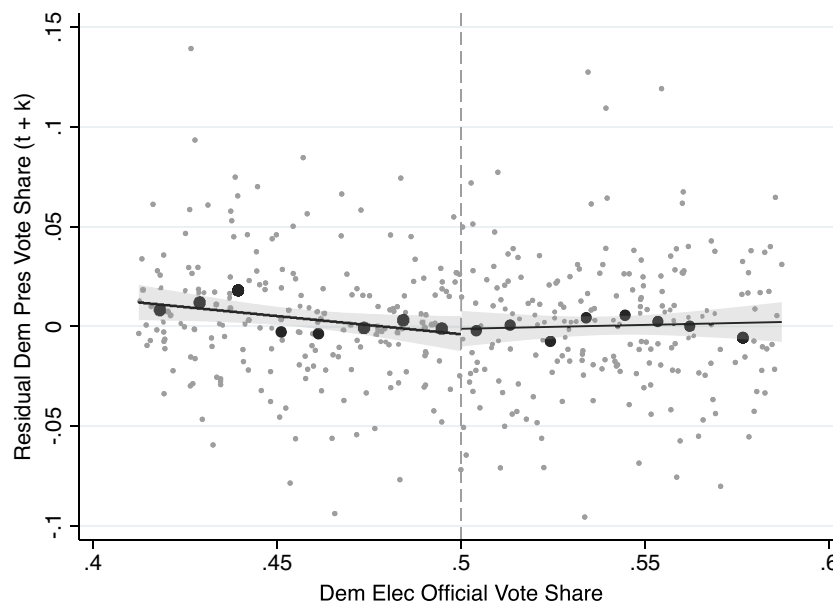


Note: The top panel presents the relationship between Democratic presidential vote share and lagged Democratic presidential vote share separately in counties with Democratic and Republican clerks. The relationship is nearly identical in both sets of counties. The bottom panel presents the distribution of the residuals from predictions of Democratic presidential vote share in counties with Democratic and Republican election officials. On average, Democratic clerks oversee elections that are slightly less favorable for Democratic presidents than expected.

elections. On the horizontal axis, we plot the two-party Democratic vote share in the race for local election official. We subset to elections with a Democratic and Republican candidate both on the ballot and finishing in the top two places. This means that a Democratic official runs elections to the right of 0.5, and a Republican official runs elections to the left of 0.5. On the vertical axis is the residual of Democratic presidential vote share in each county in the first presidential election after the election official was elected. Each of the small gray points represents the election of a county election official and the subsequent presidential election result. The large black points are equal-sized binned averages made up of 25 elections each, computed separately for counties that elect a Democratic clerk and those that elect a Republican. The solid lines are simple linear regression lines fit separately for

¹⁸ See our Empirical Strategy section for a discussion of how we compute the residuals.

¹⁹ The average of the residuals is 0.002 in Republican-controlled counties and -0.004 in Democratic-controlled counties. The standard deviation of the residuals is 0.028 in Republican-controlled counties and 0.034 in Democratic-controlled counties.

FIGURE 3. Electing a Democratic Election Official Rather than a Republican Does Not Noticeably Increase Democratic Presidential Vote Share

Note: Two-party Democratic vote share for contested local election official elections is the running variable, making 0.5 the threshold above which a county elects a Democratic election official and below which they elect a Republican. Democratic presidential vote share in the following presidential election is plotted along the vertical axis. The large black points are equal-sized binned averages marking the average of 25 elections each. The binned averages are computed separately for each side of the 50–50 threshold. The black line is a linear regression fit separately on each side of the 50–50 threshold. The full tabular results are found in column 1 of [Table 1](#).

counties that elect Democratic election officials and those that elect Republicans. We plot data within the bandwidth selected by the automated procedure described in Calonico, Cattaneo, and Titiunik (2014).

We can learn about the effect of electing a Democrat rather than a Republican as local election official by focusing on the 50–50 point in the middle of the plot. To the left and right of 0.5, the average residual Democratic presidential vote share is nearly identical. If clerks were advantaging their party, we would expect the average vote share for Democratic presidential candidates to be higher in counties that narrowly elected a Democratic clerk compared to those that narrowly elected a Republican clerk. This would be visible as a vertical jump in the regression line on the plot with the line being noticeably higher on the right side of the 50–50 line than on the left side of the 50–50 line. This suggests that election officials do not noticeably advantage their party.

Regression Estimates also Suggest Clerks Do Not Advantage Their Party

In [Table 1](#), we provide formal estimates of the effect of electing a Democrat rather than a Republican as election official on Democratic presidential vote share. Column 1 reports the estimate from a local linear regression with uniform kernel weights and the bandwidth selected by the procedure described in Calonico, Cattaneo, and Titiunik (2014). Column 2 reports

estimates from the same procedure used in column 1 but with a bandwidth twice as wide. Column 3 reports estimates from the same procedure used in column 1 but with a bandwidth half as wide. Column 4, our primary specification, reports estimates from a local linear regression with triangular kernel weights and the bandwidth selected by the procedure described in Calonico, Cattaneo, and Titiunik (2014).

We find consistent evidence across all four specifications that local election officials do not meaningfully advantage their party's candidate for president. The point estimates range from –1.1 to 0.3 percentage points, with three out of four point estimates falling below 0.1 percentage points. Across all four columns, our 95% confidence intervals include zero.

In the final row of [Table 1](#), we present the minimum detectable effect. As we discuss in our Empirical Strategy section, three of our four estimators are able to detect partisan advantages as small as 1.7 percentage points with 80% power.

While [Table 1](#) presents results across only four specifications, we estimate very similar effects across a much wider set of potential estimators. Section A.6.4 in the Supplementary Material shows that our estimates are similar for every choice of bandwidth from 0.02 to 0.25. In Section A.6.3 in the Supplementary Material, we demonstrate that, though our estimates are noisier when using outcomes that are not first residualized, they are substantively similar.

TABLE 1. Effect of Democratic Election Officials on Democratic Presidential Vote Share

	Dem. pres. vote share			
	1	2	3	4
Dem. elec. official	0.003 (0.006)	0.000 (0.005)	-0.011 (0.009)	0.000 (0.007)
N	383	696	195	408
Bandwidth	0.08	0.16	0.04	0.09
Bandwidth selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular
Min. detectable effect	0.016	0.012	0.023	0.017

Note: Robust standard errors in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the maximum clerk win margin allowed for inclusion in each specification. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure. Min. detectable effect refers to the minimum effect that a one-sided test with a 0.05 alpha would have 80% power to detect.

In Table A.14 in the Supplementary Material, we extend our data to include all governor, senate, and presidential election results. Despite adding more data, predicting governor and senate election results based on lagged results is more difficult than predicting presidential results, resulting in noisier estimates. Nevertheless, the point estimates are still substantively quite small, and a zero effect falls well within all of the 95% confidence intervals in the table.

Similar Findings across Time and States

This finding—that election officials do not noticeably advantage their party—is not limited to the early part of our study period, to states where officials have slightly less authority, or to regions with distinctive politics. In Figure 4, we present estimates of the effect of electing a Democratic local election official on Democratic presidential vote share in every presidential election since 2004. Despite the concern that election administration has become an increasingly salient and partisan issue, we do not find evidence that the marginal local election official advantaged their party in 2020 or in any previous election since 2004.

In the Supplementary Material, we also study three sets of states where we might expect clerks to give their party a larger advantage. Across all three sets, we find that clerks give their party little to no advantage. First, in Table A.9 in the Supplementary Material, we present estimates of the advantage clerks give their party in the 14 states where one partisan elected official handles all local election administration. Three of the four reported point estimates of partisan advantage are negative. Given the long tenure of clerks and the slow pace of the Southern realignment in local offices, we might expect that Democratic clerks in the South may favor the Republican party in statewide and national elections, especially in the first few elections in our data (Kimball et al. 2013). In Table A.11 in the Supplementary Material, we report estimates of the partisan advantage clerks provide, removing counties in Southern states from the analysis. We find

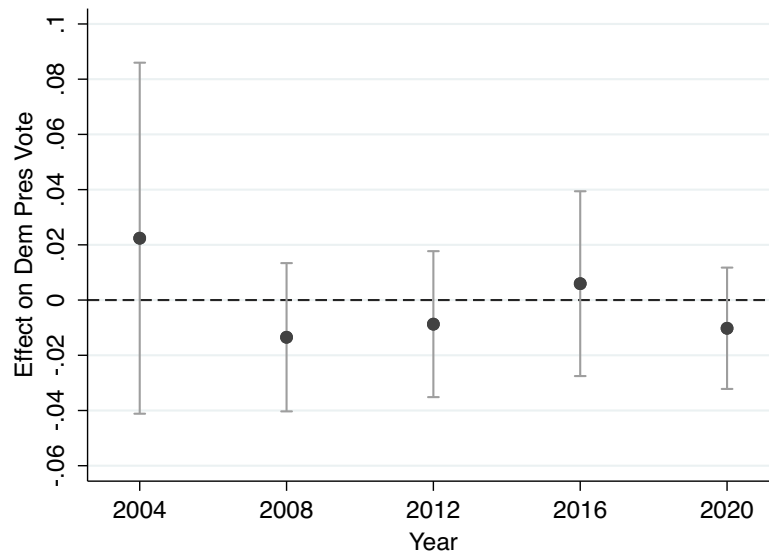
substantively similar point estimates, implying that our national estimates are not masking positive effects in places where clerks are most likely to favor national co-partisans. Finally, some counties in our data were subject to pre-clearance requirements under the Voting Rights Act prior to the 2013 Supreme Court ruling in *Shelby County v. Holder*. In Table A.12 in the Supplementary Material, we find that, even when omitting counties subject to the pre-clearance requirement, clerks do not appear to advantage their party. In Table A.13 in the Supplementary Material, we subset to counties previously covered under the pre-clearance provisions but in years after the *Shelby County v. Holder* decision, finding a similar pattern of results. In other words, there is no indication that local election officials have used their new discretion post-*Shelby* to advantage their party.²⁰ In addition to these more powerful tests, in Figure A.4 in the Supplementary Material, we also present evidence that clerks do not noticeably advantage their party in any of the eight states that we have sufficient data to study. This suggests that state-level laws are not the primary reason clerks do not advantage their party. Put together, these results suggest that clerks do not meaningfully advantage their party.

Generalizing beyond Close Clerk Elections

Using a regression discontinuity design, we find that clerks elected in close elections do not give their party a substantial advantage in presidential elections. Might clerks elected by wider margins give their party an advantage?

Our data suggest that, even when clerks win by a relatively large margin, they do not grant their party a sizable advantage. In Figure 2, we document the difference in Democratic presidential vote share between counties controlled by Democratic and Republican clerks. Though the majority of these clerks are elected

²⁰ This is in line with Komisarchik and White (2021).

FIGURE 4. Clerks Provide Their Party Minimal Advantages Over Time

Note: Each dot represents a regression discontinuity-based estimate of the effect of electing a Democratic clerk on residual Democratic presidential vote share in a given presidential election. Vertical lines extending from each point represent 95% confidence intervals. Estimates come from regressions that mimic column 4 of Table 1 using local linear regression with triangular kernel weights. Full tabular results are found in Table A.8 in the Supplementary Material.

by large margins or in uncontested races, the average Democratic clerk oversees an election with slightly lower Democratic presidential vote share than the average Republican clerk. This descriptive evidence suggests that our finding is not limited to counties with close clerk elections. In Section A.6.11 in the Supplementary Material, we present a more formal analysis of how local our estimates are drawing on the approach described in Angrist and Rokkanen (2015) and Hainmueller, Hall, and Snyder (2015). We find that, even including counties where the Democratic clerk candidate won as little as 25% or as much as 75% of the vote, partisan clerks do not appear to advantage their party on average.

Given this evidence, in our Mechanisms section, we consider explanations for clerks not advantaging their party that apply to all clerks rather than just those elected by very small margins.

Democratic and Republican Clerks Produce Similar Turnout and Policies

While conventional wisdom holds that high-turnout elections favor Democrats (Lijphart 1997; Piven and Cloward 1988), some reforms that increase turnout do not noticeably increase Democratic vote share (see, e.g., Thompson et al. 2020). Might local election officials successfully affect turnout but fail to offer their party an advantage?

Table 2 presents regression discontinuity estimates of the effect of electing a Democrat rather than Republican election official on turnout. The first two columns mirror columns 1 and 4 from Table 1. Across both

specifications, we find that, after accounting for differences in where and when Democrats and Republicans run for office, members of both parties oversee similar levels of voter participation on average.

In the final row, we report the minimum detectable effect using each estimator. Both estimators can detect an effect as small as 1.1 percentage points with 80% power or greater. Even with these high-powered tests, we find no evidence that electing a Democratic rather than a Republican election official increases turnout on average.

While Democrats are often expected to pursue policies that increase turnout, vote-maximizing partisans will only work to increase participation when their party makes up a majority of the people affected by their policies (Burden et al. 2013; Kimball, Kropf, and Battles 2006). Might Democratic clerks oversee lower turnout in Republican-majority counties and higher turnout in Democratic-majority counties?

Table 2 presents evidence that Democratic and Republican officials do not strategically increase turnout when their party makes up a majority and decrease turnout when their party is in the minority. Columns 3 and 4 report the effect of electing a Democratic clerk in Republican-majority counties. There, marginal voters are more likely to be Republicans, so we would expect vote-maximizing Democratic clerks to decrease turnout relative to Republican clerks. Instead, we find that Democratic and Republican clerks oversee similar turnout rates in these counties. Columns 5 and 6 report the effect of electing a Democratic clerk in Democratic-majority districts, where Democrats are most likely to make up a majority of marginal voters. Still, we find

TABLE 2. Effect of Democratic Election Officials on Turnout

	Votes per voting-age resident					
	All counties		Rep. counties		Dem. counties	
	1	2	3	4	5	6
Dem. elec. official	0.001 (0.004)	0.001 (0.004)	0.001 (0.005)	0.002 (0.004)	0.002 (0.010)	0.003 (0.009)
<i>N</i>	541	720	400	584	168	188
Clusters	313	418	237	344	106	120
Bandwidth	0.06	0.09	0.07	0.10	0.07	0.08
BW selection	CCT	CCT	CCT	CCT	CCT	CCT
Kernel	Unif.	Tri.	Unif.	Tri.	Unif.	Tri.
Min. detectable effect	0.011	0.010	0.012	0.011	0.025	0.023

Robust standard errors clustered by clerk election in parentheses. Rep. counties are those where the last Republican presidential candidate received more votes than the last Democratic presidential candidate. Dem. counties are all remaining counties. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the maximum clerk win margin allowed for inclusion in each specification. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure. Min. detectable effect refers to the minimum effect that a one-sided test with a 0.05 alpha would have 80% power to detect. Unif. refers to a uniform kernel. Tri. refers to a triangular kernel.

that Democratic and Republican clerks oversee similar levels of participation.

These results could arise if partisan clerks implement different policies that have very modest effects on turnout. Committed partisan clerks could pursue these policies anyway if they are unaware of their ineffectiveness or if they have ideological positions about how elections ought to be administered. In Section A.6.12 in the Supplementary Material, we present evidence that Democratic and Republican clerks representing comparable places make similar administrative decisions across many parts of the job, including the number of polling places sited per voting-age resident, the share of votes cast provisionally, the provisional ballot rejection rate, the registration rate, the registration removal rate, the partisan balance of registrants, and voter wait times.

Put together, the analyses presented in Table 2 and Section A.6.12 in the Supplementary Material cast doubt on the claim that partisan clerks are strategically changing turnout or policies while failing to convert those changes into noticeable advantages in election results. Instead, partisan clerks oversee similar turnout and policies even when it is in their party's interest for them to increase or decrease turnout.

WHY DON'T CLERKS ADVANTAGE THEIR PARTY?

Why do elected clerks not advantage their party? Drawing on our discussion in the Theory section, we explore four explanations. The first explanation we explore is that clerks are elected officials and want to win reelection, so clerks from both parties work to satisfy the median voter in their county and produce similar policies and outcomes. The next three explanations are countervailing forces within the citizen-

candidate framework that could lead clerks to not advantage their party: (1) qualified candidates hold similar views across parties, (2) administration has modest effects on turnout and outcomes, and (3) clerks face a collective action problem because elections are decided jointly by many counties. No single piece of evidence we present conclusively answers why clerks do not advantage their party, but we provide suggestive evidence against the reelection incentive and collective action problem as meaningful constraints and discuss existing research that favors preference convergence and the limited ability of clerks to influence electoral outcomes as explanations.

Reelection Incentives Do Not Noticeably Affect Partisan Advantage Clerks Provide

Might Democratic and Republican clerks oversee similar election outcomes because they are competing for the support of the median voter in their next election? This is the prediction of one class of standard political economy models of elections (Downs 1957; Fearon 1999). We study this question using election official term limits. Clerks in Indiana are allowed to serve for no more than two consecutive 4-year terms in a 12-year period.²¹ If the threat of being thrown out of office is the main constraint on clerks advantaging their party, clerks should advantage their party more in their second term than their first term, since the reelection incentive is removed entirely. To test this prediction, we compare the change in Democratic presidential vote share from the first term to the second term of Democratic clerks to the same change for Republican clerks.

²¹ The effect of lifetime term limits is larger than consecutive term limits in state legislatures, but consecutive limits still substantially reduce the reelection incentive (Fourmaies and Hall 2022)

TABLE 3. Estimates of Increase in Partisan Advantage Provided by Term-Limited Clerks

	Change in Dem. pres. vote share		
	1	2	3
Dem. elec. official	−0.008 (0.017)	−0.005 (0.012)	−0.006 (0.014)
Counties	66	66	66
N	75	75	75
Year FE	No	Yes	Yes
Lag Dem. vote share	No	No	Yes

Note: Robust standard errors clustered by county in parentheses. The data are limited to term-limited, incumbent clerks in Indiana. The outcome is the change in Democratic presidential vote share from the first term to the second term of the term-limited clerk.

Table 3 presents our estimates. In the first column, we present the simple difference in means between Democratic and Republican clerks in how much more of their county's presidential vote goes to the Democratic candidate in their second term than their first term. The second column presents regression estimates with year fixed effects to account for state-wide changes in support for Democratic presidential candidates across years in our data. The third column presents regression estimates with lagged Democratic presidential vote share in addition to year fixed effects to account for any polarization across counties in voting trends over the years.

Across all three regression specifications, we find that clerks do not give their party a bigger advantage when they are ineligible for reelection. While this simple analysis does not fully account for differences in trends in presidential vote across counties unrelated to the party of the clerk, which our regression discontinuity estimates do account for, we take this as suggesting that reelection incentives are not a key constraint limiting the advantage clerks give their party.

This result suggests that concerns about reelection are not the main reason clerks do not advantage their party, but it does not imply that elections fail to motivate clerks. Clerks seem to be held accountable for bad behavior in many cases. For example, in 2010, a lawsuit was filed against Boone County, West Virginia clerk Gary Williams alleging sexual harassment right after he was reelected without opposition.²² He was challenged in the Democratic primary 6 years later and lost, receiving only 34% of the vote. Bosque County, Texas clerk Brigitte Bronstad was arrested for taking money from the county in 2002, right before the general election. Four write-in challengers quickly jumped into the race, successfully ensuring her defeat.²³ In other cases, election officials caught engaging in malfeasance

retired rather than face the voters. This was the case for Montezuma County, Colorado clerk Carol Tullis in 2012, who faced a lawsuit alleging she demoted an employee for running against her,²⁴ and likely played a role in Whitman County, Washington auditor Eunice Coker's retirement, who faced a lawsuit in 2018 alleging improper denial of employee medical leave, financial mismanagement, ballot irregularities, audit failures, discriminatory behavior, and politically partisan efforts to alter election outcomes.²⁵

Clerk Candidates May Have More Similar Preferences across Parties

Might Democratic and Republican clerks agree on how to run elections? Looking at the public, this seems unlikely. The average Democrat and Republican have meaningfully different views on issues like automatic voter registration, all-mail voting, and moving election day to a weekend (Stewart III 2021). On the other hand, candidates and winners often have experience in election administration and may have more similar policy views. Manion et al. (2021) surveys members of the public and clerks, and compares their responses across parties. While Democratic and Republican clerks still have meaningfully different responses to some policy questions, their preferences are more similar than Democrats and Republicans in the public and fully converge on some policy issues. For example, Democratic and Republican clerks express equivalent levels of voter confidence in national elections, agree that voting is a duty, and believe that local, state, and federal elections should be consolidated. Like their co-partisans in the public, Democratic and Republican clerks are divided on the issue of voter ID but hold much more similar views across parties on expanded early voting than members of the public—a policy that many clerks have discretion over. This explanation only partially accounts for the similarity in policies, turnout,

²² https://www.wvgazette.com/news/lawsuit-alleges-sexual-harassment-by-county-clerk/article_dcbac0b3-e6f7-5f8e-bb5c-38c960d76026.html.

²³ <https://www.mrt.com/news/article/Bosque-County-clerk-pleads-guilty-to-theft-7791967.php>.

²⁴ <http://api.the-journal.com/articles/8636>.

²⁵ https://dnews.com/local/whitman-county-former-auditor-on-the-hook-for-70k/article_9a3cdc46-ac5a-5a43-bc86-ecf6e0ed1bad.html.

and vote shares in elections run by Democrats and Republicans serving similar counties, but it is consistent with our main findings and existing survey data of these individuals.

In Section A.6.12 in the Supplementary Material, we also document that clerks from both parties serving identical counties implement roughly the same policies. While we cannot rule out that they do this because they expect these policies would have minimal effects (as we discuss below), this is consistent with clerks agreeing more on election administration across parties than the public.

Clerks May Have Limited Ability to Affect Election Outcomes

Even if clerks are unconstrained by reelection incentives and want to offer their party an advantage, they may not be able to. As we discuss in the Clerk Responsibilities section, clerks are given wide latitude to make important decisions such as where to locate polling places and when to host in-person early voting. These decisions may make it easier or harder to vote and likely affect some groups more than others. However, these policies do not necessarily affect election outcomes. First, when the cost of voting goes up, citizens may simply find the next cheapest way to vote (Clinton et al. 2020). Second, even if more people vote when the cost goes down, the new voters may be similar in partisan composition to the people already voting (Burden et al. 2014).

This explanation is difficult to directly test. If clerks know that they cannot meaningfully affect outcomes, and they only care about changing policy if it affects outcomes, we may not observe partisan differences in policies or turnout because clerks never even try to advantage their party. Still, based on the existing work on the limited effect of election administration, it is reasonable to expect clerks are at least somewhat constrained by the modest effects these policies have on partisan outcomes.

Clerks Do Not Advantage Their Party More When It Is Less Costly or When the Stakes Are Higher

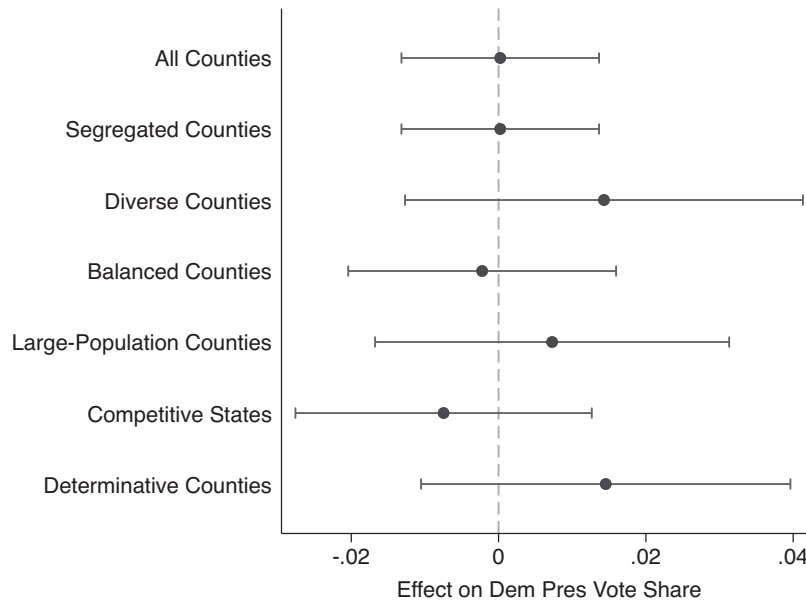
Suppose most election officials would like to see their party win and that they all have authority to advantage their party in their county. If they bare costs for tilting elections in their party's favor, they would only want to advantage their party when it would plausibly change the statewide outcome. In this world, the fragmented nature of local election administration creates a collective action problem where partisan clerks would like to work together and swing the election in their party's favor, but they know that every individual clerk would have a reason to shirk and avoid bearing the costs. This collective action problem does not arise if an individual clerk could reasonably expect their decisions to be pivotal and worth the cost.

We offer suggestive evidence that even clerks who face the lowest costs to advantaging their party or have

the greatest chance of swinging an election in their party's favor do not advantage their party. We do this by identifying six related conditions that either make it less costly for an official to advantage their party or increase the value of the advantage they provide. The first two conditions—residential segregation and racial and ethnic diversity—make use of the fact that race and ethnicity are some of the most useful heuristics for guessing the party a citizen may vote for (Carlson and Hill 2022; Carmines and Stimson 1989; Hersh 2015). Even if clerks are primarily motivated by providing their party an advantage, they may fail to do so if they cannot easily distinguish between members of their party and the opposing party. Accordingly, local election officials may have an easier time giving their party an advantage in counties that are more diverse and segregated. The third factor we consider is county-level partisan balance. As we discuss in Section A.7.3 in the Supplementary Material, we find using a stylized model that clerks serving counties evenly split between Democrats and Republicans will have a larger effect on election outcomes than clerks in places dominated by one party. The fourth factor we consider is the capacity of the office, which we proxy with population. We would expect clerks serving in larger counties to have greater capacity to affect election outcomes (Kimball and Baybeck 2013). The final two factors we consider—how close the last presidential election was in the state and whether the county is large enough to meaningfully alter the outcome—build on the prediction that election officials might be most motivated to advantage their party when it would be most likely to help their party win.

Figure 5 reports estimates of the effect of electing a Democratic local election official on Democratic presidential vote share in counties where we would expect clerks to be most likely to advantage their party if collective action problems were the primary barrier. Each point is an effect estimated using local linear regression with triangular kernel weights—the same specification we use in column 4 of Table 1. The lines extending out from the points are 95% confidence intervals. From top to bottom, the plot presents estimates using seven subsets of the data: (1) all counties, (2) segregated counties—that is, those with residential racial dissimilarity scores above the median, (3) counties where non-Hispanic white people make up less than 80% of the population, (4) counties in which the last Democratic presidential candidate won or lost the county by less than 15 percentage points, (5) counties with over one hundred thousand residents, (6) counties in states in which the last Democratic presidential candidate won or lost by less than 5 percentage points, and (7) counties with populations that are at least half as large as the margin by which the last Democratic presidential candidate won or lost in the state.

The estimates reported in Figure 5 are more consistent with clerks intending to administer elections in neutral ways than with a collective action problem preventing clerks from advantaging their party. If they want to advantage their party but fail due to a collective

FIGURE 5. Clerks Do Not Advantage Their Party More When It Is Easier or Most Advantageous

Note: Each dot represents a regression discontinuity-based estimate of the effect of electing a Democratic clerk on residual Democratic presidential vote share for a subset of the data. The lines around each point represent 95% confidence intervals. Estimates come from regressions that mimic column 4 of Table 1 using local linear regression with triangular kernel weights. Segregated counties are those with residential racial dissimilarity scores above the median. Diverse counties are those less than 80% non-Hispanic white. Balanced counties are those in which the most recent Democratic presidential candidate won or lost by less than 15 percentage points. Large-population counties are those with over one hundred thousand residents. Competitive states are those in which the most recent Democratic presidential candidate won or lost by less than 5 percentage points. Determinative counties are those where the population of the county is at least half as large as the most recent Democratic presidential candidate's margin of victory or loss at the state level. Full tabular results are found in Section A.7 of the Supplementary Material.

action problem, we might observe a partisan advantage in the cases where a county is closer to being pivotal or the cost of advantaging one party is lower. Instead, across the seven subgroups that we study, we cannot reject the null hypothesis that Democratic and Republican clerks fail to advantage their party. Our evidence suggests that clerks do not noticeably advantage their party even when they have the greatest ability to affect the statewide outcome and the lowest costs.

The regression specifications chosen and the rules used for including a county in each subgroup are somewhat arbitrary. In Section A.7 in the Supplementary Material, we present estimates using all four of our regression specifications for every outcome and estimates across many different rules for inclusion in each subgroup analysis. The results reported in Figure 5 are similar to those we estimate across our different specifications and subgroup inclusion rules.

CONCLUSION

The unusual American practice of electing partisan local officials to oversee elections concerns many experts and members of the public. When an official runs as a member of a party, it is natural to expect that they will use their authority to advance their party's

goals. Even some local election officials themselves report feeling uncomfortable running as partisans when they have a duty to be neutral.²⁶

Using a credible research design with new partisan clerk election data from 21 states, we find that partisan election officials do not typically offer a large advantage to their party. While we cannot be confident that partisan officials do not offer rare and large or very small but consequential advantages to their party, our findings make clear that clerks are not consistently providing their party a meaningful advantage to date.

While clerks do not advantage their party, this does not imply that we ought to use partisan elections to select election administrators. In many parts of the country and around the world, elections are run by appointed bureaucrats, and future work should consider how the benefits and costs of such a system weigh against the benefits and costs of the system we study in this article (Ferrer 2022). Also, a recent survey of the public found that about 75% of both Democrats and Republicans support requiring that election officials be selected on a nonpartisan basis (Stewart III 2021). Future work should consider if even neutral partisan

²⁶ <https://www.npr.org/2018/11/29/671524134/partisan-election-officials-are-inherently-unfair-but-probably-here-to-stay>.

election administration leaves citizens suspicious that the election was unfair.

How concerned should we be that future changes in who runs and wins clerk races may lead to highly partisan election administration? Our explanation that election policies only have modest effects on electoral outcomes provides some reason for optimism. However, our explanation that clerks are neutral because they share more similar preferences across parties than the public does leave room for concern. If the next generation of election officials begins to exhibit higher levels of preference polarization, there is no guarantee that partisan election officials will continue to administer elections neutrally.

SUPPLEMENTARY MATERIAL

The supplementary material for this article can be found at <https://doi.org/10.1017/S0003055423000631>.

DATA AVAILABILITY STATEMENT

Research documentation and data that support the findings of this study are openly available at the American Political Science Review Dataverse: <https://doi.org/10.7910/DVN/SHYPDU>.

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CONFLICT OF INTEREST

The authors declare no ethical issues or conflicts of interest in this research.

ETHICAL STANDARDS

The authors affirm this research did not involve human subjects.

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Understanding the Role of Local Election Officials: How Local Autonomy Shapes U.S. Election Administration

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SEPTEMBER 2021



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Executive Summary

This policy brief will examine the independence and discretionary powers of local election officials and offer a framework to better understand local autonomy in our electoral system. It will also describe the larger system within which the local election official operates and demonstrate how local power and voter-focused decision-making varies across the country. The brief will use illustrative examples of the exercise of autonomy by local election officials from past elections as well as examples of shifts in local discretionary powers from the recent wave of state legislative efforts that seek to restrict autonomy.

It will also address the implications of local autonomy for those with an interest in increasing voter access and promoting voter participation. This brief can be a resource for those seeking a better understanding of the possible levers of change in their own state or locality's electoral system.

Introduction

On September 29, 2020, the headline of the *Philadelphia Inquirer* read “The start of early voting in Philly was riddled with technical issues: The much anticipated debut of early voting got off to a rocky start, leaving voters frustrated and confused while they waited in line unable to cast their ballots.” Local elections officials in Philadelphia scrambled to provide voters with accurate information on the locations of new early voting sites and worked to continue to process applications when the state's voter database system went down that first day.¹ Meanwhile, across the state to the west, county officials in Armstrong County were still working to operationalize this new voting method and were not yet offering voters the option to come in person to request and return their ballot before Election Day. And to the north of Philadelphia, voters were hearing from election officials in Schuylkill County that they would not be offering the option to vote early in person in the county at all.

As voter protection staffers in Pennsylvania, charged with ensuring that all voters had accurate and reliable information, we were part of a broader community of advocates, officials, and campaigns rushing to provide this new information to voters. Throughout the fall of 2020, voters in Pennsylvania were left to decode a patchwork of options to access the ballot, which varied depending on which county that voter lived in. At the same time, local election officials were working around the clock to interpret, implement, and communicate the logistical details of new voting options for their constituents. “The volume of calls we have been getting has been overwhelming,” said Marybeth Kuznik, elections director in Armstrong County, northeast of Pittsburgh. It was preventing her office from working on anything else: “It has been almost like a denial of service attack at times because it seemed that sometimes all I could get done was answer the phone!”²

On September 28, 2020, the Pennsylvania Department of State had issued official guidance to the local election officials in 67 counties concerning absentee and mail-in ballot procedures.³ This official guidance from the secretary of the commonwealth was one of many issued in the months leading up to the 2020 general election in an attempt to clarify how local election officials should implement features of the new voting law in Pennsylvania. Passed in 2019, Act 77 allowed all Pennsylvania voters to vote by mail for the first time. It also allowed any voter to “vote early” before Election Day by going in person to an elections office to instantly request, receive, and return their ballot.

In the days and weeks after Election Day, local election officials continued to work around the clock to count thousands of mail ballots. They also exercised their autonomy to interpret voting laws and determine which ballots should be counted. One state Senate race that was too close to call in the days following the election came down to decisions on whether or not a small number of mail ballots should be counted. The Senate district straddled two counties; in one, local election officials

in Allegheny County decided to count mail ballots that arrived in time but lacked a handwritten date on the envelope. In the other part of the state Senate district, local election officials in Westmoreland County determined they would *not* count mail ballots that lacked dates, classifying them as incomplete. Nicole Zicarelli, one of the candidates for the state Senate, wrote in a court complaint that “mail-in ballots will be counted or not counted based solely on the happenstance of which county election board reviews the ballot.”⁴ These county election officials exercised their local authority to make consequential decisions about which voter’s ballots would count and thus which candidate would prevail. This autonomy and local discretion is the cornerstone of election administration in the United States.

During the 2020 election, we worked in Pennsylvania to ensure every eligible voter could access the ballot and that their vote would be counted. In the process, we observed firsthand the significance of local election administration and, in particular, the ways that local election officials regularly exercise their autonomy and their discretion to make consequential decisions that directly impact voter access and participation. Before our work in Pennsylvania, we, like many, thought that a state writes the rules for the election and the local officials simply implement the rules. The reality is much more complicated. Local election officials often make independent, localized decisions that have a direct impact on the voting experience, from voter registration to early vote to Election Day and through the postelection process. Sometimes these decisions have the effect of reducing barriers to participation for voters, but other times the decisions of local officials have the effect of suppressing or obstructing access to the ballot or even rejecting votes that would have counted in a neighboring county. Sometimes this autonomy is explicitly part of the process; at other times it *de facto* exists due to the state’s lack of enforcement authority or simply a lack of capacity to oversee what happens in all of its local jurisdictions.

Since our work in 2020 protecting access to the ballot for voters in Pennsylvania, we have sought to understand how the autonomy and influence of local election officials may vary across the country and have learned that no state is the same. This policy brief seeks to offer a framework for understanding eight key arenas of flexibility, discretion, and independence held by local election officials in a particular state, how this autonomy can impact voters’ access to the ballot, and whose vote is counted.

Promoting Voter Participation

In this brief we seek to explore the different ways local-level actors make autonomous, independent decisions and how this impacts the voting experience. We approach the topic from the perspective that, generally speaking, local discretion can be used in beneficial ways if it is done for the purpose of helping eligible voters to register, vote, and have their vote counted. Our view is that most election officials adhere to the highest principles of their jobs, and that includes acting in the interest of the voter. So we operate from the view that when it comes to actions that promote voter participation, state policy should normally be considered a floor not a ceiling, absent any countervailing *evidence-based* reason that those actions would be detrimental to the democratic process.⁵

There is a debate underway about whether there should be uniform standards for elections nationally, or at least at the state level. In the debate about uniform standards, we operate from the view that local autonomy in election administration can be exercised to meet the localized needs of voters in different communities to reduce barriers to participation and increase equitable access to the ballot. The nature of our decentralized system will undoubtedly result in variations in the voting experience; however, local discretion can and should be used to ensure that all eligible voters—regardless of the community they live in—can participate in the democratic process and have their vote counted. As is demonstrated in this brief, it makes little sense for voters in one county to have to overcome numerous hurdles to cast a ballot and have their vote count, while voters in another county in the same state do not encounter such barriers. For example, how is it that some voters have easy access to a drop box as

an option to return their mail ballot and voters in the next county over do not? Why should some voters be provided with more information on how to vote than others in the same state? Why should a voter's provisional ballot be counted in one county and a ballot completed in the same manner not be counted in another county? This has led to vast inequities in the voting system, in ways that often disproportionately negatively impact historically disenfranchised communities of color.

In places where state policy and law work to make registration and voting harder, with no legitimate justification, it has been a benefit that local elections officials can do what is within their discretion, like expand early voting or open additional polling sites, to meet the needs of voters in their communities. In other words, some state policies that appear to “equalize” voting options across a state could actually perpetuate inequities in voting access for different communities. To the extent that rules could be more standardized in ways that promote greater voter participation, that might be the ideal scenario, but uniformity should not mean a race to the bottom.

What Is a Local Election Official?

For purposes of this brief, we are defining “local election officials” to be the key decision makers at the local unit of administration. In some states, these are county officials, while in others these are city or town officials. Many jurisdictions, particularly large ones, have multiple local election officials who work together to make these decisions as part of a board, and others may have one key decision maker. Some are elected to their positions, and some are appointed by state or local authorities. Their official titles vary by state, but some common titles include county clerk, county commissioner, registrar, director of elections, or probate judge.

The size of the election jurisdiction often shapes the role and responsibilities of the local election official. Some local election officials administer elections in counties with fewer than 500 voters, while others are responsible for ensuring that millions of people can vote.⁶ For officials in smaller jurisdictions, they may have other duties outside of election administration, and election-related matters may only constitute a portion of their job.⁷ Local election officials in large jurisdictions can play an outsized role—as of 2004, the local election officials responsible for administering elections in just 4% of jurisdictions covered almost 64% of American voters in the election.⁸ Scholars of election administration often note that this system is unique to the United States and has advantages and disadvantages. The National Council of State Legislatures (NCSL) notes that a disadvantage could be ineffective or inefficient administration, whereas a strength could be a system immune to being “rigged” at a national level.⁹

The stress and strain that our electoral system faced during the coronavirus pandemic resulted in local election officials receiving an unprecedented spotlight in 2020, from celebrities championing their work to outside organizations recruiting poll workers or marshaling funding. In this brief, we focus on how local election officials also are key decision makers, playing a substantive role in deciding how our elections are run.

Framework to Understand Local Autonomy

In this brief, we examine eight arenas of autonomy for local election officials. This is not meant to be exhaustive but is instead a framework to understand the components that shape local autonomy and how a local election official's decision-making powers may vary based on these arenas of autonomy and context of state in which they are operating in.

To begin, it may not be useful to think of a local election official as “autonomous” or “not autonomous.” Instead, a local election official might have different opportunities to exercise their autonomy

based on their state's legal framework, their local resources, their method of appointment, among other factors. Our eight arenas of autonomy that we explore in this brief can help one assess the types of decision-making powers held by local election officials in their own state. We believe this framework of autonomy helps fill a gap in public understanding of the role and influence of local election officials. While it is already well documented that our elections are administered locally, we instead explore the range of independent decisions of these local officials, which can result in different voting experiences from county to county or town to town. This dynamic has important consequences for efforts to increase voter participation and decrease the racial inequities already present in our electoral system.

Our eight arenas of autonomy for local election officials generally fall under three broad categories: local discretion in voting options and voter outreach, state-local relationship, and local resources and capacity. They are as follows:

Local Discretion in Voting Options and Voter Outreach

1. Ability to expand or restrict voting options
2. Discretion over voter eligibility and control over voter registration
3. Responsibility for voter education and outreach

State-Local Relationship

4. State oversight of local officials and investigatory powers
5. Power to select and remove local election officials
6. Partisan affiliation and influence

Local Resources and Capacity

7. Local power to address financial resource needs
8. Training for election workers and local officials

Local Discretion in Voting Options and Voter Outreach

One of the most consequential ways that the autonomy of local election officials impacts the voting experience is in the discretion that officials have in their offering of voting options, voter information, and voter registration services. The autonomy of local election officials was clearly visible during the 2020 election cycle, when the coronavirus pandemic forced many states to adopt new voting procedures. According to Persily and Stewart, “29 states and the District of Columbia enacted 79 different bills to expand voting access in 2020.”¹⁰ But the passage of new laws was only the beginning of the process: once new voting laws were passed by state legislatures, it was up to local elections officials to interpret and implement new rules and procedures in communities across the country.

A local election official's autonomy in making decisions that expand or restrict voting options varies widely across states, even among states that would appear to have similar structures of local governance. For example, while Pennsylvania and Ohio both have county boards of elections, Ohio's board members are selected by Ohio's secretary of state and operate under fairly uniform early vote guidelines,¹¹ whereas Pennsylvania's county board members are local election officials and have more flexibility in deciding to offer drop boxes, offer satellite voting centers, or adjust voting hours.¹² We explore in detail examples like these later in this brief.

Local election officials also play a key role in interpreting state law and guidance to resolve disputes of which ballots should count. This can happen when the voter's intent is not clear, when their eligibility is disputed, or when the voter made a mistake. This part of the local election official's role came into sharpest focus during the 2000 election, as county officials in Florida were making determinations

around which ballots should count during a recount,¹³ but, in fact, local election officials make these decisions in every election cycle, sometimes playing a pivotal role in the outcome of an election.¹⁴

Finally, local election officials often have wide discretion in terms of the types of voter education or outreach they pursue to inform voters about their voting options, changes to voting rules, or information about the ballot. A particularly proactive election official might pursue innovative social media campaigns or have specific outreach programs to underserved communities, while an underresourced or less proactive local election official may only offer the bare minimum. This section explores each of these areas.

1. ABILITY TO EXPAND OR RESTRICT VOTING OPTIONS

Local election officials use their discretion to interpret state guidance and can have explicit authority to decide how early voting and vote-by-mail options are offered. This autonomy has the benefit of enabling officials to use their local expertise to tailor voting options to meet the needs of the communities they serve. This local independence in deciding how to offer voting options can sometimes impact voters' ability to access the ballot, by erecting additional barriers for some voters depending on income, geography, or other factors. This section explores how local election officials' authority and interpretation of guidance can impact voter access and the voting experience.

Explicit Authorities for Early Voting

The NCSL's 2020 state-by-state review of the laws governing early vote shows how state law defines the role of local election officials in the implementation of early voting. Their role varies widely across states. Depending on the state, a local election official may have the discretion to determine when early vote can begin, the days and hours of the early vote period, or the number and location of early vote centers.¹⁵ For example, several states grant authority to local election officials in determining whether "satellite" early voting locations are needed. Other states limit early voting to only the main office of the local election official (often the registrar or county elections office).

In states where local election officials do have this discretionary power, their decisions can have a direct impact on how much time voters have to cast their ballot. For example, while local election supervisors in Florida were required to begin the early vote period 10 days before the 2020 election, they also had the discretion to offer early vote as early as 15 days before the election in their county. Fifty-two out of 67 counties ultimately opted to allow early voting on this earlier timetable.¹⁶ In Michigan, where voters can vote early by absentee ballot, state law allows local officials to make in-person absentee voting available for "additional times and places beyond what is required." Clerks in some jurisdictions ultimately offered weekend and evening hours and also opened satellite voting locations.¹⁷ Clerks likely recognized that in their jurisdictions, traditional in-person voting hours could be restrictive for those with inflexible job schedules or caretaking responsibilities, and thus they sought to increase access for these voters. In these states, voters in neighboring counties or towns may have significantly different windows of time to cast their ballot.

Some states, like New York and New Mexico, set standards for the number of early vote locations based on the population of the election jurisdiction. In other states, the chief election official may have the ability to limit the discretion of local officials when it comes to early vote options. For example, in Ohio, there is a "uniform statewide schedule" for early voting, set by the secretary of state, who also has the ability to limit the number of drop boxes per county.¹⁸ In 2020, the secretary of state limited each county to one drop box regardless of the geographic size or population of the county, a decision that resulted in a number of legal challenges as well as a lawsuit from several civil rights organizations that said the decision "severely diminishes" access to the ballot.^{19,20} In other words, in some places the state

election official may be the key decision maker, which can end up minimizing a local election official's ability to use their expert knowledge of their communities to meet voters' needs.

Implicit Authorities for Early Voting

Local election officials are generally the ultimate link between a state's voting rules and the voter, which means they occasionally make judgment calls in their implementation of state law or state guidance. Beyond the explicit authorities granted by state law, local election officials also may have to make judgment calls in their offering of early vote options through interpreting ambiguities in the law.

For example, in Pennsylvania in 2020, counties were permitted, but not required, to offer drop boxes for voters to drop off their mail ballots.²¹ This resulted in a patchwork of different decisions by local election officials across the commonwealth, with some officials opting not to use drop boxes at all in their county and others installing 35 drop boxes across their county for voters to use.²² Local election officials also had to come up with their own plans to monitor and secure the drop boxes.

In Texas, Harris County local election officials offered drive-through voting and 24-hour voting at certain locations during the early voting period for the 2020 general election to accommodate voters who were vulnerable to COVID-19 as well as voters who were shift workers or otherwise found it difficult to vote during daytime hours. This decision was made following the March primary, where many Democratic primary voters, in neighborhoods that were predominantly voters of color, waited as long as six hours to cast their vote. While all counties did offer the option for voters to vote early, few offered as many options as Harris County. As a response to the barriers that voters experienced in the primary, the new county clerk used his understanding of the voting barriers local to Harris County and his decision-making powers to triple the number of early vote locations for the general election and allowed for 24-hour voting at some of those locations.²³ As of July 2021, the Texas State Legislature is considering new voting bills that would ban 24-hour voting and other measures used in Harris County to expand voting access, particularly for communities of color.²⁴

Local Autonomy for Voting by Mail

Voting by mail has gradually increased in popularity over time and dramatically increased in 2020 due to the COVID-19 pandemic.²⁵ Just like in-person voting, voting by mail is implemented by local election officials. Some local election officials may also have the ability to make certain steps of the vote by mail process easier for voters, such as providing a prepaid postage for an absentee ballot application.²⁶ In Michigan²⁷ and in Pennsylvania, for example, each state offered to reimburse local jurisdictions the cost of prepaid postage for absentee ballots. However, this funding was simply offered to jurisdictions, not required to be used. Clerks in Michigan and county election directors in Pennsylvania made the final decision²⁸ on whether to use this funding for prepaid postage for voters. While most local officials did take advantage of this funding, some chose not to. This resulted in some voters having to take extra steps to acquire a stamp and pay for the cost of postage to return their ballot during the pandemic.

The authority of local election officials is not always clear-cut, including on vote by mail. This can result in legal challenges and the involvement of the courts. For example, in 2020, auditors in three Iowa counties decided to mail voters absentee ballot applications that were prefilled with their personal information. However, Iowa's secretary of state issued a directive to invalidate these applications, leading to a lengthy court battle that was ultimately not resolved until weeks before Election Day.²⁹

In 2021, state legislatures across the country have taken various steps making it harder for some voters to cast their ballot, including by focusing on this particular type of local election autonomy. Georgia, for example, has made it illegal for counties to send out absentee ballot applications to all eligible voters in their jurisdiction,³⁰ and a new law passed in Iowa also bans local officials from using their discretion to mail absentee ballot applications to voters who have not requested them.³¹

Impact of Local Election Officials' Ability to Expand or Restrict Voting Options

Local election officials can exercise their autonomy to expand voting options to meet the needs of voters in their community. One county may be 45 times the size of another county in the same state when it comes to geography or population, as is the case in Ohio,³² thus suggesting a need for more drop box options. Some counties may include a significant number of voters who work hourly jobs in which it is difficult, if not prohibitive, to take off up to six hours to vote, as was the case in Harris County, Texas. To reduce barriers to participation and increase equitable access to the ballot, local election officials exercise their autonomy to expand voting options, and within this arena of autonomy they may also use their discretion to make decisions that restrict voting options.

2. DISCRETION OVER VOTER ELIGIBILITY AND CONTROL OVER VOTER REGISTRATION

The autonomy of local election officials is also relevant to questions of a voter's eligibility. Local election officials play a role in resolving when voters need to fix a mistake in their ballot, when their identity needs to be verified after a voter casts a provisional ballot, or when the voter rolls are being "purged" or updated.

Review of Provisional Ballots and the "Canvass" of Mail Ballots

Provisional ballots are used when a voter's eligibility cannot be determined at their polling place on Election Day, for example, when the voter's name does not appear on the voter rolls. These ballots are reviewed in the days after Election Day by the local election officials. In 2018, nearly one million provisional ballots were ultimately counted, and about 600,000 were rejected.³³ While states vary in the level of guidance they offer to local election officials, local election officials generally make the final decision regarding which of these ballots were cast by an eligible voter and thus should be counted.

Similarly, mail ballots may be "canvassed," or opened and tabulated, before, during, and after Election Day. For example in Pennsylvania, the law prohibits election workers from beginning to canvass mail ballots before Election Day, delaying the timeline in which the results can be tabulated. The law also does not require that the tabulation begins on Election Day, granting local election officials the authority to decide when to begin counting what could be tens of thousands of votes. In Pennsylvania in the 2020 election, the secretary of the commonwealth urged local election officials to begin canvassing the historic numbers of mail ballots as soon as the law allowed; however, local election officials in several counties exercised their decision-making powers by refusing to begin counting these votes on Election Day, maintaining that they did not have enough personnel to do it while also running an in-person election.³⁴

In the process of canvassing ballots, states and courts often provide guidance on which types of errors should result in a vote being rejected. In many cases, it is ultimately the local election officials who use their discretion to determine which errors are disqualifying. For example, a ballot missing a handwritten date on the outer envelope but marked as received by Election Day may be counted in one county and not counted in another based on the discretion of the local election officials.

Notice and Cure

“Notice and cure” refers to a local election official alerting a voter that their ballot is in some way deficient (“notice”) and the voter’s opportunity to correct the problem with their mail ballot (“cure”), ensuring that their vote will be counted. Deficiencies can result when a voter’s signature does not match the state database, if the voter forgets to fill out a portion of their ballot (such as adding the date), or if the secrecy of their ballot is compromised (by failing to include an inner envelope when returning a mail ballot). Local election officials exercise significant discretion and independence in this process. The rules or availability for this process vary widely from state to state, but the role of the local election official is critical since they make the determination about a ballot’s validity, make the voter aware of the problem, and provide a process for the voter to resolve the problem. In some cases the state election authority does not prescribe each step of this process, meaning the local election official may have discretion on how to handle the issue.³⁵

As of September 2020, notice and cure was required by law in only 18 states.³⁶ In other states, the practice may be permitted but not required. For example, in Wisconsin, clerks are merely “encouraged” to contact voters whose mail ballot has a mistake.³⁷ The method of contacting the voter can also vary, as Wisconsin clerks can elect to either email, call, or contact the voter through the mail to alert them about their deficient ballot. In some cases, the local election official may make the list of deficient ballots publicly available or available on request and allow outside groups to contact voters.³⁸ Similarly, the method by which a voter is allowed to “cure” their ballot could vary from jurisdiction to jurisdiction.³⁹

The practice of notice and cure may also be neither explicitly authorized nor specifically forbidden in the state’s election code, resulting in confusion or litigation. In Pennsylvania, the state issued guidance indicating that counties could allow voters with deficient ballots to use provisional ballots on Election Day, but local election officials in at least seven counties ignored the guidance, citing the fact that this type of “curing” is not mentioned in the state’s election code.⁴⁰

Purges

While the local election official’s role in administering “purges,” or updates to the voter rolls, is beyond the scope of this brief, local and state officials have varying responsibilities regarding the maintenance of voter lists. For example, in Ohio in 2019, voting rights groups across the state identified that more than 40,000 of the voters on the state’s purge list had been wrongly included by county election officials or in error by election software.⁴¹ Further research is needed to fully understand the role of local election officials in these types of purges.

Impact of Local Election Officials’ Discretion Over Voter Eligibility and Control Over Voter Registration

As demonstrated in the examples of local implementation of canvassing and notice and cure, state policy can, and should normally be, considered a floor, not a ceiling, to administering elections. In some states, notice and cure was allowed but only implemented by officials in some counties and not others. Local election officials who take action to operationalize notice and cure for their voters would seem to operate under principles that promote voter participation. Other local officials who exercise their autonomy in choosing not to offer voters the opportunity to correct minor ballot errors ultimately suppress the vote by disqualifying those ballots. While our decentralized election system empowers election officials to use their knowledge of local conditions to make decisions about voter eligibility and registration, in some cases those decisions can have the effect of suppressing the vote for certain communities.

3. RESPONSIBILITY FOR VOTER EDUCATION AND OUTREACH

Local election officials have various ways of communicating with the public, from mailers to their website to notices in local papers to social media. Local election officials serve communities that may have specific needs when it comes to sharing information about elections, like language translation. While political parties and advocacy organizations do much of this work, it is often narrowly targeted to their own supporters as opposed to tailored to all voters.⁴² The local election official's capacity to engage the public, as well as their willingness to do so, can result in variations from jurisdiction to jurisdiction in the amount of critical information available to voters. In some states, the state election code explicitly assigns responsibility to the local election official for voter education, but the local election official still has the ability to go above and beyond legally required responsibilities if it would better serve voters.⁴³ For example, King County in Washington state offers a grant program to organizations that provide information to underserved voters.⁴⁴ This can result in varying amounts of information being available to voters regarding their voting options or their eligibility.

While local election officials may vary in how proactive their outreach programs are, they also may be constrained by available resources. In a Democracy Fund report, local election officials told researchers that “resource constraints are a major limitation” in their efforts to educate voters.⁴⁵ We will discuss the impact of resource constraints on local election official autonomy later in this brief.

Local election officials are also often responsible for providing translation services to voters. While some jurisdictions are required to offer translation services under the Federal Voting Rights Act, many voters who are not proficient in English live in jurisdictions where translation services are not required. In these cases, their ability to cast their ballot may be greatly impacted by how proactive their local election official is in providing materials in the language they understand best.⁴⁶ For example, election officials in Arizona have used resources to have Navajo translators at some polling locations; however, the translation needs of smaller tribes are often overlooked, presenting additional barriers to those voters.⁴⁷

This issue is also intertwined with other arenas of autonomy discussed in this brief. For example, if a local election official is deciding to change their satellite center's voting hours, it is important for them to be able to communicate this update to their voters. If voters in a certain election jurisdiction made a common type of mistake when completing their ballot due to unclear instructions, a local election official may need to tell the voters in that election jurisdiction about their options for “curing” their ballot. This type of outreach played a significant role in the 2020 election, as the coronavirus pandemic caused significant changes to voting procedures and a shift to vote by mail. An analysis by the Associated Press demonstrated that voter outreach campaigns—including but not limited to those led by election officials—resulted in a significant drop in the number of rejected mail ballots.⁴⁸

As with other aspects of election administration, there is no particular consistency from state to state with respect to voter education and outreach. While the Help America Vote Act of 2002 (HAVA) includes a requirement that states submit voter education plans, it does not include any requirements for what this plan should entail.⁴⁹ There is also significant variation within states regarding whether the state or local election officials are primarily responsible for voter engagement or if the responsibility is shared.

For advocacy organizations and other groups supporting voters, it can be difficult to inform voters of their options, especially if details of the election vary from county to county. A particularly proactive or innovative election official can have a significant impact on voters' ability to access the ballot and advocacy groups' ability to spread reliable information. For example, in 2020, local election officials in California implemented a voting reform law that permitted “voting centers,” allowing voters to cast their ballot early and in person. In Los Angeles County, to help communicate this new option to voters, local election officials developed a “Vote Center Locator” app, which provided voters with a resource where they could easily look up information about their nearest Vote Center on their mobile phone.

The resource contained information about a Vote Center's hours, location, and even wait time so voters could go to the Vote Center with the shortest wait.⁵⁰

Impact of Local Election Officials' Responsibility for Voter Education and Outreach

Local election officials are the experts on how elections are administered in their jurisdiction, and their ability to educate voters is critical to increasing voter participation. Many election officials use their local knowledge and discretion to tailor voter education materials and outreach efforts to serve voters with different language needs or disabilities or to reach across digital divides. Proactive local election officials can also partner with advocacy groups to provide accurate and up-to-date information that organizations can amplify in their own voter education efforts. While some local officials go above and beyond the minimum federal or state requirements, others are hampered in doing so by resource constraints. As states change their voting rules or increase the number of ways a voter can cast their ballot, voter education becomes increasingly important. The role of local election officials in promoting voter education is critical considering the number of states proposing significant overhauls to voting laws in 2021.

State-Local Relationship

The dynamic between state and local election officials is not well understood. State guidance and oversight of local election authorities varies widely across the country and meaningfully shapes to the autonomy of local election officials as they make key decisions, from how voters access the ballot in their jurisdiction to which ballots might be eligible to be counted. These local decisions can have a profound and disparate impact on voters, and the degree to which the state election authority is involved plays a key role. Even in 2020, where our election infrastructure faced unprecedented scrutiny in states like Georgia and Pennsylvania, much of the public was still left with the impression that secretaries of state run elections, when the reality is a much more complex relationship between state and local officials.

This section on the state-local relationship will examine the ways in which the dynamics between local and state election authorities shape how local election officials exercise their autonomy and decision-making powers. First, we explain the state's oversight of local election administration and how it contributes to the decision-making powers of local officials. Next we examine the ways in which local election officials are selected for and removed from their positions, which shape accountability mechanisms for local officials and inform the role that state authorities may play in their hiring and firing. Finally, this section will explore how partisanship can influence local decision-making and local election officials' relationship to state authorities and national political parties. In light of the wave of new voting laws being proposed and passed in state legislatures in 2021, it is critical to understand how state authorities influence local autonomy and can shift the powers of election administration away from local decision makers.

4. STATE OVERSIGHT OF LOCAL OFFICIALS AND INVESTIGATORY POWERS

Although a state's chief election official, often a secretary of state, is seen as "running" a state's election, the reality is more complicated. As a general matter, legislatures pass laws constituting the election code, the state's chief election official is responsible for promulgating guidance and directives to local election officials, and the local election officials implement these laws and guidance.

State and local officials, both elected and appointed, have a range of overlapping and complementary responsibilities and authorities.⁵¹ This relationship differs widely from state to state, but at the state level, the relevant actors generally are the chief election official, which may be a secretary of state or an election commission. In some states, other state authorities such as the lieutenant governor or other state agencies may also play a role. At the local level, while there may be a primary local election official,

there are often multiple “election officials” in a jurisdiction, from county commissioners to town clerks to election directors.

In the lead up to the election, during Election Day, and after Election Day, the state’s tools, capacity, and authority to oversee the local election officials varies widely across the country. The summation of these tools, capacities, and authorities can generally be thought of as a way of understanding how “top down” the state’s control over election administration is and how autonomous local officials may be in administering our elections.

State Guidance and Directives

The legal structure and authority of the office of the state election official looks different across the country, and the tools they have available to influence or direct local election officials vary as well. While some chief election officials have regulatory and enforcement authority, many only provide “guidance” to local election officials. In some cases, local election officials may also be consulting their “in-house” legal authority, such as a county solicitor, to make decisions when election code may either be ambiguous or intentionally provide local jurisdictions with flexibility.⁵² State guidance may not necessarily be public or broadly available, meaning advocates and voters may be unaware of whether their local election authority is complying with guidance from the state election authority.

This relates to local election officials’ autonomy in a few key ways. First, state guidance may not be legally binding for a local election official. For example, as cited earlier in this brief, in 2020 several Pennsylvania counties opted to not follow guidance on ballot “curing” from the secretary of the commonwealth, offering their own interpretation of the election code that “curing” was not accounted for under the election code. In Pennsylvania the secretary of the commonwealth does not have direct hiring and firing power of local election officials, and control through other methods is weak.⁵³

Second, a state may not have the capacity or willingness to issue guidance with respect to every matter pertaining to the local election official’s administration of the election. For example, in October 2020, the Harris County Clerk in Texas repeatedly requested definitive guidance from the secretary of state’s office regarding the legality of their drive-through voting system, which resulted in significant media coverage about whether the service would continue, potentially confusing some voters in the process.⁵⁴ Judges in both federal courts and Texas’s Supreme Court threw out legal challenges to this voting option.⁵⁵

Third, state guidance may only be the first step of a process involving legal challenges and the court system, leaving local election officials unsure of whether state guidance is the final word on an election-related matter. For example, a county clerk in Michigan sued the secretary of state’s office in October 2020 over guidance related to signature matching, and the matter was ultimately resolved in the court system after the election was over.⁵⁶

Fourth, guidance from the state may come late, could be unclear, or could be difficult to implement, and a locality’s level of autonomy could determine whether or how it will follow the state’s directive. For example, in 2020, about two weeks before the election, Florida’s secretary of state issued guidance requiring most drop boxes to be staffed, just as counties were about to implement their drop box plans. Some counties altered their plans, while others indicated that their plans were already in place and complying with the guidance at a late stage would not be practical.⁵⁷

Impact of State Oversight of Local Officials and Investigatory Powers

The structure and powers of the state election administration office can have a profound effect on the ways in which local election officials can exercise their autonomy in different states. The state election official is often in a position to offer guidance that can help local officials interpret and equitably implement election procedures. The state official may also have the power to exert uniform standards in an

attempt to “equalize” voting options across a state, which can sometimes constrict the decision-making powers of local officials as they work to administer elections in a manner that meets the needs of voters in their jurisdiction. In some cases, there may be gray areas in which it is unclear whether the state or local election official has the authority to decide how to administer aspects of an election.

5. POWER TO SELECT AND REMOVE LOCAL ELECTION OFFICIALS

The manner in which local election officials are hired, fired, and ultimately held accountable varies across states and sometimes within states. Local election officials can be selected for the role through several methods and may hold the position as an individual administrator or as part of a multiperson board.⁵⁸ The selection mechanisms for local election officials can influence the level of autonomy that officials feel they can exercise and the ways that they may be held accountable for their decisions. If a local election official is directly elected by voters, as opposed to being appointed by a state authority, they may feel more locally accountable to the voters in their community than to a state election official. On the other hand, officials appointed by a state authority may be more insulated from public pressures and make decisions based on their expertise. It is important for advocacy organizations and civic groups to help make voters aware of their role in selecting their local election official and how that selection process may influence the ways in which local election officials use their discretion when administering elections.

Methods for Hiring and Firing Local Election Officials across States

While the methods for selecting and removing local election officials vary widely across states and within states, generally there are three main authorities that could be responsible for hiring and firing them: the voters, a state authority, or a local government authority. Local election officials can be directly elected to hold their position, as in Utah where the county clerks are selected by voters through a popular vote.⁵⁹ By some estimates, approximately 60% of elections officials are an individual administrator elected by voters.⁶⁰ Local election officials elected by the voters in their election jurisdiction may view themselves as accountable to the voters who selected them for the role, as it is the voters who will decide whether or not to reelect those officials. The voters may also be empowered to recall local officials for reasons such as misconduct, incompetence, or failure to perform their duties.⁶¹ While more often than not, state authorities have few options available to remove elected local election officials,⁶² there are cases in which a local official can be subject to removal from office by state authorities who had no role in appointing them, as is the case in Alabama.⁶³

There are many examples from the 2020 election of local election officials exercising their autonomy in ways that demonstrated who they ultimately felt accountable to.

In Pennsylvania, during the 2020 election, Al Schmidt was one of the three city commissioners, who are the elected officials responsible for running elections in Philadelphia. Despite being the only Republican commissioner and one of the few Republican-elected officials in the city, voters had elected him repeatedly to the office since 2011. In the days following the election, members of his political party, from President Trump to Republican state legislators, attempted to pressure Commissioner Schmidt to use his discretion to reject large swaths of mail ballots and not count certain votes. Members of the state legislature even attempted to create a Republican-led investigative body that could subpoena election officials and take control of the election administration. Despite calls from state authorities to not count certain votes, they had no direct hiring or firing power over Commissioner Schmidt. As the Philadelphia charter states, “the power is vested in the electorate to recall officials elected by them so

that such officials may be directly responsible for their behavior in office to the electorate.”⁶⁴ Commissioner Schmidt exercised his autonomy to interpret and decide which mail ballots could be legally counted, and he was likely influenced by the preferences of the voters who elected him and had the power to remove him.⁶⁵

Local election officials can also be appointed to their position by state officials, such as the governor, or by a state board. In Oklahoma, county election board secretaries are appointed by the State Elections Board on the recommendation of the Senate. Finally, in some states, local government authorities, such as a mayor or a county board of election, have the power to appoint local election officials to their positions. In Ohio, while the county boards are appointed by the secretary of state, those boards hold the power to appoint the county election director.⁶⁶ Local election officials who are appointed are usually removed by the state or local officials who appointed them.

An example of a case in which a local election official can be selected by one method but removed by another occurred when Florida Governor Ron DeSantis suspended Susan Bucher, the Palm Beach County supervisor of elections following the 2018 election, and hand-selected someone else to take the office after Bucher missed a recount deadline.⁶⁷ Bucher stated that her office did everything in its power to effectively administer the election and report the results, but the outdated equipment caused them to miss the deadline for the machine recount.⁶⁸ In Florida, local election officials, called election supervisors, are elected by the voters in the county. However, the Florida State Constitution authorizes the governor to suspend county officers on certain grounds, such as neglect of duty, incompetence, and commission of a felony. In cases where the governor suspends an elected local official, the Florida Senate becomes the responsible party for judging the merit of the suspension and either removing or reinstating the suspended official.⁶⁹

Following the 2020 election, there has been a surge of new voting laws in state legislatures across the country, some of which aim to reform the accountability mechanisms for elected local officials. Georgia’s Senate Bill 202, which was signed into law in March 2021 by Governor Brian Kemp, empowers the partisan state legislature to do what the Pennsylvania legislature was unable to with Commissioner Schmidt and suspend county election officials. The law grants the partisan state legislature more control over the Georgia State Election Board and provides the board the power to suspend county election officials and name a replacement of their choice. Advocates assert that the Republican-led legislature will use this new power to diminish the autonomy of local election officials in Democratic counties, such as Fulton County.⁷⁰

Variations in Hiring and Firing Local Election Officials within States

Not only is there significant variation across states in the manner in which local officials are selected to administer elections and who they are ultimately accountable to, but there is also variation in selection methods *within* some states. While many states use the same method of selecting local officials statewide, such as New York where all the local election boards are appointed by a county legislative body, other states use different methods in different jurisdictions.⁷¹ These methods often vary based on the size of the election jurisdiction.⁷² Kansas is an example of a state where the selection method for local election officials varies within the state. In Kansas, the four most populous counties have a sole election administrator who is appointed by the secretary of state, whereas in all other counties, the voters elect a partisan county clerk to be the election administrator.⁷³ Nebraska’s local election officials are also hired and fired differently based on the size of the county. For counties with a population of under 20,000, an elected, partisan county clerk administers elections. For counties with populations between

20,000–100,000, the election commissioner is appointed by the county board; for counties with more than 100,000 residents, the election commissioner is appointed by the governor.⁷⁴

This remarkable variation within a single state is usually a result of one of two legal features of American local government law: home rule or special legislation. A thorough exploration of these two pieces of law is outside of the scope of this brief, but it is important to note their impact on intrastate variation. Home rule allows local jurisdictions, such as towns and cities, to make their own decisions about who they empower to administer elections. It also limits the ability of state authorities to influence the local structures of election administration, including how local officials are selected and removed.⁷⁵ Special legislation, on the other hand, allows a state legislature to impose different systems of local election administration within a state, passing laws that apply specifically to one or a few jurisdictions in the entire state, potentially dictating not only the selection method of local election officials but also precisely who those officials are. For example, in Missouri, counties are generally empowered to choose between having a county clerk or a board of election commission administer their elections. However, special legislation in Missouri detailed specific conditions under which a jurisdiction would not be allowed to choose. The outcome of this special legislation was that every county in the state would have the ability to choose their structure of local election administration, except St. Louis County, St. Louis City, and Kansas City.⁷⁶ These jurisdictions that fall under state control, as opposed to allowing the county to choose their structure of local election administration, also have some of the largest populations of African American voters in Missouri.⁷⁷

There is limited research suggesting that the outcomes of election administration may differ depending on whether the official is elected or appointed. One study in Wisconsin suggested that local officials who were elected were more likely to take actions to increase the ease and accessibility of voting in their jurisdiction, resulting in higher voter turnout, in comparison to appointed, local election officials.⁷⁸ While this study was limited to Wisconsin, it shows how incentives and mechanisms of accountability may differ between elected and appointed officials. The potential impacts of how local election officials are selected and removed is understudied and merits further research. As we explore in the next section, the appointment versus election of local officials may also interact with the partisanship of some local officials, influencing their decision-making in the administration of elections.

Impact of the Power to Select and Remove Local Election Officials

The role of state authorities and of voters, in the selection and removal of local election officials, impacts who officials may ultimately feel accountable to serving. These complex hiring and firing mechanisms may also influence local election officials' decision-making where they have the discretion to implement pro-voter election procedures, such as expanding early vote options. Furthermore, as election jurisdictions vary across a state in terms of the racial and economic makeup of voters, it is critical to understand how local administrative power structures may vary within a state and the implications for racial equity in our democratic process.

6. PARTISAN AFFILIATION AND INFLUENCE

The ways in which local election officials exercise their autonomy and decision-making powers can also be shaped by partisanship. In the United States, the officials who supervise and administer our elections are often affiliated with a political party. Two of the potential indicators of the partisanship of election administrators are 1) whether or not their party affiliation was a factor in their election or appointment and 2) the election officials' party registration.⁷⁹ As discussed in the previous section, approximately 60% of elections officials are elected by voters to their position, and most of them are openly affiliated with a national political party. In fact, about half of the voters in the United States are represented by

partisan election authorities.⁸⁰ While national surveys show that Americans prefer having an elected, nonpartisan board of elections, this is the least common type of local election authority in the US.⁸¹ In recent years, it is more often than not partisan election officials in large jurisdictions, or in key counties in swing states, whose autonomy is challenged and decisions are scrutinized.

There are arguments for and against moving to a system of entirely nonpartisan election administrators. Some argue that partisan local election officials may be more vulnerable to national political pressures as they interpret rules and implement election procedures, while others would assert that having partisan election officials who are appointed or are elected by the voters from the majority political party is a mechanism to ensure accountability.⁸² While there are arguments in favor of and opposed to a system of partisan local election officials, the wave of voting restrictions being considered in heavily gerrymandered state legislatures across the country in 2021 indicate that the arguments for nonpartisan election administration should be urgently considered in order to protect voting access for all Americans.

Studies have produced some suggestive evidence that election administration can be influenced by the partisanship of local officials, specifically in large jurisdictions. As national political parties are more likely to dispute the administration of elections in large jurisdictions, those local election officials may be influenced by the views of the national party or already hold the views of the national party with which they are affiliated.⁸³ Researchers have suggested that partisan differences in local election officials can influence the voter turnout rate, differences in how they administer the casting and counting of provisional ballots, and their likelihood to purge voter registration lists, among other decisions.⁸⁴

Partisan Pressures on Election Administration

In addition to the findings from such studies, local reporting also cites instances of partisan pressures on local election officials. In the governor's race in Kansas in 2018, a local election official decided to relocate a polling place that served a majority Latino community to a new, less accessible location miles away. The local election official who made the decision was an elected Republican, who some members of the public viewed as an ally of Kris Kobach, the secretary of state and Republican candidate for governor on the ballot that year. In responding to criticism of voter suppression, Kobach stated that the decision was made locally and he had no jurisdiction over them as secretary of state.⁸⁵

In 2018 in Hays County Texas, students at Texas State University—37% of whom identify as Hispanic⁸⁶—spoke out about hours-long lines at the only early voting location on campus. They advocated for county election officials to expand early voting hours to accommodate the high numbers of student voters. Upon becoming aware of the student's outreach to local officials, the North Hays GOP President Wally Kinney rallied local Republicans to oppose any extensions of early voting hours, stating in an email that this “probably means that it is going to favor the Democrats, so maybe I should not be in favor of this.”⁸⁷ In response to residents' outreach, the Republican commissioner, who was elected to his position by the voters, stated “I want everyone to know and understand that we are doing our best. We are trying to follow the laws and allow the opportunity for all to be able to vote in the most efficient manner possible.” The county was pressured by voters from both political parties and threatened with multiple lawsuits. In an emergency meeting, the Republican-dominated commission ultimately voted to expand the hours at the early voting site that operated on campus.⁸⁸

Influence of State and National Partisanship on Local Election Officials

The context in which local election officials are operating is particularly important when examining the role of partisanship in administrative decisions. It is important to consider factors such as whether a local election official belongs to the minority or majority party in their county and state as well as the political pressures of an election, for example, a presidential election in a swing state. The 2020 election brought intense partisan rhetoric and political pressure on local election officials in battleground states. In Michigan, each of the state's 83 counties has a Board of County Canvassers, which is composed of two Republicans and two Democrats who are each appointed by the County Board of Commissioners to four year terms.⁸⁹ In 2020, there was enormous political pressure directed at the partisan county board members, particularly in the swing state of Michigan and in Wayne County where Detroit is located. When the Republican members of the Wayne County Board of Canvassers were preparing to certify the election results, they found themselves singled out by conservative talk show personalities, the state's Republican Party, and President Trump, who personally called the chairwoman of the Board of Canvassers.⁹⁰ The two Republican canvassers on the board initially blocked the certification despite being legally required to send to the secretary of state a certified copy of a statement of the election returns within 24 hours of the vote count.⁹¹ These Republican officials finally fulfilled their duty to certify the election results after they were promised an audit of the election.⁹²

Partisan pressures will continue to be a factor in the local administration of our elections, especially in large jurisdictions in swing states. In the future, partisan pressures could have even more consequential outcomes than members of one party conditioning their certification of results on an audit.

Impact of Partisan Affiliation and Influence

The partisanship of local election officials, particularly in relationship with state and national politics, can influence the ways in which they exercise their autonomy and use their discretion. There may be additional mechanisms for accountability for partisan election officials in comparison to nonpartisan officials; however, their autonomy can also be heavily influenced by the political forces at the state and national levels, especially in highly contested federal elections. While partisan local election officials have long been a feature of the American system, the 2020 election and the wave of new, restrictive voting laws being considered by partisan state legislatures in 2021 have underscored the potential benefits of having nonpartisan officials administering our elections.

Local Resources and Capacity

Local election officials rely on local resources and capacity to administer free, fair, and secure elections. In the 2020 election, government underfunding and new sources of philanthropic funding impacted local election officials' ability to provide services to voters and make administrative decisions based on local needs during the pandemic. This section will examine how the availability of resources to administer elections impacts the autonomy and discretion of local officials. This includes the local election officials' power to secure funds and use their discretion to address resource needs. It also examines their role in building capacity through training local officials and their coordination with state authorities in doing so.

7. LOCAL POWER TO ADDRESS FINANCIAL RESOURCE NEEDS

Local officials require adequate and reliable funding to effectively administer elections. Local election officials with more autonomy over their budget may also have more latitude to administer elections in ways that meet the needs of their voters and make the process more secure. Tammy Patrick, a former local official in Maricopa County, Arizona, has written about the critical lack of funding for elections in the United States. In her testimony to the Committee on Homeland Security, she stated that “episodic federal funding does not sufficiently bridge the gap nor stabilize election administration to allow for planned considerations of improvements and modernizations. Infusion of funding far too often happens late in the election cycle, thus limiting its effectiveness.”⁹³ Many local election officials agree. In the Democracy Fund/Reed College 2018 Survey of Local Election Officials, respondents said that a lack of resources was one of the biggest limitations on their ability to engage and educate voters and to assure a positive voter experience. Local election officials highlighted the critical need for funding to pay staff and poll workers, purchase new technology, and provide training and voter education. As one local election official stated, “Simply put, it is a funding issue. If you want voter education & reach out that’s funding. If you want more early vote centers that’s money. If you want shorter lines at the polls . . . that’s money. If you want equipment you can count on that’s money . . . If you want to continue to stay up to date and educated, that’s money! It all comes back to funding for me!”⁹⁴

The Cost of Administering Elections

Despite the importance of funding, we do not know how much elections cost in the United States. There are several reasons for this, from a lack of a common “chart of accounts” to the variation in data policies to the fact that some jurisdictions do not track certain costs, such as the cost to register voters.⁹⁵ Some costs of running elections can be intermittent, like buying new voting equipment, but the majority are ongoing costs required for every election, such as printing ballots and paying local officials.⁹⁶ In most states, counties and localities are responsible for the majority of election-related costs; however, some funds to run various aspects of elections can come from federal, state, and local government coffers.⁹⁷ While it is difficult to precisely account for the cost of elections, it is clear that there are significant resource disparities that local election officials must contend with in jurisdictions throughout the country and within states. For example, in North Carolina, some counties had as few as 51 voters per voting machine in 2018, while local officials in other counties in the state had to manage more than 500 voters per machine.⁹⁸ It is more often than not lower-income and communities of color that have fewer resources available to meet the needs of their voters when administering elections. The Brennan Center reported in 2018 on the strong correlation between demographic and economic change and electoral resources, finding shrinking electoral resources in counties where the white population also shrank over the past decade.⁹⁹

Election policies are typically developed and passed into law by state legislators, and then local election officials must figure out how to use the available resources and their authority to translate policy into practice. New reforms or procedures can impact the total cost of administering an election, and state legislatures do not always provide localities additional funds to enact voting changes. Local election officials who participated in the Democracy Fund/Reed College 2018 Survey of Local Election Officials reported frustration with legislative changes to elections that all too often occurred without input from local officials or the funding needed to implement new policies.¹⁰⁰ Only in Alaska and Delaware does the state cover all expenses for federal or state elections. Furthermore, only some states pay for things like training and compensation for local election officials, information for voters, polling places, ballots, and related supplies. Local officials can find ways to expand voter access with available funding when they have the authority to implement services such as vote centers or to establish all-mail

elections, which can also lower the overall cost of administering elections.¹⁰¹ With many states instituting new election reforms in 2021, local election officials will require resources to implement changes to voting procedures. Local officials who lack the autonomy to address their financial resource needs may be unable to take steps to implement changes equitably and educate voters about their options for participating in future elections.

Role of Philanthropic Funding of Elections

The 2020 election shone a light on the desperate need for funding to go along with changes in election administration. Local election officials found themselves preparing to administer a presidential election during a once-in-a-century global pandemic without the financial resources required to do so. Without adequate government funding, philanthropy stepped in to try to fill the gap in state and local resources. In one case, Mark Zuckerberg and his wife Priscilla Chan donated \$350 million to the Center for Tech and Civic Life (CTCL), almost the same amount that was appropriated by Congress in the CARES Act. CTCL in turn awarded grant money directly to over 2,500 jurisdictions to administer their elections at the local level. This open-call grant program was ultimately available to every local election department in every state. For the local election officials who had the interest and capacity to apply for these grants, some were able to more than double their funds available to administer the election.¹⁰² CTCL reported that election offices generally used the grant funds to pay for staffing costs, including poll workers and overtime pay and for equipment and supplies, such as ballot printers, tabulation machines, and envelopes.¹⁰³

While these funds helped local election officials administer a fair and secure election in 2020, the influence of these private funders and implications on our elections is not yet well understood, and many would argue that private funders should not replace consistent and reliable government funding of our elections. Following the 2020 election, some states are considering passing legislation in direct response to outside funding. In April 2021, Florida Governor Ron DeSantis signed into law S.B. 90, containing provisions that will make it harder for local election officials to get additional resources to administer elections. The new law prohibits officials from soliciting, accepting, or using any outside funding or donation for election-related expenses, including funds for voter education or outreach.¹⁰⁴

While states are restricting local election officials' options for obtaining additional funding to administer elections, many new voting procedures proposed by state legislatures in 2021 are not accompanied by the funding to implement them. One of the many changes made to how voters would access the ballot in a new Florida law include ID requirements to apply to vote by mail and limiting access secure ballot drop boxes. The Pasco County GOP Supervisor of Elections Brian Corley responded to the proposed changes, writing that "the current Vote-By-Mail [VBM] statutes, as well as the established security procedures with regards to VBM 'DropBoxes' worked extremely well in Pasco County, and to my knowledge, all of Florida. The provision that voids all vote-by-mail requests will not only impact millions of Florida voters but will also cause an unfunded mandate of millions of dollars."¹⁰⁵

Impact of Local Power to Address Financial Resource Needs

Local election officials' ability to implement voting procedures is dependent on the financial resources to do so. Limited budgets in turn limit local officials ability to meet the needs of voters and promote participation in elections. Local resource constraints often hamper the autonomy of local officials in lower-income jurisdictions and disproportionately impact voters in communities of color. Meanwhile, changes in voting laws at the state level often do not account for the financial resources that local officials will need to implement reforms, ultimately impacting the voting experience and exacerbating the inequities in our electoral system.

8. TRAINING FOR ELECTION WORKERS AND LOCAL OFFICIALS

Local election officials can exercise their autonomy in deciding how to train election workers in their jurisdiction. Local election officials themselves receive varying levels of training and guidance in how to administer the election, based on the dynamic with the state election authority, and in turn those same local officials are largely responsible for hiring and training poll workers. Given the complexity of election law and the challenge of implementing new voting reforms, providing sufficient guidance and training to local officials on state law and procedures is extremely important.¹⁰⁶

The State's Role in Training Local Officials

Statewide training is one tool that state election authorities can use to promote compliance with federal requirements, set standards for uniformity across the state, and promote best practices for election administration. But state election authorities vary in the degree to which they require, or even offer at all, training to local election officials.¹⁰⁷ Local election officials who have fewer proscribed training requirements from the state may have more autonomy to develop their own resources that are tailored to the unique context of their election jurisdiction, in comparison to local officials in a state whose training model is more “top-down.” In 35 states, local officials are either certified by the state or are provided mandatory training by the state, whereas 13 states provide only voluntary training for local election officials or training on certain aspects of elections, and 2 states do not provide any training programs for local election officials, though they may publish handbooks with information on election laws.¹⁰⁸

New York is one of two states where local election officials are not offered any training by the state, putting an additional burden on local election officials.¹⁰⁹ In the 2020 election, local officials used different practices across New York to assist in their counting of ballots, leading to errors, confusion, and a delay in the certification of the results. In New York State, election laws require that absentee ballots challenged by election commissioners or campaigns must be marked in ink with the specific challenge or objection.¹¹⁰ In New York's 22nd district (NY-22), seven out of the eight counties did not properly follow procedures laid out in New York State election laws, which requires objections to be written on the ballot in ink and initialed by board of elections commissioners.¹¹¹ In Oneida County, officials used sticky notes to record challenges. When those notes fell off the ballots and the recorded objections were mixed up or lost, it put the contested ballots into question and threw the congressional election in NY-22 into chaos. It took over 100 days of counting, lawsuits, and recounts to certify the results.¹¹² The autonomy of county officials in how they train workers, a lack of state resources, or available funding for training could have all contributed to the failure of local election officials to effectively count and record the votes in NY-22.

Training Poll Workers

In the 2020 election in Pennsylvania, many first-time poll workers who were running polling locations as a judge of elections reported being unprepared, receiving essential training materials only days before the election. Pennsylvania state election law is mostly silent on the training of poll workers, only requiring that the state develop a training program in consultation with county officials. The depth and quality of training is mostly at the discretion of the county officials.¹¹³ The requirements for a poll worker vary by state and also within a state, county by county.¹¹⁴

Across the country there are also plenty of examples of local election officials leveraging local resources and expertise from the community to effectively train election workers. In Maryland, the Anne Arundel County Board of Elections partnered with a local university to develop online educational modules for election judges on cybersecurity and the voting process. Election workers trained in 2020 not only completed the training modules, but they also took assessments to ensure they understand cybersecurity practices and were prepared to administer the election securely.¹¹⁵ The Wake County Board of Elections in North Carolina developed creative ways to train poll workers during the pandemic through a weekly virtual talk show for election officials called “Coffee with Nick,” which led to a 28-page resource of frequently asked questions and answers.¹¹⁶ The discretion that local election officials can use to develop and provide tailored training, or little formal training at all, can result in disparities in election administration within states, in contrast to a state that is more top down in their training requirements.

Impact of Training for Election Workers and Local Officials

Local election officials can exercise their autonomy in how they train election workers. This discretion in the approach, tools, and information used to train Election Day poll workers can be incredibly beneficial if those staff gain a comprehensive understanding of how to administer the election and have a nuanced understanding of the specific needs of voters in their jurisdiction. Training can be a tool to expand access to the ballot and ensure that every eligible vote is counted. This local discretion in training can also result in some election workers being underprepared or unaware of the complexities of the election code, especially if local officials are resource constrained. Ultimately, this impacts the voting experience in these jurisdictions and can even result in suppressing votes that should have otherwise been counted.

Conclusion and Recommendations for Further Research

Local election officials use their discretionary powers and expertise to administer elections for the voters in their jurisdictions. They are key decision makers with local knowledge and play a substantive role in deciding how elections are run, directly impacting the voting experience, from voter registration to early vote to Election Day and through the postelection process. These local decisions have the potential to promote voter participation and make access to the ballot more equitable across communities; they also have the potential to erect barriers for voters. To ensure inclusive and fair elections and to protect and expand voter access, we must understand the role local election officials play and the diversity of their powers.

We hope that the framework offered in this brief can help readers better understand the arenas in which local election officials exercise their autonomy and decision-making powers in their own communities. Local discretion can be used to administer an election in ways that meet the specific needs of voters living in very different communities across the United States. Our decentralized election system will never look “equal” from county to county, but voting access can be made more equitable if local officials are empowered to use their expertise, have the resources they require, and have appropriate accountability mechanisms in place to ensure they are acting in the interests of all eligible voters. It is important that the implications that local autonomy has in terms of ballot access, voter participation, and the racial inequities in our electoral system continue to be studied. In writing this brief, we identified numerous aspects of local election administration and local authority that merit further scholarly research.

- I. The implications of the methods of selection and removal of local election officials is not well understood. Further research is needed to understand how the hiring and firing methods of local officials may impact equitable access to the ballot and voter participation rates more

- broadly. This information would help inform policy decisions to improve the structure of local election administration and the balance of independence and accountability mechanisms for local officials.
2. The consequences of partisanship in local election administration are also an understudied area that merits more research. As we noted, we came across arguments both for and against a system of entirely nonpartisan election administrators. However, in light of the partisan-driven changes in state and local voting laws in 2021, it is critical to understand the impacts of partisanship on officials' interpretation and implementation of election procedures at the local level and how it might affect voter participation outcomes.
 3. The impacts of local variations in voting access within a state and the state-local dynamics that contribute to variations in voting access within a state is an understudied area as well. The ways in which state-local dynamics, as well as the divergent administrative decisions of local officials across a single state, impact voter participation merit further research.

Finally, and perhaps most critically, the autonomy and influence of local officials in administering our elections is still not well understood by the public and by advocates seeking to protect and expand voting access. Particularly in the context of the new voting laws following the 2020 election, there is an urgent need to empower communities with the information to understand the role and authority of their local election officials and to share approaches for how to partner with those local officials to ensure that every eligible voter can access the ballot and have their vote counted.

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Political Appointees vs. Elected Officials: Examining How the Selection Mechanism for State Governing Agency Board Members Influences Responsiveness to Stakeholders in Higher Education Policy-Making

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Abstract: Through an exploratory comparative case study of two U.S. states (Georgia and Nevada), this study investigates how the selection mechanism to state higher education governing agency boards influences the responsiveness of board members to stakeholders and their role in the policy-making process. Framed around the recent national policy agenda to improve postsecondary degree attainment and college completion, findings suggest that state agency board members in both states prioritized the opinions, insights, and goals of the state governor and governing agency staff, regardless of selection mechanism. However, for more localized issues and on-the-ground decision-making, stakeholders formally involved in the day-to-day operation of higher education, such as administrators, faculty, and students, serve a larger role, though this influence can be mediated by the selection mechanism of board members.

Keywords: higher education; governing boards; board members; policy process; stakeholders; stakeholder salience; United States

Designados políticos vs. funcionarios electos: Examinar la selección de los miembros del consejo de las agencias estatales y su influencia en su capacidad de respuesta a los líderes en la formulación de políticas de educación superior

Resumen: A través de un estudio de caso comparativo exploratorio de dos estados de EE. UU. (Georgia y Nevada), este estudio investiga cómo el mecanismo de selección del consejo de las agencias estatales de educación superior influye en la capacidad de respuesta de los miembros del consejo a las partes interesadas y su papel en el proceso de formulación de políticas. Enmarcados en la reciente agenda de política nacional para mejorar la obtención de títulos postsecundarios y la finalización de estudios universitarios, los hallazgos sugieren que los miembros del consejo de agencias estatales en ambos estados priorizaron las opiniones, conocimientos y objetivos del gobernador estatal y el personal de la agencia gobernante, independientemente del mecanismo de selección. Sin embargo, para cuestiones más localizadas y toma de decisiones sobre el terreno, las partes interesadas que participan formalmente en el funcionamiento diario de la educación superior, como administradores, profesores y estudiantes, desempeñan un papel más importante, aunque esta influencia puede verse afectada. mediada por el mecanismo de selección de consejeros.

Palabras-clave: educación superior; consejos directivos; miembros del consejo; proceso de políticas; partes interesadas; prominencia de las partes interesadas; Estados Unidos

Nomeados políticos vs. funcionários eleitos: Examinando a seleção dos membros do conselho da agência governamental estadual e sua influência em sua capacidade de resposta aos líderes na formulação de políticas de ensino superior

Resumo: Por meio de um estudo de caso comparativo exploratório de dois Estados Unidos (Geórgia e Nevada), este estudo investiga como o mecanismo de seleção para os conselhos de agências governamentais de ensino superior influencia a capacidade de resposta dos membros do conselho às partes interessadas e seu papel no processo de formulação de políticas. Enquadrado em torno da recente agenda de política nacional para melhorar a obtenção do diploma pós-secundário e a conclusão da faculdade, os resultados sugerem que os membros do conselho da agência estadual em ambos os estados priorizaram as opiniões, percepções e objetivos do governador do estado e funcionários da agência governante, independentemente do mecanismo de seleção. No entanto, para questões mais localizadas e tomadas de decisão no local, as partes interessadas formalmente envolvidas na operação do dia-a-dia do ensino superior, como administradores, professores e alunos, desempenham um papel mais amplo, embora esta influência possa ser mediada pelo mecanismo de seleção de conselheiros.

Palavras-chave: ensino superior; conselhos de administração; membros do conselho; processo de política; partes interessadas; saliência das partes interessadas; Estados Unidos

Political Appointees vs. Elected Officials: Examining How the Selection Mechanism for State Governing Agency Board Members Influences Responsiveness to Stakeholders in Higher Education Policy-Making

Governance of the public higher education sector in the US is highly decentralized when compared to other countries. Unlike nationalized structures, where oversight and management of higher education is conducted by a single governmental agency (Berdahl & Millett, 1991; Glenny & Schmidlein, 1983; Schmidlein & Berdahl, 1992), the federalized U.S. government reserves oversight and policy decision-making for higher education to the 50 states. Consequently, each state maintains independent arrangements to govern their public postsecondary education sector through entities referred to in this study as *state governing agencies*. These organizations are “charged with statewide policy for higher education” (McGuinness, 2005, p. 207) and are often discussed as factoring into disparate policy decisions across the country (Hearn & McLendon, 2012). In particular, previous research suggests differences in characteristics of state governing agencies, including organizational structure, fiduciary responsibilities, and level of autonomy from the state government all can influence the state policy process (Hearn & McLendon, 2012; Tandberg, 2013; Tandberg et al., 2017). However, an organizational feature often ignored in these analyses centers on the state governing agency boards overseeing these agencies and, relatedly, the individuals comprising these boards (“board members”; Morgan et al., 2021). As board members serve in oversight and decision-making capacities, including being charged with approving statewide policy agendas and setting policy, better understanding their influence in the policy process is crucial.

Complicating our understanding of state governing agency board members, however, are the varied mechanisms utilized for their selection. Most often, board members are appointed directly or through a combination of decisions made by the state governor and state legislature, with Nevada representing the sole instance where the voting public elect board members (Longanecker, 2006; Pusser & Ordorika, 2001). These mechanisms differ state-by-state but are usually outlined in a state’s constitution or higher education law. Variation in selection mechanism has previously been researched regarding whether appointment moderates the independence and decision-making of board members from the government. For example, citing Pusser and Ordorika’s (2001) comparative case study of the University of California and the Universidad Nacional Autónoma de México (UNAM), Longanecker (2006) proposes that the elected boards at UNAM “clearly have a reflection of the public good as defined by the public” (p. 107) while the governor-appointed board at the University of California often reflects the perspectives of the state executive. This work suggests that governmental officials may be utilizing political appointments as a way to extend their influence in public higher education decision-making beyond limits established by law through the selection of individuals willing to enact their agenda.

This reality is in stark contrast to the original intent of state governing agencies, which were established to limit the influence and power of politics and government on higher education policy-making (McGuinness, 2005). Given the range of stakeholders with interests in how higher education operates, including those directly involved in the higher education sector (e.g., students, staff, faculty, and administrators) and those indirectly impacted (e.g., government entities and workforce organizations; Benneworth & Jongbloed, 2010; Burrows, 1999; Jongbloed et al., 2008), state governing agency board members can also serve as arbiters of which stakeholders are directly and indirectly considered in decision-making processes and which are not. This prioritization can guide the perspectives influencing policy-making, potentially politicizing decisions guiding higher education in a given state and contributing to the growing political partisanship facing U.S. higher education (Ellis et al., 2020; Parker, 2019). Ultimately, understanding whether selection mechanisms

influences the association of board members with stakeholders can be critical for researchers examining policy adoption and implementation processes as well as higher education constituents seeking policy change.

Drawing on previous literature on higher education board member appointment in the US and the concept of stakeholder salience, the following research questions motivate the current study:

- 1) What has been the role of board members in the state policy process aimed at improving college completion and degree attainment?
- 2) How do different selection mechanisms for board members influence responsiveness to stakeholders in the policy-making process?

These questions will be considered through an exploratory comparative case study of two state governing agencies (the University System of Georgia (USG) and the Nevada System of Higher Education (NSHE)) and their decision-making in response to a policy agenda aimed at improving postsecondary degree attainment and rates of college completion. These state governing agencies were established by their respective state constitutions and are structured as the primary organization overseeing all of public higher education in their state. Both states are also members of Complete College America, which has served as an influential policy advocate associated with improving college completion rates nationally. A key difference, however, between the case study sites centers on the selection mechanism for board members, where the Georgia Governor appoints USG board members while the voting public elect NSHE board members. Notably, while USG's association with and responsiveness to the governor's office has been previously highlighted (Rubin & Hearn, 2018), NSHE represents the only state higher education governing agency where the voting public select board members. To this end, an underlying purpose of this study is to gauge whether different selection mechanisms of board members ultimately influence responsiveness and consideration of stakeholders in the policy process.

Literature Review

Research exploring connections between board selection mechanisms and decision-making in the US is often discussed in broader terms related to state governance characteristics. For example, Lowry's (2001) study examines the influence of board characteristics on financial policy including the structure of the governing agency, number of oversight boards, and the selection mechanism of board members. His most significant finding for the current study suggests that institutions in states where board members are selected by external, nonacademic, stakeholders tend to charge less than those institutions in states with self-perpetuating boards, where current board members select new individuals, or where board members were selected by academics or other internal stakeholders. Lowry argues these findings indicate board members are held accountable to the concerns of their appointing constituency, with stakeholders internal to the institution having a better likelihood of benefitting from an increase in institutional financial gains than those situated externally. Similarly, Kaplan (2004) suggests the inclusion of faculty on boards increases the likelihood of the institution having an academic as president and reduces the likelihood of academic programs being shut down, which aligns with the goals of faculty stakeholders. Notably, Kaplan's analysis also examines the influence of the selection mechanism of board members on policy decisions but finds mostly inconclusive results.

Despite the mixed findings, Minor (2008) considered the appointment mechanism of board members as a potential lever for policy change. He examines the relationship between various characteristics of state governing agency boards and state higher education performance, as defined by the National Center for Public Policy and Higher education's 2004 *Measuring Up* report

(participation, affordability, completion, and benefits). Through a comparison of five top performing states (Minnesota, Massachusetts, New Jersey, Utah, and Colorado) and five bottom performing states (Arkansas, West Virginia, Louisiana, New Mexico, and Nevada), he uses a typological analysis to identify patterns around board membership selection across the two groups of states. Minor (2008) determines that lower performing states had fewer qualification requirements and less evidence of scrutiny for individuals serving on an overseeing board. He ultimately questions whether a more rigorous appointment process, akin to top performing states, might be able to help lower performing states improve their higher education performance metrics.

More recently, Tandberg et al. (2017) explored a similar organizational characteristic through an examination of the institutionalized relationship between state governors and the state higher education executive officer (SHEEO), who serve as the day-to-day executive overseeing the state's higher education governing agency. Specifically, this study focuses on two aspects of this association—whether the governor has the ability to independently appoint the SHEEO and whether the governor has the ability to independently dismiss the SHEEO—and how this relationship impacts state higher education financing. Using a fixed-effects model, the authors find that unilateral gubernatorial appointment of the SHEEO results in increased spending towards higher education, while unilateral gubernatorial dismissal power results in decreased spending towards higher education. They explain their findings by theorizing that gubernatorial appointment power bolsters a SHEEO's ability advocate for higher education funding from the state legislature by empowering the executive to action and influencing state legislators to support the governor's indirect goals and agenda. On the other hand, gubernatorial dismissal power strengthens the governor's monitoring ability of a SHEEO's actions, which, in turn, result in the executive aligning their decisions to support the governor and state rather than greater monetary funding for their higher education sector and institutions. Although the Tandberg et al. (2017) study focused on a different actor within state higher education governing agencies, their findings remain compelling for the current study regarding how responsiveness to stakeholders may be influenced by selection mechanisms.

Underlying these studies are an understanding of the structural position of state governing agencies as operating between the state government and higher education sector. Previous research has characterized these organizations as “buffers” (Schmidtlein & Berdahl, 1992, pp. 32-33), “boundary-spanning organizations” (Tandberg, 2013, p. 507), “multi-facing organizations” (Rubin & Ness, 2021), and “intermediaries” (Morgan et al., 2021, p. 573), underscoring that they must be responsive to and are also held accountable by two different types of actors that often have different goals and ideals. Consequently, individuals in leadership roles in these agencies, such as board members and SHEEOs, must prioritize one audience over others in decision-making processes. To this end, the current study will expand on the extant literature by focusing on board members and considering whether selection mechanism influences the responsiveness of these individuals to stakeholders regarding a common state-level policy agenda. Moreover, as many of the previous studies focusing on board members are framed by highly partisan topics, such as tuition setting and institutional spending, which may conflate the influence of appointment by not considering potential alignment between a board member's personal bias and their appointing agent, the policy context for the current study centers on a non-partisan policy agenda in the US—improving college completion and postsecondary degree attainment—that has been a priority nationally for over a decade.

Policy Context: Improving College Completion and Postsecondary Degree Attainment

Following reports indicating that the U.S. fell in international standing regarding educational attainment and that postsecondary degree completion rates would not meet national workforce needs in the near future (Carnevale & Rose, 2011), improving these metrics became a national policy focus across the nation (Rubin & Hearn, 2018). Notably, President Barack Obama (2009) proclaimed to the U.S. Congress that “by 2020, American will once again have the highest proportion of college graduates in the world,” which was subsequently supported by several philanthropic, advocacy, and policy-oriented organizations. Consequently, many policy solutions began to emerge shortly after these public commitments. For example, a survey conducted by the American Association of State Colleges and Universities highlighted thirteen different initiatives implemented across the US within two years of President Obama’s speech (Russell, 2011). The programs presented in this document vary in policy solutions and level of implementation (state-level versus institutional-level), with several initiatives focusing on students of particular socioeconomic and racial backgrounds or institutions in specific higher education sectors (two-year institutions versus four-year institutions). Despite these underlying differences, all of these initiatives focused on the single goal of improving postsecondary degree completion in the country and showcases the various options available for states to consider for implementation.

Nevertheless, state-level commitment towards improving college completion rates has varied across the US. In particular, previous research noted political and economic factors as well as state governing agency leadership and structure as influencing state policy responses (Perna & Finney, 2014; Rubin & Hearn, 2018). For instance, Perna and Finney (2014) consider five states (Georgia, Illinois, Maryland, Texas, and Washington) in their examination of responses to the national movement to improve college completion. They conceptualize a model for understanding how state public policy can improve higher education performance, structuring their framework around and emphasizing the importance of “state-specific context” (Perna & Finney, 2014, p. 204). In addition to mentioning several of the previously mentioned characteristics, they also emphasize the critical role of state leadership in regards to steering the higher education sector to reach its goals. Specifically, Perna and Finney (2014) suggest that without effective leadership guiding statewide higher education policy, “colleges and universities [will] respond to other incentives and act (rationally) to advance their own priorities” (p. 206). Considering board members are responsible for the oversight of the state governing agency’s leadership, individuals in these leadership roles are more likely to be influenced by the opinions of the board. It is, therefore, important to better understand the role of board members in these policy processes and what potential influences may alter their opinions and decision-making.

Conceptual Framework

Developed in organizational management and ethics, stakeholder salience seeks to understand the conditions that influence stakeholder involvement in processes and decision-making (Mitchell et al., 1997; Phillips, 2003). Extending Freeman’s (1984) work defining stakeholders and their involvement in organizational management, Mitchell et al. (1997) examined why managers of an organization prioritize the opinions of certain groups over others. They argue that three criteria determine the “stakeholder salience” of a group: “(1) the stakeholder’s *power* to influence the firm, (2) the *legitimacy* of the stakeholder’s relationship with the firm, and (3) the *urgency* of the stakeholder’s claim on the firm” (Mitchell et al., 1997, p. 854). Based on these measures, Mitchell et al. (1997) create a typology ranging from *latent stakeholders*, who may only possess one of these

attributes, to *expectant stakeholders*, who exhibit two attributes, to *definitive stakeholders*, who are the highest category and have all three attributes. It is expected that as a party develops more of these attributes that their interests are more likely to be considered by the organization, increasing the likelihood for influence by those stakeholders.

Although Mitchell et al. (1997) focused primarily on for-profit firms in their examination of salience, its utility has also been considered in the higher education context (Benneworth & Jongbloed, 2010; Gonzalez-Perez et al., 2021; Powell & Walsh, 2018). In particular, stakeholder salience can provide insights into how an entity navigates the multiple, and potentially competing, perspectives of the varied constituents to the sector. For example, Benneworth and Jongbloed (2010) considered stakeholder salience through a comparative case analysis of three national programs aimed at enhancing the status of humanities, arts, and social sciences (HASS) as fields of study. Comparing policies in Canada, Scotland, and the United Kingdom, the authors highlighted the role of stakeholders internal and external to higher education institutions noting both play a role in the promotion of HASS programs (e.g., the government through funding and faculty and staff through research and support). They also concluded that non-governmental community representatives were secondary stakeholders but generally were not involved in policy processes, with the exception of occasionally working with institutional researchers and those with other roles connected to university decision-making. In part, the authors suggested the limited role of community stakeholders was due to only “short periods” of salience (Benneworth & Jongbloed, 2010, p. 582) courtesy of added pressure directed by the main stakeholders that ultimately impacted their role.

Two more recent studies by Powell and Walsh (2018) and Gonzalez-Perez et al. (2021) considered stakeholder salience within change efforts in the UK and Latin America, respectively. Powell and Walsh (2018) examined the 2017 introduction by the U.K. government of a new degree apprenticeship model in England, which “combines vocational and academic learning and is delivered to employees rather than students” (p. 93). The authors argued that this shift represented an on-going goal of the government to be “more responsive to the needs of industry” (p. 93), while offering minimal new support for non-apprentice students. Meanwhile, Gonzalez-Perez et al. (2021) explored how eight private higher education institutions in Latin America reoriented decision-making and actions to align with various stakeholders during the COVID-19 pandemic. Although previous research would suggest that institutions would prioritize stakeholders that could in-return provide resources, the researchers found social responsibility guided institutional decision-making as evidenced by institutions supporting national and local efforts to combat the pandemic as well as their students and current employees. Both studies underscored the many stakeholders impacted by these decisions but, through considering stakeholder salience, explained factors influencing the prioritization and responsiveness towards certain groups over others.

For purposes of the current study, stakeholder salience can offer insights into how different selection processes may impact state governing agency board members’ level of responsiveness to certain parties throughout the policy process. Based on the criteria discussed by Mitchell et al. (1997), the appointing party would have similar levels of *legitimacy* with the organization due to their involvement in shaping board membership. Therefore, any variation in the influence of these parties should center on differences in levels of *power* to influence the organization and *urgency* of the stakeholder’s claim to impact action. Utilizing stakeholder saliency for the current study also extends the use of the framework by considering it within the federalized U.S. higher education system.

Research Design

This study employs a comparative case study (Simons, 2009; Yin, 2017), where two cases will be analyzed and compared to gain a more complete understanding of an underlying question. Further, as an exploratory study, the goal of this analysis is not to create generalizable findings but to identify potential research questions to be considered in subsequent studies (Yin, 2017). Case selection followed purposive sampling (Yin, 2017) and considered a set of three primary conditions: 1) membership in Complete College America (CCA), 2) retention of a consolidated governing board, and 3) variation in the way individuals join the state agency board. Given that the study is framed by the policy issue of improving postsecondary degree attainment and college completion rates, the inclusion of CCA membership as a sampling strategy served two goals: 1) signifying a state-level commitment to the college completion agenda, which suggests governing agencies and board members were informed and making decisions around the policy agenda, and 2) ensuring an influential voice in the policy process around this context was equally impactful across cases. In particular, CCA serves as one of the largest single-issue membership organizations focused on improving college completion rates in the US and recent studies discuss the effectiveness of this specific organization in influencing decision-making and the policy process among member states, through its messaging, publishing, and advocacy (Hammond et al., 2019; Ness et al., 2021; Russell, 2011). Considering only states that retain a consolidated governing board ensures the state examined maintain a similar level of responsibility over policy direction, while retaining a comparable level of autonomy from the state government and over the higher education sector (Hearn & McLendon, 2012; McGuinness, 2005). Lastly, considering states that varied regarding board appointment mechanism was critical as it served as the primary state characteristic in question for this study and offers different models to compare results.

Based on the selection criteria, two state governing agencies were selected: the University System of Georgia (USG) and the Nevada System of Higher Education (NSHE). Georgia and Nevada both became members of CCA's network of states within the first two years of the organization's existence, with Georgia joining in August 2011 and Nevada joining as one of the original member states in February 2010. Since joining, Georgia and Nevada have been involved in CCA's annual convenings and workshops suggesting greater participation beyond merely being a member (Ness et al., 2021). Both states govern their public higher education sector through state governing agencies classified as consolidated governing boards (McGuinness, 2005), which were established by their respective state constitutions and explicitly created to be autonomous entities from the state government. Finally, USG selects its 19 board members via a common appointment mechanism—nomination by the state governor and approval by the state senate—while NSHE's 13 board members are elected by the general citizenry. Notably, Nevada is the only state in the U.S. where state governing agency board members are selected in this manner. More information about each state's fulfillment of these conditions, as well as additional comparative characteristics, are presented in Table 1.

Table 1*Dimensions of Sample States*

Case Criteria	Georgia	Nevada
State Higher Education Agency	University System of Georgia (USG)	Nevada System of Higher Education (NSHE)
Constitutionally Established Agency	✓	✓
Institutions Reporting to Board of Regents	28 ^a	8
No. of Voting Members on Board of Regents	19	13
Board Appointment Mechanism	Governor Appointed and Confirmed by Georgia Senate	Publicly-Elected ^b
Board Member Term Length	7 Years	6 Years
Member of Complete College America	✓	✓

^aThere were over 30 USG institutions reporting to the board when the state initially joined Complete College America in 2009. Several institutions were subsequently consolidated or closed due to previously planned efforts.

^b The Nevada Governor appoints members to the NSHE board when there is an unexpected vacancy due to death or retirement. However, all board members are subject to reelection via public election regardless of whether their initial appointment was made by the state executive.

The data for this paper include 16 interviews with state governing agency board members and state governing agency officials, including chancellors, vice chancellors, and associate vice chancellors who oversaw pertinent areas (academic affairs, students affairs, government relations, data and research, etc.). The interviews occurred between September 2016 and March 2017, after receiving IRB approval, with each conversation lasting between 30 and 90 minutes. The interviews followed a semi-structured protocol with relevant probes (Rubin & Rubin, 2011), including questions about the evolution of statewide college completion policy activity starting in 2009 (aligning with President Obama's call to action), the role of state governing agency board members, preferred sources of information, and the relationship between board members and stakeholder groups. A selection of key questions from the interview protocol are included as Appendix A. Interview data was supplemented with the analysis of 358 documents, including state governing agency meeting agenda and meeting minutes, statewide strategic plans, reports created by non-governmental organizations regarding statewide policies, and local and national media articles. These documents provided background information, names of potential informants for the study, and served as a resource when developing the interview protocol and to understand the overarching timeline and major events in the early stages of developing each case. As the study developed further, the document analysis was also considered to verify findings and emergent themes. Table 2 provides a distribution of the data collected.

Table 2
Distribution of Data Collected

	<u>Georgia</u>	<u>Nevada</u>
Total Interviews Conducted	8	8
State Governing Agency Board Members	4	5
State Governing Agency Officials	4	3
Total Documents Reviewed	182	176

Data analysis included coding transcriptions of the interviews and archival data using the qualitative data analysis program Dedoose with *a priori* codes based on an analytical framework that included: the role of state governing board members, research utilization, stakeholder group involvement, stages of the policy process, characteristics of the education sector (e.g., higher education attainment, higher education financing, higher education governance, K-12 education sector), and state-level features (e.g., political ideology, state economy and workforce, demographics of citizenry). I also induced emergent themes from the data collected by capturing local language (Saldaña, 2016). Finally, I employed several techniques to maximize trustworthiness throughout the analysis, including triangulation of data sources, member checking by conducting multiple interviews, peer debriefing with individuals familiar with the topic, and rival explanations analysis (Yin, 2017).

Limitations

Before discussing the cases and analysis of findings, there are a few limitations to note. First, the response rate among informants was lower than anticipated. The 16 individuals who participated represent approximately 38% of the 42 originally contacted, which was particularly lower among board members (nine of a potential 32), possibly due to the prestigious and political nature of serving as a board member. Despite this constraint, though, data saturation was achieved across interview and archival data collected (Yin, 2017), and I am confident on the themes and findings presented in this manuscript. Additionally, as this study primarily focused on a state-level characteristic's influence on statewide policy-making, I did not consider institutional-level or community-level characteristics in case selection. The interview protocol did, however, request insights regarding local-level policy decision-making as a comparison point with the statewide college completion agenda, which ultimately emerged as a thematic finding and will be discussed in greater detail. Relatedly, as this study focused on state responses to improving postsecondary degree attainment and college completion, results may be limited to this specific context given the national focus and expansive support towards this agenda. Finally, while I utilized several techniques to increase validity and reliability outlined by methodologists (Yin, 2017), the accuracy of the study is entirely reliant upon what respondents could remember and were willing to share.

Case Narratives

This section will offer separate narratives for the two states examined. These overviews will focus on the development and implementation of the statewide college completion agendas in each state and the role of board members in these processes.

Georgia: A Board of Political Appointees

In Georgia, the college completion agenda pre-dated President Obama's 2009 proclamation and was developed originally through actions by the Board of Regents. A University System of Georgia (USG) official explained:

The Board of Regents was already involved with a project they called 'RPG.' It was 'Retention, Progression, Graduation rates'... there was no sexy name to it, they just nicknamed it RPG, and there were probably two, three, maybe even four cycles into making that a point of emphasis. The campuses were reporting a little more regularly about what they were doing with regard to retention and graduation rates, and the Board was making it a center piece of their meeting schedule and to have regular conversations around that.

This respondent continued by mentioning that the "University of Georgia and [Georgia] Tech were already involved with their own unique plan with what they were doing about RPG," and that board members believed this should be a policy goal for all system institutions. Although there was not a system-wide policy outlining what RPG should include, several individuals mentioned this early goal promoted by board members as a stepping stone to a formal system-level initiative concerning degree completion.

In particular, respondents focused on 2011 when Complete College America (CCA) approached Governor Nathan Deal regarding joining their Alliance of States as a key event in the development of broader completion policy. Appealing to Deal's prioritization of workforce development, as well as USG's established action around RPG, Georgia joined CCA in August of that year. In announcing the state's commitment to improving postsecondary degree completion, Deal discussed the "Complete College Georgia Initiative" (CCG), which focused on five policy areas: 1) college readiness, 2) improving access and completion for underserved students, 3) shortening time to degree, 4) restructuring instructional delivery, and 5) transforming remediation. As part of CCG, USG institutions would also be required to submit institutional plans outlining institutional-level goals, implemented policies, and various additional metrics to inform the statewide completion goal. Discussing this system-wide policy shift, USG Regents suggested that the development of CCG and joining CCA was generally a governor-led process with additional insights from the USG Chancellor. One board member explained:

The way I remember it is it was really the Governor's initiative, that he had done some background work and came to the Chancellor, said "you know, we've got to be a more efficient operation," and "we've got to get more children – more young people in Georgia through our system graduated to meet the needs of what our economy is going to be, and, of course, we're preparing people for jobs right now that we don't know if they're going to be there in the future."

This perspective aligns with how CCA recruited member states, specifically through the governor's office and in partnership with the higher education sector head. Moreover, the USG board's involvement in the development of the previous RPG initiative was also central to ease and speed at which CCG was adopted as several respondents mentioned this suggested improving degree completion "was already a priority in Georgia."

A USG official offered a more nuanced perspective. Specifically, they noted that improving degree completion "meant everything to the governor's office in terms of relaying their priorities for Governor Deal's first term, and then... a more educated Georgia became the centerpiece of his agenda." This individual continued by suggesting, "I don't know that I've observed too many

governing boards getting too far out of step with the governor that appointed them” and argued Governor Deal’s vocal support of the initiative was sufficient for the board and agency to commit.

Once the agenda was in place, respondents suggested the constitutionally-granted autonomy of USG and its board from the state became a key factor in its implementation and proliferation across institutions. A USG official explained:

One thing that’s not been as important for Georgia’s completion agenda was to have SREB or Lumina or Gates or even CCA coming in and vouching for the importance of this to constituencies... We don’t have to persuade the House, and the State, and the Governor for this to be a priority. [USG] controls its agenda from the Board of Regents. We can do what we want to do. If this is a priority, we’ll pursue it.

Ultimately, there was limited need for the USG agency or board to consider perspectives outside of the system when gauging whether a focus on completion was worthwhile. Although board members did not serve a central role in the decision or creation of CCG, they did make decisions pre-dating this initiative that contributed to college completion becoming a statewide policy priority. Moreover, as a board member noted, “We monitor the metrics for Complete College Georgia – progression, retention, and graduation rates – the different metrics that we need to make sure that we have a high functioning system.” Through these monitoring efforts, this respondent noted board members’ roles in “identifying best practices and really praising those best practices, and facilitating knowledge transfer to and between institutions.” Consequently, while board members may not have been as central in the policy development process of CCG, they still served an important role in the oversight of policies.

Respondents from Georgia also argued that the focus on college completion was a unique instance of policy-making and not necessarily reflective of the involvement by USG board members traditionally in these processes. A USG official explained:

I think [college] completion is unusual in that they’re generally different programs or approaches that have to be tried out... it’s based on what we’re hearing from our sister states, what we’re hearing at national meetings through Complete College America, the Gates Foundation, Lumina, and others about best practices... It takes moving forward and giving this a go and seeing what the data might say, whereas in other situation... when [board members are] involved with fiscal policy or capital projects, it might be a little more straight forward in manner regarding the way they act... We have a little more room in academic affairs to truly develop a hypothesis and a treatment that we run through our campuses and see what the outcome is.

This official also suggested that board members perceive system staff to be the “content experts” in academic affairs and, therefore, rely on them to drive decision-making in this area. Other USG officials made similar assertions with one noting, “board members feel like they’re on the hook in terms of legal and fiduciary risk and responsibilities” and that their expertise is not on the “academic side of the house.” As a board member explained, “Our staff, we have a remarkable staff... They’ll give us very concise reports ... with maybe fifteen bullet points on a [policy solution], and how it could serve students... the staff drives so much of [decision-making] with the information that they give us.” Consequently, because the USG staff were viewed as the experts on the topic, respondents argued that the board members were less involved than they would in other topical areas.

While discussing the development and implementation of CCG and statewide college completion policy, respondents offered counter narratives of board members’ actions with local and narrower issues as a comparison point regarding their involvement in the policy process. For

instance, a board member explained that “being a [board member] is a very high visibility position, so you get a lot of calls.” A different board member provided a related anecdote:

When I go to a Rotary meeting... I’ll have probably, on average, five or six people come up and tell me something about education, either a success story of their children or a need of their children, maybe where [the system is] failing or where we’re succeeding, and I think all board members take that into account, but we get bombarded with it.

This respondent did not discuss the outcome of these interactions, namely if these conversations resulted in any change, but a different board member suggested these conversations often focus on voicing an opinion, especially “if we’re dealing with a controversial decision... the name change [of a university] or, you know, undocumented students.” This respondent continued by noting:

If it’s something that’s really controversial that needs to be aired, we want to hear their concerns and let everyone know we’re listening... I know where they’re coming from, but you also can’t let those types of things drive your decisions. You’ve got to look at the big picture, what’s best for the students, what’s best for the institution.

Several board members noted instances when these personal communications resulted in action on behalf of individual students, such as regarding financial aid, or served as an introduction into a broader issue that required examination, such as transferring course credit between institutions, which led to board members discussing these concerns or requesting information from USG staff.

However, USG board members acknowledged that these personal conversations with the community rarely influenced significant decision-making or directly informed system-wide action. As one board member explained, “I do think it’s our responsibility to hear parents’ concerns and the community’s concerns, but really that is the role of the institution and their town and gown [relationship]. That’s the role of the [institution’s] president to make sure he has that relationship with the community.” In other words, this individual argued that the USG board overseeing system-wide decision-making may be too far removed from institutions to warrant a focus on community relationships and issues. This perspective was furthered by a different board member, who noted, “We recognize students’ concerns, but then again, I don’t feel the tail should be wagging the dog.” Ultimately, while USG board members discussed their role in bringing attention to certain issues and serving in an oversight role of policies after their adoption, as a USG official explained, “They’re a policy board. They’re a governing board. They’re not an implementation board.”

Nevada: A Board of Elected Officials

College completion was an often-discussed topic during the Nevada System of Higher Education’s (NSHE) board meetings, though mostly as an explanatory factor or justification for decisions. For example, during an April 2009 meeting, NSHE officials discussed summer course offerings and available instructor summer salaries as a means to aid students complete their degrees in a reasonable time. Similarly, in a June 2009 meeting, board members discussed the potential use of distance education as a cost-saving strategy, but considered poor completion rates of these programs as an important factor in determining whether to pursue it as a policy across the system. However, it was not until the board’s September 2010 meeting, which announced Complete College America’s (CCA) inclusion of Nevada in a pilot completion academy, that improving college completion was discussed directly as a statewide policy focus. In addition to serving as a focusing event around college completion, respondents noted that CCA’s completion academy also advocated their “game changer” policies solutions that later were considered for Nevada’s policy plan, including

performance-based funding, changes to academic remediation, course delivery methods, and considerations around time to degree.

Two events in 2011 furthered the statewide emphasis on improving NSHE degree completion rates. First, following the election of Brian Sandoval as Governor, he made several speeches and statements connecting the success of the higher education sector with economic development of the state. One board member suggested that Governor Sandoval aimed to “focus on, in a very public way... that we want [current and prospective students] to finish their degree and then stay in Nevada and be long-term contributing vital members of the workforce.” The second event that year was NSHE released a new strategic plan, entitled *The State and the System: NSHE Plan for Nevada's College and Universities, Combining Excellence and Austerity to Attain Success*, which similarly argued that a successful higher education sector was necessary for the future of the state. The strategic plan also included all four policy areas emphasized by CCA during their completion academy (performance funding, remediation, course delivery methods, and reducing time to degree). Remarking on the inclusion of CCA policy solutions in the 2011 strategic plan, NSHE officials spoke highly of the intermediary organization’s role in influencing the policy agenda and discussion in the state. One respondent even explained, “We often give credit to CCA [for non-CCA related policies] because CCA has name recognition in the state, so when we go forward and say ‘hey, this is something that’s recommended by CCA’ they listen.”

In discussing board members’ role in the development and prioritization of improving college completion, opinions from respondents were mixed but generally connected to the system’s decision to join CCA. For example, one board member suggested that they played an integral role from the beginning:

The [NSHE] Chancellor’s Office, at our direction, started providing data to the board on what it would take for us to be able to implement or develop and implement a [college completion] program. And the board directed the Chancellor’s Office to become a member of the CCA and to report back to us as to what that meant and what that would entail for our institutions. Then, the Chancellor’s Office... put together the data for us to take a look at, so that we would be able to come up with a policy.

A different board member recalled the completion initiative “was presented to [the board] by the Chancellor” and then the board members’ made official decisions whether to join CCA and adopt a focus degree attainment as a priority. A third perspective came from an NSHE official, who stated, “Our involvement in CCA did not come from the board, it came from the governor.” Although these accounts appear to be contradictory regarding the board’s involvement and consideration of working with CCA, there was agreement across all respondents that the board’s job is to “set policy” for the system and, therefore, college completion was not deemed a priority until it was formally adopted by the board.

When discussing policy solution development, respondents suggested board members were influential prior to policy implementation. For example, two of the main policies adopted by the system was an outcomes-focused funding model and “15 to Finish,” which aimed to shorten time to degree by increasing baseline credit requirements for full-time enrollment. Although both of these policies were advocated by CCA, which helped expedite interest in these solutions by the board, respondents noted board members were consulted with requests for their input in the development of metrics included in the funding formula and direction of the credit requirement program. Nevertheless, as an NSHE official explained, board members “are not very engaged in the nitty-gritty of policy except from the perspective that they often decide that there’s something that they’ve heard about or encountered that really ought to be taken up.”

In particular, as described by one board member, board members rely on the Chancellor's Office to determine potential system-wide policies to consider and research possible changes to current policy, including the college completion agenda:

I think in many instances, [the Chancellor's Office] were the ones that were up on the latest literature and latest initiatives throughout the nation. So, I think the Chancellor's Office and the Vice Chancellors were the ones that really brought [policies] to [the board's] attention. And, you know, told us about things like Complete College America and "15 to Finish"... but when they brought it forward to us, they encouraged us to embrace it and we did, so I think we work in concert together on ways to make it all work.

NSHE officials held a similar stance, with one noting a caveat that "for academic affairs [issues]... a lot of the policy changes are born out of the Chancellor's Office because we have a research team that is using data to continuously look at ways that we can be better, do better, serve our students better." Put differently, because NSHE has a team focused on researching academic affairs issues, board members do not need to take a lead in this functional area and can focus on other issues and concerns. Respondents did, however, mention the academic affairs committee of the system, which includes a "smaller committee of board members that is sort of an offshoot of the larger board," as a location where "a lot of policy decisions are coming from." Therefore, although the Chancellor's Office may drive academic affairs policies, such as those connected to improving degree attainment, board members are still intimately involved in the process prior to decisions around whether implementation occurs.

In comparison, respondents noted a greater role by board members in more localized policies and specific cases. An NSHE official explained a possible reason why:

Our board members are all accessible, and you know, they can be emailed or called... do they get ideas from the folks that are either disgruntled or even have just good ideas that they want to, you know, share and hopefully that will rise to the top to be an actual policy change? I think that happens. And then sometimes... they have a neighbor or a friend or someone that has a problem that is, you know, trying to understand why does this happen. Why don't we do it this way or a student transferring from another state and what issues they might have? But people are pretty savvy and if they don't like the answers that they get from the provost or their advisor, and it rises on up and they don't get the resolution that they want, they email board members and then the board member are like, "oh, well, why do we do it this way?"

Underlying this perspective, is the reality that NSHE board members are publicly elected by congressional district. Specifically, as a board member explained, "they're all your constituents besides being, you know, educators or students and so you have to listen and take it all into account." While one respondent suggested the focus on constituents may be a function of the "newly competitive environment... being brought to campuses" due to an increasingly crowded higher education marketplace, there was general agreement that board members, as one NSHE official noted, face greater "pressure for customer service improvements than in most systems." In fact, one board member even viewed their role to be akin to serving as a "state representative" because they are elected by congressional district and must be "accessible to our constituency." This individual continued by explaining that "it's up to you to get yourself reelected... it's not up to the governor... they don't endorse you or anything like that." Ultimately, because board members in Nevada are not politically appointed, there is much greater need for them to appeal to their voting

constituency to retain their position and, by consequence, to listen and act according to the needs of the individuals in contact with them.

Emergent Themes

The Georgia and Nevada cases discussing the development and implementation of a statewide college completion policy agenda provide insights into the role of state governing agency board members in the policy process and influential stakeholders guiding their decision-making. Respondents offered insights suggesting factors that made the college completion framing unique as well as areas where board members and certain stakeholders are more likely to directly influence the policy process. This section will discuss two themes that emerged across the cases: 1) board member roles depend on policy agenda, and 2) statewide versus local policy issues.

Board Member Roles Can Depend on Policy Agenda

Respondents in both states noted that the college completion agenda was anomalous in many ways when compared with other policy initiatives undertaken by state governing agencies, which ultimately limited the role of board members. In particular, because of the national attention directed towards improving degree attainment from President Obama and national organizations, there was a noticeable interest to enact solutions at the state-level that could serve the broader agenda. This was shown in Georgia, where an established USG board's focus on retention, progression, and graduation rates quickly became the foundation for a significantly larger and broader system-wide initiative, and in Nevada, where tangentially-related conversations on the topic was succeeded by a much more concise and focused agenda.

Furthermore, as both states were early members of CCA, a variety of policy solutions were promoted to state policymakers and the state governing agency absent of board member involvement or intervention. Relatedly, since CCA membership was contingent on the involvement of the state governor, the success of a college completion agenda became of greater importance to the state executive than traditionally seen around other higher education policy issues, which led several respondents to suggest that their state's college completion agenda was primarily a governor-driven initiative. Collectively, these factors suggest a more limited direct role for board members in the early stages of the completion agenda, regardless of selection mechanism, but instead a reliance on the state executive and policy advocacy organizations, like CCA, for focusing attention on the issue and presenting possible solutions for enactment.

Once college completion was established as a priority, the role of board members and stakeholders influencing their decisions shifted but remained framed by the policy agenda. In particular, board members in both states noted that improving degree attainment was viewed as an academic affairs-related concern and, unlike other policy areas that are within board members' portfolio (e.g., hiring officials or financial decisions), policy areas related to academics are considered to be outside of their expertise. Although respondents, particularly in Nevada, noted some board members were more involved in these processes due to their role on the academic affairs committee, there was general agreement that agency staff served in a central role in gauging the potential utility and value of policy solutions. Board members suggested there was greater reliance on governing agency staff and other "content experts" who better understand the academic side of higher education. Moreover, given that many of the policies under consideration in both states were advocated by CCA, much of the decision-making centered on adapting these solutions to the state's context rather than determining whether they were best practices or would be successful, which

eliminated a significant role for board members in this process and mechanisms for individuals to influence policy adoption.

Ultimately, respondents highlighted unique traits to the current college completion agenda that impacted the board members' responsibilities, such as it being in the domain of academics and being influenced by CCA and other national organizations, but it is worth noting that neither of these factors are exclusive to this policy agenda. Policies associated with curriculum and majors may similarly be characterized as academic affairs-related, and the increasing role of intermediary and advocacy organizations in the U.S. higher education sector suggest this limited role of board members is not exclusive to the completion agenda. Therefore, based on the current analysis, the role of board members in the state policy process as well as the stakeholders considered in these processes may be contingent on the policy agenda and policy context itself.

Statewide Versus Local Policy Issues

A second theme emerged through comparisons made by respondents between state and more localized policy issues. For example, individuals in Georgia and Nevada suggested local policy issues, including those at single institutions or those impacting single or groups of students, are often introduced and influenced by board members. Because of the public nature of the role, board members suggested these concerns are often brought to their attention at community activities, such as the Georgia respondent who heard stories of accomplishments and barriers to success at a Rotary meeting, as well as respondents in Nevada who noted the availability of emails and phone numbers of NSHE board members. These anecdotal interactions were discussed as a mechanism for institutional policy change or to serve as a catalyst for board members to seek additional information to inform their perspective. However, these discussions also alluded to the true influence of appointment mechanism on how board members in the two states interact and view these constituents.

For example, as the broader Nevada citizenry double as their electing population, NSHE board members discussed paying close attention to these individuals needs and were willing to find answers or solve issues as best they could. To this end, multiple board members viewed their role to be akin to serving as a state representative because they are elected based on congressional district and believed they needed to be "accessible to out constituency." On the other hand, although USG board members stated that they would be willing to help individuals in the public, there was significant reservation with one board member suggesting the "tail should not wag the dog" and that their primary responsibility was to the state and to consider the broader agency's plan and goals. In fact, rather than considering the role to be similar to a state official, respondents in Georgia suggested board members often aimed to align decisions with the governor and not be "far out of step" with the state executive.

One important caveat to this distinction was the role of governing agency staff, who were the most often cited source of information for board members regardless of policy issue or appointment mechanism. These individuals were highlighted as offering "concise reports," being "up on the latest literature and latest initiatives throughout the nation," and handling a lot of the research informing decision-making. Some respondents even suggested that the provision of data and research to board members was a primary responsibility of agency staff, solidifying the important role these staff members play in providing information.

Discussion

A primary finding in this study was the overall limited involvement of board members in the policy process around improving postsecondary degree attainment statewide. Given previous

research suggesting board member training and appointment could serve as a potential mechanism to improve various student metrics, including completion (Minor, 2008), this common narrative in both states around board member involvement was striking. Respondents suggested this reality was due, in part, to the nature of goals of improving degree attainment—namely that it is considered an academic affairs-related issue—which falls outside of the expertise of board members, but also emphasized the integral role of Complete College America and other intermediary organizations in the introduction and advocacy of policy solutions. Relatedly, there was an acknowledgement of ownership of the initiative by the state governor that supplanted traditional duties and responsibilities of board members. This was evident through remarks by respondents regarding other policy issues that suggested board members are often responsible in introducing new policy concerns, questioning potential policy solutions for different agenda areas, and monitoring the outcomes of implemented policies more directly than was evident with degree attainment.

However, through these discussions of the role of board members in different issue areas, the influence of selection mechanism on responsiveness to stakeholders became increasingly evident. More specifically, as board members discussed their role and consideration of state-level versus local-level policy concerns differences between the Georgia and Nevada cases emerged. Respondents in Nevada emphasized the importance of helping all constituents and being willing to speak with these individuals to work through any issues faced. Board members even suggested their position was akin to serving as a state representative and emphasized their role in being “accessible to our constituency.” On the other hand, although respondents in Georgia mentioned receiving similar calls to action from stakeholders, there was notably less willingness for these individual to drive policy decisions, with one board member stating that the “tail should not wag the dog” and another suggested their role was entirely “apolitical.” Nevertheless, underlying many of the comments by Georgia board members was a goal of aligning decision-making with the state’s governor and serving as a means to help the state executive’s agenda become realized.

In order to make sense of the similarities and differences between cases, I return to the guiding conceptual framework of this study. Based on the work on stakeholder salience by Mitchell et al. (1997) and Phillips (2003), the common responses to the state-level college completion agenda and divergent responses to local policy issues can be attributed to the levels of legitimacy, power, and urgency represented by each stakeholder and each situation. Specifically, within the college completion agenda, the state’s governor maintained significant legitimacy and power in both states due to their position as the state executive and presented significant urgency through their desire to implement policy from the involvement of Complete College America and other external pressures. Although the governor has less legitimacy among board members in Nevada because of less direct involvement in shaping the governing agency, the overall power of the position and role in selecting board members due to sudden openings on the board likely trumped this differential. Conversely, when considering the smaller and more local policy issues, concerns from internal higher education stakeholders (e.g., students and their parents, faculty, alumni) were much more effective in Nevada than Georgia because these individuals maintain more direct legitimacy and power from their ability to influence board member selection through public elections. Ultimately, an important narrative develops from this study that suggests the public election of board members increases the influence of the general citizenry in policy decision-making without changing the potential influence of the state governor, due to existing power derived from the position. These findings align with other research on the association between the state governor and higher education officials (Tandberg et al., 2017), which may suggest that U.S. state executives maintain significant power over higher education due to institutionalized powers through appointment and, more generally, due to their position within the state.

Conclusion and Future Research

Despite state governing agency boards' central role in the operation and oversight of public higher education in the US (Longanecker, 2006; McGuinness, 2005; Morgan et al., 2021), their responsibilities regarding policy-making are not as well researched or understood as other key actors. Moreover, the limited research addressing this question focus on more partisan topics such as higher education finances and tuition setting. The current study aimed to contribute to this gap in the literature by examining the role of board members in the policy-making process around improving postsecondary degree attainment, and questioning whether board members would be more beholden to the state officials in charge of appointment (University System of Georgia) when compared to similarly positioned individuals selected by the voting public of the state (Nevada System of Higher Education) regarding responsiveness and prioritization to stakeholders. Although respondents from both cases suggested board members had a limited direct role in the policy process around this issue, insights were gained regarding the policy role of these individuals and the influence of their selection mechanism on their preferred sources of information. Moreover, considering the emergent themes within the framing of stakeholder salience offers additional academic purchase to this study and line of research.

Given the exploratory nature of the current study, there remain areas for future research. Researchers could reconstruct the current study around a policy area that board members are more centrally involved, such as personnel hiring, which may provide greater insight into specific roles of board members in decision-making. Additionally, although the two cases investigated were selected through purposive sampling, there remain characteristics among state governing agency board membership that were not addressed in this study, including the influence of having mixed appointment mechanisms, retaining members of the faculty or student body on the board, and the inclusion of *ex-officio* state officials among board members. Finally, given the significant attention afforded to state governors by board members in the current study, there would also be value in examining this direct relationship further regarding appointment mechanism and membership makeup of state governing agency boards. Ultimately, considering the central role state agency board members serve in governing public postsecondary education in the US, it is critical that more research is conducted around how they operate and ways in which various constituencies inform and influence their policy and oversight decision-making.

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Appendix A

Select Questions from Interview Protocol

1. How would you characterize the role of [state governing board] in [state]?
 - a. What do you consider to be your primary responsibilities as a member of [state governing board]?
2. How does the [state governing board] and its members navigate the various stakeholder perspectives interested in the operation of higher education?
3. As increasing postsecondary degree attainment rates has become a central policy concern nationally, how did college completion become part of the state's policy agenda?
 - a. Who brought the idea to the state's attention?
 - b. Were there any additional influential actors in the early stages of the completion agenda?
 - c. [State] is a member of Complete College America, has their involvement influenced the development of the completion agenda?
4. How similar or different has the development and implementation of the college completion agenda been to other policy issues?
5. How would you characterize the role of [state governing board] in the process of adopting [college completion policy]?
 - a. From your perspective, would you characterize this as a standard example of your role as a member of the [state governing board]?
6. What is the role of [state governor] in the policy process at [state governing agency]?
7. Given the public facing role of [state governing board], does the general citizenry play a role in policy decisions?
 - a. Did they play a role in the college completion agenda?
 - b. Is there a time when they have played a key role from your perspective?
8. When a policy arrives at [state governing agency/board], what is the process that you take to inform yourself about the merits of the initiative?
9. Are there any sources that stood out for the completion-related agenda?
10. Can you name a time that research changed your mind on a policy decision?

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The benefits of patronage: How political appointments can enhance bureaucratic accountability and effectiveness*

Guillermo Toral[†]

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Abstract

The political appointment of bureaucrats is typically seen as a rent-seeking strategy that helps politicians sustain clientelistic networks and manipulate public administration to their advantage. I argue that political appointments can also increase bureaucratic accountability and effectiveness because they provide political and social connections between bureaucrats and politicians. These connections grant access to material and non-material resources, enhance monitoring, facilitate the application of sanctions and rewards, align priorities and incentives, and increase mutual trust. In certain conditions, political appointments can thus enhance bureaucrats' accountability and effectiveness in public service delivery. I test this theory using data on Brazilian municipal governments, leveraging two quasi-experiments, two original surveys of bureaucrats and politicians, and in-depth interviews. The findings challenge the traditional view that patronage is universally detrimental to development, and highlight how political appointments and connections can be leveraged to enhance public service delivery.

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1 Introduction

The political appointment of bureaucrats –or patronage, in short¹– is ubiquitous throughout the developing world ([Grindle, 2012](#)). Patronage is typically understood as a rent-seeking strategy in which politicians build and maintain clientelistic networks and steer bureaucratic efforts for political and/or private gain, which hurts development. In contrast to this view, I propose a theory of how and when patronage can enhance bureaucrats’ accountability and effectiveness.² Political appointments and connections provide bureaucrats with “upward embeddedness”, i.e., social and political connections with politicians, which facilitate bureaucratic accountability and effectiveness. In particular, I argue that patronage gives bureaucrats access to material and non-material resources, facilitates monitoring by politicians, enables the application of sanctions and rewards, aligns priorities and incentives, and increases mutual trust. In certain contexts, patronage can thus make bureaucrats more accountable and effective at service delivery.

My argument is not that patronage is universally good, nor that it comes without costs. Indeed, the costs of patronage have long been recognized ([Pollock, 1937](#)), and recent studies have demonstrated how it can distort the allocation of public jobs and disincentivize bureaucratic performance ([Xu, 2018](#); [Colonnelli et al., 2020](#)). In contrast to this traditional view of patronage as a system that changes who enters the bureaucracy or how much they work, my theory emphasizes its influence on *how* they work. By changing how bureaucrats work, patronage can be leveraged to extract rents, but also to deliver public services. In this paper I focus on the often overlooked advantages of patronage, and offer a theory of the conditions under which they can benefit public service delivery.

I outline three scope conditions to identify the contexts in which the benefits of patronage are more likely to enhance governance. First, in contexts where there are no easy substitutes for the advantages upward embeddedness provides. This is often the case in developing settings, and particularly in poor and small localities, which typically have dire financial constraints, small labor markets, and limited human capital. In these settings, a competitive, merit-based recruitment

¹I use *patronage* to refer to the political appointment of bureaucrats or, more specifically, the discretionary appointment of bureaucrats by politicians based, at least partly, on political criteria or other non-merit-based factors. The concept of patronage is contested, and the literature contains a variety of definitions that are both narrower and broader than mine.

²I use *accountability* to refer to bureaucrats’ responsiveness to the demands of their principals (politicians and senior officials), and their career paths being affected by it. I use *effectiveness* to indicate bureaucrats’ success at delivering services and improving outcomes within their area of responsibility.

process is less likely to be sufficient for selecting and motivating effective bureaucrats. Second, patronage may be particularly beneficial for service delivery when appointing “street-level managers” (Gassner and Gofen, 2018), namely bureaucrats such as school directors³ or health clinic managers who lead public service delivery units. The effectiveness of these managers, who occupy a critical position in between senior officials and front-line providers, depends on their ability to motivate and coordinate street-level employees, and to align their work with both senior officials’ and citizens’ demands. Political appointments and connections help street-level managers achieve these two tasks. Third, politicians must at least partly value the effective delivery of public services. This is more likely to be the case in contexts with electoral accountability and strong oversight institutions.

The argument that patronage can improve service delivery builds on insights from political science, public administration, and economics. First, previous comparative politics research has highlighted that patronage can facilitate party building (Sorauf, 1960; Huntington, 1968), nation building (Weingrod, 1968), interest aggregation (Scott, 1969), and state building (Grindle, 2012). Second, the public administration literature often treats the politicization of the bureaucracy in high-income countries as a resource that politicians use to tighten their control over policy and implementation (Peters and Pierre, 2004; Bach and Veit, 2017) and to build party networks (Kopecky et al., 2012). Third, American politics scholars often view political appointments as helping presidents increase policy control over federal agencies (Lewis, 2008; Aberbach and Rockman, 2009). Such studies commonly theorize a trade-off between policy control and bureaucratic performance (Moe, 1985). Finally, development economists have recently explored the theoretical possibility that patronage may help politicians address selection and agency problems, although no empirical evidence has been uncovered to support this idea (Xu, 2018; Colonnelli et al., 2020).

I build on these contributions to develop a theory that links patronage to public service delivery and specifies testable mechanisms and scope conditions. Theoretically, this paper departs from economic theories of the bureaucracy that assume a loyalty-competence trade-off. I instead argue that, in low-capacity settings, connections between bureaucrats and politicians may help not only decrease agency losses but also enhance bureaucrats’ ability to do their jobs. Empirically, the paper contributes causal evidence of the benefits of patronage for service delivery in a developing context. Previous research has identified various governance advantages generated by connections among politicians at different levels of government (Jiang, 2018), among bureaucrats (Schneider, 1991), between politicians and party brokers (Brierley and Nathan, 2021), and between politicians and

³I use the term *school director* to refer to school leaders, also called principals, headmasters, or head-teachers.

citizens (Tsai and Xu, 2018). I contribute to this literature on the advantages of connections for governance by highlighting the value of ties between bureaucrats and politicians.

Empirically, I study patronage and its effects on bureaucratic effectiveness and accountability using three methods: quasi-experiments, surveys, and interviews. I focus on municipal governments in Brazil, a context where scholars have long noted the pervasiveness of patronage (Hagopian, 1996; Ames, 2001). A critical advantage of this environment is that political appointments coexist with other modes of bureaucratic selection, which enables comparisons within a single institutional environment and thus facilitates inference (Giraudy et al., 2019).

First, to test the core claims of the theory, I use two quasi-experimental studies that leverage administrative data on municipal schools throughout the country. The results of a difference-in-discontinuities study (hereafter diff-in-disc, a design that combines a difference-in-differences with a regression discontinuity) demonstrate that when politically appointed school directors lose their connections to the local government (after the mayor who appointed them is voted out) their schools experience a drop in quality, compared to schools with unappointed directors. This finding demonstrates that political connections can increase bureaucratic effectiveness. A separate regression discontinuity design (RDD) shows that politically appointed school directors who meet their target in a highly visible school quality indicator are less likely to be replaced, whereas meeting the target has no effect on the turnover of unappointed school directors. This result shows that patronage can enhance bureaucratic accountability.

Second, to test the mechanisms through which patronage can enhance bureaucratic effectiveness and accountability, I administered two original surveys in the Brazilian state of Rio Grande do Norte. The first was a face-to-face survey of 926 street-level managers (school directors, clinic managers, and social assistance center coordinators), representative of urban areas in 150 municipalities. Observational regressions indicate that appointed bureaucrats have more frequent contact with, higher levels of trust in, and better alignment with politicians than unappointed bureaucrats do. The results of a conjoint experiment embedded in the survey also show that managers expect bureaucrats who are politically appointed or connected to communicate better with the government, to be more responsive to its demands, and to be more effective at raising funds from it. The findings of a separate online survey of 455 local politicians corroborate these results. Politicians' responses in another conjoint experiment reveal that they perceive bureaucrats with political connections as being more responsive, better at communicating with them, and more likely to exert greater effort.

Third, I used in-depth interviews to investigate the informal institutions of bureaucratic politics in Brazilian local governments, develop hypotheses, and probe mechanisms. During 18 months of fieldwork, I conducted 121 in-depth interviews with bureaucrats, politicians, and anti-corruption actors (such as auditors and prosecutors) in 45 municipalities in seven states across three different regions of Brazil.⁴ Specific accounts from local actors in widely diverging contexts help illustrate how appointments work in practice, as well as the costs and benefits of patronage.

The finding that political appointments and connections can improve bureaucratic accountability and effectiveness has at least three important implications for both researchers and policymakers. First, the paper suggests that, in some contexts, reforms designed to insulate local bureaucrats from politicians can have detrimental effects on service delivery, at least in the short term and when not complemented by significant increases in human capital that would strengthen the capacity of more autonomous bureaucrats. The paper thus contributes to an emerging literature on the costs of anti-corruption strategies ([Rasul and Rogger, 2018](#); [Jiang et al., 2020](#); [Wang, 2021](#)) and on how bureaucratic arrangements that deviate from the Weberian ideal can aid development ([Ang, 2016](#); [McDonnell, 2020](#)). Second, the paper helps reconcile the standard view of patronage as a form of corruption with other views linking it to political development. Patronage can facilitate both rent-seeking and public service delivery precisely due to the governance advantages that upward embeddedness provides. This helps explain the remarkable resilience of patronage around the world ([Grindle, 2012](#)). Finally, the paper advances our understanding of the mechanisms through which political appointments facilitate policy control and implementation, and thus helps bridge the gap between comparative politics research on patronage in developing contexts, on the one hand, and public administration and American politics research on political appointments in high-income countries, on the other.

2 Theory

2.1 How patronage delivers

Patronage is usually understood as a rent-seeking strategy. Scholars have long studied the critical role of government jobs in clientelistic equilibria ([Wilson, 1961](#); [Robinson and Verdier, 2013](#)). From this perspective, patronage is thought to impede development through the misallocation of public jobs and the depression of bureaucratic effort ([Xu, 2018](#); [Colonnelli et al., 2020](#)).

⁴See Appendix [B](#) for additional details on the interviews.

I advance an alternative view of patronage that emphasizes its potential benefits for bureaucratic accountability and effectiveness. I start by proposing the concept of “upward embeddedness” to refer to bureaucrats’ political and social connections to politicians. Scholars of bureaucratic governance often use the idea of embeddedness ([Granovetter, 1985](#)) to describe bureaucrats’ (downward) relations to local communities and how they can foster government effectiveness ([Evans, 1995](#); [Tsai, 2007](#); [Bhavnani and Lee, 2018](#)). Applying the concept of embeddedness upward (i.e., in relation to politicians instead of societal actors) enables a richer view of connections between bureaucrats and politicians than Weberian theories allow. From a Weberian perspective, it is the insulation of bureaucrats from politicians that increases bureaucratic effectiveness ([Dahlström and Lapuente, 2017](#); [Oliveros and Schuster, 2018](#)). The concept of upward embeddedness also helps integrate different types of connections within bureaucracies into a single framework, including connections based on partisanship ([O’Dwyer, 2006](#)), family ([Fafchamps and Labonne, 2017](#)), and ethnicity ([Hassan, 2020](#)).

I argue that political appointments and connections upwardly embed bureaucrats, which provides a set of governance resources that can be beneficial for both bureaucrats and politicians. Depending on how these resources are used, patronage can enhance either rent seeking or public service delivery. In any case, upward embeddedness operates through five mechanisms: increasing bureaucrats’ access to material and non-material resources, helping politicians monitor bureaucrats, facilitating the application of sanctions and rewards, aligning priorities and incentives, and increasing mutual trust. In the remainder of this section I investigate each of these mechanisms and their relevance for bureaucratic governance in turn.

First, upward embeddedness increases bureaucrats’ access to political leaders and, through them, to material resources for public service delivery (e.g., funds), as well as non-material resources (e.g., legitimacy and authority). Both help mobilize and coordinate other bureaucrats. Prior studies have demonstrated that resources and legitimacy are important drivers of bureaucratic effectiveness ([Dasgupta and Kapur, 2020](#); [Carpenter, 2001](#)).

Second, upward embeddedness facilitates politicians’ monitoring of bureaucrats and reduces information asymmetries through shared political and social networks. Although in high-corruption contexts enhanced monitoring may fuel rent extraction ([Brierley, 2020](#)), in other development contexts bureaucratic oversight has been shown to improve government effectiveness ([Gulzar and Pasquale, 2017](#); [Raffler, 2020](#)).

Third, upward embeddedness enhances bureaucrats' accountability to politicians by facilitating the application of formal and informal sanctions and rewards. This motivates bureaucrats to exert more effort, and makes them more responsive to politicians' demands. Political appointees are usually hired at will, which makes it easier to sanction bad performers (through firing) and reward good performers (through promotions). Transfers can be used for both sanctions and rewards (Iyer and Mani, 2012; Khan et al., 2019), and career incentives and extrinsic rewards can improve bureaucratic effectiveness (Ashraf et al., 2014; Bertrand et al., 2020). Shared social and political networks also enhance the application of informal sanctions and rewards.

Fourth, upward embeddedness fosters the alignment of priorities and values between bureaucrats and politicians by virtue of their common political background and shared networks. Bureaucrats often operate in highly complex environments that require them to multi-task and to negotiate contradictory priorities from different societal actors (Lipsky, 1980; Dasgupta and Kapur, 2020). In such challenging environments, the alignment of bureaucrats' and politicians' priorities can improve service delivery. Management scholars have long recognized the importance of alignment for organizations' performance (Biggs et al., 2014), and political scientists have shown political alignment can improve policy implementation (Williams, 2017; Niedzwiecki, 2018). Upward embeddedness also synchronizes the incentives of bureaucrats and politicians, given their shared fate. Unlike civil service bureaucrats, political appointees are usually fired after a change in government, which ties their incentives to those of the incumbent (Oliveros, 2021). As the formal literature on delegation has long recognized through the ally principle (Bendor et al., 2001), alignment between bureaucrats and politicians can alleviate principal-agent problems.

Finally, by reinforcing their shared political and social networks and aligning their priorities and incentives, upward embeddedness fosters mutual trust between bureaucrats and politicians. Abundant evidence from psychology shows that trust improves organizations' performance by lowering transaction costs and increasing compliance (Kramer, 1999). Together with the alignment of priorities and incentives, trust may also decrease the need for monitoring.

In summary, political appointments and connections foster bureaucrats' upward embeddedness, which provides a number of governance resources, namely access to material and non-material resources, monitoring technology, better ability to apply sanctions and rewards, alignment of priorities and incentives, and increased trust. There is, however, an inherent ambivalence in patronage. On the one hand, political appointments and connections can make political machines more effective at extracting rents. On the other hand, upward embeddedness can help governments deliver public

services more effectively, because service delivery also depends on the coordination and accountability of bureaucrats. I do not claim that patronage does not have costs: it may, for example, lead to the selection of bureaucrats with less education and experience. Rather, I argue that patronage has under-appreciated benefits for bureaucratic accountability and effectiveness that, in certain contexts, can be leveraged for service delivery.

2.2 Scope conditions

The benefits of patronage will be greater in contexts in which there are no easy substitutes for the governance advantages it provides. This is true in developing contexts, and particularly for local governments outside large metropolitan areas, where financial constraints are more dire and human capital is scarcer. This reduces governments' ability to use higher wages ([Dal Bó et al., 2013](#)) or performance pay ([Hasnain et al., 2014](#)) to improve bureaucratic selection and performance. In these challenging environments, the counterfactual to a political appointee is not necessarily the highly capable, autonomous and driven bureaucrat that Weberian theories presume. Without adequate human capital and incentives, civil servants may simply lack the capacity and motivation to deliver services. Patronage can thus alleviate developmental constraints on bureaucratic governance.

The effectiveness of street-level managers (e.g., school directors or clinic managers) is particularly likely to benefit from the advantages of upward embeddedness. These managers can significantly improve the quality of public services ([Bloom et al., 2015](#); [Tavares, 2015](#)), but their effectiveness depends on their ability to coordinate efforts and align a complex set of tasks to objectives that are often multidimensional and hard to assess ([Lipsky, 1980](#)), especially in transaction-intensive services like healthcare or education ([Pritchett and Woolcock, 2004](#)). Upward embeddedness can help street-level managers leverage the necessary trust, legitimacy, and the ability to coordinate efforts and align teams ([Gassner and Gofen, 2018](#)).⁵

For the benefits of patronage to outweigh the costs, politicians must value public service delivery, be it due to intrinsic beliefs and norms ([Habyarimana et al., 2018](#)), political competition ([Gottlieb and Kosec, 2019](#)), electoral accountability ([Healy and Malhotra, 2013](#)), or anti-corruption institutions ([O'Donnell, 1998](#)). The availability of regular and credible measures of bureaucratic

⁵In contrast, the effectiveness of frontline providers like teachers or doctors is less dependent on their ability to coordinate efforts with other bureaucrats or with senior officials. The effectiveness of low-skill employees like clerks and cleaners is the least likely to benefit from the advantages of patronage because their tasks are simpler and easier to monitor.

performance can strengthen politicians' concerns about service delivery through all these mechanisms. Where politicians do not value service delivery, the governance advantages of upward embeddedness are more likely to be mobilized for extracting rents.

3 Institutional context

3.1 Formal institutions in Brazilian municipalities

Brazilian local governments are a particularly useful setting in which to study the impact of political appointments and connections on bureaucratic accountability and effectiveness, for three main reasons. First, local governments use a variety of systems to appoint bureaucrats. This enables comparisons within a single institutional environment, which makes inference easier ([Giraudy et al., 2019](#)). Second, the federal government publishes rich datasets about local bureaucracies. This facilitates measuring variation in local governance and service delivery. Third, Brazilian municipalities vary widely in their economic and political development. This variation makes them a relevant case for other contexts beyond Brazil.

Brazil is a federal country with 5,570 municipalities, which together are responsible for providing primary education, healthcare, and social assistance to over 200 million people ([Arretche et al., 2020](#)). Local governments spend in average around 60% of their revenue (which mostly comes from inter-governmental transfers) in these three sectors.⁶ Financial constraints are usually dire, and employees' salaries are low.⁷ Due to municipalities' prominent role in service provision and to the scarcity opportunities in the private sector, local governments are typically a very important employer, hiring on average 4.7% of the local population and 38.2% of those who have jobs in the formal sector (Appendix [A.1](#)). From the perspective of the local government, however, these are small labor markets with low levels of human capital.⁸ Most municipalities are small (with a median population of fewer than 12,000 people) and far from state capitals, which makes it hard to attract talent.

Municipal elections are held every four years to elect a mayor (through a majoritarian system) and city councilors (though a proportional, open-list system).⁹ Mayors, who can run for re-election

⁶My own calculations using data from the National Treasury.

⁷The median municipal employee earned a monthly salary of 1,763 Brazilian reais in 2016 (roughly two minimum salaries or USD445).

⁸For example, management skills in public schools and hospitals are low (Appendix [A.2](#)).

⁹State and federal elections are held two years before/after municipal elections.

only once, appoint secretaries to run specific policy areas. Mayors, city councilors, and secretaries are overseen by a network of horizontal accountability institutions (including audit courts, prosecutors' offices, and standard courts) that have been shown to reduce rent extraction (Avis et al., 2018; Litschig and Zamboni, 2019). Federal and state governments also oversee municipal governments, especially on their use of transfers and on their performance in delivering public services paid for by these transfers (Gomes, 2009). The federal government regularly measures and publicizes the performance of municipal bureaucracies in several areas.

Municipal governments maintain a network of schools, health clinics, and social assistance centers to serve the local population. While there are strict legal constraints on how frontline providers (e.g., teachers) are hired,¹⁰ laws give politicians considerable discretion for the selection of street-level managers (e.g., school directors). Managers are typically political appointees, but they can also be elected (by community members or professionals) or deployed through the civil service (tenured for life after passing a competitive examination).¹¹ Among the managers I surveyed, political appointees have significantly lower levels of education than those chosen via other methods, are more likely to live in the municipality where they work, and are more likely to have worked for a local electoral campaign (Appendix E.3). This demonstrates that appointments are based on political criteria rather than merit. Multiple appointment systems often coexist within the same municipality, particularly in the education sector.¹² Government data on education show that political appointments are more common in smaller and poorer municipalities, and in schools serving students of a lower socioeconomic status (Appendix A.3).

Municipal politicians generally value public service delivery, at least partly. Recent experimental evidence shows that Brazilian mayors value high-quality evidence on policy effectiveness, update their priors in response to such evidence, and use it to improve public programs (Hjort et al., 2021). My survey of politicians provides some additional evidence. A large majority of the mayors (76%) declared that they have the highest level of responsibility for improving the quality of public services, out of a list of seven actors. Secretaries of education, healthcare and social assistance report, on average, about one weekly meeting with street-level managers in their area.

¹⁰For example, the Brazilian constitution stipulates that bureaucrats are by default to be hired under a civil service regime. In practice, many of them are employed under temporary contracts.

¹¹Civil service managers cannot be fired, but they can be transferred to a different unit.

¹²As of 2017, about 65% of municipal directors were appointed by politicians, 24% elected by the community, 5% tenured in a civil service regime, and the rest were appointed through other means.

3.2 Informal institutions in bureaucratic appointments

Interviews with bureaucrats and politicians provided valuable insights into the informal institutions that govern bureaucratic appointments in Brazilian municipalities. In practice, mayors usually appoint street-level managers, sometimes in consultation with city councilors in their coalition or with the corresponding secretary. Secretaries are in charge of selecting temporary street-level bureaucrats (like teachers or nurses), who generally hold one-year contracts. It is generally secretaries, and not street-level managers, who control the hiring, firing, or transferring of street-level bureaucrats.¹³

Street-level manager posts are important for politicians, given their strategic position in social networks, their visibility within the community, and their wide territorial reach in both urban and rural areas. In clientelistic settings, street-level managers can be useful for political mobilization. An elected director reported that “at the time of elections, [a previous, appointed director] asked school staff to wear the party’s t-shirt, intimidating temporary teachers with the possibility of them losing their contract, and intimidating tenured teachers with them being transferred to another school. [...] People were expected to go to the city councilor’s rally.”¹⁴ During my interviews, bureaucrats under different appointment systems and politicians conveyed multiple accounts like this, stressing how the resources, monitoring, and accountability of upward embeddedness were used for extracting rents.

More frequently, interviewees reported that political connections were leveraged to improve public services. Both street-level managers and secretaries suggested that the political appointment system pressures bureaucrats to work more and to be more responsive to community needs and local government demands. For example, one secretary said: “our directors are political appointees, but we do it with some criteria, including that they have a university degree, that they live in the community, that they communicate well [...]. But it has to be someone we trust, that’s why political appointments matter.” When I asked them what trust was important for, they said: “to meet deadlines, to implement programs within the law, to treat families well, and to be a bridge between the government and the families – whether we like it or not, the director is a very political position, they relate to many people, they manage many people.”¹⁵

Interviewees also mentioned the advantages of political connections for bureaucratic account-

¹³See Appendix A.4 for diagrams of accountability relationships under different appointment modes.

¹⁴School director interviewed in the state of Rio de Janeiro in February of 2017.

¹⁵Secretary of education interviewed in the state of Paraíba in August 2018.

ability. For example, one director said that “when the director is appointed they want to measure up to the invitation that was made to them. But the person who became director because they passed a test thinks they have that position because of a test and that they owe nothing to anybody.”¹⁶ When I asked a secretary whether they felt any difference in the relationship to elected versus appointed directors, they said: “yes, absolutely. One would expect elected directors to be better, that we would see them more committed. But it is quite the opposite, it’s as if elected directors felt that it was the people who gave them the post and thus they owe nothing to the secretariat.”¹⁷

Finally, street-level managers recounted how political connections helped increase the material and non-material resources they can leverage on the job. For example, when I asked a bureaucrat what connections were valuable for, they said: “Things are really hard with connections already, I do not know what I would do without them. [...] For example, we do not have running water in the center, and it is thanks to political connections that I manage to get a water truck to come and fill our tank. That requires an articulation with the secretary of transportation and other actors – I only manage that thanks to my connections to the mayor.”¹⁸

Taken together, my interviews with municipal street-level managers and secretaries suggest that the political appointment of bureaucrats can respond to concerns about both rent-seeking and service provision. Political appointments may come with some costs (like the deployment of bureaucrats with less education or experience), but politicians often appoint street-level managers based on their professional abilities –not just their political ones– and leverage political connections to improve public services.

While the interviews helped me develop and probe hypotheses, as well as design quasi-experiments and interpret their results, it is hard to quantify relationships using qualitative data. The next section turns to quantitative evidence.

4 Empirical evidence

I leverage three types of data and methods to test whether upward embeddedness enhances the accountability and effectiveness of street-level managers. First, I use a diff-in-disc to show that an electoral defeat of the mayor causes a drop in the quality of schools with appointed directors, relative

¹⁶School director interviewed in the state of Goiás in March 2017.

¹⁷Secretary of education interviewed in the state of Paraíba in August 2018.

¹⁸Social assistance center coordinator interviewed in the state of Rio Grande do Norte in December 2018.

to schools with unappointed directors. This shows that upward embeddedness helps bureaucrats deliver public services. Second, I use an RDD to show that appointed directors (but not elected or civil service ones) are less likely to be replaced if they meet their school quality target. This is consistent with upward embeddedness enhancing bureaucratic accountability, and with politicians caring about public service delivery. Third, I leverage original surveys of bureaucrats and politicians, including conjoint experiments, to show that upwardly embedded bureaucrats have more access to, trust in, and alignment with politicians. They are also perceived as communicating better with and being more responsive to the government, exerting more effort, and raising more resources. Together, these three sets of causally identified evidence, together with the interviews, demonstrate that bureaucrats' upward embeddedness can enhance their accountability and effectiveness. Table 1 synthesizes the links between the theory and the empirical tests presented below.

Table 1: Mapping of theory to empirics

Theoretical claims	Test	Data	Evidence
<i>Core arguments: Upward embeddedness facilitates...</i>			
... bureaucratic effectiveness in service delivery	Diff-in-disc	Administrative data	Figure 2
... bureaucratic accountability	RDD	Administrative data	Figure 3
<i>Mechanisms: Upwardly embedded bureaucrats...</i>			
... have higher levels of trust in, alignment with, and access to politicians	Correlations	Bureaucrat survey	Figure 4
... communicate better with and are more responsive to the government	Conjoint	Bureaucrat and politician surveys	Figures 5 and 6
... have more access to material resources	Conjoint	Bureaucrat survey	Figure 5
... exert more effort	Conjoint	Politician survey	Figure 6

4.1 Losing political connections makes appointed bureaucrats less effective: Difference-in-discontinuities evidence

A central idea to the theory in section 2 is that upward embeddedness helps politically appointed bureaucrats deliver public services. If that is the case, political turnover should differentially impact the effectiveness of appointed and unappointed bureaucrats. For appointed bureaucrats, mayoral turnover means a decrease in upward embeddedness, and therefore a reduction in the governance resources that help them deliver public services. However, mayoral turnover does not change the upward embeddedness of unappointed bureaucrats. Both types of bureaucrats are exposed to the general effects of political turnover, including the organizational costs of transition, the benefits

of a renewed leadership, and other shocks to the bureaucracy (Akhtari et al., 2020; Toral, 2021). If my theory is correct, appointed bureaucrats' performance should worsen as a result of political turnover compared to their unappointed counterparts.

To exploit the differential impact of political turnover on upward embeddedness, I use a diff-in-disc (Grembi et al., 2016). In essence, this design combines a difference-in-differences (comparing the performance of appointed and unappointed bureaucrats, before and after the election) with a close-races regression discontinuity (comparing the performance of bureaucrats in municipalities where the mayor lost their bid for re-election to those in municipalities where the mayor was re-elected).¹⁹ I use data for municipal school directors, for whom the federal government releases a performance metric (based on student test scores and passing rates) every two years as well as an administrative survey that includes data on their mode of selection. The results show that an electoral defeat of the mayor causes a 0.25-standard-deviation drop in the quality score of schools with appointed directors compared to those with unappointed directors ($p < 0.05$).

4.1.1 Design

The design exploits two treatments: whether a municipality m experiences political turnover (P_m), and whether a school s experiences a decrease in upward embeddedness after the election (U_{sm}). The political turnover treatment is assigned by the difference between the vote share of the strongest challenger (V_m^c) and that of the incumbent (V_m^i): $D_m = V_m^c - V_m^i$. If this forcing variable is above 0, the municipality experiences political turnover; otherwise the mayor is re-elected and there is no political turnover. The upward embeddedness treatment is assigned by whether the municipality experiences political turnover and the school director had been appointed by the mayor (A_{sm}):

$$P_{sm} = \begin{cases} 1 & \text{if } D_m > 0 \text{ (mayor loses re-election)} \\ 0 & \text{otherwise} \end{cases} \quad (1)$$

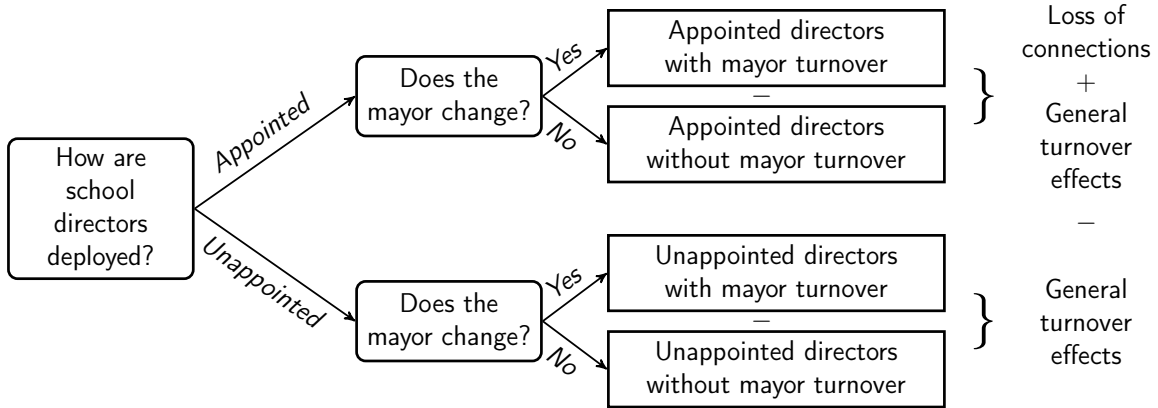
$$U_{sm} = \begin{cases} 1 & \text{if } D_m > 0 \text{ and } A_{sm} = 1 \text{ (mayor loses re-election, director was appointed)} \\ 0 & \text{otherwise} \end{cases} \quad (2)$$

To separate the effect of a decrease in upward embeddedness from that of political turnover, I

¹⁹I focus on the electoral performance of the mayor rather than their political party because this setting is characterized by weak partisan attachments and pervasive party switching by politicians (Boas et al., 2019; Klačnja and Titunuk, 2017). Incumbents often switch parties before seeking re-election (e.g., 26% mayors did so in 2016), so examining the electoral performance of the incumbent party would be misleading.

exploit the difference between appointed directors (who lose upward embeddedness when the mayor fails to win the re-election) and unappointed directors (whose upward embeddedness is not affected by political turnover). Figure 1 illustrates the logic of the design.

Figure 1: Diagram of the difference-in-discontinuities design



The design exploits three sources of variation in performance: within-bureaucrat changes from before to after the election, between appointed and unappointed bureaucrats, and between localities with and without political turnover.

Potential outcomes are a function of both $P_{sm} = p \in \{0, 1\}$ and $U_{sm} = u \in \{0, 1\}$, so we can define them as $Y_{sm}(p, u)$. With that notation, the estimand of interest is:

$$\tau_{disc} = \mathbb{E}[Y_{sm}(1, 1) - Y_{sm}(0, 0) | D_m = 0, A_{sm} = 1] - \mathbb{E}[Y_{sm}(1, 0) - Y_{sm}(0, 0) | D_m = 0, A_{sm} = 0] \quad (3)$$

We can identify the local average treatment effect (LATE) around the cutoff by taking the difference in means from below and above the threshold for each type of school, and subtracting them:

$$\hat{\tau}_{disc} = \left(\lim_{D_m \downarrow 0} \mathbb{E}[Y_{sm} | D_m = 0, A_{sm} = 1] - \lim_{D_m \uparrow 0} \mathbb{E}[Y_{sm} | D_m = 0, A_{sm} = 1] \right) - \left(\lim_{D_m \downarrow 0} \mathbb{E}[Y_{sm} | D_m = 0, A_{sm} = 0] - \lim_{D_m \uparrow 0} \mathbb{E}[Y_{sm} | D_m = 0, A_{sm} = 0] \right) \quad (4)$$

This design relies on three assumptions (Grembi et al., 2016). First, potential outcomes should be continuous in the forcing variable around the threshold. Appendix C.1 shows that pre-treatment covariates are continuous around the cutoff. Second, the effect of political appointment when there is no change to upward embeddedness should be constant over time, such that schools with appointed and unappointed directors follow parallel trends. Appendix C.2 shows that schools with

appointed and unappointed directors, as well as schools in municipalities with and without political turnover, follow parallel trends in performance before the election. With these two assumptions, the diff-in-disc estimates the causal effect of a decrease in upward embeddedness, close to the threshold, and for appointed directors. If we make a third homogeneity assumption that the effects of the decrease in upward embeddedness and of political turnover do not interact, then we can recover the LATE of a decrease in upward embeddedness for schools in municipalities around the threshold.

The design focuses on within-director changes in performance, from before to after the election. I conduct this analysis on the subset of schools for which the director was assigned in the three years before the election, and was still in their post one year after the election. Schools that experience director turnover in this period are excluded from the sample because their changes in performance cannot be associated to changes in the director's upward embeddedness. However, since director turnover increases after the election, this may introduce sample selection bias ([Heckman, 1979](#)). In Section 4.1.4 I discuss this issue more fully, and show that it is likely to bias my results towards zero, that removing part of that bias increases the size of the effect, and that bounds that account for the worst possible case of sample selection bias remain fully below zero.

The design focuses on the relatively short-term effects of a decrease in upward embeddedness. Elections took place in October of 2016, winners took office on January 1st of 2017, and the next student tests were done in October and November of that of that same year. Improving student learning is a complex task that requires long-term efforts. Still, actions taken in the months leading up to the tests can have a significant impact on the results, including offering special remedial classes, doing practice tests, and raising awareness among teachers and students of the importance of student evaluations. All of these actions depend critically on directors' management efforts and their coordination with both school staff and the local government.²⁰

4.1.2 Estimation and inference

I use local linear regression with a triangular kernel, as recommended by [Cattaneo et al. \(2019\)](#), within the bandwidth selected by the [Calonico et al. \(2020\)](#) algorithm,²¹ and apply it to the following estimating equation:

²⁰Consistent with short-term director actions having the potential to improve school performance, civil society organizations and governments regularly produce materials to help directors prepare schools for the tests in the months immediately before them. Appendix A.6 includes a sample of such materials.

²¹Results of both the diff-in-disc and the RDD are similar when using a uniform kernel.

$$Y_{sm} = \alpha + \beta_1 P_m + \beta_2 D_m + \beta_3 P_m D_m + A_{sm}(\gamma_1 + \gamma_2 P_m + \gamma_3 D_m + \gamma_4 P_m D_m) + \sum_{k=1}^K \eta_k X_{sm}^k + \varepsilon_{sm} \quad (5)$$

Y_{sm} is the change in the quality score of school s in municipality m , from 2015 to 2017. $\sum_{k=1}^K \eta_k X_{sm}^k$ is a set of state fixed effects and director-, school-, and municipality pre-treatment covariates that significantly predict whether the director is politically appointed,²² which I include in some specifications to partially address the endogeneity of appointment modes. ε_{sm} is the error term. Standard errors are clustered at the municipality level, where political turnover is determined. If the diff-in-disc assumptions hold, γ_2 identifies the LATE, around the threshold, of a decrease in upward embeddedness.

4.1.3 Data

I leverage regular, valid, and well-established measurements of school performance conducted by the federal government every two years through the National Assessment of School Performance (ANRESC, *Avaliação Nacional do Rendimento Escolar*).²³ This system tests students at the end of primary and middle school in public schools across the country. Exams are based on item response theory, which ensures that its measures of learning outcomes are valid and comparable over time. ANRESC also surveys school directors and teachers, collecting data about their appointment, experience, demographics, and perceptions of the school. The federal government uses the test results and administrative data on student passing rates to calculate for each school a score in the Basic Education Development Index (IDEB, *Índice de Desenvolvimento da Educação Básica*). IDEB scores are normalized to range from 0 to 10. The system is managed by INEP, an autonomous, high-capacity federal agency (Bersch et al., 2017). All in all, and in words of a group of World Bank economists, ANRESC is “one of the world’s most impressive systems for measuring education results” (Bruns et al., 2012, 7).

I use IDEB data for all municipal primary schools, in the years immediately before and after the 2016 elections.²⁴ I use the government’s survey of directors to identify schools where the director was deployed in the years leading to the election and remained their post one year after, and to identify their mode of selection. I merge the school-level data with data on municipal election

²²Appendix A.3 reports the results of the correlational regressions of appointment modes on covariates.

²³Appendix A.5 details the timeline of ANRESC tests and the publication of results.

²⁴I focus on the 2016 election because before 2013 the questions on director turnover and on director appointment mode have different response options and higher levels of non-response.

candidates and their performance, obtained from Brazil's Supreme Electoral Court.

4.1.4 Results

Table 2 reports the diff-in-disc results. A decrease in upward embeddedness (identified by the differential effect of political turnover on appointed versus unappointed directors) damages school performance. In particular, as shown in model 2, it reduces the school quality score by 0.28 points or 0.25 standard deviations ($p < 0.05$). Figure 2 illustrates the two discontinuities on which the design is based. The results are robust to the inclusion of state fixed effects and covariates predictive of appointment (models 3-4) and to alternative bandwidths (Appendix C.3).

Table 2: Difference-in-discontinuity estimates of the differential impact of political turnover on school quality for appointed versus unappointed directors

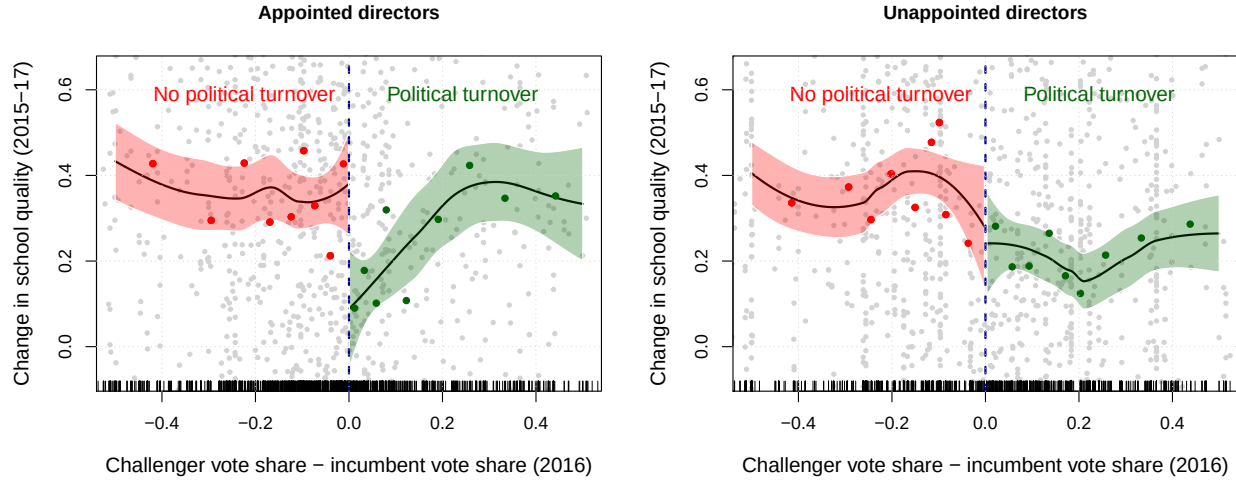
	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Political turnover	-0.161* (0.069)	-0.000 (0.097)	-0.007 (0.095)	0.003 (0.094)
$\hat{\gamma}_2$: Political turnover $\times$ Appointed		-0.283* (0.128)	-0.316* (0.124)	-0.377** (0.125)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.204	0.206	0.206	0.206
N	1628	1623	1623	1569

Predictors of whether the director is appointed come from a regression detailed in Appendix A.3. Municipality-clustered standard errors in brackets. * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

A potential concern with this design is that it may suffer from sample selection bias because, in order to examine within-director changes in performance, schools for which the director changed after the election are excluded from the sample. Director turnover, however, is directly affected by mayor turnover (Appendix C.4). This generates groups of schools (under mayor re-election and mayor turnover) that are not necessarily comparable. I address this issue through three complementary strategies. First, I show in Appendix C.5 that when the mayor is not re-elected, directors with better performance at baseline or who have a number of characteristics associated with performance are significantly more likely to stay in their post.²⁵ This implies that including in the

²⁵This is consistent with several interviewees' reports that school performance is an important input when a new government decides which directors to keep. For example a school director interviewed in the state of Goiás in March 2017 said, "political appointment makes sense, it's a position of trust – but when the government changes, and the director has made a good job (with a good diagnosis, a good IDEB score, has sent paperwork in time...) he gets to stay."

Figure 2: Effect of political turnover on school quality, by director appointment mode



Colored dots are local averages for equally-sized bins. Lines are loess regression lines estimated at both sides of the threshold with no controls. Shaded regions denote 95% confidence intervals.

analysis schools without mayor turnover that would have changed directors if there had been mayor turnover biases the results towards zero. Second, I show in Appendix C.6 that diff-in-disc estimates are larger and significant when pre-processing the data with matching on the covariates that predict directors to be replaced after mayor turnover. Third, I show in Appendix C.7 that sharp bounds adapted from Lee (2009) to this diff-in-disc setting remain fully below zero. This suggests that, even in the worst-case scenario of sample selection bias, the diff-in-disc estimate would be negative.

To explore potential mechanisms behind the results in Table 2, I use the ANRESC director survey, and in particular survey items about obstacles directors find while managing the school. Consistent with the theory of upward embeddedness, political turnover differentially depresses appointed directors' responses to a binary question about whether their work is supported by higher instances (Table 3).²⁶ On the other hand, I find no evidence in favor of alternative mechanisms that would not support the theory: there are no significant, differential changes in directors' reports of problems with financial resources, the supply of teachers, or teacher turnover (Appendix C.8). Another alternative mechanism might be that appointed directors who remain in their post after mayor turnover anticipate being replaced later. Yet, most directors who are not replaced in the first year of a new administration continue in their post three years after the election.²⁷

²⁶This result is also robust to alternative bandwidths (Appendix C.3).

²⁷63.3% of the directors that enter the diff-in-disc sample with mayor turnover remained in their post in late 2019, versus 69.7% in the re-election group. Others could have been relocated to another school.

Table 3: Difference-in-discontinuity estimates of the differential impact of political turnover on directors reporting their work is supported by higher instances, for appointed versus unappointed directors

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Political turnover	-0.059 (0.049)	0.089 (0.083)	0.070 (0.080)	0.069 (0.081)
$\hat{\gamma}_2$: Political turnover $\times$ Appointed		-0.239* (0.110)	-0.229* (0.110)	-0.267* (0.104)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.178	0.177	0.177	0.177
N	1587	1583	1583	1528

See notes under Table 2.

In sum, I find that a mayor's lost bid for re-election hurts the quality of schools with directors that had been appointed by them, when compared to schools with unappointed directors. This demonstrates that a drop in upward embeddedness jeopardizes bureaucratic effectiveness. The results therefore suggest that patronage can be leveraged to improve public service delivery, and that upward embeddedness can substitute for at least some of the potential disadvantages of political appointees.

4.2 Appointed bureaucrats are held accountable for their performance in a service delivery indicator: Regression discontinuity evidence

To test whether the political appointment of bureaucrats enhances accountability, I leverage an RDD in which I study the effect of schools meeting their quality target on the probability that the director is replaced. Among schools with appointed directors, meeting the quality target reduces director turnover by 0.19 standard deviations ($p < 0.01$). For schools with elected or civil service directors, the rate of director turnover is not affected by whether they meet their target. These results support the hypothesis that political appointments enhance accountability.

4.2.1 Design

Along with establishing IDEB as a system for measuring the quality of public schools, the federal government defined targets for the period 2007 to 2021 using an algorithm that projected schools' progress along logistic trajectories (Fernandes, 2007). Thus, every two years schools receive a

quality score for their performance, which can be compared to their pre-defined target for that year. If the difference between the score and the target is zero (or above), the school met (or surpassed) its target. If the difference is negative, it missed its target. I exploit this discontinuity to measure the causal effect of a school meeting its target in the 2013 test (the results of which were published in 2014) on its director being replaced between 2014 and 2015, and to explore heterogeneity by the director's appointment type.²⁸ IDEB scores are widely disseminated, and the emphasis is usually on whether targets were met (Boas et al., 2020). While local actors have other sources of information about the quality of schools, IDEB scores serve as simple, reliable metrics and thus facilitate common knowledge and accountability.²⁹

More formally, treatment for school s (meeting the school quality target), T_s , is assigned by the difference between its quality score and target ($D_s = score_s - target_s$):³⁰

$$T_s = \begin{cases} 1 & \text{if } D_s \geq 0 \quad (\text{quality score} \geq \text{quality target}) \\ 0 & \text{if } D_s < 0 \quad (\text{quality score} < \text{quality target}) \end{cases} \quad (6)$$

The estimand of interest is $\tau = \mathbb{E}[Y_s(1) - Y_s(0)]$, where $Y_s(1)$ and $Y_s(0)$ represent the potential outcome of interest (director turnover in school s), under treatment (having met the target) and under control (having missed it). We can identify the LATE around the cutoff by taking the difference in means from above and below the threshold:

$$\hat{\tau}_{rdd} = \lim_{D_s \downarrow 0} \mathbb{E}[Y_s(1) | D_s = 0] - \lim_{D_s \uparrow 0} \mathbb{E}[Y_s(0) | D_s = 0] \quad (7)$$

The key assumption of this design is that potential outcomes are continuous around the threshold. Reassuringly, the forcing variable and pre-treatment covariates are continuous around the cutoff (Appendix D.1).

²⁸I focus on 2013-2015 to avoid years with municipal elections.

²⁹Note, for example, that actors in financial markets also respond to binary signals (e.g. credit rating downgrades) despite being in a much thicker information environment (Ismailescu and Kazemi, 2010).

³⁰While the government uses figures with one decimal only, I use a continuous measure to increase statistical power and avoid issues associated with discrete forcing variables in RDDs (Lee and Card, 2008). Since -0.05 in the continuous measure is equivalent to 0 with the rounding applied by the government, I re-center the forcing variable by adding 0.05.

4.2.2 Estimation and inference

I use local linear regression with a triangular kernel (Cattaneo et al., 2019) within the optimal bandwidth (Calonico et al., 2020), applied to the following estimating equation:

$$Y_s = \alpha + \beta_1 T_s + \beta_2 D_s + \beta_3 T_s D_s + \varepsilon_s \quad (8)$$

Y_s indicates whether school s experienced director turnover between 2014 and 2015. ε_s is the error term. If the RDD assumptions hold, β_1 identifies the LATE in Equation 7. For inference, I use the HC1 heteroskedasticity-consistent estimator.

In order to examine whether appointed directors are held accountable for their performance, I examine the heterogeneous local average treatment effect or HLATE (Becker et al., 2013). To estimate it, I allow for separate slopes for appointed and unappointed directors:

$$Y_s = \alpha + \beta_1 T_s + \beta_2 D_s + \beta_3 T_s D_s + A_s(\gamma_1 + \gamma_2 T_s + \gamma_3 D_s + \gamma_4 T_s D_s) + \sum_{k=1}^K \eta_k X_s^k + \varepsilon_s \quad (9)$$

A_s indicates whether the school's director in 2013 was appointed. $\beta_1 + \gamma_2$ identifies the HLATE for appointed directors, under two additional assumptions. First, the subgroup indicator A_s must be continuous around the threshold, as shown in Appendix D.1. Second, the subgroup indicator must be conditionally ignorable, such that around the threshold and conditional on their distance to it, schools with appointed and unappointed directors do not differ systematically in a way that affects their turnover. To relax this assumption, I include $\sum_{k=1}^K \eta_k X_s^k$: state fixed effects and a vector of director-, school-, and municipality-level pre-treatment covariates that predict whether the school has an appointed director (Appendix A.3).

4.2.3 Data

I use ANRESC data on primary education quality scores and on directors' mode of selection and turnover. I code a school as having director turnover when the director in 2015 reports they have been in their post for one year or less.³¹

4.2.4 Results

Table 4 presents the RDD results. Model 2 demonstrates that, among schools with an appointed director, meeting the target depresses the probability of director turnover in the year following the publication of the results by 7.3 percentage points or about 0.19 standard deviations ($p < 0.01$).

³¹School directors are not identified, so I cannot track the destination of those who are replaced.

Figure 3 visualizes this effect. Among elected or civil service directors, however, meeting the target does not significantly change in the probability of turnover.³² The results are robust to the inclusion of state fixed effects and covariates that predict whether directors were appointed (models 3-4).

Table 4: Regression discontinuity estimates of the effect of reaching the performance target on director turnover, by whether the director was politically appointed

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Quality target met	-0.033 (0.018)	0.016 (0.027)	0.015 (0.027)	0.005 (0.028)
$\hat{\gamma}_2$: Quality target met $\times$ Appointed		-0.089* (0.036)	-0.088* (0.036)	-0.082* (0.037)
$\hat{\beta}_1 + \hat{\gamma}_2$		-0.073** (0.023)	-0.073** (0.023)	-0.077** (0.024)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.518	0.516	0.516	0.516
N	8458	8387	8387	7734

Predictors of whether the director is appointed come from a regression detailed in Appendix A.3. HC1 standard errors in brackets. *p<0.05; **p<0.01; ***p<0.001.

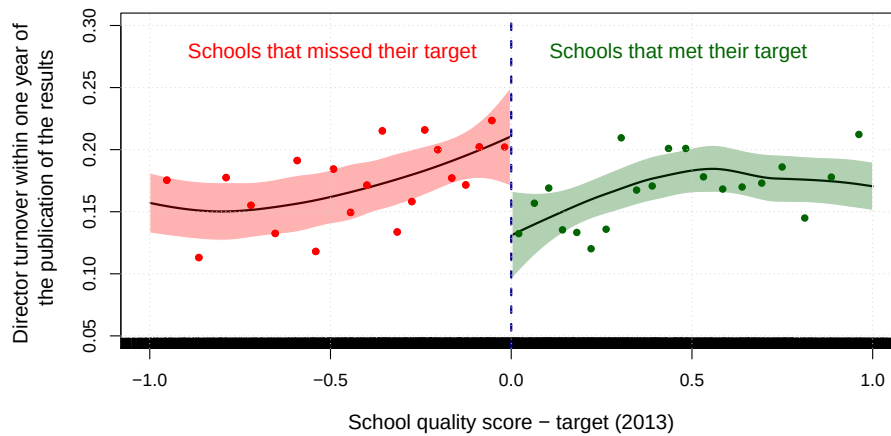
Additional robustness checks lend further support to these results. Alternative bandwidths lead to similar estimates (Appendix D.4). Placebo tests changing the RD threshold generally return insignificant results (Appendix D.5). The results are larger in municipalities with a large, programmatic party in office (Appendix D.6), which is consistent with directors being held accountable for their performance in service delivery.

In summary, these results demonstrate that appointed directors are held accountable for school quality, while elected and civil service ones are not. This suggests that upward embeddedness facilitates bureaucratic accountability, and that politicians consider public service deliver when making political appointments. Multiple interviewees made statements consistent with this. For example, a secretary of education said “the school’s IDEB is a factor to decide if the director continues or not.”³³

³²Appendix D.2 reports the null results among elected and among civil service directors. Appendix D.3 discusses how low competition, capture, and low participation limit the ability of director elections to promote accountability. While tenured directors generally cannot be fired, they can be transferred to a different school.

³³Secretary of education interviewed in the state of Paraíba in August 2018.

Figure 3: Effect of meeting the performance target on director turnover, for schools whose director was politically appointed



See notes under Figure 2.

4.3 Local actors perceive political appointments and connections as making bureaucrats more responsive: Survey evidence

To explore how upward embeddedness fosters bureaucratic effectiveness and accountability, I leverage a face-to-face, representative survey of 926 street-level managers and an online survey of 455 local politicians. The results from conjoint experiments in these surveys suggest that upwardly embedded bureaucrats communicate better with – and are more responsive to – the local government, obtain more resources, and exert more effort.

4.3.1 Face-to-face survey of street-level managers

Based on my in-depth interviews with bureaucrats and politicians, I designed and implemented (with 23 research assistants that I hired, trained, and coordinated) a face-to-face, representative survey of municipal street-level managers (school directors, health clinic managers, and social assistance center coordinators) in late 2018. This is, to my knowledge, the first representative survey of street-level managers to collect data about their political connections and attitudes.

The survey was administered in Rio Grande do Norte, a state in the heart of Brazil's Northeastern region. The Brazilian Northeast has historically been characterized by inferior development outcomes, corruption, and clientelism, especially in the municipalities of the interior (Leal, 1948;

Nichter, 2018; Frey, 2020).³⁴ The survey focused on the urban areas of 150 small and medium-sized municipalities – all but the largest 17 municipalities in the state, which were excluded for security concerns.³⁵ We traveled more than 25,000 kilometers over four weeks to locate every municipal school, health clinic, and social assistance center in the urban areas of those municipalities. The managers of 926 out of 1,027 units (over 90%) were surveyed, with a median number of five respondents per municipality.³⁶

First, I present observational data suggesting that appointed managers have more access to, trust in, and alignment with politicians. I leverage survey questions on the number of meetings street-level managers held, over the previous three months, with the mayor and the secretary in their area, among other stakeholders; and questions on their level of agreement (on a 4-point scale) with statements about the mayor and the secretary. To determine if there are robust correlations between these outcomes and political appointments, I regress respondents' answers on indicators for appointment modes and controls:

$$Y_i = \alpha + \beta_1 A_i + \beta_2 E_i + \sum_{k=1}^K \gamma^k X_i^k + \varepsilon_{im} \quad (10)$$

Y_i is the response given by manager i (namely, the logged number of reported meetings with a given stakeholder +1, or the level of agreement with a given statement). A_i and E_i indicate whether that manager is appointed or elected (leaving civil service as the baseline). $\sum_{k=1}^K \gamma^k X_i^k$ are all the demographic, professional, and political covariates I collected.³⁷ To facilitate comparisons between appointment modes, I exclude from these regressions the 11% of respondents who reported having been appointed through more than one method. For inference, I use HC1 heteroskedasticity-consistent standard errors.

Figure 4 presents the results.³⁸ Compared to civil service managers, political appointees report, on average, more meetings with the mayor, the secretary, technical staff in the secretariat ("technicians"), and social service users ("clients"). They also report higher levels of trust in the

³⁴Rio Grande do Norte is no exception to this regional pattern (Lacerda and de Oliveira, 2004; Carvalho, 2018).

³⁵The state is consistently ranked as one of the most violent in Brazil.

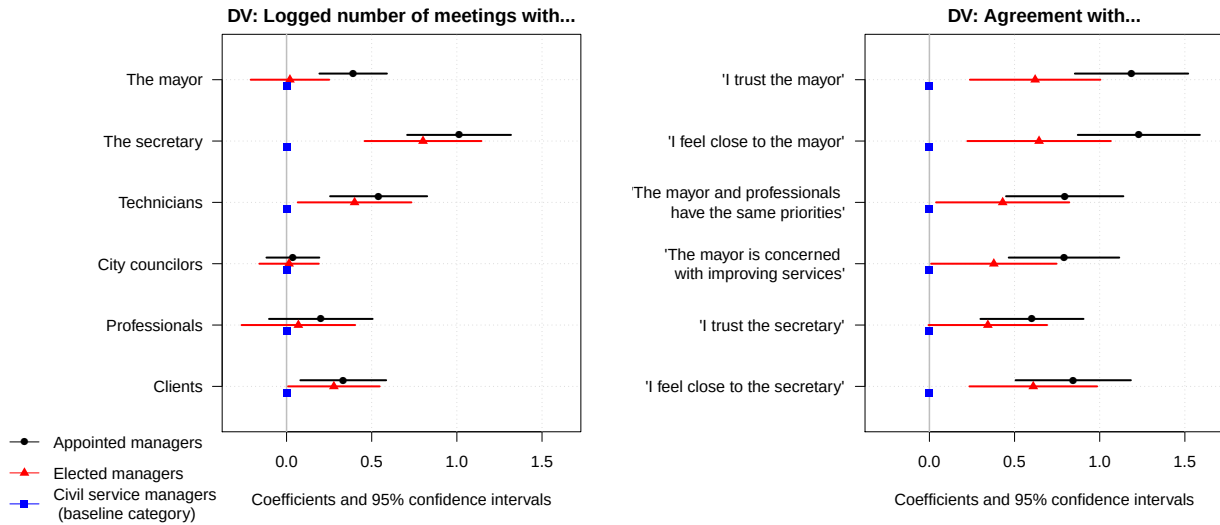
³⁶Appendix E.1 reports details on respondent recruitment. Appendix E.2 reports descriptive statistics.

³⁷The controls include respondents' sector, age, gender, years of experience as a professional, years of experience as a manager, party membership, union membership, whether they have worked for a local electoral campaign, whether they have more than a college degree, whether they have other jobs, and whether they live in the municipality where they work.

³⁸Regression details are in Appendix E.4.

mayor and the secretary, more proximity to them, and stronger beliefs that the mayor cares about improving public services and has the same priorities as bureaucrats. If we restrict the comparison to appointed versus elected managers, appointed ones report more meetings with the mayor and the secretary, and warmer attitudes on all items about them (Appendix E.4). While not causal, these associations are strong and aligned with the theory's predictions.

Figure 4: Association between street-level managers' appointment mode and meetings with and attitudes about politicians.



Points are the regression coefficient corresponding to each appointment mode (as per Equation 10) and bars denote 95% confidence intervals.

To more directly test the mechanisms of upward embeddedness, I use a conjoint experiment I included in the survey. Conjoint experiments allow researchers to non-parametrically identify and estimate the causal effect of several variables simultaneously while limiting social desirability bias (Hainmueller et al., 2014).

Respondents saw four sets of two hypothetical profiles of managers, with randomly assigned attributes in six dimensions (appointment mode, political connections, education, experience, relationship to professionals in their unit, and whether the unit had met performance targets).³⁹ To avoid primacy and recency effects, the order of the attributes was randomized across respondents. For each pair, respondents were asked to choose which manager they believed would be more likely to: (i) maintain better communication with the secretariat; (ii) implement school changes requested by the municipal government; (iii) raise more material resources to reform the school /

³⁹Details of the attribute values for the conjoint profiles are included in Appendix E.5.

clinic / social assistance center; and (iv) increase the unit's performance in indicators of learning / healthcare / social assistance. These four choice tasks measure the relative impact of different characteristics on perceptions of bureaucrats' ability to perform in key areas of management that my theory predicts upward embeddedness should facilitate.

I estimate the average marginal component effect (AMCE) for each attribute's value using the following linear regression ([Hainmueller et al., 2014](#)):

$$Y_{ijk} = \alpha + \beta W_{ijkl} + \varepsilon_{ijk} \quad (11)$$

Y_{ijk} is the choice expressed by respondent i for profile j in the choice task k (i.e., whether that manager profile was chosen). W_{ijkl} is the vector of dummy variables for the l levels of each attribute in profile j (omitting a baseline category in each attribute). ε_{ijk} is the error term. I cluster standard errors at the respondent level to account for the dependencies between their choices. β nonparametrically identifies the AMCE for each of the attributes (and their values) on a hypothetical manager being chosen for a given task in the sample.

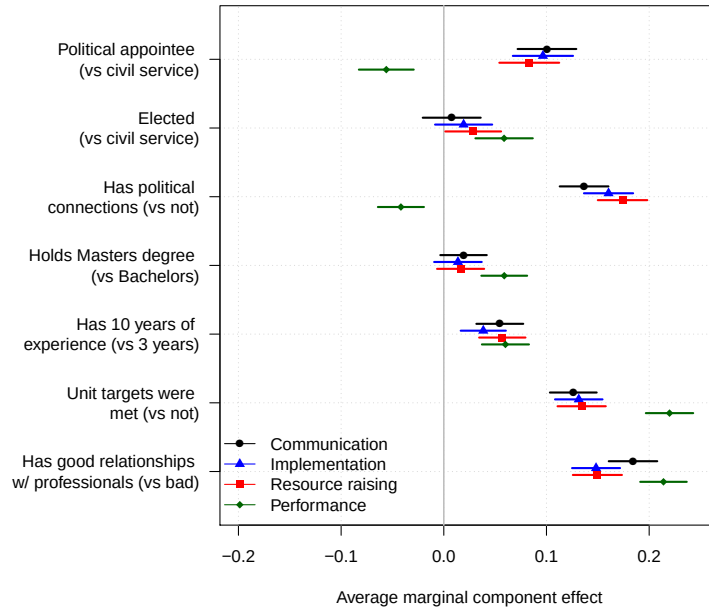
The results of the conjoint experiment, shown in Figure 5,⁴⁰ demonstrate that street-level managers see upward embeddedness as facilitating bureaucrats' communication with and responsiveness to the local government, as well as access to resources. Profiles of managers with political connections, or who are political appointees, are seen as significantly more likely than civil service managers to have better communication with the secretariat of their area, to implement changes requested by the local government, or to raise resources for reforming their unit.⁴¹

However, managers who are politically appointed or have political connections are seen as less likely to improve the performance of the unit. This suggests that patronage may hinder public service delivery. Three factors may explain this result. First, managers may be simply expressing that politically appointed bureaucrats are worse types (e.g., less educated, as shown in Appendix E.3). Second, respondents may be underestimating the indirect channels through which upward embeddedness can aid public service delivery, helping political appointees perform better. Third, the result may be driven by street-level managers who work in the highly clientelistic environments typical of the Northeast, where political appointments are more likely to be leveraged to extract rents. In line with this interpretation, the negative result for performance disappears (without substantively altering other results) when examining the responses of managers who perceive politicians as more

⁴⁰Regression details are in Appendix E.5.

⁴¹The results are similar when examining only responses given by unappointed managers (Appendix E.6).

Figure 5: Results from the conjoint experiment with bureaucrats



Points are the average marginal component effect (AMCE), and bars their 95% confidence intervals. AMCEs estimated for each choice task separately, as per Equation 11.

programmatic (Appendix E.6). In any case, these results draw attention to the potential costs of upward embeddedness, illustrating empirically a trade-off between responsiveness and performance in localities with more clientelistic politicians.

4.3.2 Online survey of politicians

Local politicians also perceive upwardly embedded bureaucrats as more accountable. In partnership with the state audit court of Rio Grande do Norte, I implemented an online survey of local politicians. The survey was sent by the court to the mayor and the secretaries of education, healthcare, social assistance, finance, and administration of all 167 municipalities in the state. A total of 455 politicians completed the survey, for a response rate of 45% (a high value for a survey of elites) and a median number of three responses per municipality.⁴²

In this conjoint experiment, respondents saw four pairs of hypothetical bureaucrats (without specifying their rank or area of work), with randomly assigned attributes in six dimensions (contract type, political connections, education, experience, union membership, and gender).⁴³ Contract type

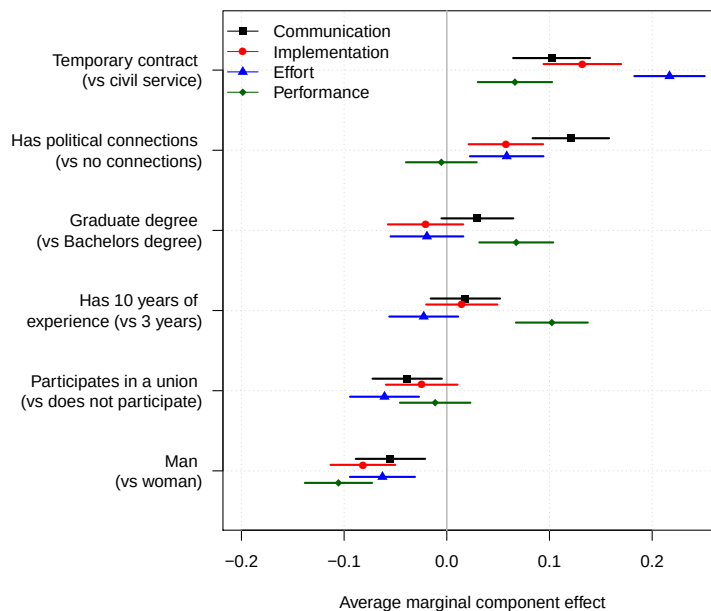
⁴²Appendix F.1 has details on recruitment and non-response. Appendix F.2 reports descriptive statistics.

⁴³Details of the attribute values for conjoint profiles are included in Appendix F.3.

(temporary versus civil service) was used instead of appointment mode because political appointments and election are not legal selection methods for most bureaucrats. Like political appointments, temporary hires are at will and often based on political connections (Colonnelli et al., 2020).⁴⁴ For each pair, respondents were asked to choose which one they believed would be more likely to: (i) maintain better communication with the local government; (ii) implement changes requested by the local government; (iii) work extra hours when necessary; and (iv) achieve better performance.

The results, shown in Figure 6,⁴⁵ suggest that politicians view upwardly embedded bureaucrats (i.e., those who were politically appointed or hired under a temporary contract) as more responsive and exerting more effort. They also perceive bureaucrats working under temporary contracts as likely to perform better than those hired under the civil service regime. As a secretary of education explained, “almost all civil service bureaucrats are from other towns. They don’t work with the true grit we need. [...] Temporary hires dedicate themselves more.”⁴⁶

Figure 6: Results from the conjoint experiment with politicians



See notes under Figure 5.

To summarize, the results of the conjoint experiments with bureaucrats and politicians generally support the key mechanisms of the theory. Both managers and politicians perceive upwardly

⁴⁴58% of the street-level managers surveyed said political appointments influence “a lot” the hiring of street-level bureaucrats, and only 16% responded “not at all” or “a little.”

⁴⁵Regression details are in Appendix F.3.

⁴⁶Secretary of education interviewed in the state of Paraíba in August 2018.

embedded bureaucrats as more likely to communicate well with the local government and to respond to its demands. Managers also perceive upwardly embedded bureaucrats as more likely to raise funds from the government, and politicians believe they are more likely to work extra hours when needed. Together, these results show that actors in the field perceive upward embeddedness as benefiting bureaucratic accountability. Perceptions of the impact of upward embeddedness on effectiveness are more mixed, in line with the high prevalence of clientelism in this region of Brazil (Nichter, 2018; Frey, 2020).

5 Conclusion

Patronage, or the political appointment of bureaucrats, is typically seen as a clientelistic exchange that jeopardizes development by selecting worse types into the bureaucracy and by depressing bureaucratic effort (Pollock, 1937; Xu, 2018; Colonnelli et al., 2020). This paper offers an alternative view of patronage as a system that works by changing not (only) who enters the bureaucracy or how much they work, but *how* they work. I argue that patronage provides bureaucrats with upward embeddedness (political and social ties to politicians), which can make them more accountable and effective. Upward embeddedness works by giving bureaucrats access to material and non-material resources, providing politicians with monitoring technology, facilitating the application of sanctions and rewards, aligning their priorities and incentives, and increasing mutual trust. These governance resources can be leveraged to extract rents, deliver public services, or both. I argue that patronage is most likely to be beneficial for development in the appointment of street-level managers and in contexts where politicians value the delivery of public goods but face human capital and financial constraints on their capacity to attract and motivate bureaucrats to perform.

The paper's main empirical contribution is to provide causally identified evidence of the benefits of patronage for bureaucrats' effectiveness and accountability. It does so by leveraging administrative and survey data on municipal bureaucracies in Brazil, a setting in which multiple appointment systems coexist. To test the core claims of the theory I use two quasi-experiments. Using a diff-in-disc, I show that the quality of schools with appointed directors decreases (relative to those with unappointed directors) if the mayor loses their bid for re-election. Using an RDD, I show that appointed directors (but not unappointed ones) are less likely to be replaced when they meet a highly visible school quality target. To investigate the mechanisms of upward embeddedness, I use original surveys of bureaucrats and politicians. Observational analyses show that appointed bureaucrats have more meetings with local politicians and express higher levels of trust in and alignment with

them. The results of the conjoint experiments show that both street-level managers and politicians perceive upwardly embedded bureaucrats as being better at communicating with the government and responding to its demands. These quantitative findings are grounded in 121 in-depth interviews with bureaucrats, politicians, and anti-corruption agents, which provide rich accounts of how patronage can foster not only rent extraction but also public service delivery.

The findings demonstrate that political appointments and connections provide useful governance resources, that these resources can strengthen bureaucratic effectiveness, and that politicians hold political appointees accountable for their performance in service delivery. These often-overlooked benefits of patronage suggest that politics in the developing world can be a source not only of corruption and misallocations, as is often assumed, but also of governance resources that can help overcome development challenges. A key implication of the paper is that severing the connections between bureaucrats and politicians can hurt public service delivery, at least in the short term and when other sources of bureaucratic effectiveness (e.g., high levels of human capital and strong bureaucratic norms) are not developed.

However, the theory and findings presented here suggest two important weaknesses related to the use of patronage for service delivery, which complements more established views of the costs of patronage. The first weakness is that the same system can be mobilized to *actively* extract rents (e.g., by rigging procurement processes or targeting public services to core supporters). By changing how bureaucrats work—for example, by making them more aligned and more easily monitored and sanctioned—upward embeddedness makes it easier for corrupt politicians to use the bureaucracy to their advantage. This helps explain why patronage is so central to political machines, and why it has proven to be so resilient throughout history ([Grindle, 2012](#)). The second weakness is that, even when patronage is used for public service delivery, its benefits are particularly vulnerable to political turnover. While a change in government can make civil service bureaucrats less effective ([Toral, 2021](#)), political appointees are especially likely to see their capacity diminished after such a turnover. If political appointees and civil servants rely on different sources of effectiveness (upward embeddedness and a combination of human capital and strong bureaucratic norms, respectively), unaligned political appointees count on none of them. Patronage without connections may therefore be the worst of both worlds.

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Appendices

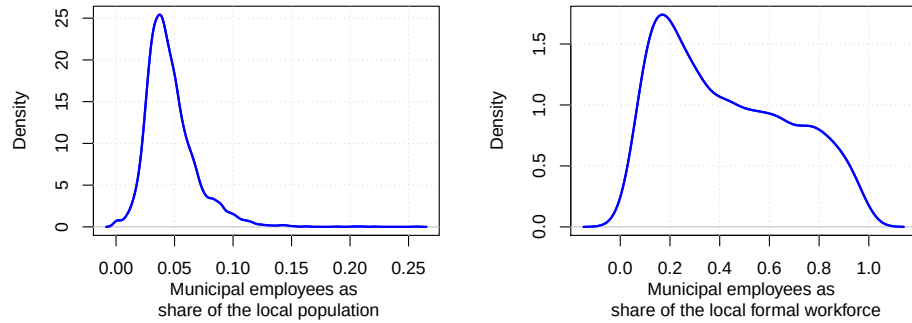
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A Additional details of the institutional context

A.1 Size of the municipal government workforce

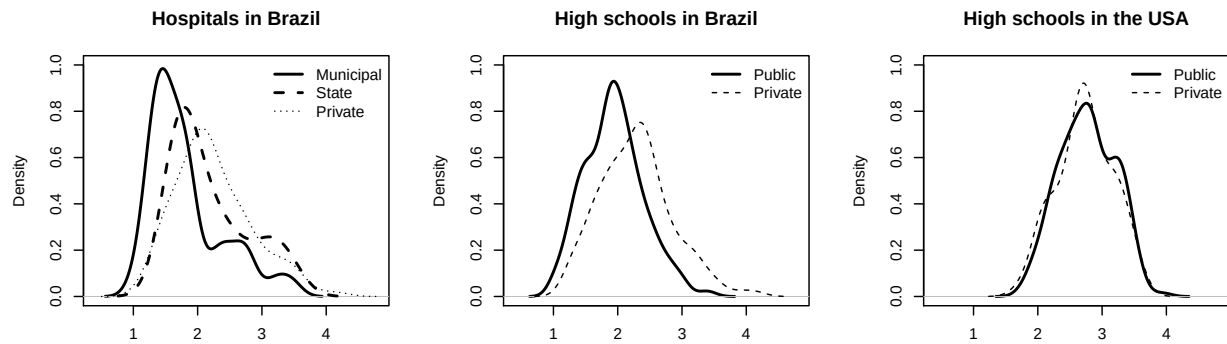
Figure 7: Size of the municipal government workforce as a proportion of the total local population and the total formal labor market workforce



Calculated using administrative data of the universe of formal labor market contracts in 2016, and official population statistics for 2016.

A.2 Management practices in Brazilian schools and hospitals

Figure 8: Scores of the World Management Survey for hospitals and high schools in Brazil, and for high schools in the USA



Data are from [Bloom et al. \(2014, 2015\)](#) and correspond to 289 hospitals and 513 high schools that were randomly selected in Brazil, as well as 270 high schools in the USA for comparison. Most public high schools in Brazil are managed by state governments. I only code as municipal or state hospitals those that have those words in their name.

A.3 Predictors of school directors' appointment mode and school quality score

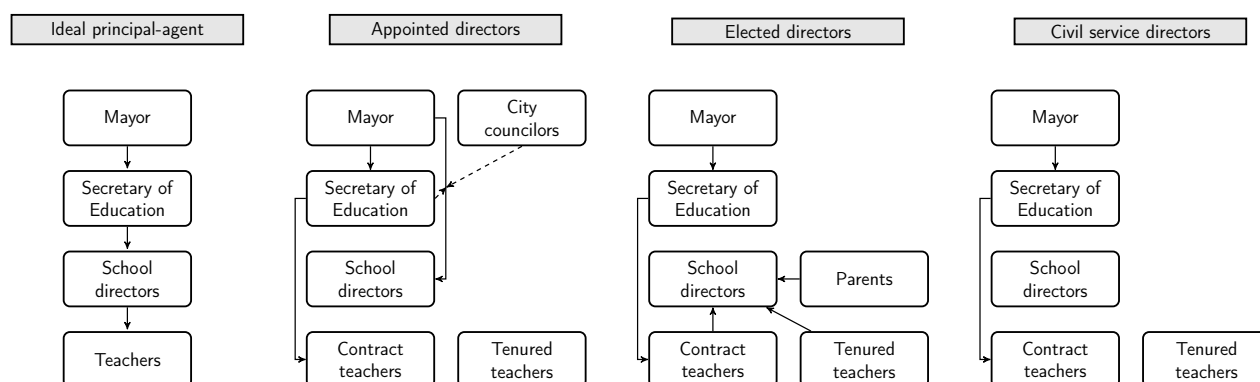
Table 5: Observational predictors of municipal schools' director appointment mode and quality score (IDEB), from cross-section data on municipalities, schools, and directors (2013)

	Appointed			Elected	Civil service	IDEB score				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Intercept	0.965*** (0.018)	2.587*** (0.038)	2.418*** (0.043)	-1.075*** (0.039)	-0.278*** (0.022)	3.735*** (0.068)	-1.991*** (0.138)	-2.141*** (0.121)	-1.451*** (0.114)	4.739*** (0.009)
Director covariates										
Female	0.004 (0.007)	0.022*** (0.007)	0.033*** (0.007)	-0.021** (0.006)	-0.015*** (0.004)	0.279*** (0.026)	0.169*** (0.023)	0.015 (0.020)	0.006 (0.018)	
Age 40-49 (vs <40)	-0.066*** (0.007)	-0.021*** (0.006)	-0.018** (0.006)	0.020*** (0.006)	-0.007* (0.003)	0.073*** (0.020)	0.021 (0.018)	-0.021 (0.015)	-0.012 (0.014)	
Age 50+ (vs <40)	-0.097*** (0.008)	-0.016* (0.008)	-0.012 (0.008)	0.010 (0.008)	-0.005 (0.004)	0.073** (0.026)	0.006 (0.024)	-0.057** (0.020)	-0.052** (0.018)	
White (vs other race)	-0.032* (0.014)	0.019 (0.013)	0.038** (0.014)	-0.054*** (0.013)	0.023*** (0.007)	0.700*** (0.047)	0.397*** (0.042)	0.113** (0.035)	0.002 (0.032)	
Black/brown (vs other race)	0.070*** (0.014)	0.059*** (0.013)	0.055*** (0.013)	-0.023 (0.013)	-0.019** (0.006)	-0.096* (0.046)	-0.049 (0.042)	-0.030 (0.035)	-0.050 (0.032)	
Tertiary degree (vs < tertiary)	-0.090*** (0.011)	-0.012 (0.010)	-0.006 (0.011)	0.026** (0.009)	-0.013** (0.005)	0.448*** (0.036)	0.291*** (0.033)	0.165*** (0.030)	0.102*** (0.028)	
Postgraduate degree (vs < tertiary)	-0.086*** (0.006)	-0.061*** (0.006)	-0.054*** (0.006)	0.038*** (0.006)	0.005 (0.003)	0.299*** (0.020)	0.212*** (0.018)	0.131*** (0.016)	0.118*** (0.014)	
No other jobs	-0.038*** (0.006)	-0.027*** (0.005)	-0.023*** (0.006)	0.033*** (0.005)	-0.015*** (0.003)	0.213*** (0.019)	0.167*** (0.017)	0.139*** (0.014)	0.097*** (0.013)	
6-15 years of teaching exp. (vs <6)	-0.050*** (0.007)	-0.027*** (0.007)	-0.024*** (0.007)	0.026*** (0.006)	0.011** (0.004)	0.090*** (0.023)	0.040 (0.021)	-0.004 (0.018)	-0.024 (0.017)	
>15 years of teaching exp. (vs <6)	-0.032*** (0.009)	-0.015 (0.008)	-0.010 (0.008)	0.030*** (0.007)	-0.005 (0.004)	0.186*** (0.027)	0.080*** (0.024)	0.004 (0.021)	-0.024 (0.019)	
3-10 years of director exp. (vs <3)	-0.019* (0.009)	0.033*** (0.008)	0.033*** (0.008)	-0.082*** (0.007)	0.031*** (0.005)	0.092*** (0.027)	0.072** (0.024)	0.021 (0.020)	0.014 (0.019)	
>10 years of director exp. (vs <3)	-0.007 (0.012)	0.078*** (0.012)	0.080*** (0.012)	-0.197*** (0.010)	0.095*** (0.008)	0.194*** (0.037)	0.124*** (0.034)	0.022 (0.028)	0.005 (0.026)	
3-10 years as director of school (vs <3)	-0.182*** (0.009)	-0.180*** (0.008)	-0.180*** (0.008)	0.222*** (0.007)	-0.010 (0.005)	-0.023 (0.026)	0.003 (0.024)	0.062** (0.020)	0.053** (0.019)	
>10 years as director of school (vs <3)	-0.265*** (0.015)	-0.248*** (0.014)	-0.245*** (0.014)	0.275*** (0.013)	0.002 (0.011)	0.029 (0.043)	0.057 (0.040)	0.151*** (0.034)	0.115*** (0.030)	
Municipality covariates										
GDP per capita (logged)		-0.088*** (0.004)	-0.043*** (0.005)	0.020*** (0.004)	0.020*** (0.002)		0.665*** (0.014)	0.089*** (0.013)	0.038*** (0.011)	
Population (logged)		-0.082*** (0.002)	-0.087*** (0.002)	0.068*** (0.002)	0.006*** (0.001)		-0.104*** (0.005)	-0.097*** (0.005)	-0.073*** (0.004)	
Number of deaths per 1,000		-0.017*** (0.002)	-0.009*** (0.002)	0.012*** (0.002)	-0.002** (0.001)		0.108*** (0.005)	0.020*** (0.005)	0.007 (0.004)	
Mayor is in first term		-0.000 (0.006)	0.006 (0.006)	-0.022*** (0.006)	0.004 (0.003)		0.071*** (0.017)	0.034* (0.015)	0.004 (0.014)	
Electoral concentration		0.062** (0.021)	0.044* (0.021)	0.017 (0.019)	-0.071*** (0.011)		0.282*** (0.060)	0.452*** (0.052)	0.275*** (0.049)	
School covariates										
Rural			0.028*** (0.007)	-0.025*** (0.006)	0.003 (0.003)			-0.014 (0.021)	-0.041* (0.019)	
Number of staff (logged)			0.005 (0.005)	0.015** (0.005)	-0.016*** (0.003)			0.009 (0.014)	0.005 (0.012)	
Students per classroom (average)			0.006*** (0.001)	-0.006*** (0.001)	0.001*** (0.000)			-0.009*** (0.002)	-0.008*** (0.002)	
School socioeconomic index			-0.009*** (0.001)	0.007*** (0.001)	0.003*** (0.000)			0.141*** (0.002)	0.078*** (0.002)	
Director is appointed (vs civil service)						-0.631*** (0.030)	-0.377*** (0.028)	-0.174*** (0.023)	0.000 (0.022)	
Director is elected (vs civil service)						-0.333*** (0.031)	-0.234*** (0.028)	-0.155*** (0.024)	0.011 (0.022)	
IDEB target									0.585*** (0.010)	
Municipality fixed effects	No	No	No	No	No	No	No	No	No	Yes
Adj. R ²	0.099	0.252	0.259	0.186	0.071	0.243	0.382	0.570	0.659	0.000
Num. obs.	30748	30038	29273	29273	29273	17404	17402	16814	15622	19118

***p < 0.001; **p < 0.01; *p < 0.05. HCl standard errors in brackets

A.4 Accountability relationships by director appointment mode

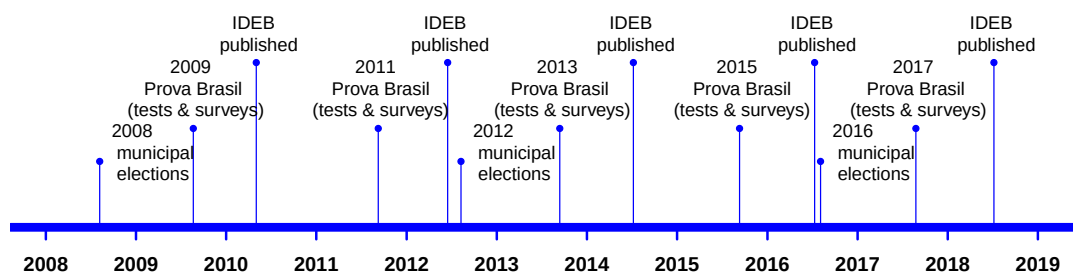
Figure 9: Four models of appointments in Brazil's municipal basic education sector



The dashed lines represent occasional participation of city councilors and secretaries in political appointments.

A.5 Timeline of student testing and information release

Figure 10: Timeline of IDEB tests and information release



A.6 Illustrative materials for schools to prepare for student tests

The following resources were produced by governments and NGOs to help schools and school directors prepare in the short term for the ANRESC student tests.

- *7 ações para aproveitar bem a Prova Brasil* (7 actions to take advantage of Prova Brasil), produced by Gestão Escolar (School Management), the director-gearred section of Nova Escola, which is a leading education magazine in Brazil. Published in September, 2 months before the implementation of the tests. Available [here](#).
- *Como preparar a escola para a Prova Brasil* (How to prepare the school for Prova Brasil), also produced by Gestão Escolar. Published on August, 3 months before the implementation of the test. Available [here](#).

- *Dicas para preparar sua escola para a Prova Brasil* (Tips to prepare your school for Prova Brasil), produced by Educador360, another education site, in a section called Pedagogic management. Published in early November, at the beginning of the period when the test was implemented. Available [here](#).
- *Escolas da SEMED reforçam atividades de preparação para Prova Brasil* (Municipal Education Secretariat schools reinforce preparation activities for Prova Brasil), produced by the municipality of Manaus (in the Amazon), describing a number of overlapping strategies the municipality implemented to prepare for Prova Brasil. Published in late September, weeks before the test. Available [here](#).
- *Como preparar a escola para a Prova Brasil* (How to prepare the school for Prova Brasil), slides for how to prepare for the test, dated in October just one month before the test. Published on the site of the secretariat of education of the state of Goiás. Available [here](#).

B Additional details of in-depth interviews

In-depth interviews with local actors gave origin to the hypotheses tested in this article, but were part of a larger empirical study of patronage in Brazil. Over 18 months of fieldwork in the period 2016-2019 I conducted 121 in-depth, semi-structured interviews with municipal bureaucrats and politicians, and with state-level horizontal accountability actors (e.g., prosecutors). I recruited interviewees at their offices, and collected their oral consent after providing information about the research project and their rights as participants. I conducted interviews in Portuguese, face-to-face, and at the interviewee's office. I chose not to record interviews because some of the topics discussed were highly sensitive, including corrupt and illegal uses of public employment. While recording interviews would have allowed for more complete transcripts, it would have seriously hindered the reliability of the data and subjects' willingness to participate. Some subjects agreed to participate on the condition of anonymity or confidentiality. When quoting interviewees, I specify only their position, the state, and the month of the interview in order to safeguard their identity. In total, I interviewed 51 municipal politicians, 54 municipal bureaucrats, and 16 horizontal accountability actors.⁴⁷ Interviews were done in 45 municipalities in 7 states across 3 different regions of Brazil.⁴⁸ Locations were chosen to ensure diversity in political and socioeconomic variables.

Within each municipality, fieldwork focused on the center, where government offices are. I approached potential interviewees at their offices and requested an interview after introducing myself and the research project. No compensation of any sort was offered or given to participants. Most subjects that I managed to speak to directly agreed to participate.⁴⁹ Interviews were semi-

⁴⁷41 of the 51 politicians were secretaries. 46 of the 54 bureaucrats were school directors, clinic managers, and social assistance center coordinators. Of the 16 horizontal accountability actors, 8 were state prosecutors or prosecutorial staff.

⁴⁸Interviews were done in the states of Ceará (43 interviews), Rio Grande do Norte (21), Paraíba (15), Rio de Janeiro (19), Minas Gerais (10), São Paulo (1), and Goiás (12).

⁴⁹Some refused, mostly arguing they did not have time. Two refused due to the research topic.

structured, and usually started as an open conversation about the interviewee’s background, the challenges they faced in their position, and their perception of public services in the municipality. As the conversation advanced, I followed up with questions about the local dynamics of public employment, including in some cases specific questions about the connection between political turnover, bureaucratic turnover, and public service delivery. I took handwritten notes during and after the interviews. The median duration of interviews was one hour.

Interview locations are quite diverse in their social, economic, and political characteristics. Descriptive statistics for interview locations are available from the author.

C Additional details of the difference-in-discontinuities

C.1 Continuity of pre-treatment covariates and the forcing variable

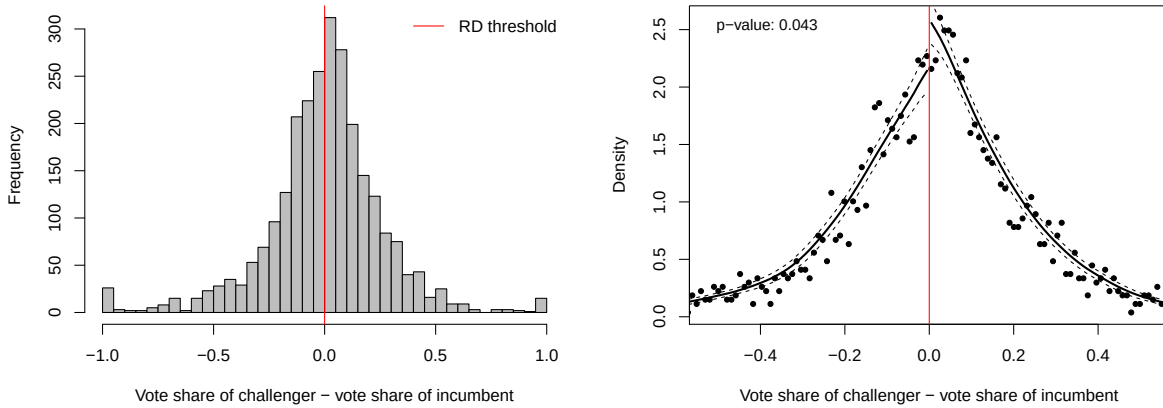
Table 6: Continuity in pre-treatment covariates at the director, municipality, and school level, estimated by applying Equation 5 with pre-treatment covariates as the dependent variable

	RD estimate	Standard error	p value
Director is elected	0.141	0.256	0.581
Director is civil service	-0.017	0.099	0.867
Director is female	0.006	0.070	0.928
Director is aged <40	0.057	0.088	0.519
Director is aged 40-49	-0.179	0.099	0.070
Director is aged 50+	0.166	0.091	0.067
Director is white	-0.221	0.116	0.058
Director is black/brown	0.199	0.119	0.093
Director has other race	0.028	0.016	0.093
Director has < tertiary education	-0.023	0.037	0.527
Director has a tertiary degree	0.023	0.037	0.527
Director has a postgraduate degree	0.012	0.057	0.833
Director has no other job	-0.006	0.071	0.929
Director has <6 years of teaching experience	0.096	0.044	0.028
Director has 6-15 years of teaching experience	-0.129	0.084	0.124
Director has >15 years of teaching experience	0.021	0.086	0.810
Director has <3 years of director experience	-0.075	0.081	0.351
Director has 3-10 years of director experience	0.091	0.069	0.189
Director has >10 years of director experience	-0.020	0.041	0.624
Director has held position for <3 years	0.000	0.000	1.000
Municipality GDP per capita (log)	-0.261	0.219	0.234
Municipality population (log)	0.106	0.554	0.849
Municipality deaths per 1,000	-0.225	0.430	0.601
Municipal electoral concentration	0.050	0.033	0.128
School is rural	-0.029	0.067	0.663
Number of staff in the school	2.192	6.770	0.746
School is in a settlement	0.003	0.018	0.887
School is in indigenous land	0.000	0.000	1.000
School is in quilombola land	0.004	0.007	0.543
Students per classroom (average)	2.203	1.098	0.045
School socioeconomic index	-0.528	1.571	0.737
School IDEB score in 2015	-0.239	0.302	0.429
School IDEB target for 2017	-0.287	0.235	0.222

RD estimate corresponds to $\hat{\beta}_1$ in Equation 5.

While “a running variable with a continuous density is neither necessary nor sufficient for identification” (McCrary, 2008, 701), it is important to consider reasons that may drive the discontinuity

Figure 11: Continuity of the forcing variable: Histogram and [McCrary \(2008\)](#) density test

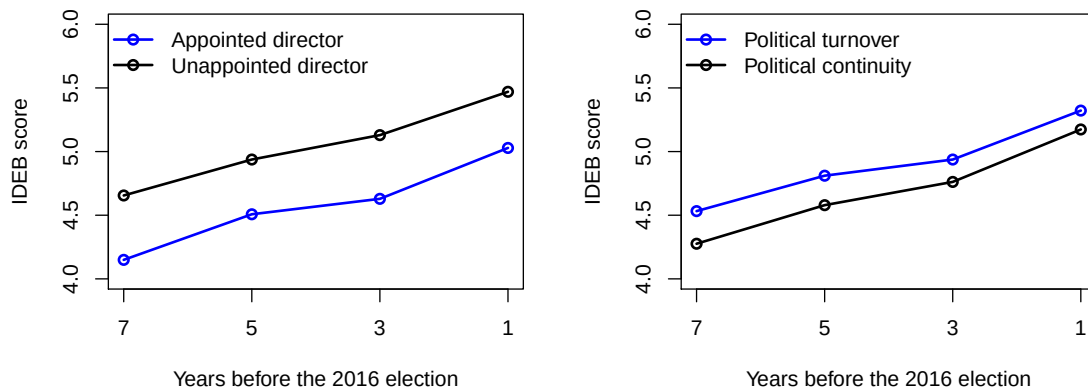


identified by the density test in Figure 11. This may be due to Brazilian mayors' incumbency disadvantage ([Klašnja and Titiunik, 2017](#)). In any case, the key is that actors (in this case, mayors and their challengers) do not have precise manipulation of the forcing variable ([Lee and Lemieux, 2010](#)). An additional observable implication of the lack of precise manipulation assumption is that there should be no discontinuous jumps in covariates around the threshold, as shown in Table 6.

C.2 Pre-election trends

Figure 12 shows how the average IDEB score of schools that enter the diff-in-disc evolve from 2009 to 2015, by whether their director is appointed and whether the mayor loses the 2016 election.

Figure 12: Pre-treatment trends in school quality scores, by director appointment mode and by political turnover



C.3 Alternative bandwidths

This Appendix shows how the main result of the diff-in-disc ($\hat{\gamma}_2$ in Equation 5) changes when varying the RD bandwidth, for the results in Tables 2 and 3. If treatment effects are sometimes insignificant with bandwidths smaller than the optimal (likely due to reduced power from smaller samples), coefficients remain large and negative.

Figure 13: Robustness of results in model 4 in Table 2 to alternative bandwidths

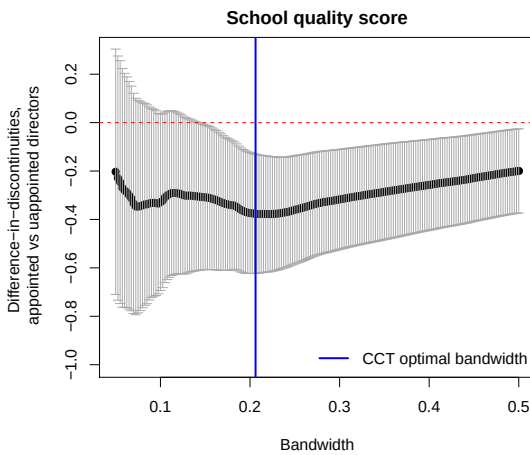
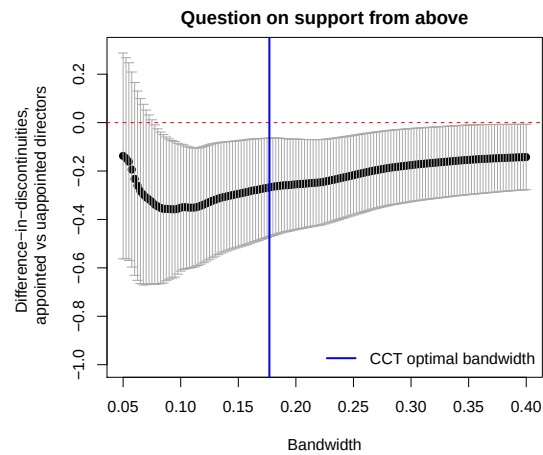


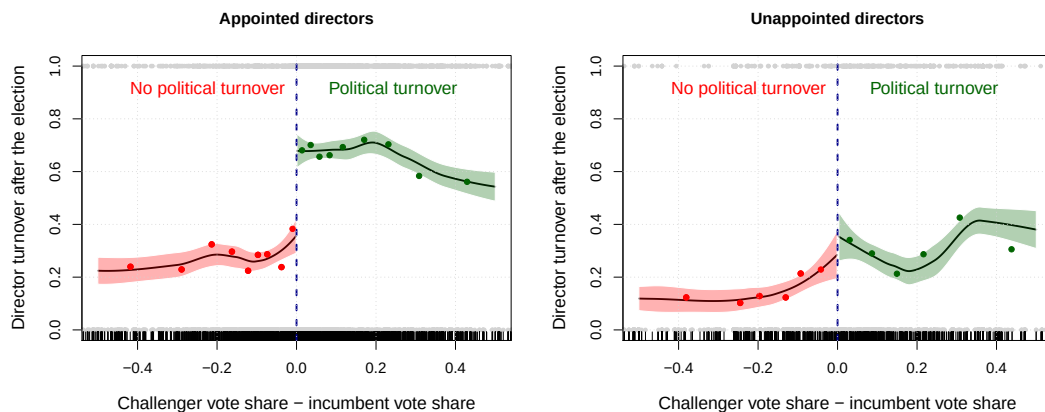
Figure 14: Robustness of results in model 4 in Table 3 to alternative bandwidths



C.4 RD estimates of the effect of political turnover on director turnover

This appendix shows that an electoral defeat of the incumbent has a significant effect on the replacement of school directors within one year after the election (model 1 in Table 7). This effect is mostly driven by the replacement of appointed directors (models 2-4).

Figure 15: Effect of political turnover on director turnover, by director appointment mode



See notes under Figure 2.

Table 7: Regression discontinuity estimates of the effect of political turnover on director turnover, by director appointment mode

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Political turnover	0.255*** (0.059)	0.039 (0.103)	0.046 (0.095)	0.019 (0.100)
$\hat{\gamma}_2$: Political turnover $\times$ Appointed		0.287** (0.110)	0.298** (0.103)	0.335** (0.105)
$\hat{\beta}_1 + \hat{\gamma}_2$		0.326 (0.216)	0.344** (0.118)	0.355*** (0.096)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.116	0.134	0.134	0.134
N	2636	2627	2627	2546

See notes under Table 2.

C.5 Predictors of director turnover after mayor turnover

This appendix examines correlates of directors being replaced after political turnover, using data for municipalities where the mayor loses the election.

Table 8: Observational predictors of school directors being replaced after political turnover.

	(1)
Intercept	0.680 (0.055)***
Director was appointed	0.334 (0.012)***
IDEB score before the election	−0.036 (0.006)***
Female	−0.007 (0.016)
Age 40-49 (vs <40)	−0.025 (0.015)*
Age 50+ (vs <40)	−0.038 (0.018)**
White (vs other race)	−0.076 (0.034)**
Black/brown (vs other race)	−0.031 (0.034)
Tertiary degree (vs < tertiary)	−0.074 (0.030)**
Postgraduate degree (vs < tertiary)	−0.031 (0.016)**
No other jobs	−0.023 (0.013)*
6-15 years of teaching exp. (vs <6)	−0.009 (0.017)
>15 years of teaching exp. (vs <6)	0.033 (0.019)*
3-10 years of director exp. (vs <3)	0.006 (0.015)
>10 years of director exp. (vs <3)	−0.046 (0.021)**
1-2 years as director of school (vs 3)	0.000 (0.015)
Num. obs.	6558
Adj. R ² (full model)	0.151
Adj. R ² (proj model)	0.151

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. HC1 standard errors in brackets

C.6 Alternative estimation: Matching similar schools with and without political turnover

To partially address the sample selection bias, here I match schools in the group without mayor turnover to similar schools in the group with mayor turnover. I do this with exact matching on the covariates that significantly predict director turnover after mayor turnover, as per Appendix C.5. The relevant coefficient is larger than in the main specification in Table 2 and, despite the significantly smaller sample size, remains statistically significant.

Table 9: Difference-in-discontinuity estimates of the differential impact of political turnover on school quality for appointed versus unappointed directors, after matching

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Political turnover	-0.237* (0.116)	0.075 (0.112)	0.031 (0.130)	0.063 (0.129)
$\hat{\gamma}_2$: Political turnover $\times$ Appointed		-0.513** (0.196)	-0.469* (0.206)	-0.486* (0.204)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.180	0.204	0.204	0.204
N	489	488	488	486

See notes under Table 2.

C.7 Bounds to account for sample selection bias

C.7.1 The Lee (2009) bounds for sample selection bias

To deal with issues of sample selection bias, Lee (2009) proposes a simple procedure to generate bounds for experimental treatment effects. In his framework, each unit has two latent potential outcomes (Y_1^*, Y_0^*) as well as a potential sample selection indicators (S_1, S_0) under treatment ($D = 1$) and under control ($D = 0$). For each unit we only observe S_1 or S_0 , and one potential outcome Y_1^* or Y_0^* and only if they select into the sample ($S = 1$). To construct the bounds we need to make two assumptions: independence ($\{Y_1^*, Y_0^*, S_1, S_0\} \perp\!\!\!\perp D$) and monotonicity (either $S_1 \geq S_0$ or $S_0 \geq S_1$). I use the case where $S_0 \geq S_1$ (i.e., more units are selected into the sample under control than under treatment), for symmetry with my setting. Lee's procedure consists of the following steps:

- Estimate p_0 , the proportion of units in the control group that are induced to have a outcome

data ($S = 1$) because of their assignment to control:

$$p_0 = \frac{Pr(S = 1|D = 0) - Pr(S = 1|D = 1)}{Pr(S = 1|D = 0)} \quad (12)$$

- Estimate the p_0^{th} and $(1 - p_0)^{th}$ quantiles of the distribution of $Y|D = 0, S = 1$, which we will call y_{p_0} and y_{1-p_0} , respectively.
- Estimate the lower bound of the treatment effect by taking the difference in means between the treated and between a trimmed control group where all observations above y_{1-p_0} are excluded: $\Delta_0^{LB} = \mathbb{E}[Y|D = 1, S = 1] - \mathbb{E}[Y|D = 0, S = 1, Y \geq y_{1-p_0}]$.
- Estimate the upper bound of the treatment effect by taking the difference in means between the treated and between a trimmed control group where all observations below y_{p_0} are excluded: $\Delta_0^{UB} = \mathbb{E}[Y|D = 1, S = 1] - \mathbb{E}[Y|D = 0, S = 1, Y \leq y_{p_0}]$.
- Using the sample analogues of $p_0, \Delta_0^{LB}, \Delta_0^{UB}$, one can construct sharp bounds for the average treatment effect for units with $S_1 = 0, S_0 = 1$ (i.e., those that will be selected irrespective of treatment assignment): $[\Delta_0^{LB}, \Delta_0^{UB}]$.

C.7.2 Adaptation of the Lee (2009) bounding procedure to the diff-in-disc setting

Lee makes it clear that his procedure can be applied to non-experimental settings (Lee, 2009, 1073). In this case, the quantity of interest is not a difference in means but a difference in discontinuities, where treatment is determined at a discontinuity, and I am comparing how treatment affects one group relative to another. To account for these complications, I adapt the Lee bounding procedure as follows to produce bounds for $\hat{\tau}_{disc}$

- I first simplify the design to a localized experiment, immediately around the threshold, and therefore based on an assumption of local randomization instead of one of continuity (Cattaneo et al., 2018).⁵⁰ I focus exclusively on schools immediately around the cutoff – I use the 0.015 bandwidth but results are similar using 0.01 or 0.02. Within this small bandwidth the density of the forcing variable is flat, as required when invoking the local randomization assumption.⁵¹
- Then I build four trimmed datasets: two trimmed datasets for upper and lower bound for appointed directors, and two trimmed datasets for unappointed directors. This is because

⁵⁰Depew and Eren (2016) and others have also used this strategy for adapting Lee (2009) bounds to a regression discontinuity setting.

⁵¹Replicating the diff-in-disc as a localized diff-in-disc within this narrow bandwidth renders similar results to those presented in Section 4.1. Results are available from the author.

the rates of director turnover (S) are very different for both types of directors, as shown in Appendix C.4.

- To estimate the lower bound, I join the data for the group with no mayor turnover to the two trimmed datasets for lower bounds (one for appointed directors and one for not appointed directors). Then I regress the change in IDEB scores on an indicator for mayor turnover and its interaction with an indicator of the director being politically appointed.

$$Y_{sm} = \alpha + \beta_1 P_m + A_{sm}(\gamma_1 + \gamma_2 P_m) + \varepsilon_{sm} \quad (13)$$

- I do the same with the trimmed datasets for the upper bound.
- The $\hat{\gamma}_2$ of each of the two regressions gives me the bounds for $\hat{\tau}_{disc}$.

Using this procedure, I obtain the bounds [-0.473, -0.211].

C.7.3 Inference

To make inference about the bounds, I use the bootstrap. For each of 50,000 replications:

- I first draw, with replacement, a sample of appointed directors (with or without attrition) within the narrow bandwidth.⁵² With that data, I calculate p_0^a .
- I then draw a sample with replacement from the set of schools that did not experience director turnover, within the narrow bandwidth, and that had appointed directors. I trim the set of schools without mayor turnover according to the $\hat{p}_0^a$ estimated before, applying the $\hat{p}_0^a$ and $1 - \hat{p}_0^a$ quantiles to the distribution of $Y|D = 0, S = 1$ within this sample. With that data, I build a trimmed sample of appointed directors for a lower bound, and a trimmed sample of appointed directors for an upper bound.
- I replicate steps 1-3 for unappointed directors, estimating p_0^{-a} and creating a trimmed sample of unappointed directors for a lower bound, and a trimmed sample of appointed directors for an upper bound.
- I merge the datasets for appointed and unappointed directors, creating datasets for a lower and an upper bound.

⁵²The following steps take into account whether this sample has more director turnover in the treatment or in the control group, adjusting accordingly as explained in Lee (2009). For brevity below I describe the steps I take when the bootstrapped sample has more attrition in the mayor-turnover group, which is by far the most common scenario.

- I estimate Equation 13 with each of the two datasets to estimate the difference in the treatment effect for appointed and unappointed directors. I store the two values of $\hat{\gamma}_2$ from each of the two regressions into corresponding vectors.

As a result of this bootstrapping exercise, I obtain two distributions, one of lower bounds and one of upper bounds. I then estimate the standard deviation of those distributions, and use it to build a confidence interval for the bounds following [Imbens and Manski \(2004\)](#), as suggested by [Lee \(2009\)](#):

$$\left[\hat{\Delta}^{LB} - \bar{C}_n \times \frac{\hat{\sigma}_{LB}}{\sqrt{n}}, \hat{\Delta}^{UB} + \bar{C}_n \times \frac{\hat{\sigma}_{UB}}{\sqrt{n}} \right] \quad (14)$$

The value of $\bar{C}_n$ is chosen such that it satisfies:

$$\Phi \left(\bar{C}_n + \sqrt{n} \frac{\hat{\Delta}^{LB} - \hat{\Delta}^{UB}}{\max(\hat{\sigma}_{UB}, \hat{\sigma}_{LB})} \right) - \Phi(-\bar{C}_n) = 1 - \alpha \quad (15)$$

Following this procedure, I obtain a 95% confidence interval of [-0.51, -0.15].

C.8 Mechanisms, as measured through the government's survey of school directors

To explore mechanisms I replicate the diff-in-disc using, as the dependent variable for Equation 5, changes in director responses in the ANRESC official survey. In particular, I leverage items in a module called *Views about school problems and obstacles to management*. First, I leverage their answers on a question asking them whether their work is supported by higher instances. That's a question directly aligned with my theory of upward embeddedness, and one where I find significant effects (Table 3). Next, I leverage director answers on questions asking them about the extent to which the school's functioning was hindered by a number of problems, including insufficient financial resources, insufficient teachers, or teacher turnover. These questions, which address potential alternative mechanisms through which political turnover could hurt the effectiveness of appointed directors, are measured on a 4-point scale (from "no" to "yes, a lot"). The diff-in-disc returns statistically insignificant results for all of them, as shown below.

Table 10: Difference-in-discontinuity estimates of the differential impact of political turnover on directors reporting problems with teacher turnover

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Political turnover	0.102 (0.101)	0.163 (0.155)	0.199 (0.141)	0.194 (0.140)
$\hat{\gamma}_2$: Political turnover $\times$ Appointed		-0.113 (0.217)	-0.103 (0.212)	-0.083 (0.217)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.211	0.212	0.212	0.212
N	1812	1807	1807	1749

See notes under Table 2.

Table 11: Difference-in-discontinuity estimates of the differential impact of political turnover on directors reporting problems with insufficient teachers

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Political turnover	-0.128 (0.137)	-0.059 (0.231)	0.023 (0.217)	0.046 (0.228)
$\hat{\gamma}_2$: Political turnover $\times$ Appointed		-0.104 (0.250)	-0.152 (0.273)	-0.312 (0.289)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.114	0.130	0.130	0.130
N	1325	1321	1321	1274

See notes under Table 2.

Table 12: Difference-in-discontinuity estimates of the differential impact of political turnover on directors reporting problems with insufficient financial resources

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Political turnover	0.158 (0.150)	-0.065 (0.252)	-0.163 (0.229)	-0.100 (0.233)
$\hat{\gamma}_2$: Political turnover $\times$ Appointed		0.433 (0.301)	0.484 (0.271)	0.424 (0.290)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.169	0.221	0.221	0.221
N	1847	1842	1842	1783

See notes under Table 2.

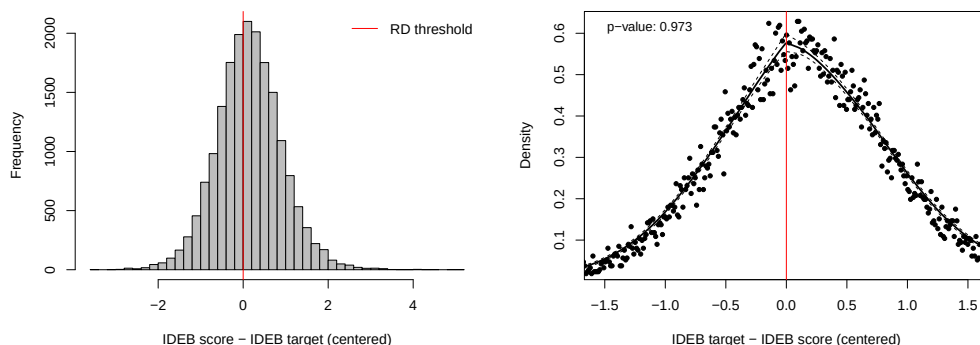
D Additional details of the regression discontinuity

D.1 Continuity of pre-treatment covariates and the forcing variable

Table 13: Continuity in pre-treatment covariates at the director, municipality, and school level, estimated by applying Equation 8 with pre-treatment covariates as the dependent variable

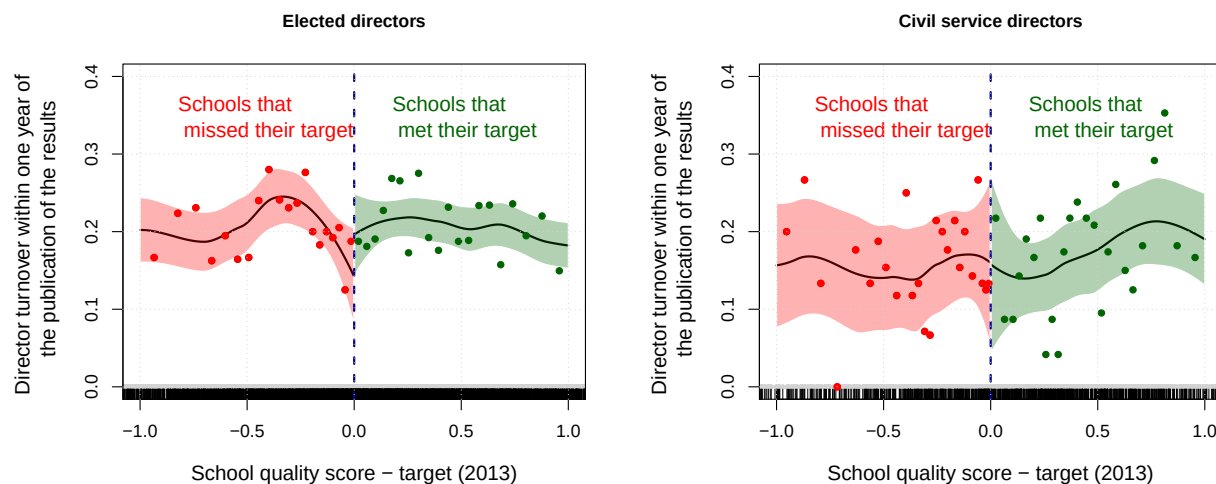
	RD estimate	Standard error	p value
Director is appointed	0.019	0.021	0.369
Director is elected	-0.001	0.019	0.956
Director is civil service	-0.027	0.012	0.023
Director is female	0.008	0.013	0.556
Director is aged <40	-0.002	0.018	0.913
Director is aged 40-49	0.029	0.019	0.130
Director is aged 50+	-0.026	0.019	0.157
Director is white	-0.012	0.022	0.586
Director is black/brown	-0.003	0.022	0.875
Director has other race	0.014	0.007	0.057
Director has < tertiary education	-0.001	0.009	0.912
Director has a tertiary degree	0.001	0.009	0.912
Director has a postgraduate degree	0.008	0.019	0.647
Director has no other job	-0.002	0.019	0.909
Director has <6 years of teaching experience	-0.020	0.015	0.189
Director has 6-15 years of teaching experience	-0.004	0.019	0.853
Director has >15 years of teaching experience	0.028	0.020	0.159
Director has <3 years of director experience	0.005	0.019	0.798
Director has 3-10 years of director experience	0.044	0.021	0.038
Director has >10 years of director experience	-0.048	0.015	0.002
Director has held position for <3 years	0.019	0.018	0.298
Director has held position for 3-10 years	0.008	0.019	0.676
Director has held position for >10 years	-0.034	0.011	0.002
Municipality GDP per capita (log)	-0.009	0.031	0.766
Municipality population (log)	-0.090	0.072	0.209
Municipality deaths per 1,000	0.029	0.060	0.626
Mayor is in their first term	0.019	0.017	0.264
Municipal electoral concentration	0.001	0.005	0.863
Mayor belongs to a large, programmatic party	-0.003	0.018	0.881
School is rural	0.017	0.015	0.276
Number of staff in the school	-1.247	0.844	0.140
School is in a settlement	0.006	0.004	0.126
School is in indigenous land	0.002	0.002	0.315
School is in quilombola land	0.002	0.003	0.503
Students per classroom (average)	-0.252	0.175	0.148
School socioeconomic index	-0.307	0.239	0.199
School IDEB target for 2013	-0.001	0.041	0.977
School IDEB score in 2011	-0.017	0.045	0.711
School ANRES test scores 2011	-0.018	0.036	0.616
School student passing rate 2011	0.002	0.004	0.643

Figure 16: Continuity of the forcing variable



D.2 Treatment heterogeneity among unappointed directors

Figure 17: Effect of meeting the performance target on director performance, for schools whose director was unappointed



See notes under Figure 2.

Table 14: Regression discontinuity estimates of the effect of reaching the school quality target on director turnover, by whether the director was elected

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Quality target met	-0.033 (0.018)	-0.066** (0.021)	-0.068** (0.021)	-0.074*** (0.022)
$\hat{\gamma}_2$: Quality target met $\times$ Elected		0.104** (0.039)	0.105** (0.038)	0.103** (0.040)
$\hat{\beta}_1 + \hat{\gamma}_2$		0.038 (0.032)	0.038 (0.032)	0.029 (0.033)
State fixed effects			✓	✓
Predictors of Elected				✓
Bandwidth	0.518	0.520	0.520	0.520
N	8503	8432	8432	7603

See notes under Table 4.

Table 15: Regression discontinuity estimates of the effect of reaching the school quality target on director turnover, by whether the director was civil service

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Quality target met	-0.033* (0.014)	-0.035* (0.015)	-0.036* (0.015)	-0.041** (0.016)
$\hat{\gamma}_2$: Quality target met $\times$ Civil service		0.006 (0.055)	0.008 (0.055)	0.005 (0.059)
$\hat{\beta}_1 + \hat{\gamma}_2$		-0.029 (0.063)	-0.028 (0.063)	-0.036 (0.069)
State fixed effects			✓	✓
Predictors of Civil service				✓
Bandwidth	0.518	0.517	0.517	0.517
N	8473	8402	8402	7574

See notes under Table 4.

D.3 Details on director elections that help explain the null result for elected directors

The results of the RDD presented in Section 4.2 show that while appointed directors are held by politicians for their performance in IDEB, but that elected (and civil service) ones are not. The fact that voters (teachers and parents, mostly) are not holding directors accountable is remarkable, given their stakes in the quality of the school, their relatively high levels of information, and their ability to take action through voting and coordination among relatively small groups.

My interviews in the education sector provide some insights as to why this may be the case. The election of school directors – which is in practice the most common alternative in this setting – establishes even more complex accountability relationships. Director elections are regulated by municipal laws, but generally they provide for the electoral participation of teachers, other school staff, and parents (or students, in high schools), sometimes with larger weights for teachers' votes. Interviews provided evidence of why director elections fail to boost accountability and performance. Elections for school director are often uncompetitive – several school directors reported having been elected with vote shares above 95%. My survey of school directors provides some quantitative data on school director elections, representative for the urban areas of all but the largest municipalities in Rio Grande do Norte. In this setting, elected directors reported a median level of support of 90% of the votes in the last election. More than 70% of directors report having run unopposed.⁵³

Oftentimes schools have no candidates, and in those cases the director is normally directly appointed by the mayor. When the election does happen, it is easily prone to capture. A director

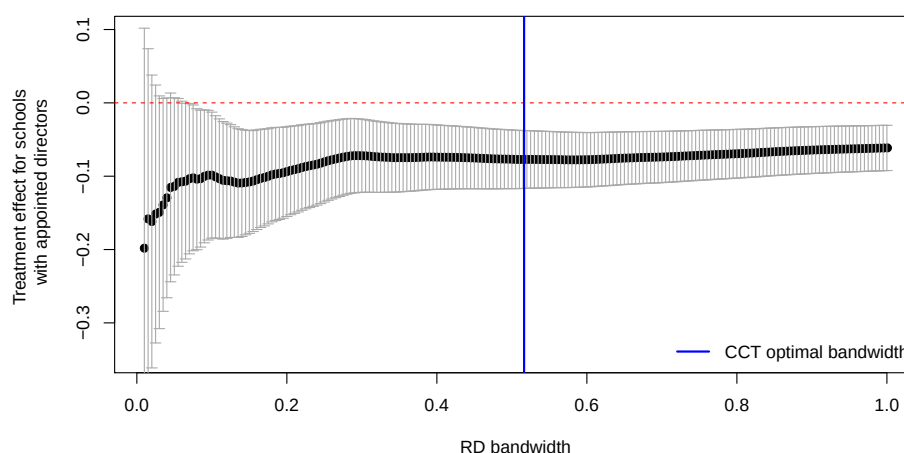
⁵³The uncompetitive nature of director elections is not unique to this state. In the municipal school director elections held in 2015 in Vitória da Conquista (the third largest city in the state of Bahia), the average vote share of the winner for schools where valid elections were held was 95.96%. Over a third of the schools had no candidates. The results for the urban, municipal school director elections held in 2013 in Santarém, the third largest city in the state of Pará, had winners with an average vote share of 81.95%.

said that “in community consultations [elections] it is very easy to get the support of the community – your supporters show up to vote, the rest does not show up.”⁵⁴ In practice, the results of the election are usually determined by teachers, especially tenured ones. These dynamics of capture are strengthened by the erosion of the democratic norm once elections are established – interviewees often reported a significant drop in community interest and participation in director elections after the first wave. In the words of a secretary, “first there was a democratic response – the first election was genuine, with interest, but the second one had just the very same candidates, and after that it just became a mere [formal] commitment, with the same people. After four years when candidates reached their re-election limit no one ran and the mayor had to appoint somebody.”⁵⁵

D.4 Alternative bandwidths

If treatment effects are sometimes insignificant with bandwidths smaller than the optimal (likely due to reduced power from smaller samples), their size remains stable.

Figure 18: Sensitivity of model 4 in Table 4 to alternative bandwidths



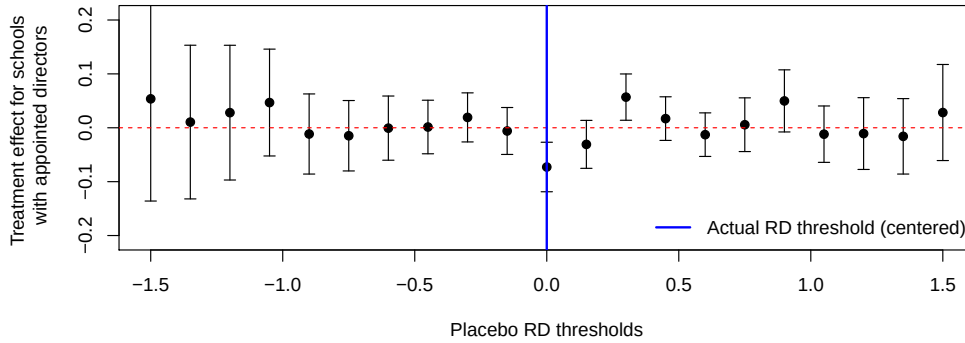
D.5 Placebo tests varying the RD threshold

Only one of the 20 placebo tests returns a statistically significant result, which is roughly what we would expect with a $\alpha = 0.05$.

⁵⁴School director interviewed in the state of Rio de Janeiro in February 2017.

⁵⁵Secretary of education interviewed in the state of Rio de Janeiro in February 2017.

Figure 19: Placebo tests for model 2 in Table 4, moving the RD threshold



D.6 Alternative sample: Municipalities with mayors from programmatic parties

This appendix replicates the diff-in-disc subsetting to municipalities where the mayor belongs to a large, programmatic party (PT or PSDB). Programmatic parties are those having identifiable platforms, and are generally thought as less likely to rely on clientelism and more likely to strengthen bureaucratic accountability (Cruz and Keefer, 2015). Until recently Brazil had two large programmatic parties, PT and PSDB, although their programmatic profile has eroded over time (Samuels and Zucco, 2018).

Table 16: Regression discontinuity estimates of the effect of reaching the school quality target on director turnover, by whether the director was appointed, among municipalities with a programmatic party in office

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Quality target met	-0.046 (0.029)	0.027 (0.043)	0.031 (0.043)	0.031 (0.045)
$\hat{\gamma}_2$: Quality target met $\times$ Appointed		-0.134* (0.058)	-0.152** (0.058)	-0.137* (0.060)
$\hat{\beta}_1 + \hat{\gamma}_2$		-0.107** (0.038)	-0.121** (0.038)	-0.106** (0.039)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.652	0.666	0.666	0.666
N	2935	2911	2911	2656

See notes under Table 4.

E Additional details of the face-to-face survey of bureaucrats

The survey instrument (in English and Portuguese) is available from the author, as are descriptive statistics about municipalities in Rio Grande do Norte.

E.1 Details on sampling and non-response

I excluded the largest 17 municipalities in the state (which had as of the 2010 census more than 30,000 inhabitants) for budget and security reasons. Surveying street-level managers in these large municipalities would significantly increase the cost of the survey, and more importantly it would have exposed enumerators to the serious security challenges typical of large urban areas of the Northeast. Rio Grande do Norte is consistently ranked among the most dangerous states in Brazil.

Rural areas in all municipalities were excluded from the study's population, for three main reasons. First, rural schools, clinics, and social assistance centers in Brazil are often staffed for a limited number of days and hours per week. Second, the managers of these units often work at the municipality's urban center, and tend to direct several units at once. Third, rural areas in the Northeast are logistically hard to reach – they are often accessible only through dirt roads with limited or no GPS service, unmapped on GPS services – and pose additional security challenges. Therefore, including rural areas in the sample would have heavily increased the time and budget required for the survey, and would have risen security issues for enumerators. While there are many schools and clinics in rural areas, most of the population lives in urban areas.

Before the survey, and using the most up-to-date administrative data, I identified 1,027 schools, clinics, and social assistance centers in the urban areas of the target 150 municipalities. Throughout four weeks of fieldwork, we managed to interview 926 street-level managers. The gap between the two numbers is due to rejections (17 managers refused to participate), overlaps (15 units had as manager somebody who had already been surveyed), misclassification (25 units were mis-identified as urban, when in fact they were in rural areas), and failures to locate some managers (we tried at least twice with each of them). On the other hand, we located and did surveys at 38 urban units that, mostly because they were of recent establishment, were not in the administrative data.

Survey participants were recruited in their offices, and consent was collected (and recorded on the tablets we used) after providing information about the research project and their rights as participants. Participants were not compensated in any form.

E.2 Descriptive statistics

Table 17: Descriptive statistics of the survey of street-level managers, by sector

	All sectors N=926		Education N=481		Healthcare N=292		Social assistance N=153	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Age	40.71	9.76	45.20	8.34	35.27	8.37	36.99	9.57
Female	0.86	0.34	0.85	0.36	0.86	0.35	0.92	0.27
High school degree or less	0.08	0.27	0.01	0.11	0.22	0.41	0.03	0.16
College degree	0.31	0.46	0.25	0.43	0.29	0.46	0.51	0.50
Politically appointed	0.77	0.42	0.79	0.41	0.67	0.47	0.87	0.34
Elected	0.09	0.28	0.17	0.38	0.00	0.00	0.00	0.00
Civil service	0.04	0.19	0.01	0.09	0.09	0.28	0.03	0.16
Experience as a manger	4.66	4.33	5.58	4.79	3.98	3.71	3.05	2.97
Experience as a professional	15.15	10.61	20.89	9.00	8.77	7.17	8.61	10.23
Exclusive dedication	0.57	0.50	0.80	0.40	0.00	0.00	0.92	0.28
Union member	0.35	0.48	0.54	0.50	0.17	0.38	0.10	0.31
Party member	0.16	0.37	0.16	0.37	0.15	0.36	0.18	0.38
Worked for a campaign	0.40	0.49	0.42	0.49	0.34	0.47	0.47	0.50

E.3 Results of observational regressions of appointment modes

Table 18: Correlates of street-level managers' appointment mode

	Appointed	Elected	Civil service
Party member	-0.008 (0.035)	0.007 (0.026)	0.000 (0.013)
Has worked for an electoral campaign	0.117 (0.027)***	-0.054 (0.019)**	-0.033 (0.010)***
Union member	-0.174 (0.033)***	0.080 (0.024)**	0.070 (0.020)***
Experience as manager	-0.006 (0.004)	-0.005 (0.003)	0.007 (0.002)***
Experience as professional	-0.000 (0.002)	0.001 (0.001)	-0.000 (0.000)
Lives in the municipality	0.156 (0.047)***	-0.018 (0.023)	-0.091 (0.031)**
Has no other jobs	-0.046 (0.045)	-0.027 (0.041)	0.026 (0.013)*
Female	-0.007 (0.040)	-0.009 (0.029)	0.014 (0.014)
Age	0.005 (0.002)*	0.001 (0.001)	-0.003 (0.001)***
Has more than a college degree	-0.139 (0.029)***	0.059 (0.017)***	0.014 (0.013)
Has less than a college degree	0.096 (0.044)*	0.045 (0.022)*	-0.003 (0.021)
Healthcare sector (vs education)	-0.203 (0.054)***	-0.157 (0.042)***	0.087 (0.022)***
Social assistance (vs education)	0.001 (0.037)	-0.112 (0.020)***	0.035 (0.017)*
Constant	0.653 (0.093)***	0.130 (0.066)*	0.116 (0.045)*
Observations	883	883	883
R-squared	0.163	0.135	0.126

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$. HC1 standard errors in brackets.

E.4 Results of observational regressions of meetings and attitudes

Table 19: Observational regressions of self-reported number of meetings with stakeholders on street-level managers' appointment mode (baseline category is civil service), as per Equation 10

	<i>Dependent variable: Self-reported, logged number of meetings with</i>					
	Mayor (1)	Secretary (2)	Technicians (3)	City councilors (4)	Professionals (5)	Clients (6)
Appointed	0.391*** (0.101)	1.013*** (0.155)	0.541*** (0.145)	0.037 (0.079)	0.201 (0.155)	0.333** (0.128)
Elected	0.020 (0.117)	0.801*** (0.175)	0.399* (0.170)	0.015 (0.089)	0.069 (0.170)	0.278* (0.137)
Observations	786	775	786	786	786	786
R-squared	0.096	0.115	0.090	0.025	0.063	0.167

HC1 standard errors in brackets. *p<0.05; **p<0.01; ***p<0.001. All regressions control for respondents' sector, age, gender, party membership, union membership, whether they have worked for a local electoral campaign, years of experience as manager, experience as professional, whether they live in the municipality, whether they have other jobs, and whether they have more than a college degree.

Table 20: Observational regressions of self-reported number of meetings with stakeholders on street-level managers' appointment mode (versus elected), as per Equation 10

	<i>Dependent variable: Self-reported, logged number of meetings with</i>					
	Mayor (1)	Secretary (2)	Technicians (3)	City councilors (4)	Professionals (5)	Clients (6)
Appointed	0.374*** (0.071)	0.216* (0.097)	0.142 (0.104)	0.023 (0.045)	0.129 (0.072)	0.058 (0.062)
Observations	754	743	754	754	754	754
R-squared	0.082	0.080	0.077	0.025	0.062	0.167

See notes under Table 19.

Table 21: Observational regressions of attitudes about the mayor and the secretary on street-level managers' appointment mode (baseline category is civil service), as per Equation 10

	<i>Dependent variable: Agreement with (1-4 scale)</i>					
	Trust mayor (1)	Feel close to mayor (2)	Mayor & professionals aligned (3)	Mayor is concerned w/ quality (4)	Trust secretary (5)	Feel close to secretary (6)
Appointed	1.187*** (0.169)	1.230*** (0.182)	0.794*** (0.175)	0.790*** (0.165)	0.602*** (0.154)	0.844*** (0.172)
Elected	0.621** (0.195)	0.644** (0.215)	0.430* (0.199)	0.378* (0.188)	0.343 (0.177)	0.610** (0.191)
Observations	785	781	786	785	774	774
R-squared	0.247	0.193	0.145	0.172	0.114	0.138

See notes under Table 19.

Table 22: Observational regressions of attitudes about the mayor and the secretary on street-level managers' appointment mode (baseline category is civil service), as per Equation 10

<i>Dependent variable: Agreement with (1-4 scale)</i>						
	Trust mayor	Feel close to mayor	Mayor & professionals aligned	Mayor is concerned w/ quality	Trust secretary	Feel close to secretary
height	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.571*** (0.102)	0.591*** (0.120)	0.372*** (0.101)	0.419*** (0.095)	0.259** (0.094)	0.237** (0.090)
Observations	753	749	754	753	742	742
R-squared	0.143	0.100	0.086	0.109	0.070	0.059

See notes under Table 19.

E.5 Details of the conjoint experiment with bureaucrats

Table 23: Attribute and attribute values for bureaucrat profiles used in the conjoint experiment

Attribute	Values
Education	Bachelors degree / Masters degree
Experience	3 years / 10 years
Political connections	Has / lacks connections with the municipal government
Relationship to professionals	Good / bad relationship to professionals
Unit performance	Targets were met / not met
Selection mode	Civil service exam / election by the community / political appointment

The next table details the regression results visualized in Figure 5. These correspond to the following choice tasks of the conjoint experiment: (i) *Communication*: Which of these [directors/managers/coordinators] do you think would have a better communication with the Secretariat of [education/healthcare/social assistance]?; (ii) *Implementation*: Which of these [directors/managers/coordinators] do you think would have more chances of implementing changes requested by the mayor's office?; (iii) *Resources*: Which of these [directors/managers/coordinators] do you think would obtain a reform for the [school/clinic/social assistance center]?; (iv) *Results*: Which of these [directors/managers/coordinators] do you think would achieve better scores in [student learning/community healthcare/social assistance center indicators]?

E.6 Conjoint results among subsets of respondents

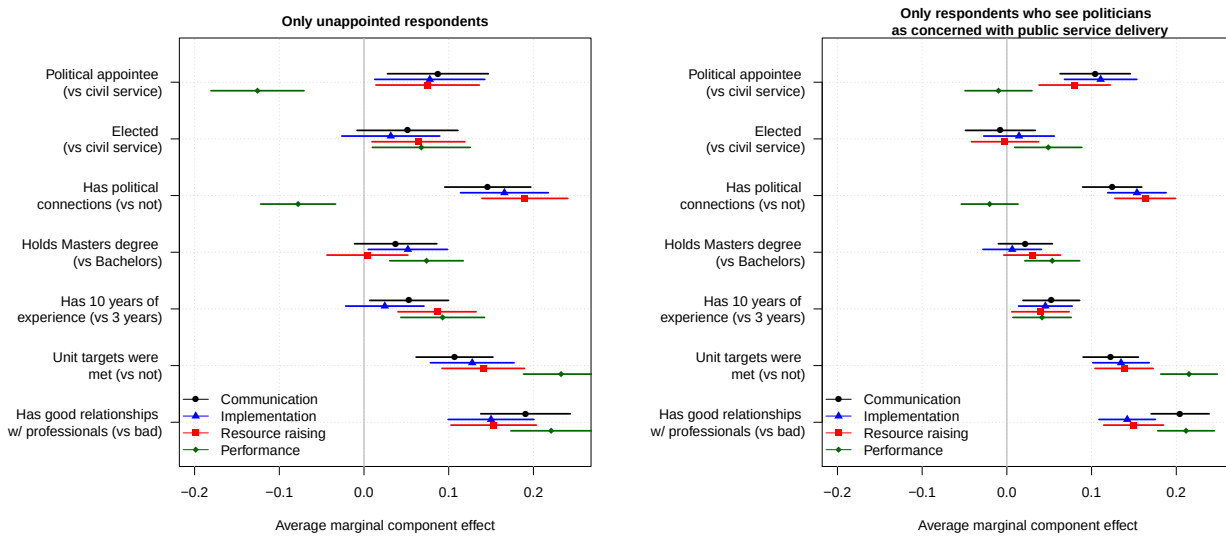
The plot on the left includes only responses from bureaucrats who are not political appointees. The plot on the right includes only responses from bureaucrats who expressed the highest level of agreement with the following statements: "The mayor and [education/healthcare/social assistance] professionals have the same priorities for [units, i.e., schools/clinics/social assistance centers]"; "The mayor is concerned with improving the quality of [units]"; "The secretariat of [education/healthcare/social assistance] helps us improve the performance of the [unit]"; and "The secretariat of [area] holds this [unit] accountable for its results."

Table 24: Results of the conjoint experiment with street-level managers

	Communication	Implementation	Resources	Performance
Appointment: Political (vs civil service)	0.1*** (0.015)	0.097*** (0.015)	0.083*** (0.015)	-0.056*** (0.014)
Appointment: Election (vs civil service)	0.008 (0.014)	0.019 (0.014)	0.029* (0.014)	0.059*** (0.014)
Political connections: Yes (vs no)	0.137*** (0.012)	0.16*** (0.012)	0.174*** (0.012)	-0.042*** (0.011)
Education: Masters (vs Bachelors)	0.019 (0.012)	0.014 (0.012)	0.016 (0.012)	0.059*** (0.011)
Experience: 10 years (vs 3 years)	0.055*** (0.012)	0.038*** (0.011)	0.057*** (0.012)	0.06*** (0.012)
Unit performance: Targets were met (vs not met)	0.126*** (0.012)	0.131*** (0.012)	0.134*** (0.012)	0.22*** (0.012)
Relationship to professionals: Good (vs bad)	0.184*** (0.012)	0.148*** (0.012)	0.15*** (0.012)	0.214*** (0.012)
Number of respondents	917	917	916	917
Number of valid profiles	7224	7224	7222	7224

*p<0.05; **p<0.01; ***p<0.001. Standard errors clustered at the respondent level.

Figure 20: Conjoint results among subsets of respondents



See notes under Figure 5.

F Additional details of the online survey of politicians

The survey instrument (in English and Portuguese) is available from the author.

F.1 Respondent recruitment and non-response

The state audit court of Rio Grande do Norte sent the survey to all mayors and to secretaries of five key areas (education, healthcare, social assistance, finance, and human resources) in the 167 municipalities of the state through its online platform.⁵⁶ Participation was voluntary. A total of 455 politicians participated and finished the survey, of which 50 were mayors and 405 secretaries.

⁵⁶The survey was also sent to city councilors, but their responses are excluded here because the theory in this paper focuses on executive politicians. Including city councilors' responses, however, does not alter the results.

Table 25: Correlates of the number of responses per municipality

	Respondents (log)	No respondents (dummy)	Respondents (log) w/o zeroes
Population (logged)	0.024 (0.062)	0.004 (0.037)	0.036 (0.048)
GDP per capita (logged)	−0.216 (0.139)	0.114 (0.108)	−0.115 (0.136)
Deaths per thousand	0.033 (0.041)	−0.018 (0.023)	0.007 (0.035)
Mayor was reelected in 2016	0.373 (0.149)*	−0.218 (0.085)*	0.101 (0.122)
Constant	2.502 (1.423)	−0.702 (1.051)	1.956 (1.232)
R-squared	0.125	0.138	0.025
Observations	75	75	64

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$. HC1 standard errors in brackets.

These respondents come from 142 municipalities. Municipalities where mayors were in their second term were more likely to participate, but conditional on some politicians responding there are no significant correlations between the number of respondents and basic political and socioeconomic characteristics of the municipality, as shown in Table 25.

Survey participants were recruited through the court's online platform, where they received information about the research project and their rights as participants. Participants were not compensated in any form.

F.2 Descriptive statistics

Table 26: Descriptive statistics for the survey of politicians, by position

	All (N=455)		Mayors (N=50)		Secretaries (N=405)	
	Mean	SD	Mean	SD	Mean	SD
Age	42.62	10.61	48.68	11.09	41.87	10.32
Female	0.57	0.50	0.22	0.42	0.61	0.49
High school degree or less	0.10	0.30	0.32	0.47	0.07	0.26
College degree or more	0.79	0.41	0.58	0.50	0.81	0.39
Party member	0.52	0.50	0.98	0.14	0.46	0.50
Experience as bureaucrat (years)	0.72	0.45	0.38	0.49	0.76	0.43
Experience as politician (years)	4.61	4.88	7.26	6.90	4.28	4.47

F.3 Details of the conjoint experiment with politicians

Table 27: Attribute and attribute values for bureaucrat profiles used in the conjoint experiment with politicians

Attribute	Values
Education	Bachelors degree / Masters degree
Experience	3 years / 10 years
Political connections	Has / lacks connections to the municipal government
Union membership	Participates / does not participate in a union
Gender	Woman / Man
Contract type	Civil service contract / Temporary contract

Table 28 details the regression results visualized in Figure 6. These correspond to the following choice tasks of the conjoint experiment: (i) Which of these bureaucrats do you think would have a better communication with the local government?; (ii) Which of these bureaucrats do you think would have more chances of implementing changes requested by the local government?; (iii) Which of these bureaucrats do you think would work extra hours if necessary?; and (iv) Which of these bureaucrats do you think would achieve better performance?

Table 28: Results of the conjoint experiment with politicians

	Communication	Implementation	Effort	Performance
Contract: Temporary (vs civil service)	0.102*** (0.019)	0.132*** (0.019)	0.217*** (0.017)	0.066*** (0.018)
Political connections: Yes (vs no)	0.121*** (0.019)	0.058** (0.018)	0.058** (0.018)	-0.005 (0.018)
Education: Masters (vs Bachelors)	0.018 (0.017)	0.015 (0.018)	-0.022 (0.017)	0.102*** (0.018)
Experience: 10 years (vs 3 years)	0.03 (0.018)	-0.021 (0.019)	-0.019 (0.018)	0.068*** (0.018)
Union membership: Yes (vs no)	-0.039* (0.017)	-0.024 (0.018)	-0.061*** (0.017)	-0.011 (0.018)
Gender: Male (vs female)	-0.055** (0.017)	-0.082*** (0.016)	-0.063*** (0.016)	-0.106*** (0.017)
Number of respondents	455	455	455	455
Number of valid profiles	3640	3640	3640	3640

*p<0.05; **p<0.01; ***p<0.001. Standard errors clustered at the respondent level.