The impact of the Voting Rights Act on city elections and finances

Richard T. Boylan*

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The Voting Rights Act had two distinct effects on city elections: it removed barriers to voting faced by minorities and increased the election of council members by district. We examine the impact of these changes on municipal expenditures using a panel of 2,727 cities over the years 1957 through 2012. We find that the expanded use of district elections increased non-infrastructure spending and pension benefits per city worker. Further, the expansion of voting rights was followed by reduced infrastructure spending and higher pension benefits per city worker. These results support the theoretical arguments behind one of the major election law controversies: whether city council members should be elected by district. Our results are consistent with 'reformers' claims that district elections hinder the long term viability of cities, and with 'new reformers' assertions that district elections give greater influence to minorities. JEL: D72, H75

*Rice University, Department of Economics, MS#22, 6100 S. Main Street, Houston, TX 77005-1892, rboylan@rice.edu. This paper was supported by a grant from George Mason University School of Law, Law & Economics Center. Geoffrey Lawrence shared the updated data on union laws, while Fernando Ferreira provided data on mayoral elections. Chris Elmendorf, Bernie Grofman, Bard Hartstad, Daniel Hemel, Daniel Ho, Vivian Ho, Bruce Johnsen, Andrea Prat, and Razvan Vlaicu provided helpful comments.

1 Introduction

In 1964, only 38% of the black voting age population in Southern states was registered to vote [Campbell and Feagin, 1975]. The Voting Rights Act of 1965 removed barriers to minority voting, but minorities still struggled to attain elected office. In response, Congress amended the Voting Rights Act in 1982 to outlaw procedures "leading to nomination or election ... not equally open to participation by members of a class of citizens." This amendment led to an increase in the number of city council members elected by district, which in turn led to an increase in the number of minority city council members [Sass and Mehay, 1995, Trebbi et al., 2008]. In this paper we examine the impact on city spending of the expansion of suffrage and use of district elections brought upon by the Voting Rights Act.

Increased minority turnout changed the economic characteristics of voters. For instance, in 1960, only 35% of nonwhite housing units were owner occupied rather than renter occupied, versus 61% of white housing units. Thus, we expect the Voting Rights Act to have increased the proportion of voters that are renters. Since homeowners tend to invest more social capital and live longer in a community, higher representation of renters among voters could lead to decisions adverse to a city's long run viability [DiPasquale and Glaeser, 1999]. Therefore, we might expect increased voting by minorities to shift spending priorities away from preserving the long run viability of a city, because minority voters may face lower costs of moving out of a city's boundaries.

Similarly, the increased use of district elections could have led to spending decisions that negatively affect the long run viability of cities. The reason for this is that city councils representing different districts face a common pool problem. That is, council members fully value the benefits of public spending in their own district, but internalize only a fraction of their costs. This common pool problem has been shown to lead to excess pork barrel spending, under-provision of public investment goods, and excessive debt.[1]

We examine two types of expenditures that have been linked to the long run viability of cities: infrastructure spending and pension benefits. Infrastructure investments use current resources to

[1]Formally, the common pool problem has been modeled as inefficient universalism [Weingast et al., 1981] or by spending that only benefits a minimum winning coalition of districts [Leblanc et al., 2000, Besley and Coate, 2003, Battaglini and Coate, 2008]. Both formalizations lead to the similar predictions.

provide future benefits, while pension benefits represent the opposite: employees provide benefits to current residents, but part of the employees' compensation is shifted into the future.[2] For instance, in 2001, Lee P. Brown was facing a tough re-election campaign for mayor of Houston, in what would have been his last term in office (because of term limits). He was instrumental in a large increase in municipal employees' pension benefits and won reelection in a runoff, by a 51.67% to 48.33% margin. He later justified his decision to increase pension benefits by the fact that he was told that it was budget neutral, and the fact that he did not have the funds to give municipal employees "the raises they deserved" [Brown, 2004].

We examine the impact of the expansion of suffrage and use of district elections on city finances using a panel of 2,727 cities over the years 1957 though 2012. We find that the expanded use of district elections raised non-infrastructure spending and pension benefits per city employee. Further, the removal of barriers to minority voting lowered infrastructure spending and increased pension benefits. Between 1964 and 1968, voter turnout for the Southern cities in our sample increased by 6%. Our estimates imply that this increased turnout decreased infrastructure spending by $6 \times 0.014 = 8.4\%$.

In 1981, the fraction of councils elected by district was 0.19 for Southern cities in our sample, versus 0.31 for non-Southern cities. By 2012, the fraction of councils elected by district had increased to 0.47 for Southern cities in our sample, versus 0.35 for non-Southern cities. Thus, it appears that the 1982 amendment to the Voting Rights act raised the fraction of councils elected by district in the South by $0.24 = (0.47 - 0.19) - (0.35 - 0.31)$. Our estimates imply that a 0.24-increase in the fraction of the council elected by district leads to a $0.24 \times 0.378 = 9\%$ increase in city non-infrastructure spending.[3]

These findings are of importance given recent concerns regarding the fiscal viability of cities. Between 2007 and 2013, residents of twenty-eight cities suffered drastic cuts in fire and police protection as their cities went into bankruptcy or receivership [Anderson, 2014]. Population losses and overly generous municipal pensions are common explanations for these cities' financial plight

[2]The costs of pension benefits would not be shifted into the future if pensions were fully funded, which they are not [Brown and Wilcox, 2009, Novy-Marx and Rauh, 2011].

[3]This estimate assumes that the impact of district elections on spending is the same for all cities. This assumption is relaxed in Section 7.2.

[Cooper and Walsh, 2010, Greenhut, 2010, Niedowski, 2011]. In many municipalities, fiscal difficulties have been compounded by the need to repair an aging infrastructure [Hulten and Peterson, 1984, McWhirter, 2016]. Thus, our results suggest that changes in electoral rules may have contributed to cities' difficulties.

Our study contributes to understanding the economic consequences of the Voting Rights Act by examining the impact of both the expansion of suffrage and use of district elections. Prior work examined changes in state spending that follow the expansion of voting rights, but did not control for the adoption of district elections. For instance, Husted and Kenny [1997] and Cascio and Washington [2014] found that the elimination of poll taxes and literacy tests raised the portion of state spending allocated to welfare, and the portion of state transfers allocated to counties with higher black population shares.

In contrast, Snyder and Ueda [2007] examined changes in state spending in response to the changes in electoral rules that followed the Voting Rights Act but did not control for the expansion of suffrage. Before the Voting Rights Act, some metropolitan areas elected their state legislature representatives in at-large elections. Snyder and Ueda found that switching to single-member districts for the selection of state legislators reduced the amount of state aid received by these metropolitan areas.

Our results also support the theoretical arguments behind one of the major election law controversies; namely, whether city councils should be elected at-large or by district. In district elections, individuals vote only for their district's council member. In contrast, in at-large elections, all voters in a city choose all council members. For instance, in November 2013, all voters in Winfield, Iowa voted for three candidates from the same list of four candidates, and the three candidates with the most votes were elected to the city council (the sample ballot is reproduced in the next page).

Precinct Official's Initials

OFFICIAL BALLOT
City Election
Henry County, Iowa-November 5, 2013
Northeast-Winfield (0302)

Shelly Barber
County Auditor & Commissioner of Elections

Style:7-B

INSTRUCTIONS TO VOTERS

Using blue or black ink, completely fill in the oval next to the candidate or question response of your choice like this:

WRITE-IN: You must darken the oval AND write the name of your candidate in the space provided.

Do not cross out. If you change your mind exchange your ballot for a new one.

City of Winfield

For Council Member

Vote for no more than THREE.

- Ryan J Kinneberg
- Kathy K Nelson
- Ryan Rees
- Jan Walter
- (Write-in vote, if any)
- (Write-in vote, if any)
- (Write-in vote, if any)

SAMPLE BALLOT

Shelly Barber

COUNTY COMMISSIONER OF ELECTIONS
Henry County, Iowa

Municipal reformers (1890–1960) supported at-large elections claiming that they improve the quality of municipal government, since council members elected at-large make decisions benefiting the entire city, rather than just one geographic segment.[4] In contrast, the new reformers (1980s) favored district elections, asserting that they lead to city councils that are more representative and more responsive to the entire city.

Prior evidence that at-large elections improve governance has been purely anecdotal.[5] While there is strong evidence that district elections increase minority representation, prior studies have not been able to show that minorities prefer policies enacted in cities with district elections over policies selected in cities with at-large elections [Engstrom and McDonald, 1986]. In contrast, we find that at-large elections raise the share of city spending allocated to infrastructure and lower pension benefits, consistent with the reformers' position. We also find that district elections have a similar effect on city spending as measures that increased minority turnout: they both decrease the share of speding allocated to infrastructure and increase pernsion benefits. Thus our findings are also consistent with district elections giving greater influence to minorities, the new reformers' position.

Prior longitudinal studies of U.S. cities found that changing from at-large elections to district elections does not affect city spending [MacDonald, 2008, Coate and Knight, 2011]. This result could be driven by the sample period in these studies: 1982 through 2002. Between 1957 to 2012, the major change in election laws was the shift from at-large to district elections that started in 1975 and accelerated in 1982 (see Figure 1 on page 12). Thus, by starting the sample in 1982, these studies may have had too few observations before the changes in election rules were enacted to obtain significant estimates.

More generally, Persson [2002] lamented that "the lack of time variation [in political institutions]

[4]Many cities adopted the reformers' model city charter, thus leading 44% percent of U.S. cities to elect councils at-large in 1968 vs. 16% in 1934 [Frederickson et al., 2004]. The first model city charter in the United States recommended abandoning district representation and instead electing city councils in at-large, non-partisan elections [National Municipal League, 1900, page 216]. The most current model city charter still recommends at-large elections, but makes two exceptions [National Civic League, 2003, page 9]. When necessary to assure minority representation, some council members should be elected by district, while others should be elected at-large. The entire council may need to be elected by district to comply with a court order.

[5]For instance, the reform of Galveston's city government is often credited for building a seawall, raising the level of the entire city, and avoiding a replay for the Sept 8, 1900 disaster [Bixel, 2011]. Rauch [1995] provides statistical evidence that reformed cities spend more on infrastructure. However, Rauch measures 'reform' by the adoption of civil service, rather than at-large elections.

is unfortunate in that it provides us with almost no 'experiments' in the form of regime change" [page 891]. Beath et al. [2016] obtained time variation in political institutions by randomly choosing which of 250 village councils in Afghanistan would be elected by district or at-large.[6] Beath et al. found that in villages where councils were elected at-large, council members were more educated, development projects were implemented faster, and children were less likely to have diarrhea. In our study, we are able to obtain time variation by looking at a large number of U.S. cities over a long time period, and thus observe 1,001 cities that changed the fraction of the council elected at-large by at least 20%.

We first discuss the Voting Rights Act. Next, we discuss the data, provide a graphical illustration of our findings, describe our empirical framework, and present our empirical results.

2 The Voting Rights Act and district voting

In 1964, only 38% of the black voting age population in Southern states was registered to vote [Campbell and Feagin, 1975].[7] These low registration rates were in part due to literacy tests and poll taxes. However, even in states such as Florida that did not have literacy tests and poll taxes, blacks where kept from voting through threats of bodily harm and job loss. For instance, in 1958, only seven out of 10,930 adult African Americans in Gadsden County, Florida, were registered to vote [Wood, 2016]. In response, the States ratified the Twenty-Fourth Amendment to the U.S. Constitution and the U.S. Congress passed the Voting Rights Act of 1965. Section 2 of the Act prohibited voting practices or procedures that discriminate on the basis of race, color, or membership in a language minority group. Section 5 required pre-approval by the Department of Justices of changes in voting rules for jurisdictions specified in Section 4; namely, jurisdictions that required literacy tests to vote and where less than 50 percent of persons of voting age had voted in 1964, 1968 or 1972.[8] Congressional actions were effective in increasing minority turnout and

[6] Before the intervention, councils members had tended to be the elders of the village.

[7] The Southern states are defined as Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas and Virginia.

[8] In 1975, the definition of "literacy tests" was expended to include the practice of providing election information only in English in jurisdictions were members of a single language minority group constitute more than five percent of the citizens of voting age.

by 1968, 62% of the Southern black voting age population was registered to vote. Nonetheless, at-large elections combined with racial bloc voting (where individuals only vote for candidates of their own race) limited the influence of minorities [Trebbi et al., 2008].

In 1975, courts began ordering district elections for city councils to ensure minority representation.[9] However, the legal basis for these orders was muddled [O'Rourke, 1982]. For instance, courts ordered district elections in two cases where defendants conceded the unconstitutionality of at-large council elections.[10] In another case, some cities agreed to conduct district elections, while other cities refused to abandon at-large elections until 1985.[11] In 1980, the U.S. Supreme Court clarified the law: at-large elections were not prohibited unless the defendant could prove that a city intended to limit minority representation when it enacted at-large elections.[12] In the 1982 amendment of the Voting Rights Act, the U.S. Congress replaced proof of intent with a totality of circumstances test.[13] In particular, courts were to assess the extent of a history of discrimination in the right to vote when deciding whether at-large elections were legal [Katz et al., 2006].

Katz et al. [2006] examined all electronically published decisions in which plaintiffs filed a claim under Section 2 of the Voting Rights Act between 1982 and 2004.[14] They found that many courts assessed a history of discrimination by examining conduct dating from the nineteenth century and continuing through much of the twentieth, such as literacy tests and poll taxes. More generally, they found that plaintiffs were more likely to win in Section 5 jurisdictions (i.e., jurisdictions that had low voter turnout and literacy tests).

Similarly, O'Rourke [1982] reviewed court decisions on the legality of at-large elections for the period up to the article's publication (i.e., between 1975 and 1982). He found that the old Fifth Circuit (Alabama, Florida, Georgia, Louisiana, Mississippi, and Texas) emerged as the focal point for challenges to at-large elections, perhaps because of a history of voter discrimination in these states. Alternatively, challenges to at-large elections may have been more common in the old Fifth

[9] In contrast, courts mandated district elections for state legislatures starting in 1966 [Snyder and Ueda, 2007].

[10] *Wallace v. House*, 515 F.2nd (5h Cir. 1975) (involving the town of Ferriday, Louisiana) and *Perry v. City of Opelousas*, 515 F.2d 639 (5th Cir. 1975).

[11] *Stewart v. Waller*, 404 F. Supp. 206 (N.D. Miss. 1975). See Davidson and Grofman [1994].

[12] *City of Mobile v. Bolden*, 446 U.S. 55 (1980).

[13] *Thornburg v. Gingles*, 478 U.S. 30 (1986), *Johnson v. DeGrandy*, 512 U.S. 997 (1994).

[14] According to the ACLU, approximately 1 out of 5 Section 2 cases filed in Georgia and in South Carolina ended with a published decision [Katz et al., 2006].

Circuit, because justices in that Circuit were more receptive to legal arguments against at-large elections.

The timing of the legislation, and the court decisions, leads us to the following predictions. Voter turnout increased in 1960 and to a greater amount after 1964. Further, starting in 1975 and accelerating in 1982, cities increasingly used district elections, in particular in jurisdictions that had a history of voter suppression.

3 Data and sample

We collected data on the political system for 2,727 cities over the years 1957 through 2015.[15] Specifically, it was determined whether council members are elected at large or by district; whether elections are partisan or nonpartisan; and whether the executive is the mayor, the city-manager, or a commission.[16] Over our sample period, 1,001 cities increased or decreased the fraction of the council elected at-large by at least 20%, 382 cities changed the assignment of executive power (mayor, city manager, or commission), while 108 cities changed whether elections are partisan or

[15] The dataset is International City Managers Association, *Municipal Year Book*, various years. There are numerous errors, inconsistencies, and missing values in this data that were resolved by cross checking it with:

1. Chandler Davidson and Bernard Grofman, Quiet Revolution in the South: The impact of the Voting Rights Act, 1965–1990. ICPSR version. Houston, TX: Rice University, Sociology Dept./Irvine, CA: University of California at Irvine, School of Social Sciences [producers], 1994. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 1996. `http://doi.org/10.3886/ICPSR06646.v1`.
2. United States Department of Commerce. Bureau of the Census. Census of Governments, 1992: Government Organization. ICPSR04421-v2. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2014-02-11. `http://doi.org/10.3886/ICPSR04421.v2`.
3. *National municipal review*, years 1955 through 1958.
4. *National civic review*, years 1959 though 1966.
5. J. Mills Thornton III, *Dividing lines: Municipal Politics and the Struggle for Civil Rights in Montgomery, Birmingham, and Selma*, The University of Alabama Press, Tuscaloosa, 2002.
6. Online newspaper databases (Newspaper Archive, America's Newspapers, Factiva, ProQuest Historical Newspaper, Google Newspapers, Newspapers.com).
7. City charter, city council minutes, vote tallies at county board of elections, consultation with city and county officials.
8. Enacted State laws in states that require city charters to be approved by the legislature (e.g., Georgia where city charters can be found at the GALILEO Digital Initiative Database).
9. Section 5 Objection letters, US Department of Justice, Civil Rights Division, `www.justice.gov/crt/records/vot/obj_letters/`.

[16] In partisan elections, the national party affiliation of a candidate appears on the ballot.

nonpartisan. Thus, a large number of cities have changed their system of governance, although the most common change is increasing the fraction of council members elected by district.

Fiscal data for cities for the years 1957 through 2012 is taken from U.S. Census of State and Local Finances. The 2012 total population in this 2,727-city sample was 155 million. Summary statistics for the variables in the main sample are listed in Table 1. Overall, cities spend $1,856 per capita (in 2012 dollars), and infrastructure expenditures comprise 15% of overall municipal expenditures.[17]

The nature of infrastructure spending varies by city depending on whether a city owns an electric and gas utility, schools, or an airport. Nonetheless, half of the infrastructure expenditures are on roads, sewers, and water utilities.

Pension data for the years 1957–2015 is obtained from the U.S. Census Survey of Public Pensions (1993 through 2015) and the Historical Database on Public Employee-Retirement Systems 1957–2007. This data provides information for city-managed pensions. The pension sample is significantly smaller (844 cities with a 2012 population of 88 million), since it does not include cities in which workers are covered through state-managed pensions. The main dependent variable is the ratio of total pension benefits expenditures divided by the number of beneficiaries. Summary statistics of the pension sample are provided in Table 2. Overall, yearly pension benefits per beneficiary are $22,103 (in 2012 dollars).

We control for state laws that affect collective bargaining for police officers, fire fighters, and other municipal employees. These laws are coded using an ordinal 1–8 scale:

1. Collective bargaining prohibited.
2. No legal provision for collective bargaining.
3. Collective bargaining permitted, but not required.
4. Public employers are required to "meet and confer" with union leaders.
5. Public employers have a compulsory duty to bargain collectively, express or implied.

[17] In our sample there are 200 cities which operated a large hospital in some year, where 'large hospital' is defined as accounting for at least 20% of city spending. In order for our results not to be affected by changes in municipal hospital funding (e.g., repeal of Hill-Burton Act), we subtract out hospital expenditure from city spending for those cities (for all years, not just the years when the hospitals accounted for more than 20% of these 200 cities' expenses).

6. Collective bargaining is compulsory and mediation is required.
7. Collective bargaining is compulsory and strikes are protected.
8. Collective bargaining is compulsory and arbitration is required.

This data was originally collected by R.G. Valletta and R.B. Freeman, and then successively updated by Kim Rueben, Henry S. Farber, Geoffrey Lawrence, James Sherk, Kevin D. Dayaratna, and Cameron Belt, see Lawrence et al. [2016].

Residential segregation measures used in Cutler et al. [1999] were obtained at `http://www.nber.org/data/segregation.html`.[18] Specifically, the authors compute for each decade between 1960 and 1990 dissimilarity and isolation indices at the Metropolitan Statistical Area level.[19] Our measure of court ordered school desegregation are the school districts identified in Welch and Light [1987].[20]

We also control for demographics, the city population, the share of the population that is black, and median family income. These variables are obtained from the Census of Governments and Decennial Census (various years); intercensal and missing values are interpolated. The number of individuals who voted in Presidential elections in the county that includes most of the city population is obtained from the Congressional Quarterly, Voting and Elections Collection. Voter turnout is computed as the number of voters as a percent of individuals of voting age.

4 A graphical illustration of the findings

As we discussed earlier, the Twenty-fourth Amendment to the United States Constitution and the Voting Rights Act of 1965 removed barriers to minority voting. Further, courts began requiring district elections for city councils in 1975, and to a greater degree after the 1982 amendment to the Voting Rights Act. Finally, the likelihood of court intervention was greater in jurisdictions with a history of voter discrimination. In this section we provide graphical evidence of the effects of these changes on voting, district elections, and city expenditures.

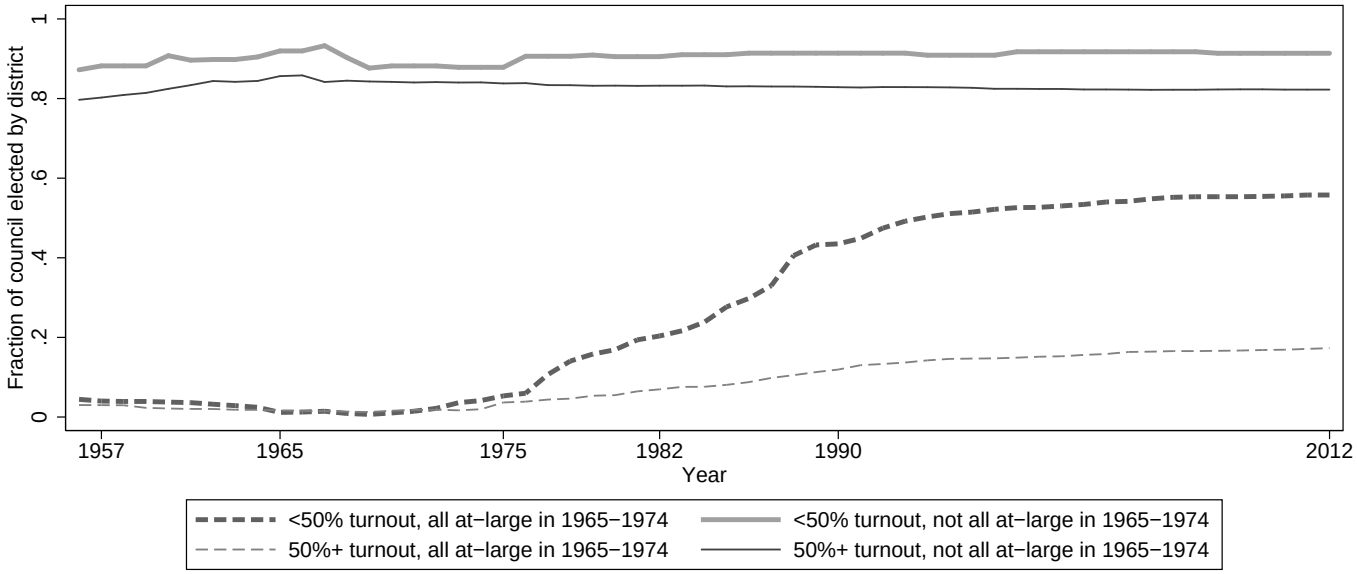
[18] Downloaded May 5, 2017.

[19] The dissimilarity index is high if blacks disproportionately reside in some areas of a city relative to whites, while the isolation measure if high if a large share of the black population would need to change areas for the races to be evenly distributed within a city.

[20] 231 cities in our sample are in school districts listed in Welch and Light [1987].

In this paper, we summarize the history of prior voter discrimination by the percent of individuals eligible to vote who cast ballots in the 1964 Presidential election. Specifically, for each city we compute the percent of the voting age population that voted in the county that contains most of a city's population. In this section, in order to easily graph the information, we split cities by whether the turnout in the 1964 election was less or more than 50%. The 50% threshold for turnout in the 1964 Presidential election was employed in Section 4 of the Voting Rights Act to determine which jurisdictions needed pre-clearance to change electoral rules. In our sample, 67% of Southern U.S. cities had a 1964 voter turnout of less than 50%, versus 2% of non-Southern cities.

Figure 1: Share of city council elected by district: cities with voter turnout in 1964 Presidential election $< 50\%$ vs. cities with 50%+ turnout

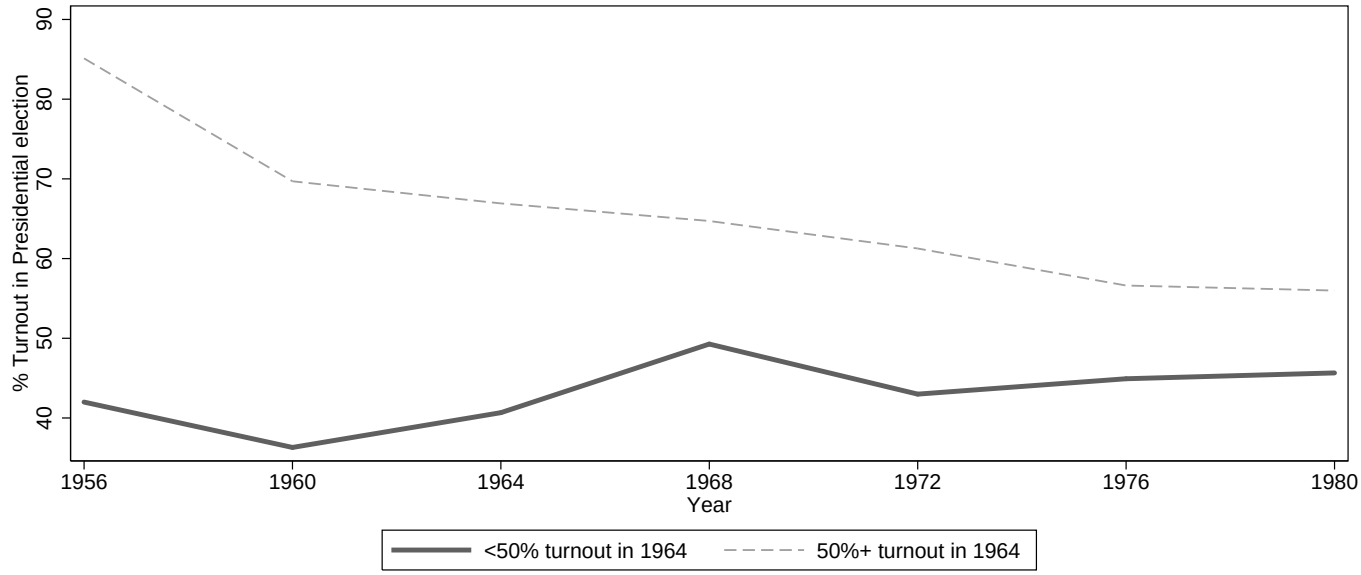


The thick dashed line represents the fraction of the council elected by district for cities with <50% turnout in the 1964 Presidential election that also had the entire council elected at-large in 1965–1974, while the thick solid line describes the fraction of the council elected at-large for cities with <50% turnout which did not have the entire council elected at-large in 1965-1974. The regular sized lines correspond to cities with 50%+ turnout.

In Figure 1 we graph the yearly-average fraction of the council that is elected by district, where the yearly-average is computed over four groups of cities. The thick dotted line represents the fraction by district for cities with a turnout of less than 50% that had the entire council elected at-

large in some year between 1965 and 1974. We can see that for this group of cities, the prevalence of district elections increased drastically from 1975 to 1994, and more slowly since. In contrast, for cities with a greater than 50% turnout that had the entire council elected at-large in some year between 1965 and 1974 (thin dotted line), district elections become somewhat more prevalent. For cities that did not elect the entire council at large between 1965 and 1974, the fraction elected by district does not change significantly over time.[21] Thus, it appears that the cities that were most affected by the 1982 amendment of the Voting Rights Act were cities that had both low voter turnout in 1964 and that elected the entire council at-large for at least one year between 1965 and 1974.

Figure 2: Percent of the voting age population that voted in a Presidential election: cities with <50% voter turnout in the 1964 Presidential election vs. cities with 50%+ turnout: sample of cities where the entire council was elected at-large in 1965–1974



The thick solid line describes Presidential voter turnout in cities with less that 50% turnout in 1964, where turnout is the percent of the county voting age population that voted. The regular dashed line is Presidential turnout in cities with more than 50% turnout in 1964. The sample consists of cities where the entire council was elected at-large in 1965–1974.

In the next three figures, we examine voter turnout and city spending for the subset of cities

[21] One small exception, is the decrease in the fraction of the council elected by district between 1967 and 1969 for cities with less than 50% turnout. Trebbi et al. [2008] explain this phenomenon as cities switching to at-large elections to counteract the increased minority voter turnout brought by the Voting Rights Act.

where the entire council was elected at-large any year between 1965 and 1974, and split the sample by the turnout in the 1964 Presidential election. As pointed out by Filer et al. [1991], minority voter turnout increased after the passage of the Civil Rights Acts of 1960 and the ratification of the Twenty-fourth Amendment to the United States Constitution (January 1964), although the greatest increase came with the Voting Rights Act of 1965, which eliminated poll taxes and literacy tests.[22] In Figure 2, we provide evidence consistent with this finding. Specifically, we graph average voter turnout in each Presidential election for each city group and year between 1956 and 1980. When examining 1964-low-turnout cities, the biggest change in voter turnout occurred between 1964 and 1968, although turnout also increased between 1960 and 1964.

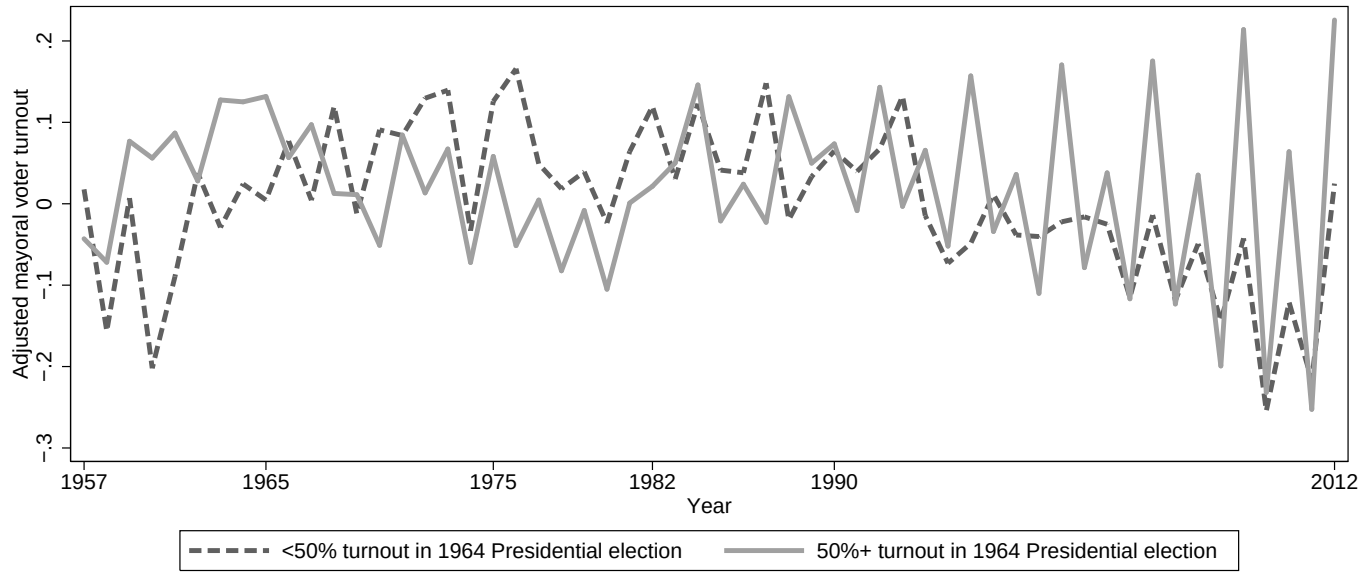
We may be concerned that changes in voter turnout were different at the municipal versus Presidential level. For instance, voting rights may have been protected differently at the local versus federal level, and minorities may have different predispositions to vote in the federal and local elections. For this reason we examine the total number of votes cast for the top two candidates for mayor in 12,767 elections in which at least two candidates ran for mayor, in cities where the entire council was elected at-large any year between 1965 and 1974.[23] For each year, our sample includes different cities; since cities hold their elections in different years, and we were not able to obtain voting data for all elections. For this reason, rather than graphing the actual voter turnout, we graph 'adjusted mayoral voter turnout,' the residual from regressing the log number of votes on the log of the population and city fixed effects. For instance, an adjusted mayor voter turnout of -0.1 indicates that voter turnout is 10% lower than turnout for that city in an average year. In Figure 3, we graph the average adjusted mayoral voter turnout by 1964 Presidential turnout. It appears that for low-Presidential-turnout cities, mayoral voter turnout increased from 1960 to 1976, remained relatively constant between 1976 and 1993, and decreased thereafter.

Our graphical evidence is thus consistent with the legal consequences of the Voting Rights Act: the expansion of suffrage between 1964 and 1968, and the increased use in district elections starting in 1975 and accelerating in 1982. In Figure 4 we examine city spending over the same period to see if it appears to have been affected by the Voting Rights Act. The solid line describes

[22] Cascio and Washington [2014] provided similar empirical evidence by comparing Southern states with literacy tests to Southern states without.

[23] Data was obtained from Ferreira and Gyourko [2009] and 6. and 7. in footnote 15.

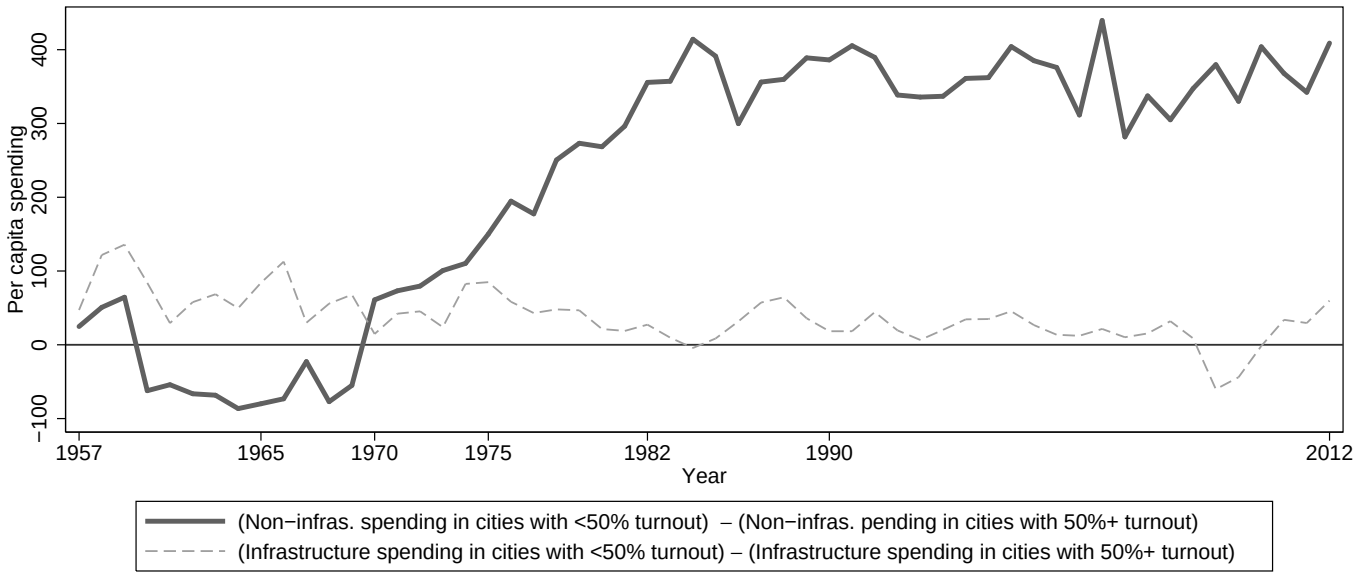
Figure 3: Adjusted mayoral voter turnout: cities with <50% voter turnout in the 1964 Presidential election vs. cities with 50%+ turnout: sample of cities where the entire council was elected at-large in 1965–1974



'Adjusted mayoral voter turnout' is the residual from regressing the log of number of votes cast in a mayoral election on the log of the city population, and city fixed effects. The sample includes all mayoral elections in which at least two candidates ran for office, in cities where the entire council was elected at-large any year between 1965 and 1974. The dashed line describes the average adjusted mayoral voter turnout in cities with less than 50% turnout in the 1964 Presidential election, while the solid line is the adjusted mayoral voter turnout for cities with more than 50% turnout in 1964.

the difference in real per capita non-infrastructure spending between cities that had less than 50% turnout in 1964 and cities with greater than 50% turnout. We can think of the first group of cities as the treatment group, and the second as the control group.

Figure 4: Difference in spending on non-infrastructure and infrastructure spending between cities with <50% voter turnout in the 1964 Presidential election and 50%+ turnout: sample of cities where the entire council was elected at-large in 1965–1974



The thick solid line describes the difference between real per capita non-infrastructure spending for cities with less that 50% turnout in the 1964 Presidential election, minus non-infrastructure spending for cites with greater than 50% turnout. The regular dashed line describes the difference between infrastructure spending for cities with less than 50% turnout, minus infrastructure spending for cities with more than 50% turnout. Many of the year-to-year jumps are due to sampling issues. Before 1970, a complete census was conducted only in 1957 and 1967. The number of cities surveyed every year was reduced in 1993, and thus after that date, a complete survey is only conduced in 1997, 2002, 2007, and 2012.

We see that non-infrastructure spending for cities with less than 50% turnout increased steadily from about 1970 to 1984. On average, over the years 1957 through 1975, cities with less than 50% turnout spent $8 more per capita than cities with more than 50% turnout (all in 2012 dollars), while over the years 1976 through 2012, these cities spent on average $345 more. We saw that voter turnout increased starting in 1960 and more intensively after 1965. Thus, it appears that most of the increases in non-infrastructure spending occurred many years after the increase in voter turnout. In contrast, changes in the fraction of the council elected by district occurred gradually

between 1975 and 1990, following the same pattern as changes in non-infrastructure spending. Many of the year to year jumps are due to sampling issues. Before 1970, a complete census was conducted only in 1957 and 1967. The number of cities surveyed every year was reduced in 1993, and after that date, a complete survey was only conducted in 1997, 2002, 2007, and 2012.

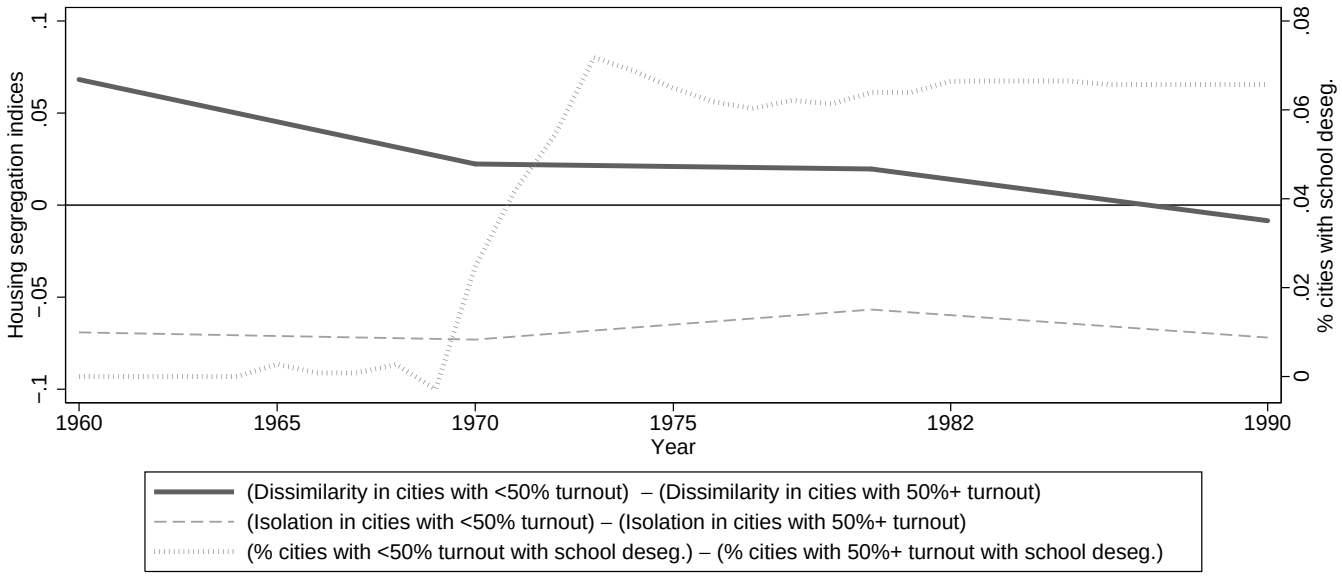
Cascio and Washington [2014] also examined the impact of the Voting Right Act on government spending. They found that between 1967 and 1972, state transfers increased in counties where voting discrimination had been prevalent before the Voting Right Act. Thus, Cascio and Washington provided evidence that governments respond quickly to changes in the electorate. However, Cascio and Washington did not find any further increases in transfers after these initial increases (i.e., transfers remained constant between 1972 and 1977, and declined between 1977 and 1982). Thus, these results suggest that it unlikely that the drastic increases in city spending between 1970 and 1990 that we document are due to the expansion of suffrage that occurred between 1960 and 1965. Given that the timing of spending increases seems to coincide with the adoption of district elections, the change in how councils are elected appears to have changed city spending.

The status of minorities in areas with less than 50% turnout, may have improved following *Brown v. Board of Education* 347 U.§. 483 (1954), the Civil Rights Act of 1964, the Fair Housing Act of 1968, and this in turn may have lead to changes in city spending. Hence, it is possible that the increase in city spending is due to the broad set of right acquired by minorities over our sample period, rather than merely suffrage and electoral representation. For this reason, we examine whether changes in city spending can be explained by changes in residential and school segregation. Figure 5 describes the difference between residential segregation for cities with less than 50% voter turnout versus residential segregation for cities with greater than 50% turnout. We can see that cities with less that between 1960 and 1990, cities with less than 50% turnout became less residentially segregated than cities with greater than 50% according to one housing segregation index ("dissimilarity") but not the other ("isolation"). Further, most changes in the dissimilarity index occur in 1960 through 1970, rather than 1970 to 1984, and thus dot match the timing of changes in non-infrastructure spending.

Figure 5 also graphs the difference between the fraction of cities with less than 50% voter

turnout under court ordered school desegregation versus the fraction of cities with greater than 50% turnout with school desegregation orders. We see that between 1970 and 1973 there is a stark increase school desegregation orders for cities less than 50% turnout. However, this pattern does not continue after 1973, and thus does not match the pattern of changes in election methods and non-infrastructure spending.

Figure 5: Difference in segregation measures between cities with <50% voter turnout in the 1964 Presidential election and 50%+ turnout: sample of cities where the entire council was elected at-large in 1965–1974

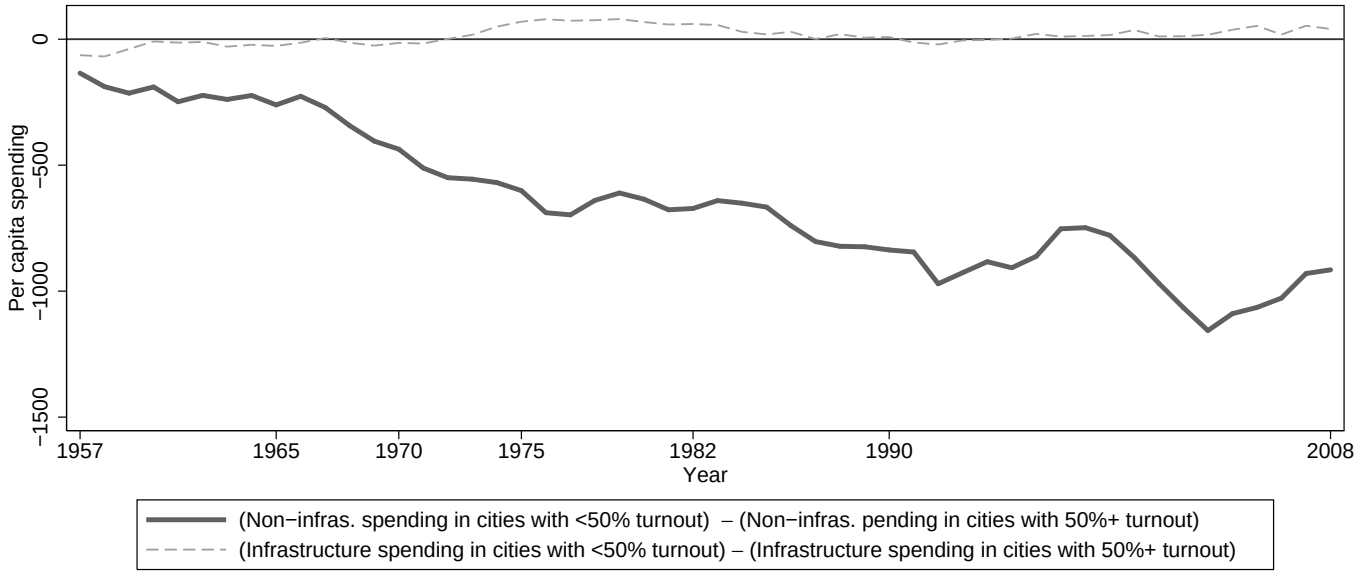


The thick solid line describes the difference in the dissimilarity index for cities with less that 50% turnout in the 1964 Presidential election, minus non-infrastructure spending for cites with greater than 50% turnout, while the regular dashed line describes the isolation index for cities with less than 50% turnout, minus the isolation index for cities with more than 50% turnout. The dissimilarity index and isolation index measure the separation of white and black within a particular metropolitan statistical area. The thick dotted line denote the difference in the fraction of cities with less than 50% turnout that are under school desegregation orders, minus the fraction of cities with greater than 50% turnout that are under school desegregation orders.

Residential and school desegregation do not capture all the social changes occurring during this time period. Ideally, we would have measures of black activism, white guilt, and so on. Nonetheless, if these unobserved factors affect city spending, we might expect them to also affect state spending. We compute per capita state government non-infrastructure spending and assign its value to all the cities in our sample from that state. The solid line Figure 6 describes the difference between

state non-infrastructure spending between cities that had less than 50% turnout in 1964 and cities with greater than 50% turnout. Unlike city spending, state spending is decreasing. Further, there is not inflection point in state spending in the early 1970s and no tapering of the increases in the mid 1980s. Thus, there does not appear to be an unobserved factor that leads to an increase in both city and state spending.

Figure 6: Difference in state spending on non-infrastructure and infrastructure spending between cities with <50% voter turnout in the 1964 Presidential election and 50%+ turnout: sample of cities where the entire council was elected at-large in 1965–1974

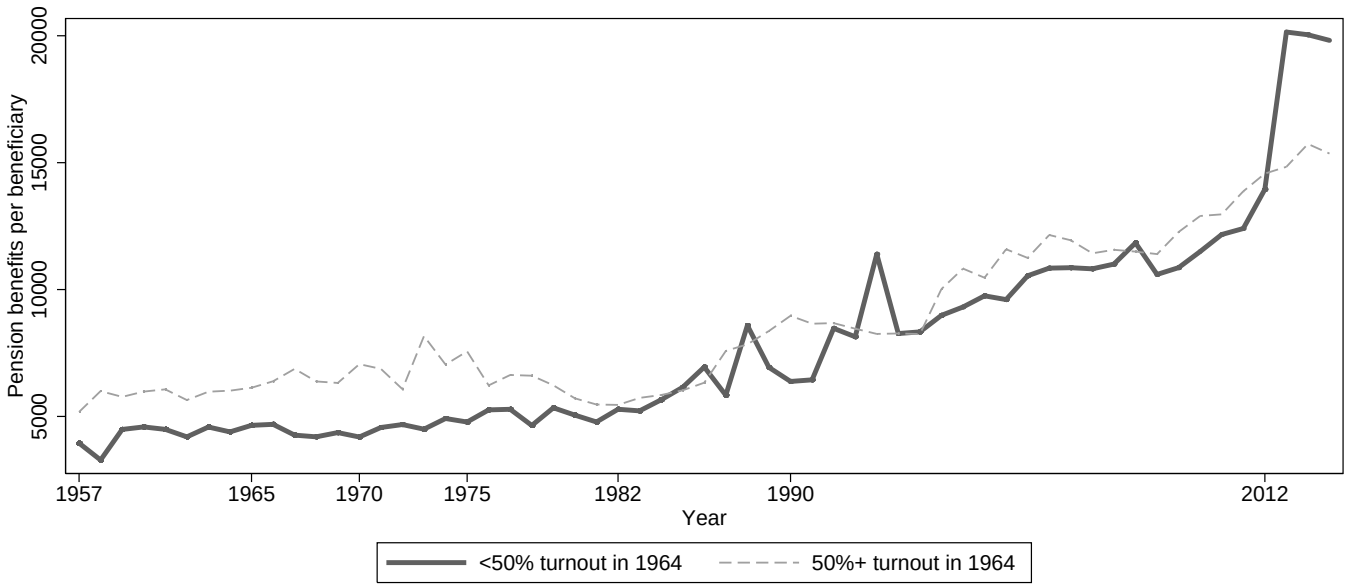


The thick solid line describes the difference between real per capita state non-infrastructure spending for cities with less that 50% turnout in the 1964 Presidential election, minus non-infrastructure spending for cites with greater than 50% turnout. The regular dashed line describes the difference between state infrastructure spending for cities with less than 50% turnout, minus infrastructure spending for cities with more than 50% turnout.

A portion of the increases in non-infrastructure spending appear to have come at the expense of infrastructure spending. The dashed line in Figure 4 is the difference in infrastructure spending between cities that had less than 50% turnout and cities with greater than 50% turnout. Infrastructure spending for cities with less than 50% turnout appears to decrease from 1975 on. Thus, it appears that district elections reduced the fraction of city spending allocated to infrastructure.

In Figure 7 we examine average pension benefits for municipally run pensions. We see that before 1982, pension benefits in cities with greater than 50% turnout are consistently higher than

Figure 7: Average pension benefits in cities with <50% voter turnout in the 1964 Presidential election and 50%+ turnout: sample of cities where the entire council was elected at-large in 1965–1974



The thick solid line describes real pension benefits per beneficiary in cities with less that 50% turnout in 1964 Presidential election. The regular dashed line describes real pension benefits per beneficiary for cities with more than 50% turnout.

pension benefits in cities with less than 50% turnout. After 1982, which group of cities has higher pension benefits varies from year to year. Thus, the shift to district elections appears to have increased pension benefits.

5 Empirical framework

There are alternative explanations for changes in city spending and use of district elections that do not involve the Voting Rights Act. Over our sample time period, differences in average state personal income shrunk [Barro and Sala-i-Martin, 1992]. Thus, increases in non-infrastructure spending in low-1964-voter-turnout cities may be due to growth in personal income in those cities. Similarly, the increased use of district elections in these cities may be due to population growth.

In order to control for these alternate explanations, we examine the impact of the Voting Rights Act on election rules and city spending by estimating the following regressions:

$$\begin{aligned}\frac{\text{District \#}_{it}}{\text{Council size}_{it}} &= \alpha_i' + \tau_t' + \gamma' \cdot (\Delta\%\text{Vote 1968})_i \times (\text{Year} > 1965) + \zeta \cdot \text{At-large}_{65-74,i} \times (\text{Year} \geq 1982) \\ &\quad + \theta \cdot (\%\text{Vote 1964})_i \times \text{At-large}_{65-74,i} \times (\text{Year} \geq 1982) + \gamma' X_{it} \qquad (1)\end{aligned}$$

$$\text{Spending}_{it} = \alpha_i + \tau_t + \beta \cdot \frac{\text{District \#}_{it}}{\text{Council size}_{it}} + \gamma \cdot (\Delta\%\text{Vote 1968})_i \times (\text{Year} > 1965) + \gamma X_{it} + \epsilon_{it}, \qquad (2)$$

where i denotes the city, t denotes the year, α_i, τ_t are city and year fixed-effects, $\frac{\text{District \#}_{it}}{\text{Council size}_{it}}$ is the fraction of seats on the council elected by district (rather than at-large), and Spending_{it} denotes the log of city non-infrastructure spending, the log of infrastructure spending, or the log of pension benefits per beneficiary. Further, the change in voter turnout between the 1964 and the 1968 Presidential elections, ("Δ%Vote 1968") proxies for the increase in voter turnout brought upon by the Voting Rights Act of 1965, while voter turnout in the 1964 Presidential election, ("%Vote 1964"), proxies for a history of voter discrimination. Finally, the indicator variable for whether the city had the entire council elected at-large in some year between 1965 and 1974 ("At-large_{65-74}") identifies the cities most affected by a requirement of district representation.

Regression (1) examines the impact of the Voting Rights Act on the fraction of the council

elected by district. Cities that we expect to be most affected are the ones that had the entire council elected at-large in some year between 1965 and 1974 ("At-large$_{65-75}$" equals one) and had a history of of voter discrimination ("%Vote 1964" small). Further, we expect the impact of the Voting Rights Act to be greater after it was amended in 1982 ("Year $\geq$ 1982" equals one). Regression (1) includes city and year fixed-effects and a vector of control variables (X_{it}): whether the executive is a mayor, commissioner or manager (excluded category), the log of the population, the percent of the population that is African-American, the log of median family income, the log of city council size, whether elections are partisan, and indicator variables for state laws that affect collective bargaining for police officers, fire fighters, and other municipal employees. Finally, the regression controls for the change in voter turnout between the 1964 and 1968 Presidential elections ("Δ%Vote 1968"). This is not a variable that we expect to affect district elections. We include this variable in order to use Regression (1) as the first-stage of a two-stage least square estimator that we discuss below.

Regression (2) examines the impact of the Voting Rights Act on city spending; namely, the impact of increased voter turnout and use of district elections. Since the regression includes city and year fixed effects, the estimates are identical to the difference-in-differences estimator. In the difference-in-differences estimator, the treated group are the cities that changed electoral rules, while the control group are the cities that did not change voting rules. Moreover, the estimator assumes that we observe spending for all cities before the treated group changed electoral rules and after. Then, the difference-in-differences estimator measures the impact of changes in electoral rules as the change in spending for the treated group minus the change in spending for the control group.

The difference-in-differences estimator examines all changes in electoral rules, voluntary and induced by the Voting Rights Act. To restrict the attention to law-induced changes, we instrument the fraction of the council elected by district. Specifically, rather than using the actual percent of the council elected by district, the instrumental variables estimates use the predicted percent of the council elected by district using the estimates from Regression (1).

The instrumental variables estimator measures the impact of law-induced changes in electoral

rules under the assumption that the impact of district elections on spending is the same for all cities. However, when the impact of district elections on spending differs among cities, the instrumental variable estimator can be interpreted as the impact of district elections on "complier cities," cities that switch to district elections if and only if they have a history of voter discrimination (see Section 7.2).

In almost all commission form governments, the commission is elected at-large. Thus, law-induced district elections led to a reduction in the number of cities that use a commission. For this reason, as a robustness test we instrument for whether the executive is a mayor, a commission or a manager, using as additional instruments an indicator variable for whether a city used a commission any year between 1965 and 1974, interacted with whether the year is greater equal to 1982 and voter turnout in the 1964 Presidential election.

Regression (2) includes the same vector of control variables as Regression (1). Nonetheless, it is possible that cities with district versus at-large elections differ in ways not accounted for by the control variables used so far. The use of city fixed effects helps mitigate this concern, as it removes unobserved effects that are constant over time. However, they do not account for differences in the distribution of the control variables. The propensity score match estimator examines the impact of districts elections on cities that have similar distributions of the control variables.

6 Results – full sample

We examine the impact the Voting Rights Act on voting laws and city spending. We saw in Figure 1 that, starting in 1975, there was an increase in the fraction of the council elected by district in areas that had lower voter turnout in 1964, and this trend intensified after 1982. In order to support the hypothesis that this change was due to the Voting Rights Act, we need to control for other variables that could have led to changes in voting laws. For instance, the increased use of district elections may have been due to an increase in city population. Thus, we regress the fraction of the council elected by district on variables measuring the enforcement of the 1982 amendment of the Voting Rights Act, as well as controls for other variables that might influence district elections, such as population size.

We find that cities that had the entire council elected at-large between 1965 and 1974 saw a large increase in the fraction of the council elected by district after 1982 (Table 3, Regression 1). Further, this effect is larger in cities that had a lower turnouts in the 1964 Presidential election. For instance, 45% of the voting age population in the South voted in 1964. Our estimates suggest that, Southern cities that had the entire council elected at-large between 1965 and 1974, saw the fraction of the council elected by district increase by $0.7 - 0.009 \times 45 = 0.3$. We also see that consistent with our discussion of the law, the increase in voter turnout ("Δ % Vote 1968") does not affect the fraction of the council elected by district. This allows us to estimate empirically the separate impact of changes in turnout and changes in election methods. We discussed on page 23 that the Voting Rights Act also influenced the allocation of executive powers. For this reason, we ran separate regressions for the percent of the council that is elected by district and indicator variables for Commission and Mayor-Council government (Table 3, Regression 2–4). We find that the Voting Rights Act shifted executive powers from commissions to mayors.

The second set of regressions examines the impact of voter turnout and district elections on the log of non-infrastructure spending. We find that non-infrastructure spending is increasing in the fraction of the council that is elected by district, when we estimate the effect by ordinary least squares (Table 4, Regression 1). Cities that employ district elections are more likely to have large city councils and partisan elections. Thus, we may be concerned that district elections proxy for council size or partisan elections. For this reason, we re-estimate the regression with controls for the log of council size and an indicator for partisan elections (Regression 2). The inclusion of these additional variables does not significantly affect our findings. Our estimates may also be affected by outlier observations for expenditures. However, the results still hold after winsorizing non-infrastructure spending at the 5th and 95th percentiles (the regression results are omitted from the tables).

We may be concerned that both the shift towards district elections and the increase in city spending are caused by the civil rights movement. Thus, the relations between district elections and city spending is not causal. However, we find that other manifestations of the civil rights movement do not effect election methods when we regress the fraction of the council elected by district on

residential segregation (index of dissimilarity and isolation) or school segregation. Consequently, we obtain qualitatively similar results when we estimate the impact of district elections on non-infrastructure spending with as additional controls residential and school segregation (the results are omitted from the tables).

The regressions so far examined all types of adoptions of district elections. However, we would expect law-induced district elections to have a greater effect on city spending than voluntary adoptions, since they are more likely to reallocate political power within the city. The impact of law-induced district election is estimated by instrumenting the fraction of the council elected by district by the 1964 Presidential turnout. We find that instrumental variable estimates to be seven times larger ($\approx 0.378/0.055$) than the ordinary least squares estimates.[24]

In Regression 4, we instrument for executive power and obtain similar results.[25]

The third set of regressions in Table 5 examines the impact of voter turnout and district elections on the log of infrastructure spending. Both ordinary least square and two-stage least squares estimates indicate that higher voter turnout leads to lower infrastructure spending, but district elections do not affect the level of infrastructure spending. It thus follows that the share of expenditures that go to infrastructure decreases when cities adopt district elections.[26]

Finally, the fourth set of regressions in Table 6 examines pension benefits per beneficiary. For this regression, we use two additional control variables: the number of members and beneficiaries in the municipal pension plan. Both ordinary least squares and two-stage least squares suggest that district elections and higher voter turnout lead to higher pension benefits.

In addition to district elections, three characteristics of city government are believed to affect city spending: whether the executive is the mayor ("mayor-council government"), the city-manager, or a commission; partisanship; and the size of the city council. Coate and Knight [2011] provide empirical and theoretical evidence that cities spend less under mayor-council government. Sim-

[24]Using a Hausman test we can show that the two-stage least square coefficients (3) are different than the ordinary least squares coefficients (1) at the 5% confidence level. The first stage F-statistic is equal to 48, thus alleviating concerns that the instruments may be weak.

[25]The first stage F-statistic is equal to 2, thus raising the concern that the instruments are weak. For this reason we rerun the regression using the LIML estimator and obtain similar results.

[26]This follows since district elections do not increase the level of infrastructure spending, but raise the level of non-infrastructure spending. Alternatively, one can obtain this result by estimating a regression with, as the dependent variable, infrastructure spending as a percent of total spending.

ilarly, we find that cities with mayor council government spend less on both infrastructure and non-infrastructure.[27]

We also find that city spending is unaffected by whether the party affiliation of a candidate appears on a ballot. However, it is conceivable that partisan elections have a different effect on cities where a vast majority of voters support one party, compared to cities where both Democrat and Republican parties are competitive. Bagchi [2016] examines city pensions in Pennsylvania, a state where elections are partisan. He finds that cities where the Democratic and Republican parties are competitive have pensions that are less funded, more generous, and that use higher interest rates to discount actuarial liabilities. Thus, in cities with partisan elections, party competition seems to negatively affect the viability of cities. We find similar results. Specifically, we re-estimated the regressions in this paper with a variable measuring party competition on the subsale of cities with partisan elections.[28] We find that greater party competition leads to higher non-infrastructure spending.

Finally, a large literature finds that spending is increasing in the size of the legislatures ("law of $1/n$", see Baqir [2002], Kessler [2014]). MacDonald [2008], finds that this relation only holds in a cross-section and disappears when she accounts for omitted variable bias.[29] Similarly, we do not find an effect of council size on spending.

Prior work has examined the effects of income inequality and ethnic fractionalization on city spending [Alesina et al., 1999, Boustan et al., 2013]. For this reason, we examine the robustness of our results to controlling for income inequality (proxied by the log of the ratio of mean to median family income) and ethnic fractionalization (proxied by one minus the sum of the square of the fraction of the population that is Black and the square of the fraction of the population that is Hispanic). For this regression, the sample starts in 1970, since for earlier years the Census

[27] Lower need for infrastructure may be the cause for city-manager government, rather than a consequence. Vlaicu and Whalley [2016] provide evidence that more flood prone areas are more likely to adopt city-manager government.

[28] Specifically, for each mayoral election, we compute the percent of vote received by the Democratic candidate. We then compute a rolling average of the Democratic vote share over a 9 year window (four years before and four years after). "Party competition" is defined as the negative absolute difference between the rolling average and 50%.

[29] Pettersson-Lidbom [2012] argues that the size of the legislature is potentially endogenous, since a large public sector may require a large number of legislators to participate in the budget process due to its increased complexity. Pettersson-Lidbom provides regression discontinuity evidence that government spending may be *decreasing* in the legislature size using Finnish and Swedish data.

does not provide city level mean family income and the share of the population that is Hispanic. Thus, we can examine the effect of district elections, but we cannot estimate the effect of the expansion of suffrage. Nonetheless, we find including these two additional control variables does not qualitatively change our findings.[30]

7 Results – Matching estimators sample

In this section we address two issues. First, the ordinary least squares estimates in the last section are inconsistent if the covariates impact spending nonlinearly and if the covariate distribution for cities that switched to district elections differs substantially from the distribution for cities that did not [Imbens, 2015]. We address this concern by constructing subsamples ("blocks") in which cities have similar covariates.

Second, in the last section we interpreted, without justification, the instrumental variables estimates as the impact of law-induced district elections. However, the impact of law-induced district elections is the difference between two terms that are difficult to measure. The first term is the growth in spending for cities that change to district elections because of election law, while the second term is the growth in spending for cities that keep at-large elections because they are not targeted by the law. These terms are difficult to measure since we do not know which cities switched to district elections against their will. In many cases, mere threat of expensive litigation led cities to switch to district elections. Imbens and Angrist [1994] have shown that, even though we cannot identify which cities changed electoral rules voluntarily, we can identify these two terms, and thus the impact of law-induced district elections.

To simplify our analysis, throughout this section we restrict ourselves to cities that elected the entire council at-large some year between 1965 and 1974. Further, the district variable is recoded as an indicator variable for whether a majority of the council is elected by district in 2002.

[30]For instance, when regressing non-infrastructure spending on the expanded set of controls, the coefficient for district elections is significant at the 1% confidence level.

7.1 Propensity score estimates

Figure 8 illustrates how the covariate distribution for cities that switched to district elections ("treated cities") differs substantially from the ones that did not ("control cities"). To illustrate the difference in the covariate distribution, we have drawn the probability density for percent voter turnout in 1964 for treated and control cities ("Entire Sample" quadrant). Not surprisingly, treated cities have significantly lower voter turnout than control cities.

In order to generate samples with similar covariates ("blocks"), we compute the probability that each city switches to district elections by regressing adoption of district elections on quadratics of 1964 voter turnout, the population share that is black, growth in median family income, population size, and population growth.[31] The predicted probabilities from this probit are the propensity scores.

The cities in our sample are then split into fix blocks, where each block is determined by an interval of propensity scores, and where t-tests in each block confirm that the distribution of covariates for the treated cities is the same as the control cities.[32] For concreteness, in Figure 8 we have graphed the probability densities for block 1 (the lowest interval of propensities), block 7 (the highest propensities), and block 3 (middle propensities). It can be seen that within each block the distribution of 1964 voter turnout for treated cities is similar to the one for control cities.

For each block, we compute the average growth rate in per capita non-infrastructure spending for treated cities minus the average growth rate for control cities. The matching estimator is then the weighted average of these block differences in average growth rates, where the weight is the fraction of observations in each block that are treated (thus we estimate the average effect of treatment on the treated). According to the propensity score estimator, per capita non-infrastructure spending in cities that elect a majority of the council by district increased by 5.9% more than spending in other cities.[33]

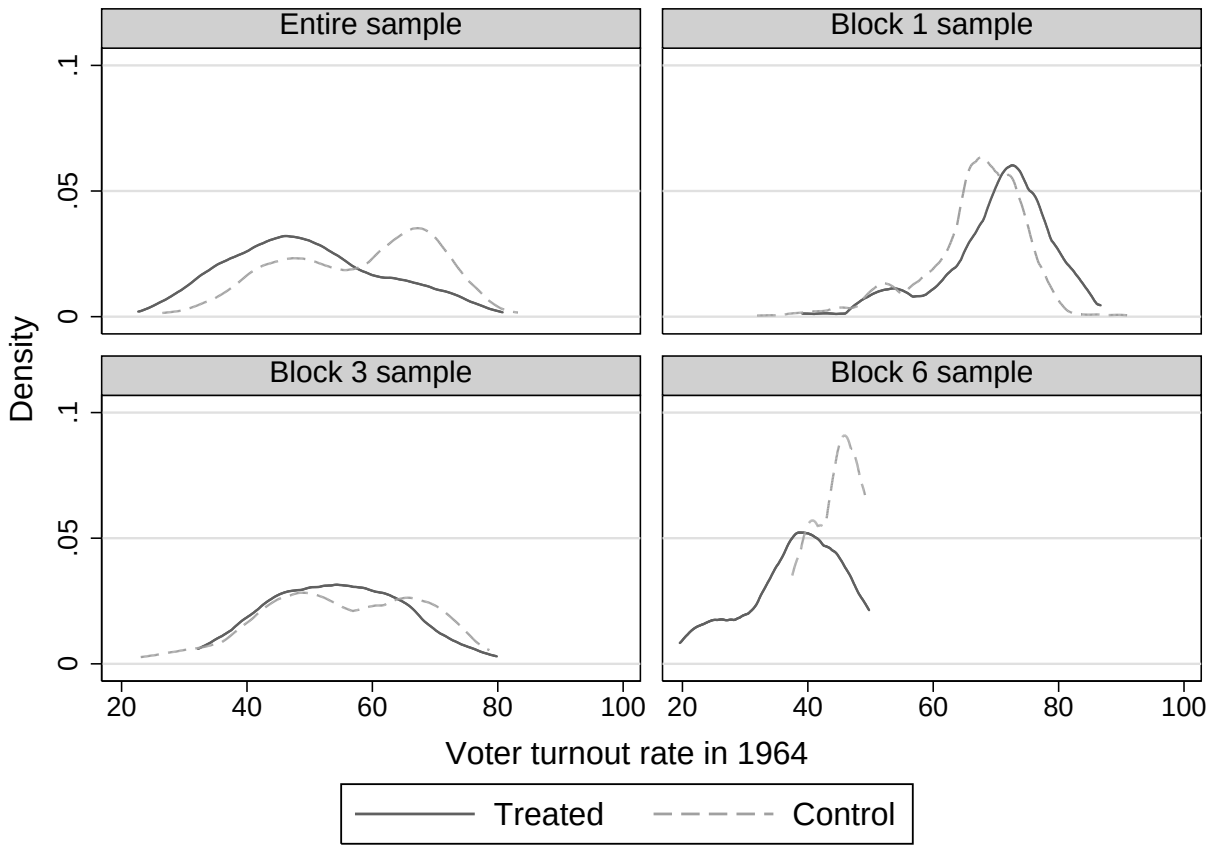
Alternatively, on each block, we regress the growth rate in non-infrastructure spending on in-

[31] The percent of the city population that is Hispanic is not included in the regression since it does not affect the probability of treatment.

[32] Specifically, first the sample is split into five equally spaced intervals of the propensity score. Then, each interval is split in half if the average propensity of treated and control units differ [Becker and Ichino, 2002].

[33] The estimate is omitted from the tables. The coefficient is significant at the 5% confidence level. The sample consists of 457 cities that switched to majority district election and 1,176 that did not.

Figure 8: Probability density for 1964 voter turnout for "treated" cities and "control" cities in matched samples



The "entire sample" consists of cities that elected the entire council at-large some year between 1965 and 1974. "Treated" cities had a majority of the council elected by district in 2002, while "control" cities did not. We then partition the entire sample into blocks, where each "block sample" contains cities for which the value of the propensity score falls into a particular interval. The propensity score is the probability that a city is treated as a function of quadratics of the voter turnout rate in 1964, the population share that is black, population size, and population growth. For each sample, we have graphed the probability density for the percent voters turnout in 1964 for treated and control cities.

come and population growth, and the change in the share of the population that is black and Hispanic. The matching estimator is then the weighted average of these blocks regression coefficients, where the weight is the fraction of observations in each block that are treated [Imbens and Rubin, 2015, 386-390]. According to this propensity score estimator, district elections increase non-infrastructure spending by 6.4%. When we split non-infrastructure spending into categories, we find that district elections lead to a 5.7% increase in police spending, an 6.7% increase in other categories spending, and to statistically insignificant changes in spending on fire protection, roads, and health and welfare, see Table 7.[34]

In order to interpret these estimates as causal, one needs the concept of "potential outcomes." In our context, each city i has two potential outcomes: $Y_i(0)$ is the 1977 to 2002 growth rate in non-infrastructure spending if the city does not switch to district elections, and $Y_i(1)$ is the growth rate if it does. Then, our estimate are causal if, conditional on the controls, potential outcomes are independent from election rules ("unconfoundedness"). Imbens [2015] suggests providing evidence of unconfoundedness by showing that treatment does not affect the lagged values of the outcome (i.e., ruling out an Ashenfelter dip). We find that treated cities have the same 1967–1972 growth in non-infrastructure as control cities, thus providing support for unconfoundedness.

7.2 Assignment vs. receipt of treatment

In this section, we measure the impact of law-induced district elections as the difference between two terms. The first term is the growth in spending for cities that are induced by the law to change to district elections. The second term is the growth in spending for cities that keep at-large elections because they are not targeted by the law. Thus, we need to remove from the first term the growth in spending for cities that adopted district elections even thought they were not targeted by the law. These cities are referred to as "alwaystakers." Similarly, we need to remove from the second term the growth in spending for cities that keep at-large elections even when they are

[34]These are the same expenditure categories used in Boustan et al. [2013], except we have combined health and welfare. In their study, income inequality increases police, fire, and highways, and has no statistically significant effect on highways, public welfare, or health and hospitals. Public welfare expenditure are mostly the cost of administration of medical and cash assistance, children services, social services for the physically disabled, and payments made to private vendors for medical assistance.

targeted by the law. These cities are referred to as "nevertakers."

There are a variety of reasons for cities to be alwaystakers. Often cities switch from district to at-large election or vice versa when they have had episodes of endemic corruption and seek a drastic change in how things are done. For instance, San Jose, California, switched to district elections in 1978 following the conviction of four councilors [Troustine, 2008]. Thus, alwaystakers could be cities that have experienced endemic corruption. Trebbi et al. [2008] argues that cities may voluntarily switch to district elections (i.e., are alwaystakers) when whites are the majority, but the fraction of the population that is black is substantial. They argue that in these cities, whites are concerned that in later years they will become the minority, and thus ensure their future representation by changing to district elections. Finally, alwaystakers could be cities where partisanship has come to the forefront, and where district elections allow both parties to be represented on the city council.

There are also several reason for cities to be nevertakers. For instance, Voting Right Act cases are tried in the Federal Judicial District that covers a city. The political party of the President who appointed the federal judges in each of the 94 federal judicial districts varies, since judgeships are lifetime appointments. Further, it has been shown that Democratic appointed judges find a violation of the law in 46% of cases brought under Section 2 of the Voting Rights Act, vs. 28% for Republican appointed judges [Cox and Miles, 2008]. Thus "nevertakers" could be cities in a Federal Judicial District where judges are unlikely to find violations to the Voting Rights Act.

"Nevertakers" could also be cities where minorities are dispersed and thus where district elections would not ensure minority representation. Note that geographic concentration of minorities need not be correlated with a history of voter discrimination. To see this, consider the two indices of residential segregation in Cutler et al. [1999]: dissimilarity and isolation. In 1970, the average housing dissimilarity index for cites in our sample with less that 50% turnout was 0.73 versus 0.81 for cities with greater than 50% turnout, while the average housing isolation index for cities with less than 50% turnout was 0.56 versus 0.55 for cities with greater than 50% turnout.

In order to estimate the growth rate in spending for alwaystakers and nevertakers, we need to distinguish "receipt" from "assignment" of treatment, and define the "potential treatment response." For exposition sake, we first illustrate these concepts with a more straightforward exam-

ple: the effect of the draft lottery on Vietnam-era military service [Angrist, 1990]. In this example, "assignment of treatment" means receiving a low lottery ticket, while "receipt of treatment" means Vietnam-era military service. The "potential treatment response" refers to how service is affected by the lottery number. Formally, the indicator variable Z_i is equal to 1 if individual i receives a low lottery number, while the indicator variable W_i is equal to 1 if individual i serves in Vietnam. The potential treatment response is a function '$W_i(Z_i)$' which denotes how military service depends on Z_i. 'Compliers' are individuals who serve in Vietnam if and only if they receive a low number (specifically, individuals i with $W_i(0) = 0$ and $W_i(1) = 1$).

In the voting law context, "Receipt of treatment" occurs in cities that changed electoral rules so that a majority of the city's council is elected by district in 2002. "Assignment of treatment" refers to a city facing legal pressure to adopt district representation. As discussed in O'Rourke [1982], until 1982, there was considerable disagreement among courts on the legal basis for challenges to local at-large elections. Thus, the "potential response function" refers to how a city's electoral rules are effected by the law. For instance, a "complier city" is going to choose to enact district elections by 2002 if the law puts pressure on the city do so, and keep at large elections otherwise.

Thus, this situation is similar to the one facing a potential conscript. As discussed in Katz et al. [2006], with the 1982 amendment to the Voting Rights Act, cities under the greatest legal scrutiny were cities with a history of voter discrimination.[35] For cities that had a history of voter discrimination, this outcome is analogous to a potential conscript drawing a low number. In contrast, those cities would have drawn a high number if Congress had not overruled *Mobile v. Bolden*, which stated that at-large election were illegal only in cases when it could be shown that they were enacted to limit minority representation.

The analogy is far from perfect, though, since we might be concerned that a history of voter discrimination has an independent effect on city spending, while we expect lottery numbers to only affect military service. Thus, after discussing our results, we will provide some justification for the assumption that a history of voter discrimination only influences spending through its effect on electoral rules.

[35]Note that the history of voter discrimination is not the only factor affecting the likelihood that a city faces legal liability. The model accommodates other factors by making cities of different types, as discussed below.

Formally, the indicator variable Z_i is equal to 1 if city i is in a county with less than 50% turnout. The function '$W_i(Z_i)$' denotes how the election method employed by city i depends on Z_i. Specifically, $W_i(0) = 1$ if city i would switch to district elections if it were in a county with more than 50% turnout, while $W_i(1) = 1$ if city i would switch to district elections if it were in a county with less than 50% turnout. We assume that cities are of three types. "Complier cities" are cities that switch to district elections only if the percent turnout is less than 50% ($W_i(0) = 0$ and $W_i(1) = 1$). "Nevertakers" never change to district elections ($W_i(0) = W_i(1) = 0$). Finally, "alwaystaker" always switch to district elections ($W_i(0) = W_i(1) = 1$). Thus, we assume that there are no "defier cities," cities that only switch to district election if they don't have to ($W_i(0) = 1$ and $W_i(1) = 0$).

Given our classification, the only cities that are affected by the Voting Rights Act are complier cities. Consequently, it is important to find a set of assumptions that allows us to compute the impact of the Voting Rights Act on complier cities' spending and to determine the fraction of cities that are compliers. The first assumption is that the history of voter discrimination does not affect the type of city; i.e., the fraction of complier cities is the same among cities with a history of voter discrimination and among cities without. The second assumption is that the history of voter discrimination does not affect potential outcomes. Namely, let $Y_i(0)$ and $Y_i(1)$ be the growth rate in city i's non-infrastructure spending if city i keeps at-large elections and switches to district elections. Then, we assume that the history of voter discrimination does not affect the values of $Y_i(0)$ for nevertakers, $Y_i(1)$ for alwaystakers, and the values $Y_i(0)$ and $Y_i(1)$ for compliers. This assumption is akin to the exclusion restriction for Z_i to be a valid instrument: 1964 voter turnout can only affect spending by changing elections procedures, which in turn affect spending.

In the Appendix, we show that these assumptions imply that 42% of cities are nevertakers, 19% are alwaystakers, and 39% are compliers. Further, the 1977-2002 growth in noninfrastructure spending is 47% for nevertakers, 47% alwaystakers, 38% for compliers without a history of voter discrimination, and 54% for compliers with a history of voter discrimination. Thus, the impact of the Voting Rights Act is to raise spending in cities with a history of voting discrimination by 16% $= 54\% - 38\%$. This increase in spending for the compliers can also be computed by regressing

1977-2002 growth in non-infrastructure spending on whether a majority of the council is elected by district in 2002, where district elections are instrumented by whether voter turnout in 1964 is greater than 50%.

Note that the magnitude of the effect on the compliers is smaller than the instrumental variable estimate we obtained in the previous section because we have not included any control variables, and thus we do not take into account the fact that median family income grew 2% percentage points less in cities with a history of voter discrimination. One way to improve covariate balance is to restrict the sample to cities where 1964 voter turnout was between 30 and 70%.[36] On this sample, there is no statistically significant difference in median family growth between cities with turnout greater or smaller than 50%. Further, on this sample, we find district elections increase spending for the compliers by 23%.

These results hinge on the assumption that a history of voter discrimination does not affect the potential outcomes $\{Y_i(0), Y_i(1)\}$, i.e., that assignment to treatment is unconfounded. This assumption would not hold if the extension of suffrage brought about by the Voting Rights Act had led to an increase in non-infrastructure spending between 1977 and 2002. However, we see in Table 4 that the increased voter turnout after passage of the Voting Rights Act did not lead to significantly higher non-infrastructure spending (when we control for whether the council is elected at-large).[37] Hence, there is some empirical support for the assumption of unconfoundedeness of assignment to treatment.

Cascio and Washington [2014] provide further evidence for unconfoundedeness by examining changes in state transfers to counties where voting discrimination had been prevalent before the Voting Rights Act. They found that all changes in state spending due to the expansion of the voting franchise occurred between 1967 and 1972. Thus, it is unlikely that the 1977 to 2002 changes in city spending can be explained by the delayed impact of the expansion of the voting franchise.

[36] Ideally, we would restrict our cities to an even smaller interval, but the sample size decreases drastically as we make the interval smaller.

[37] Specifically, the coefficient for "(Δ % Vote 1968) $\times$ (Year>1965)."

8 Conclusion

Since 1890, policy makers have disagreed on whether city councils should be elected at-large or by district. Municipal reformers (1890–1960) argued that at-large elections improve the quality of municipal government, since council members elected at-large are interested in making decisions benefiting the entire city rather than just one geographic segment; while the new reformers (1980s) favored district elections, because they lead to city councils that are more representative of the entire city.

We find that court-imposed district elections decrease the fraction of spending allocated to infrastructure and increase pension benefits per beneficiary. These changes are of importance, since pension expenses and an aging infrastructure are seen as some of the major challenges faced by cities. Thus our empirical evidence supports municipal reformers' claims.

In order to examine whether district elections lead to decisions that give greater weight to the interests of minorities and the poor, we need to determine which city policies are preferred by these groups. We expect minorities and the poor to be more likely to rent than own their homes, and hence to care less about the long run fiscal viability of their city. In particular, these voters are less interested in infrastructure spending, which provides benefits into the future. Further, by raising municipal pension benefits rather than salaries, voters can shift part of the employee compensation into the future. Our theoretical predictions match our empirical evidence. Specifically, we examine the impact to the increase in voter turnout that accompanied the elimination of literacy tests and poll taxes. We find that this led of a decrease in infrastructure spending and an increase in pension benefits. Thus, our result that district elections decrease the fraction of city spending allocated to infrastructure and increase pension benefits, can also be interpreted as district elections giving greater representation to minorities and the poor.

Table 1: Summary statistics: Main Sample

Variable	Mean	Std. dev.	Min	Max
Per capita city spending	1856	1450	14	48851
Infrastructure as a percent of municipal expenditures	15	12	0	90
Percent of infrastructure expenditures devoted to				
Roads	24	25	0	103
Sewer	14	21	0	100
Water utility	13	20	0	102
Police and fire	10	15	0	120
Parks	8	14	0	100
Housing	6	16	0	100
Electric utility	6	17	0	100
Education	4	16	0	104
Percent council elected by district	34	43	0	100
All at-large council 1965–1974 (=100)	48	50	0	100
Mayor-council government (=100)	45	50	0	100
Commission government (=100)	3	18	0	100
Council size	7	3	2	51
Partisan election (=100)	26	44	0	100
City population (in 1,000)	66	250	0	8275
Percent black	11	15	0	99
Median family income (in $ 1,000)	66	22	19	277
% Vote in 1964	62	12	18	91
% Vote in 1968 − % Vote in 1964	-0	6	-18	39
Police unions	5	3	1	8
Firefighter unions	5	2	1	8
Municipal unions	5	2	1	8

The unit of observation is the city-year. Data is for the years 1957 through 2012. In district elections, individuals vote only for their district's council member, while in at-large elections, all voters in a city choose all council members. Our main explanatory variable is the fraction of the council which is elected by district (vs. at-large), which in this table is expressed in percentages. Per capita spending is in 2012 dollars. Note that infrastructure spending on particular categories can be negative, thus leading the percent of infrastructure expenditures devoted to a particular category to exceed 100%. The variables police, fire fighter, and municipal unions, refer to state laws that affect collective bargaining, which are coded using an ordinal 1–8 scale.

Table 2: Summary statistics: City Pension Sample

Variable	Mean	Std. dev.	Min	Max
Pension benefits per beneficiary	22,103	28,889	341	1,487,395
Members of pension receiving benefits	1,322	8,385	1	245,519
Members of pension who have yet to receive benefits	2,585	14,529	1	333,889
Percent council elected by district	43	43	0	100
All at-large council 1965–1974 (=100)	59	49	0	100
Mayor-council government (=100)	47	50	0	100
Commission government (=100)	4	20	0	100
Council size	8	6	2	51
Partisan election (=100)	27	45	0	100
City population	176,500	542,389	1,271	8,446,241
Percent black	15	17	0	98
Median family income (in $1,000)	58	41	9	452
% Vote in 1964	63	12	20	86
% Vote in 1968 − % Vote in 1964	-1	4	-10	21
Police unions	5	3	1	8
Firefigher unions	6	2	1	8
Municipal unions	5	2	1	8

The unit of observation is the city-year. Data is for the years 1957 through 2015. Pension benefits are in 2012 dollars. The variables police, fire fighter, and municipal unions, refer to state laws that affect collective bargaining, which are coded using an ordinal 1–8 scale.

Table 3: Effects of Voting Rights Act on percent of fraction of council elected by district, commission government, and mayor-council government

	(1) District #/ Council size	(2) District #/ Council size	(3) Mayor-Council	(4) Commission
(At-large in 1965-74)×(Year≥1982)	0.699***	0.690***	-0.036	-0.009
	(0.111)	(0.116)	(0.028)	(0.012)
(%Vote 64)×(At-large in 1965-74)×(Year≥1982)	-0.009***	-0.009***	0.001*	0.000
	(0.002)	(0.002)	(0.000)	(0.000)
(Commission in 1965-74)×(Year≥1982)		0.374***	0.751***	-0.895***
		(0.125)	(0.159)	(0.150)
(%Vote 64)×(Commiss. in 65-74)×(Year≥1982)		-0.004	-0.006**	0.006**
		(0.002)	(0.003)	(0.003)
(Δ % Vote 1968) × (Year>1965)	0.002	0.002	0.000	0.000
	(0.003)	(0.003)	(0.001)	(0.001)
Mayor-council	0.139***			
	(0.025)			
Commission	-0.235***			
	(0.051)			
Population	-0.008	-0.010	-0.019	-0.002
	(0.020)	(0.021)	(0.014)	(0.003)
% Black	0.002**	0.003**	0.001	-0.001*
	(0.001)	(0.001)	(0.001)	(0.000)
Median income	-0.032	-0.031	0.009	-0.011
	(0.037)	(0.039)	(0.025)	(0.007)
R-squared	0.84	0.83	0.92	0.80
N. of observations	115,409	115,409	119,844	119,912

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

NOTES – The unit of observation is the city-year and the dependent variables are the fraction of the council elected by district (versus at-large) and indicator variables for whether a city employs mayor-council or commission government (council-manager government is the excluded category). The regressions include indictors variable for state laws that affect collective bargaining, city and year fixed effects. Robust standard errors are clustered at the state level. All variables are in logs, except for indicator variables and variables are in percentages. * significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

Table 4: Effects of district elections and voter turnout on city non-infrastructure spending

	(1)	(2)	(3)	(4)
District # /Council size	0.055***	0.049***	0.378***	0.393***
	(0.018)	(0.017)	(0.125)	(0.133)
(Δ % Vote 1968) $\times$ (Year>1965)	0.005	0.005	0.002	0.001
	(0.004)	(0.004)	(0.004)	(0.004)
Mayor-council	-0.069***	-0.067***	-0.113***	0.461
	(0.017)	(0.017)	(0.028)	(1.037)
Commission	-0.035	-0.016	0.070*	0.572
	(0.023)	(0.025)	(0.041)	(0.828)
Population	-0.159***	-0.161***	-0.179***	-0.166***
	(0.022)	(0.022)	(0.018)	(0.034)
% Black	0.004***	0.004***	0.003***	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
Median income	0.377***	0.372***	0.394***	0.390***
	(0.073)	(0.074)	(0.071)	(0.081)
Council size		0.024		
		(0.024)		
Partisan		-0.023		
		(0.032)		
R-squared	0.91	0.91	0.90	0.89
N. of observations	88,583	87,198	80,915	80,915
IV for at-large			Yes	Yes
IV for mayor-council				Yes
IV for commission				Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

NOTES – The unit of observation is the city-year and the dependent variable is the log of city non-infrastructure spending. "District # / Council size" denotes the fraction of the council elected by district (versus at-large). The instruments for (3) and (4) are defined on page 22. The regressions include city and year fixed effects. Robust standard errors are clustered at the state level. All variables are in logs, except for the variables are in percentages. * significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

Table 5: Effects of district elections and voter turnout on infrastructure spending

	(1)	(2)	(3)	(4)
District # /Council size	-0.010	-0.023	0.098	0.160
	(0.041)	(0.041)	(0.138)	(0.170)
(Δ % Vote 1968) $\times$ (Year>1965)	-0.013***	-0.014***	-0.014***	-0.017**
	(0.005)	(0.005)	(0.005)	(0.008)
Mayor-council	-0.117**	-0.132**	-0.149**	3.240
	(0.056)	(0.057)	(0.068)	(2.323)
Commission	-0.029	-0.005	-0.001	2.817
	(0.073)	(0.076)	(0.063)	(1.846)
Population	-0.150***	-0.149***	-0.172***	-0.089
	(0.047)	(0.048)	(0.051)	(0.085)
% Black	-0.000	-0.001	-0.002	-0.002
	(0.003)	(0.003)	(0.003)	(0.004)
Median income	0.952***	0.932***	0.948***	0.920***
	(0.092)	(0.093)	(0.089)	(0.151)
Council size		0.047		
		(0.061)		
Partisan		-0.100*		
		(0.057)		
R-squared	0.40	0.40	0.40	0.26
N. of observations	87,217	85,864	79,762	79,762
IV for at-large			Yes	Yes
IV for mayor-council				Yes
IV for commission				Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

NOTES – The unit of observation is the city-year and the dependent variable is the log of infrastructure spending. The instruments for (3) and (4) are defined on page 22. The regressions include city and year fixed effects. Robust standard errors are clustered at the state level. All variables are in logs, except for the variables are in percentages. * significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

Table 6: Effects district elections and voter turnout on municipal employees' pension benefits per beneficiary

	(1)	(2)	(3)	(4)
District # / Council size	0.089*	0.096*	0.390	0.417*
	(0.050)	(0.051)	(0.248)	(0.243)
(Δ % Vote 1968) $\times$ (Year>1965)	0.010*	0.010*	0.011**	0.011**
	(0.005)	(0.005)	(0.005)	(0.005)
Beneficiaries	-0.129***	-0.130***	-0.131***	-0.131***
	(0.025)	(0.026)	(0.026)	(0.026)
Members	0.069***	0.069***	0.070***	0.070***
	(0.016)	(0.016)	(0.017)	(0.017)
Population	0.351***	0.359***	0.288***	0.282***
	(0.091)	(0.093)	(0.078)	(0.078)
% Black	0.251	0.260	0.242	0.241
	(0.234)	(0.234)	(0.232)	(0.232)
Median income	-0.174	-0.176	-0.120	-0.116
	(0.216)	(0.219)	(0.192)	(0.193)
Mayor-council	0.008	0.013	-0.022	-0.024
	(0.042)	(0.045)	(0.060)	(0.061)
Commission	0.117*	0.081	0.241**	0.252**
	(0.062)	(0.057)	(0.101)	(0.103)
Council size		-0.058		
		(0.054)		
Partisan		0.005		
		(0.052)		
R-squared	0.68	0.68	0.68	0.68
N. of observations	17,216	16,944	16,431	16,431
IV for at-large			Yes	Yes
IV for mayor-council				Yes
IV for commission				Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes – The unit of observation is the city-year and the dependent variable are pension benefits expenditures per beneficiary. The variables police, fire fighter, and municipal unions, refer to state laws that affect collective bargaining and are coded using an ordinal 1–8 scale. The instruments for (3) and

(4) are defined on page 22. The regressions include city and year fixed effects. Robust standard errors are clustered at the state level. All variables are in logs, except for the variables are in percentages and union variables. * significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

Table 7: Propensity score estimates, relation between district elections and non infrastructure expenditures per capita

Total	Police	Fire	Roads	Health & Welfare	Other
0.064***	0.057**	0.028	0.057	0.139	0.067**
(0.024)	(0.024)	(0.044)	(0.052)	(0.147)	(0.030)

The sample consists of 1,612 cities that had the entire council elected at-large in some year between 1965 and 1974. We regress the probability that each city switches to district elections on quadratics of 1964 voter turnout, the population share that is black, growth in median family income, population size, and population growth. The predicted probabilities from this probit are the propensity scores. The cities in our sample are then split into seven blocks, where each block is determined by an interval of propensity scores, and where t-tests in each block confirm that the distribution of covariates for the treated cities is the same as the control cities. On each block, we regress the 1977-2002 growth rate in non-infrastructure spending on 1977-2002 income and population growth. By "non-infrastructure spending," we mean total non-infrastructure spending, or non-infrastructure spending on police, fire protection, highways, health and welfare, or other categories. The matching estimator is the weighted average of these blocks regression coefficients, where the weights are the fraction of observations in each block that are treated. * significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

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Appendix Computation of growth rates in non-infrastructure spending by type of city

Table 8 provides the number of cities (N_{ZW}) and the average adjusted growth rate in non-infrastructure spending (Y_{ZW}) by assignment (Z) and receipt (W) of treatment. "Assignment of treatment" is an indicator variable for whether voter turnout in 1964 was less than 50%, while "receipt of treatment" is an indicator for whether a majority of the council is elected by district in 2002. In cases where Z and W are equal, one cannot distinguish whether a city is a complier, or nevertaker/alwaystaker. Yet one needs to isolated the effect of compliance on spending in order to characterize the effect of the Voting Rights Act. Fortunately, one can compute the growth rate in spending for alwaystakers and nevertakers by looking at cases where Z is different than W. This allows us to factor out the effect of alwaystakers and nevertakers from the overall growth rate when Z and W are equal.

Table 8: Summary statistics by assigned and received treatment

Z (% Vote < 50)	W (District)	Type	Y_{ZW} (Adj. spend growth)	N_{ZW} (cities)
0	0	Complier or nevertaker	0.43	1,010
0	1	Alwaystaker	0.47	234
1	0	Nevertaker	0.47	165
1	1	Complier or alwaystaker	0.51	223

The sample consists of cities where the entire council was elected at-large some year between 1965 and 1974. "Assignment to treatment" ($Z_i = 1$) denotes whether the turnout in the 1964 Presidential election was less than 50%. A city receives treatment ($W_i = 1$) if by 2002 a majority of the council is elected by district. The adjusted growth in city non-infrastructure spending is computed over the period 1977 to 2012. A city is a "complier" if it switches to district elections if and only if turnout is less than 50%. "Nevertakers" keep at-large election regardless of 1964 turnout, while "alwaystakers" switch to district elections regardless of 1964 turnout.

Given that the assignment of treatment Z_i is assumed to be random, the fraction of nevertakers among cities with $Z_i = 0$ is the same as the fraction of nevertakers among cities with $Z_i = 1$. This

last expression is equal to

$$\pi_{nt} = \frac{N_{10}}{N_{10} + N_{11}} = \frac{165}{165 + 223} = 0.42 \quad \text{(s.e. 0.03)}.$$

Similarly, the share of alwaystakers is equal to the share of the alwaystakers when $Z = 1$, which is

$$\pi_{at} = \frac{N_{01}}{N_{01} + N_{00}} = \frac{234}{234 + 1{,}010} = 0.19 \quad \text{(s.e. 0.01)}.$$

Finally, the share of compliers is equal to the difference in the likelihood of switching to district election ($W_i = 1$) between cities with less and greater than 50% turnout ($Z_i = 1, 0$):

$$\pi_{co} = \frac{N_{11}}{N_{10} + N_{11}} - \frac{N_{01}}{N_{00} + N_{01}} = \frac{223}{165 + 223} - \frac{233}{1{,}010 + 23} = 0.39 \quad \text{(s.e. 0.03)}.$$

Given the unconfoundedness assumption, the growth in spending for alwaystakers is the same in cities with voter turnout higher or lower than 50% ($Z_i = 0, 1$). Thus, Table 8 indicates that the adjusted growth in spending for nevertakers is 47%. Similarly, the adjusted growth for alwaystakers is also 47%.

Estimating the growth rate in spending for compliers takes a little more work. Note that the expected growth rate for cities that have $Z_i = W_i = 0$ is equal to the weighted average of the growth rate for compliers and nevertakers:

$$E[Y_i|Z_i = 0, W_i = 0] = \frac{\pi_{co}}{\pi_{co} + \pi_{nt}} E[Y_i|i \text{ is a complier}, Z_i = 0] + \frac{\pi_{nt}}{\pi_{co} + \pi_{nt}} E[Y_i|i \text{ is a nevertaker}, Z_i = 0].$$

Hence, the expected growth for compliers that are in a county with voter turnout greater than 50% is

$$\begin{aligned} E[Y_i|i \text{ is a complier}, Z_i = 0] &= \frac{\pi_{co} + \pi_{nt}}{\pi_{co}} E[Y_i|Z_i = 0, W_i = 0] - \frac{\pi_{nt}}{\pi_{co}} E[Y_i|i \text{ is a never taker}, Z_i = 0] \\ &= 0.38. \end{aligned}$$

Similarly, the expected growth for compliers that are in a county with voter turnout less than 50%

is

$$E[Y_i|i \text{ is a complier}, Z_i = 1] = 0.54.$$

These calculations show that growth rates in non-infrastructure spending vary by type of city. Among cities that keep at-large elections, spending grows by 47% for noncompliers versus 37% for compliers. Among cities that switch to district elections, spending grows by 47% for alwaystakers versus 54% for compliers. Note further that we do not have any information on the growth rate for alwaystakers who keep at-large elections or nevertakers that switch to district elections. Hence, we can only determine the impact of district elections on compliers. For these cities, district election increases the growth rate in non-infrastructure spending by 16% points, since

$$E[Y_i|i \text{ is a complier}, Z_i = 1] - E[Y_i|i \text{ is a complier}, Z_i = 0] = 0.16 \quad (\text{s.e. } 0.05).$$