Columbia University

Department of Economics
Discussion Paper Series

Non-Discriminatory Electoral Rules, Local Public Goods, and Redistribution: Evidence from US Municipalities

Ferran Elias

Discussion Paper No.: 1314-20

Department of Economics
Columbia University
New York, NY 10027

July 2014

Non-Discriminatory Electoral Rules, Local Public Goods, and Redistribution: Evidence from US Municipalities

Ferran Elias *

Columbia University

June 24, 2014

Abstract

The Voting Right Act guaranteed the right to vote for minorities, including the prohibition of any electoral discriminatory practices on the basis of race. This triggered a series of court decisions outlawing discriminatory electoral rules between 1970 and 1990. I study the effects of several court orders that guarantee minority representation on city public budgets, and find that both local public good expenditures (5-7.5%) and city tax collection (5-10%) increased, after the changes towards non-discriminatory electoral rules. I also explore the distributional consequences of non-discriminatory elections and find that the fraction of black public workers and citizens increased after changes in the election system, while those of whites decreased. The growth rates of black house values and rents also increase more. The findings are inconsistent with a negative effect of ethnic heterogeneity in the city council on public goods, and with common-pool theories. I show evidence that the most plausible channel that explains the results is the new legislative bargaining power that black communities gained.

*I want to thank Chris Boone, Varanya Chaubey, Ethan Kaplan, Wojciech Kopczuk, Bentley MacLeod, Suresh Naidu, Petra Persson, Raúl Sánchez de la Sierra, and seminar participants at Columbia University, the American Law and Economics 2014 Meetings at Chicago University, and at the 2012 Alexander Hamilton Graduate Student Conference for Political Economy at NYU for helpful comments. I also want to thank Kathleen Dreyer for help accessing data, and Nathan B. Anderson for sharing his data on property tax limits. All resulting mistakes are my own. fe2139@columbia.edu

"Whites put fire hydrants mainly in their neighborhoods and few in the black areas... and small water lines to black hydrants made them useless." -Black town councilman, Gretna, Florida. (Button, 1989, p. 114).

1 Introduction

When blacks and whites prefer different candidates at the polls, electoral districts can be drawn such that whites obtain all representatives, ensuring that minorities can never elect a candidate of their choice. In 1973, the US Supreme Court considered that such election methods could be proven to be unlawful if members of a group "had less opportunity to participate in the political processes and to elect legislators of their choice."[1] This ruling triggered the development of a national jurisprudence that, on a gradual basis, further clarified and reduced the burden of proof necessary to outlaw discriminatory electoral methods (Davidson and Grofman, 1994a; Gerken, 2001).[2] In this paper I exploit the court-orders that externally enforced changes towards non-discriminatory election rules to look for the effects on local public goods, taxation, housing values and rents, public workers, and sorting, of the prohibition of discriminatory elections.

The sample is a panel of cities that were externally required to change their electoral rule and cities that were not. I focus on a specific type of discriminatory electoral rule, at-large elections, and the solution engineered by the courts to it: single-districts. In an at-large system, city elections are organized with only one electoral district, the city, and through plurality rule. There are several candidates that run for election. Crucially, citizens can cast as many votes as seats are to be filled. When voting is polarized across ethnic groups, at-large elections do not give representation to minorities because they are outnumbered by the majority. In contrast, in single-district elections, the city is partitioned into several

[1] See White v. Regester, 412 US 755 (1973).

[2] See Kousser (1984) for a list of discriminatory electoral methods, known in the literature as vote-dilution techniques. He also discusses how these mechanisms work, and provides a history of their origins, which dates back to the first Reconstruction and post-Reconstruction periods.

electoral districts. In general, one candidate is elected from each district in a plurality vote. Minorities are packed in a number of districts proportional to their population share. Thus, single-districts guarantee that minorities can effectively elect a candidate of their choice.

The findings can be summarized as follows: first, local public goods and tax collection increase after single-districts are enacted; second, there is no effect on intergovernmental transfers; third, cities that have to change to single-district elections increase the fraction of black public workers and experience gains in black population after the change. These effects are matched by similar decreases in white and other ethnicities public workers and citizens; fourth, the growth rate of black house values and rents increases more than that of whites. The latter effect could be driven both by capitalization of the higher public spending and by an increase in demand of housing by blacks.[3] Fifth, I do not find crime reductions after the changes in electoral rule, but some evidence of increases in the number of black homicides; fifth, the changes in the city budget happen in general for all cities no matter their level of previous discrimination, the effects being weakly stronger for cities where discrimination had been more important in the past. Overall, this set of results shows that electoral rules that give more proportional representation to minorities matter for the level of public goods and for its potential distribution across communities, as well as for the distribution of public jobs across ethnic groups. In addition, even cities with a former strong support for segregation change their public budgets. Last but not least, the common law system, in which both courts and legislative bodies create law, is crucial to protect the voting rights of minorities.

Identification in this paper exploits three main advantages offered by the quasi-

[3]Municipal services affect the quality of life of a community and, consequently, are capitalized into home values (Oates, 1969; Bradbury et al., 2001). Brueckner and Joo (1991) argue that the effect of public spending on the voter's wealth must enter in his decision calculus along with consumption and cost considerations. They show that the voter's ideal level of spending is a mixture of their preferences and those of an eventual buyer of their house. Moreover, the house is the main asset for most families (Tracy et al, 1999) and renters seem not to oppose to increases in local public goods (Oates, 2005).

experiment and the data : exogenous changes, abundant variation, and a panel dataset. Firstly, during the 1970s and 1980s many cities, specially in the South of the US, were forced by courts to change towards single-districts electoral rules.[4] This provides a unique quasi-experiment to test for the effects of electoral rules in ethnically divided societies. The electoral reforms were related to the development of a nationwide jurisprudence that is exogenous to local conditions that could bias the estimates, and to any changes at the local level that could drive both the changes in electoral rules and in policy.[5] The solution proposed by the courts was to engineer single districts in which black minorities were packed to allow them to elect their own representatives.[6]

Secondly, by focusing on cities I can exploit many electoral reforms, which in general are a rare event (Katz, 2005). The literature studying the effects of electoral rules has mainly focused on cross-sectional or panel datasets of countries (Persson and Tabellini, 2003; Albalate et al., 2012) to show the correlations between electoral rules and fiscal policy. Thus, the context of the US South offers very interesting variation not only because of its exogeneity, but also because of the abundant changes that happened.

Finally, the longitudinal nature of the cities' public finance data allows me to run a fixed-effects specification. Identification relies on a parallel trends assumption. I provide graphical evidence of no pre-treatment trends by including leads and lags in a dynamic specification. The evidence supports the identification as-

[4]I focus on cities that were forced to change to single-district elections due to a court-order or that had some sort of external pressure to do so, such as a lawsuit. The development of an anti-vote dilution jurisprudence implied that all cities with a sizable minority and with at-large elections were pressured to change. Specially after being sued, since they did not have the resources to defend themselves in court.

[5]Trebbi et al (2008) also focus on electoral reforms in US cities but they drop municipalities that were court-ordered to change because their interest was on endogenous institutions. In contrast, the sample in this paper is comprised of cities that were forced externally to change their electoral rule, and of those that did not change it.

[6]Single-districts served as geographic political reservations to guarantee representation for a historically disadvantaged community in the US. See Duflo (2004) for a review of the literature on political reservations in India.

sumption of parallel trends, allowing me to interpret the estimates as the causal effect on the city budget of outlawing at-large elections.

The results can be used to discriminate between alternative theoretical mechanisms of the effects of electoral rules on local public goods and the distribution of resources. First, there is a literature that argues that ethnic heterogeneity has negative effects on public goods (Alesina and LaFerrara, 2005; Alesina et al., 1999; Easterly and Levine, 1997). With single-district elections, the local councils become more ethnically diverse.[7] If that channel is operating in the local parliament, local public goods and taxation should decrease once at-large elections are outlawed. Since they actually increase, ethnic heterogeneity in the city council does not have a negative effect on public goods and taxes. The results are of interest because very little is known about how the documented negative effects of ethnic diversity might be mediated by the institutions that determine fiscal policy, and how changes in their design might alleviate the problem.[8] This is of special importance since minorities tend to be underrepresented or even excluded (Lijhpart, 1986) in these institutions.[9] The results suggest that the underprovision of public goods is partially caused by the lack of representation that minorities suffer, and that the underprovision disproportionally affects the members of the minority group. Moreover, the underprovision can be made less severe if the electoral game is designed to guarantee minority representation.

A second potential channel is related to the literature on how representatives of geographic constituencies choose the amount of local public goods to provide. Weingast et al. (1981) show theoretically that when a political constituency is divided into districts, spending is higher. Moreover, they predict that the level of spending will be higher the more districts there are. Thus, a move from at-large

[7]There is a large body of evidence documenting that single-districts increase black representation: Davidson and Grofman, 1994a; Trebbi et al., 2008; Valelly, 2004.

[8]Alesina and LaFerrara (2005) survey the literature.

[9]Sample comprises all cities with at least 2.5% black people or at most 40% black citizens, which are the ones where the minority is big enough to benefit from the changes in institutions that I study. Thus, all these cities have a certain degree of fractionalization. The results are robust to changing the lower threshold to 5% and to moving the higher threshold up to 50%.

elections (1 district) to single-district elections (several districts) should increase public expenditures. In addition, the more districts are created, the more spending and taxation should increase. I test for the latter prediction using the number of districts created and I do not find significant differences when more districts are created. Moreover, note that this mechanism applies for all ethnicities: the fact that only black public workers and black population increase, and that changes in growth rates for black house values and rents are larger, provides further evidence to discard this channel.

The third theoretical channel to explain the empirical results is drawn from legislative bargaining models (Baron and Ferejohn, 1989; Persson and Tabellini, 2000). A central result in this literature is that minimum winning coalitions consist of the agenda-setter and those legislators whose support is the easiest to obtain. Black representatives are likely to have a weak bargaining position in the council because they will be a minority, and because it will be their first time in office. Legislative bargaining models show that even minority groups can negotiate and get benefits for their districts. There is also support for this mechanism from historical sources documenting the emergence of black pragmatic leaders ready to close deals with white politicians (Lawson, 1985). Thus, aggregate public good spending should increase as long as that does not happen at the expense of public goods in white districts. In that case, there should also be an increase in taxes and an increase in local public goods targeted at black communities. Moreover, black concilmen should also be able to improve black job finding in the public sector. The results exploring who benefits from changes in electoral rules give support to this mechanism: there are more black public workers and black citizens after a change to single-districts representation. In addition, increases in growth rates of black house values and rents are higher than those for whites.[10]

This paper brings together two literatures. On one hand, the impact of voting rights statutory law on economic outcomes has been documented in the literature

[10]The effects on house values and rents could be driven both by capitalization of the new public expenditures, and by an increase in demand of houses by blacks given the increase in black population.

(Husted and Kenny, 1997; Cascio and Washington, 2014; Naidu, 2012). On the other hand, there is evidence of how court decisions affect economic outcomes: Miles (2000) for effects on employment outcomes, Currie and MacLeod (2008) and Avraham et al. (2012) for impacts of tort reforms on health care. This paper bridges these literatures by showing how the court system affects economic outcomes through minority voting rights protection. The main contribution of this paper is to show the effects of a sequence of court cases, that considered that minority voters "should enjoy an equal opportunity to coalesce effectively despite the mandate of majority rule," on local public goods, taxation, house values and rents, public workers, and population sorting. The results highlight the importance of the common law system, in which both courts and legislative bodies create the law, to protect the voting rights of minority voters.

The second contribution of this paper is that it sheds light on why some federal policies to end segregation might have partially failed.[11] For instance, Cascio et al. (2013) study the effects of federal grants and federally mandated desegregation in schools, and find that black dropout was not reduced, while white dropout was. The authors consider that a potential explanation for their result is the leeway that local institutions have to allocate the money to schools attended by whites. At-large elections were also in place in school district boards (Kousser, 1999; NCVR, 2006), and could have been used to divert the money to only white schools, as the results for city budgets suggest.

Finally, I also show evidence that politics matters at the local level (Tiebout, 1956; Epple and Zelenitz, 1981; Epple and Romer, 1991). The presence of changes in policy could be partly due to the fact that a change in electoral rule modifies completely electoral competition and, consequently, legislative bargaining in the council. Moreover, the anti-vote dilution jurisprudence implied that all cities were subject to these changes, probably limiting Tiebout sorting. Thus, the change towards single-districts is likely to have more important policy effects that changes in mayor's partisanship or gender.[12]

[11] See Almond et al. (forthcoming) for a study of the positive effects on black infant survival after hospital desegregation.

[12] Ferreira and Gyourko (2009) and Ferreira and Gyourko (2011) show that there are no effects

The paper is organized as follows. In section 2 I explain the details of at-large and single-district elections and review the history of voting rights in the US. In section 3 I provide a theoretical interpretation of the results. Section 4 describes the data that was used. Section 5 explains the empirical strategy and shows the results. Finally, in section 6 I conclude.

2 Electoral Rules Background

In this section I provide two types of background information relevant for the analysis. First of all, I will describe the electoral rule used to hamper minority representation, at-large elections, and its origins; as well as the solution engineered to give political representation to black communities, single-districts. Secondly, I explain the history of the prohibition of at-large elections by courts, and argue that the court orders provide exogenous variation that allows for identification of the effects of single-districts on economic outcomes.

2.1 At-large Elections vs. Single-Districts

At-large elections make it impossible for minorities to be represented when voting behavior is polarized across communities.[13] In this electoral system, the municipality is a single constituency, and all aldermen are elected from the same district (the city) through plurality rule. Crucially, all people who live within the city borders can cast as many votes as seats are in the city legislature. This allows

of mayor's partisanship and gender respectively. Gerber and Hopkins (2011) show partisanship effects for the categories of spending over which the city has budget authority. See Glaeser (2013) for a recent review.

[13]Voting behavior in the South of the US was polarized during the decades after the approval of the VRA, and in many cases is still polarized. Black people vote for black candidates and white people vote for white candidates. For more information see NCVRA (2006, p. 90).

white communities to cast all votes for white candidates and fill all the positions in the city council, making it impossible for minorities to be represented.[14] Figure 1 and 2 gives an example for the City of Florence, in Alabama.

At-large elections have their origins in the first Reconstruction and post- Reconstruction era (Kousser, 1984). Their objective was to minimize the threat of black political power efficaciously but quietly, so that there was no federal government intervention to protect black voters. Not denying the franchise, this election method was aimed at hampering minority representation.[15] After the passage of the VRA, at-large elections started to be enacted again with the same purposes as in the XIXth Century. Trebbi et al. (2008) show empirical evidence of how between 1960 and 1967 cities in the South of the US where black people constituted a minority of the population were adopting at-large elections.[16] The court-orders that I exploit created single-districts in those places that enacted at-large elections in both the XIXth and XXth century to avoid black representation.

Single-districts enhance a more proportional representation of minorities and were the alternative election method proposed by courts after the outlaw of at-large elections in certain cities. This electoral structure divides the city in different districts, each of them electing one representative by majority rule. The candidate most voted in her district will win a seat in the council. Citizens can only cast one vote. Due to the segregation of racial minorities, this type of electoral rule enhances minority representation.[17] In fact, the districts created for black communities can

[14]See Amy (1993) for a more detailed explanation of both at-large elections and single-district elections.

[15]At-large elections were not the only method used to prevent minority representation. These techniques are known as vote-dilution mechanisms in the history and political science literatures. Kousser (1984) identifies sixteen different devices for vote dilution. See Kousser (1984) and Davidson (1994, p. 22-24) for more details on how these other mechanisms work.

[16]For example, Mississippi's legislature started in 1966 a Massive Resistance Legislation (Parker, 1990) aimed at setting new hurdles for black officeholding after the passage of the VRA.

[17]See Cutler et al. (1999) for an explanation of the dynamics of the ghetto in US. According to them, in 1970 the average urban black lived in a neighborhood that was 68% black. In 1990 it was the case for 56% urban black (p. 456)

be understood as geographic political reservations for black candidates because they were designed to contain enough black population so that they would be able to elect black representatives. The widespread use of single-districts in the second half of the XXth Century was possible thanks to the VRA and several court-orders that I explain in the next subsection (Davidson and Grofman, 1994).

Finally, mixed-systems combine features of both at-large and single-district elections. Some candidates will run for the city-constituency (at-large) and some will run for one of the districts partitioning the municipality (single-district). For instance, in a city with seven aldermen, four might be elected through at-large elections and three might be elected through single-district elections.

2.2 The Creation of Single-Districts

Single-districts were created in many US cities thanks to the VRA and several court-orders that simplified the judicial process necessary to outlaw at-large elections and implement single-districts. In this section I will detail the history behind the sources of variation I exploit, and argue and show evidence that a fixed-effects specification will be able to capture the causal effects of single-districts on spending and taxation.

The legal starting point to ban at-large elections is section 2 of the VRA, which prohibits practices that discriminate on the basis of race.[18] Its initial interpretation made that the first lawsuits had to prove that at-large elections had a discriminatory intent. This supposed a burdensome requirement that made it very difficult to win a case. However, the burden of proof required was about to be simplified significantly by some court-orders that influenced the following lawsuits and changes in electoral rules. In a nutshell, these lawsuits listed several factors that were enough to prove indirectly that the election system had a discriminatory intent. With the adoption of an easier to proof results-based test, plaintiffs had to

[18]The exact wording is: "No voting qualifications or prerequisite to voting, or standard, practice, or procedure shall be imposed or applied by any State or political subdivision to deny or abridge the right of any citizen of the United States to vote on account of race or color."

prove a history of discrimination in the specific jurisdiction and the discriminatory consequences of the dilutive procedures.

The first cases to simplify the proof requirements to be brought to court were White v. Regester (1973) and Zimmer v. McKeithen (1973).[19] They are the first two landmark cases for the creation of a national anti-vote dilution jurisprudence. As can be seen in figure 3, before 1973 there are barely changes in electoral rules, whereas the number of changes peaks for first time after these court-orders. Despite these first advances, the White-Zimmer framework was demolished when the Supreme Court (Mobile v. Bolden, 1980) asserted that "the XVth Amendment does not entail the right to have Negro candidates elected".[20] Figure 3 shows how after 1980 the number of changes decreases until being zero in 1982. However, in 1982 the VRA had to be renewed by the US Congress and Senate, and knowing the difficulties that Mobile v. Bolden imposed for plaintiffs seeking to dismantle vote-dilutive devices, section 2 was amended to incorporate the White-Zimmer framework. In addition, the Senate Judiciary Committee wrote a report listing several factors that courts could use to decide whether a particular device had a discriminatory effect prohibited by section 2.[21] The Senate factors became very influential for the courts interpretation of the VRA and reduced the uncertainty about the burden of proof that was necessary to show by plaintiffs to win a case. After the amendment of the VRA the number of changes from at-large elections to single-district systems peaks again. Finally, Thornburg v. Gingles (1986) simplified even further the anti-vote dilution jurisprudence. We can see in figure 3 how the changes in electoral rules peak again after 1986.[22]

Thus, the development of a national anti-vote dilution jurisprudence originated

[19]See White v. Regester, 412 US 755 (1973) and Zimmer v. McKeithen, 485 F.2d 1297 (5th Cir. 1973).

[20]See Mobile v. Bolden, 446 U.S. 55 (1980).

[21]The factors are: a history of official discrimination, the presence of vote polarization across racial lines, the usage of potentially discriminatory voting practices, the denial to minorities to participate in the candidate slating process, the presence of discrimination in education, employment and health; a history of racial appeals in political campaigns, and the extent to which members of a minority have been elected to public office in the jurisdiction.

[22]See Thornburg v. Gingles, 478 U.S. 30 (1986).

the changes towards single-districts. New election methods were not enacted because of voluntary agreement between the ethnicities of each city, but because of external forces. This provides a compelling justification for a difference-in-differences strategy because it is unlikely that the changes in electoral rules happen after significant changes in local economic and political conditions that would violate the parallel trends assumption . In order to further test for the identifying assumption, in section 5 I run dynamic specifications including leads and lags. None of the leads are significantly different from zero, and there is no evidence of any pre-treatment trend. These results provide evidence of the validity of the empirical strategy and allow to interpret the effects of changes to single-districts on expenditures and taxation as causal.[23] Moreover, the differential timing of the changes provides evidence ruling out the possibility that another common shock to the treated municipalities is driving the results.

Another relevant question is what are the reasons behind the differential timing of changes. It would be worrisome for the identification strategy if the time of adoption of single-districts was correlated with city characteristics. I run a test for joint significance of the year of change with several city variables and I do not find any evidence pointing to a relationship between the year in which a change happens and some city characteristics. For only two of the variables the test of joint significance is significant at the 5% level. Results are summarized in figure 4. [24]

Finally, what guaranteed that after a change to single-districts the cities did not adopt other methods to hamper black representation? This was achieved by Section 5 of the VRA, which requires that any election system change in certain covered jurisdictions has to be submitted to the Department of Justice for preclearance.[25] Submitting jurisdictions have to show that the modifications do not

[23]See Acemoglu (2005) for a critique of empirical political economy research dealing with the identification problem using selection on observables and exclusion restriction methods.

[24]The specification for the test of joint significance is: $y_i = \alpha + \sum_{j=1970}^{1990} \beta_j Changej_i + \epsilon_i$.

[25]The covered jurisdictions are Alabama, Alaska, Arizona, Georgia, Louisiana, Mississippi, and South Carolina were fully covered by section 5. Texas and Virginia were also covered with the exception of some counties. There are also scattered counties and townships covered.

have a racial discriminatory purpose or effect. Thus, section 5 was essential to limit the extension of new dilutive practices.[26] Thus, the combination of section 2 and 5 of the VRA with several court-orders allowed a stable shift in minority representation.[27]

The implementation of single-districts increased black representation (Davidson and Grofman, 1994; Trebbi et al. 2008; Welch, 1990).[28] Figure 5 shows the difference between the percentage of black voting-age people and the proportion of black local representatives. As can be seen, there are gains in local representation. The most important ones happen in Alabama, Mississippi, Lousiana, Georgia and South Carolina.

To sum up, the creation of single-districts in the US during the 1970s and 1980s was related to the development of a national anti-vote dilution jurisprudence. The exogeneity of the changes and the lack of pre-treatment trends (proved in section 5) give reassurance that a fixed-effects specification will capture the causal effect of electoral rules on local public good spending and taxation.

3 Theoretical Interpretation

There are three channels that predict different effects on local public good expenditures and taxes. In this section, I explain each of them, as well as the different repercussions that they have for the distribution of spending across ethnic communities. Each model provides implications that I test in the empirical section to give evidence of the mechanism driving the results.

[26] In Allen v. State Board of Elections (1969), the Supreme Court gave a broad interpretation to the coverage of section 5, meaning that even minor changes or those affecting voting indirectly had to be submitted for preclearance.

[27] This is an important feature of the changes to single-districts given the degree of institutional persistence in the South of the US. See Acemoglu and Robinson (2006 and 2008).

[28] This is a well-known result in the literature and that is why I do not focus on it. Instead, I explore the budgetary and distributional consequences of changes in electoral rules.

The first potential mechanism is related to the link between ethnic heterogeneity and public good provision. A negative effect of ethnic diversity on public good provision has been documented in the literature (Alesina et al., 1999; Easterly and Levine, 1997; Miguel, 2004). Alesina et al. (1999) argue that representatives of ethnic groups value only the benefits of local public goods that are concentrated in their community and discount the benefits that accrue to other groups. This leads to underprovision of local public goods since not all their positive effects are taken into account, and the population prefers to pay less taxes and devote these resources to private goods rather than public goods.

The question of interest for us is whether this channel applies after a change to single-districts. Since at-large elections exclude the minority and single-districts guarantee its representation, the local parliament will become more diverse racially. In addition, electoral competition will be organized through ethnic representatives and ethnic constituencies, emphasizing the fact that representatives only value the benefits they provide to their community. Then, following Alesina et al. (1999) we expect to see that the level of public spending and taxes goes down.

A critical assumption in Alesina et al. (1999) is that politicians discount the benefits that public goods provide to other constituencies. However, this might not necessarily be the case. The second channel is developed by Weingast et al. (1981), who model how representatives of geographic constituencies decide over the amount of local public goods to provide. They show that when the political constituency is divided into districts, the level of spending is higher. This happens because the electoral districts create a divergence between the political benefits and costs of each local public good when taxes are the same across districts. Thus, a change from at-large (1 district) to single-districts (several districts) should increase public expenditures. Moreover, Weingast et al. (1981) also predict that the level of spending will be higher the more districts there are.[29] Consequently, if that is the mechanism, the increases in spending should be higher the more districts the city has. Note that the mechanism applies no matter who gains representation

[29]See Baqir (2002) for empirical evidence supporting this result based on a cross-section of US cities in 1992.

and its ethnicity. Thus, we should expect that the increases in local public goods happen in all districts and that it is capitalized into both white- and black-owned house, and in the rents both groups pay. In addition, changes in public workers and population should be of similar size for both communities.

A last channel is based on models of legislative bargaining, such as Baron and Ferejohn (1989)[30], that predict that minimum winning coalitions emerge among the agenda-setter and those legislators whose support is the easiest to obtain. Since blacks will get representation for first time after a change towards single-districts, and they will only hold a minority of the seats, they are likely to hold a weak bargaining position in the council. However, precisely because of that weak position, they can manage to be part of the minimum winning coalition.[31] Thus, the weak representation power gained by black minorities allows them to influence the level of provision of public goods and its distribution. Self-interested white politicians had a new companion that, given the low levels of investment in his neighborhood, was easier to please and to obtain his support for other policies. That is in stark contrast with at-large elections, when black communities had no representatives in the council and thus had no role in budget bargaining within the council. Consequently, if blacks manage to increase spending in their districts, and that does not happen at the expense of white districts, the level of provision should increase and its distribution should be more favorable to black communities. In that case, we should expect that capitalization of public goods into home values happens only in black districts and not in white neighborhoods.

There is also some support for that mechanism from historical sources. Lawson (1985) explains that in a scenario of white-supremacist majorities, black politicians had to necessarily forgo their legitimate grievances to get support for their policies from white legislators. Kenneth Clark sums up the attitude necessary for successful policy change: "The Negro political official must assume the additional burdens inherent in defining politics as requiring a tough-minded and realistic appraisal of the power available to him, a determination to obtain and use effectively the

[30] See also Persson and Tabellini (2000) for a review.

[31] Austen-Smith and Banks (1988) show that in a parliamentary setting the smallest party will always be included in the coalition since their support is the cheapeast to obtain.

power necessary to effect a desired and observable change, and balance this by a stable, deep, and broad sense of human values."[32] In the transition from civil rights to electoral competition, black communities turned from leaders with a more radical vision to more pragmatic leaders, or as stated by Lawson (1985, p. 267), to politicians "who practiced politics as the art of the possible around the bargaining table." At the same time, white politicians were interested in promoting rapid industrial growth in the South. By joining this alliance, black politicians were able to provide benefits to their communities (Lawson, 1985). Thus, the rise of more pragmatic elected black leaders gave black communities the clout needed to benefit from the distribution of local public goods.

4 Data and Main Variables

I construct a unique panel dataset of 126 cities from 1970 until 2000 with both data of the creation of single-districts, government finances, home values and rents by race, public workers by race, and sorting by race. This dataset allows me to look at whether public good provision changes after single-districts are enacted, and to learn who benefits from these changes by looking at capitalization effects, the fraction of public workers of each race, and population movements. In this section I present the datasets, provide descriptive statistics, and explain the main independent and dependent variables that I use.

The sample is restricted to cities in which blacks were enough people to have their own single-district (more than 2.5% of population) and not a majority of the city inhabitants (less than 40% of population).[33] The reason to justify such sample selection stems from the findings in Trebbi et al. (2008): during the 1960s, around the years when blacks would regain their right to vote thanks to the VRA, cities that had a black minority switched their election system to at-large elections

[32] Lawson (1985, p. 267).

[33] The results are both robust to changing the lower threshold to 5% and to moving the higher threshold up to 50%.

to difficult black representation. On the other hand, cities in which blacks were a majority, the council switched to single-districts to minimize their representation. Thus, is in cities in which blacks are a minority in which we should expect that the electoral rule matters the most to obtain minimal representation.[34] If the political representatives of the 1960s hurried up to change the system of representation is probably because they feared that black representation would change the way in which the city was ruled, and the distribution of public services and public jobs across communities.[35] It is therefore in this cities were single-districts should make a difference.

I collect data on local government revenues, expenditures, employment, and population from the Census of Governments. This dataset contains yearly information for the largest cities and every 5 years for all local governments. To take advantage of high frequency data, I keep in the sample cities for which there is data every year. All the financial data is deflated using as a base year 2005.[36]

If we want to understand the effects of single-districts on the city budget, we should use as outcome variables those categories of spending in which local governments do not share authority with other level of governments (federal, state, and local).[37] Table 1 shows the eight main categories in which municipalities spend money. The first column shows the mean of the budget share of each item. The second and third columns are the mean of the spending per capita and the

[34]In cities with a black majority and single-districts, blacks can elect representatives proportionally, as well as whites. In that case, the switch to single-districts from at-large elections passed by the political representatives was to ensure that whites maintained some representation despite being a minority.

[35]Trebbi et al. (2008) also show that such strategic behavior happened between 1970 and 2000. However, their sample is comprised of cities that were not court-ordered to change their electoral, since their focus is on endogenous institutions. In contrast, I focus on externally enforced changes to study effects on the public budget and the distribution of resources across ethnicities.

[36]The deflator is from the Bureau of Labor and Statistics.

[37]Ferreira and Gyourko (2009) study whether partisanship matter at the city level. They use aggregate expenditure variables and do not find any significant effect. Gerber and Hopkins (2011) follow the same empirical strategy, but disaggregate the spending categories and find that partisanship does matter for fire and police protection, two of the budget items in which cities have total authority.

mean of the ratio between the amount of intergovernmental transfers and direct expenditures by the local government. Low levels of this ratio reflect the budget items that are decided at the city council. According to table 1, local politics will matter for local public goods such as police and fire protection, sewerage and parks.[38] [39] It is in these categories that we should expect to see effects of changing the electoral rule to guarantee minority representation.[40] Finally, the last column shows how most of the money spent on roads, local hospitals and public housing comes from transfers, implying that municipalities have at most small influence on these policies. Thus, from now on, I will refer as local public good spending the sum of fire, police, park, and sewage expenditures (LPG). In terms of taxes, municipalities can fix the property tax rate and can change part of the sales tax rate. These are the main sources of own revenue and I will refer to them as *Local taxes*. I will also look for effects in the number of workers in the local public good sector.

Data on changes in electoral rule and year of the change is obtained from Davidson and Grofman (1994). Their dataset is available at the Interuniversity Consortium for Political and Social Research (ICPSR) and contains 241 cities in the southern states of Alabama, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Texas and Virginia. For 74 of these cities I have yearly financial

[38]For utilities, police and fire protection, and parks there does not exist a category of transfers. That is why the ratio is 0. Even if I added all the transfers earmarked for other purposes and the transfers for general local government support and divided it by the sum of spending in utilities, police and fire protection, and parks, the ratio is of 0.0001517.

[39]The absence of intergovernmental transfers for these categories is also reassuring that there is no crowding out of local government spending in these budget items because of federal or state grants. See Bradford and Oates (1971, a,b) for a theoretical analysis of how intergovernmental transfers can crowd out government spending and Knight (2002) for empirical evidence of crowding out using exogenous variation in delegation political power.

[40]Even if the level of intergovernmental transfers spent in utilities is zero, we should not expect changes on this policy because of changes in the electoral rule. Local utilities are managed by public and private firms. Then, local governments do not have complete authority over this policy either. Moreover, households pay for the utilities used through fees or taxes that target each household according to the amount consumed, so that there is no space for transfers across households depending on who pays a service and who actually benefits from it.

data. I expand the sample with 52 cities with data from the International City Managers Association (ICMA) surveys on the municipal form of government for the years 1981, 1986, 1991, 1996, and 2001. The surveys contained questions about the number of aldermen elected by single district. Out of the 52 cities, 12 changed their electoral rule and 40 did not change its election system. The variable of interest is *Change*, a dummy indicating whether a city had to change its electoral rule.

In order to understand who benefits from the changes in expenditures I will use house values, rents, public workers, and population disaggregated by race. These were downloaded from the National Historical Geographic Information System at www.nhgis.org. The data on public workers contains all public workers that live in the city, not only those who work for the city.[41] I also use crime data disaggregated by race to explore whether there was a decline in homicides after a change in electoral rule. The data is from the Uniform Crime Reports of the FBI, and it contains the number of murders that happened in each city, differentiating both for the race of both the victim and the offender.

I also got from NHGIS the main control variables: population by race, share of old people, number of rooms in each house by race, and the year in which the house was built by race.

Table 2 shows summary statistics for both the treatment and control group in 1970 and 2000. The main difference between cities that had to change their electoral rule and those that did not have to is that the former are located mostly in the US South. This is not surprising given the different history of discrimination of blacks in the southern states. The percentage of aldermen elected by single-districts changed dramatically between 1970 and 2000 for treated cities as a consequences of the court orders and lawsuits. By 2000, 87.8% of local politicians were elected by single-districts, compared to none in 1970 Note also that some cities switched to a mixed system with some aldermen still elected citywide, since

[41]Despite city representatives having the most to say about city public workers, they might still be able to influence who gets hired at other levels of governments: they can announce job openings in their communities, propose candidates, and make sure that the hiring process is fair.

the percentage is not 100 in 2000. The descriptive statistics also show that the treatment cities are larger and have a bigger fraction of black population. Blacks also live in cheaper houses and pay lower rents than whites. It is also worthy to point out the important changes in black public workers between 1970 and 2000 in cities that had to change their electoral rule. Finally, cities that had to switch to single-districts spent and collected less taxes than cities in the control group. These differences, however, are much smaller in 2000 than in 1970. The summary statistics show important differences between cities in the treatment and control group. However, identification relies in the parallel trends assumption. In section 5 I provide evidence of no significant pre-treatment trends.

As is shown in table 2, most of the sample is composed of cities in the South of the US. This is natural since most black people live in the Southern states. Almost 20% of the people in these cities were black. The median black house value was lower than the median white house value, reflecting the disadvantaged economic condition of blacks in the US. The share of aldermen elected by single-districts in 1970 was less than 7%, reflecting the widespread use of discriminatory electoral rules. In 2000, this share was 66%, a sharp rise caused by the combined force of the VRA and the courts.

In order to construct measures of discrimination at the county level and test for the mechanisms that are driving the results, I use the electoral vote that George Wallace obtained as a presidential candidate in 1968 when he ran with a segregationist platform (Clubb et al., 2006). As a second proxy for discrimination I use data of the number of black people lynched in each county between 1900 and 1930 from the Historical American Lynching Data Collection Project. [42]

Property tax limits data is from the Advisory Commision on Intergovernmental Relations (ACIR, 1995). It is a variable that identifies if the state government imposed any tax limitation for the local governments. Since the endogenous variables are public expenditures it is important to control for this variable because property tax limits reduce public spending and affect house prices through that channel (Bradbury et al., 2001).

[42] See http://http://people.uncw.edu/hinese/HAL/HAL%20Web%20Page.htm

5 Empirical Strategy and Results

In this section I will explain the empirical strategy and the results. The empirical strategy has four parts: firstly I use a difference-in-difference strategy to show that minority representation increases local public good provision, taxes, and public employment in the local public good sector. The increases in local public goods happen mainly in fire protection. These results rule out the first potential mechanism: that heterogeneity of preferences decreases always public good provision. Secondly, I test whether the effects are stronger the more districts are created. I show that it is not the case and discard that the results are driven by a common-pool problem. Thirdly, if black representatives were able to influence decision-making, there should be changes in the fraction of black public workers and black citizens. Changes in black population should also increase black house values and rents if housing supply does not keep up with demand. Moreover, if the increases in local public goods are targeted to black neighborhoods should also be capitalized into black house values and rents. The results are consistent with black representation benefiting mostly minorities. These results point to blacks' new legislative bargaining power as being effective in changing policy. Fourthly, I use proxies for the level of hostility that blacks faced in their cities to explore if the effects also happen for cities that faced high discrimination in the past. The changes in the public budget are of similar size regardless of the past level of discrimination, and for some variables slightly stronger in places that were more hostile to blacks. Overall, this set of results shows that electoral rules that give more proportional representation to minorities matter for the level of public goods and its distribution across ethnic groups. Moreover, the court system plays a crucial role in guaranteeing an equal access to public resources for minorities through protecting their voting rights.

5.1 Effects on Local Public Goods and Taxes

I implement a difference-in-difference strategy, controlling for city and year fixed effects, county-linear trends, as well as time-varying control variables. In addition, I report results omitting the county linear trend. City fixed effects control for all constant municipal characteristics that could be correlated with expenditures in local public goods, including geographic features and historical features of the local government. Year fixed effects control for local public good spending shocks that are common to all cities in a given year, like those related to national economic and political factors. County-linear trends control for trends in the endogenous variable that are common at the county level. The main specification is:

$$y_{it} = \alpha + \beta_1 Change_{it} + \gamma_t X_{i1970} + \delta_i + \theta_t + \omega_c * year_t + \mu_{it} \quad (1)$$

where δ_i are the city fixed effects and θ_t are the year fixed effects, and $\omega_c * year_t$ is the county linear trend. y_{it} are public spending and local taxes variables in logs per capita. X_{i1970} is a vector of baseline characteristics that contains the log of population, which controls for the size of the city and for its economic growth;[43] the share of old people, which controls for the effect on public spending of an elderly population; the share of black people, which controls for the size of the main racial minority; and a dummy variable for the presence of property tax limits, which controls for potential effects on local public goods of tax limits. β_1 is our coefficient of interest. It captures the effect of outlawing at-large elections and enacting single-districts on local public good spending or local taxes.

Table 3 shows that the changes towards single-districts increase local public good provision and local public good employment. Spending per capita in local public goods increases by 5-7%, and local public good employment per capita increases by 4-5.7%. The coefficients of the main regressor are statistically significant, except for when the dependent variable is LPG and the specification

[43] Within a country, labor is free to move across cities. Since people migrate according to growth opportunities, population captures the extent to which a city has become more attractive for economic reasons. See Glaeser et al. (1995) for a study of economic growth across cities.

includes county linear trends, in which the coefficient is marginally insignificant at the 10% level. Table 4 show the results on public revenues: the change from at-large elections to single-districts increases only local tax collection, and does not affect transfers from other levels of government. The increase in tax collection per capita is between 5-9%. Note that the size of the effects both on the expenditure and revenue side are of similar size. If cities had to finance the new spending they had to do so either by an increase in taxes, transfers, or debt. They do so through the first channel. The results show a positive effect on local public goods and tax collection of non-discriminatory electoral rules. So far, we can conclude that ethnic heterogeneity in the city council does not decrease public expenditures, as has been documented in other contexts (Alesina et al., 1999; Easterly and Levine, 1997). Thus, minority representation guaranteed through single-districts might play a role in alleviating public good underprovision, an issue largely unknown in the literature (Alesina and LaFerrara, 2005).

Table 5, panel A shows that when I disaggregate by spending categories the effect is driven by increases in fire spending. Panel B shows similars results when I disaggregate the categories of public employment. The results show that the increases in city expenditures are driven by a rise in fire protection services. This raises the question of why fire expenditures are the only ones increasing after a change towards single-districts, but not police, parks and sewage. There is evidence from several sources that fire protection was an important issue for black communities in the US South. For instance, in Hawkins v. Town of Shaw Mississippi the plaintiffs won a case proving that the town of Shaw provided municipal services in a discriminatory manner. Black areas suffered from an underprovision of fire hydrants and low water pressure. The same kind of anecdotal evidence regarding fire hydrants is provided by Button (1989, p.120) , who studies cities in Florida. Moreover, Button (1989) also documents that physical divisions between white and black neighborhood reduced the effectiveness of fire protection in black neighborhoods. In some of the cities in his sample, white and black communities were divided by railroad tracks, which sometimes impeded fire trucks to access black neighborhoods when there was a fire. In two out of six cities studied in his

book, once black elected officials gained representation in the city council, they pushed successfully for the construction of fire stations in black areas. There is also evidence of fire departments ran by members of the Ku Klux Klan in Georgia (US House Subcommittee on the Constitution of the Committee on the Judiciary, 2005), which let homes in black communities burn for a long period before any action was taken.[44] Finally, black areas are more prone to fire because of old and dilapidated housing (Button, 1989). In fact, even today blacks are disproportionately affected by fire deaths: they represent 13% of the population but account for 22% of fire deaths (FEMA, 2009).

Why does not spending on police, parks and sewage increase? Firstly, recall that the public finance data is at the city level. Thus, it could be the case that there are no increases but that there is a reallocation of resources within the city. However, the data does not allow to test for that. Button (1989) also documents racial discrimination in the provision of these services. With respect to police protection, *"the historical legacy of police brutality and a double standard of justice instilled deep-seated fears and hatred of the police in many blacks."*[45] Thus, it is plausible that black elected officials were not interested in promoting more police protection until blacks did not suffer from police discrimination. In order to test if potential changes in the allocation of police services across city districts affected homicides, I will later test wheter changes towards single-districts reduced the number of homicides by race.

Park and sewage were also public services in which blacks were discriminated. Button (1989) explains that blacks lived in districts with no parks, or very small and not well-maintained recreational areas. Likewise, black neighborhoods had worse water drainage and sewage systems. There could also be a change in the allocation of resources between districts, but the data does not allow to test for that. However, cities' public budget is rather limited by economic competition between them and constraints on their ability to raise revenue (Ladd and Yinger, 1989). In a study of the impact of partisanship in cities' budget, Gerber and

[44]See page 510 of the report for more information .

[45]Button (1989, p. 115).

Hopkins (2011) find it only matters for fire and police protection. Thus, I interpret the zero result on park and sewage spending as a consequence of the limited budget that cities have.

As a robustness test of the main results, I run a placebo regression. The results are shown in table 6. I use a sample of cities that did not change their electoral rule during this period. I simulate 1000 distributions of changes in electoral rules similar to the actual one (figure 3), and I run the same regressions as in the main specification.[46] The mean coefficient of the 1000 simulations is always insignificant and very close to zero. I obtain the same results when instead of using a fake distribution similar to the actual one I use a uniform distribution to generate placebo changes in electoral rules.

I use a dynamic specification to show that the results are not driven by pre-treatment trends. A potential concern is that there could be a preexisting trend so that local public good expenditures started increasing before the court order. The high frequency of the local finance data allows for the inclusion of leads and lags in the regression to show reassuring evidence that that was not the case. I include 5 leads and 5 lags, omitting the time period -1, the year before any change to single-districts happens. Thus, the dynamic coefficients show the change in local public spending with respect to the year before the change in electoral rule. The specification is as follows:

$$y_{it} = \alpha + \sum_{k=-5}^{-2} \beta_k Change_{it+k} + \sum_{k=0}^{5} \beta_k Change_{it+k} + \gamma_t X_{i1970} + \delta_i + \theta_t + \mu_{it} \quad (2)$$

Figures 6a to 6e show that there is no pre-treatment trend and that the positive and significant effects happen some years after the change. They plot the coefficients of the dynamic specification when the dependent variable is LPG, $LPG\,Emp.$, $Local\,taxes$, and fire protection spending and employment. The pre-treatment coefficients have confidence bands that are quasi symmetric around 0, confirming the absence of a pre-treatment trend. However, there is an insignifi-

[46] To increase the sample size of the placebos, I include cities with more than 40% and less than 2.5% of black population.

cant jump upwards for LPG and $LPG\,Emp.$ two periods before the change that might show some anticipation effects. The changes in electoral rules normally happen some years after the city has been sued. Local politicians, knowing that the changes were inexorable, could have started modifying the city budget to suit their new interests.[47] Regarding the lagged coefficients, its range of values is consistent with the results of the static specification. There are immediate positive effects after a change, though not yet significant. Further significant increases happen 3-4 years after the change for LPG, whereas the increases happen smoothly over time for the rest of the variables. The peak of the effect for all variables except employment in the fire sector happens 3-4 years after single-districts are externally enforced.

As a robustness check, I run placebo dynamic regressions following the same approach as outlined above for the static specification. Figures 7a to 7e show the results. As can be seen, the coefficient is always very close to zero and the confidence intervals are symmetric around zero. In addition, I show in figures 8a to 8e the estimated effect for three percentiles of the distribution of estimates ranked by the size of the estimate of the fourth lag.[48] The actual estimates are similar or above the estimate of the 97.5 percentile, providing evidence that the probability that the results are obtained by chance is small.

Thus, the empirical results show a positive and significant relationship between $Change$ and local public good expenditures and local taxes. There is no evidence of pre-treatment trends and the placebo regressions do not show any effect. Therefore, single-districts created to give to the black minority political representation increase local public good provision. Thus political institutions can make underprovision less severe and it is not always the case that ethnic divisions decrease public expenditures and taxes. It is yet unknown what is the channel driving the

[47] For example, in Alabama, a court-ordered change in a county electoral system originated a new lawsuit affecting many of its local jurisdictions. The prospects for maintaining at-large elections were very low. Thus, city councilmen could react with some anticipation to protect their seats.

[48] I pick the fourth lag because it is were the effects become significant for the actual distribution of changes.

results. As exposed in section 3, it could be that the rise in public expenditures and tax collection is driven either by a common-pool problem or by the legislative power that black communities obtain with single-districts. In the following sections I will show evidence against the first channel and consistent with the second one.

5.2 Are the Effects Stronger the More Districts Are Created? Testing the Common-Pool Hypothesis

Weingast et al. (1981) predict that public spending increases with the number of electoral districts that a political jurisdiction has. Moreover, the increase is positively monotonic in the number of districts. Thus, if the reason why we observe a rise in local public good spending and tax collection is due to a common-pool problem, we should see that the effects are stronger the more districts the city created. I test that hypothesis by focusing only on cities that changed their electoral rule, and including dummies for the number of districts created. The specification is:

$$y_{it} = \alpha + \beta_1 Change_{it} + \sum_{k \in G} \beta_{2k} Change_{it} * \#Districts_{itk} + \gamma_t X_{i1970} + \delta_i + \theta_t + \mu_{it} \quad (3)$$

$\#Districts_{itk}$ is a dummy equal to 1 if the city has k districts. G refers to the set of cities that created 5 or more districts grouped in the following way, $G = \{5-6\, districts, 7-8\, districts, 9\, or\, more\}$. Thus, I include three groups in the regression, and its coefficients have to be interpreted relative to the effect on cities that created 4 or less districts. In figures 9a to 9e I plot the coefficients β_{2k}. The figures do not show a positive monotonic relationship between changes in public spending, public employment, taxation, fire spending or fire protection employment with respect to the number of districts. Only for tax collection, cities that create 7-8 districts have larger increases, but the coefficient drops for those

cities with 9 or more districts. Overall, the results point to a similar effects regardless of the number of districts created. These findings are against the increases happening through a common-pool channel. In the next section, I provide further evidence against that mechanism and in favor of the rise being caused by the legislative power that blacks enjoy with single-districts.

5.3 Who Benefits? Evidence from Housing Values, Rents, Public Workers, Sorting, and Crime

I have shown that changes towards single-districts elections increase local public good provision and tax collection. However, such increases might not benefit blacks even after they gained representation, if those new local public goods are not targeted to them. Since the public finance data is at the city level, it does not allow to test for changes in its distribution within the city.

Nevertheless, if single-districts representation benefited mostly blacks we should observe further changes in variables like house values, rents, the fraction of black public workers, the fraction of black population in the city, and a decrease of murders of black people. If black representatives were able to influence policy-making, more blacks should move to the city, maybe attracted by the improved local public goods, or because they found a job in the public sector. Very important might have been the ability of black local politicians to provide black communities with public jobs. Button (1989) explains how black representatives encouraged minority members to apply for public jobs in the city, specially in the fire and police departments.

Changes in the population should also be reflected in house values and rents as long as the housing supply is not flexible enough to match the demand. Note also, that if the increased fire protection was mostly targeted to black neighborhoods, it could also capitalize into house values and rents. It is beyond the scope of this paper to test for the exact causal chain of effects. Moreover, they are likely to be intertwined, making identification very difficult. Instead, I will focus on the

reduced form effect of a change in electoral in each of these variables.

I use decadal data from the US Census to learn about who benefits from single-districts representation. I first difference the data , and look for the effects with different window sizes. I first pool all decades, and then show results for 10, 20, and 30 years differences. I increase the window of time and not focus only on short-run effects because it is likely that such distributional effects take time to happen. This comes with a caveat, as I increase the window of time, it is more likely that the coefficient captures the effects of other changes on cities' house values and rents, as well as on public workers and sorting behavior. However, all the effects appear in the specification in which I pool all decades and the one for a 10 year window, providing supportive evidence of a causal interpretation. The more long-run estimates can be interpreted as suggestive evidence of the overall effect of guaranteeing minority representation on house values and rents, the fraction of black and white public workers, and the fraction of population of each ethnicity. The specification is as follows:

$$\triangle y_{it} = \beta_1 \triangle Change_{it} + \gamma \triangle X_{it} + \triangle \mu_{it} \tag{4}$$

dependent variables are the first difference of the log of black and white median house value, log of mean black and white rent, the fraction of public workers of each ethnicity who live in the city, and the fraction of people of each race living in the city. For the variables in logs, the coefficients have to be interpreted as the effects on the growth rate. For those dependent variables that are fractions, the coefficients are the effect of the change on the fraction of public workers or population of each race.

Table 7 shows the coefficients of *Change* for each dependent variable. Cities that changed towards single-distrcts experienced a 2.42% increase in the fraction of black public workers in the short-run (10 years), and a decrease in the fraction of public workers of white and other ethnicities. Results are similar for the fraction of each population group: the fraction of blacks increases by 2.77%. The growth rate of the black median house increases by 5.1%, whereas the coefficient for white houses is negative and not significant. For black and white rents, both coefficients

are positive and significant, but that of black rents is significantly larger. The results are very similar when I pool all decades.

With respect to longer windows of time, the coefficients have the same sign and remain significant. The effects in the long-run (30 years) indicate an increase in the fraction of black public workers of 6% and of 8% for the fraction black citizens. Regarding the growth rate of black house values, it increases by 16%. The growth rate of white house values is also positive and significant in the long-run, but smaller than that for blacks. The coefficient for black rents remains similar indicating an increase of 31.4% increase in the growth rate of black rents, whereas that for white rents is of 15.3%.

It remains to know whether black representation had any effect on crime. Black communities could have also benefited from the change towards single-districts if their neighborhoods experienced a decline in crime, both as victims and offenders. I run specification 1 using as dependent variables the number of homicides per capita in each city, as well as the number of homicides differentiating between the race of both the victim and the offender.

Results are shown in tables 8 and 9. Cities that changed their electoral rule did not experience any increase in the overall number of murders, and murders in which the victim was white. They did however experience increases in murders for which the victim was black (though the coefficient is not significant when I do not include a trend). Results in table 9 are consistent with those just described. There are no increases for cases when the victim was white, both for when the offender was either white or black. The rise in homicides in which the victim was black is driven by murders in which the offender was also black. As before, though, the coefficient is not significant when the specification does not include a trend. Thus, results do not suggest that police spending was reallocated between districts. The absence of drops in the number of black homicides could reflect the limited ability to bargain that blacks had given their minority condition. Also, the increase in black homicides could reflect the increase in black population in cities that enacted single-districts.

The results show evidence consistent with black representatives being able to

influence some policy decisions and benefit black communities. Cities with single-districts experience higher increases in black public workers and population, as well as on black house values and rents. This is consistent with the blacks exploiting their new bargaining position in the council, despite its condition of minority, as predicited by legislative bargaining models. It is also consistent with historical evidence that elected black officials had a pro-bargaining attitude despite the the history of discrimination in the US South (Lawson, 1985). There is though no evidence of drops in blacks homicides, but a positive increase in cities that adopted single-districts.

5.4 Heterogeneity

In this section I will show that the effects are of similar size for all cities no matter what was their level of previous discrimination. In order to do that, I will use a variable that proxies for the level of hostility that blacks faced in each city: the support that George Wallace obtained in the 1968 presidential election at the county level when he ran for president with a segregationist platform. The specifications are as follows:

$$y_{it} = \alpha + \beta_1 Change_{it} + \sum_{k=20}^{90} \beta_{2k} Change_{it} * WallaceSupport_{ck} + \gamma_t X_{i1970} + \delta_i + \theta_t + \mu_{it} \tag{5}$$

where $WallaceSupport_{ck}$ is a dummy equal to 1 if in the county c Wallace's electoral support was between k and $k-10$ of the % of votes casted. I omit category $k = 10$. Thus, all the coefficients β_{2k} show the effect of the interaction $Change_{it} * WallaceSupport_{ck}$ with respect to those places where support for Wallace was between 0% and 10% of the votes. In both specifications I cluster the standard errors at the county level. Significance does not change if I cluster at the city level.

Graphs 10a-10e show the ρ_{2k} coefficients and its 95% confidence intervals

for the five main dependent variables, LPG, $Local\,taxes$, $LPG\,emp.$, $Fire$ and $Fire\,Emp.$ There are no significant effects for $Local\,Taxes$. For the case of LPG and $LPGemp.$, the graphs show significantly larger effects where most voters supported Wallace, but the evidence is not as clear-cut for $Fire$ and $Fire\,Emp.$. Also, for municipalities in counties where Wallace obtained between 10 and 20% of the votes, the effect is significant when the dependent variable is LPG. Overall, the evidence points that the effects on spending and taxes are of equal size no matter the past level of discrimination in the city, with the effects being only weakly stronger for places that had more discrimination. This highlights that the court-orders were effective in benefiting black neighborhoods even in the most pro-segregation areas.[49]

6 Conclusions

This paper measures the effects of the creation of a national jurisprudence, together by courts and the legislative body, to outlaw discriminatory election methods. Section 2 of the VRA prohibited electoral discriminatory practises on the basis of race. This triggered a series of court-orders that forced cities to change their election systems. I exploit these changes to study the effects on local public goods, taxation, house values and rents, public workers, population sorting, and crime. The main contribution of this paper is to show how the court system affects economic outcomes through minority voting rights protection. While the effects of voting rights statutory law have been studied (Husted and Kenny, 1997; Cascio and Washington, 2014; Naidu, 2012), the impact on economic outcomes of voting

[49] Graphs 11a to 11e show a robustness check of the heterogeneity analysis but in that case using the number of lynchings in each county between 1900 and 1930 as a proxy for discrimination. Results are very similar: the effects on spending and taxes are evenly distributed for all levels of past discrimination. The only difference is that for tax collection the effect is stronger in places that suffered more discrimination when measured by lynchings, which is not the case when I proxy discrimination with % of Wallace's support.

rights protection through court decisions is a topic that deserves further study.

I have focused on one specific discriminatory electoral method, at-large elections, and its alternative, single-district elections. The first results show that after the enactment of single-districts local public good spending (mainly fire protection spending) and tax collection increased. Such finding is contrary to what we would have expected from an increase in ethnic diversity in the city council: if racial heterogeneity brings disagreement over public goods, there should have been a decrease in spending and taxation (Alesina et al., 1999). Very little is known about how the negative effects of ethnic heterogeneity documented in the literature (Alesina and LaFerrara, 2005; Alesina et al., 1999; Easterly and Levine, 1997) might be mediated through institutions. This paper suggests that public good underprovision might be related to a lack of representation of minorities, and that the lack of public goods can be alleviated by guaranteeing their representation.

A second set of results shows that blacks are likely to benefit from the changes towards non-discriminatory electoral methods: the fraction of black public workers and black citizens increases at the expense of whites and other minorities after a change towards single-districts. In addition, the growth rate of black home values and black rents increases more than that of whites after a change in electoral rule. These findings are consistent with models of legislative bargaining (Baron and Ferejohn, 1989; Persson and Tabellini, 2000). If blacks are among the less powerful groups in the city council, they will form part of minimum winning coalitions because their support is the easiest to obtain. Thus, we should expect that the changes in the public budget and public jobs are targeted to black communities. Historical sources that document the pragmatism of local black politicians, who saw politics as the art of the possible, and were ready to bargain with whites, also support this point (Lawson, 1985). At the same time, this set of findings discards that the increases in local public goods and taxation arise from a common-pool problem (Weingast et al., 1981), because such framework predicts increases for all districts. I provide further evidence against a common-pool channel by showing that the effects are not stronger the more districts are created.

Finally, I show that the effects happen for all cities independently of their

level of past discrimination. Indeed, the evidence suggests that the effects are weakly stronger for places that suffered more discrimination. Given past levels of institutional persistence in the US South (Valelly, 2004; Acemoglu and Robinson (2006 and 2008), this and the previous findings underscore the relevance of the common law system in protecting minority voting rights.

Further work could focus on other discriminatory electoral devices other than at-large elections (Kousser, 1984), or on its effects on other levels of government (i.e., school districts. NCVR (2006)). Likewise, future research could concentrate on understanding if federal interventions to improve minorities' welfater through transfer systems might not succeed (Cascio et al., 2013) because of the leeway that local institutions have in allocating federal grants.

References

[1] Acemoglu, D. 2005. "Constitutions, Politics, and Economics: A Review Essay on Persson and Tabellini's *The Economic Effects of Constitutions*.". *Journal of Economic Literature*. 43: 1025-1048.

[2] Acemoglu, D. and J. Robinson. 2006. "De Facto Political Power and Institutional Persistence.". *American Economic Review.* 96 (2): 326-330.

[3] Acemoglu, D. and J. Robinson. 2008. "Persistence of Power, Elites and Institutions.". *American Economic Review*. 98 (1): 267-293.

[4] Acemoglu, D. and J. Robinson. 2012. Why Nations Fail. New York: Crown Business.

[5] ACIR. "Tax and Expenditure Limits on Local Governments". ACIR, 1995.

[6] Albalate, D; G. Bel and F. Elias. 2012. "Institutional Determinants of Military Spending.". *Journal of Comparative Economics*. 40 (2): 279-290.

[7] Alesina, A; R. Baqir and W. Easterly. 1999. "Public Goods and Ethnic Divisions.". *Quarterly Journal of Economics*. 114 (4): 1243-84.

[8] Alesina, A. and E. La Ferrara. 2005. "Ethnic Diversity and Economic Performance". *Journal of Economic Literature*. 43 (September): 762-800

[9] Almond, D; K. Chay and M. Greenstone. "Civil Rights, the War on Poverty, and Black-White Convergence in Infant Mortality in the Rural South and Mississippi.". *American Economic Review*. Forthcoming.

[10] Amy, D. 1993. Real Choices/ New Voices. The Case for Proportional Representation Elections in the United States. NY: Columbia University Press.

[11] Austen-Smith, D. and J. S. Banks. 1988. "Elections, Coalitions, and Legislative Outcomes.". *American Political Science Review*. 82(2): 405-422.

[12] Avraham, R; L. Dafny and M. Schanzenbach. 2010. "The Impact of Tort Reform on Employer-Sponsored Health Insurance Premiums.". *Journal of Law, Economics, and Organization*. December: 1-30.

[13] Banzhaf, H. and R. Walsh. 2008. "Do People Vote with Their Feet? An Empirical Test of Tiebout's Mechanism.". *American Economic Review*. 98 (3): 843-63.

[14] Baqir, R. 2002. "Districting and Government Overspending.". *Journal of Political Economy*. 110 (6): 1318-1354.

[15] Baron, D. and J. Ferejohn. 1989. "Bargaining in Legislatures.". *American Political Science Review*. 83 (4): 1181-1206.

[16] Besley, T. and S. Coate. 2003. "Centralized versus Decentralized Provision of Local Public Goods: A Political Economy Approach.". *Journal of Public Economics*. 87: 2611-37.

[17] Besley, T.; R. Pande; L. Rahman and V. Rao. 2004. "The Politics of Public Good Provision: Evidence from Indian Local Governments.". *Journal of the European Economic Association*. 2 (2-3): 416-426.

[18] Boyle, J. and D. Jacobs. 1982. "The Intracity Distribution of Services: A Multivariate Analysis.". *American Political Science Review*, 76 (2): 371-79.

[19] Bradbury, K; C. Mayer and K. Case. 2001. "Property Tax Limits, Local Fiscal Behavior, and Property Values: Evidence from Massachusetts under Proposition $2\frac{1}{2}$.". *Journal of Public Economics*, 80: 287-311.

[20] Bradford, D. and W. Oates. 1971a. "The Analysis of Revenue Sharing in a New Approach to Collective Fiscal Decisions.". *Quarterly Journal of Economics*, 85 (3), pp. 416-39.

[21] Bradford, D. and W. Oates. 1971b. "Towards a Predictive Theory of Intergovernmental Grants.". *American Economic Review Papers and Proceedings*, 61 (2), pp. 440-48.

[22] Brueckner, J. and M. Joo. 1991. "Voting with Capitalization". *Regional Science and Urban Economics*, 21: 453-467.

[23] Button, D. 1989. Black and Social Change. NJ: Princeton University Press.

[24] Cascio, E; N. Gordon; E. Lewis and S. Reber. 2010. "Paying for Progress: Conditional Grants and the Desegregation of Southern Schools.". *Quarterly Journal of Economics.* 125 (1): 445-82.

[25] Cascio, E; N. Gordon and S. Reber. 2013. "Local Responses to Federal Grants: Evidence from the Introduction of Title I in the South.". *American Economic Journal: Economic Policy.* 5 (3): 126-159.

[26] Cascio, E. and E. Washington. 2014. "Valuing the Vote: The Redistribution of Voting Rights and State Funds After the Voting Rights Act of 1965". *Quarterly Journal of Economics.* Forthcoming.

[27] Charles, G. and L. Fuentes. 2006. "Rethinking Section 5 of the Voting Rights Act,", in The Future of the Voting Rights Act, ed. Epstein, D; R. Pildes; R. De la Garza and S. O'Halloran. New York: Russell Sage Foundation.

[28] Chattopadhyay, R. and Esther Duflo. 2004. "Women as Policy Makers: Evidence from a Randomized Policy Experiment in India.". *Econometrica.* 72(5): 1409-1443.

[29] Clubb, J. M.; W. H. Flanigan and N. H. Zingale. Electoral Data for Counties in the United States: Presidential and Congressional Races, 1840-1972. ICPSR08611-v1. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2006-11-13. doi: 10.3886/ICPSR08611.v1

[30] Currie, J. and W. B. MacLeod. 2008. "First Do No Harm? Tort Reform and Birth Outcomes". *Quarterly Journal of Economics.* 123 (2): 795-930.

[31] Cutler, D.; E. Glaeser and J. Vigdor. 1999. "The Rise and Decline of the American Ghetto". *Journal of Political Economy.* 107 (3): 455-506.

[32] Davidson, C. 1994. "The Recent Evolution of Voting Rights Law Affecting Racial and Language Minorities.". In Quiet Revolution in the South: the Impact of the Voting Rights Act 1965-1990, ed. Davidson, C. and B. Grofman. Princeton: Princeton University Press.

[33] Davidson, C. and B. Grofman. 1994a. Quiet Revolution in the South: the Impact of the Voting Rights Act 1965-1990. Princeton: Princeton University Press.

[34] Davidson, C. and B. Grofman. 1994b. "The Effect of Municipal Election Structure on Black Representation in Eight Southern States.". In Quiet Revolution in the South: the Impact of the Voting Rights Act 1965-1990, ed. Davidson, C. and B. Grofman. Princeton: Princeton University Press.

[35] Duflo, E. 2005. "Why Political Reservations?". *Journal of the European Economic Association.* 3 (2-3): 668-78.

[36] Easterly, W. and R. Levine. 1997. "Africa's Growth Tragedy: Policies and Ethnic Divisions". *The Quarterly Journal of Economics.* 112 (4): 1203-1250.

[37] Epple, D. and A. Zelenitz. 1981. "The Implications of Competition among Jurisdictions: Does Tiebout Need Politics?". *Journal of Political Economy.* 89 (6): 1197-1217.

[38] Epple, D. and T. Romer. 1991. "Mobility and Redistribution". *Journal of Political Economy*. 99 (4): 828-858.

[39] FEMA. 2009. Fire in the United States, 2003-2007.

[40] Ferreira, F. and J. Gyourko. 2009. "Do Political Parties Matter? Evidence from US Cities.". *Quarterly Journal of Economics*. 124 (1): 399-422.

[41] Ferreira, F. and J. Gyourko. 2011. "Does Gender Matter for Political Leadership? The Case of US Mayors.". WP.

[42] Fischel, W. 1992. "Property Taxation and the Tiebout Model: Evidence for the Benefit View from Zoning and Voting.". *Journal of Economic Literature*. 30 (March): 171-77.

[43] Fischel, W. 2001. "Municipal Corporations, Homeowners, and the Benefit View of the Property Tax.". In Property Taxation and Local Government Finance: Essays in Honor of C. Lowell Harriss. ed. W. Oates. Cambridge, Mass.: Lincoln Institute for Land Policy.

[44] Gerber, E. and D. Hopkins. 2011. "When Mayors Matter: Estimating the Impact of Mayoral Partisanship on City Policy.". *American Journal of Political Science*. 55 (2): 326-339.

[45] Gerken, H. 2001. "Understanding the Right to an Undiluted Vote.". *Harvard Law Review*. 114 (6): 1663-1743.

[46] Glaeser, E. 2013. "Urban Public Finance.". In Handbook of Public Economics, ed. Auerbach, A. J.; R. Chetty; M. Feldstein and E. Saez. North Holland.

[47] Glaeser, E.; J. Scheinkman and A. Shleifer. 1995. "Economic Growth in a Cross-Section of Cities.". *Journal of Monetary Economics*. 36: 117-143.

[48] Gyourko, J.; A. Saiz and A. Summers. 2008. "A New Measure of the Local Regulatory Environment for Housing Markets: the Wharton Residential Land Use Regulatory Index.". *Urban Studies*. 45 (3): 693-729.

[49] Hawkins v. Town of Shaw Mississippi, 437 F. 2d 1286 (1971).

[50] Husted, T. and L. Kenny. 1997. "The Effect of the Expansion of the Franchise on the Size of Government.". *Journal of Political Economy.* 105 (1): 54-82.

[51] Katz, L. and K. Rosen. 1987. "The Interjurisdictional Effects of Growth Controls on Housing Prices.". *Journal of Law and Economics.* 30 (1): 149-60.

[52] Knight, B. 2002. "Endogenous Federal Grants and Crowd-out of State Government Spending: Theory and Evidence from the Federal Highway Aid Program.". *American Economic Review.* 92 (1): 71-91.

[53] Kousser, J. M. 1984. "The Undermining of the First Reconstruction: Lessons for the Second.". In Minority Vote Dilution, ed. Chandler Davidson. Washington, D.C.: Howard University Press.

[54] Kouser, J. M. 1999. Colorblind Injustice. Minority Voting Rights and the Undoing of the Second Reconstruction. The University of North Carolina Press.

[55] Ladd, H. and J. Yinger. 1989. America's Ailing Cities: Fiscal Health and the Design of Urban Policy. Baltimore: John Hopkins University Press.

[56] Lijphart, A. 1986. "Proportionality by Non-PR Methods: Ethnic Representation in Belgium, Cyprus, Lebanon, New Zealand, West Germany and Zimbabwe.". In Electoral Laws and Their Political Consequences, ed. Grofman, B. and A. Lijphart. New York: Agathon Press.

[57] Miles, T. 2000. "Common Law Exceptions to Employment at Will and US Labor Markets.". *Journal of Law, Economics, and Organization.* 16 (1): 74-101.

[58] Miguel, E. and M. K. Gugerty. 2005. "Ethnic Diversity, Social Sanctions, and Public Goods in Kenya.". *Journal of Public Economics.* 89: 2325-2368.

[59] Mobile v. Bolden, 4 US 55 (1980).

[60] Naidu, S. 2012. "Suffrage, Schooling, and Sorting in the Post-Bellum US South". WP.

[61] NCVR. Protecting Minority Voters: The Voting Rights Act at Work 1982-2005. Lawyers' Committee for Civil Rights Under Law.

[62] Oates, W. 1969. "The Effects of Property Taxes and Local Public Spending on Property Values: An Empirical Study of Tax Capitalization and the Tiebout Hypothesis.". *Journal of Political Economy.* 77, 957-971.

[63] Oates, W. 2005. "Property Taxation and Local Public Spending: the Renter Effect.". *Journal of Urban Economics.* 57: 419-31.

[64] Pande, R. 2003. "Can Mandated Political Representation Increase Policy Influence for Disadvantaged Minorities?". *American Economic Review.* 93 (4): 1132-1151.

[65] Parker, F. 1990. Black Votes Count. Political Empowerment in Mississippi after 1965. Chapel Hill and London. The University of North Carolina Press.

[66] Persson, T. and G. Tabellini. Political Economics: Explaining Economic Policy. Cambridge: MIT Press.

[67] Thornburg v. Gingles, 478 US 30 (1986).

[68] Tiebout, C. 1956. "A Pure Theory of Local Expenditures". *Journal of Political Economy.* 64 (5): 416-424.

[69] Tracy, J; H. Schneider and S. Chan. 1999. "Are Stocks Overtaking Real Estate in Household Portfolios?" *Federal Reserve Bank of New York. Current Issues in Economics and Finance.* 5 (5): 1-6.

[70] Trebbi, F.; P. Aghion and A. Alesina. 2008. "Electoral Rules and Minority Representation in US Cities.". *Quarterly Journal of Economics.* 123 (1): 325-357.

[71] US House. Subcommittee on the Constitution of the Commitee on the Judiciary. *Examine the Impact and Effectiveness of the VRA*, Hearing, October 18, 2005 (Serial No. 109-70). Washington: Government Printing Office, 2005.

[72] Valelly, R. M. 2004. The Two Reconstructions. The Struggle for Black Enfranchisement. Chicago: The University of Chicago Press.

[73] Weingast, B.; K. Shesple and C. Johnsen. 1981. "The political economy of benefits and costs: a neoclassical approach to distributive politics.". *Journal of Political Economy.* 89 (4): 642-664.

[74] Welch, S. 1990. "The Impact of At-Large Elections on the Representation of Blacks and Hispanics.". *Journal of Politics.* 52: 1050-76.

[75] White v. Regester, 412 US 755 (1973).

[76] Zimmer v. McKeithen, 485 F. 2d 1297 (1973).

A Figures

Figure 1: Map of Electoral District with At-Large Elections in Florence, AL

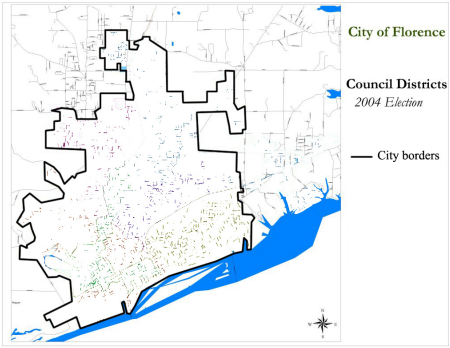
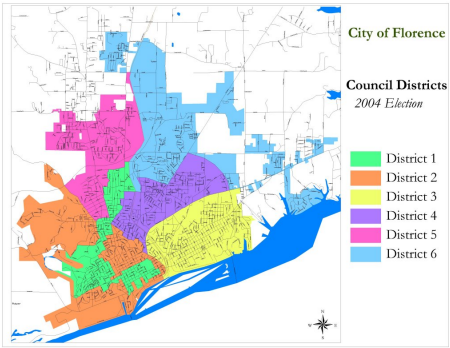
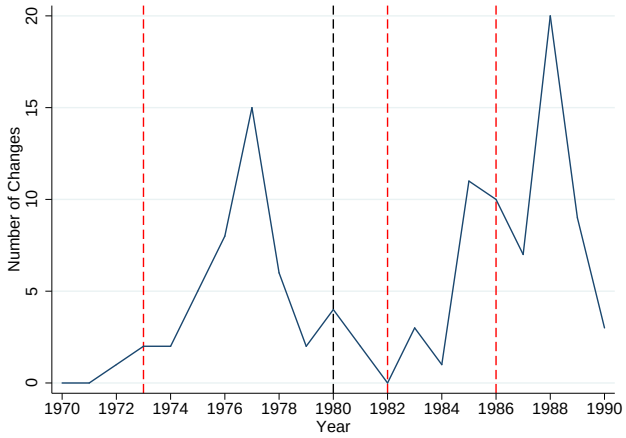


Figure 2: Map of Electoral Districts with Single-District Elections in Florence,AL



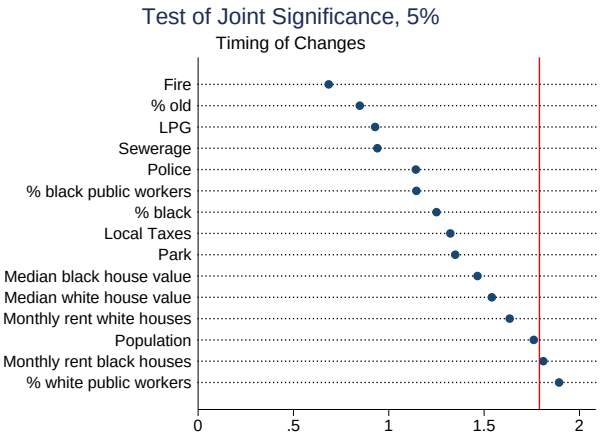
Notes: Figures 1 and 2 show the electoral constituencies when elections are at-large (figure 1) or organized through single-districts (figure 2). The maps are from one of the municipalities in the sample, the city of Florence, Alabama. When elections are at-large, the whole city is the electoral district. In contrast, when elections are organized by single-districts, the city is partitioned in many electoral constituencies. For the case of Florence, district 1 concentrates most of 19.4% of black population in the city, so that the black community could have a representative.

Figure 3: Number of Changes Towards Single-Districts by Year



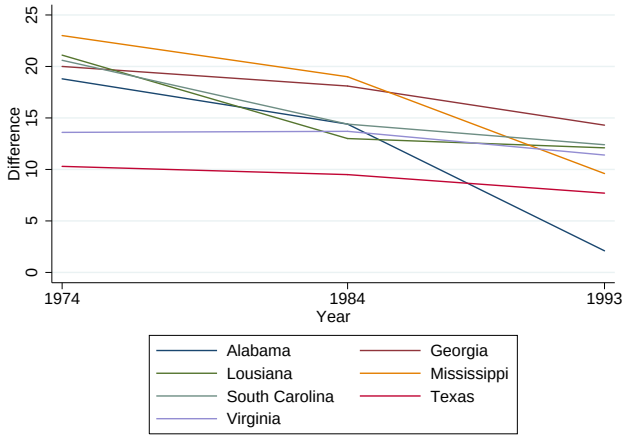
Notes: The red dashed lines show years in which a landmark case for the anti-vote dilution jurisprudence was ruled. The black line shows the only case that increased the burden of proof that plaintiffs needed to demonstrate in court to ban at-large elections. Each peak of changes happens after each of the three significant anti-vote dilution rulings, whereas the years in which there were barely changes correspond to periods when discriminatory intent had to be proven (1970-1972), or after the negative ruling for anti-vote dilution (1980-1982). Sources are Davidson and Grofman (1994) and International City Managers' Association surveys.

Figure 4



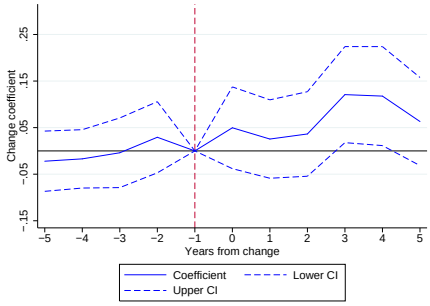
Notes: The figure shows the F statistic of joint significance of the year of change for the main city variables that I use. The red solid line is F's test critical value. For only two dependent variables, the monthly rent paid by blacks and the share of white public workers, the joint test of significance is significant at the 5% level. Overall, there does not appear to be a systematic relationship between the timing of the changes and city characteristics.

Figure 5: Difference % Black Voting-Age Population and % Black Local Elected Officeholders

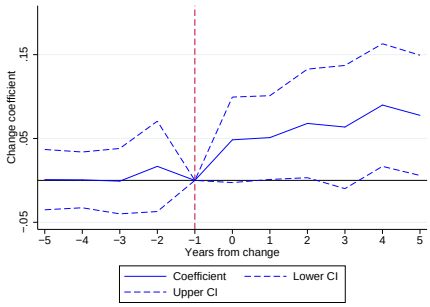


Notes: The figure shows the difference between the fraction of voting-age blacks with respect to the voting-age population and the fraction of black local elected officeholders. The higher the difference, the less are blacks represented in local offices proportionally to the size of its population. We can see a reduction in the difference during the 1970s and 1980s, years after the poll taxes and literacy tests were outlawed, that is related to the creation of single-districts. Source is Valelly (2004).

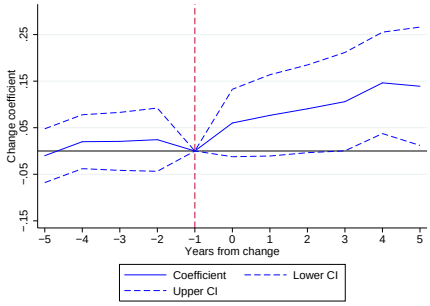
Figure 6: Dynamic effects of changes towards single-district elections



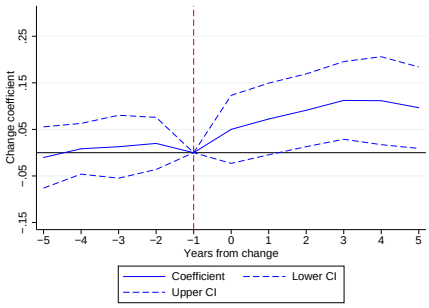
(a) Dependent variable is *LPG Spending*



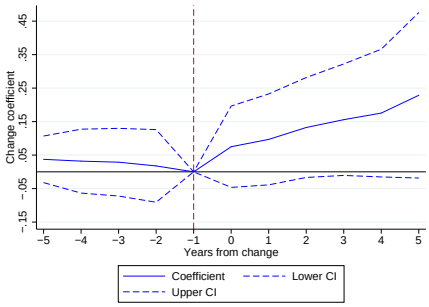
(b) Dependent variable is *LPG employees*



(c) Dependent variable is *Local taxes*



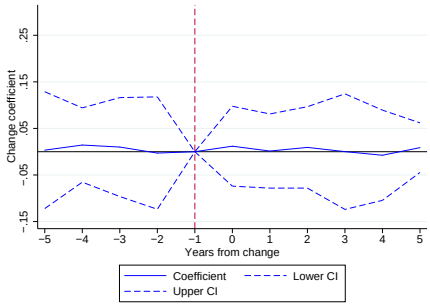
(d) Dependent variable is *Fire Spending*



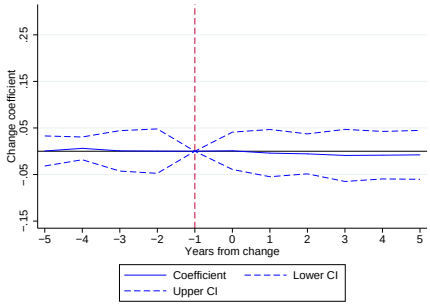
(e) Dependent variable is *Fire employees*

Notes: the graphs show the β_k coefficients of regression 2. I include 5 leads and lags, and omit period -1. Thus, the coefficients reflect the size of the effect relative to a year before the change took place. The figures do not show pre-treatment trends. The effects start the year of the change, and grow over time.

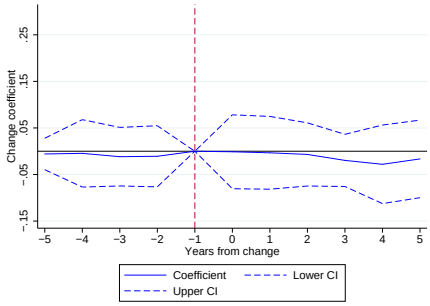
Figure 7: Dynamic effects of changes towards single-district elections (Placebo)



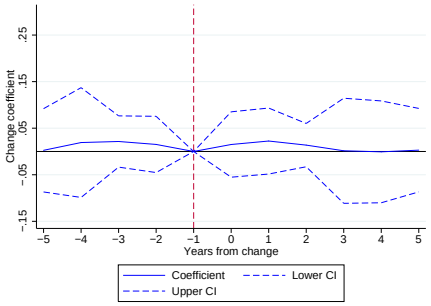
(a) Placebo: dep. variable is *LPG Spending*



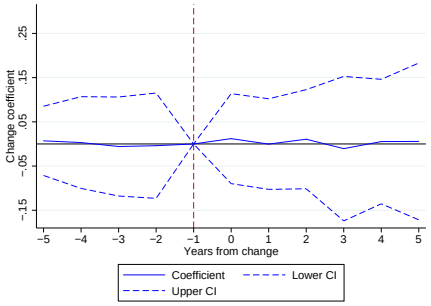
(b) Placebo: dep. variable is *LPG employees*



(c) Placebo: dep. variable is *Local taxes*



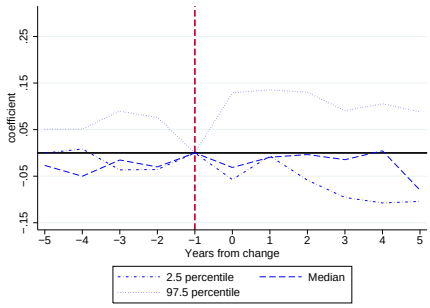
(d) Placebo: dep. variable is *Fire Spending*



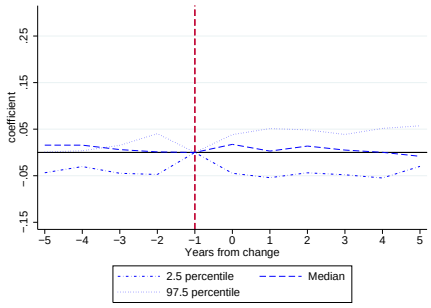
(e) Placebo: dep. variable is *Fire employees*

Notes: the graphs show the β_k coefficients of regression 2 for simulated placebos. I include 5 leads and lags, and omit period -1. Thus, the coefficients reflect the size of the effect relative to a year before the change took place. The placebo coefficients are always around zero, and confirm that the actual effect is driven by the change in electoral rules.

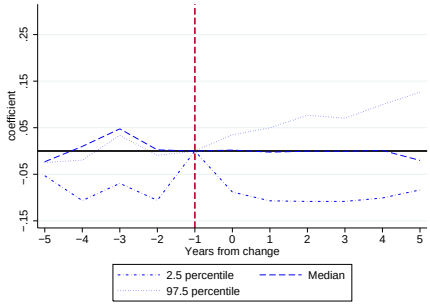
Figure 8: Dynamic Placebo Percentiles



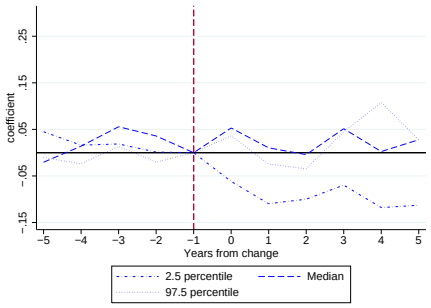
(a) Dep. variable: *LPG*



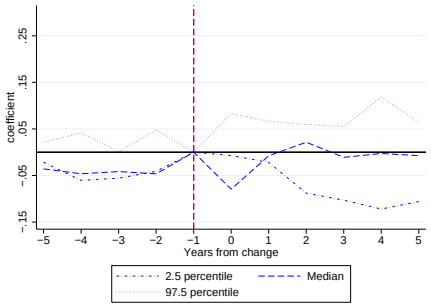
(b) Dep. variable: *LPG Emp.*



(c) Dep. variable: *Local Taxes*



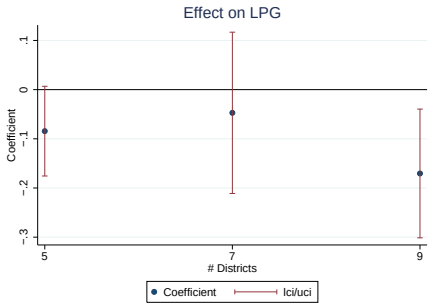
(d) Dep. variable: *Fire*



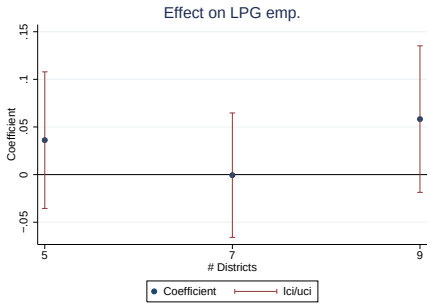
(e) Dep. variable: *Fire Emp.*

Notes: the graph shows the coefficients of three of the 1000 simulated placebos. The dynamic specification is equation 2. I rank the regressions according to their 4th lag coefficient and I pick the coefficients in the 2.5, 50, and 97.5 percentiles..

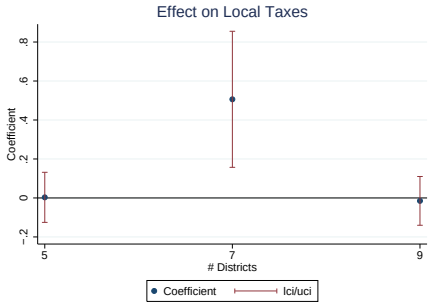
Figure 9: Test of Common Pool Hypothesis



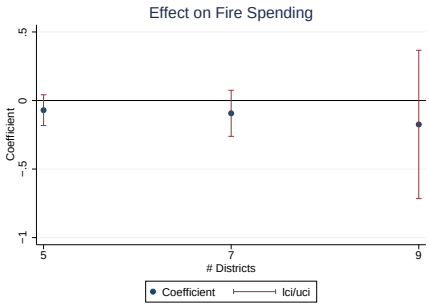
(a)



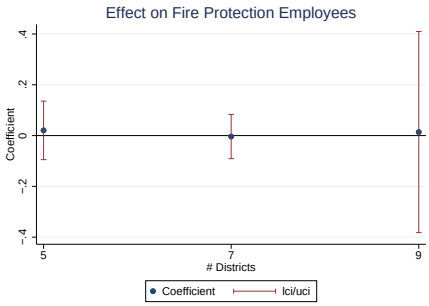
(b)



(c)



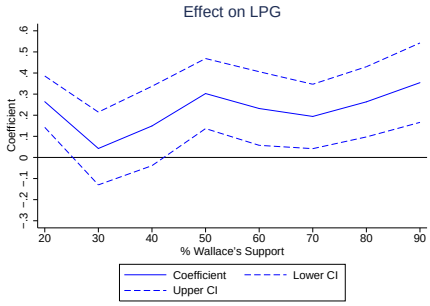
(d)



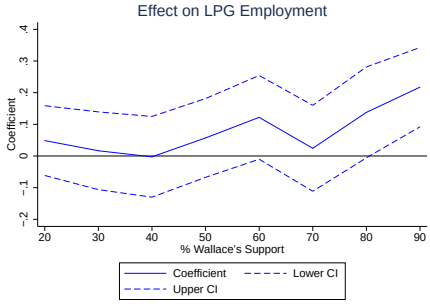
(e)

*Notes: The figure plots the coefficients β_{2k} of regression 3. The sample includes only cities that outlawed at-large elections. The coefficients β_{2k} reflect the change in the outcome variable with respect to cities that created 4 or less single-districts. The results provide evidence against the common-pool hypothesis, that predicted a positively monotonic relationship between spending and the number of districts.

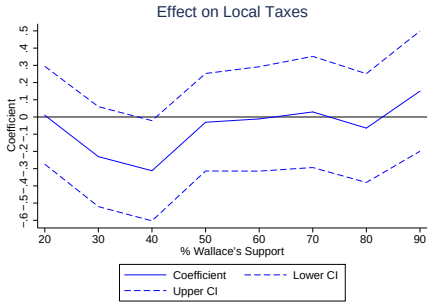
Figure 10: Heterogeneity Analysis: Effects by George Wallace's Vote in 1968



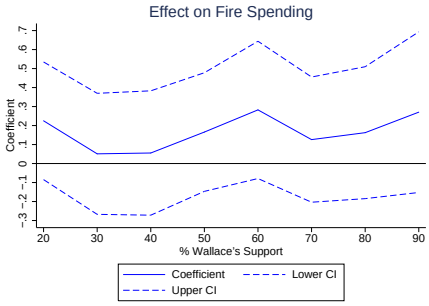
(a)



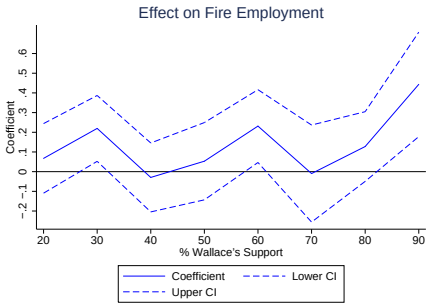
(b)



(c)



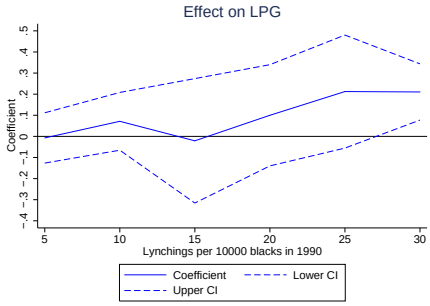
(d)



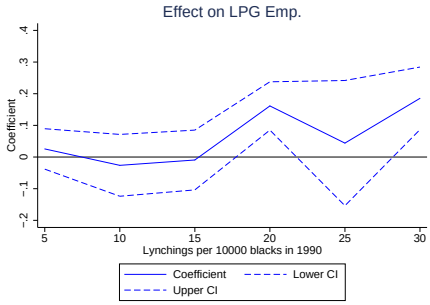
(e)

Notes: the solid line shows the coefficients of the interaction $Change_{it} * WallaceSupport_{ck}$. The specification is equation 5. The coefficient shows the increase in each dependent variable of interest with respect to a city in a county where electoral vote for George Wallace was lower than 10%.

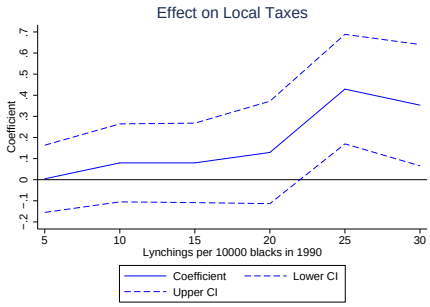
Figure 11: Heterogeneity Analysis: Effects by Number of Lynchings for every 10000 blacks in 1900



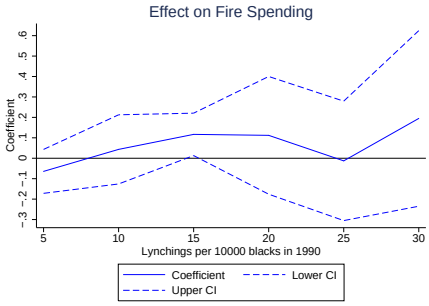
(a)



(b)



(c)



(d)

(e)

Notes: the solid line shows the coefficients of the interaction $Change_{it} * Lynchings_{ck}$. The specification follows equation 5. The coefficient shows the increase in each dependent variable of interest with respect to a city in a county where no blacks were lynched.

B Tables

Table 1: City Budget

	(1) % Budget	(2) $ Per Capita	(3) Ratio Trans./Exp.
Utilities	0.303	615.9	0
	(0.00399)	(12.97)	(0.0)
Police Protection	0.0964	118.8	0
	(0.000817)	(0.884)	(0.0)
Roads	0.0755	91.88	596.1
	(0.000871)	(0.949)	(239.3)
Sewage	0.0619	86.02	0.00207
	(0.00104)	(1.696)	(0.000908)
Fire Protection	0.0652	79.94	0
	(0.000580)	(0.580)	(0.0)
Parks	0.0440	56.38	0
	(0.000531)	(0.708)	(0.0)
Hospitals	0.0268	57.53	8651.2
	(0.00170)	(3.912)	(3131.8)
Housing	0.0261	34.32	36628.3
	(0.000730)	(0.983)	(5109.1)
Observations	3906	3906	3906

Notes: sample of 126 cities between 1970 and 2000. Standard errors in parentheses. The table shows the 8 main spending categories in cities, sorted by expenditure per capita. The first column is the fraction they represent in the budget. The column does not add up to 1 because I am not showing all the budget items. The second column is spending in $ per capita. the last column is the ratio of targeted transfers to each item with respect to the level of spending in each category. For police and fire protection, as well as parks there are no targeted transfers and that is why the table has a 0.

Table 2: Summary Statistics

	Control Group						Treatment Group					
	Year 1970			Year 2000			Year 1970			Year 2000		
VARIABLES	**Mean**	**N**	**sd**	**Mean**	**N**	**sd**	**Mean**	**N**	**sd**	**Mean**	**N**	**sd**
Population	53,722	40	48,229	68,669	40	61,028	65,079	86	115,821	79,043	86	140,570
% Single-district	16.4	40	31.9	16.4	40	31.9	0	86	0	87.8	86	15.6
% older 65	11	40	5.27	13.6	40	4.78	9.61	86	2.82	14.2	86	3.3
% black	7.84	40	5.33	10.3	40	9.31	24.8	86	9.52	32.9	86	14.8
Median white house value	16,938	40	6,461	153,500	40	130,826	14,840	86	3,167	98,837	86	24,703
Median black house value	11,656	40	5,685	123,333	39	111,186	8,279	85	2,224	65,206	85	13,743
Monthly rent black houses	304.9	40	137.8	668.5	40	256.5	179.4	86	49.71	485	86	87.16
Monthly rent white houses	393.1	40	141.9	722	40	300.3	294.5	86	80.93	599.4	86	108.2
% black public workers	6.606	40	7.801	13.12	40	12.28	11.28	86	7.058	30.78	86	16.48
% white public workers	91.94	40	7.903	74.78	40	16.7	88.53	86	7.085	66.03	86	15.8
% Northeast	7.5	40	26.7	7.5	40	26.7	11.6	86	10.8	11.6	86	10.8
% Midwest	37.5	40	49	37.5	40	49	2.33	86	15.2	2.33	86	15.2
% South	30	40	46.4	30	40	46.4	96.5	86	18.5	96.5	86	18.5
% West	25	40	43.9	25	40	43.9	0	86	0	0	86	0
Local Public Good ($ per cap.)	215.5	40	89.88	514.8	40	139.3	178.6	86	121.6	502	86	172
Local Taxes ($ per cap.)	267.2	40	239.4	510.9	40	391	202.8	86	117.8	488.7	86	286.2
Fire Spending per capita ($)	57.5	40	23.17	118.6	40	37.3	43.09	86	18.01	118.9	86	40.85
Police Spending per capita ($)	71.22	40	26.96	183.4	40	57.82	61.07	86	23.66	185.5	86	50.95
Park Spending per capita ($)	39.69	40	25.88	91.6	40	50.85	23.86	86	20.58	87.53	86	64.43
Sewage Spending per capita ($)	47.06	40	58.62	121.2	40	68.15	50.62	86	112.7	110.1	86	106

Notes: the table shows summary statistics both for cities that did not change their electoral rule and for those that had to change it, for the first and last year of the sample. The variable % Single-district is the % of aldermen elected by single-district. Cities in the control group are smaller, had some single-district aldermen in 1970, their proportion of blacks is lower, and are located in the midwest, south and west of the US. Cities in the treatment group have a higher proportion of blacks, and almost all of them are located in the US South. Cities that changed their electoral rule have in general a smaller budget. However, between 1970 and 2000 they converged in terms of spending and tax collection to cities in the control group

Table 3: Results for Local Public Goods and Local Public Goods employment

	(1) LPG	(2) LPG	(3) LPG emp.	(4) LPG emp.
Change	0.0751**	0.0541	0.0572***	0.0412**
	(0.0331)	(0.0328)	(0.0187)	(0.0164)
Population	-0.178**	-0.187	-0.339***	-0.389***
	(0.0807)	(0.128)	(0.0529)	(0.0618)
% old	-0.0716***	-0.0658	-0.0536***	-0.0288
	(0.0226)	(0.0425)	(0.0185)	(0.0291)
% black	-0.0111	-0.00821	0.00773	0.00495
	(0.00894)	(0.0117)	(0.00743)	(0.0111)
Property tax limit	0.00988	0.0124	-0.0167	-0.0110
	(0.0253)	(0.0254)	(0.0135)	(0.0131)
City FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
County trend	N	Y	N	Y
Observations	3906	3906	3485	3485
R^2	0.570	0.606	0.343	0.501

Notes: The table shows the coefficient of the variable of interest, *Change*, for the main dependent variables and for several specifications. All the dependent variables are in logarithm and per capita terms. The first two columns show the results when dependent variable is spending in local public good. Columns 3 and 4 show the results when dependent variable is the number of employees in the local public good sector. Sample is a balanced panel and comprises all cities with at least 2.5% black people or at most 40% black citizens. The results are robust to changing the lower threshold to 5% and to moving the higher threshold up to 50% The lower sample size when the dependent variable is public employees is because cities not always report it. Robust standard errors, clustered at the city level, are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: Results for Local Taxes and Intergovernmental Transfers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Local Taxes	Local Taxes	Federal Transfers	Federal Transfers	State Transfers	State Transfers	Local Transfers	Local Transfers
Change	0.0991**	0.0572*	-0.0908	-0.0904	-0.0315	0.0468	-0.176	-0.222
	(0.0439)	(0.0329)	(0.136)	(0.159)	(0.0594)	(0.0612)	(0.316)	(0.295)
Population	-0.145	-0.299**	-0.200	-0.590*	-0.131	-0.361*	-1.403***	-0.583*
	(0.100)	(0.130)	(0.256)	(0.322)	(0.185)	(0.187)	(0.432)	(0.315)
% old	-0.0610*	-0.107*	0.132	-0.0699	0.130	0.0767	-0.210	0.439*
	(0.0353)	(0.0614)	(0.204)	(0.0830)	(0.0875)	(0.105)	(0.262)	(0.234)
% black	-0.0195	-0.00836	-0.00801	0.00505	-0.0313	-0.0723**	-0.0453	0.286***
	(0.0137)	(0.0192)	(0.0605)	(0.0165)	(0.0254)	(0.0347)	(0.0999)	(0.0429)
Property tax limit	-0.0429**	-0.0193	-0.242	-0.196	0.000197	-0.00110	0.202	0.0192
	(0.0208)	(0.0173)	(0.183)	(0.162)	(0.0538)	(0.0469)	(0.220)	(0.235)
City FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
County trend	N	Y	N	Y	N	Y	N	Y
Observations	3906	3906	1178	1178	3410	3410	620	620
R^2	0.521	0.637	0.360	0.476	0.190	0.327	0.389	0.531

Notes: The table shows the coefficient of the variable of interest, *Change*, for local taxes (property tax and local sales tax) and intergovernmental transfers, and for two specifications. All the dependent variables are in logarithm and per capita terms. Sample is a balanced panel and comprises all cities with at least 2.5% black people or at most 40% black citizens. The results are robust to changing the lower threshold to 5% and to moving the higher threshold up to 50% The different sample sizes are due to cities not always receiving transfers from other levels of government. Robust standard errors, clustered at the city level, are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Results for Disaggregated Categories of Local Public Good

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(A)	Fire	Fire	Police	Police	Sewerage	Sewerage	Park	Park
Change	0.119***	0.104***	0.0389	0.0332	0.168	0.0394	0.0598	0.0120
	(0.0351)	(0.0327)	(0.0260)	(0.0245)	(0.111)	(0.111)	(0.0569)	(0.0434)
Population	-0.184	-0.221	-0.196**	-0.237**	-0.334	-0.669**	-0.0238	0.0102
	(0.113)	(0.161)	(0.0854)	(0.105)	(0.301)	(0.275)	(0.155)	(0.189)
% old	-0.0198	0.0473	-0.0445	-0.0492	-0.0250	0.0504	-0.0739	0.0497
	(0.0240)	(0.0592)	(0.0300)	(0.0442)	(0.0731)	(0.104)	(0.0548)	(0.0582)
% black	-0.00106	-0.0107	-0.00680	-0.0164	-0.0218	0.0483	-0.0104	0.00693
	(0.0129)	(0.0229)	(0.00774)	(0.0117)	(0.0322)	(0.0310)	(0.0230)	(0.0199)
Property tax limit	0.0361	0.0290	0.00313	0.00497	-0.0101	0.0374	0.0374	0.0458
	(0.0222)	(0.0185)	(0.0188)	(0.0170)	(0.0838)	(0.0839)	(0.0432)	(0.0435)
Observations	3813	3813	3906	3906	2418	2418	3782	3782
R^2	0.585	0.656	0.725	0.768	0.217	0.312	0.298	0.426
(B)	Fire emp.	Fire emp.	Police emp.	Police emp.	Sewerage emp.	Sewerage emp.	Parks emp.	Parks emp.
Change	0.0726*	0.0221	0.00740	-0.00318	0.126	0.0870	0.0825	0.0538
	(0.0420)	(0.0314)	(0.0199)	(0.0165)	(0.0819)	(0.0769)	(0.0600)	(0.0526)
Population	-0.455***	-0.415***	-0.342***	-0.424***	-0.0870	-0.265	-0.0131	0.0518
	(0.100)	(0.0763)	(0.0567)	(0.0572)	(0.248)	(0.300)	(0.147)	(0.207)
% old	-0.0738***	-0.0207	-0.0365	-0.0654*	0.0734	0.179	-0.0901*	-0.0792
	(0.0275)	(0.0348)	(0.0295)	(0.0372)	(0.0798)	(0.122)	(0.0519)	(0.113)
% black	0.00631	0.00540	0.00929	-0.00134	-0.0403	0.0336	0.0302	0.0585*
	(0.0147)	(0.0170)	(0.00694)	(0.0103)	(0.0260)	(0.0496)	(0.0209)	(0.0340)
Property tax limit	-0.0541**	-0.0463**	-0.0280**	-0.0181	0.0831	0.0700	-0.0508	-0.0389
	(0.0259)	(0.0229)	(0.0130)	(0.0132)	(0.0507)	(0.0576)	(0.0497)	(0.0505)
Observations	3433	3433	3459	3459	1942	1942	2859	2859
R^2	0.088	0.317	0.387	0.532	0.079	0.336	0.119	0.255

Notes: The table shows the coefficient of the variable of interest, *Change*, for the categories of spending that comprise the definition of local public good (part A) and for the number of employees in each category (part B). All the dependent variables are in logarithm and per capita terms. All columns include city fixed-effects and year fixed-effects. The second column for each variable also includes a county linear trend. Sample comprises all cities with at least 2.5% black people or at most 40% black citizens. The results are robust to changing the lower threshold to 5% and to moving the higher threshold up to 50% Robust standard errors, clustered at the city level, are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Placebo for Local Public Good

	(1) LPG	(2) LPG	(3) Local taxes	(4) Local taxes	(5) LPG emp.	(6) LPG emp	(7) Fire	(8) Fire	(9) Fire emp.	(10) Fire emp.
Mean *Change* coefficient	.0084388	.0111486	.0089469	.0183714	-.0058301	-.0017131	.0083202	.012348	-.0101536	-.0032923
	(.0300369)	(.0310699)	.0215411	.0167613	.0076968	.009229	.0293231	.0267518	.0446924	.0386927
Time-varying controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
City FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
County trend	N	Y	N	Y	N	Y	N	Y	N	Y
Observations	5332	5332	5332	5332	5332	5332				

Notes: The table shows the mean effect of a change in *Change* on LPG (columns 1 and 2), local taxes (columns 3 and 4), LPG employment (columns 5 and 6), fire spending (columns 7 and 8), and fire protection employees (columns 9 and 10). For a sample of cities for which there are not changes in electoral rule (172 cities), I generate a distribution of changes following a similar distribution to the actual distribution of changes. I repeat the procedure 1000 times and for each fake distribution I run the two preferred specifications: with time-varying covariates, city- and time-fixed effects (column 1), and also including a county linear trend (column 2). The results are the same when instead of generating a similar distribution of changes to the actual distribution, I use a uniform distribution to assign changes to cities across years. Robust standard errors, clustered at the city level, are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: Distributional Results

Dependent Variable	All Decades	10 Year	20 Year	30 Year
Frac. Black Pub. Wor.	2.873**	2.42***	4.432***	5.959***
	1.14	0.486	0.29	1.705
Frac. White Pub. Wor.	-0.72	-1.496**	-3.007***	-3.288
	1.224	0.677	0.558	2.481
Frac. Other Pub. Wor.	-2.152**	-0.951**	-1.472***	-2.67
	1.011	0.427	0.473	1.979
Frac. Black Pop.	0.016***	2.773***	4.759***	8.009***
	0.004	0.196	0.209	1.02
Frac. White Pop.	-3.07**	-1.716**	-6.177***	-6.487***
	1.279	0.737	0.46	2.335
Frac. Other Pop.	-0.002	-1.059*	1.459***	-1.522
	0.005	0.627	0.345	2.292
Black Median Value	0.05	0.051**	0.16***	0.159**
	0.047	0.023	0.028	0.071
White Median Value	-0.084*	-0.029	0.055*	0.104*
	0.047	0.022	0.029	0.057
Black Rent	0.259***	0.366***	0.523***	0.314***
	0.054	0.027	0.026	0.066
White Rent	0.181***	0.234***	0.281***	0.153***
	0.036	0.019	0.019	0.045

Notes: The table shows the coefficients of the variable of interest, *Change*, for the main dependent variables used to explore the distributional consequences of changing the electoral rule from at-large to single-districts. The dependent variables are from the US Census and are thus decadal. The specification is in first differences. Column 1 pools data from all decades, and thus captures immediate effects of the change. Columns 2, 3 and 4 explore effects in 10 years, 20 years, and 30 years windows. Robust standard errors, clustered at the city level, are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 8: Number of Homicides

	(1) Murders	(2) Murders	(3) White Victim	(4) White Victim	(5) Black Victim	(6) Black Victim
Change	-0.000142	0.00717	-0.00495	-0.00272	0.00474	0.00969**
	(0.00794)	(0.00829)	(0.00441)	(0.00487)	(0.00493)	(0.00477)
Population	-0.0142	-0.0159	-0.00434	-0.00636	-0.0108	-0.0106
	(0.0127)	(0.0172)	(0.00811)	(0.00935)	(0.00922)	(0.0111)
% old	-0.00205	-0.0188**	0.000256	-0.00657	-0.00175	-0.0107*
	(0.00547)	(0.00897)	(0.00281)	(0.00479)	(0.00317)	(0.00553)
% black	0.00832***	0.00395	0.00315**	0.00223	0.00512***	0.00160
	(0.00279)	(0.00455)	(0.00149)	(0.00290)	(0.00181)	(0.00225)
Property tax limit	0.00326	0.00325	-0.00333	-0.00331	0.00858	0.00856
	(0.0105)	(0.0106)	(0.00648)	(0.00657)	(0.00696)	(0.00704)
City FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
County trend	N	Y	N	Y	N	Y
Observations	3402	3402	3402	3402	3402	3402
R^2	0.173	0.221	0.105	0.141	0.120	0.166

Standard errors in parentheses

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

Notes: The table shows the coefficients of the variable of interest, *Change*, for several measures of the number of murder per capita that happened in each city for every year. The first two columns aggregate all homicides, third and fourth column have as dependent variable the number of murders in which the victim was white. Similarly, columns 5 and 6 are for the case of homicides in which the victim is black. Even numbered columns include a county linear trend. Robust standard errors, clustered at the city level, are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 9: Number of Homicides, by Victim's and Offender's Race

	(1) White Vic.-White Off.	(2) White Vic.-White Off.	(3) White Vic.-Black Off.	(4) White Vic.-Black Off.	(5) Black Vic.-White Off.	(6) Black Vic.-White Off.	(7) Black Vic.-Black Off.	(8) Black Vic.-Black Off.
Change	-0.00481	-0.00408	0.000734	0.00110	0.0000467	0.000697	0.00454	0.00820*
	(0.00333)	(0.00380)	(0.00102)	(0.00101)	(0.000568)	(0.000648)	(0.00420)	(0.00434)
Population	-0.00292	-0.00290	-0.00447**	-0.00619***	-0.00119	-0.000935	-0.00817	-0.00186
	(0.00464)	(0.00636)	(0.00193)	(0.00219)	(0.00125)	(0.00153)	(0.00773)	(0.00991)
% old	0.00134	-0.000768	0.000273	-0.00246**	-0.000430	-0.00175***	-0.000244	-0.00737*
	(0.00194)	(0.00313)	(0.000554)	(0.00105)	(0.000359)	(0.000578)	(0.00229)	(0.00430)
% black	0.00103	0.000417	0.00116***	0.000822*	0.0000629	-0.000189	0.00185	-0.0000900
	(0.00103)	(0.00217)	(0.000313)	(0.000445)	(0.000131)	(0.000210)	(0.00143)	(0.00188)
Property tax limit	-0.00531	-0.00531	0.00236	0.00238	-0.000744	-0.000750	0.00763	0.00755
	(0.00475)	(0.00480)	(0.00221)	(0.00224)	(0.00127)	(0.00129)	(0.00677)	(0.00684)
City FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
County trend	N	Y	N	Y	N	Y	N	Y
Observations	3402	3402	3402	3402	3402	3402	3402	3402
R^2	0.084	0.108	0.037	0.059	0.014	0.034	0.105	0.142

Standard errors in parentheses

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

Notes: The table shows the coefficients of the variable of interest, *Change*, for several measures of the number of murder per capita that happened in each city for every year. I differentiate murders by both the race of the victim and the offender. Even numbered columns include a county linear trend. Robust standard errors, clustered at the city level, are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%