Black Candidates and Black Turnout: A Study of Viability in Louisiana Mayoral Elections*

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Abstract

What effect does candidate race have on co-racial voter turnout? Recent studies have found mixed results, largely because it is difficult to separate the effect of candidate race from other factors that drive voter turnout. We argue that viability is a key element in the theory of turnout among co-racial voters that has been overlooked in the extant literature. We develop a broad-based concept of candidate viability that is dependent on both the candidate but also the electoral environment. To test this hypothesis, we make use of the unique runoff structure of mayoral elections in the state of Louisiana between 1988 and 2011. We argue that runoff elections heighten viability in ways rarely seen in most elections. We find that while there is an effect of candidate race on black turnout in general elections, the effect is much more robust in runoff elections.

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1 Introduction

On Tuesday, November 7, 2006 Cedric Glover was elected the first African American mayor of Shreveport, LA. Glover's election came as a surprise to many who doubted he could gather enough support to win in a southern city where in years past blacks had attempted but failed to gain control of city hall. Glover's victory was ultimately attributed to his campaign's ability to exploit the racial politics of Shreveport through the mobilization of African American voters. The day after the election the local newspaper, *The Times*, surmised that Glover's victory was likely due at least in part to, "Several high-profile get-out-the-vote initiatives targeting black voters in the last week" that "energized Glover's core voter base." Similarly, one local commentator noted that, "Cedric did a stupendous job of turnout... Black turnout is way up from the primary, much closer to proportion," (Mahfoufi November 8, 2006). The election of Shreveport's first African American mayor illustrates a common explanation for black electoral success: that black candidates succeed by mobilizing black voters. This belief, well known among political pundits, has existed for decades as a kind of anecdotally substantiated fact about black political behavior.

The question of how minority candidates influence minority political participation continues to be of deep political significance. One of the intended goals of the 1965 Voting Rights Act was, upon lifting many of the long-standing impediments to voting, that racial minorities would then be free to elect candidates that they believe would best represent their interests. For groups such as African Americans, Latinos and Asian Americans, this includes the election of co-racial and co-ethnic candidates. If minority candidates increase turnout among co-racial voters, the recruitment of minority candidates can serve as way to increase minority political participation. If minority voters do not turnout for co-racial candidates, then other avenues for increasing minority political participation will need to be found.

Here, we argue that missing from much prior work is attention to the role *candidate viability* plays in determining whether minority voters will turnout when there are co-racial

candidates on the ballot. Building upon previous research that examines the correlation between viability and vote choice (Tate 2003; Manzano and Sanchez 2010), in this paper we seek to better isolate the role of viability in co-racial turnout by focusing on runoff elections. Utilizing a unique data set of mayoral elections in Louisiana spanning more than two decades (1988-2011), we capitalize on Louisiana's unique electoral system that often triggers runoff elections. We find support for our hypothesis that candidate viability is a strong predictor of voter turnout. Specifically, in general elections, the effect of black candidates on black voter turnout is weak. In runoff elections, however, the effects of a black candidate are much stronger, suggesting the relationship between candidate race and co-racial voter turnout to be more nuanced than earlier studies argue. We conclude with implications of our research for African American participation more generally.

2 Black Voter Turnout

Over the last twenty-five years, a great deal of research has been devoted to understanding the effect that minority candidates have on minority electoral behavior. This work has been inspired in part by a need to more fully comprehend the impact of the 1964 Voting Rights Act and its subsequent amendments on the electoral behavior of minority citizens. To that end, a number of scholars have sought to empirically test the linkage between the presence of a minority candidate running for office and the turnout behavior of minority voters within that district (Barreto, Segura and Woods 2004; Brace et al. 1995; Gay 2001; Griffin and Keane 2006; Tate 1991, 2003; Voss and Lublin 2001; Washington 2006; Whitby 2007).

Several studies have found a clear correlation between the race of the candidate and co-racial turnout Bobo and Gilliam (1990); Voss and Lublin (2001); Barreto, Segura and Woods (2004); Washington (2006). The dominant structural explanation for this correlation is that when minorities witness co-racial group members pursuing political office, they have a heightened sense of political empowerment (Barreto, Segura and Woods 2004; Bobo and Gilliam 1990; Browning, Marshall and Tabb 1984; Gilliam and Kaufman 1998; Leighley

2001). Some scholars have found strong support for the empowerment theory among black and Latino voters (Barreto, Segura and Woods 2004; Bobo and Gilliam 1990; Browning, Marshall and Tabb 1984; Gilliam 1996; Gilliam and Kaufman 1998; Leighley 2001; Washington 2006). However, Fraga (2016) finds evidence that co-racial turnout is higher, but the estimates for African-Americans are quite small. Sekhon and Titiunik (2012) analyze congressional elections in California redistricting after 2000, and find that, again, co-ethnic representation has little impact on the rates of political participation among Hispanic voters, and conclude that empowerment benefits from majority-minority districting under the VRA is most likely spurious. Finally, Keele and White (2011) examine the effect of being moved to an African-American representing by redistricting in North Carolina and Georgia, matching voters who moved and those that did not. They find that co-racial representation has no effect on the turnout of black voters. As such, the literature currently stands with a set of fairly mixed results in terms of whether a turnout is higher among minority voters when a co-racial candidates is on the ballot.

However, the extant literature has identified two key correlates for African American turnout that will be important observed confounders. The first factor, specific to mayoral elections, is electoral institutions. Two institutions in particular, the city manager form of government and nonpartisan elections, have been viewed as critical determinants of low voter minority turnout (Hajnal and Lewis 2003). In addition, a number of scholars have found that off-cycle elections (i.e. not at the same time as state and federal elections) reduce turnout (Hajnal and Trounstine 2005; Anzia 2011). As Brown-Dean et al. (2015) recently concluded, as overall turnout declines in local elections, the electorate may become less representative of the racial diversity of the community as a whole. A second key confounder is the size of the minority population. As Leighley (2001, p.8) argues: "For minority individuals, the potential benefits of participating are greater as the racial/ethnic group increases in size [or becomes more pivotal] because the group consequently enjoys a higher probability of being successful in its political efforts." More recently, Fraga (2016) corroborates this argument,

concluding from an analysis of congressional primary and general elections that African American (and Latino) turnout is greater when they comprise a larger proportion of the electorate, regardless of the race of the candidate.

3 Viability and Co-racial Turnout

We argue that candidate viability is a key missing element in the theory of co-racial turnout. Theories of candidate viability and voting are well established (Abramowitz 1989), but have rarely been applied to studies of minority candidates. We argue that minority voters will be more likely to turnout when the co-racial candidate is perceived to be *viable*. Next, we outline the role viability plays in minority turnout.

First, following others, we define viability as a property candidates possess, and voters use to gauge electability. Candidate viability includes candidate characteristics such as name recognition, political experience and campaign funds and organization. In short, it signals candidate quality. That is, for many minority voters to vote at higher rates for co-racial candidates, the candidate must signal some level of electability. Several experiments have shown that voters are willing to vote for a less preferred candidate if that candidate has a greater chance of winning based on opinion polls or previous victories (Forsythe et al. 1996; Utych and Kam 2014). Similarly, Jackman and Vavreck (2011) made a connection between racial attitudes and candidate viability, finding that negative racial attitudes among white voters was associated with doubts about President Obama's ability to win in 2008. However, these previous studies did not investigate the consequences for black voters, despite evidence that viability matters to black voters. For example, Williams (1990) analysis of the 1987 JCPS/Gallup poll revealed that while black voters were likely to support black candidates based on race, they were often perceived to be less experienced and less likely to win.

Second, we argue that candidate viability is a context dependent property as it relates to minority turnout. That is, viability only matters to voters when they view the election as competitive. For example, many candidates in majority-minority Congressional districts

win easily. These candidates are viable, but given the lop-sided nature of the election, voters may see little urgency in voting for candidates of this type. In other words, when voters have cues to suggest that a co-racial candidate is likely to win and needs support, they will be more likely to support a co-racial voter. Without a viability cue, racial identification with a minority candidate will not be enough to increase turnout among co-racial voters. As such, we would not designate one as more important than the other, but each is necessary to increase turnout. Moreover, it would also be a mistake to assume that viability operates independently of racial composition of the electoral district. Without a large enough voting bloc, it is unlikely that minority candidates will be present at all, which would render any viability signal from a runoff immaterial. Instead our argument is that district composition is necessary but often not sufficient. That is, voters will also need the viability signal before deciding to turnout in higher numbers.

We argue that the inconclusive results from extant literature are most likely a result of not accounting for viability. That is, viability may be an important mechanism for the causal effect of co-racial candidates, since viability may be an important effect modifier. That is, the effect of co-racial candidates may be present when viability is present, and the effect may be smaller or non-existent when viability is absent. If extant studies find effects, it may be due to the fact that larger effects from races where viability is present are mixed with smaller effects from races where viability is absent. For example, Keele and White (2011) focus only on elections that are uncompetitive and find little in the way of effects. The difficulty is that it can be difficult to isolate races where the candidate is viable and some level of competition exists. However, as we explain next, in Louisiana, we can better isolate the role of viability.

3.1 Isolating Viability Using Runoff Elections

We hypothesize that the trigger of a runoff election serves as a viability cue to voters. A candidate that qualifies for a runoff election has demonstrated that he or she has a greater chance of being elected for office. It signals not only that the candidate has the resources

to qualify for a runoff, but also that minority support may be critical for that candidate to win the election.

Previous research has argued that runoff elections are a good indicator of candidate viability as well. For example, Bullock and Furr (1997) examine more explicitly the idea that runoff elections signal electability, or what we call viability, to voters. They argue that runoff elections should benefit challengers, who are able to capitalize on the longer process of a general-then-runoff election cycle to increase name recognition and mobilize voters. Looking more explicitly at the effects of runoff elections on black voter turnout, Bullock's (1984, p. 244) study of fifty-two Atlanta area elections concludes that "runoffs may induce blacks to bloc vote." Bullock finds that the excitement of the runoff often stimulates higher turnout in the runoff than the general election, particularly if the black candidate leads in the general election, and attributes this higher turnout to both racialized voting patterns, and relatively inattentive voters who sat out the general election and do not rely heavily in race as a cue (see also Bullock and Smith (1990)).

We argue that, on average, a runoff election serves as a viability cue to voters. A candidate that qualifies for a runoff election has demonstrated that he or she has a greater chance of being elected for office. It signals not only that the candidate has the resources to qualify for a runoff, but also that minority support may be critical for that candidate to win the election. Moreover, an African American candidate in the runoff also serves as broader signal of the advantage that the African American racial group has in a particular community. That is, it signals that there is a large enough voting block that the candidate can qualify for the runoff election.

Thus, runoff elections are more likely to contain *both* properties required: candidates that are viable and competitive environments. Of course, this will not be true in all runoff elections with African American candidates, but all other things being equal, runoff elections should be more likely to include viable candidates in a competitive environment. Moreover, we cannot rule out that there are mechanisms besides runoffs that signal to voters that a

candidate is viable. As such, we argue that the viability signal to voters should be higher in runoffs *on average*.

We focus on Louisiana elections for two reasons. First, all elections in Louisiana include the possibility of a runoff. In the first election, all candidates for an office are placed on a single ballot. If one candidate in the primary receives a majority of the vote share, that candidate is the winner and no further elections are held. However, if no candidate receives a majority of the vote, the two candidates with the largest number of votes in the primary election advance to a runoff election, which is typically held between two weeks and a month after the primary. Thus, the electoral system in Louisiana provides us with frequent runoff elections. While other states have similar electoral systems, particularly in the South, Louisiana is the only one that also maintains a candidate database that records basic information on all candidates for all elections. One element of the database is candidate race. This allows us to record the race for candidates across a large number of elections. Taken together, our focus on Louisiana elections allows us a unique opportunity to examine candidate race across a large number of elections with runoffs.

4 Data

Before, we describe the data, we define key concepts. First, our unit of analysis is a municipality *not* candidates. Specifically, $D_i \in \{0, 1\}$ is an indicator of treatment that is 1 if in a mayoral election at least one of the candidates is an African American and 0 otherwise and Y_i records the turnout among African Americans expressed as a percentage for each municipality. We are interested in the average effect of the treatment: $E[Y_i|D = 1] - E[Y_i|D = 0]$.[1] This expression is the average observed difference in turnout across places with at least one African American candidate and places without an African American candidate. We now turn to our data sources.

We created a data set from three different sources maintained by the state of Louisiana.

[1] In the analyses that follow, we estimate average treatment on the treated: $ATT = E[Y_i(1) - Y_i(0)|D_i = 1]$.

First, the state of Louisiana maintains a candidate database. This database contains information on candidates for all state and local elections. The information in the database includes candidate name, address, office, data, sex, and most importantly for our purposes, race. From the database, we extracted all mayoral candidates from 1988 to 2011. We then aggregated this candidate level data to record the number of African American candidates in each mayoral election. While information on race is typically reported, it is at times missing. We found that for all candidates in our time period, information on race was missing 3% of the time. While we could have used an imputation model for the missing race data, instead we use a bounds approach. We generated two alternative treatment measures to assess the effect of missingness. In one, we code all missing data on race to white, and in the second we code all missing data on race to African American. We can then generate estimates for all three treatment indicators. We found the results were not sensitive to which measure we used, so we conclude that missingness on treatment is ignorable.

Next, we merged electoral returns (also maintained by the state of Louisiana) to the candidate database. Once election returns were matched to each candidate in the database, we converted the raw votes into percentages and denoted the candidate's rank in the election outcomes. We ranked candidates in the primary since there are often three or more candidates. Ranking the candidates allows us to know which candidate was in third place for primary elections that led to a runoff election, which becomes an important feature in our research design. We should note that other than race, we have little other information on these candidate since there is little data on mayoral candidates, particularly those that lose elections. As such, we are unable to include data on campaign spending. Moreover, given that the municipality is the unit of analysis, any measure of this type would have to be based on aggregates.

The final part of our data collection consisted of compiling turnout for mayoral elections. The state of Louisiana records precinct level turnout numbers by race and party. These data are online from 1998 to the present. For earlier years, the state has paper records, which

we converted to an electronic format. Next, we matched the precincts to municipalities. In the electoral returns data, results for mayoral elections are reported at the precinct level which allowed us to map which precincts fall within specific city limits. We aggregated the precinct level turnout data for each municipality, which results in a municipal level data set with indicators for whether at least one of the candidates in the mayoral election was black.

For each municipality, we collected measures of population, the percentage of African American residents of voting age, the percentage of residents with a high school degree, the percentage of residents with a college degree, the percentage of residents that were unemployed, the percentage of residents below the poverty line, and median household income. We use 1990 Census data because it may be the case that African Americans move to cities with African American mayors. If so, the percentage of black population may be affected by the treatment. Since the majority of our elections occur after 1990, conditioning on Census data from 1990 should reduce the possibility of bias from conditioning on a post-treatment variable. We exclude New Orleans in any of our analyses, since we found it incomparable to any other city in the state in terms of both population and the portion of African American residents.

Finally we considered whether the structure of municipal government might also be a possible confounder given other past research (Hajnal and Lewis 2003). In general, municipalities in Louisiana have a fairly uniform structure (see the appendix for a more in-depth discussion of local government structure in Louisiana.) There was one aspect of municipal structure worth including in the specification. Most Louisiana municipalities are governed by the Lawrason Act, passed in 1898. The Act requires a mayor-council form of government, and is continued to be used by most small and mid-size cities in Louisiana. The majority of municipalities in Louisiana are governed by the Lawrason Act except for those with a home rule charter. As such, we created a indicator variable for whether a municipality was covered by the Lawrason Act or a home rule charter, and included this variable in our match.

In short, we have a unique data source that allows us to observe candidate race across a

long time period of mayoral elections that are subject to the possibility of runoff elections. It would clearly be advantageous to expand the data collection to other states, but we know of no other state that has a data source of this nature along with the same structure of elections. We now describe the identification strategy that we use.

5 Research Design

In our study, we focus on the link between co-racial candidates and turnout in mayoral elections. Using elections rather than general legislative elections often reveals important dynamics about race and politics (Branton 2009). First, municipal boundaries are generally not subject to manipulation via redistricting. Consequently, they do not suffer from the endogenous selection that legislative districts are subject to, since those districts might be drawn in a way to influence turnout. Second, mayoral candidates may be more visible to voters in ways than U.S. House members as the mayor may receive attention in the local media. Thus it is more likely voters will be aware of candidate race in mayoral elections.

The major challenge in our study is strategic entry by minority candidates. Given the relative absence of racial cross-over voting in Southern, especially in the South, viable black candidates are more likely emerge in places that already have a large, mobilized black base. Recent research has found this to be especially true in the South (Marschall and Ruhil 2006). Strategic behavior by candidates implies that raw comparisons of elections with and without a black candidate are likely to inflate the effect of the black candidate on black turnout because places with low black turnout for other reasons are also less likely to have a black candidate, and vice versa. That is, it might be the case that places with African American candidates are just different from places without these candidates. For example, if African American candidates choose to run in cities with higher than average socio-economic status, which tends to be associated with higher turnout, this term may be positive and so large that the observed difference in turnout may be solely a function of selection bias due to place effects. What we hope to do is isolate the candidate effect from these place effects by

removing the place effects through statistical means of adjustment.

Simple summaries of the data are sufficient to reveal the selection problem. Figure 1 displays box plots for the proportion of voting age African American residents in a municipality for general elections across treatment status. The differences are striking. In races with at least one black candidate, the median percentage of African American residents is 40%. In races without any black candidates, the median percentage of African American residents is typically less than 20% in that town or city. While the medians are clearly different, the overall distributions are also quite distinct. The grey boxes represent the inter-quartile range for each distribution. Here, we find that the inter-quartile ranges do not even overlap. Figure 1 provides compelling evidence that there may be place effects: places with African American candidates may be different from places that do not have African American candidates.

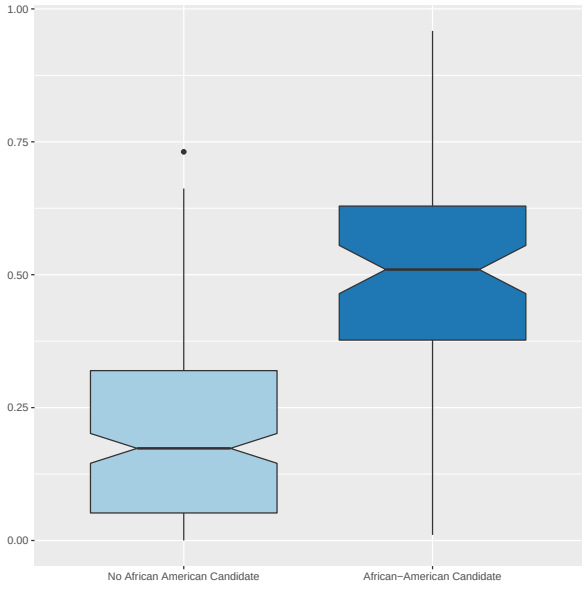

Figure 1: Differences in distribution of African American population for general election races with and without an African American candidate for mayor. Notches indicate median.

5.1 An Observational Study Design

The difficulty we face is that our data only reveal statistical associations. These observed associations contain some unknown mix of causal and non-causal (spurious) components. Informally, a causal effect is identified when we have removed all non-causal components from the observed association (Pearl 2009). For this task, we need an identification strategy: a research design we use to give estimated statistical associations a causal interpretation (see Keele, McConnaughy and White (2012) for an overview of identification strategies used in political science.)

We use the "selection on observables" identification strategy. Under this approach, we assume that treatment selection is based on observable covariates (Barnow, Cain and Goldberger 1980). In short, we must assume that all the differences between treated and control municipalities are observable. As we describe below, we use matching to make treated units similar to control units. By comparing similar cities and towns, we hope to remove the place effects and only observe the candidate effect. However, we can make these places comparable only on observable covariates, we cannot account for differences caused by hidden confounders. Moreover, the selection on observables assumptions is nonrefutable, insofar as it cannot be verified with observed data (Manski 2007). This implies that we cannot test whether there are some unobservable differences across treated and control places.

We could improve on the design in one important way. Runoff elections in Louisiana create a discontinuity for minority candidates. That is, we could find one set of races where minority candidates just barely qualify for the runoff, and compare turnout in these runoff elections to turnout in runoff elections where a minority candidate just barely failed to qualify for the runoff in the first round of voting. Ideally such a design would balance electoral factors such as election year, the number of candidates in the first election, etc. However, we found a very small number of elections in our data where this occurred. There is one alternative way to get as close to this design as possible. For runoff elections with an African American candidate, we could use as controls runoffs that had at least one African

American candidate in the general election round of voting. Using these runoffs as controls should help comparability, since these are elections where an African American at least managed to make the general election ballot. Such places are undoubtedly different from municipalities where African Americans do not appear on the general election ballot at all. As we detail later, this is still a rather small subset of elections. Therefore, we analyze all runoffs as well as those with African American candidates in the first round of voting in two separate analyses.

5.2 Sensitivity Analysis

Given that our identification strategy is selection on observables, we need to probe whether our results would be sensitive to bias from a hidden confounder. That is, while matching will make treated and places comparable on observed covariates, we need to probe if our conclusions could be easily altered if there were a key variable that we failed to match on. In a sensitivity analysis, we seek to quantify the degree to which a key assumption must be violated in order for our inference to be reversed. Note that a sensitivity analysis is identical to a partial identification strategy, and thus is an identification strategy in and of itself (Keele 2005). While there are a number of different methods of sensitivity analysis, we use a method of sensitivity analysis designed for matching estimators and discussed in Rosenbaum (2002, ch. 4).

We first provide a non-technical introduction to this form of sensitivity analysis, before providing technical details. If our identification conditions hold, then quantities such as point estimates and p-values are point identified. Under a sensitivity analysis, we ask how much would these quantities change if a hidden confounder had an effect on the probability of treatment. Specifically, we seek to understand whether a hidden confounder would have to have a large or small effect on the probability of treatment before the point estimate is zero. If a hidden confounder with a small effect could make the treatment effect estimate zero, then our study is not robust. However, if the hidden confounder would need to have a

large effect to explain our results, then our study would be more robust.

Next, we provide more technical details on the sensitivity analysis. We first apply randomization inference as our mode of statistical analysis (see Keele, McConnaughy and White (2012) for a basic introduction to randomization inference.) Under randomization inference, we assume that within matched pairs, receipt of the treatment is effectively random, conditional on the matches. Formally, in our analysis, there are I matched pairs, $i = 1, \ldots, I$, with two subjects, $j = 1, 2$, one treated and one control or $2I$ total subjects. If the j^{th} subject in pair i receives the treatment, write $D_{ij} = 1$, whereas if this subject receives the control, write $D_{ij} = 0$, so $D_{i1} + D_{i2} = 1$, for $i = 1, \ldots, I$. In our study, each matched pair consists of one municipality with at least one African-American candidate and one municipality without any African-American candidates. We assume that matching on observed covariates $\mathbf{x}_{ij}$ made cities and towns the same in their chances of being exposed to the treatment. To formalize this idea, define π_{ij} as the probability that municipality j in pair i receives the treatment. For two matched cities in pair i, say j and j', because they have the same observed covariates $\mathbf{x}_{ij} = \mathbf{x}_{ij'}$, we assume that after matching that $\pi_{ij} = \pi_{ij'}$. If this condition holds, our estimates are valid causal effects since we controlled for all relevant covariates.

However, we may have failed to match on an important unobserved covariate. Assume that this unobserved covariate is a binary covariate u_{ij} such that $\mathbf{x}_{ij} = \mathbf{x}_{ij'} \ \forall \ i, j, j'$, but possibly $u_{ij} \neq u_{ij'}$. If so, unlike in a randomized experiment, the probability of being exposed to treatment may not be the same within matched pairs due to the fact that we failed to match on a relevant covariate. If true, that would imply that $\pi_{ij} \neq \pi_{ij'}$, and our estimates will be biased. Rosenbaum (2002, ch. 4) shows that if two matched units differ in the probability of being treated due to an unobserved covariate, $u_{ij} \neq u_{ij'}$, then these two units may differ in their odds of being exposed to the candidate race treatment by at most a factor of $\Gamma \geq 1$ such that

$$\frac{1}{\Gamma} \leq \frac{\pi_{ij}/(1-\pi_{ij'})}{\pi_{ij'}/(1-\pi_{ij})} \leq \Gamma, \ \ \forall \ i, j, j', \text{ with } \mathbf{x}_{ij} = \mathbf{x}_{ij'}.$$

This inequality is useful since it shows that we can place bounds on quantities like point estimates for different possible levels of confounding due to u_{ij}. For example, if $\Gamma = 1$, then $\pi_{ij} = \pi_{ij'}$, and our point estimate for the effect of a black candidate is identified. Under selection on observables, we assume that this is true. However, if $\Gamma > 1$ due to some level of confounding from u_{ij}, then the true value of the point estimate is bounded by a known interval.

In a sensitivity analysis, we exploit this fact by using several values of Γ to compute bounds on the quantities of interest for the treatment effect. For example, say we observe that the estimated treatment effect is two percentage points. This estimate assumes $\Gamma = 1$, that is the unobserved confounder does not change the odds of treatment within matched pairs. If we make $\Gamma > 1$, we can place bounds on this point estimate. Specifically, we increase Γ until the bounds on the point estimate include zero. If the bounds include zero for a relatively small value of Γ, we can conclude that a confounder with a small effect on the treatment odds would change our conclusions. In other words, if the bounds include zero when the value of Γ is small, then we have little confidence in the results since a relatively small amount of confounding could overturn our conclusions. However, if the value of Γ is large when the bounds include zero, then we can greater confidence that a confounder would not change the conclusions of the statistical analysis. Thus the sensitivity analysis indicates the magnitude of bias due to an unobserved covariate that would need to be present to alter the conclusions reached under the selection on observables assumption. In the tables below, we report the value of Γ, for which the point estimate bounds include zero. This method of sensitivity analysis is most commonly used in conjunction with the Wilcoxon sign rank test. Rosenbaum (2014) notes that a sensitivity analysis based on m-estimation is a better choice of test statistic, as it is accurately reflects sensitivity to hidden bias. As such, we report sensitivity analyses using m-estimates based on the `sensitivitymw` package in `R` (Rosenbaum 2015*a*,*b*).

Moreover, we may be able to use the sensitivity analysis to test our theory. That is,

we may find that general election results are sensitive to bias from confounders, while the results from runoff elections are less sensitive. This pattern of evidence would be additional evidence in favor of our theory.

6 Analysis

Under our identification strategy, we use matching to make treated and control units comparable on observable covariates. We use an integer programming based match as implemented in the `R` package `mipmatch` (Zubizarreta 2012). Matching based on integer programming achieves covariate balance directly by minimizing the total sum of distances while constraining the measures of imbalance to be less than or equal to certain tolerances. This form of matching also allows us to impose constraints for exact and near-exact matching, and near and near-fine balance for more than one nominal covariate. Results based on genetic matching (Diamond and Sekhon 2013) produced similar results.

Under this form of matching, the analyst sets a tolerance for the imbalance for each covariate. For example, we might set the tolerance on the difference in means on a covariate to be less than 2 points across treated and control groups. The algorithm then attempts to achieve that level of balance. If this balance constraint is infeasible the algorithm stops and reports an error. The analyst must then select a less strict balance constraint. As we also noted above, we suspect that the percentage of African American residents will be particularly important. For the proportion of African American residents, we maintained the strictest balance constraints. For this continuous measure, we also focused on ensuring that both central moments and the higher moments of the treated and control group distributions were similar. To check imbalances in higher moments, we use the Kolmogorov-Smirnov (KS) test. The KS test measures the distance between the empirical distribution functions of the treated and control groups, which allows for the detection of discrepancies in higher moments of the distributions. For all other covariates we set the smallest possible balance constraints.

We also believe that election year is an important variable to match on. That is, we

would prefer to find matches from within the same yearly electoral cycle, which will hold constant larger national or state level trends (like presidential elections) that might also drive turnout in a particular year. Ideally, we would impose an exact match on election year. The difficulty, particularly for runoff elections, is that an exact match on election year tends to make balance very poor for other covariates. Instead, we used an almost exact match on election year. Under almost exact matching, if an exact match is possible, one will be produced, if not, the match will be as close to exact as possible, within a specified tolerance (Rosenbaum 2010, ch. 9). Section A.2 in the appendix contains tables that report the extent to which we were able to exact match on election year. For general elections, we obtained an exact match on election year in every matched pair but one. In runoff elections, we obtained an exact match on election year in very matched pair but three.

We used the integer programming match in conjunction with optimal subset matching (Rosenbaum 2012). Optimal subset matching allows us to match as many treated subjects as possible, recognizing that some treated subjects may be too extreme to match. The algorithm thus discards any treated units that are too extreme to match but does so such that balance differences are minimized but the number of treated units is optimized. This technique is analogous to trimming treated units via a caliper but in an optimal fashion. Under this strategy, it does change the estimand. That is, because we have discarded some treated units, the estimand is a more local version of the treatment effect on the treated. We found it impossible to retain all the treated units and achieve acceptable levels of balance.

7 Results

7.1 Balance Results and Unadjusted Estimates

We begin by reporting levels of imbalance before and after matching. Table 1 contains the difference in means and a KS test p-value for general elections. A few items are worth noting. In general elections, African American candidates tend to run in cities that are 34% black as compared to places that are 14% black. Places with African American candidates also

Table 1: Covariate Balance in General Elections Before and After Matching

	Before			After		
	Treated Mean	Control Mean	KS test p-value	Treated Mean	Control Mean	KS test p-value
Municipal Population	11872.2	5125.2	0.00	12537.4	9591.8	0.23
African American (%)	33.7	13.7	0.00	25.9	25.9	0.27
College Degree (%)	5.6	5.8	0.20	6.2	6.2	0.27
High School (%)	26.5	30.3	0.00	27.3	27.3	0.12
Unemployed (%)	14.6	10.8	0.00	13.2	13.1	0.25
Median Income	14238.6	18140.0	0.00	15623.4	15699.5	0.33
Below Pover Line (%)	38.7	27.0	0.00	34.5	34.2	0.27
Home Rule (0/1)	11.0	8.8	0.27	13.7	12.2	0.65

tend to have lower levels of education, lower incomes, and higher poverty rates. Given the large differences between places with and without African American candidates, we must be careful to remove such differences before comparing levels of turnout. Table 2 contains the same information for runoff elections. We observe that generally the differences between treated and control places are somewhat larger than in general elections.

Table 1 also contains the results after matching in general elections. For general elections, we achieve good balance. The difference in the percentage of African American residents is a mere tenth of a point. For municipal population, the difference in means is reduced from over six thousand to less than three thousand. To achieve this level of balance, we had to discard a number of treated observations. For general elections, we started with 335 treated elections. To balance the covariates, we discarded 220 treated observations and found suitable matches for the remaining 196 treated elections. In sum, before matching we have an apples to oranges comparison, but after matching we are more confident that this is an apples to apples comparison.

For runoffs elections, we also produce a set of successful matches. Table 2 contains the balance results after matching. For the percentage of African American residents, the difference is one percentage point. For municipal population, the difference in means is less

Table 2: Covariate Balance in Runoff Elections Before and After Elections

	Before			After		
	Treated Mean	Control Mean	KS test p-value	Treated Mean	Control Mean	KS test p-value
Municipal Population	21346.5	10721.6	0.14	29866.9	25147.3	0.87
African American (%)	38.5	17.4	0.00	29.8	27.8	0.67
College Degree (%)	5.5	6.1	0.20	6.0	6.4	0.89
High School (%)	25.3	29.1	0.00	26.3	26.4	0.99
Unemployed (%)	16.3	11.4	0.00	14.3	13.6	1.00
Median Income	13702.6	17600.4	0.00	14547.1	15160.0	0.89
Below Poverty Line (%)	41.7	29.1	0.00	38.1	35.7	0.60
Home Rule (0/1)	11.8	18.5	0.21	18.5	18.5	1.00
# First Round Candidates	4.3	3.7	0.10	4.6	4.0	0.82

than 600 residents. We also performed one additional match where we exactly matched on whether or not there was at least one African American candidate in the general election. We found that among the elections with at least one African American candidate in the first round of voting, 68 triggered a runoff with African American candidate and 28 did not. We used the matching algorithm to produce balance results identical to those with the full set of runoff elections. This left us with 15 matched pairs. Of course, in all cases we may have failed to match on an important hidden confounder. We consider that possibility in the sensitivity analysis.

Next, we present unadjusted estimates from general elections and runoff elections. The unadjusted estimates are simply comparisons between races with African American candidates and races without African American candidates. Table 3 contains the unadjusted estimates. In both cases, we find African American turnout is higher when an African American candidate is in the mayoral race. For general elections, African American turnout is 7.27 percentage points higher. For all runoff elections turnout is 7.3 percentage points higher, and it is 9.9 points higher in runoffs that had at least one African American candidate in the first round of voting. Based on the unadjusted data, there is little reason to suspect that viability plays any role as the magnitude of the estimates is similar in all three contexts.

These estimates, however, contain some mix of both candidate and place effects. We should expect these estimates to change once we match, since we will remove place effects through the matching.

Table 3: Unadjusted Estimates of The Effect of African American Candidates in Mayoral Elections in Louisiana, 1988-2011

	General Election	Runoff Election All	Runoff Election Af-Am in First Round
Point Estimate	7.27	7.3	9.9
95% Confidence Interval	[4.7, 9.8]	[2.3, 12]	[2.9, 17]
p-value	$< .001$	0.004	0.008
N	1006	187	96

Note: Point estimates are the Hodges-Lehmann estimates from the Wilcoxon sum rank test.

7.2 Estimates After Matching and Sensitivity Analysis

We now turn to the estimates from the matched analysis. In Table 4, we report point estimates, 95% confidence intervals, one-sided p-values, and the value for Γ the sensitivity analysis parameter for the point estimate. In particular, we are interested in whether there are clear differences in the results between general elections and runoff elections. Evidence for our theory should manifest itself in differences between results from the two different election types that account for the variation in viability.

First, we discuss the results from the general elections. Recall that in the unadjusted data the point estimate was 7.27 percentage points, and once we match–assuming there are no hidden confounders–the point estimate is now 3.4 percentage points. The sensitivity analysis, however, reveals that these estimates are sensitive to bias from a hidden confounder. If the odds of treatment (Γ) differ by as much 69%, we would conclude that the actual treatment effect is zero. How can we tell if these are large or modest values of Γ? To provide a baseline, we regressed the treatment status on municipal population and percentage of African American residents using a logit model. We then calculated the odds-ratios for the covariates in this model. We can compare these odds-ratios from this model to those from

the sensitivity analysis. For example, increasing the percentage of African Americans by one percentage point increases the odds of an African American candidate by 9%. Thus if we failed to match on a covariate that alters the odds of treatment the same magnitude as a little over a three percentage point increase in African American population that could explain the estimate we observe in general elections. Our inference is clear: once we compare cities with similar racial distributions, the effect of an African American candidate is much more modest in general elections, and the effect could easily be explained by unobserved confounding. Therefore, we find that in general elections, where viability signals to voters are weaker, the point estimate from the unadjusted estimate is much reduced, and the sensitivity analysis reveals that bias from confounders could easily explain the results.

For runoff elections, however, the point estimate after matching slightly increases to 8.4 percentage points and is statistically significant ($p = .015$). Thus when the viability signal is stronger the point estimate is about five points larger than in the general election. Moreover, now the point estimate is fairly robust to the presence of a hidden confounder. A hidden confounder would have to change the odds of treatment by 400%, to explain this point estimate. Again, we used observed covariates to understand whether this is a large or small Γ value. In runoffs, increasing the percentage of African Americans by one percentage point increases the odds of an African American candidate by 19%. For runoffs, we would have to fail to match on an unobserved confounder that has an effect on the odds of treatment with the same magnitude as an 8 percentage point increase in African American population. Next, we calculate results for runoff elections where there was at least one African American candidate in the first round of voting. Here, the point estimate decreases to 6.5 percentage points, but it is not statistically significant ($p = .303$). The point estimate is fairly robust to the presence of a hidden confounder. A hidden confounder would have to change the odds of treatment by 295%, to explain this point estimate. However, we should note that our estimates are not precisely estimated. In all three cases, the 95% confidence intervals overlap.

Table 4: Estimates of The Effect of African American Candidates in Mayoral Elections in Louisiana, 1988-2011 After Matching

	General Election	Runoff Election All	Runoff Election Af-Am in First Round
Point Estimate	3.4	8.4	6.5
95% Confidence Interval	[0.74, 6]	[2.1, 14]	[-7.7, 17]
p-value	0.011	0.015	0.303
Γ	1.69	4.01	2.95
N	394	52	30

Note: Point estimates are the Hodges-Lehmann estimates from the Wilcoxon sum rank test. Γ value is value at which lower bound on the point estimate includes zero.

Thus the pattern from our theory holds up. When viability signals are weaker, we observe a more modest increase in turnout, and the results are more sensitive to the possibility of a hidden confounder. In runoff elections, where viability signals are stronger, the point estimate is larger and more resistant to bias from hidden confounders. However, even in the general election, our estimates are larger than those found in studies of Congressional elections. We think this is mostly likely a function of the fact that we examine mayoral elections, which may receive more attention than Congressional elections that are often lop-sided in favor of incumbents.

8 Conclusion

In this study we sought to isolate whether viability matters for turnout among minority voters who can vote for a co-racial candidate. While no single result from observational data can be considered definitive, the pattern in our data is fairly clear. In general elections, once we evaluate comparable mayoral races, there was weak evidence that black voters turnout to vote more for black candidates. Specifically, the effect is relatively modest once we adjust for confounders, and the sensitivity analysis suggests that it would take a small amount of bias from a hidden cofounder to explain the observed difference across treated and control groups. These results are consistent with recent work on this topic (Fraga 2014; Keele and White 2011; Sekhon, Titiunik and Henderson 2010). In runoff elections, however, we

found much larger estimates that were more robust to bias from hidden confounders. As we argued, viability should be higher in these races. That is, when a runoff is triggered, it should signal to voters that the candidate is viable and that their vote matters because of the competitiveness. Our study then reveals an important mechanism that future studies should consider. That is, designs that do not attempt to separate races by the level of viability may mix effects with very different magnitudes.

How else do our findings relate to the extant literature? Past research has focused on how the demographic composition of districts has influenced minority turnout (Leighley 2001; **?**). In our study, we seek to show that demographics are not destiny. By holding demographic composition fixed in our study, we have demonstrated that when viability signals are increased, through the presence of a minority candidate in a runoff election, turnout tends to be higher. This does not imply that the racial composition of the district does not matter. Our study confines itself to the set of places where the percentage of African American residents is large enough that an African American candidate will be present. Indeed our argument is that in places where African American candidates are viable due to the racial makeup of the place, an additional viability signal, here provided by runoff elections, is critical to boosting turnout.

Next, we discuss the wider relevance of our study given that it is confined to a subset of municipalities in Louisiana. To preserve internal validity, we restricted our analysis to places with a moderate to high percentage of African-American residents. This means that, by definition, we excluded places in Louisiana that had a high proportion of white residents. However, we would argue that our results are likely to generalize to other municipalities in the South that have a similar racial composition. Thus we would conjecture that the places we study in Louisiana are not so different from many other Southern locales with a similar racial makeup. This pattern of effects, however, creates a paradox in terms of what we might learn more broadly about African American participation. While black voters may prefer to vote for a co-racial candidate, most black voters will only be motivated to do so if they

see that candidate as viable. In legislative districts, where many African Americans run in majority-minority districts viability signals may be missing. Thus these elections lack a key component of viability, and thus there is little chance of boosting minority participation.

Furthermore, this paper also shows that theories of minority empowerment and strategic electoral behavior are compatible. If black candidates are indeed not running in places where they believe they are less likely to win because of a perceived racial disadvantage, then to the extent that black voters living in these places are also aware of this disadvantage, they have no reason to turnout and vote, even when a black candidate does run. Black voters in these places are essentially disempowered, not by the lack of black candidates but by a chronic lack of black electoral power. For blacks in low black population districts (and especially in places where blacks have few viable coalition partners) it is not until black population numbers reach a certain threshold that they will perceive the group to have any electoral strength. Thus, black turnout in these communities is likely to remain low as voters doubt the prospects of electing candidates that they think will represent their interests. It is, of course, this very logic that drove the creation of majority-minority districts under the Voting Rights Act.

Our predictions are also consistent with research on candidate emergence which suggests that the under-representation of blacks in elected office is not an issue of potential candidates, but a shortage of elections black candidates feel they have the potential to win. For example, one analysis of why so few black House members do choose to run for Senate seats points to the role of racial demographics (Johnson, Oppenheimer and Selin 2012). Similarly, Shah's (2014) analysis of black candidate emergence in local offices across Louisiana concludes black candidates are most likely to run when the demographics of the jurisdiction are in their favor. Thus, the issue does not seem to be a shortage of potential candidates, but a shortage of elections viable black candidates feel they have the potential to win. Any examination of the ability of black candidates to influence turnout should take into account the strategic considerations of black voters who do not respond merely to racial cues, and the strategic

selection of candidates into elections.

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Appendices

A.1 Municipal Structure in Louisiana

Local governments and local elections across the U.S. are quite varied, and many of these differences could impact turnout. Cities often differ across a number of dimensions, from the form and structure of government (city manager vs. mayor-council, term limits vs. no term limits), to electoral rules (timing of elections, at-large vs. appointed mayors). A number of scholars have outlined how these differences influence turnout, and minority descriptive representation (Hajnal and Lewis 2003; Hajnal and Trounstine 2005)

By focusing solely on mayoral elections in Louisiana, we sidestep much of this variation, and thus are able to better isolate the effects of black population turnout. According to the 2010 Census, there are 304 municipalities in Louisiana. These municipalities are classified according to population size. Villages have populations of less than 1,000 residents, and account for 107 (or 33%) of the municipalities. Towns have populations between 1,000 and 5,000 residents, and account for 126 (or 41%) of municipalities. Cities are larger than 5,000 residents, and comprise the smallest share of municipalities (71 cities (23%)).

The vast majority (92%) of municipalities in Louisiana are governed by the Lawrason Act (1898), which details the form of governments and electoral rules. The officers of a Lawrason Act municipality are a mayor, aldermen, a chief of police, a tax collector, and a clerk. The number of aldermen varies from three to nine, depending upon the size of the municipality (modal category is 5). Mayors are elected at-large, and serve 4-year terms. Last, the Louisiana Secretary of State notes that all municipal elections are held off-cycle, with primaries occurring in either March or April, followed by a general one month later. The method of election for city council members is more varied. In our analysis of the form of government of 166 Louisiana cities, we find that the modal category (56%) is all seats elected at-large. This is particularly true for smaller (¡5 council members). Larger councils are more likely to use a mixed method of some at large, and some ward-based elections.

Twenty-seven municipalities in Louisiana have drafted home rule charters, which provide them with greater local governing authority. In our analysis of 166 cities in Louisiana, the vast majority of these municipalities still have a mayor-council form of government, and there electoral structures and elections are held at similar times as Lawrason cities. As we noted in the paper, we classified places as either home rule or not, and matched on this covariate as it seemed the most relevant property in terms of municipal structure.

A.2 Near Exact Match on Election Year

In this section, the tables document the distribution of near exact matches for each of the three matches.

A.3 Summary Statistics for Discarded Observations

Table 5: Near Exact Matches on Election Year in General Elections

	T	C
1988	5	5
1990	12	12
1991	1	1
1992	10	10
1993	2	2
1994	22	22
1995	1	1
1996	5	5
1997	1	1
1998	19	19
1999	1	0
2000	13	14
2001	4	4
2002	24	24
2003	2	2
2004	13	13
2005	2	3
2006	22	21
2007	0	0
2008	12	12
2009	2	2
2010	23	23
2011	1	1

Table 6: Near Exact Matches on Election Year in Runoff Elections

	T	C
1988	0	1
1990	2	1
1991	0	0
1992	2	2
1994	3	4
1995	1	0
1996	1	1
1998	2	2
2000	3	3
2002	2	2
2003	0	1
2004	2	2
2005	0	0
2006	5	5
2007	0	0
2008	1	0
2009	0	0
2010	3	3
2011	0	0

Table 7: Descriptive Statistics for Observations in the Match and those Excluded – General Elections

	Excluded From Match		Included in Match	
	Mean Treated	Mean Control	Mean Treated	Mean Control
Municipal Population	11048.0	3182.8	12537.4	9591.8
African American (%)	43.2	8.4	25.9	25.9
College Degree (%)	4.9	5.7	6.2	6.2
High School (%)	25.5	31.7	27.3	27.3
Unemployed (%)	16.4	9.7	13.2	13.1
Median Income	12522.8	19201.3	15623.4	15699.5
Below Pover Line (%)	43.8	23.9	34.5	34.2
Home Rule (0/1)	7.5	7.3	13.7	12.2

Table 8: Descriptive Statistics for Observations in the Match and those Excluded – Runoff Elections

	Excluded From Match		Included in Match	
	Mean Treated	Mean Control	Mean Treated	Mean Control
Municipal Population	15735.6	6487.9	29866.9	25147.3
African American (%)	44.2	14.4	29.8	27.8
College Degree (%)	5.1	6.0	6.0	6.4
High School (%)	24.7	29.9	26.3	26.4
Unemployed (%)	17.6	10.8	14.3	13.6
Median Income	13146.5	18316.7	14547.1	15160.0
Below Poverty Line (%)	44.0	27.2	38.1	35.7
Home Rule (0/1)	7.3	18.5	18.5	18.5
# First Round Candidates	4.1	3.6	4.6	4.0