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Dissertation

THREE ESSAYS IN POLITICAL ECONOMY

by

STEVEN SPRICK SCHUSTER

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THREE ESSAYS IN POLITICAL ECONOMY

(Order No.)

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ABSTRACT

"Delivering the Vote: The Political Effect of Free Mail Delivery in Early Twentieth Century America(with Elisabeth Perlman)", examines the effect of Rural Free Delivery on the behavior of voters and elected officials. Using a panel dataset covering the years surrounding the roll-out of RFD, and a set of instrumental variables to address the endogenous allocation of routes, we find that voters in communities receiving more routes vote more frequently, spread their votes more widely across candidates, and vote for smaller parties. Additionally, we find evidence of political shifts in elected officials, with representatives voting more in line with rural communities.

"Duvergers Law and Strategic Voting: an Empirical Test Using Floridas Elimination of Primary Runoff Elections", provides evidence from an empirical test of Duvergers Law and strategic voting, utilizing the 2001 decision by the state of Florida to eliminate runoffs in primary elections. Using a differences-in-differences specification, I find that while changes in voting behavior in Florida are consistent with predictions of Duvergers Law and models of strategic voting, the measured effects are much smaller than findings in other settings. I provide evidence that this may be due to low turnout in primary elections, which may induce two-candidate equilibria in runoff elections.

"What We Talk About When We Talk About Campaign Spending" utilizes the 2012 American National Election Survey, which asked about voter intent before the election and actual voting behavior after, and candidate disbursement data which allows me to track candidate spending throughout the campaign, I can determine the effect of candidate

spending on individual voting behavior. I find that spending by candidates on advertising and campaign events increases the likelihood that voters will change their preferences in favor of that candidate. I also find that using aggregate candidate spending to measure the causal effect of incumbent and challenger spending could lead to biased estimates, and could be driving previous results that have found incumbent spending to be less effective than challenger spending.

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List of Abbreviations

ANES	American National Election Survey
FEC	Federal Election Commission
FTF	Face-to-Face
IV	Instrumental Variable
OLS	Ordinary Least Squares
RFD	Rural Free Delivery

Chapter 1

Delivering the Vote: The Political Effect of Free Mail Delivery in Early Twentieth Century America(with Elisabeth Perlman)

1.1 Introduction

Changes to information flows affect the behavior of both the electorate and politicians;[1] when deciding whether to vote and for whom to vote, coordinating with other voters, and interacting with their elected officials, potential voters rely on information from candidates, media sources and other potential voters. However, because information networks and access to mass media are almost always endogenous to political activity, the ability for researchers to identify quasi-experimental settings through which to measure the causal effects of information on political outcomes is limited. However, Rural Free Delivery (RFD), which introduced daily mail to millions of rural homes at the turn of the twentieth century, provides us with a unique opportunity to explore this relationship.

In the late nineteenth century and early twentieth century there were great changes in how information was acquired and disseminated in the United States. The invention of the web rotary press made large-scale newspaper and magazine printing runs possible,[2] and the introduction of radio dramatically reduced the marginal cost of disseminating information. The establishment of telegram and telephone lines across the country made interpersonal

[1] Almond and Verba wrote, in their seminal 1963 book, "The uneducated man or the man with limited education is a different political actor from the man who has achieved a higher level of education."

[2] Hamilton (2004) discusses how large scale print runs lead through the desire to attract a wider variety of subscribers to the development of the independent newspaper.

communication much more affordable. The potential for these changes to affect the political process was manifold, particularly in the way that they changed the ability of individuals to acquire information, and of political candidates and parties to send messages to voters.

These advancements were especially important for residents of rural areas, whose isolation during post-Civil War America was an acute concern for policy makers.[3][4] This relative isolation was notably apparent in rural residents' almost complete lack of access to daily mail. Since 1864, city dwellers enjoyed either at-home mail delivery or close proximity to post offices, while rural residents were forced to travel many miles to the nearest post office to receive mail. These concerns led to a push for the expansion of daily mail delivery to rural homes. First created on an experimental basis in 1896, and rolled out across the country during the first decade of the twentieth century, RFD changed the flow of information to rural communities as well as the information networks within them.

This paper explores the effect of RFD on the functioning of the political process. The program dramatically changed both the frequency and the richness of information rural communities could receive, providing us with a unique opportunity to measure the causal effects of access to mass media and information networks. Any attempt to measure the causal effect of voter information on political outcomes faces a severe endogeneity problem, since people's consumption of information is typically endogenous. People who consume more information and groups with more robust information networks will vote in different ways, elect representatives with different characteristics, and elicit different results from these representatives due to a number of variables and characteristics, many of which are unobserved. RFD, however, led to an almost immediate change in the availability of information to individuals affected by the service. Because not every person received RFD, and because eligibility for the service was determined by a complex set of rules established

[3]In his 1903 Annual Message to the Senate and House of Representatives, President Theodore Roosevelt said, "Rural free delivery, taken in connection with the telephone, the bicycle, and the trolley, accomplishes much toward lessening the isolation of farm life and making it brighter and more attractive."

[4]While in much of the world rural resistances tend to cluster there dwelling in small villages, rural area in the US tend to be populated by evenly spaced isolated farm houses, where ones closet neighbor is often more than a quarter mile away.

by the United States Post Office, we can use a set of instruments to estimate the causal effect of RFD on political activity.

Using this variation we find that one RFD route increases voter turnout, as well as electoral competition within a county, measured through the number of parties receiving 5%, 10%, or 20% of the vote. This increase in competition appears to benefit small parties, as the share of the vote going to neither the Republican nor Democratic party increases with more RFD routes. Using a dataset on daily, semi-weekly and three times weekly newspapers, we find that these observed effects for several of our results are stronger in communities with newspapers, providing support to the hypothesis that RFD changed voting behavior primarily by changing the level of information available to voters. We also find a change in the behavior of elected officials in response to RFD allocation. The policy position of members of the House of Representatives, based on their roll-call votes on the floor of Congress, shifts towards stances that were, at the time, associated with rural communities. In particular, we look at the contentious issues of temperance and restricting immigration, and find that the support for both increases with RFD rollout. Of these results, our findings of the effect of RFD on the distribution of votes are both the most robust and the most surprising, in that these results are not predicted by existing theoretical models.

The rest of this paper is organized as follows: Section 1.2 provides additional details on the motivations for this paper, while section 1.3 describes the historical context for the rollout of RFD. Section 1.4 provides our results split into two parts—section 1.4.1 that examines a fixed effect estimation and section 1.4.2 that examines a instrumental variables estimation. Within these parts section 1.4.1.1 and section 1.4.2.1 discuss our data, the later discuses the collection of the instrument, while section 1.4.1.2 and section 1.4.2.2 discuss the results. The potential mechanisms for these results are explored in section 1.5, and the changes to the behavior of elected officials is examined in section 1.5.1. Section 1.6 concludes.

1.2 Motivation

Rural Free Delivery, according to reports from both postal workers and recipients, led to significant changes in three areas: in the amount of mail sent and received by communities, in newspaper circulation, and in the quality of roads in RFD communities.

The 1902 Report of the Postmaster-General states that postal revenue for counties receiving RFD increased by an average of ten percent. One major source of this increase in mail volume was an increase in newspaper circulation. We can observe this using a dataset of newspaper circulation based on Gentzkow et al. (2012) with our own additions (see section 1.4.1.1).

Finally, the introduction of RFD in rural communities had the effect of improving road quality in these communities. Passable roads were a requirement for RFD; rural road construction and maintenance was largely in the hands of farmers. The Post Office required that families along approved routes that traversed low-quality roads sign a pledge to work to improve and maintain roads, or face losing the route.

Increased mail and better roads affected the bidirectional flow of information, while higher newspaper circulation changed the dissemination of information across communities. Each of these effects changed the structure of networks and information flows in rural communities, and could in turn change the way in which voters reach their decisions, and thus in turn their relationship with their Representatives.

Previous research can help us develop hypotheses for the possible effect of RFD on political outcomes. Firstly, empirical studies have consistently found that higher levels of connectivity and information lead to higher levels of voter turnout. Gentzkow et al. (2011), using data on newspaper entry and exit, show that the entrance of the first newspaper in a county is associated with a small but significant increase in voter turnout. Gerber et al. (2009) conducting a field experiment, found that people were 30 percent more likely to vote when researchers said they would tell the respondent's neighbors whether or not they voted. The theoretical motivation for these results lies in the groundbreaking work by

Riker and Ordeshook in 1968, who introduced the theory of social motivation for voting. Under this theory, even with small probability of being pivotal and relatively high costs to voting, people will respond to senses of civic duty or goodwill. Stronger ties to groups or neighbors could increase the social benefit to voting, therefore increasing turnout.

Voter turnout is not the only relevant margin. Because RFD did not affect all communities equally, an increase in turnout in those communities would also affect the distribution of voters. Increased connectivity in rural communities could also affect the ability for rural voters to coordinate their votes behind individual parties or candidates. Small parties, including the Greenback and Populist parties, advocated for farmer-friendly policies, while The Grange continued to be a strong unofficial political player. The ability of many of these groups, which lacked centralized political machines, to reach rural voters may have been minimal, and therefore would have benefited the most from RFD.

Richer levels of information and connectivity also translate to increased social capital, which research has repeatedly shown leads to an increased ability of voters to elicit favorable policies from elected officials. Strmberg (2004) found that communities in the United States with greater access to radios received greater relief funds from the federal government during the New Deal. Given the increase in both information (through the increase in newspaper circulation) and exchange of ideas through the mail, we would expect voters receiving RFD to increase their ability to garner favorable policies from Representatives. In Strmberg's model, this increase in political power is driven by an increased likelihood that voters learn about the behavior of their elected officials. When one group becomes better informed, politicians change their behavior by choosing policies favored by the better informed group. Within the context of RFD, this translates to a prediction that rural communities receiving more routes will see the behavior of Representatives shift towards policies favored by rural communities.

This paper contributes to two literatures: the relationship between information and political behavior, and historical studies of the effects of the establishment of RFD. The complex relationship between voter information and politics has received considerable attention through both theoretical and empirical research. Milligan et al. (2004), found that higher levels of education lead to increased political involvement in the U.S. and the U.K. Gerber et al. (2009), using data from a field experiment in the Washington D.C. area, found that people who received a left-leaning newspaper were eight percent more likely to vote for a Democrat for Governor. Gentzkow et al. (2006) found the conversion of newspapers from politically affiliated to independent, which occurred rapidly in the period before 1920, to be correlated with a fall in political corruption. Drago et al. (2013) showed that newspaper exit in Italy corresponded to decreases in political efficiency (as measured by corruption). In the development literature, several experiments have found that information plays a crucial role in the way voters hold politicians accountable in terms of corruption. Ferraz and Finan (2008) showed that when audits reported two instances of corruption of mayoral incumbents in Brazil, likelihood of re-election decreased by seven percent compared to the control group. Banerjee et al. (2010) conducted a field experiment in India, finding that when voters were provided with newspapers reporting on audits of incumbents, they exhibited high levels of sophistication in their voting, rewarding high-performing incumbents, while average-performing incumbents received no such boost.

The motivation for such empirical work lies in voting models of imperfect information and models outlining the social motivation for voting. The importance of well-informed voters goes back to Condorcet's Jury Theorem (1785), which relied on well-informed voters. In describing what they call the "Swing Voter's Curse", Feddersen and Pesendorfer (1996) illustrate the role of information both on the decision of potential voters to participate in an election, and the ability of voters to influence the behavior of others. The "Bandwagon Effect", as described by Simon (1954), Bowden (1987), and Mehrabian (1998), predicts that people will become more inclined to vote for a candidate as the candidate's odds of winning increase. Within the context of information networks, richer networks may allow voters to better identify their most preferred candidate, and to coordinate behind their preferred candidates. Assuming that larger parties are already known to more candidates, these effects would be felt most strongly by smaller, less salient candidates.

The literature on Rural Free Delivery is less rich. While Fuller (1955, 1959, 1964) provide valuable historical context on the establishment of Rural Free Delivery, few papers have used RFD to test economic or political science hypotheses. Carpenter (2000) investigates models of state building through several large-scale postal initiatives (including RFD), while Kernell (2001) considers the effect of the individual political gains that members of Congress believed they would receive with the implementation of RFD during the Post Office's transition from a system of patronage to a service. Though RFD rapidly changed millions of individuals' access to information, we are unaware of any research that attempts to use RFD to explore causal effects of information acquisition on political outcomes. Using the richness of county-level data, along with the variation in which areas benefited from RFD, we explore how trends in political behavior correlate with voters' access to information.

1.3 Rural Free Delivery

While daily mail delivery is now taken for granted, the disparity in the quality of service between rural and urban households in the U.S. in the late nineteenth century is difficult to overstate. Though people living in cities enjoyed close proximity to post offices or direct home delivery (often two or three times daily in the largest cities (Greathouse, 1900)), households in rural areas had no access to daily mail and had to pick up any mail at the nearest post office, generally several miles away from their homes. RFD was conceived of as a way to address this disparity of postal service by bringing free daily mail to rural residents. Under the system, rural routes were established, emanating from existing post offices, which were served daily by rural carriers, who were postal employees. Any family wishing to be served by the system needed only to erect a weatherproof box along the route to receive mail (the service was free in that no cost but postage was required).

Early advocates of RFD highlighted the programs potential to alleviate the monotony of rural life. In 1900 State Senator Thomas J. Lindley of Indiana applauded RFD, writing that "[the farmer] no longer feels the isolation of country life. I think the system will contribute largely to prevent the threatened congestion of population in our cities and town" (Greathouse, 1900). Before daily delivery brought mail to their doorsteps, the only way for rural homes to receive news or receive or send mail was to travel to the nearest fourth class post office, which was typically the nearest general store. Even in the best conditions, a trip to the post office for someone who lived five miles away would likely entail three and a half hours spent on travel alone; this was unlikely to be a feasible trip for rural residents to make every day. Conditions were seldom ideal, making travel time much longer, and the mail itself was often delayed (Fuller, 1964, pg. 15). Families living on farms would sometimes go weeks at a time without mail in periods of bad weather.

The first high profile call for RFD came in 1891, from Postmaster General John Wanamaker. As a way to test the feasibility of RFD, he proposed that the Post Office implement limited delivery in a few rural towns (Fuller, 1964, pg. 18). This experiment was delayed for several years due to insufficient allocation of funds from Congress and two changes in Postmaster in three years. (Fuller, 1964, pg. 21, 24). Finally, in 1896, the first experimental routes, (eighty-two in all) were established, with the stated intention of choosing locations which varied as much as possible (Fuller, 1964, pg. 39). In 1898, when only 412 routes existed nationwide, the Post Office formalized the mechanism for route allocation: communities wishing to receive a route were to petition their Representative, and route establishment required approval from both Representative and Postmaster. Due to several well-publicized successes in county-wide RFD networks, Congresspeople were inundated with petitions from farm communities hoping to use the new service. In the face of widespread support for the program from constituents, even Representatives initially opposed to RFD were forced to support the program (Carpenter, 2000).

The 1903 Yearbook of the United States Department of Agriculture described the process thus:

> The delivery of mails by rural carriers is extended in response to petitions presented by the people desiring the service upon forms prepared by the De-

> partment, which include a diagram of the proposed route. It is required that the route shall be from 20 to 25 miles in length, so laid out that the carrier will not have to traverse the same road on his return as on his outward trip, and so adjusted that at least 100 domiciles shall be included in the service. Such a petition, when presented to the Department with the approval of the Congressional Representative of the district or of one of the Senators from the State in which the service is asked for, is investigated by one of the special agents in the field, who transmits the papers, with a map of the route or routes to be followed, to the Superintendent in Washington for his adjudication.

These guidelines are the same as those outlined by the Post Office in 1898, and were determined by the feasibility and cost effectiveness of mail delivery. One hundred families was deemed to be the minimum number of households necessary to financially justify a route, while twenty-five miles was viewed as the longest route mail carriers could reliably serve year-round. It should be noted that while road and weather conditions varied across the country, resulting in large variation in what a carrier could cover, these regulations applied equally to all communities; even if a town had the misfortune of featuring rough terrain or impassable roads, the Post Office would not exhibit leniency in its decision to approve or reject a route. Additionally, these official guidelines were largely unchanged during the duration of the rollout of RFD.[5] It is important for our identification strategy that these guidelines were not determined by Congresspeople, whose motivations may have been political.

In addition to the regulations noted above, routes could not be established where roads were not passable year-round (Fuller, 1964, pg. 182). Additionally "Rural" was defined as places outside an incorporated area (Greathouse, 1900), and no home within two miles of a post office was eligible.

Rapid expansion of RFD followed quickly, as can be seen in Figure 1.1. Between 1900 and 1908, the number of RFD routes increased from 1,259 to 39,777. Though many communities were left unserved additional route allocation all but halted by 1908. By that year, more than 88% of routes that would ever be extant had been established (Kernell,

[5] In later years of the rollout (post-1904), the Post Office loosened the requirements to allow for routes serving as few as sixty families.However, this change appeared to be the results of increased Congressional funding and decreases in transportation costs.

2001); the Post Offices stated goal of "universal delivery" had been nearly achieved. In fact, during the 1910 Postal Appropriations hearing in front of Congress, Fourth Assistant Postmaster General P.V. De Graw claimed that all communities qualifying for RFD under the 1898 guidelines had received routes, and that only liberalization of the rules regarding the number of houses served (from 100 to 60) allowed for further route allocation (None, 1912, pg. 462). Additionally, in 1909, facing a deficit in the Treasury, President Taft ordered that budgets be cut dramatically, which made route creation significantly more difficult (Fuller, 1964, pg. 78). For these reasons, we consider the rollout of RFD to be the years 1901 to 1908. In our analysis, we consider only pre- and post-rollout years.

It is clear that route allocation was correlated with a number of factors that were likely associated with different levels of political activity. To obtain a route, communities had to apply for routes; therefore, more motivated communities would have received routes more quickly. Additionally, because routes required sponsorship by a Congressperson, a community's success in receiving a route would be a function of Congressperson characteristics, specifically party membership, at the time of the rollout and experience. RFD was, in its infancy, seen as a Republican program, and Carpenter (2000) provides evidence that route allocation was denser in districts featuring Republican incumbents.

To measure a causal effect of RFD, to address this endogeneity problem, we therefore use both place and time fixed effects and instrumental variables. By using county (and later in our analysis, Congressional district) level fixed effects, we control for time-invariant, unobserved location characteristics. However, as we will show, even the inclusion of fixed effects will likely still lead to a bias in our estimates. Second, to address the non-random allocation of routes, we use a set of instruments correlated with route allocation. In the presence of place fixed effects, our identifying assumption for our instruments is that they be uncorrelated with trends in our outcome variables, instead of levels. Our instruments make use of the requirement that routes be only placed along passable roads; to this extent, route allocation was a function of both climate conditions during the rollout of RFD, and of decisions made by both State- and County-level politicians in the years before the announcement of RFD.

1.4 Effect on Voters

The estimation of the effect of RFD rollout on voter behavior proceeds in two parts: a fixed-effects estimation (section 1.4.1) and, to address the bias of the fixed-effects coefficients, an instrumental variables estimation (section 1.4.2). The bulk of the data is discussed in section 1.4.1.1, while the collection of the data for the instrument is discussed in section 1.4.2.1.

1.4.1 Fixed-Effects Estimation

To better understand how counties that received more rural free delivery routes changed compared to those that received fewer, we use a fixed-effects model with year and place fixed effects to control for time and place-invariant characteristics. The basic specification for each of our county-level political outcomes is:

$$Y_{ct} = \beta Routes_{ct} + \gamma_c + \delta_t + \mu \mathbf{X_{ct}} + u_{ct} \tag{1.1}$$

where Y_{ct} are our political outcomes, such as voter turnout; γ_c and δ_t are a set of county and year dummies; $\mathbf{X_{ct}}$ is a vector of county characteristics: percent of population in the county living in communities of more than 2,500 people and the square of that value, the percent of farmland that is "improved", the percent of residents that are not white, the percent of residents that are foreign-born and white,[6] the natural log of the population, and dummies for the presence of Jim Crow voting laws and whether women have the right to vote; $Routes_{ct}$ is the number of routes a county had in year t. Therefore, β, the coefficient on the number of routes, is out estimate of the causal effect of mail routes.

Our focus in this paper is on the effects of the complete allocation of routes, as opposed to the timing of route allocation. We therefore eliminate the years 1901 to 1907 from our

[6] All percents are expressed as a number between 0 and 100.

analysis. Additionally, we hold the number of routes in all years 1908 and later constant at their 1908 values, and all year 1900 and earlier fixed at their 1900 values. Because many election characteristics, such as the number of candidates, vary only at the Congressional District level, districts typically include several counties, we cluster standard errors at the district level.

1.4.1.1 Data

We compiled and digitized the county-level RFD route allocations using the 1908 United States Official Postal Guide, which listed the number of RFD routes emanating from each post office. This gives a measure of the intensity of RFD service within a county. This is, to our knowledge, the first attempt to compile statistics on the full allocation of routes. We also compiled the 1900 number of routes in each county using the 1900 Report of the Postmaster-General.

Our voting data are from Clubb et al. (2006), which provides data on county-level voting in each year, including total number of votes, turnout, and vote share for most major and minor parties. County characteristics data are from Haines (2010), we then use the method described in Hornbeck (2010) to harmonize the county boundaries to their 1890 values. Voting behavior of elected officials come from the DW-Nominate scores, as described in Poole and Rosenthal (2001).

Our newspaper dataset was constructed by supplementing an existing dataset by Gentzkow et al. (2012), which provides circulation data on all English-language daily newspapers printed within a county, excluding professional or social publications. We added data on semi-weekly and three times weekly papers, using the N.W. Ayer and Sons American Newspaper Annual. This variable does not provide perfect data on newspaper readership, as all newspapers printed within a county are counted in the circulation within a county, and newspapers consumed in counties different from where they are printed are incorrectly attributed to the county in which the paper is printed. Gentzkow et al. (2012) estimate that more than 80% of current newspapers are read in counties in which they were printed, and this estimate is likely larger for our period of study.

Table 1.1 shows the trends in most of our outcome and explanatory variables, it does not show statistics on mid-term elections but those are included in our sample. Voter turnout decreases significantly over our sample period, and newspaper circulation significantly increases. By comparing the change in daily newspaper circulation to that of biweekly and three-times weekly newspapers, we can see that the increase in circulation is driven entirely by the expansion of daily papers (as expected from the work of Fuller (1964)). Table 1.2 provide the average treatment for counties, as measured by the allocation of routes in 1908. The average number of routes was about 14, while 81% of all counties received at lease one route.

1.4.1.2 Fixed Effects Results

First, we consider voter turnout in Congressional elections, using as our dependent variable the percentage of eligible, voting-age adults who cast a vote in elections. Table 1.3 shows the OLS regression results; an additional route is correlated with a decrease in the percent of eligible voters who cast ballots by 0.0503 in Congressional elections (compared to a mean of 59.4). A 1 standard deviation change in the number of routes results in a 0.719 percentage point change in turnout. However, this result is not statistically significant. We also convert our route variable into a dummy variable equal to 1 if a county has a route, and 0 otherwise. Our results are roughly consistent with the use of a continuous independent variable. Receiving RFD at all is associated with a 2.484%, statistically significant drop in voter turnout.

Next, as seen in table 1.3, we turn to measurements of election competition. Specifically, we construct a set of variables that measure the number of candidates who receive vote shares above certain thresholds. Since any threshold is arbitrary, we use several (5, 10, and 20 percent). These thresholds have no political significance; we are using them only to measure the number of parties that achieve a level of political support within a county.

The results from the OLS regression of the effect of routes on the number of parties, shown in columns 3-5 of table 1.3, show that more routes are associated with broader support for parties, as counties receiving more routes change their voting behavior by voting for a wider variety of parties. Regardless the of threshold, the coefficient on RFD routes is statistically significant, with an additional route being associated with an increase in the number of competitive parties within a county of between 0.0041 and 0.0059.

To better understand these findings, we consider the vote share of small parties, which we identify as any party other than Republican and Democratic parties. Using this vote share (out of 100) as the dependent variable, we use the same specification as before. The motivation behind this dependent variable is to determine if some parties benefit more than other from the increase in information. Lower information transmission costs may be more beneficial to small parties, whose low visibility may have made it difficult to attract votes before the introduction of RFD. Additionally, voters' ability to coordinate behind candidates could increase with the introduction of RFD, especially for less visible candidates.

The results are presented in column 6 of Table 1.3. The coefficient of 0.12 is statistically significant, with a t-statistic of 3.18. This coefficient means that a one standard deviation change in the number of routes is associated with a 0.13 standard deviation change in the vote share of small parties within a county. Taken along with the results from columns 3-5, we can see that counties that receive more routes change their voting behavior by voting for a wider range of parties, to the benefit of smaller political parties.

The effect of RFD on turnout may seem counter-intuitive. However given the endogenous nature of route allocation, we cannot interpret the OLS estimates as measuring causal effects, and previous research suggests a downward bias to all of our estimates. Kernell and McDonald (1999) provide evidence that Congresspeople facing competitive elections prior to the establishment of RFD were more motivated in acquiring routes for their districts. Voter turnout is typically higher in competitive elections, as is the number of competitive parties. This means that we should expect to see above average voter turnout and competitiveness in the years before RFD associated with high levels of route allocation. If these variables drop in the period after RFD, either because politicians have bought votes and reduced competition, or simply because of regressions to the mean, OLS estimates of each of our effects would be downwardly biased. We can test this hypothesis in with our data, this is discussed below.

We next test the Kernell and McDonald (1999) hypothesis that that Congresspeople facing competitive elections prior to the establishment of RFD were more motivated in acquiring routes directly. Using a cross-section of our data (using only 1908 values) we regress the number of routes allocated to a county as a function of community characteristics and a dummy variable equal to 1 if the county was within a district with a competitive election before the introduction of RFD, and 0 otherwise. Specifically, we construct a set of dummy variables, indicating whether a district had an election with a margin of victory of 10 percentage points or less, 5 points for less, and 2.5 or less in any of the four elections prior to the establishment of RFD (1894, 1896, 1898, 1900).

Table 1.4 presents the results for this regressions. When we define close elections broadly, we fail to observe a strong positive relationship between the closeness of elections and route allocation. However, when we look only at very close elections, we see a significantly positive relationship. Counties within districts that had an election decided by 2.5 percentage points or less enjoy an average of 2.2 more routes than counties without close elections, even after controlling for county characteristics. These results support the claim that our OLS results for voter turnout are downwardly biased.

1.4.2 Instrumental Variables Estimation

To address the bias of the fixed-effects coefficients, we use three sets of instruments for the number of routes a county receives. In choosing suitable instruments, we focus on the requirement that routes be placed along passable roads. The ability for communities to successfully petition for an RFD route will be correlated to the quality of roads over that time period. With the existence of place fixed effects, our goal is to find variables that will affect road quality in a time-invariant way. Therefore, even if the variable is correlated with levels of political activity, it will fall into the place fixed effect, and will be uncorrelated with the error term in our second stage regression.

First, we take the average rainfall in a county, as measured in 1901. In 1902, RFD became a permanent service of the Post Office. A large number of routes were petitioned for, and subsequently approved or rejected in the years immediately following. Approximately 45 percent of these petitions (once they were approved by Congresspeople) were rejected by postal officials, often because of poor road conditions. Areas that received rainfall quantities that were best suited for road construction therefore received more routes.

The effect of rainfall on road quality, and therefore route allocation, relates to the road construction technology at the time. King-road drags, a double 'bladed' style of drag, were popular of a range of machines that grated and smoothed roads, maintenance the farmers themselves were responsible for. According to the Farm and Garden Rule-Book (Baily, 1919),

> ...[the king road drag] does the best work when the soil is moist and yet not too sticky. This is frequently within a half-day's time after a rain. When the earth is in this state it works the best, and the effects of working it are fully as beneficial as at any other time....It often takes a whole season for the road to become properly puddled and baked to withstand the rains and traffic. After a road has been worked with a drag only a short time, it is not well to expect it to stand up to heavy traffic during a continued damp spell without being affected.

Therefore, we would expect counties with both very high and very low levels of rainfall to be allocated fewer routes. Figure 1.2, which shows the correlation between rainfall and route allocation, supports this hypothesis. Each point represents the average number of routes per thousand for 25 different bins, as determined by the amount of rainfall. Communities with moderate amounts of rainfall received more routes per person than communities with very high or very low amounts of rain. With this in mind, we will use rainfall (in inches) and its square as one set of instruments.

Second, we use data on the county-level spending of roads and bridges in 1890. By that time, roads and highways funded by the federal government had become severely neglected, and the federal and state governments had largely left any road funding to counties and townships (Fuller, 1955). Because 1890 is well before the establishment of even the first experimental RFD routes, and before the creation of the Office of Road Inquiry in 1893, which would later become the National Highway Administration, it would have impossible for county officials to have anticipated federal help or to build roads in anticipation of preferential rural route allocation. Additionally, with the establishment of the Office of Road Inquiry, government responsibility for roads were no longer fell on counties, so the concern for auto-correlation of county spending in years during our sample is minimized.

Our final instrument is a set of laws that outline the statutory environment in each state at the onset of RFD route allocation. According to the Office of Road Inquiry, most state laws concerning the establishment of roads before 1885 were largely ineffectual. Between 1888 and 1895, almost every state passed numerous laws related to roads. The nature of these laws would have lasting impacts on the later ability of rural communities to establish roads. Therefore, these laws can be used as instruments for route allocation. These laws, which are at the state level, will be used in combination with the county-level instruments. In the case of the 1890 spending variable, with many zeroes in the data, this will provide valuable variation.

Thus, the first stage of our two stage least squares estimation will be:

$$Routes_{ct} = \phi Rainfall_c * Post_{ct} + \sigma Rainfall_c^2 * Post_{ct} + \delta_t + \gamma_c + \beta \mathbf{X_{ct}} + e_{ct} \tag{1.2}$$

or

$$Routes_{ct} = \phi \mathbf{Laws_c} * Post_{ct} + \sigma Z_c * Post_{ct} + \delta_t + \gamma_c + \beta \mathbf{X_{ct}} + e_{ct} \tag{1.3}$$

where $Routes_{ct}$ is the number of routes in county c and year t; $Rainfall_c$ is the amount of rain in 1901; $Laws_c$ is the set of law dummies; Z is our county-level instrument; δ_t and γ_c are time and county fixed effects; $\mathbf{X_{ct}}$ is the set of covariates used in our second stage. We interacted each of our instruments with a $Post_{ct}$ dummy variable, equal to 1 if the year is

1908 or after, and 0 otherwise. Due to the existence of place fixed effects, we are essentially estimating the change in routes between the pre- and post-rollout years.

The results from this first stage regression are provided in Table 1.5. This shows that each of our instruments is a strong predictor of route allocation. As expected the coefficient on rainfall is positive, while its square is negative, indicating that counties with moderate rainfall receive the most routes. Increased 1890 spending in roads and bridges is associated with increased RFD route allocation a decade later. The coefficients on the set of law dummy variables reveals a complex relationship between the statutory environment and the allocation of routes. While some laws, specifically the creation of road commissioners and offices that oversee the construction and maintenance of roads, leads to increases in the number of routes, several laws (the creation of road districts, the use of state money, and the use of convict labor) actually lead to decreases in RFD routes. This result may be attributable to the legislatures focusing on highway, as opposed to local road, construction. Convict labor, for instance, was often used to break rocks, which were far more useful in the construction of highways than for use in rural roads.[7] As the F-statistics show, while each of our county-level instruments is sufficiently strong, the set of state laws is too weak to be used as an instrument by itself. Therefore, we use this state-level instrument in conjunction with each of our two county-level instruments.

Given the existence of time fixed-effects in our analysis, the identifying assumption for our specification is that an instrument for the number of routes be uncorrelated with trends in our outcome variables; time-related shocks must be identical across treatment groups. This allows us to select instruments that are correlated with levels of our outcome variables, which is typically a violation of the exclusion restriction, provided they are uncorrelated with trends. Therefore, we can compare pre- and post-treatment trends across different values of our instrumental variables. See appendix A.2 for further discussion.

[7]We will present the Cragg-Donald Wald statistic with our IV regressions.

1.4.2.1 Data

The rainfall dataset came from the U.S. Historical Climatology Network, which provides monthly and annual rainfall data for each climatological station in the U.S. For counties with one station, the average monthly rainfall in 1901 was recorded as the rainfall for that county. When more than one station existed within a county, we took the average of all stations within the county. For counties with no climatological stations, we took the averages of all values for contiguous counties. The dataset on county-level spending on roads and bridges was constructed using the Report on Wealth, Debt, and Taxation at the Eleventh Census, 1890: Valuation and Taxation.

To determine the state laws passed with regards to local road construction, we use a unique set of documents that provide data on laws passed by state legislatures in the period immediately before the establishment of the first RFD routes. "State Laws Relating to the Management of Roads: Enacted in 1888-1893" (Stone, 1894), and "State Laws Relating to the Management of Roads: Enacted in 1894-1895" (Stone, 1896), both published by the U.S. Department of Agriculture, Office of Road Inquiry, and "The Report of the Industrial Commission on Prison Labor" (Pri, 1900). These documents provide a thorough account of the legislative actions taken on the state level Using the data from these reports, we construct a set of dummy variables indicating whether a state had passed a law governing a specific characteristic of local road governance. After reviewing the laws, we found that all relevant legislation fell into one of the following categories.

1. Establishment of road commissioners, or empowering county commissioners to govern roads; in smaller states this took the form of the establishment of state road offices

2. Outlining road quality rules, or establishing office of overseer or viewer

3. Creation of road districts

4. Allocating convict labor for the use of road construction

5. Allocation of state money for road construction

We constructed a dataset with a full set of five dummy variables, each equal to 1 if a state passed a law concerning each aspect of road construction, and 0 otherwise.

Figure 1.3 provides an overview of the behavior of state legislators with regards to road construction, and reveals several interesting regional patterns. First, Southern states, where poor road quality was continually noted as an impediment to the approval of petitions for RFD routes, had few laws governing the construction of roads. All Gulf Coast and Southern Atlantic states, from Texas to Virginia, had no more than one law type on the books in 1896. Most of these laws concerned with the use of convict labor which likely did little to help establish suitable roads in locations where RFD routes were demanded. Midwestern states, with the exception of Illinois (which issued recommendations, but passed no laws) and Ohio, passed legislation, in most cases several laws. Western states seemed particularly proactive in passing legislation.

1.4.2.2 IV Results

The results from the IV regression of voter turnout on the number of routes are presented in Table 1.6.[8] Column 1 shows the results when rainfall, its square, and the set of state laws are the set of instruments; column 2 shows the results when 1890 county-level spending and state laws are the set of instruments. The negative correlation seen in the OLS results disappears, and we now observe a positive causal effect. When using rainfall as an instrument, an additional route leads to a 0.11 increase in turnout, and when using spending, we find a 0.54 increase, though statistical significance is only observed when using county-level spending as an instrument. A one standard deviation change in routes leads to a 1.57 and 7.7 percentage point change in turnout, respectively. While the direction of our results are robust to the choice of instruments, we see significant variation in the point

[8]Regressions for the instrumental variable modes were calculated using STATA's xtivreg2 command. Residual sum of squares is calculated using the structural equation, instead of the residuals for second-stage regression. Therefore, the residual sum of squares could be greater than the total sum of squares, resulting in a negative model sum of squares, and therefore a negative r-squared. Woolridge (2006) warns against making statistical judgments from r-squared in IV regressions, since its value does not have the standard interpretation of the squared correlation coefficient, and the negative values do not mean that the model in fact performs badly.

estimate. Given the heterogeneity across our sample, which includes most counties in the country, there is likely similar variation in how the instruments affect route allocation. For instance, New England counties varied little in the value of 1890 county spending, since funding decisions were typically made by the state and township. Rainfall varied little across the northern Midwest, so this instrument would likely be less useful in explaining variation in route allocation in this region. Since the instruments affected different populations, the two coefficients, instead of being interpreted as different estimates of the same average treatment effect, can be better described as different estimates of separate local average treatment effects.

The IV results for the number of parties receiving votes, shown in table 1.7 match the OLS finding of a positive causal effect, and also suggest a downward bias of the OLS regressions. Neither the sign nor statistical significance of each of our coefficients changes from the OLS regression, and the findings for both set of IV are larger by a factor of about four. The point estimates range from 0.013 to 0.027, depending on the threshold and instrument used, meaning that a one standard deviation increase in the number of routes leads to an increase of between 0.2 and 0.4 in the number of parties competitive in an election. Again, because these vote share thresholds have no inherent meaning, we are using them to show that our results for the number of parties competing within a district are robust to the threshold used to define it.

Table 1.8 present the IV regressions when using the vote share of small parties, which are consistent with downward bias of OLS estimates. The point estimates of 0.58 and 0.9 represent significant increases over the OLS estimate of 0.12. While the coefficients for the IV regression may appear at first very high (the average vote share of small parties was only 3.15 in 1908), the magnitude is not very different than the coefficients for the other IV regressions; a one standard deviation change in the number of routes leads to a change of between 0.60 and 0.93 standard deviations in the vote share of small parties.

To summarize, the IV regressions for each of our county-level voter behavior variables show the presence of downward bias in OLS regressions, consistent with previous research.

The causal effect of RFD routes is a slight increase in voter turnout, and the statistical significance of this finding depends on the instrument used. For each of our measures of the distribution of votes across a county, the IV regressions are roughly consistent with the OLS findings: route allocation leads to a wider distribution of votes across parties, with increased vote shares for small parties. These findings are robust to the choice of instruments.

1.5 Potential Mechanisms

The results presented up to this point have not made any attempt to disentangle the mechanisms through which RFD affected political behavior. Therefore, it is difficult to determine if RFD changed political behavior because of increases in the mail, because of an increase in road quality, or for some other reason. Accounts from the turn of the twentieth century suggest that these changes were largely driven by increase in the amount of information that farmers received, notably from the newly feasible daily newspaper. Using a dataset on newspapers, which provided a valuable source of political information, we can compare how our previous results differ across counties with differing access to daily news.

Anecdotal evidence supports the hypothesis that the introduction of rural routes increased the circulation of newspapers. One of the first reports from local postmen on the effect of RFD included the following statement by a postal worker in Oregon (Yea, 1903):

> Before free delivery was started there were 13 daily papers taken at Turner (OR) post office. Today there are 113. This shows that the farmers are getting in touch with the world and are quick to avail themselves of all educational facilities.

In Table 1.9, we see that one additional route is associated with a 1.7 percent increase in total newspaper readership. Breaking these results down between daily and semi-weekly newspapers, it is clear that all of this effect comes from an increase in daily newspaper readership.

The potential for newspapers to impact political behavior follows directly from their role as a conveyor of information about policy debates, news of social or political importance, and even candidate's behavior. For example, over a one week span in 1904, the Bemidji (MN) Daily Pioneer included stories about the Wisconsin Secretary of State completing the state's ballot, an Indiana Senator speaking at Indiana University, and an illness contracted by a Minnesota gubernatorial candidate (Bemidji Pioneer, October 24 – October 28, 1904).

To test the hypothesis that newspapers were an important mechanism through which RFD routes affected political behavior, we divide our sample into counties with newspapers and those without newspapers. To ensure that our sub-samples do not change over time, we define a county as having a newspaper only if it has a newspaper by 1900. We then run the IV regressions on each of the subsections separately. We then perform the IV regressions outlined above, using spending as a an instrument (using rainfall is used as an instrument, does not significantly change our results and are not presented here).

Out results, shown in Table 1.10 and Table 1.11 show that, for several of the outcome variables, the results differ dramatically across sub-samples. For one of our measures of election competition, the number of parties receiving votes, we observe different causal effects. About 75 percent of counties take on values of 0 for the newspaper dummy variable, and the estimates are about as precisely estimates as in previous IV regressions, so these findings are not the results of a loss of precision in our estimates.

If RFD only affected political behavior through the effect of better roads (or any other mechanism that would be independent of newspapers) we would expect the coefficient on the number of routes would be identical for both groups. However, for each variable describing the distribution of votes, the causal effect in counties with newspapers differs significantly from counties without newspapers.

These results suggest that changes in voting behavior caused by RFD were stronger (and in some cases only present) if a county had a daily newspaper. There are two potential explanations for these results: people in counties with newspapers react differently from identical treatment than people living with counties without newspapers; or, the presence of newspapers affects the nature of treatment by serving as a mechanism through which political information can be transmitted.

1.5.1 Congressional Votes

Each of the results to this point have focused on the behavior of voters. We look now at the behavior of elected officials. With richer information networks, voters may select different attributes for their Representatives, or they may elicit different actions from elected officials. Taking our motivation from Strmberg (2004), we consider the potential effect that better informed voters may have on Representatives. Voters may punish Representatives who act against the voters' wishes, but will only do so if they are aware of the representative's actions. Therefore, if one subset of voters receives a positive shock in their access to information, we may expect to observe a shift in the policy positions of elected officials (especially if Representatives are office-motivated) towards positions favorable to this better-informed subset. Additionally, the increase in turnout by counties may change the identity of the median voter, resulting in changes of the characteristics of the elected official. By using DW- Nominate scores, which measures the voting behavior of officials along a 2-dimensional policy space, we measure the effect of rural routes on the policy decisions of representatives.

The DW-Nominate scores give each elected member of the House of Representatives two scores, which represent their policy stances based on roll-call votes, over two dimensions. The first dimension represents the traditional liberal-conservative stances. The second dimension represent on traditionally social issues (or issues that are less likely to cut strongly across party lines), and the specific stance associated with this dimension changes over time. For our analysis, we will focus only on the first dimension. We will therefore use the DW-Nominate first dimension score as our dependent variable in both the OLS and IV specifications above, along with a set of political party dummy variables, equal to 1 if the Representative is a member of the party, and 0 otherwise. Because the DW-Nominate score only varies at the congressional district level, we aggregate each of our county-level variables up to the district level. For counties that straddle more than one congressional district, we divide each variable into the number of districts into which the county is split, and distribute those values evenly across the districts.

To motivate our results, we first consider what stances were typically associated with rural communities over our sample period. Figure 1.4 shows the correlation between the percentage of urban residents in a district, and the policy stances of elected officials. Over the first dimension, we see that, after controlling for party membership, less urban districts feature more negative Nominate scores. Therefore, we would expect that either an increase in political power of rural voters, or a rural shift in the identity of the median voter would lead to a negative shift in the policy scores of elected officials.

Table 1.12 shows the effect of routes on the policy decisions of members of the House of Representatives. No strong correlation is observed in the OLS results for the effect of RFD routes over either dimension, as neither result is statistically significant. Our instrumental variable results, however, show strong causal effects. Districts with more RFD routes see negative shifts in the DW-Nominate scores of their elected officials. Because our regression includes dummies for party affiliation, this result cannot be the result of shifts from one party to another. The point estimates for the IV regressions of -0.00021 and -0.00058 indicate that a one standard deviation change in the number of routes leads to a change of between 0.035 and 0.095 of one standard deviation in the dependent variable. Therefore, conditional on the assumption that a negative shift in DW-Nominate scores indicate more rural-friendly stances, an increased number of routes causes the elected officials adopt policies more in line with rural voters.

1.6 Concluding Remarks

Though the dramatic impact that Rural Free Delivery had on easing the isolation of rural communities has been widely recorded and discussed, very little research has explored the impact of RFD on political outcomes. Using a panel data set on RFD route allocations election returns, newspaper circulation, and county characteristics, we find that routes led to significant changes in a several crucial ways.

We find that our results are robust to the choice of instruments; RFD leads to increases in voter turnout, a wider distribution of votes, and shifts in the behavior of elected officials towards policies associated with rural communities. The IV coefficients measured do not provide causal effects for information networks directly, but for the establishment of RFD routes. Therefore, the extent to which we can take these findings as the causal effect of information flows are limited to the extent that RFD changed the structure of rural communities by changing their access to information. For the county-level measures of election competition, our results differ between counties with and without newspapers. These findings provide evidence that newspapers potentially provide an important mechanism.

Figure 1.1: Rollout of RFD

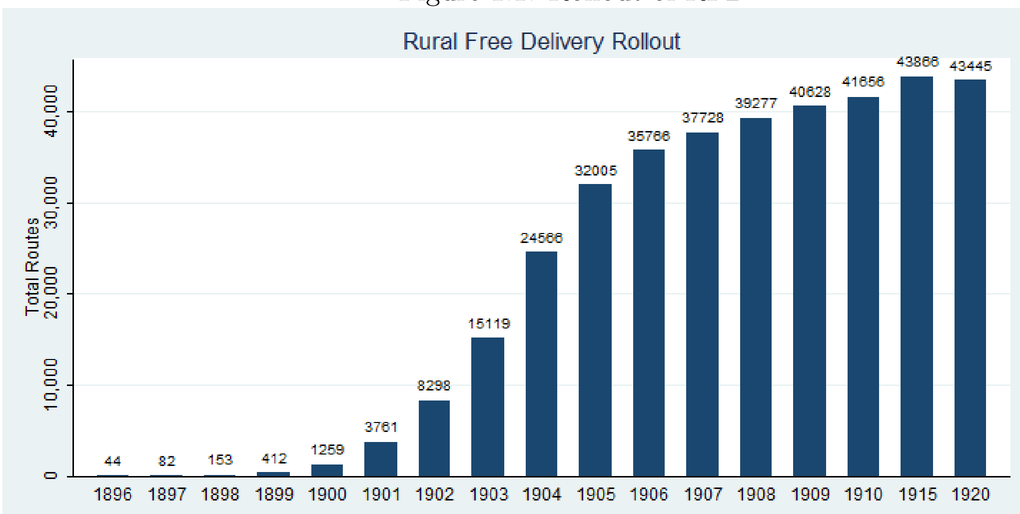


Figure 1.2: Rainfall and Route Allocation

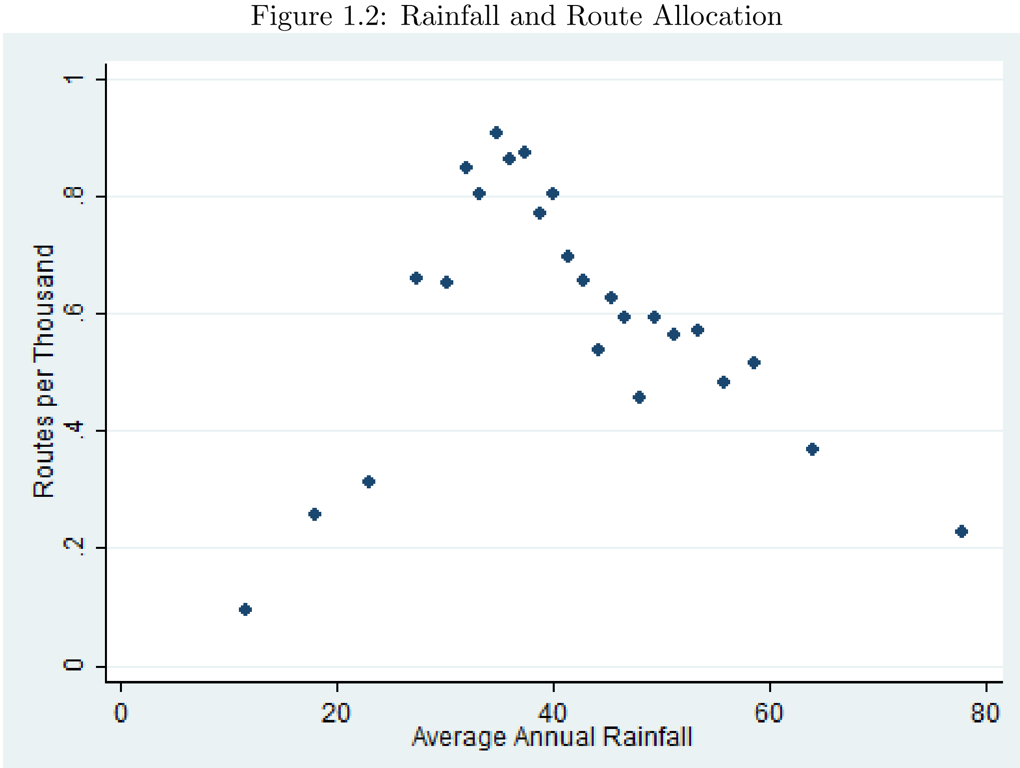


Figure 1.3: Number of State Laws

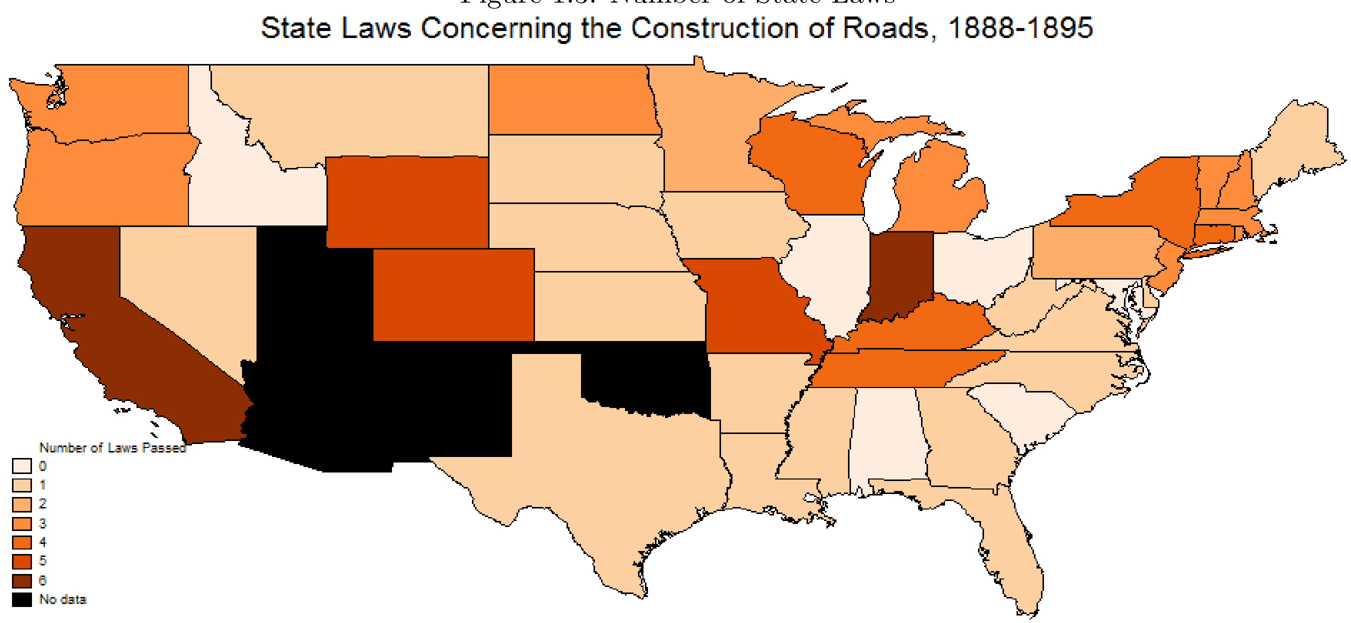
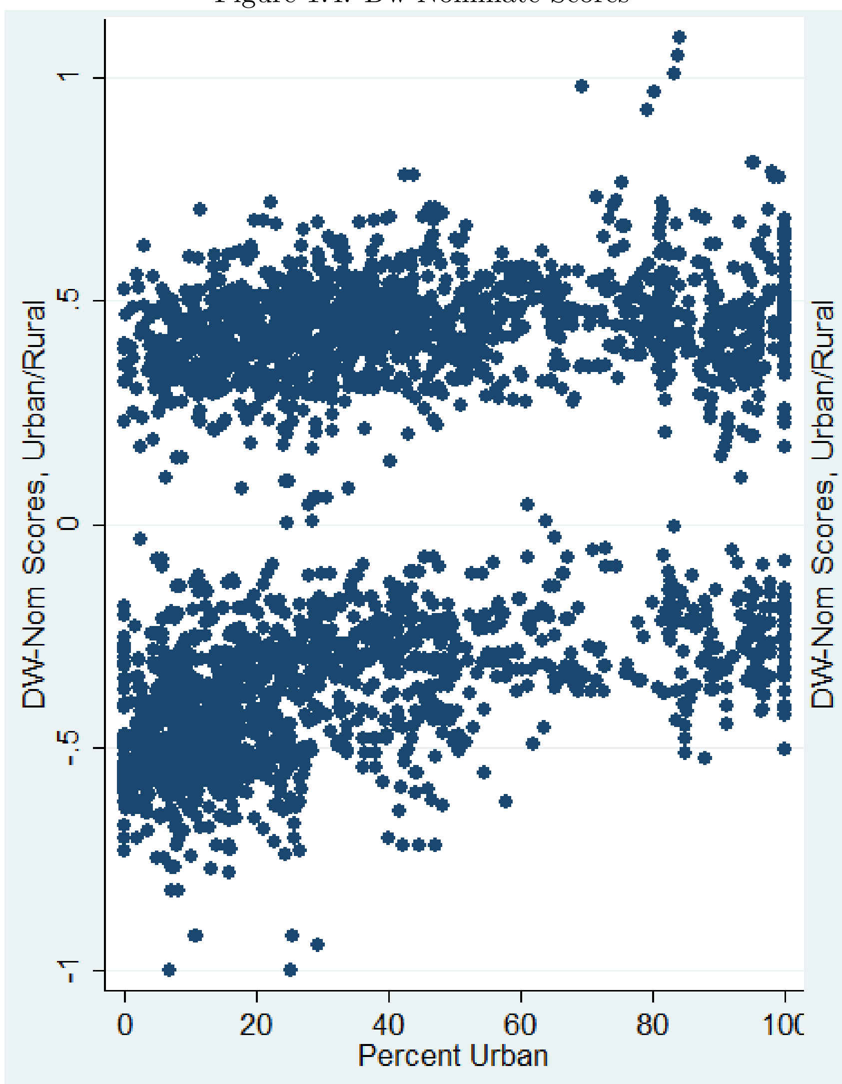


Figure 1.4: Dw-Nominate Scores



1.7 Tables

Table 1.1: Means by Year

YEAR	1892	1896	1900	1908	1912	1916
Congressional Turnout	68.02	72.15	68.96	60.55	54.56	58.87
	(22.21)	(21.59)	(22.97)	(24.17)	(21.93)	(21.38)
Candidates	2.39	2.19	1.98	2.02	2.62	2.07
	(0.58)	(0.47)	(0.37)	(0.53)	(0.92)	(0.62)
Small Party Share	12.59	10.03	2.14	3.15	14.86	5.20
	(16.13)	(17.20)	(5.56)	(5.40)	(14.77)	(12.44)
Total Newspaper Circ	1,774	2,207	4,356	7,391	9,039	10,988
	(9,869)	(12,097)	(42,102)	(72,529)	(88,091)	(102,439)
Daily Newspaper Circ	1,746	2,176	4,312	7,350	9,001	10,968
	(9,848)	(12,083)	(42,098)	(72,529)	(88,077)	(102,440)
Percent Improved Farmland	55.64	52.90	52.82	56.14	56.51	57.35
	(22.59)	(23.56)	(24.80)	(24.21)	(24.41)	(23.82)
Percent Urban	12.46	12.69	14.22	15.98	18.35	19.13
	(20.92)	(21.21)	(21.44)	(22.77)	(23.62)	(24.25)
Ln(Population)	9.55	9.58	9.62	9.78	9.81	9.84
	(1.12)	(1.13)	(1.13)	(1.00)	(1.03)	(1.04)
Non-white	9.92	11.97	11.06	10.67	9.26	8.75
	(17.52)	(19.93)	(18.92)	(18.66)	(17.30)	(16.32)
Percent Foreign	11.59	10.77	9.63	9.21	9.38	8.73
	(12.41)	(11.52)	(10.47)	(9.78)	(9.40)	(8.73)
Observations	2,162	2,249	2,308	2,342	2,191	2,148

Note: Because there are some missing counties in the election data, the number of observations is not identical for each year.

Table 1.2: Summary of Rural Free Delivery Allocation in 1908

	(1)
RFD Routes	14.36
	(14.09)
Percent of Counties with Routes	81
	(39)
Observations	2,422

Table 1.3: Fixed Effects Results

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Turnout	Turnout	>5 Percent	>10 Percent	>20 Percent	Small Party Share
RFD Routes	-0.0503		0.00432***	0.00589***	0.00407***	0.121***
	(0.0574)		(0.00159)	(0.00134)	(0.00113)	(0.0380)
RFD Dummy		-2.484**				
		(1.185)				
% Urban	-0.137**	-0.119*	-0.00681***	-0.00332*	-0.00191	-0.0612
	(0.0642)	(0.0622)	(0.00235)	(0.00171)	(0.00159)	(0.0457)
% Urban Squared	0.00177	0.00117	0.000214***	0.000128***	7.58e-05***	0.00394***
	(0.00113)	(0.00114)	(4.07e-05)	(3.21e-05)	(2.72e-05)	(0.000844)
% Improved Farmland	0.0672	0.0835	-0.00644***	-0.00587***	-0.00393**	-0.139**
	(0.0847)	(0.0839)	(0.00246)	(0.00211)	(0.00183)	(0.0620)
% Non-white	0.0227	-0.000318	0.00449	0.00237	0.00163	-0.177
	(0.264)	(0.268)	(0.00550)	(0.00557)	(0.00642)	(0.148)
% Foreign	-0.647***	-0.635***	-0.00144	-0.00966**	-0.0124***	-0.0910
	(0.159)	(0.160)	(0.00527)	(0.00416)	(0.00346)	(0.179)
Ln(Population)	-4.125	-4.022	0.218***	0.0863	0.0200	0.660
	(3.941)	(3.953)	(0.0675)	(0.0746)	(0.0798)	(1.283)
Observations	22,433	22,433	22,433	22,433	22,433	22,433
R-squared	0.774	0.775	0.434	0.434	0.514	0.365
Clusters	289	289	289	289	289	289

Standard errors, clustered at Congressional District level, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.4: Determinants of Route Allocation

	Margin of Victory and Route Allocation		
	Dependent Variable: Number of Routes		
VARIABLES	10 Percent	5 Percent	2.5 Percent
Close Election	0.0513	.977	2.207***
	(0.771)	(.780129)	.8357712
% Urban	-0.0346***	-0.0345***	-0.0343***
	(0.0114)	(0.0114)	(0.0113)
% Nonwhite	-0.126***	-0.124***	-0.124***
	(0.0147)	(0.0147)	(0.0146)
% Improved Farmland	0.211***	0.211***	0.211***
	(0.0122)	(0.0122)	(0.0122)
% Foreign	-0.271***	-0.270***	-0.268***
	(0.0325)	(0.0325)	(0.0324)
Ln(Population)	5.857***	5.849***	5.833***
	(0.294)	(0.294)	(0.294)
Observations	2,574	2,574	2,574
R-squared	0.615	0.616	0.617
% of Counties With Close Elections	14.6	12.4	10.0

Standard errors, clustered at Congressional District level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.5: First Stage Regression

VARIABLES	(1) Instrument Rainfall	(2) Instrument Spending	(3) Instrument State Law
Rainfall	1.024***		
	(0.0618)		
Rainfall Squared	-0.0119***		
	(0.000675)		
Road Spending		0.000100***	
		(2.41e-05)	
Road Districts			-6.815**
			(2.866)
State Money			-6.677**
			(2.722)
Convict Labor			-6.474***
			(2.072)
Oversight			0.606
			(2.745)
Governance			7.035**
			(3.161)
% Urban	0.244***	0.292***	0.252***
	(0.0277)	(0.0497)	(0.0357)
% Improved Farmland	-0.0379*	-0.0578*	-0.0484
	(0.0223)	(0.0338)	(0.0475)
% Nonwhite	0.230***	0.662***	0.377**
	(0.0619)	(0.136)	(0.145)
% Foreign	-0.0129	0.154*	0.249
	(0.0627)	(0.0877)	(0.194)
Ln(Population)	-7.805***	-11.30***	-9.250***
	(0.778)	(0.958)	(1.402)
Observations	21,824	11,096	22,275
F-Stat (excluded instruments)	157.52	34.38	4.06
R-squared	0.775	0.781	0.778
Counties/States	2409	1151	43

Standard errors, clustered at Congressional District level, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.6: IV Regression: Turnout

VARIABLES	(1) Instrument: Rainfall	(2) Instrument: Spending
RFD Routes	0.110*	0.542***
	(0.0573)	(0.0733)
% Urban	-0.142***	-0.214***
	(0.0353)	(0.0381)
% Improved Farmland	0.0407	0.0923**
	(0.0391)	(0.0410)
% Nonwhite	0.00425	-0.204
	(0.133)	(0.138)
% Foreign	-0.569***	-0.629***
	(0.0715)	(0.0818)
Ln(Population)	-1.989	1.872
	(1.495)	(1.397)
Observations	21,671	22,212
R-squared	0.303	0.274
Counties	2334	2403
Wald Stat.	76.91	51.89

Standard errors, clustered at Congressional District level, in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 1.7: IV Regression: Competition

IV Regression, Election Competition

Instrument VARIABLES	Rainfall > 5 Percent	Rainfall > 10 Percent	Rainfall > 20 Percent	Spending > 5 Percent	Spending > 10 Percent	Spending > 20 Percent
RFD Routes	0.0147**	0.0208***	0.0128***	0.0232***	0.0272***	0.0176***
	(0.00620)	(0.00530)	(0.00388)	(0.00658)	(0.00551)	(0.00441)
% Urban	-0.00634***	-0.00246	-0.00126	-0.00558**	-0.00185	-0.000926
	(0.00239)	(0.00181)	(0.00157)	(0.00255)	(0.00197)	(0.00165)
% Urban Squared	0.000138**	1.90e-05	8.74e-06	8.38e-05	-1.94e-05	-1.78e-05
	(5.53e-05)	(4.45e-05)	(3.30e-05)	(6.24e-05)	(5.01e-05)	(3.79e-05)
% Improved Farmland	-0.00703***	-0.00660***	-0.00422***	-0.00635***	-0.00577***	-0.00377**
	(0.00231)	(0.00190)	(0.00158)	(0.00246)	(0.00209)	(0.00178)
% Nonwhite	0.000207	-0.00360	-0.00200	-0.00228	-0.00547	-0.00336
	(0.00580)	(0.00579)	(0.00594)	(0.00633)	(0.00646)	(0.00633)
% Foreign	-0.000870	-0.00921**	-0.0118***	-0.000735	-0.00902**	-0.0118***
	(0.00528)	(0.00429)	(0.00359)	(0.00562)	(0.00459)	(0.00386)
Ln(Population)	0.354***	0.272***	0.137*	0.420***	0.313***	0.163**
	(0.0902)	(0.0881)	(0.0717)	(0.0971)	(0.0931)	(0.0803)
Observations	21,671	21,671	21,671	22,204	22,204	22,204
R-squared	0.153	0.094	0.049	-0.004	-0.048	-0.035
Counties	2,334	2,334	2,334	2,403	2,403	2,403
Clusters	285	285	285	285	285	285

Standard errors, clustered at Congressional District level, in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 1.8: IV Regression: Small Party Share

Instument	Rainfall	Spending
RFD Routes	0.584***	0.916***
	(0.0668)	(0.0776)
% Urban	-0.0505	-0.148***
	(0.0357)	(0.0395)
% Imp. Farmland	-0.0524*	-0.0273
	(0.0280)	(0.0298)
% Nonwhite	-0.235***	-0.354***
	(0.0901)	(0.101)
% Foreign	0.252***	0.264***
	(0.0634)	(0.0707)
Ln(Population)	2.991***	6.139***
	(1.018)	(1.052)
Observations	21,671	22,204
R-squared	-0.0274	-0.1458
Counties	2334	2403

Standard errors, clustered at District level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.9: Effect of RFD on Newspaper Readership

VARIABLES	(1) Total Circulation	(2) Daily Circulation	(3) Semi-Weekly Circulation
RFD Routes	0.0179***	0.0184***	-0.00668
	(0.00325)	(0.00328)	(0.00985)
% Urban	0.0487***	0.0519***	-0.00191
	(0.00558)	(0.00577)	(0.0101)
% Imp.Farmland	0.000376	-0.00300	0.0277
	(0.00318)	(0.00306)	(0.0181)
% Nonwhite	0.0107	0.0104	0.0842
	(0.0118)	(0.0116)	(0.0604)
% Foreign	0.0186**	0.0180**	0.00467
	(0.00847)	(0.00845)	(0.0333)
Ln(Population)	0.203**	0.188**	-0.571
	(0.0867)	(0.0852)	(0.657)
Observations	22,433	22,433	1,665
R-squared	0.900	0.916	0.769
Counties	2490	2490	458

Standard errors, clustered at District level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.10: Effects By Newspaper Presence

Roll of Newspapers, IV Regression (Spending Instrument)

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	>5 Perc.	> 5 Perc.	> 10 Perc.	> 10 Perc.	> 20 Perc.	> 20 Perc.
RFD Routes	-0.00455	0.0245***	0.00609	0.0240***	0.00330	0.0131***
	(0.00680)	(0.00563)	(0.00561)	(0.00501)	(0.00474)	(0.00374)
% Urban	0.00192	-0.0103**	0.000668	-0.00221	0.000257	-0.00191
	(0.00325)	(0.00455)	(0.00289)	(0.00311)	(0.00264)	(0.00190)
% Imp. Farmland	-0.00493**	-0.0107***	-0.00510**	-0.00881***	-0.00381*	-0.00362**
	(0.00248)	(0.00270)	(0.00221)	(0.00239)	(0.00198)	(0.00172)
% Nonwhite	-0.00142	0.0155	-0.00377	0.00849	-0.00218	0.00988
	(0.00522)	(0.0170)	(0.00520)	(0.0190)	(0.00647)	(0.0143)
% Foreign	-0.00901*	0.00728	-0.0143***	-0.00300	-0.0146***	-0.00856
	(0.00481)	(0.00763)	(0.00395)	(0.00672)	(0.00327)	(0.00531)
Ln(Population)	0.155**	0.558***	0.114	0.278*	0.0391	0.0340
	(0.0779)	(0.164)	(0.0797)	(0.159)	(0.0815)	(0.114)
Observations	15,210	6,994	15,210	6,994	15,210	6,994
R-squared	0.031	-0.005	0.031	-0.048	0.018	-0.032
Counties	1,685	718	1,685	718	1,685	718
Newspaper	NO	YES	NO	YES	NO	YES
Clusters	233	260	233	260	233	260

Standard errors, clustered at Congressional District level, in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 1.11: Effect of Newspapers

Roll of Newspapers: IV Regression (Spending Instrument)

	(1)	(2)
VARIABLES	Small Party Share	Small Party Share
RFD Routes	0.569**	0.536***
	(0.249)	(0.122)
% Urban	-0.00795	-0.195**
	(0.0911)	(0.0819)
% Improved Farmland	-0.125*	-0.159**
	(0.0659)	(0.0710)
% Nonwhite	-0.336*	-0.250
	(0.176)	(0.427)
% Foreign	-0.120	0.0726
	(0.190)	(0.180)
Ln(Population)	5.087*	4.892*
	(2.754)	(2.523)
Newspapers	No	Yes
Observations	15,210	6,994
R-squared	-0.020	-0.037
Counties	1,685	718
Clusters	233	260

Standard errors, clustered at Congressional District level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.12: Policy Decisions and Route Allocation
Dependent Variable: DW-Nominate Score

	(1)	(2)	(3)
VARIABLES	OLS	IV	IV
Routes	0.000114	-0.000938**	-0.00191***
	(0.000221)	(0.000432)	(0.000689)
% Urban	0.00130	0.00161	0.00190*
	(0.00111)	(0.00106)	(0.00113)
% Imp. Farmland	-0.00231*	-0.00167	-0.00109
	(0.00129)	(0.00119)	(0.00133)
% Nonwhite	-0.00360	-0.00781*	-0.0117**
	(0.00434)	(0.00432)	(0.00490)
% Foreign-born	-0.00656*	-0.0100***	-0.0132***
	(0.00358)	(0.00372)	(0.00424)
Ln(Population)	0.0128	-0.00618	-0.0237
	(0.0335)	(0.0317)	(0.0345)
Observations	2,984	2,975	2,975
R-squared	0.706	0.151	0.104
Rainfall	NO	YES	NO
Spending	NO	NO	YES
Districts	368	359	359

Standard errors, clustered at Congressional District level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Chapter 2

Duvergers Law and Strategic Voting: an Empirical Test Using Floridas Elimination of Primary Runoff Elections

2.1 Introduction

Strategic voting plays a central, if often problematic role in the discussion of optimal voting mechanisms. The efficiency of voting systems, predictions of theoretical models, and policy recommendations may differ depending on the extent of strategic voting.

This paper provides an dual test of Duverger's Law & Hypothesis, one of the best-known set theoretical predictions regarding strategic voting. The theory is a pair of corresponding predictions: Duverger's Law states that "simple-majority single-ballot favors the two party system", while Duverger's Hypothesis states that "simple majority with a second ballot or proportional representation favors multipartyism". One of the primary motivations for this claim is that voters will strategically avoid smaller parties in simple-majority, single-ballot elections, such as plurality elections, because they are likely to believe that a vote cast for a small-party candidate cannot affect the outcome of an election.

To test this claim, I consider the state of Florida's switch from two-round runoff[1]

[1]Runoff elections are used extensively throughout the world, and have become an increasingly popular method of choosing heads of state. Currently, 65% of countries electing presidents do so using a two-round runoff with a 50% first round threshold, while only 6% of democratic countries did so in 1950 (Bormann and Golder (2013)). Many newly established democracies, including Egypt and Guinea, use runoff elections. Runoff elections are also common in the United States and are used in New York, San Francisco, and Dallas, for mayoral elections and in the southern United States for party primaries.

elections to plurality elections in party primaries elections following the 2000 election. Prior to 2000, candidates for the general election were chosen using a two-round election. If no candidate earned 50% of the vote, a runoff round was held between the top two vote-getters. Following the law change, the candidate receiving the most votes in the first round won a party's nomination. This setting differs from previous research on the effect of runoff elections in several important ways. United States elections feature low voter turnouts compared to many other countries. Additionally, the setting investigated here consists only of primary elections, where each election has only one party.

Most empirical studies have supported the prediction that, compared to plurality systems, runoff elections lead to either more candidates or more support for lower-placing candidates. However, the magnitude of this effect has varied significantly over different samples and empirical strategies. Riker and Wright (1989) found that, from 1950 to 1982, all else equal, states with runoffs had 2 more candidates in their Democratic primaries in gubernatorial races than states with plurality systems. Somewhat surprisingly, Engstrom and Engstrom (2008) performed similar analysis on many of the same same statewide elections, during the period 1980 to 2002, and found that the average number of candidates was equal in states with runoffs and states with plurality primary elections, while previous research has studied general elections with numerous parties.

Abramson et al. (2010) uncovered convincing evidence of strategic voting using data from the American National Elections Survey. The survey asked respondents about both their political beliefs and their voting behavior during election years; during three of those years (1980, 1992, and 1996) third party candidates appeared on ballots nationwide. The authors showed that, while at least 95 percent of respondents whose favorite candidate came from a major party ultimately voted for that candidate, only about 60 percent of respondents whose favorite candidate was a third-party candidate ultimately voted for that candidate. In each case, the third-party candidate's chance of winning a plurality election was minimal, meaning that voters' decisions to abandon the candidate may have been strategic.

Several recent papers have employed natural experiments to test the causal effect predicted by Duverger's Law. Bordignon and Tabellinin (2009) and Fujiwara (2011) both employed regression discontinuity to determine the causal effect of switches between plurality and majority runoff elections on voter or candidate behavior in municipal elections in Italy and Brazil, respectively. Bordignon et. al. found that runoff elections lead, on average, to the addition of 1 full candidate in races, while Fujiwara found that races with runoff elections, while not featuring more candidates, exhibit increased support for candidates finishing outside the top two. Specifically, changing from a pluraility election to a runoff election increased the vote share for candidates outside the top two by 8.8 percentage points. Because the average vote share was 15.5 percent, this represents a more than 50 percent increase in the support of candidates outside the top two. This provides direct evidence on strategic voting; voters faced with 1 round (with no possibility of a second round) will be more inclined to vote for their favorite between the top two, even if that candidate is not their most preferred among all candidates.

Duverger's original hypothesis outlines two mechanisms through which runoff and plurality elections can yield a different number of candidates Duverger (1954). The "mechanical" effect, concerns the mapping of vote shares into seats. According to the hypothesis, winning parties are will be over-represented in terms of seats in bodies of government (e.g., a party that gets 51% in every election will have 100% of the seats). This effect could limit the establishment of numerous parties. The other effect, the "psychological" effect, captures the effect of voting systems on the behavior of candidates (who must decide whether to enter an election or not) and on voters (who must decide to vote sincerely or not).

Because the setting I am investigating here, primary elections, feature only a single party for each election, the mechanical effect cannot drive the results, since the setting itself limits the number of participating parties. Therefore, I am able to isolate only the psychological effect here.

2.1.1 Elimination of the Florida Runoff

Runoff elections had been used in primary elections in Flordia since the 1929. Like most southern states, Florida political parties elected their nominees using two-round elections, where the top two candidates participated in a runoff election if no candidate won a majority of the votes in the first round. The first round was traditionally held in the first week of September, with the second round (if necessary) the first week in October, in anticipation of the general election in November. General election winners were, and still are, determined by a plurality vote.

The 2000 Presidential general election triggered a reassessment of voting procedures. In that election, officials encountered an unusually high number of invalid punch-card ballots, where a voter's intent could not be determined. Compounding this complication was a lack of protocol specifying how recounts were to be held and voter intent was to be determined. This situation resulted in a lengthy and contentious re-count, which was only ended by the United States Supreme Courts ruling in Bush v. Gore on December 12, 2000. In an attempt to fix many of the problems that plagued the 2000 Presidential election, the Florida legislature enacted several changes to its voting procedure which targeted vote verification and ballor access. Punch card ballots were banned in favor of optical scan or touch-screen voting machines, and vote counting technology was standardized statewide, and voter registration and vote verification systems were streamlined.

Many other states undertook similar (and in some cases identical) measures to update their own voting systems, either on their own volition, or due to the Help America Vote Act, a federal law passed in 2002 that mandated many of the changes introduced in the Florida legislation. Given Florida's unique political setting, however, these changes to its voting procedure required a dramatic change in the timing of elections in Florida.

Because much of the HAVA legislation focused on procedures for vote tabulation and ballot access, election officials across the country were faced with potentially lengthy voter verification and vote tabulation procedures. Although many other states (including ones with primary runoffs) implemented similar procedures, no state conducted as many elections in as short a window as Florida. In most years, only 10 weeks separated the first round of primary elections and the general election; by comparison, the state of Texas usually holds its first round primary in March, and its runoffs in April. No other state with runoff primaries began is election season after the end of July.

Faced with the uncertainty of the impact that the legislative reform would have on the vote verification process, Florida lawmakers suspended the runoff election for the 2002 election cycle; after suspending the runoff election again in 2004, the legislature finally voted in 2005 to permanently eliminate runoff elections. In both 2002 and 2004, the suspension of runoff elections was made at the request of election supervisors, who claimed that conducting three elections during Floridas brief election window was infeasible. State Senator Ron Klein was quoted as saying, "The present system is not working because of the amount of time between the first and the second primary and the general election.[2]

Lawmakers chose to suspend, and ultimately eliminate one round of voting instead of moving both primary round to earlier in the year, due to their uncertainty as to how rescheduling would affect the composition of voters. Research by the University of Florida estimated that, in 2003, up to 1.5 million people, or nine percent of the population of the state, spent at least 30 days outside the state, usually in the summer months. A great proportion of these travelers were retirees, who are more likely to vote than younger residents, meaning that the effect of such a move on the population of voters would have a significant impact on turnout. The sitting incumbents in the Florida Congress disfavored a potential change in the demographic characteristics of voters. In reference to the decision to eliminate the runoff election, Governor Jeb Bush was quoted in 2005 as saying, "you would have to have the first primary at a time that would be, in Florida, difficult, like August 1. This would maintain the traditional date for our primary.[3]

Given the identification strategy of this paper, it is crucial that the decision elimination

[2]"Vote Eliminates Primary Runoffs". Sun Sentinel, April 29, 2005

[3]"Senate writes obituary for runoff elections". Tampa Bay Times, April 29, 2005

of the runoff election was unrelated to our outcome variables. If the runoff was eliminated because of changes in voter or candidate behavior, then my estimation process would suffer from endogeneity. However, in the case of Florida following the 2000 election, it is clear that the primary runoff was eliminated to accommodate potential complications in vote verification, which was instigated by the fiasco of the 2000 Presidential election. In other words, if the 2000 Presidential election not been so problematic (with many of the issues occurring in only one county, Palm Beach County), then Florida would likely still be conducting primary runoff elections. The changes in voting procedure occurred nationwide, and only Florida's traditional schedule of elections necessitated the elimination of runoff primaries in that state.

As stated earlier, eliminating the runoff election was not the only act of the Florida Congress following the 2000 election. The state also introduced no-excuse absentee ballots and provisional voting, each of which changed how elections were conducted. If these changes in election procedure were unaccounted for, estimates of causal effect would capture the effect of these changes as well, instead of isolating only the effect of eliminating the runoff election. Fortunately, other states changed major components of their election procedure at the same time. In the data section, I discuss how I utilize cross-state variation in voting laws to isolate the causal effect of runoff elections.

2.1.2 Theoretical Motivation

Duverger's Law and Duverger's Hypothesis, first laid out in his 1954 book Political Parties, says that plurality voting structures will give rise to two-party (or two-candidate) systems, while runoff elections favor "multipartism, or outcomes with more than two candidates receiving votes. One of the primary mechanisms through which these two voting structures give rise to different numbers of candidates is through strategic voting. If voters behave strategically, their votes will be determined not only by their preferences, but also by how their votes could affect an election outcome. Sincere voters will always vote for their most preferred candidate; strategic voters could vote for a candidate other than their most preferred, if doing so would lead, in expectation, to a preferable outcome.

Runoff and plurality elections would not result indifferent actions by sincere voters, given an identical set of candidates. However, strategic voters may react differently to the two systems. Take the following example: in a setting with mandatory voting, there are three potential candidates A, B, and C. Voters vary in their type. Voters are type α with probability .3, type β with probability .3, and type γ with probability .4. Types α and β represent a split majority (for type α, [A $\succ$ B $\succ$ C], while for β, [B $\succ$ A $\succ$ C]). Type γ is therefore the minority [C $\succ$ A $\sim$ B]. In a plurality election, where the winner is the candidate with the highest vote share, sincere voting results in the election of candidate C, who is both a Condorcet loser and the candidate least preferred by .6 of voters. Therefore, supporters of candidates A and B have strong incentives to rally around one of the two majority candidates; this would lead to the abandonment of either candidate A or B, as a way to avoid the election of C. Additionally, candidate A or B, anticipating such strategic abandonment, may choose to exit the election entirely.

Compare this setting to that of a two-round election with a runoff threshold of .5, where a runoff is triggered if no candidate receives a majority of the votes in the first round. With mandatory participation, voters know that candidate C would lose in a runoff to either candidate A or B; also, because candidate C does not have majority support, there is no risk of candidate C winning in the first round. In this setting, consider the incentives of a voter whose most preferred candidate is A. Suppose the voter does not know the underlying support of the candidates. If the voter votes for A, he believes he could be the pivotal voter in one of the following ways: he could give candidate A a first-round majority, giving candidate A an immediate victory with not runoff; he could move A into the second place position, thus putting candidate A in a runoff with either B or C; if B or C were close to the 50 percent threshold, the voter could push them into a runoff by not voting for them, without changing the identity of the second place candidate. In any of these cases, the voter is either indifferent between the two outcomes (as in the case where the voter pushes candidate B into a runoff with C, where the final outcome is unchanged), or he is are strictly better off (as in the case where he is the pivotal voter pushing A into second place, and therefore into a runoff with either B or C).

Now consider the same voter in the plurality case: a voter may vote for B (even if A is her favorite candidate) to keep from being part of a split majority that would ensure C a first-round victory. With the existence of the runoff threshold, however, and because C is the least favored candidate by 60 percent of voters, there is no chance that C can win in the first round. Also, with mandatory voting, C will lose in the second round to either A or B. With the threat of a candidate C victory eliminated, the voter will only be pivotal in determining the candidate to which C will lose. Therefore, the candidate will have no incentive to strategically vote for B in a two-round election.

Theoretical models often assume often find that plurality elections feature equilibria with only two candidates, while runoff elections feature equilibria of three or more candidates. Therefore, one hypothesis is that runoff elections will feature more elections with three or more candidates. However, if voters are not fully rational, or are in practice unable to perfectly identify the top two candidates, the runoff election may not have a noticeable effect on the number of candidates receiving votes. All candidates in elections, even if they have been abandoned by most of their supporters, will receive at least one vote (their own). Therefore, a more realistic measure to use in this setting may be the cumulative vote share of candidates finishing outside the top two. If voters behave strategically, I would expect them to abandon all but the expected top two candidates. Even if voters cannot perfectly identify those candidates, their strategic behavior would lead to a reduction in the support for candidates outside the top two.

2.2 Data

I acquired primary election returns from the Secretaries of State for Florida, Georgia, Texas, Arkansas, Kansas, New Hampshire, Idaho, and South Dakota, for the years 1992-2012. Each observation is a party primary, and includes data on the number of candidates, the vote shares of each candidate, seat characteristics, and whether or not an incumbent is participating in the primary. The choice of states is due primarily to data availability. Primary election returns for all elections are unavailable or difficult to compile for many states. Other states, (California, Alaska, Washington, Louisiana, Maryland), were unsuitable for comparison, because they elect some representatives using multi-seat districts (Maryland) or failed to conduct party-specific primaries at some time during my sample (California, Alaska, Washington, and Louisiana have all used Blanket Primaries at some point in my sample period). Of the eight states in my sample, four (Kansas, Idaho, New Hampshire, and South Dakota) use plurality voting to determine party nominees, while three (Arkansas, Georgia, Texas) use majority runoff elections. Only Florida changed its system at any point in my sample.

Seats included in this analysis include those for the United States House of Representatives and Senate, Governor and Cabinet (such as Attorney General or Secretary of State), as well as each state's respective house of representatives and senate. This allows me to compare the effect of the elimination of runoff elections across a variety of seat types. Excluded from the analysis are all uncontested primaries, those with either one or zero candidates, several states in the analysis fail to provide information on uncontested seats. Eliminating such races will not affect my results, as runoffs and plurality elections behave identically in races with fewer than two candidates: voters cannot vote strategically in uncontested races, and runoffs cannot draw a second candidate into a race because a two-candidate election under runoff and plurality systems behaves identically).

Although Florida's switch from runoff to plurality elections can be defended as exogenous to the outcome variables considered here, the 2000 election led to larger changes in election laws that threaten the proposed identification strategy. Specifically, the Florida legislature, as part of the bill that eliminated the runoff election, introduced no-excuse absentee voting and provisional voting. No excuse absentee voting strips voting officials of the ability to deny any application from a registered voter to apply for an absentee ballot; thus any registered voter can vote absentee without providing an excuse. Provisional voting allows voters to cast provisional ballots, meaning that even if a voter arriving at a polling location is not on the electoral roll, she is still allowed to cast a ballot. Election officials will later investigate and verify the provisional ballot if the voter is found to be registered in the state. In the presence of redistricting, polling locations for people who have not moved between election cycles can often change.

No-excuse absentee ballots and provisional voting can have an effect on observed voter or even candidate behavior. If the preferences of voters most affected by these law changes are not representative of the electorate as a whole (and there is little reason to suspect that they would be), then these law changes will lead to biased estimates if not accounted for. Fortunately, several other states introduced similar measures between 2000 and 2002. The Help America Vote Act, passed in 2002, requires all states to allow provisional voting. Therefore, all states without provisional voting in 2002 were, like Florida, required to begin accepting provisional ballots. Using data compiled by the National Conference of State Legislatures, I identified all law changes for states in my sample concerning either no-excuse absentee ballots or provisional voting. Of the states in my analysis, Arkansas, Georgia, Kansas, and Texas also introduced provisional voting in 2002. Additionally, several other states in the comparison group (Arkansas, Georgia, Maine, South Dakota) passed laws allowing no-excuse absentee voting. These law changes allow me to use this state and temporal variation in laws to disentangle the effects of these law changes and isolate only the effect of the changes in voting procedure.

Table 2.1 reports the summary statistics for the two subsets of data used for this paper. Due to reported complications in the 2002 elections (in Florida and elsewhere) in implementing some of the changes introduced through both the Florida legislation and the Help America Vote Act, I have removed 2002 from the sample[4]. 58% of contested primaries in the sample are Republican, likely due to the relative strength of the party in several of the states used in this sample. Stronger parties are more likely to have contested elections because success in the general election is more likely. State House of Representative seats

[4]The results I find are robust to including 2002 data

make up more than half of the data set. Approximately one-third of the races in the sample have an incumbent participating, about one-third are candidates attempting to be elected into an open seat, and about one-third are neither (primary elections for challengers to incumbents in the general election). The total number of candidates participating in contested elections ranges from two to twelve, with an average of 2.5.

Table 2.2 provides the same summary statistics for seats races with at least three candidates. Of the 3,795 contested elections, slightly less than one-third (1,240) had three candidates or more. Comparisons of Tables 2.1 an 2.2 demonstrate how the characteristics of my sample change when I narrow the focus to larger elections. Larger elections in terms of candidates are also larger in terms of votes, which is at least partially explained by a change in the distribution of elections across governing bodies. Large, statewide seats (Senate and Governor or cabinet positions) are more likely to have three or more candidates; although the two categories combined make up only six percent of the full sample, they make up over 10 percent of the sample of races with three or more candidates. As expected, such elections are less likely to include an incumbent, as the presence of an incumbent tends to discourage entry by other candidates. Likewise, open seats are more likely than races with incumbents to attract large fields of candidates. Finally, 65% of elections in this sample take place in states with runoff primary elections. This is a significantly larger share than in the larger sample, indicating that states with runoff primaries are more likely to feature elections with three or more candidates.

2.3 Econometric Framework

Because the variation in election systems studied in this paper is due to a change in election policy by one state, I use a differences-in-differences design to control for the potential endogeneity between runoff elections and candidate behavior. In the case of runoff elections, there is reason to suspect that different ex ante, state-specific political environments motivated state legislatures decisions to conduct primary elections using either a plurality or runoff system. For instance, it may be the case that runoff elections were only implemented in states with many factions within a party. Therefore, we would see a correlation between runoff elections and many parties, but not due to a causal relationship.

By employing differences-in-differences, however, I can control not only for such state-specific effects as those mentioned above, but also for time-specific effects. Specifically, each outcome variable Y_{ist} for election i in state s at time t, can be described in the following equation:

$$Y_{ist} = \alpha + \beta RO_{st} + \eta X_{ist} + \mu Z_{st} + \gamma_s + Post_t + \varepsilon_{ist}$$

where X is a vector of election-specific covariates; Z captures state-time specific variables (specifically , the existence of no-excuse absentee ballots and provisional voting procedures); γ captures a time-invariant state factors; $Post_t$ is a dummy variable equal to 1 if the election took place after the law change, and 0 otherwise. RO is a dummy variable equal to 1 if the election was held under a runoff system, and 0 if it was held under a plurality system. With the state fixed effects and Post-treatment dummy,the coefficient β is the differences-in-differences estimator, capturing the causal effect of conducting an election under a runoff system on my outcome variables. As runoff election laws vary only at the state level, I cluster the standard errors for all regressions at the state level.

I test two different outcome variables, both of which are based on Duverger's Law. The first is the number of candidates entering an election. Under Duverger's hypothesis, runoff elections provide increased incentives for voters to vote sincerely, which results in equilibria where more than 2 candidates receive a positive share of votes. If the elimination of the runoff election in Florida causes voters to strategically abandon candidates, affected candidates may exit the election, and I would observe a decrease in the number of candidates participating in contested elections.

The second test focuses directly on the behavior of voters, using as the outcome variable appropriate outcome variable the total vote share of all candidates finishing behind the top two candidates in elections. Under Duverger's Law, I would expect this variable to be higher, reflecting voters' increased incentives to vote for their most preferred among all candidates, instead of their most preferred of the top two candidates.

The effect voting procedure on voter behavior can only be measured if the number of candidates is unaffected. If the elimination of runoff elections causes candidates to exit elections they would otherwise enter, it becomes impossible to measure the effect of the law directly on voter behavior, because voters would be voting over a different set of candidates.

2.4 Results

I first consider the effect of runoff elections on the number of candidates entering an election. In this regression, the seat-specific co-variates used are dummies for whether a race includes an incumbent, is an open seat, and a dummy indicating the government body (state house, Unites States House, etc) in which the seat is located. The number of candidates entering an election can affect the distribution of votes, so testing for the effect of runoff elections on the number of candidates is necessary before moving on to estimating the causal effect of runoff elections on voter behavior. Only if candidate entry is truly unaffected by the existence of a runoff can I test directly for strategic voting.

Column 1 of Table 2.3 reports the results for a linear regression of the causal effect of a runoff on the number of candidates. Under this specification, runoff elections are associated with 0.07 fewer candidates, but this estimate is far from statistically significant. These results fail to indicate any causal effect of runoff elections on the number of candidates. Each of the statistically significant coefficients is of the expected sign. Races with incumbents have fewer candidates, while races for open seats attract more candidates. Compared to statewide seats for the Governor's office and cabinet seats, which is the omitted category, races for state house and senate seats attract fewer candidates, while Unites States Senate seats attract significantly more candidates. This is likely due to the attractiveness of the different seats.

However, a linear regression may suffer from unnecessary noise. Because Duverger's Law predicts that plurality elections will result in two-candidate equilibrium, while runoff elections result in equilibria of at least three candidates, the only relevant margin is the one between two and three candidates. Therefore, I perform a similar test to the one reported in Column 2 of Table 2.3; however, this time I use a logit regression, using as the dependent variable a dummy equal to 1 if a race has more than three candidates, and equal to 0 otherwise. Under this alternative specification, the results are unchanged; runoff elections do not appear to cause an increase in the number of candidates.

The elimination of runoff elections appears to have had no effect on the entry and exit decisions of candidates. However, under Duverger's Law, this is only a second-order effect. Candidates will only leave races if they anticipate abandonment by their supporters, who will rally strategically behind their most preferred of the top two candidates in plurality elections. It may be implausible to assume that candidates can perfectly anticipate if they will be the victim of such abandonment (as opposed to the beneficiary). Additionally, since the sample here consists of primary elections, the information that candidates have when they are deciding to enter or exit a campaign may be relatively limited. Finally, even if a candidate exits a campaign shortly before the election, her name will still appear on the ballot. In this case, the candidate exit would not appear in the data as one fewer candidate in the race, because she would almost certainly garner a few votes.

Therefore, I can now directly test for strategic voting behavior. In this case, the dependent variable is the cumulative vote share in the first round of elections for all candidates finishing third or lower. If voters in runoff elections are less likely to strategically vote for one of the top two candidates, I would expect the vote share for candidates finishing third or lower to be, on average, higher in runoffs. The specification for this second reduced form model is identical to those used in the previous specifications, with the following adjustments: my sample is restricted to races with three or more candidates, since these are the only races in which vote shares for candidates finishing third or lower are observed; additionally, the number of candidates is included as a race-level variable.

Table 2.4 shows the standard 2-by-2 table, comparing pre- and post-treatment averages for Florida to comparison states, which provides a preview of my findings. Vote share for candidates finishing outside the top two is lower in the post-treatment periods for both groups in all five types of elections considered. However, in four of the five cases, the average dropped by a greater amount in Florida than in comparison states. These results are consistent with Duverger's Law. When participating in plurality election (as was instituted in Florida after 2001), voters are less likely to voter for candidates outside the top two, and are likewise more likely to vote for one of the top two candidates.

Results for the regression using cumulative vote share for candidates finishing outside the top two is reported in Table 2.5. While no effect was observed of runoff elections on the number of candidates, the results support the hypothesis that voters in runoff elections are less likely to vote strategically. As compared to plurality elections, vote shares for candidates finishing outside the top two in runoffs is 1.73 percentage points higher with the estimates statistically significant at the 5% level. Given the baseline of 22.55 vote share for such candidates, this represents a 10 percent increase in the vote share. This finding is significantly smaller than those found by Fujiwara (2011), who found that runoff elections led to a 50 percent increase in vote share. Similarly, Bordignon and Tabellinin (2009) found a large effect of runoff elections in their study, with a 30% increase in the number of candidates and 50% increase in the number of parties when moving from a plurality to runoff election. Also, as shown in Appendix B.1, the statistical significance of the results shown in Table 2.5 is not robust to more conservative estimates of estimation.

There is significant variation in the causal effects across different types of elections. I compare the effect of runoff elections in statewide elections (United States Senate, Governor and cabinet seats), to the effect in smaller, regional elections (state house, state senate, and United States House). There are reasons to suspect that these races behave differently. All else equal, statewide elections attract more candidates; 37.9 percent of contested statewide elections featured three or more candidates, as opposed to just 30.7 percent of contested regional elections. Voters in larger elections may be better informed about candidates other than their most preferred, as these elections receive more media coverage. Therefore, voters may be better able to identify the top two candidates, and determine their most preferred among the top two candidates. Finally, voter turnout in the second round of runoff elections is higher in statewide elections than in regional elections. As I will demonstrate in the last part of this paper, this difference in voter turnout can influence the types of equilibria in terms of strategic voting and the number of candidates.

Columns 2 and 3 of Table 2.5 separate the results of column 1 into local and statewide elections, respectively. While voters participating in the first stage of a runoff election in regional elections increase their support of candidates finishing third or lower by only 1.59 percentage points, voters in statewide elections increase their support of similar candidates by 11 percent. The baseline of both of these groups is around 22.5 percent, so these differences in treatment effects are not being driven by differences in the baseline. There is reason to suspect that voters in larger elections may be more likely to vote strategically.

When compared to the findings of other studies, the effects found in the results above are quite small. This could be due to the differences in the settings (American primaries as opposed to Brazilian or Italian general elections) or to differences in participation rates between the two settings. Voting is mandatory in Brazil, and it sees turnout rates of close to 90 percent. While voting is not mandatory in Italy, participation rates are higher in national elections than corresponding participation rates in the United States.

Voter turnout (and, specifically, relative participation rates between election rounds) is of unique importance in this setting, due to the impact it can have on the equilibrium number of candidates, as well as on the behavior of voters. Recent theoretical models (Callander (2005), Bouton (2013)) have illustrated the conditions under which runoff elections can feature two-candidate equilibria. As Bouton (2013) has shown, runoff elections can feature two-candidate equilibrium when the populations between the two rounds of the runoffs differ. The intuition is as follows: if voters do not know which voters will participate in the runoff, they face the possibility of a non-majority candidate winning a runoff, even if he was the Condorcet loser in the first round. When the populations of voters in the two rounds are identical, it would be impossible for a Condorcet loser to win in the second round of an election. With low turnout, a two-candidate equilibrium can exist where voters abandon all but two candidates, abandoning those who they believe have no chance of winning in the first round, in order to avoid an upset victory in the second round. If candidates are rational and anticipate this abandonment, only two candidates will enter the race. This would mean that voters can still be strategic and behave similarly in runoff and plurality elections.

In 2000, the final year of runoff elections, voter turnout in the 20 runoff elections held in Florida was, on average, 69.9 percent of the turnout in the first round of elections. Using this turnout rate, it is easy to imagine an example where a Condorcet loser is elected with a runoff. Suppose that Candidate A is a Condorcet loser and receives 45 percent of first round votes, with a split majority divided between Candidate B (30 percent) and Candidates C (25 percent). In a runoff with full participation, Candidates A and B advance to a runoff , and Candidate B wins with 55 percent of the vote. First round majority voters can vote sincerely for their preferred candidate (between B and C), knowing that the winner between those two will win in the second round.

Suppose now that, instead of all voters participating in the runoff election, only 70 percent, participate. Suppose that 80 percent of those who voted for Candidates A or B participate in the 2nd round, while only 40 percent of those who voted for an eliminated candidate participate. Even if all Candidate C supporters participating in the runoff vote for Candidate B, Candidate A would win the runoff with 51.4 percent of the vote. If voters anticipate the possibility of this outcome, they have an incentive to rally behind their favorite of the top two candidates (in this case, Candidate C supporters would abandon him in favor of candidate B). Note that these results are identical to the results we would predict in a plurality election.

Testing this theory directly is not possible, since runoff voter turnout is not observed when only two candidates enter the race (as no runoff will be held). Also, the behavior of individual voters in both rounds of elections is not observable. Instead, I construct a variable of voter participation to determine whether potential candidates anticipating lower runoff election turnout respond by abandoning races. Using the State Legislative Election Returns, 1967-2003 dataset, compiled by Carsey et al. (2008), I restrict my sample to primary elections held seven states[5], between 1968 and 1988[6]. I then use a fixed effects model to test the hypothesis that larger changes in voting populations between the first and second round of voting leads to a smaller number of candidates entering a race.

If the equilibrium number of candidates is more likely to be two when the uncertainty over voting populations between the two rounds is greater, I would expect to see large changes in the turnout between the first two stages of a runoff election to discourage candidate entry. Using the data on southern primary elections, I use as the primary independent variable a lagged term for the change in turnout between the two rounds, taken from the previous election. However, most elections do not go to runoffs, and thus candidates in the next election cannot observe what the change in population would have been. Therefore, I use the average change in voter turnout over the entire legislative body (House or Senate) for the previous election cycle as the primary independent variable. The claim here is that candidates and voters can observe and respond to changes in participation rates in the previous election when making their decisions in the current election.

To construct this variable measured the absolute value of the percentage change between the turnout for each of the two rounds of runoff elections:

$$Difference = \left| 100 - \frac{Turnout_{Round2}}{Turnout_{Round1}} * 100 \right|$$

If the turnout is identical between the two rounds, this value is equal to 0; if the turnout in the second round is 99 or 101 percent of the turnout in the first round, this value is equal to 1, and so on. This measurement therefore provides a monotonic measurement of the degree to which the turnout between the two rounds changes. This is the most

[5] Alabama, Arkansas, Georgia, Florida, Mississippi, Oklahoma, Texas

[6] North & South Carolina data is not included (though both states conduct primary runoffs), and the data set does not include primary elections after 1988.

conservative estimate for the change in population; the implicit assumption is that everyone who participates in the lower-turnout round participates in both rounds. In reality, some people only participate in one of the two rounds. For each state, year and body of legislature (House or Senate) I constructed a mean of this variable for each year. To estimate the effect of this variable on future behavior of candidates, I used the following model:

$$Candidates_{ist} = \gamma_s + \delta_t + \mu * Party_{ist} + \psi * Body_{ist} + \beta * Difference_{is,t-1} + \varepsilon_{ist}$$

where $Candidates_{ist}$ is the number of candidates in election i in state s, year t. γ_s and δ_t are state and year fixed effects. $Party_{ist}$ is a set of dummy variables for each party. $Body_{ist}$ is a set of dummy variables indicating if the election was for a seat in the House of Representatives or Senate. $Difference_{is,t-1}$ is the average of the change in turnout in the body of government in the election immediately before. Both the year and state fixed effects are important to note, as they both time invariant state fixed effects and common time shocks across all states.

As before, I cannot look at the behavior of voters without first considering if fluctuations in turnout affect the entry and exit decisions of candidates. The hypothesis is that election participants (voters or candidates) will use information about the most recent election when making entry decisions in the current election. Specifically, I will test whether larger variation between the two rounds leads to a smaller number of candidates entering elections, or to a higher likelihood of a two-candidate equilibrium. As before, I use two different outcome variables to measure the number of candidates: the number of candidates itself, and a dummy variable equal to 1 if there are three or more candidates, and equal to 0 otherwise. Because the relevant margin is two to three candidates, this variable provides a simpler measure. If larger differences in turnout between the two rounds results in a two-candidate equilibrium, we would expect β, the coefficient on $Difference$, to be negative in both regressions

Table 2.6 shows the results of regressions for both measures of the number of candidates. For each of the variables, I test the hypothesis using both a logit and linear model. In every case, I only consider contested elections (elections with at least two candidates). When using the number of candidates as the dependent variable, I see a slightly negative, statistically insignificant coefficient. However, when the dependent variable is a dummy indicating if there were three or more candidates, we see a statistically significant (at the 10% level) negative effect for both the logit and linear model, indicating that candidates are less likely to enter races when fluctuations in turnout were high in the previous election. A 1% increase in the turnout variation in the previous election leads to a decrease of 0.09% to 0.37% in the likelihood of a competitive election featuring a third candidate.

The claim that these changes in voter turnout in one election could change the entry decisions of candidates in the following election may seem far-fetched. However, it is not the change in a population itself, but the increased chance of an upset victory that would affect the behavior of participants. Such upsets are likely to be well-publicized. One example of an upset victory in a runoff election is the Democratic primary in the 2010 Arkansas Senate election. Blanche Lincoln, the incumbent, was widely expected to lose the Democratic nomination. Although Bill Halter, the challenger, was leading her in polls taken leading up to the runoff election, Lincoln won a shocking victory. However, of the state's 1.5 million registered Democrats, fewer than 260,000 of them voted in the Democratic primary runoff, and Halter received 15,000 fewer votes than he did in the first round of the election. The primary result proved to have a negative impact on Democratic fortunes in the state. In the general election, Lincoln lost by more than 20 percentage points, and became the first Democratic Senate nominee in 138 years to lose a general election in Arkansas.

These results indicate that candidates are less likely to enter an election when larger variations in turnout were experienced between the two rounds of election in the prior election year. These findings provide evidence that the strategy of voters in Florida before the elimination of the runoff may have already been similar to those employed in plurality elections, as turnout was very low in both rounds (between 20 and 30 percent of all voter eligible to vote in primaries), and changed significantly between the two rounds. To reconcile the findings here with those in other settings, the different estimates of causal effect may not be due to different fractions of strategic voters, but instead to voters responding to different political settings. Additionally, these results indicate that policy decisions concerning runoff elections should take into account the political setting, as efficiency gains from runoff elections could vary significantly.

2.5 Conclusion

One argument in favor of the use of runoff elections is that it encourages voters to vote sincerely, and therefore creates an voting system that will more likely reflect voters' true preferences. Duverger's Hypothesis provides a clear, testable prediction of the effect of runoff and plurality elections on the number of supportable candidates. Empirical tests of this theory under different setting have largely supported the claim that voters often vote strategically, and that runoff elections feature either more candidates, or greater support for candidates outside the top two. Utilizing the elimination of the state of Florida's runoff election in primary elections, I test for the effect of runoff elections on the behavior of voters and candidates.

This setting offers two important differences from previous work. First, primary elections feature only candidates from the same party. Therefore, I can measure the impact of runoff elections on strategic voting only, instead of the effect of runoffs on the establishment of legitimate third parties. Second, the low turnout in primary election provides a very different setting than those used in previous research. Voters may face different incentives to vote strategically in low-turnout elections, and the effect of runoff elections could likewise differ.

I find that, after the elimination of runoff elections, voters became less likely to vote for candidates finishing third or worse in the first round of primary elections. This finding supports the hypothesis that voters in plurality elections employ different strategies from those in runoff elections. However, the magnitude of the estimates I obtain are significantly smaller than those found in previous research.

One possible explanation for this difference can be found in the low voter turnouts in American primary elections. With low turnouts in both rounds of a two-round election, a candidate who is the Condorcet loser can beat a second-round opponent. To test this hypothesis, I investigate how changes in voter turnout in one election affect outcomes in the next. I find that third candidates are less likely to enter an election when large changes in turnout occurred in the previous election, which supports the models of strategic voting in runoffs with uncertainty over the voting population.

Table 2.1: Summary Statistics: All Contested Elections

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**
Republican (=1)	0.588	0.491	0	1
Runoff State(=1)	0.562	0.496	0	1
Open (=1)	0.388	0.487	0	1
Incumbent Race(=1)	0.344	0.475	0	1
Total Votes	42,217	148,681	122	2298880
Candidates	2.55	1.036	2	12
Type of Election				
Governor	0.051	0.206	0	1
State House	0.621	0.486	0	1
State Senate	0.185	0.389	0	1
US House	0.128	0.341	0	1
US Senate	0.016	0.126	0	1
N=3,795				

Table 2.2: Summary Statistics: At Least 3 Candidates

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**
Republican (=1)	0.578	0.495	0	1
Runoff State(=1)	0.623	0.477	0	1
Open (=1)	0.524	0.5	0	1
Incumbent Race(=1)	0.192	0.397	0	1
Total Votes	70,902	207,983	396	2298880
Candidates	3.68	1.174	3	12
Vote Share (Top Two)	77.50	11.42	38.46	100
Type of Election				
Governor	0.080	0.257	0	1
State House	0.553	0.497	0	1
State Senate	0.157	0.363	0	1
US House	0.180	0.39	0	1
US Senate	0.030	0.176	0	1
N=1,240				

Table 2.3: Number of Candidates Entering Races

	(1)	(2)
	Linear	Logit
Runoff	-0.079	-0.164
	(0.135)	(0.417)
Republican	-0.000971	-0.0351
	(0.0677)	(0.131)
Incumbent Involvement	-0.470**	-1.178***
	(0.081)	(0.0794)
State House	-0.513**	-0.713***
	(0.130)	(0.0794)
State Senate	-0.527***	-0.719***
	(0.076)	(0.237)
U.S. House	-0.040	-0.187*
	(0.106)	(0.096)
U.S. Senate	0.554*	0.676
	(0.251)	(0.453)
Observations	3,795	3,795
R^2	0.124	.

Robust standard errors, in parentheses, are clustered at the state level.
State Level Controls include dummies for no-excuse absentee voting and provisional ballot laws

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.4: Pre and Post-Treatment Averages

Average cumulative vote share of candidates finishing third or worse			
	Pre-2001	Post-2001	Diff
Governor & Cabinet			
Florida	19.37	13.36	-6.01
Other States	24.4	24.33	-0.07
Difference	-5.03	-10.98	**-5.94**
State House of Rep.			
Florida	24.47	22.66	-1.81
Other States	22.34	21.64	-0.7
Difference	+2.15	+1.02	**-1.1**
State Senate			
Florida	24.51	17.96	-6.55
Other States	23.66	19.20	-4.46
Difference	+0.85	-1.24	**-2.09**
U.S. House			
Florida	25.32	24.91	-0.41
Other States	23.86	22.32	-1.54
Difference	+1.46	+2.59	**+1.13**
U.S. Senate			
Florida	22.82	16.06	-6.76
Other States	23.84	20.29	-3.55
Difference	-1.02	-4.23	**-3.21**

Table 2.5: Dependent Variable: Vote Share

	(1) All Elections	(2) Local	(3) Statewide
Runoff	1.732**	1.591	10.994***
	(0.689)	(0.924)	(2.683)
Num. of Candidates	6.057***	6.51***	3.840***
	(0.540)	(0.695)	(0.960)
Republican	0.319	0.335	-0.211
	(1.003)	(0.900)	(2.262)
Incumbent Involvement	-4.861***	-5.084***	-0.640
	(0.979)	(1.135)	(2.531)
State House	3.147**	-	-
	(0.581)		
State Senate	1.823	-1.82	-
	(1.10)	(1.029)	
U.S. House	-0.446	-3.064***	-
	(1.112)	(0.746)	
U.S. Senate	-5.707***	-	-4.692*
	(2.101)		(2.340)
N	1,240	1,104	136
R^2	0.430	0.460	0.375
Mean of Dep. Var	22.5	22.513	22.40

Robust standard errors, in parentheses, are clustered at the state level. State Level Controls include dummies for no-excuse absentee voting and provisional ballot laws

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.6: Number of Candidates Entering Races

VARIABLES	(1) Linear	(2) Logit	(3) Linear	(4) Logit
Difference	-0.00156	-0.00305	-0.000901*	-0.00372*
	(0.00151)	(0.00229)	(0.000456)	(0.00194)
Democrat	0.312***	0.522***	0.106***	0.441***
	(0.0333)	(0.0648)	(0.0237)	(0.104)
House of Rep.	0.0697	0.116	0.0177	0.0743
	(0.0583)	(0.0983)	(0.0245)	(0.104)
Observations	4,794	4,794	4,794	4,794
R-squared	0.046		0.033	
Dependent Var	Candidates	Candidates	>2 Dummy	>2 Dummy

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Chapter 3

What We Talk About When We Talk About Campaign Spending

3.1 Introduction

The role of campaign spending in elections has been a focal point of both empirical research and policy discussions for decades. Several well-publicized court decisions and recent legislative actions have kept campaign finance in the forefront of public debate. However, despite a large body of research on the topic, research has failed to reach a consensus on the causal effect of spending. Much of this research, especially studies of United States elections, has relied on summaries of total candidate disbursements to measure candidate spending, instead of finer measurements that could yield more fruitful results.

This paper has two primary goals, both of which are made possible through newly available data. First, by utilizing new questions asked on in the 2012 American National Election Study (ANES), I can track changes in individuals' preferences for candidates during a single election cycle. This allows me to control for unobserved campaign and individual characteristics and estimate the causal effect of candidate spending under relatively weak identifying assumptions. Second, the Federal Election Commission's digitization of transaction-level data of candidate spending make it possible to identify different types of candidate disbursements and track daily spending throughout the campaign, which allows me to isolate spending that is likely to be used to sway voter opinion from other types of candidate disbursements.

Much of the existing research on the role of campaign spending on voter behavior has relied on summaries of total candidate disbursement as the independent variable of interest. The implicit assumption is that all candidate disbursement is being used in similar ways in raising a candidate's vote share. However, political campaigns are complex, and money can achieve many goals, not all of which are used to sway the opinions of voters. Incumbents maintain campaign offices between elections and engage in fund-raising efforts to build war chests during elections years when they are relatively unchallenged. If only some money is being used to persuade voters, then estimates of that effect using all spending as the independent variable could be biased. Unfortunately, total candidate disbursement is not only the most obvious measurements to use, but is often the only one available to researchers.

One primary motivation of this paper is to try and resolve a paradox between the finding of previous research and observed behavior of candidates. Empirical research on elections has consistently questioned the effectiveness of campaign spending, with many studies finding small or insignificant estimates of causal effect. However, spending by candidates has continually increased in United States elections. Since 1990, spending by the average winner of House elections has increased by 112%, in real terms, while real spending by Senate election winners increased by 79%. This raises an obvious question: if campaign spending is useless, why are candidates continuing to increase their spending?

This paper makes three primary contributions. First, I find that campaign spending is effective in swaying voter opinion, even late in an election cycle, when many voter decisions have already been made. Second, I find that these estimates of causal effect are strongest, and most precisely estimated, when campaign spending is measured only using types of spending that are likely used to sway voter opinion. Finally, turning to the relative effectiveness of challenger and incumbent spending, I find that systematic differences in the way challengers and incumbents spend their fund could be driving previous results that have frequently found challenger spending to be more effective than spending by incumbents.

3.1.1 Previous Literature

This paper builds on an extensive literature on campaign spending. Models of campaign spending have explored how candidates may influence election outcomes, and have primarily hypothesized that messages sent from candidates to voters operate as the important mechanism. Several models (Austen-Smith (1987), Baily (2002), Barron (1994)) describe the function of candidate spending as a way of informing voters of candidates true policy position. In others (Coate (2004)), candidates use spending to inform voters about their quality. However, only a portion of candidate disbursements go towards creating and sending these signals to voters. As I will show later, systematic differences are present in the way different candidates spend their money particularly between challengers and incumbents. Therefore, one primary goal of this paper will be to provide a more specific measurement of candidate spending on "signals", and measure its causal effect.

Although empirical research has frequently addressed the impact of campaign spending, little consensus has been reached. While some studies (Goldstein and Freedman (2000), Gerber (1998)) have found large effects of campaign spending on candidate success, others (Levitt (1994), Welch (1981)) have found small or insignificant effects of spending.

A great of the empirical work on the subject has focused on the surprising result that spending by incumbents appears to be less effective than spending by challengers. Jacobson (1978) first noted this phenomenon, and attributed it to reactive spending by incumbents. Incumbents are only likely to spend more if when they face high quality competition, in which case they will receive lower vote share. However, this finding has been supported by a great deal of research in both United States (Gerber (2004), Levitt (1994), Abramowitz (1988)) and international elections(Pattie et al. (1995), Benoit (2010)), though a variety of papers studying a number of settings (Gerber (1998), Erickson and Palfrey (1998)) have challenged these conclusions, claiming that incumbent and challenger spending are equally effective. While this focus by researchers on this phenomenon has at times seemed little more than a statistical puzzle, the desire to resolve this puzzle is understandable. I show that when total disbursements are used to measure candidate spending, I find incumbent spending to be less effective than challenger spending. However, when I use only spending by candidates on communication to voters, challenger and incumbent spending appears to be equally effective.

A strength of my estimation strategy is the ability to use individual-level data, which allows me to compare changes in voter preferences within a single election, instead of relying on cross-election variation to estimate causal effects. While most studies of the effect of campaign spending have relied on cross-election variation, several papers have utilized individual-level data. Jacobson (1990) uses a panel survey data taken during the 1986 elections measures the effect of candidate spending on changes in voter preferences during the last weeks of the campaign, finding that challenger spending has a larger effect on voter preferences. Goldstein and Freedman (2000) combine data on advertising exposure with the 1996 ANES, and find that, not only is candidate spending on advertising largely effective, but that challenger and incumbent spending on advertising is equally effective. The claim that I make in this paper is that these results are not necessarily inconsistent with those that find that total challenger spending is more effective that total incumbent spending, but that they result from systematic differences in the way the two types of candidates allocate financial resources.

To my knowledge, only one paper, Ansolabehere and Gerber (1994), has specifically addressed the issues that might arise from using aggregate spending data as a measure of candidate spending. Using a fully itemized dataset of 1990 Congressional election candidates, the authors compare estimates of causal effect under three different measures of campaign spending. The most restrictive measure of spending the call “communications”, which is advertising, direct voter contact, phone banks, and campaign rallies. The authors find this more precise measure of campaign spending, when used as the right-hand side variable in a simple OLS regression of spending on vote share, instead of eliminating the negative correlation between incumbent spending and vote share, leads to a larger gap in the effect of challenger and incumbent spending. This result is not surprising, as advertising spending is certainly reactive spending: only candidates in competitive elections will spend money in advertising. The paper also highlights the fact that a precise measure of campaign spending does not resolve the underlying endogeneity issues plaguing estimates of causal effect.

3.2 Empirical Framework

The ANES time-series survey has been conducted every Presidential election year since 1948, and every Congressional election year since 1958. The survey gathers information on voter opinion before the general election, and on the ultimate voting behavior of those same individuals after the election. The pre-election survey typically asks voters about their intent to vote in the presidential election, but has only once before (in 1996) asked voters how they intend to vote in Congressional elections. The structure of the 2012 ANES time-series survey allows me to investigate the effect of late-election cycle spending on changes in voter preferences. By using variation in the timing of campaign spending, I can test if changes in the relative spending by candidates predicts changes in preferences by individuals.

With only two major participants in an election, individuals have the option of voting for either of the two parties, voting for a minor party, or abstaining. This decision can be converted into a single linear variable by considering votes received by the Republican candidate, net the vote received by the Democratic candidate. I use as the dependent variable the net vote for the Republican candidate, taking on a value +1 if the voter supports the Republican candidate, -1 if she supports the Democratic candidate, and 0 if she either votes for a minor candidate or abstains. This framework has an intuitive appeal, as it can be easily converted to vote shares (a 0.1 increase in the net votes for a Republican would be 5% swing in the vote share for a Republican in a 2-candidate race).

I assume that individual voter preferences can be described by the following linear model:

$$NetVotes_{ie} = \phi\mathbf{X_i} + \beta(ln(RepublicanSpending) - ln(DemocraticSpending))_e + \delta_i + \zeta_e + \epsilon_{ie}$$

Where $NetVotes_{ie}$ is the net votes of individual i in election e. This is determined by a set of observed voter characteristics, $\mathbf{X_{ie}}$ as well as spending by candidates. Spending of candidates is determined by spending of candidates between the beginning of the general election (as measured by the end of the primary election) and the date of the general election. This framework assumes that all candidate spending is equally effective, and that spending by one candidate offsets the effect of the spending by the other. Additionally, individual voting decisions are determined by unobserved, time-invariant individual and election characteristics characteristics, $\delta_i + \zeta_e$. Candidate quality, political climate and unobserved voter preferences bias any measure of causal effect from this equation. Popular candidates will be better able to raise (and therefore spend) money, and will also be preferred by more individuals. There is no time variation in this regression, as this data comes only from people who voted on election day, and is a purely cross-sectional look at voter behavior and spending.

During pre-election surveys, which took place in the weeks before the general election, potential voters were asked how they intend to vote. The pre-election preferences of voters can be modeled as

$$NetVotes_{iet} = \gamma_t + \phi\mathbf{X_{it}} + \beta(RepublicanSpending - DemocraticSpending)_{et} + \delta_i + \zeta_e + \epsilon_{iet}$$

Where the dependent variable is determined by the candidate for whom the individual states they intend to vote at time t. Potential voters were assigned a +1 if they intended to vote for the Republican candidate, -1 if they intended to vote for the Democratic candidate, and 0 if they did not know for whom they would vote, if they intended to vote for a different candidate, or intended not to vote[1]. Candidate spending is determined by the sum of spending between the beginning of the general election and the date of the pre-election survey. Since the pre-election survey was conducted over several weeks, I include time fixed effects, γ_t which control for common, time-variant shocks.

By taking the difference between these two values, unobserved, time-invariant characteristics across elections and voters are fully captured.

$$\Delta NetVotes_{it} = \Delta\gamma_t + \Delta\beta(RepublicanSpending - DemocraticSpending)_{et} + \epsilon_{et}$$

This framework offers an improvement over studies utilizing variation across different elections. By observing changes in voter opinion within an election, I am able to reduce the potential for omitted variables to lead to biased estimates of causal effect. Though the characteristics of potential voters does not vary over time, and should therefore fall into the error term of the differenced equation, I also include individual voter characteristics as a robustness check. Additionally, I can utilize the variegation in the timing of the pre-election survey to control for time shocks by including a time dummy.

The identifying assumptions of this framework are relatively weak. I only need to assume that election fixed effects remain time invariant during a the general election campaign. Specifically, the assumption implicit in this specification is that the unobserved characteristics of individuals that affect both candidate spending and the preferences of voters is fixed between the pre and post-election surveys. This assumption would be violated if, for instance, time-variant shocks occurred during the election cycle that affected both the spending patterns of candidates, and the preferences of voters.

Two characteristics of this framework help address these concerns. First, the pre-election survey took place after most of the fundraising activity for the election had taken place. This means that even if, for example, a negative shock to candidate likability reduced

[1]The ANES survey asks ‘For whom does respondent intend to vote”, with a list of candidate names.

the likelihood that a potential voter would vote for that candidate, it likely would not affect candidate spending ability, as most funds are acquired early in the election cycle. Second, the variation in pre-election timing allows me to control for aggregate, time-variant shocks.

Given the voter preference and behavior is determined by survey data, there is a concern of missmeasurement and response bias. Specifically, I will cover several potential biases in responses that have been discussed in previous research. First, it was noted that in earlier versions of the ANES, respondents were sometimes significantly more likely to have reported voting for incumbents. Second, there is the potential for people to claim to have voted even when they did not, especially in face-to-face interviews (Holbrook and Krosnick (2010)). The theory is that people face a social pressure, and claim to vote even when they don't. Newer versions of the survey have been redesigned to combat these sources of bias. To these, I will address one more: winner bias. If voters are more likely to claim having voted for the winner in the post-election survey, this could lead to biased estimates, especially if winners end up spending more.

Even if incumbency bias existed, it would not pose a threat to my identification strategy, as the bias would be captured by the individual fixed effect, as the identity of the incumbent does not change throughout the election cycle. The over-reporting bias, however, could bias my estimates, especially if people who claim to have voted are more likely to both not state a preference in the pre-election survey, and then claim to have voted for the candidate who spent the most on advertising and campaign events. I test for the existence of this bias by estimating causal effect separately for respondents for whom the survey was administered face-to-face, and those who self-administered the survey online. Previous research has found that self-administered online surveys feature less over-reporting of voter turnout that face-to-face surveys (Holbrook and Krosnick (2010)). To test for winner bias, I test whether people are more likely to switch their vote for the winner in the post-election survey. The results are presented in Appendix C.1.

In my estimates of causal effect, I use each of the three measures of candidate spending, which are detailed in more depth in the following section: disbursements, spending, and communication. Also, there is a strong reason to expect heterogeneous treatment effects of campaign spending. Not all voters are affected by campaign spending equally, and many individuals are not going to vote no matter the messages from candidates, or have selected their preferred candidate long before the general election. Additionally, in models of voting behavior, the identity of the marginal voter is of utmost importance. The richness of the American National Election Study is therefore particularly helpful in determining the characteristics of voters who are most likely to be affected by campaign spending.

3.3 Data

Candidate spending data comes from the Federal Election Commission candidate disbursement dataset, which details each individual transaction in support of a candidate of the 2012 Congressional and Senate elections. So called "memo line" transactions were dropped from the data, as these disbursements represent double-payments (i.e. credit card payments, where the original purchase had already been itemized). One of the central goals of this paper is to better understand different types of candidate spending, and to isolate only those that are most likely to be used to sway voter preferences. Therefore, a discussion of the disbursements found in the 2012 FEC Candidate Disbursement dataset is needed.

In support of the 2012 election, House and Senate candidates spend almost $2 Billion, consisting of 722,782 disbursements. Importantly for this research, a note attached to each transaction allows me to distinguish different types of campaign spending.

FEC filing rules separate disbursement into 12 categories[2] Only 286,969 (39.7%) of disbursements, constituting $0.775 Billion (38.9%) of spending have this filing code explicitly code provided. However, 719,017 (99.5%) of disbursements, constituting $1.977 Bill (99.3%) of spending have notes detailing the type of spending, allowing me to assign spending codes to observations in which it is missing. The procedure used for assignment

[2]The categories are: 1: Administrative/Salary/Overhead Expenses; 2: Travel; 3: Fundraising; 4: Advertising; 5: Polling Expenses; 6: Campaign Materials; 7: Campaign Events; 8: Transfers to other committees of same candidate; 9: Loan Repayments; 10: Refund of Contributions; 11: Political Contributions; 12: Donations.

is outlined in Appendix C.2.

This data provides an in-depth view of how candidates spend their money. First, a portion of disbursements are coded as “refunds of contributions” “contributions to other candidates”, and “donations”. These are all disbursements that are transfers of funds out of a campaign, instead of being used in support of a candidate, and are likely only to be incurred if a candidate is likely to win the election[3]. Second, large amounts of money are spent on polling, transfers between committee, and administrative purposes, including rent, car leases, and staff salary. While these expenses are likely necessary for a successful candidate, they differ in a fundamental way from spending that is being used to create and deliver candidate messages to potential voters. Additionally, a larger portion of these expenses are incurred long before the election, including the year before the campaign.

In order to consider how different measures of spending affect the estimates of causal effect, I follow a framework similar to Ansolabehere and Gerber (1994). The first measure I use is the all disbursements. This is the standard measure used in previous literature. I call this measure “disbursements”. The second measure is all disbursements but refunds, donations, and contributions, I call “spending”, since this is all spending that is done is support of a candidate, regardless of its purpose. Finally, I construct a third measure of candidate spending, which consists only of advertising and campaign events. These are the types of spending that are used to delivery messages directly to voters, and I call this measurement “communication”.

Though empirical papers have rarely been able to utilize different measurements of candidate spending, this distinction was discussed by Jacobson (1978). He identified two types of spending by candidates: spending that a candidate would incur in any setting, determined by fixed costs, taste of spending, or wealth, and spending that depends on a candidates election circumstances (reactive spending). My claim is similar: all candidates, regardless of the competition that they face, spend some positive amounts of money. However, there is little reason to assume that all of this spending will be used to increase a

[3] This is especially the case of refunds, which are frequently labeled “refund of unneeded funds”

candidate's vote share.

Figure 3.1, which shows the spending of candidates who ultimately won election in the 2012 House of Representatives election[4]. Candidates are considered to be uncontested if they face either no challenger in the general election, or only independent challengers who failed to earn 5% of the vote share. Surprisingly, candidates who faced no contest in their general election spent more than those who were in contested ones. While some of this may have been used to deter potential challengers from entering the election in the first place, or spending during primary elections, the continued spending during the general election campaign should lead us to question our assumptions about the true function of campaign spending. If the only purpose of all campaign spending were to persuade voters, it is difficult to conceive of circumstances under which those who have no need to sway voters spend the greatest amount of money doing so. Safe incumbents are likely to maintain large staffs and begin building war chests to prepare for future political battles.

Figure 3.2 shows the same comparison, but only measuring spending on advertising and campaign events, which I classify as "communications". Though uncontested candidates have spent more on these activities than contested candidates before the beginning of the year, almost all of their total spending occurs before the beginning of the general election. This early spending could be due to primary election competition or as an entry deterrent. Importantly, once the general election starts, spending all but ceases for uncontested candidates. It is the use of this spending, and not of aggregate spending, that appears to follow patterns we would expect if spending is changing voter opinion during the general election.

The data also reveals important distinctions between the behavior of incumbents and other candidates. Figure 3.3 shows the behavior of general election candidate spending throughout the election cycle. By the beginning of 2012, incumbents has already spent an average of over $500,000, and have spent significantly more than other candidates in the months before the beginning of the general election campaign (about September 1).

[4]No Senate election winner were uncontested; I therefore dropped all Senate elections for this comparison

While candidates for open seats eventually catch up in spending to incumbents, challengers consistently spend significantly less than other candidates, spending less than half, on average, than other candidates.

The ability for incumbents to outspend their opponents is nothing new, but the composition of incumbent disbursement is significantly different than that of other candidates. As seen in Figure 3.4 incumbents spend a significantly larger amount than other candidates on fundraising expenses, contributions to other candidates, and donations. If incumbent spending is, as has been found repeatedly in the literature, less effective than challenger spending, the high level of spending on fundraising seems counter-intuitive. However, candidate fundraising is not restricted to their own campaign. Candidates can raise money for national parties, other federal candidates, and even state and local candidates and parties, as seen by their increased spending on contributions to other candidates. Incumbents have several advantages over other candidates in terms of fundraising. Their high visibility of elected officials may lead to more effective fundraising, as their fundraising windows likely extends further before the election than other candidates.

The share of campaign expenditures that are contributions to other political entities (candidates, parties, and committees), is 8 times higher for incumbents than it is for other candidates. While it is difficult to determine the extent to which fundraising activities are tied to these eventual transfer of funds from incumbents to other candidates, it is clear that incumbents are far more likely to commit a significant portion of their expenditures to activities that do not directly affect their own campaign.

These differences in spending habits between incumbents and challengers has an important implication to research using FEC expenditure data. While the effectiveness of total campaign spending may differ between incumbents and challengers, the observed difference in the makeup of disbursements implies that incumbent disbursements are not used in the same way. By considering only communications, I am able to make a true "apples-to-apples" comparison of campaign spending by both incumbents and challengers.

Data on potential voters come from the 2012 American National Election Study Time

Series Study. This study surveyed potential voters both before and after the November 6 election. Pre-election surveys took place between September 9 and November 5, with more than half taking place between October 16 and October 20. The full distribution can been in Figure 3.5. This shows the large share of respondents whose initial preferences are being measured late in the election cycle. Given the late timing of the pre-election survey, there are initial concerns that political decisions had already been made, leaving little variation in the dependent variable. However, almost 20% of respondents change their stated preferences between the pre and post-election survey.

By taking spending data from the SEC database, I was able to link spending data to the ANES. This means that I was able to measure the aggregate level of spending up to the point of the pre-election survey and again up to November 5. As the survey asked about all elections, each survey participant can appear up to twice in the data: once for her responses about the House election, and once for the Senate Election. Large differences in spending exist between spending levels in Congressional and Senate campaigns, and therefore I estimates causal effects separately for the two houses.

Summary Statistics are shown in Table 3.1. All 33 Senate elections are used in my sample. Of the 435 Congressional districts, only 359 were used in my sample. This is because I dropped uncontested elections, or elections where either only one candidate from a major party was participating, or two candidates from the same major party were participating in the General election (which makes voter activity impossible to determine through the ANES). By election day, the average Congressional election candidate had spent \$1.17 million, while the average Senate candidate had spend \$10.1. Between the pre-election survey and election day, House candidates spent about \$0.177 million, while Senate candidates spent \$1.96 million. To focus on spending only of ads and events, House candidates spent \$0.438 million by election day (\$0.127 million after the pre-election survey), while Senate candidates spend \$6.01 million (\$1.27. after the pre-election survey). This means that about 75% of House spending and 65% of Senate spending in the period between the pre-election survey and election day was in the form of ads or campaign events, which is a significantly larger share of total disbursements that is observed in Figure 3.4.

Individuals are only included in the dataset if they participated in both the pre and post-election survey[5], and therefore, the is the potential for non-response or attrition bias to affect the makeup of the population in my sample (Peress (2010)). However, individual characteristics of my sample appear to be mostly representative of both the full ANES sample, and of eligible voters in general. Individual characteristics appear to be roughly representative of the voting-age population, with ratios of males and nonwhite similar to the average reported by the U.S. Census Bureau. While the average age within my sample is slightly higher than the average age from the U.S. Census Bureau (49 years old), this could be because both the ANES and Census only report broad age groups, instead of actual ages, which forces me to impute estimated ages for respondents. Of the 10,044 possible responders of the ANES, only 5,144 can be used for my analysis, as respondents needed to participate and answer all voting questions in both waves of the survey for me to be able to measure changes in preferences. Individuals used in my analysis differ slightly from the full ANES sample. They are more likely to be male, married, white, and slightly younger, but these differences are not large.

Finally, the net vote variable shows that the average voter will, on election day, cast net votes of -0.132 for the Republican, relative to the Democrat. Of those who voted for either the Republican or Democrat, 55% in House elections and 59% in Senate elections voted for the Democrat. This percentage is larger than that of the general public, and may be a result of imperfect regional variation in the administration of the ANES, in favor of urban voters. This preference for Democrats seen in the general election is slightly stronger than the average preference stated by voters during the pre-election survey, meaning that the average voter preferences change slightly towards the Democratic candidate during the campaign.

[5] I also eliminate from all regressions anyone who had already voted before the pre-election survey, and anyone who was voting in a different district than they one in which the survey was taking place

3.4 Results

3.4.1 Effects of Relative Spending

First, I consider the effect of all candidate disbursements, regardless of purpose, pooling elections involving incumbents and open seats. Senate and Congressional elections are estimated and reported separately. The results from these regressions will be most comparable to previous research on campaign spending. Table 3.2 presents these findings. Column 1 presents the results of the cross-sectional data, using only voters behavior on the day of voting. Unsurprisingly, a strong, positive correlation is observed between voter preferences and the spending of candidates. However, spending is endogenous to voter preferences. Unobserved candidate quality will likely increase both the funds they can raise (and therefore spend) and the likelihood than an individual will like, and vote, for them. Other coefficients are of the expected sign. Males, whites, and married people are more likely to vote for a Republican. Controlling for other observables, age is uncorrelated with voting behavior.

Columns 2-4 provide the results for different specifications of the first-difference regression. As expected, the correlation between spending and voter behavior is significantly weaker than that seen in Column 1. Previous research has questioned the effectiveness of campaign spending, and these results seem to support these findings, with the statistical significance only present when no covariates are used (Column 2).

Point estimates do not depend greatly on model specification, and controlling for time-invariant observable voter characteristics does not change the results. Using the results from column 3 , I estimate that a 1 unit increase in the ratio of Republican to Democrat spending leads to a change of 0.0159 net votes for an individual. In other words, if the Republican candidate were to increase their spending, relative to the Democrat, by 100

Table 3.3 presents the results from the same framework using this measure that I call "spending", which is all disbursements, minus donations, refunds, and contributions. The results are almost identical to those using all candidate disbursements. This provides evidence that, though using candidate disbursement summaries may not be a perfectly accurate way of measuring spending in support of a candidate, it is unlikely to lead to a large bias in the estimate of the causal effect of spending for the average candidate. This is not surprising, as the two measures (disbursement and spending) are very similar. However, later results will show that the same cannot be said when this framework is used to address the incumbent-challenger spending puzzle.

Finally, I focus only on types of spending that are most likely to sway the opinion of voters, by counting a campaign spending classified as advertising or campaign events. Because the nature of the data being used for this study allows me to measure changes in voter opinion late in the campaign season, looking at this type of spending is appealing. As shown in Figure 3.2, late-campaign season spending appears to be particularly concentrated in communications to voters.

Panel C of Table 3.2 presents these results. Though the point estimates are significantly smaller, they are more precisely estimated and as a result all are significant at the 5% level. Additionally, when the scale of the independent variables is taken into account, these finding represent an increased responsiveness of voters to candidate spending. Using the results from Column 4, a 1 standard deviation change in the relative spending results in a 0.0334 standard deviation change in the net votes for the candidate (compared to 0.0196 standard deviations using all spending). These results indicate that campaign spending still appears to sway the opinion of voters late in an election cycle. Since ads and campaign events are the types of spending most closely associated with "campaigning", these results may be the most accurate measurement of the effect of spending used specifically to influence the behavior of voters. These findings, taken together, suggest that using the sum of all candidate disbursements in measuring the causal effect of campaign spending leads to downwardly biased estimates of the effect of campaign spending.

Importantly, the estimates of causal effect using only communication spending classified are much more precisely estimated than those using more inclusive measurements. By focusing only on the most visible forms of campaign spending (and therefore the types of spending that are most likely to be used to in close elections), I can reject the null hypothesis that campaign spending has no effect on candidate success.

The difference between causal effects in the Senate and House of Representatives is stark. Under each of the first-difference specifications, a one-unit increase the on the log of relative spending has impact on net votes in House of Representatives elections 3 to 6 times greater than the causal effect of spending in Senate elections. The impact of identical spending increases is even larger. As shown in Table 3.4, a $500,000 increase in spending on communications in House elections leads to a 0.0201 change in the net votes for a individual, representing a 2.01% shift in net votes for a candidate. In the 2012 Congressional elections, 15 of 435 districts were decided by this margin or less.

In Senate elections, the same $500,000 increase would increase net votes by only 0.0013 net votes. This could influence elections decided by 0.13% or less, which is a smaller margin than any in the 2012 Senate elections. Some have hypothesized (Jacobson (1985)) that candidate spending exhibits diminishing returns. Under this theory, all candidates may still have identical production functions, but higher-spending candidates (such as Senate candidates or incumbents) see smaller marginal returns to their spending due to their placement on that function. Given the significant spending advantage Senate candidates have over their House counterparts, the theory of diminishing returns is consistent with the results found here.

The coefficients of individual characteristics, shown in Panel C of Table 3.3, are all very close to zero. As these values do not vary across time, a finding of significance with one of these variables would give reason to question with validity of the first-difference framework, as it could be caused by a time-variant omitted variable correlated with one of these variables.

These results indicate that while I cannot reject the null hypothesis that changes in candidate spending has no effect on voter behavior when I measure candidate spending using all disbursements, this result is not robust to restricting candidate spending to spending that is most likely to be used to deliver messages to voters. Using total candidate spending to measure this effect leads to noisier, and potentially biased estimates of causal effect. The effect of communication spending on voter opinion late in the election cycle is small but precisely defined. As seen in Figure 3.2, candidates respond to competitiveness by increasing their spending in communication, and the effectiveness of this spending is of central importance to understanding political campaigns.

3.4.2 Heterogeneous Effects

The notion that campaign spending can have different effect on different types of voters is certainly a plausible one. Not only do candidates tailor their campaign messages to groups they believe can be swayed, but theoretical models often rely on assumptions regarding the receptiveness of voters to candidate signals. Given the rich set of questions asked as part of the ANES, it is possible to test for heterogeneous treatment effects based on individuals' policy positions or political commitment.

First, I compare the causal effects across individuals' self-proclaimed political views. The ANES asks people to identify themselves on a 1-7 conservative/liberal scale. About 35% of people identify themselves as "Moderate" (4 on the 1 to 7 scale). If campaign spending operates by changing the identity of the median voter, one might expect moderate voters to respond more strongly to messages from candidates, as their most preferred candidate could be harder to identify. To test this hypothesis, I created an indicator variable equal to 1 if a voter identified themselves as moderate, and 0 otherwise, and interacted it with the measure of candidate spending.

Table 3.5 shows the results of this regression, and provide little evidence that moderate voters are more responsive to campaign spending. If moderate voters are more susceptible to the message of candidates, the interaction term would be positive. For both House and Senate elections, under each of the first-difference specifications, there is no statistically significant positive estimate of the coefficient on the interaction term.

Alternatively, I consider if an individual's responsiveness to candidate spending is a function of their previous political participation. I consider whether a person participated in the Presidential Primary election, creating an indicator variable equal to 1 if a person voted in the presidential election, and 0 otherwise, and again interacting this variable with the measure of candidate spending. Presidential elections are held separately from other primaries, so this is not serving as a proxy for participation in a Congressional primary.

Table 3.6 shows the results from this regression, which indicate that the level of political participation, not self-declared policy stances, is an important element of the causal effect of campaign spending. For spending in both houses of Congress, and across all specifications of the first-difference regression, the negative coefficient attached to the interaction term indicates that people who had already participated in a caucus or primary election for President are significantly less likely to be influenced by candidate spending. Participation in primaries may measure people's commitment to voting, or may be a more accurate measure of the moderation of individuals' policy position than self-identification.

Individuals who vote in primaries are not identical to those who do not; the are older and more likely to be married. I tested if these different causal effects could be explained by demographic differences alone by interacting candidate spending with both age and a dummy variable for marriage; the results (not shown) do not allow me to reject the null hypothesis that differences in age and marital status do not change the causal effect of candidate spending. Therefore, the demographic differences between primary voters and non-voters is not driving the results.

3.4.3 Incumbent and Challenger Spending

Given that the estimate of causal effect of campaign spending differs across different categories of campaign disbursements, and the observed discrepancy in the habits of incumbent spending relative to the spending patterns of other candidates, and, there is reason to suspect that different measurements of candidate spending can yield different results of the relative effect of incumbent and challenger spending. Specifically, incumbents spend significantly more then challengers on refunds, political contributions, and donations, and a much lower portion of their spending on advertising.

Empirical work, spanning numerous countries, has found smaller observed effects of incumbent spending. However, incumbents outspend challengers by a significant amount, and spend a higher share of their spending on fundraising. If candidate spending is as ineffective as these studies have found, why do incumbents still spend so much effort raising funds? To ask a more perplexing question, why are individuals and Political Action Committee's so willing to give to fund less effective spending?

I consider the effect of incumbent and challenger spending on late-election preferences of potential voters, and explore how different measures of spending affect the estimate of causal effect. I change the specification outlined above, regressing individuals preferences on Incumbent and Challenger spending separately, instead of relative spending. The sample is restricted to districts in which an Incumbent was facing a Challenger [6]

The framework is similar to that used in Levitt (1994). Voter behavior at the time of election is described by:

$$NetVotes_{ie} = \phi\mathbf{X_i}+\beta_1 ln(IncumbentSpending)_e-\beta_2 ln(ChallengerSpending)_e+\delta_i+\zeta_e+\epsilon_{ie}$$

Here, $NetVotes_{ie}$ is equal to +1 if individual i votes for the incumbent, -1 if they vote for the challenger, and 0 if they vote for neither candidate. As before, omitted variables, at both the individual and Congressional District level, bias the estimates of causal effects. In this case, a positive shock to unobserved challenger quality would reduce the number of votes for the Incumbent, while increased competition would incentivize incumbents to increase their level of spending. Low-spending incumbents are likely those facing poorer competition. By taking the pre-election preferences of potential voters, I can difference out these unobserved variables.

$$\Delta NetVotes_{it} = \Delta\gamma_t+\Delta\beta_1 ln(IncumbentSpending)_{et}-\Delta\beta_2 ln(ChallengerSpending)_{et}+\epsilon_{et}$$

[6] As 2012 was the first election after re-districting, which resulted in several instances of incumbents facing each other.

As before, these values are measured using the date of the pre-election survey for time t. To control for aggregate, time-variant shocks, I again measure the difference between time t and the election. The minus sign in front of challenger spending reflects that increased challenger spending is assumed to have a negative impact on the incumbent's vote share; this formulation also normalizes coefficients on spending for both challengers and incumbents to be the same sign.

The cross-section columns (1 & 4) of Table 3.7 show the results on the cross-section regression, using election-day data only, which shows the common finding that incumbent spending has no positive effect on votes, while challenger spending appears to increase challenger vote share. These findings do not change dramatically as I consider different measurements of candidate spending, and the results appear to in fact get stronger for Senate elections. These findings are not surprising, though, considering the observed patterns of unchallenged incumbent spending. While unchallenged incumbents (who will ultimately earn huge vote shares) continue to spend in total, very little of their spending is in the form of advertising or campaign events. This would exacerbate the bias stemming from reactive spending: challenged incumbents (who will earn lower vote shares because they face competition) will spend money on advertising and campaign events; unchallenged incumbents will not.

Moving to the first-differences specifications, which provide unbiased estimates of causal effect, panel A shows the results for the regression using all disbursements as the measure of candidate spending, and is therefore most analogous to previous research. Unsurprisingly, these results are similar to previous findings: candidate spending seems to have little measurable effect, and incumbent spending does appear to be less effective than challenger spending. In fact, these results are so far from statistical significance, and vary so much across different specifications, that they would provide convincing support of a claim that campaign spending from any candidate has no effect on voter behavior

However, as I have shown, a great deal of spending is not likely being used to affect voter behavior, and would therefore be inappropriate to include in measures of candidate spending in this setting. Panel B shows the results when donations, refunds, and contributions are dropped from the measure of spending, and provides some idea of the extent to which including this data biases the measure of causal effect. As with Panel A, the estimates are not statistically significant, though in each case, the point estimates become more positive.

Panel C shows the estimate of causal effect when only the most salient forms of campaign spending are used, and the results are dramatically different. Two significant changes can be observed. First, particularly in the case of House elections, estimates of causal effect are now positive for both incumbent and challenge spending. For House elections, the change in the measure of casual effect from Panel A is stark, shifting from mildly negative to strongly positive. For both houses of Congress, the point estimates of the effect of incumbent and challenger spending are very similar. Using the preferred specification (Column 4), controlling for time fixed effects and individual characteristics, shows that a 1 unit increase in the log of incumbent spending leads to a 0.0173 increase in net votes for House elections, and a 0.0048 increase in Senate elections. A 1 unit increase in the log of challenger spending increases the net votes for that candidate by 0.0126 in House elections and 0.068 in Senate election, though the latter estimate is not statistically significant.

Second, points estimates are far more precisely estimated across all specifications. If advertising and events are the primary mechanisms through which candidates convey their platforms and send messages to voters, this increased precision is intuitive. I can reject the null hypothesis that incumbent spending has no effect, at the 5% level for House elections and 10% level in Senate elections for the preferred specification. Estimates of the effect of challenger spending are less precise, and are only significant at the 10% level for House elections, and not statistically significant for Senate elections. These results challenge the findings of previous research that found incumbent spending to be less effective. The specificity of the setting (late-election advertising and event spending on late-election changes in voter behavior) may explain some of the differences[7], but nonetheless questions whether

[7] However, as seen in Figure 3.2, though I am only measuring spending during the general election, this

previous findings have found incumbent spending to be less effective because of the use of inclusive measures of spending, instead of the narrower ones used to calculate estimates in Panel C.

To place these results in context, I compare the effect of identical increases in spending using estimates obtained by using all spending, and alternatively by measuring only spending on advertising and campaign events. This comparison will help illustrate both how use of a broader measure of candidate spending can lead to bias estimates (particularly for incumbents) and allow me to compare the differences in causal effect measured here to estimates from previous research. As I am measuring the effect of late-campaign spending, there is strong reason to believe that spending will have a smaller effect due to the diminishing marginal returns to campaign spending. Candidates had already spent a great deal of money by the time of the first survey, and many voters may have firmly established preferences.

Table 3.8 shows this comparison. When the sum of all candidate disbursements is used as the primary right-hand side variable, the measures of the effect of an identical increase in incumbent and challenger spending are starkly different, with incumbent spending decreasing the net votes for that candidate, while challenger spending results in a positive change in net votes for that candidate. However, both of these estimates are extremely small and imprecisely estimated.

When I consider spending only on communications, the difference in causal effect between incumbent and challenger spending disappears. While both estimates increase, the estimate of the effect of incumbent spending increases by significantly more, bringing in line with estimates of the effect of challenger spending. A \$100,000 increase in 1998 dollars (equal to \$139,000 in 2012 dollars), leads to a 0.0124 increase in net votes, which would be the equivalent of 0.62% increase in the vote share of incumbent (with corresponding, identical decrease in challenger vote share). Likewise, an increase in challenger spending of

time period accounted for two-thirds of total spending on advertising and events by winners of Congressional elections.

\$139,000 in 2012 dollars leads to a 0.0119 increase in net votes, or 0.596% increase in challenger vote share in a 2-candidate race. Though these effects are small, they nonetheless represent an important shift from estimates using all candidate spending. The estimates difference between incumbent and challenger spending went from +0.28, which is larger in absolute value than either point estimate, to only -0.024, implying that incumbent spending on communications in House elections is actually slightly more effective in this setting. This raises the question of whether estimates of the relative effect of incumbent and challenger spending found in previous studies would similarly change when spending is redefined as it is here.

3.5 Conclusion

This paper offers two primary insights. First, by using pre and post-election survey questions in the American National Election Study, I am able to track changes in voter preferences for candidates late in the general election, and evaluate how those changes are predicted by candidate spending. I find that candidate spending has a small but significant effect, indicating that, even late in a campaign, when voters have been exposed to large amounts of campaign messages, higher-spending candidates are able to sway voters.

Second, by using transaction-level disbursement data from the Federal Election commission, I am able to calculate the effects of different types of candidate disbursements. I find that some candidate disbursements comes in the form of contribution refunds, contributions to the national party or other candidates, or donations, and that incumbents are far more likely to engage in this kind of spending. As this spending is not being used towards a candidate's own campaign, it should be eliminated from most measures of true candidate spending. In order to obtain a precise measure of candidate spending, I use as my primary measure only spending on advertising and campaign events, and find that estimates using this measure provide different results and are more precisely estimated than estimates using the sum of all candidate disbursements, which is the measure normally used in previous research. Additionally, I find that when spending on advertising and campaign events is used, the common finding of less effective incumbent spending disappears, indicating that previous results finding an decreased effectiveness of incumbent spending may be the result of differences in spending patterns by incumbents and challengers, instead of different effectiveness of signals by the two types of candidates.

Empirical findings have a non-trivial implication on policy recommendations. If campaign spending has little effect on vote share, then spending limits could be supported under an argument of efficiency, but not if the primary use of limits is to prevent candidates from "buying" elections. If challenger spending is more effective than incumbent spending, public support of both candidates could be used if the goal was to discouraged unchallenged incumbents. If, however, challenger and incumbent spending is equally effective, this policy would have no use.

Similarly, campaign spending limits would hurt challengers if spending by challengers is more effective than spending by incumbents; however, if spending by challengers and incumbents is equally effective, such limits would likely help challengers, who spend significantly less and are therefore less likely to be constrained by the limit. This paper provides important context for research in the relative effectiveness of incumbent and challenger spending.

Figure 3.1: Total Spending By Election Competition

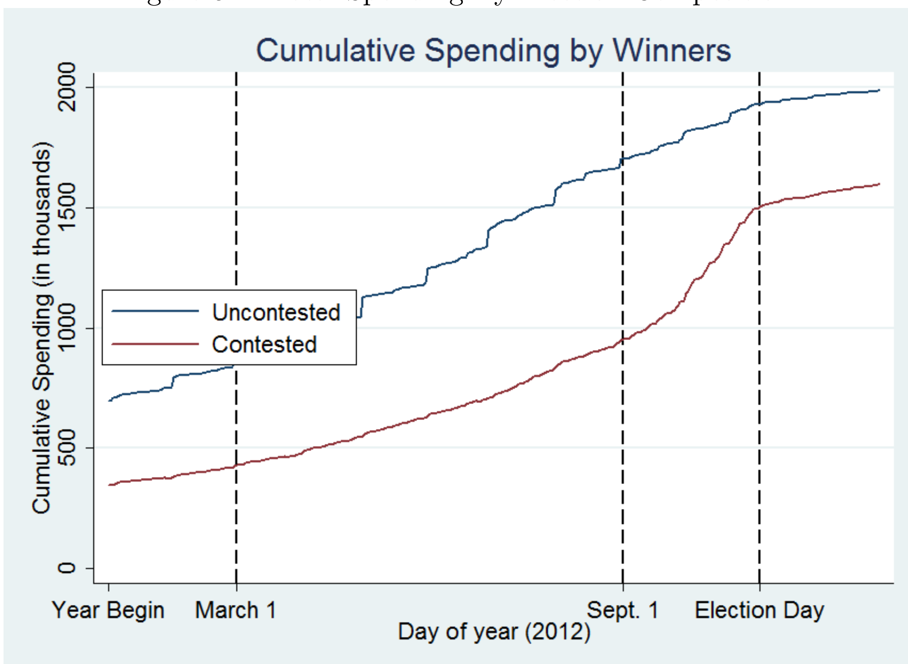


Figure 3.2: Ad and Event Spending By Election Competition

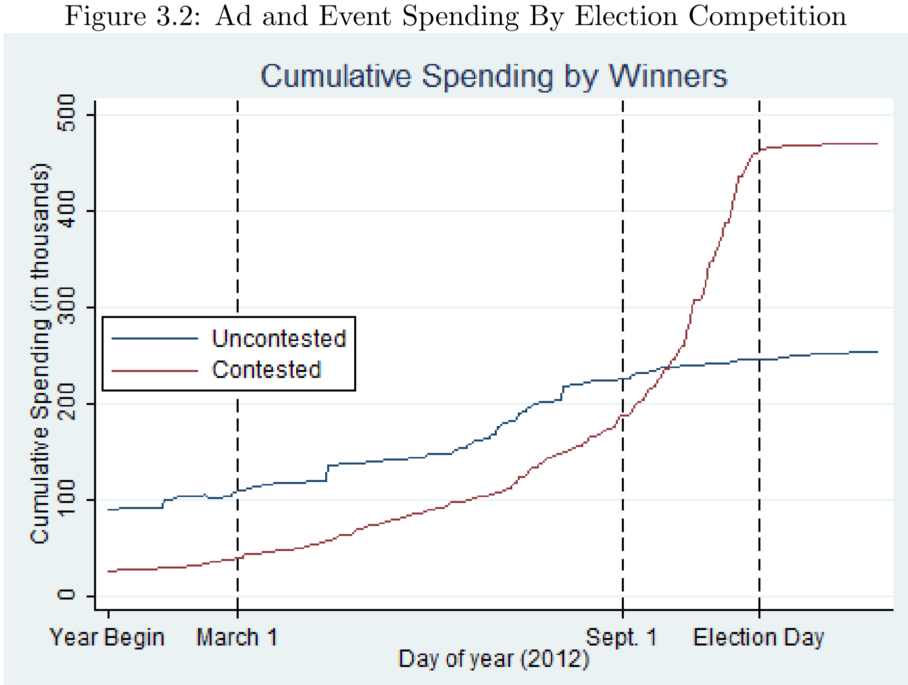


Table 3.1: Summary Statistics

	——	HOUSE	——	——	SENATE	——
Variables	Mean	Min	Max	Mean	Min	Max
Cumulative Disbursements (Pre-Election)	990,243	0	20,257,312	8,125,511	86,839	36,400,156
Cumulative Disbursements (Election Day)	1,167,219	35.25	20,400,684	10,089,221	132,690	43,585,092
Cumulative Ad and Event Spending (Pre-Election)	306,072	0	6,010,465	4,728,753	3,566	26,503,080
Cumulative Ad and Event Spending (Election Day)	437,670	34.14	9,465,565	6,007,964	6,828	31,861,580
		N=359			N=33	

	ANES Sample (Full Responses)	ANES Sample (Full Sample)	Demographics of Voters*
Percent Male	49.7	48.26	47
Percent Married	54.24	53.12	57.8
Percent Nonwhite	24.48	25	28
Age	52.14	54.5	49
Net Votes (pre-election)	-0.124	-0.116	-
Net Votes (Nov. 5)	-0.132	-	-
% Dem Vote (House)	55.05	-	50.6
% Dem Vote (Senate)	59.45	-	56.1
N	5144	10044	

*Voter Demographics from U.S. Census. Age approximated from age group averages.

Table 3.2: Effect of Campaign Spending on Voter Behavior: House Elections

PANEL A: Disbursments				
	(1)	(2)	(3)	(4)
VARIABLES	Cross-Section	FD(I)	FD(II)	FD(III)
Log(Spending)	0.309***	0.0235**	0.0194	0.0159
	(0.00264)	(0.0101)	(0.0250)	(0.0494)
R-squared	0.151	0.001	0.022	0.022
PANEL B: Spending				
	(1)	(2)	(3)	(4)
Log(Spending)	0.311***	0.0279*	0.0242	0.0212
	(0.00273)	(0.0157)	(0.0164)	(0.0172)
R-squared	0.149	0.000	0.022	0.022
PANEL C: Communications				
	(1)	(2)	(3)	(4)
Log(Spending)	0.269***	0.0108**	0.0108**	0.0111**
	(0.00259)	(0.00530)	(0.00549)	(0.00565)
Male	0.0464			0.0076
	(0.0299)			(0.0208)
Married	0.215***			0.0150
	(0.0282)			(0.0205)
Age	0.000445			0.000301
	(0.000882)			(0.00066)
Nonwhite	-0.493***			-0.137
	(0.0386)			(0.0254)
R-squared	0.139	0.001	0.022	0.023
Observations	3,465	3,398	3,398	3,373
Time FE	NO	NO	YES	YES
Ind. Characteristics	YES	NO	NO	YES

Robust standard errors cluster at Congressional district level in parentheses.

***p<0.01 **p<0.05 *p<0.1

Table 3.3: Effect of Campaign Spending on Voter Behavior: Senate Elections

PANEL A: Disbursements				
	(1)	(2)	(3)	(4)
VARIABLES	Cross-Section	FD(I)	FD(II)	FD(III)
Log(Spending)	0.0546***	0.0163	0.0126	0.0112
	(0.0061)	(0.0121)	(0.0124)	(0.0494)
R-squared	0.115	0.000	0.025	0.028
PANEL B: Spending				
	(1)	(2)	(3)	(4)
Log(Spending)	0.0548***	0.0151	0.0107	0.00947
	(0.00617)	(0.0107)	(0.0108)	(0.0111)
R-squared	0.114	0.000	0.022	0.028
PANEL C: Communications				
	(1)	(2)	(3)	(4)
Log(Spending)	0.0259**	0.00528**	0.00423**	0.00351
	(0.0113)	(0.00176)	(0.00211)	(0.00211)
R-squared	0.102	0.001	0.025	0.028
Observations	2,767	2,703	2,703	2,682
Time FE	NO	NO	YES	YES
Ind. Characteristics	YES	NO	NO	YES

Robust standard errors cluster at Congressional district level in parentheses.
***p<0.01 **p<0.05 *p<0.1

Table 3.4: Effect of $500,000 Increase in Spending

Model	FD(I)	FD(II)	FD(III)
Spending on Advertising and Events			
House	1.96%	1.96%	2.01%
Senate	0.19%	0.15%	0.13%

Table 3.5: Differential Effects of Campaign Spending(I)

	(1)	(2)	(3)
PANEL A: House			
VARIABLES	FD(I)	FD(II)	FD(III)
Log(Spending)	0.00903	0.00907	0.00909
	(0.00605)	(0.00650)	(0.00656)
Moderate*Log(Spending)	0.00536	0.00542	0.00623
	(0.0128)	(0.0128)	(0.0126)
Observations	3,396	3,396	3,371
R-squared	0.001	0.022	0.023
PANEL B: Senate			
Log(Spending)	0.00695**	0.00658***	0.00643***
	(0.00265)	(0.00178)	(0.00190)
Moderate*Log(Spending)	-0.00666	-0.00907	-0.0112*
	(0.00627)	(0.00584)	(0.00621)
Observations	2,703	2,703	2,682
R-squared	0.001	0.026	0.028
Time FE	NO	YES	YES
Ind. Characteristics	NO	NO	YES

Robust standard errors cluster at Congressional district level in parentheses.
***p<0.01 **p<0.05 *p<0.1

Figure 3.3: Total Spending By Incumbency Status

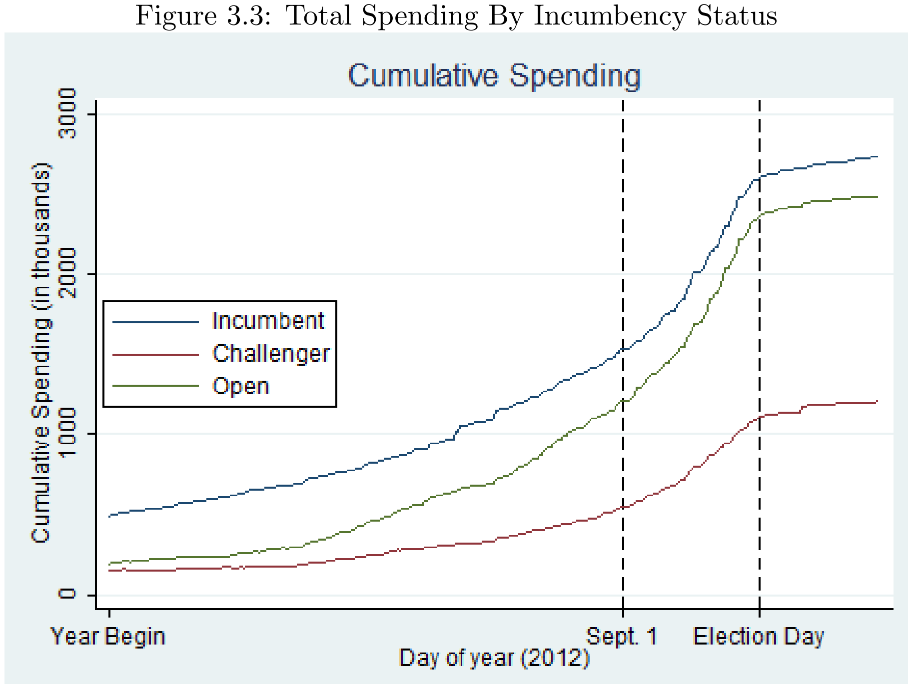


Table 3.6: Differential Effects of Campaign Spending(II)

PANEL A: House			
	(1)	(2)	(3)
VARIABLES	FD(I)	FD(II)	FD(III)
Log(Spending)	0.0189**	0.0194**	0.0201**
	(0.00799)	(0.00855)	(0.00876)
Log(Spending)*	-0.0196*	-0.0204*	-0.0214*
Primary Voter	(0.0105)	(0.0112)	(0.0113)
Observations	3,396	3,396	3,371
R-squared	0.002	0.023	0.024
PANEL B: Senate			
Log(Spending(	0.0123***	0.0114***	0.0104***
	(0.00229)	(0.00224)	(0.00226)
Log(Spending)*	-0.0138***	-0.0139***	-0.0131***
Primary Voter	(0.00259)	(0.00252)	(0.00216)
Observations	2,703	2,703	2,682
Time FE	NO	YES	YES
Ind. Characteristics	NO	NO	YES

Robust standard errors cluster at Congressional district level in parentheses.
***p<0.01 **p<0.05 *p<0.1

Figure 3.4: Distribution of Spending

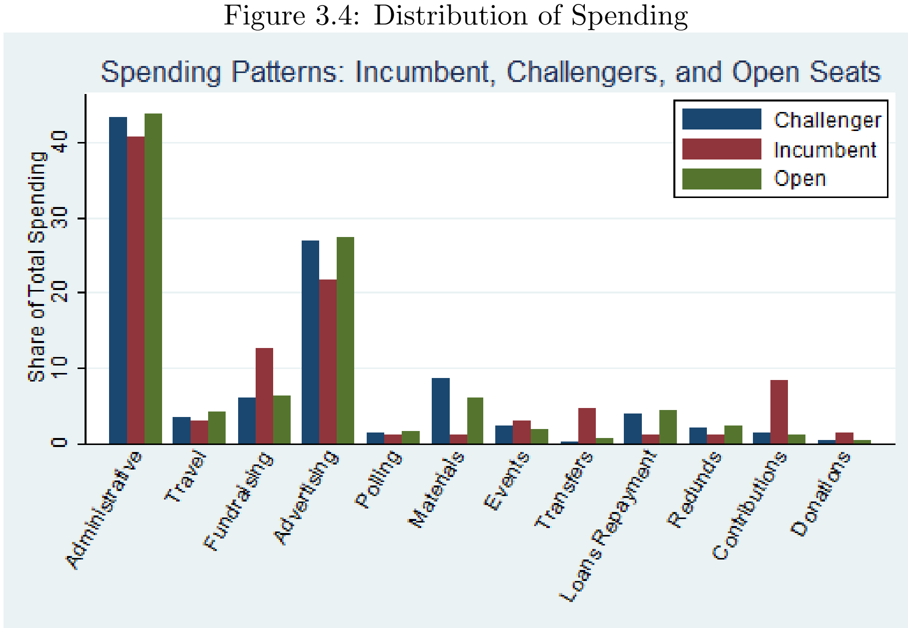


Table 3.7: Effect of Incumbent and Challenger Spending

	(1)	(2)	(3)	(4)	(5)	(6)		
VARIABLE	Cross Section	FD(I)	FD(II)	FD(III)	Cross Section	FD(I)	FD(II)	FD(III)
	House				Senate			
PANEL A: Disbursments								
Log(Incumbent	-0.063*	0.0025	-0.026	-0.027	0.038	-0.0030	0.0070	0.0051
Spending)	(0.0352)	(0.0271)	(0.0349)	(0.0355)	(0.0451)	(0.0165)	(0.0161)	(0.0172)
Log(Challenger	0.028***	0.0013	0.0096	0.0089	0.088***	-0.085	-0.0039	0.0057
Spending)	(0.085)	(0.0238)	(0.0203)	(0.0211)	(0.0229)	(0.0549)	(0.0869)	(0.0846)
Observations	2085	2030	2030	2030	2040	1984	1984	1984
R-Squared	0.020	0.000	0.031	0.033	0.064	0.001	0.030	0.033
	House				Senate			
PANEL B: Spending								
Log(Incumbent	-0.070**	0.0133	-0.0099	-0.012	0.039	-0.00045	0.0100	0.0077
Spending)	(0.0352)	(0.0268)	(0.0336)	(0.0342)	(0.422)	(0.0150)	(0.0140)	(0.0149)
Log(Challenger	0.025***	0.0078	0.00172	0.0168	0.090***	-0.081	-0.0018	0.0076
Spending)	(0.00894)	(0.0226)	(0.0197)	(0.024)	(0.0238)	(0.0544)	(0.0861)	(0.0837)
Observations	2085	2030	2030	2030	2040	1984	1984	1984
R-Squared	0.0021	0.000	0.031	0.033	0.064	0.001	0.030	0.033
	House				Senate			
PANEL C: Communication								
Log(Incumbent	-0.025*	0.0197***	0.0178**	0.0173**	-0.075**	0.0040	0.0059**	0.0048*
Spending)	(0.0130)	(0.0723)	(0.00784)	(0.0080)	(0.028)	(0.00252)	(0.00256)	(0.0027)
Log(Challenger	0.012**	0.0073	0.0127*	0.0126*	0.016**	-0.00621	0.0053	0.0068
Spending)	(0.00563)	(0.0066)	(0.00744)	(0.00756)	(0.00660)	(0.0168)	(0.0161)	(0.0158)
Observations	2085	2030	2030	2030	2040	1984	1984	1984
R-Squared	0.012	0.002	0.035	0.037	0.060	0.001	0.030	0.033
Time FE	NO	YES	YES	NO	YES	YES		
Ind. Characteristics	NO	NO	YES	NO	NO	YES		

Robust standard errors cluster at Congressional district level in parentheses.

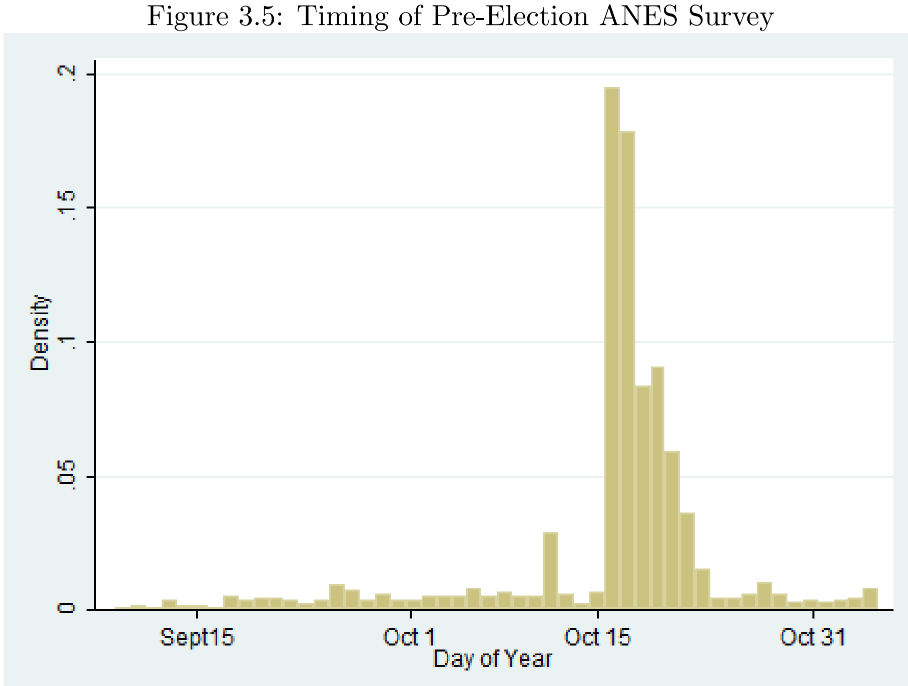
***p<0.01 **p<0.05 *p<0.1

Table 3.8: Comparison of Impact
Impact of $100,000 Increase in Spending in
House Elections (1998 Dollars)

VARIABLES	Incumbent	Challenger	Difference
Jacobsen (1985)	+0.14	+2.17	+2.03
Green & Krasno	+1.8	+1.96	+0.16
Levitt	+0.07	+0.24	+0.17
Erikson & Pallfrey	+ 0.57	+1.07	+0.50
SS (Disbursment)	-0.22	+0.06	+0.28
SS (Communication)	+0.62	+0.594	-0.026

Other estimates taken from Gerber (2004)

Figure 3.5: Timing of Pre-Election ANES Survey



Appendix A

Chapter 1 Appendix

A.1 First-Differences Analysis

An alternative specification to the fixed effects analysis described above is a first-differences analysis. Because our treatment variable (the number of RFD routes) does not vary within the pre- and post-rollout time periods, we simply take as a measure of our outcome variables the change in the average between the pre- and post-rollout periods. One concern with the fixed-effects analysis is that our instruments may be correlated with regions or county characteristics that will place counties on different trends. However, we cannot control for these characteristics within the fixed-effects framework, since any variable that does not change over time would be collinear with the county fixed effects.

The specification for the first-differences analysis is

$$\Delta Y_c = \beta \Delta Routes_c + \mu \mathbf{\Delta X_c} + \psi \mathbf{W_c} + u_c \tag{A.1}$$

where ΔY_c is the change in the average of each of our outcome variables. We calculate this by taking the average of each variable within a county over all elections held in 1908 and later, and subtracting from this value the average each variable within a county over all elections held in 1900 and before. Each of our independent variables is calculated the same way. $\Delta Routes_c$ is simply the change in RFD routes between 1900 and 1908. The key difference in this specification is the set of variables $\mathbf{W_c}$, which remain fixed within a county. We will use this to control for population density (as measured by the 1890 measure of people per square mile). This will allow us to control for the possibility of differences in trends in counties that are correlated with these values, which we were not able to do in the fixed-effects analysis.

As before, we also perform an IV regression using the same set of instruments as in the fixed-effects regressions:

$$(4)\Delta Routes_c = \phi \mathbf{Laws_c} + \sigma Z_c + \beta \mathbf{X_c} + \psi \mathbf{W_c} + e_c \tag{A.2}$$

The results of the OLS regression, shown in table A1, show that the results seen in the fixed-effects analysis is robust to this specification, with the point estimates changing little from the fixed-effects regression. Again, we see a statistically insignificant negative coefficient when using voter turnout as our dependent variable, and positive, significant results for all measures of election competition. The precision of each of these estimates is less precise, which may be due to the decrease in observations (due to the aggregation).

Results of the IV regressions are presented in Table A2 and Table A3. From these results, we see that the previous findings for the effect of RFD on the level of competition within a county remain, and are robust to the choice of instruments. As before, our point estimates increase when compared to the OLS regressions, supporting the claim of a downward bias in those estimates. However, this does not hold when turnout is used as the dependent variable. Regardless of the choice of instrument, we observe a statistically insignificant negative effect of RFD, with point estimates slightly more negative than in the OLS estimates.

A.2 Parallel Trends in the Instruments

Given the existence of time fixed-effects in our analysis, the identifying assumption for our specification is that an instrument for the number of routes be uncorrelated with trends in our outcome variables; time-related shocks must be identical across treatment groups. This allows us to select instruments that are correlated with levels of our outcome variables, which is typically a violation of the exclusion restriction, provided they are uncorrelated with trends. Therefore, we can compare pre- and post-treatment trends across different values of our instrumental variables. Parallel trends are necessary to address primary concerns with regards to our instruments. If our instruments are valid, time shocks across different values of our instruments will be identical, so when we graph these values over time, the trends in our outcome variables across different values of the instrument should be parallel.

Since one of our instruments (rainfall) is continuous, the most straightforward way of testing for parallel trends is to divide counties into groups based on their relative value of this variable. We therefore place each county in one of three groups. We then calculate the average of each outcome variable in each year for each of the three groups. For spending, we will simply divide counties by whether they spent positive amounts on roads and bridges or did not.

Figure A.1 and Figure A.2 show the pre- and post-rollout trends in several of our county-level outcome variables, across different values of each of our two instruments. Figure A.1 shows annual averages of each of our outcomes for counties that experienced the highest, middle, and lowest rainfall respectively. For turnout in Congressional elections, as seen in the upper left panel, we see several time shocks that are not common across all counties; however, there are no consistently different trends across the three groups. Results for the other county-level outcome variables are similar. Counties do not experience perfectly parallel trends, though we see so no divergence in the groups.

The results for county level spending are more convincing. The behavior of counties that spent positive amounts of money on roads and bridges appears to follow the same trends as counties that spent nothing, supporting our identifying assumption. Additionally, the causal effect can also be more easily seen in Figure A.2. Since counties spending positive amounts on roads receive more treatment, we would expect to see upwards shifts in the trends of those counties to occur between the pre- and post-rollout periods. Consider the fifth panel of Figure A.2 before RFD, counties spending nothing on RFD consistently have a higher share of the vote going to small parties than counties that spent money on roads. However, in the post-rollout period, though the trends between the groups are parallel, counties that spent money on roads (and therefore received more routes) are consistently voting more frequently for small parties. The trends within both periods are parallel, and the shift that occurred between the two periods is a visual representation of the causal effect. Based on the behavior of these pre- and post-rollout trends, we consider 1890 county-level spending the preferred instrument, though we will present results for both, to aid in the interpretation of results, and as a robustness check for our findings.

Figure A.1: Trends: Rainfall Variable

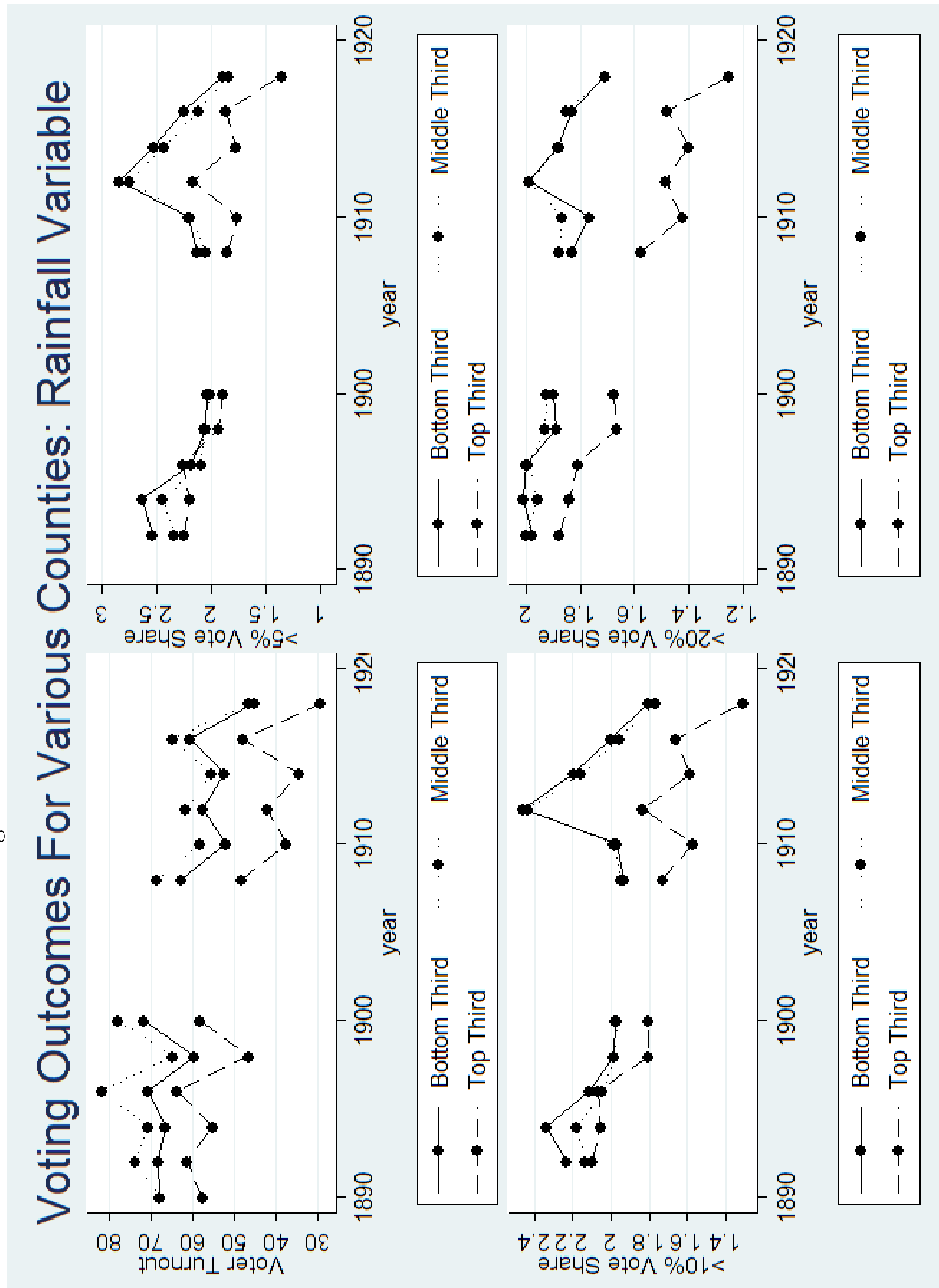


Figure A.2: Trends: Spendingl Variable

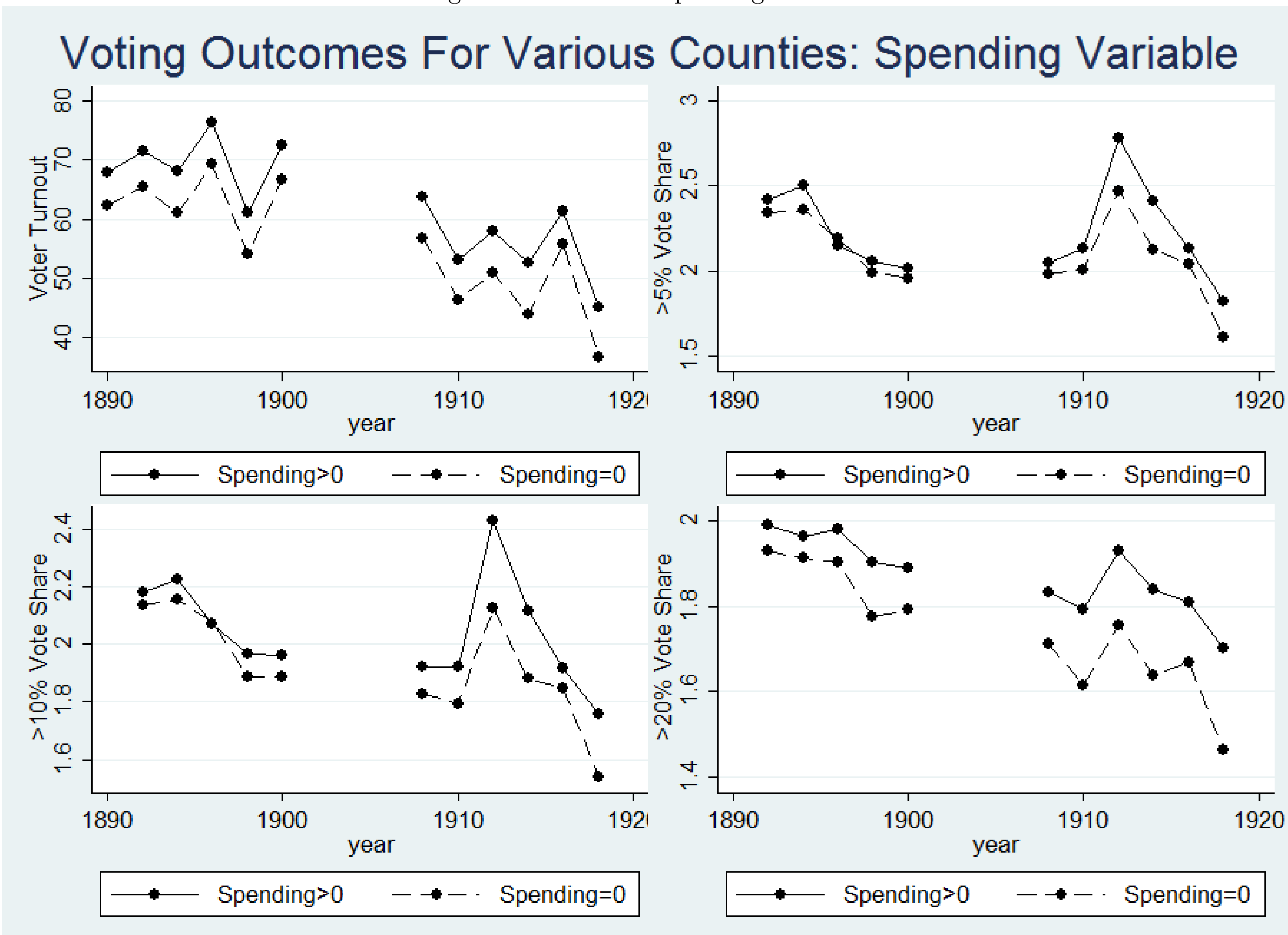


Table A1: First Differences Results
OLS Regression

VARIABLES	(1) Congressional Turnout	(2) > 5 Percent	(3) > 10 Percent	(4) > 20 Percent	(5) Small Party Share
Routes	-0.0743	0.00889**	0.0104***	0.00887***	0.0837*
	(0.150)	(0.00399)	(0.00347)	(0.00318)	(0.0479)
% Urban	-0.257**	-0.00938**	-0.00667*	-0.00511	-0.0983
	(0.119)	(0.00421)	(0.00341)	(0.00306)	(0.0703)
% Urb. Sq.	0.00213	0.000268***	0.000174***	0.000114**	0.00388***
	(0.00196)	(7.61e-05)	(5.29e-05)	(4.64e-05)	(0.00109)
% Imp. Farmland	0.00194	-0.00823*	-0.00759**	-0.00590**	-0.199***
	(0.0993)	(0.00445)	(0.00375)	(0.00291)	(0.0678)
% Nonwhite	-0.331	0.0145	0.0113	0.00887	-0.0498
	(0.314)	(0.0101)	(0.00921)	(0.00814)	(0.113)
% Foreign	-0.493	-0.00940	-0.0136	-0.0128*	-0.0971
	(0.300)	(0.0110)	(0.00893)	(0.00694)	(0.222)
Ln(population	0.590	0.222***	0.157**	0.119	1.632
	(3.463)	(0.0752)	(0.0716)	(0.0719)	(1.058)
Density(1890)	-0.000145	4.23e-05***	4.23e-05***	1.40e-05**	0.000553***
	(0.000370)	(7.47e-06)	(7.46e-06)	(6.22e-06)	(0.000193)
Observations	2,414	2,414	2,414	2,414	2,414
R-squared	0.029	0.194	0.181	0.130	0.106

Standard errors, clustered at Congressional district level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A2: IV Regerssion with First Differences

VARIABLES	(1) Turnout	(2) Turnout
Routes	-0.369	-0.224
	(0.324)	(0.505)
% Urban	-0.197**	-0.215*
	(0.0997)	(0.121)
% Urb. Sq	0.00453*	0.00349
	(0.00241)	(0.00348)
% Imp. Farmland	-0.00437	0.0156
	(0.124)	(0.107)
% Nonwhite	-0.128	-0.126
	(0.320)	(0.266)
% Foreign	-0.669***	-0.612***
	(0.233)	(0.212)
Ln(Population)	-2.859	0.206
	(5.775)	(5.279)
Density	-0.000177	-0.000107
	(0.000390)	(0.000444)
Observations	2,549	2,633
R-squared		0.012
Instrument	Rainfall	Spending

Standard errors, clustered at Cong. District level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A3: IV:Competition

First Difference with Instruments: Competition

VARIABLES	(1) < 5 Percent	(2) < 5 Percent	(3) < 10 Percent	(4) < 10 Percent	(5) < 20 Percent	(6) < 20 Percent
Routes	0.0201**	0.0275**	0.0243***	0.0303***	0.0179**	0.0238**
	(0.00975)	(0.0121)	(0.00819)	(0.0104)	(0.00738)	(0.00977)
% Urban	-0.00851**	-0.00820*	-0.00575*	-0.00563*	-0.00434*	-0.00430
	(0.00406)	(0.00431)	(0.00316)	(0.00340)	(0.00263)	(0.00290)
% Urb Sq	0.000182*	0.000127	7.17e-05	2.91e-05	4.87e-05	6.78e-06
	(9.70e-05)	(0.000112)	(7.69e-05)	(9.12e-05)	(6.53e-05)	(8.17e-05)
% Imp. Farmland	-0.00831**	-0.00712*	-0.00739**	-0.00620*	-0.00562**	-0.00472**
	(0.00390)	(0.00389)	(0.00320)	(0.00319)	(0.00248)	(0.00237)
% Nonwhite	0.0117	0.0103	0.00816	0.00720	0.00766	0.00699
	(0.00913)	(0.00864)	(0.00848)	(0.00796)	(0.00715)	(0.00643)
% Foreign	-0.0107	-0.00910	-0.0144*	-0.0128	-0.0142**	-0.0126*
	(0.0102)	(0.0101)	(0.00824)	(0.00827)	(0.00658)	(0.00669)
Ln(Population)	0.320***	0.370***	0.268***	0.316***	0.185*	0.243**
	(0.118)	(0.122)	(0.102)	(0.107)	(0.0973)	(0.104)
Density	4.74e-05***	5.05e-05***	4.83e-05***	5.08e-05***	1.83e-05***	2.08e-05***
	(8.25e-06)	(9.02e-06)	(8.21e-06)	(8.72e-06)	(6.58e-06)	(7.29e-06)
Observations	2,549	2,633	2,549	2,633	2,549	2,633
Instument	Rainfall	Spending	Rainfall	Spending	Rainfall	Spending
R-squared	0.159	0.084	0.095	0.010	0.086	0.012

Standard errors, clustered at Congressional district level in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Appendix B

Chapter 2 Appendix

B.1 Robustness checks

This section presents the results of several robustness checks of the primary findings of the differences-and-differences model outlines above. First, I run two placebo tests, using different years as dates of policy change. I regress the effect of runoff elections on vote share using just elections held before 2001, with a placebo law change between the 1994 & 1996 elections. I then repeat this procedure with post-2000 elections, with a placebo law change in Florida between the 2004 and 2006 elections. Since no actual law change occurred at these points, a statistically significant result would give reason to question the validity of the model's assumptions.

Table A1 presents the results from these two placebo tests. Neither test gives statistically significant results, and are of opposite signs. These results indicate that the estimates of causal effect are not simply the result of secular trends that are not being accounted for in the primary model.

Several recent papers have addressed the issue of the potential for biased standard errors in differences-in-differences analysis, each with a proposed remedy. Bertrand et al. (2004) recommend several potential mechanisms, although the only recommendation when there are a small number of groups (as in this case) is to collapse all data into pre- and post-treatment periods, and the low predictive power of this method may be overly conservative.

For this setting, I collapse the data into pre-treatment and post-treatment averages for each of the six types of offices (Governor, Cabinet, State House and Senate, United States

House and Senate). I do this because the average of the outcome variable differs across office types, and changes between the pre- and post-treatment averages could be driven by changes in composition. I calculate the average for the cumulative vote share for all candidates finishing third or lower, the percentage of elections that are Republican, the percentage of election that involve incumbents, and the average number of candidates, and run the following regression.

$$\overline{VoteShare}_{sp} = \gamma_s + post_p + \mu * \overline{\mathbf{X}}_{sp} + \beta * Runoff_{sp} + \varepsilon_{ist}$$

Where $\overline{VoteShare}_{sp}$ is the average vote share for low-performing candidates for each body of government in state s in period p. $\overline{\mathbf{X}}_{sp}$ is the average for the set of co-variates listed above. Column 1 of Table A2 shows the results of this procedure. The point estimate of this regression is larger to that found in the primary regression, but as expected the standard errors are significantly larger, and eliminate any statistical significance. Though these standard deviations are large, they also highlight the small measured effect of runoff elections on voting behavior. While the standard errors are large compared to the primary model, they are not so large that they would have prevented me from rejecting the null hypothesis of no effect is the point estimates were comparable to those found in other settings (50% of the baseline).

I find similar results when I use the procedure outlined by Donald and Lang (2004), who propose a two-step process for calculating standard errors and t-statistics. The results for this procedure (not shown) were similar; large standard errors eliminate any statistical significance of the estimates of causal effect.

Table A1: Dependent Variable: Vote Share
Placebo Tests for of Causal Effect

	(1) 1990-2000	(2) 2002-2010
Runoff	1.310	-1.179
	(1.617)	(1.732)
Num. of Candidates	6.809***	5.210***
	(0.340)	(0.269)
Republican	0.0294	0.098
	(0.725)	(0.736)
Incumbent Involvement	-3.104***	-5.814***
	(0.972)	0.879
N	577	760
R^2	0.450	0.401

Robust standard errors, in parentheses, are clustered at the state level. State Level Controls include dummies indicating no-excuse absentee voting and provisional ballot laws
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Dependent Variable: Vote Share

	(1) Betrand et al.
Runoff	
	(2.895)
Num. of Candidates	3.20***
	(0.671)
Republican	0.721
	(3.023)
Incumbent Involvement	-2.452
	(3.625)
Post-2001	5.178*
N	75
R^2	0.387

Robust standard errors, in parentheses, are clustered at the state level. State Level Controls include dummies indicating no-excuse absentee voting and provisional ballot laws
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix C

Chapter 3 Appendix

C.1 Robustness Checks

To address the issue of individuals exhibiting a bias towards one of the candidates, or toward the incumbent, the ANES has employed several measures to elicit honest answers. First, researchers recognized that when the survey asked people a series of questions about their opinion of the incumbent at the beginning of the survey, they were more likely to claim to having voted for the incumbent. To address this, the Study moved when the questions about incumbents were asked, and questions about who the individual voted for lacked any incumbent or challenger notation. Survey-takers were presented with a ballot card which featured either the name of the candidates, or a list of political parties.

To discourage voters from claiming that they voted when they did not, the survey added a supplemental set of answer options, providing voters with opportunities to explain why they did not vote (e.g. "I usually vote, but didn't this time"). This increased the percentage of respondents who claimed to have not voted to closer to the national average.

If people who over-report voting are more likely to claim to have voter for the candidate who engaged on more advertising of campaign events, over-reporting of voting could lead to an upward bias of my estimate of causal effect. To test if voter's over-reporting of voting could lead to systemically different estimates of causal effect, I tested the primary hypothesis using only data from people who had completed the survey online. Research has shown that self-administered surveys are less susceptible to over-reporting bias (Holbrook and Krosnick (2010)). If survey respondents were more likely to claim they voted due to social pressures, online respondents would be more likely to answer honestly than respondents in face-to-face surveys. Table A1 compares the effect of spending on advertising and events separately for respondents taking a face-to-face (FTF) or online survey survey. There appears to be no systematic difference between the effect of spending on the two samples. While face-to-face respondents appeared to be slightly less affected than online respondents by spending in House of Representative elections, whey were slight more affected by Senate spending. These results offer little evidence that the causal effect of spending is being driven by face-to-face respondents, who may be more likely to claim having voted even when they did not.

To test for the existence of bias towards the winner in post-election survey, I test whether the identity of the winner predicts a change in an invidual's stated preference. I constructed a dummy variable equal to 1 if a Republican won a seat, and 0 if a Democrat won. I then regressed the change in net votes on candidate spending, indivdual characterics, and dummy variable. The results for this regression are presented in Table A2. If respondents are more likely to claim to have voted for a candidate because that candidate won, there should be a positive correlation between the dummy variable for Republican win and the change in the net votes for Republican.

C.2 Assignment of Spending Type

To assign spending to the correct codes, I used the following procedure. First, I assigned codes to observations where the attached note was identical to one of the 12 codes (i.e. "Donation", "Advertisement"). Next, I determined, for all identical notes, the most frequent disbursement code to which it was assigned. Through this, I was able to assign codes for 633,879 (87.7%) of contributions, constituting $1.687 Bill (85.03%) of contributions. Of the remaining transactions, the majority failed to be assigned due to a lack of standardization or misspelling in the description field. Assignment was done manually using each individual disbursement description.

Table A1: Causal Effect by Survey Method

PANEL A: House		
	FTF	Online
VARIABLES	FD(III)	FD(III)
Log(Spending)	0.00562	0.0133**
	(0.0104)	(0.00658)
Observations	916	2,455
R-squared	0.0067	0.0088
PANEL B: Senate		
	FTF	Online
VARIABLES	FD(III)	FD(III)
Log(Spending)	0.00662	0.000921
	(0.00463)	(0.00367)
Observations	702	1,980
R-squared	0.076	0.0074
Time FE	YES	YES
Ind. Characteristics	YES	YES

Robust standard errors cluster at Congressional district level in parentheses.
***p<0.01 **p<0.05 *p<0.1

Table A2: Test for Winner Bias

	(House)	(Senate))
VARIABLES	FD (III)	FD (III)
Republican Win (=1)	0.0304	0.0444
	(0.0200)	(0.0268)
Male	0.00843	-0.0251
	(0.0209)	(0.0247)
Married	0.0117	-0.0232
	(0.0207)	(0.0229)
Age	0.000365	-0.0000195
	(0.000661)	(0.000833)
Nonwhite	-0.0100	-0.0384
	(0.0263)	(0.0174
Spending	0.0112*	0.00175
	(0.00567)	(0.0028)
Observations	3,337	2,683
R-squared	0.022	0.029

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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