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ABSTRACT

We employ a regression discontinuity design based on close elections to estimate the rents from a seat in the U.S. congress between 1850-1880. Using census data, we compare wealth accumulation among those who won or lost their first race by a small margin. We find evidence of significant returns for the first half of the 1860s, during the Civil War, but not for other periods. We hypothesize that increased opportunities from the sudden spike in government spending during the war and the decrease in control by the media and other monitors might have made it easier for incumbent congressmen to collect rents.

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1 Introduction

A central role of political institutions is to control politicians and prevent the abuse of power for personal gain. This idea goes back to political theorists such as David Hume and James Madison who emphasized the role of Constitutions in guaranteeing that rulers behave and govern in the public – and not only their private – interest.[1] More recently, an extensive literature in political economy has addressed the conflicts of interest between elected representatives and their constituencies. The main concern is that elected representatives, once in office, may use their political power to further their own interests and not those of the electorate. For example, politicians may redistribute resources to themselves or may favor certain interest groups in return for bribes or campaign contributions. Seminal contributions such as Barro (1973), Ferejohn (1986), Buchanan (1989) and Banks and Sundaram (1993, 1998) explicitly modeled this agency problem and explored the role of elections and other political institutions in providing incentives and regulating the behavior of politicians.[2]

The most common view among scholars is that democratic political institutions – including free and fair elections, checks and balances, constraints on the executive, and a free and independent media – provide the most promising environment for keeping incumbents accountable to their constituencies and for preventing the abuse of power.[3] As Besley (2006) notes, the most renowned kleptocratic regimes have occurred in non-democratic systems. Existing empirical evidence from cross-country regressions also suggests that corruption is more prevalent in less developed countries with weaker political institutions (see for example Mauro, 1995). However, even democratic political institutions may fail to prevent rent-seeking in some situations. For example, the absence of *actual* political competition may reduce the disciplining role of elections. Similarly, as Besley (2006, p. 37) states, "real accountability requires that those who hold politicians to account have sufficient information – for example about the politician's action – to make the system work." A major empirical question in this context is understanding the environments or conditions under which demo-

[1]In the Federalist Papers (No. 57) Madison stated that the aim of every political Constitution should be to "take the most effectual precautions for keeping [rulers] virtuous while they continue to hold their public trust."

[2]See also, Harrington (1993), Persson and Tabellini (2000), Fearon (1999), Berganza (2000), Hindriks and Belleflamme (2001), Le Borgne and Lockwood (2001, 2006), Smart and Sturm (2003, 2004), Besley (2006), and Padro i Miquel (2007), as well as Stigler (1971), Peltzman (1976), Denzau and Munger (1986), Austen-Smith (1987), Baron (1994), and Grossman and Helpman (1994, 1996, 2001).

[3]In a recent paper Acemoglu, Egorov and Sonin (2010) explore theoretically the flexibility of institutions under democratic regimes. In their model, perfect democracy ensures that the most competent government possible emerges.

cratic political institutions will be more or less effective at controlling politicians behavior, creating a wedge between *formal* and *actual* accountability.

One possible way to study the degree to which democratic political institutions prevent the abuse of power for personal gain is to establish the extent of systematic rent-seeking by politicians.[4] Unfortunately, the study of rent extraction faces substantial empirical challenges, because it is often difficult to detect or measure the accumulation of rents by politicians in a systematic way. One way to assess the magnitude of political rents is to track the wealth of politicians. To the degree that rents are large, we should observe politicians accumulating substantially more wealth while in office than they would have otherwise.

In this paper we use historical census data from the U.S. to address these questions and estimate the magnitude of political rents for members of the U.S. House of Representatives during the period 1850-1880. We compare wealth accumulation by members of the U.S. House of Representatives relative to candidates who ran but lost the election. To address selection and endogeneity concerns that arise from comparing wealth accumulation of election winners and losers, we employ a regression discontinuity design (RDD) based on close elections. That is, we compare wealth accumulation in the decades between 1850 and 1880 among those who won or lost their first congressional race by a small margin. This allows us to estimate the *causal* effect of serving in congress on wealth accumulation during this period.[5] The outcome of close elections provides us with quasi-random assignment of political power. It therefore allows us to isolate the effect of serving in Congress from the effect of other characteristics of these individuals – such as talent, connections, or charisma – that are correlated with serving in congress and wealth accumulation.

There are several reasons why the U.S. during this period provides an ideal setting for exploring some of these ideas. First, the U.S. census recorded wealth in 1850, 1860, and 1870, and we have found the individual census records for a large sample of candidates. We also collected information on the number of domestic servants in each candidate's household as reported in the 1850, 1860, 1870 and 1880 censuses as other proxies for wealth. We study the number of servants because wealth was self-reported, so there could be concerns about misreporting (we address this in more detail in the data appendix). Second, the U.S. was, by

[4]Rent-seeking or wealth accumulation is only one way in which, in the absence of constraints, politicians may abuse from political power. For example, some politicians may use political power to persecute their enemies or to further their own ideological agenda.

[5]Regression discontinuity designs were first introduced by Thistlethwaite and Campbell (1960). See Hahn, Todd and Van der Klaauw (2001) and Imbens and Lemieux (2008) for a general discussion of regression discontinuity designs, and Lee (2008) for a concrete application to close elections.

most accounts, a consolidated democracy by this time. Its average polity score during this period was 8.8 – exceeded only by New Zealand and Switzerland – and it had the maximum possible score in most of its components. If democratic institutions succeed at regulating the behavior of politicians, then the U.S. is a natural setting to explore this hypothesis. Most importantly, this period exhibits important variation in the political environment that will allows us to hypothesize about the environments under which rent-seeking may be more prevalent.

Throughout the paper we focus on "free" states where slavery was prohibited, because prior to emancipation slaves were counted as part of personal wealth. Thus, it is difficult to compare wealth figures before and after the abolition of slavery for former slave owners.

Our results can be summarized as follows. We find no evidence of abnormal wealth accumulation by congressmen who served during the 1850s, during the second half of the 1860s, or during the 1870's. This is a remarkable result and strikes in contrast with evidence for other countries. One plausible interpretation is that U.S. political institutions during "normal times" were effective at controlling politicians and preventing the abuse of power for personal gain. However, we do find evidence of significant rent-seeking for congressmen who served during the first half of the 1860s, which coincided with the Civil War. Those who won their first election by a narrow margin and served during the 37th-39th Congresses (1861-1866) accumulated, on average, about 40% more wealth between 1860 and 1870 than candidates who lost the election and did not serve. For the average congressman this corresponds to about \$20,000 in additional wealth – nearly \$1,000,000 in present values.[6]

We hypothesize that increased rent-seeking during the Civil War years was caused by increased *opportunities* for enrichment from office and by decreased *control* and oversight by voters, the media and state institutions during this period. The increase in *opportunities* is illustrated in Figure 1A, that shows the evolution of nominal spending by the Federal government between 1850 and 1880. There was a dramatic spike in nominal government spending during the Civil War years, from about \$60 million just before the outbreak of the

[6]The main patterns can be seen even in the simplest summary statistics. For those who ran during the Civil War but lost their first race for congress by a small margin, median wealth increased from \$22,000 in 1860 to \$39,000 in 1870. Median wealth for those who *won* their first race by a small margin and served during the Civil War increased from \$22,000 in 1860 to \$55,000 in 1870. The additional \$16,000 in wealth accumulation for winners relative to losers during the Civil War correspond to approximately \$800,0000 in present values. For the non-war years, median wealth for those who lost their first race by a small margin, increased from \$14,000 in 1860 to \$35,000 in 1870. Amongst those who won their first race and served during the non-war years, median wealth increased from \$16,500 in 1860 to \$30,000 in 1870. During this period, winners actually did slightly worse relative to losers.

war to almost \$1,200 million at the highest point during the war – an increase of almost 2,000%. This was driven by the need to mobilize, equip, feed, and move armies on a scale never before seen in U.S. history. The sudden spike in government spending might have made it easier for incumbent congressmen (and other politicians) to collect rents. For example, they could channel contracts towards firms in which they had an interest, or collect side-payments or legal fees from contractors in exchange for favorable treatment. Procurement was especially frantic and disorganized during the first half of the war, as an army of almost 700,000 men was built essentially from scratch (see Wilson, 2006a). The potential decrease in *control* and oversight are illustrated in Figure 1B, which shows reporting on corruption by local newspapers during this period. Remarkably, reporting on corruption decreased during the Civil War years, and started increasing again in 1866 right after the end of the war. A plausible interpretation of this pattern is that the the war battles and other political events during this period distracted the media from covering the dealings of politicians. Something similar may have happened to voters and other state institutions who, focused on the immediate events of the war, may have decreased their oversight over the allocation of expenditure by members of congress and other politicians. This interaction of increased opportunities for self-enrichment and reduced control by the media and the voters, may explain the higher levels of rent seeking during the first half of the 1860s that we observe.

Other findings support the hypothesis that the abnormal wealth accumulation by congressmen during this period reflected rent-seeking associated with the war. First, we find that wealth accumulation was particularly significant by congressmen who represented states that played an important role providing supplies during the war. Second, we find that congressmen who served during the Civil War in committees that were responsible for most military appropriations became richer than other members of congress and candidates who ran but never served. This, together with additional anecdotal evidence gives us further confidence in our interpretation.

Our results can be interpreted more broadly and suggest that rent-seeking may be more prevalent in episodes of crisis such as natural disasters, wars or other types of political and economic turmoil. During these periods government expenditure often increases substantially, increasing the amount of resources on which politicians might prey. At the same time control and oversight by the media and other state institutions may be less effective than in normal times.

We also report estimates from an alternative empirical strategy that relies solely on winners, not on the comparison of winners and losers. This is based on a simple "before-and-after" design first introduced in Querubin and Snyder (2009). For example, we compare the accumulation of wealth between 1860 and 1870 for representatives first elected during the five years just before 1870 with those first elected during the five years just after 1870. The first group had access to congressional rents that would appear in their 1870 wealth, while the second group did not. The estimates from this approach reveal a remarkably similar pattern to the one obtained when using the RDD, despite the different samples and methodologies. This gives us further confidence in our results. Finally, an extensive data appendix provides additional evidence that the wealth data reported in the census is reliable for the purposes of our study.

In addition to the literature on political agency, our paper is related to other strands of research. The literature on political selection – discussed in section 3 – is important for motivating our empirical strategy.[7]

Our paper also contributes to a small but recently growing literature on estimating the value of political connections. Several papers have established the unusual economic returns of individuals and firms with political connections in countries with relatively weak political institutions. See for example, Acemoglu et.al. (2008) for the case of Colombia, Ferguson and Voth (2008) for Nazi Germany, Fisman (2001) for the Indonesian case, and Johnson and Mitton (2003) for Malaysia. Finally, in the context of a more strongly institutionalized democracy, Eggers and Hainmueller (2009) collect probate records of candidates to the British parliament in the period 1950-1970, and use an RDD to estimate the effect of holding a seat in parliament on wealth at death. They find significant positive effects for Conservative MPs but not for Labour MPs.[8] The lack of returns to a seat in congress in the U.S. that we report for the non-war years, stands in contrast to this literature and provides suggestive evidence of the strength and effectiveness of American political institutions in controlling politicians during "normal times."

Other papers have explored the value of political connections in the U.S. in the current era.[9] These papers confirm the absence of returns from political connections in the U.S. during "normal times" that we report.[10] Perhaps more closely related to our paper, Acemoglu

[7]See Besley (2005, 2006) for an overview of the literature on political selection.

[8]However, the data sources in Eggers and Hainmueller (2009) do not allow them to control for initial wealth, an important determinant of wealth accumulation.

[9]Lenz and Lim (2009) use reported assets of U.S. members of congress, matched with a sample from the Panel Study of Income Dynamics, and find that members of congress do not have higher asset returns than their matched counterparts. Similarly, Fisman et. al. (2006) do not find any evidence of abnormal returns to firms connected to Vice-President Cheney around his nomination or other health-related episodes.

[10]Using different methodologies, Groseclose and Milyo (1999) and Diermeier, Keane and Merlo (2005)

et.al. (2010) find that in the context of the recent financial crisis, financial firms connected to Timothy Geithner, experience an abnormal return of 15% after his nomination as Treasury Secretary. This is consistent with the broader interpretation of our results; namely, the potential weakening of political institutions and increase in the value of political connections during times of crisis.

The rest of the paper is organized as follows. Section 2 provides a brief description on the historical and political background during the period of our study. Section 3 addresses the main methodological challenges associated to estimating the rents from congress and describes our econometric approach. Section 4 describes the different data sources and provides some descriptive statistics. Section 5 presents the main results of the paper and provides evidence consistent with our interpretation of rent extraction during the Civil War. In Section 6 we report the estimates from our "before-and-after" analysis. Section 7 concludes. An extensive data appendix describes the data sources in great detail and provides evidence for the reliability of census wealth data for the purposes of our study.

2 Historical and Political Background

In the second half of the 19th century, several features of the U.S. might have served to check corruption and allow the state, the voters and the media to control politicians against the abuse of power. First, the political system introduced checks and balances to prevent the concentration of power. Political power was diffusely distributed, at least as measured by taxes and spending. Except during the Civil War, the federal government raised far less in taxes and spent far less than state and local governments overall. State governments probably also did more in terms of economic regulation. Even though comprehensive civil service reform had not been introduced by this time, Federal statutes and local legislation also prohibited the bribery of public officials. Voters also had mechanisms to keep politicians accountable. Throughout much of this period the political competition between the two major parties was fierce. And, while electoral fraud existed at some level, election outcomes were "fair" in the sense that both parties had a reasonable chance of winning a large share of the offices in most states. For example, in about half of the congressional races during the time period of our study, the winning candidate won with less than 55% of the vote. At least in theory, in such a competitive environment voters could easily have voted the

estimate the returns to a career in the U.S. Congress. These papers cannot distinguish between the monetary returns to office and other non-pecuniary benefits, such as "ego rents." Also, they can only estimate the returns of a seat in congress at the intensive margin, because they have no data on those who run and lost.

most highly corrupt candidates out of office. Finally, the media was expanding substantially during this period. By 1860 there were approximately 2,000 local newspapers in circulation and by 1870 this number had almost doubled (see Rowell, 1869). This would have allowed the voters to learn about the dealings of politicians and learn about uncovered corruption scandals. For example, the bribery scandal of Tammany Hall's "Boss Tweed" was uncovered by newspapers in 1871.

Several scholars have written about corruption during this period. However, most accounts of political corruption focus on the *pre-war* years and the *post-war* years. A few scholars argue that corruption was rampant during the 1850s. Summers (1987, p. 14) writes, "In every way the decade before the Civil War was corrupt. The 1850s were as depraved as any other age, and, at least from the evidence available to historians, far more debauched than the 1840s."[11] Writing about the events of 1857, Stampp (1990, p. 30) notes, "Corruption was not a new phenomenon in American politics... but corruption had become distressingly common in this period of accelerating commercialization and industrial growth."[12] For the post-war years, most historians probably consider the years of Ulysses S. Grant's presidency, from 1869-1877, to be the most corrupt in U.S. history. This period has been dubbed "the era of good stealings."[13]

Interestingly, less has been written about political corruption during the Civil War. This is perhaps not surprising given the overwhelming importance and scale of the war itself. Compared to the war – the massive mobilization and casualties, campaigns and battles, strategies and tactics, actions by military leaders, and so on – corruption by politicians was a relatively minor affair that did not receive much attention by the media or historians. A

[11]Summers goes on to argue that corruption was a factor leading to secession. In particular, it helped bolster the arguments of both abolitionists and Southern Rights men. The former argued that corruption enabled the "Slave Power" to dominate the national government. It achieved its goals, especially the extension of slavery into the territories, by bribing weak and venal northerner politicians. The latter argued that "only disunion could keep the South from being infected with Northern corruption, just as revolution had freed the colonists from the contagion of British practice in 1776" (Summers, 1987, p. 290).

[12]He explains the growth as follows (p. 28): "Most of the financial corruption resulted from the temptations dangled before politicians by land speculators, railroad promoters, government contractors, and seekers after bank charters or street railway franchises. Often the politicians were themselves investors in western lands, town properties, railroad projects, or banking enterprises, and the distinction between the public good and private interests could easily become blurred in their minds."

[13]The list of scandals includes Black Friday (Gold Panic), the Whiskey Ring, the Star Route Postal Ring, the New York Custom House Ring, the Trading Post Ring, the Delano Affair, and the Credit Mobilier scandal. In his discussion of the scandals of the Grant administration, Josephson (1938, p. 127) argues, "It is high time that we cease to think of the spoilations of the General Grant Era as 'accidental' phenomena, as regrettable lapses into moral frailty.... We must turn rather to examine the systematic, rational, organized nature of the plundering which was carried on at the time." For a revisionist view, see Summers (1993).

notable exception is Wilson (2006b, p.45) for whom:

> the apogee of the spoils system occurred during the Civil War, when party leaders took advantage of the war emergency to reward party supporters with reckless abandon... The economic mobilization of the Union to defeat the Confederacy – by far the largest government spending project in the United States during the 19th Century – was nothing more than an outsized pork-barrel project for a party machine.

There is also anecdotal evidence on corruption during this period. The case of Simon Cameron, Lincoln's first Secretary of War who was dismissed in part for showing too much favoritism in awarding military contracts, is the most well-known. Also, some biographies describe cases where congressmen, senators and other politicians profited from the war – e.g., Oakes Ames and his family obtained lucrative contracts to supply shovels, swords and other equipment to the Union army, and Thurlow Weed engaged in a variety of money-making schemes. But most of the literature, both by contemporary observers and historians, focuses on *private* war profiteering by businessmen, and the general problems of military contracting on a massive new scale.[14] This emphasis probably makes sense, since this is where the bulk of the wartime profits were.

Despite the claims about political corruption during the 19th Century, there is no systematic evidence regarding how widespread and pervasive the corruption actually was. In fact, our evidence suggests that corruption may not have been as widespread during the 1850's and 1870's as many historians claim. The anecdotes cited by contemporary observers and historians for this period, may have been isolated incidents and may actually constitute most of the actual cases of corruption.

[14]See, e.g., Nagle (1999), Wilson (2006a), and Keeney (2007). For example, Keeney (2007, p. 16) notes that "profiteering and fraud were hallmarks of government business during the Civil War. Hasty mobilization, loose enforcement, large-scale emergency buys, and lack of coordination at the federal level led to a situation very attractive to people looking for a quick fortune." Nagle (1999, p. 177) describes the wartime years as a time when previously honest businessmen became "rapacious profiteers" who "hurried to the assault on the treasury, like a cloud of locusts." Suppliers charged exorbitant prices, sold shoddy blankets, uniforms, and boots, and even supplied dangerous weapons and ammunition; middlemen extracted large fees for suspect services; businessmen, politicians, and even military officers engaged in trade with the enemy – especially buying cotton from the south. Congress investigated and catalogued many of the abuses, then tried to tighten regulations with laws such as the False Claims Act (1863), but, as Keeney (2007, p. 17) notes, "most of these regulation were only loosely enforced and soon of necessity went by the wayside."

3 Methodology

Estimating the monetary rents of political office-holding is difficult for a variety of theoretical and methodological reasons. In this section we discuss various reasons why a simple comparison of wealth accumulation by politicians and non-politicians may lead to biased estimates of the rents from office. Next, we introduce the regression discontinuity design (RDD) based on close elections in order to estimate the *causal* effect of serving in congress on wealth accumulation.

3.1 Selection into Politics

The main problem underlying the estimation of the rents of a seat in congress is the fact that congressmen are different from other citizens with respect to various characteristics that may be difficult to measure or observe. In many societies, basic literacy, age and wealth restrictions that must be satisfied by anyone attempting to hold a political position already create a wedge between politicians and the remainder of the population. More generally however, the decision to become a politician is influenced by a series of personal characteristics that are plausibly correlated with other personal outcomes such as economic success. On the one hand, individuals more talented at accumulating wealth may find holding office more costly, since they must sacrifice high returns in the private sector. If this were true, then a simple comparison of wealth accumulated by politicians and non-politicians would tend to *underestimate* the rents from politics.[15] On the other hand, if only the most talented individuals, who would have been very successful in the private sector anyway, manage to win elections and become politicians, then a naive comparison of politicians and non-politicians will tend to *overestimate* the rents from holding office. There is, in other words, selection into politics which makes it very hard to disentangle the effect of access to office from other personal characteristics.[16] The direction and magnitude of the bias from a

[15]Several papers, such as Besley and McLaren (1993) and Casselli and Morelli (2004) develop models in which "low quality" – i.e., less competent and more dishonest – citizens have a comparative advantage in pursuing politics. In a similar vein, Mattozzi and Merlo (2008) develop a dynamic optimization model in which individuals with heterogenous market ability and political skills must choose between a job in the private and the political sector. Their model suggests that higher salaries for politicians actually decrease the average quality of citizens deciding to become politicians.

[16]Several theoretical papers in the political economy literature have attempted to understand the process of selection into politics. Osborne and Slivinski (1996) and Besley and Coate (1997) analyzed the decision of individuals to participate as candidates in the political process as the result of optimizing behavior. In these models, the preferences of individuals, their policy-making abilities, and the costs of running for office play an important role in the decision to become a politician. Most importantly, these models illustrate that the

simple, nave comparison of politicians and non-politicians is not trivial to measure.

3.2 Empirical Specifications

The previous discussion suggests that a simple comparison of the wealth accumulation of politicians and non-politicians will likely yield a biased estimate of the economic returns to politics. The descriptive statistics in section 4 will indeed reveal that individuals who ran for office were different from the rest of the population. In particular, they were mainly very rich individuals even when compared to individuals with similar high-paying and high-status occupations. It is likely that these individuals also differed from other citizens on other characteristics that are correlated with the fact that they were significantly richer.

To estimate a causal effect of political office-holding on wealth accumulation we employ a regression discontinuity design (RDD). We must consider the following counterfactual: how much wealth would politician i have accumulated had he not been elected? Close elections, i.e. elections where the winner won by a very small margin, provide us with an empirical counterpart of the above counterfactual. If we believe that the outcome of close elections is as good as random, then we can assume that any differences in wealth accumulation between close winners and close losers can be attributed to holding the political office sought.

In the empirical analysis below, we focus on what happens to candidates in their *first* race for congress. This allows us to estimate the effect of *ever* serving in congress, i.e., the extensive margin. For those who win their first race by a close margin, the first election provides the cleanest quasi-experimental assignment to office. As shown below, the vast majority of candidates who lose their first election never run for congress again. Thus, we focus on the first race even for losers, because those who run more than once may be different from "typical" losing candidates in ways we cannot measure.[17]

In our empirical approach we follow Hahn, Todd and Van der Klaauw (2001), Imbens and Lemieux (2008) and Angrist and Pischke (2009). We estimate regressions of the form:

$$Wealth_i^t = \beta_0 + \beta_1 Wealth_i^{t-1} + \beta_2 Winner_i^t + \beta_3' \mathbf{X}_i + f(VoteShare_i) + \epsilon_i^t$$
$$\text{for all } i \text{ such that } |0.5 - VoteShare_i| < h \quad (1)$$

process of entry into politics is not trivial, since "citizens contemplating standing for office must anticipate who else will enter the race and the resulting voting equilibrium" (Besley and Coate, 1997, p. 86).

[17] In other situations it might make sense to consider more than just the first race. For example, if the vast majority of candidates ran twice, then we would probably want to consider both races, and classify candidates on the basis of their closest race.

where $Wealth_i^t$ captures the wealth of candidate i in census year t, $Winner_i^t$ is a dummy variable equal to one if candidate i won his first race for congress and served in the U.S. House during the period between the two census years, and $Wealth_i^{t-1}$ corresponds to the initial value of wealth in the preceding census year (10 years earlier). The vector X_i includes a battery of controls such as age, occupation dummies and state fixed effects. The term $f(\cdot)$ corresponds to an nth order polynomial of the forcing variable, i.e., each candidate's vote share in their first race for congress. We allow a different polynomial on either side of the 0.5 threshold. The choice of bandwidth h implies a sensitive trade-off. A small bandwidth more closely resembles the quasi-experimental assignment of close elections, but comes at the expense of efficiency due to small sample sizes. Large bandwidths are undesirable because they make use of observations far away from the threshold, posing a threat to the identification assumption.

In our benchmark specifications, we follow Imbens and Lemieux (2008) and estimate local linear regressions using a 3% bandwidth. With this bandwidth we observe balance across covariates for winners and losers of close races. Moreover, this value of h is consistent with the optimal selection procedure proposed by Imbens and Kalyanaraman (2009).[18] Following previous work we also report the robustness of our results to alternative values of h and to the inclusion of higher order polynomials in the vote share.

The estimates based on equation (1) correspond to the reduced form effect of winning the first race by a small margin on wealth accumulation – they correspond to "intention to treat" estimates. However, some candidates who lose their first election run again and win. Similarly, a few candidates do not serve in congress despite winning their first election. As we discuss below, this is not a major concern in our case, because a substantial majority of candidates (92%) who lose their first election never serve in Congress, and thus the number of "non-compliers" is small. Therefore, we do not report IV estimates but focus throughout the paper on the reduced form.[19]

In all our analysis we report the regression results using wealth in levels and logs as our dependent variable. This allows us to capture different forms of rents from office. If rents

[18]Following Imbens and Lemieux (2008) we compute the optimal bandwidth after discarding observations in the tails of the distribution – 50% of observations on either side of the threshold. For most periods and dependent variables, the Imbens and Kalyanaraman (2009) procedure suggests using a bandwidth of 3% with the exception of specifications using ending log wealth as dependent variable where the optimal bandwidth is closer to 2%. For simplicity, we focus on a 3% bandwidth in the benchmark specifications but show the robustness of our results to using a 2% bandwidth.

[19]The patterns revealed by our IV estimates are identical, but all of the estimated coefficients are slightly larger.

correspond mostly to bribes or side-payments to politicians, then specifications using wealth in levels may be preferred. On the other hand, if rents correspond to returns on initial wealth – as might be the case, for example, of access to privileged information regarding investment opportunities – then the log functional form may be more appropriate. For regressions using wealth in levels as a dependent variable we estimate median regressions in order to reduce the influence of outliers on our estimates.

4 Data and Descriptive Statistics

In this section we describe briefly our data sources and data collection process. We defer a more extensive discussion to the data appendix. We rely on two main data sources for our analysis. The first is the electoral and biographical data on candidates to the U.S. Congress between 1845 and 1875. The second is the U.S. Censuses of 1850, 1860, 1870 and 1880, which provide us with the wealth, occupation and other characteristics of the candidates. In this section we describe our data sources and present some descriptive statistics.

4.1 Electoral and Political Data

The electoral data consists of election results for each election to the U.S. House of Representatives between 1845 and 1875. Additional information on the winners of each election is available from a biographical dataset compiled by the ICPSR, as well as the *Biographical Directory of the U.S. Congress*. Finally, we use Canon, Nelson and Stewart (1998) to construct measures of party leadership and committee positions.

Several features of the electoral and political environment of the mid-19th century are relevant for our analysis. As shown in more detail in the data appendix, congressional elections were quite competitive compared to today. About 33% of congressmen received less than 53% of the vote in their first race. This will be important for our empirical analysis that will rely mainly on these close races. There were also very few "career congressmen" compared to today. Fewer than 24% of those who won their first race ran for congress in more than two elections, and only 16% served three or more terms. Finally, those who lost their first race almost never served in congress – 80% of those who lost their first race never ran again, and fewer than 9% served in congress during our period. The numbers are similar for those who lost their first race by a small margin. This suggests that the issue of "non-compliers" is not a major concern when interpreting our reduced form estimates.

4.2 Census Wealth Data

The wealth data is from the 1850, 1860 and 1870 Federal U.S. censuses. These are the only years in which the Federal census collected information on citizen's wealth. The census reported real estate wealth in 1850, 1860 and 1870, and personal wealth in 1860 and 1870. As an alternative measure of wealth, we also collected information on the number of domestic servants in the 1850, 1860, 1870 and 1880 censuses. In addition, census records provide information on year and place of birth, county and town of residence and occupation.

We attempted to find the census record in each census year of every candidate for the House of Representatives during our period who obtained at least 25% of the vote. All census records before 1930, including slave schedules, are available in ancestry.com. This is a genealogical website that provides images of the original census records and a search engine to locate records by first, middle and last name, as well as year and place of birth, and place of residence. Despite the automated matching of many of the records, wealth figures and occupations had to be entered manually. Also, there are many miss-spelled names in the search engine, probably due to the fact that is difficult to decipher the handwriting on many census pages. Figure 2 shows one sample census record, the page on which Abraham Lincoln was listed in 1860 (noted by the arrow). This illustrates the various types of data that had to be coded manually for each record – in particular, occupation, wealth, and the number of servants. We provide more details on the matching and searching process in the data appendix.

We successfully located and entered data on about 10,000 census records, out of a universe of about 12,000 cases.[20] This corresponds to an overall success rate of about 80%, higher than that reported by previous work using census data for this period. As reported in the data appendix, the attrition rates for the different census years and types of individuals were similar. Thus, we do not believe that selection bias from those individuals whose records we could not locate is a major concern.

The wealth figures in the census records were self-reported and were not verified by other government officials. In addition, it was often difficult to distinguish cases in which the respondent had no wealth from cases in which the individual respondent refused to provide a figure to the enumerator, because in both of these types of cases the wealth fields in the census record were left blank. However, we provide extensive evidence on the reliability of the census data in the data appendix. In particular, based on alternative wealth sources we

[20]The biographical information allowed us to know the year of death of those who served. Naturally, we did not attempt to find the census record of those who were dead in a given census year.

show that election winners are not more or less likely to misreport their wealth than election losers. As an alternative measure of wealth, we also collected information on the number of servants living with each individual in every census year (this will be our only measure of wealth for 1880). Servants living in every dwelling had to be reported to the enumerator and were, naturally, harder to hide and misreport than real or personal wealth figures. We report all our results using both reported wealth and the number of servants as dependent variables. See the data appendix for a detailed description of the coding of servants.

Perhaps more important for our purposes is whether census wealth data can be used to detect wealth accumulation of individuals known to have been corrupt and to have received bribes. We explore this in the context of a prominent scandal: the Lacrosse & Milwaukee Railroad scandal.

4.3 Detecting Corruption using Census Data: The Lacrosse & Milwaukee Railroad Scandal

In 1856 the Federal government ceded land for a major railroad project to the state of Wisconsin, but left it to the state to decide which railroad(s) would receive the grant. Several railroads competed for the land grant in 1856 and 1857, among them the Lacrosse & Milwaukee Railroad Company. In 1858, amidst fears that the railroad was in danger of going bankrupt, creditors demanded an audit of its accounts. A major scandal broke as the accounting revealed many troubling items, most importantly that the railroad had spent over \$800,000 to bribe various federal and Wisconsin officials.[21]

Especially useful for our purposes, the committee appointed to inquire into the alleged bribery of the railroad reported the exact value of the bribes received by all Wisconsin officials involved in the scandal. The report revealed, for example, that 49 state representatives each

[21] At the Federal level, the railroad paid \$105,000 to congressmen for federal approval of the land grant. However, the bulk of the bribes were paid to various Wisconsin officials, including 59 assemblymen and 19 state senators, \$10,000 to state Supreme Court Justice Abram Smith, and \$17,000 to state house clerks to expedite business. Republican Governor Coles Bashford received over \$50,000 in stocks and \$15,000 in cash. The railroad also appeared to be concerned about the scandal becoming public, since it paid \$25,000 to silence a key member of the Wisconsin state legislative committee investigating the scandal (Horace A. Tenney), and also paid bribes to journalists, including \$10,000 to the editor of the *Milwaukee Sentinel*. Government officials however, appeared to be less worried about their involvement in the scandal and taking bribes from the railroad. State assemblyman George W. Parker explained why the railroad's payments were not bribes: They were made *after* voting for the railroad's bill, not before. Moreover, having adjourned, they "were no longer a Legislature... [so] our acceptance could in no way be considered or regarded as a bribe... it could in no way affect our honor or integrity as men... and further, that coming at the time it did, and especially when we remembered that the Company had just received at our hands 3 to \$10,000,000 worth of lands as a gratuity, we could not find it in our hearts to refuse."

received \$5,000 in bribes, 7 more received \$10,000 each, 1 received \$20,000, and 1 received \$25,000. State senators generally received larger bribes – 10 received \$10,000, 4 received \$20,000 and 1 received \$5,000. We attempted to find the census records in 1850 and 1860 of all those Wisconsin officials who served in the state government in 1856.[22] This allows us to test whether those who received larger bribes, accumulated, on average, more wealth between 1850 and 1860. To do this analysis we can estimate a regression of the form:

$$Wealth_i^{1860} = \gamma_0 + \gamma_1 Wealth_i^{1850} + \gamma_2 BribeAmount_i + \gamma_3' \mathbf{X}_i + \epsilon_i \qquad (2)$$

where $\mathbf{X}_i$ includes age and age^2 as controls. The coefficient of interest is γ_2, which measures the extent to which larger bribes in 1856 are reflected in higher census wealth in 1860.

In Table 1 we report estimates of γ_2 in equation (2). Columns 1 and 2 report the estimated coefficients when all wealth variables – wealth in 1850 and 1860 and bribe amount – are expressed in levels. Column 1 shows median regression estimates and column 2 shows OLS estimates. Column 3 reports the OLS estimate when the logs of these variables are used.[23] The estimates reveal that the bribe amount is strongly and positively correlated with reported wealth in 1860, and the coefficient is statistically significant at conventional levels. The point estimate in column 1 implies that a bribe of \$10,000 translates into about \$10,000 in additional census wealth in 1860. These results provide further evidence of reliability of the census wealth data. They suggest that we can detect changes in wealth that occur over a decade resulting, partly, as a consequence of rents and bribes from holding office.

4.4 Descriptive Statistics

In this section we present some basic descriptive statistics of wealth levels and changes during this period. To compare candidates in this period to other groups of the population, we use the Integrated Public Use Microdata Series (IPUMS) census samples for 1850, 1860, 1870 and 1880 collected by the Minnesota Population Center. These constitute representative

[22]We found both the 1850 and 1860 records for 94 out of 139 government officials that were in power during the 1856 legislature, when the scandal took place.

[23]We only estimate the regression for the government officials who, according to the committee's report, received a positive bribe by the railroad. It is not clear what to assume regarding officials not listed in the report. In particular, 21 state legislators supposedly did not receive any bribe. Of these, 8 voted in favor of the Lacrosse & Milwaukee Railroad's bill, and 13 voted against. Those who voted "yes" may have benefited in other ways – e.g., the committee report noted that John Fitzgerald had a direct interest in the railroad – and those who voted "no" might have received payments from other railroads. If we assign a value of zero bribes to those not mentioned in the report, and who voted *against* the railroad, then the estimate for the specification in levels reported in column 1 remains positive and statistically significant. However, the point estimate for the log specification falls substantially and is no longer statistically significant.

1% samples from each population census and provide information on every single variable collected in the census. This allows us to compare the candidates not only to the population as a whole but also to individuals of similar occupations. These IPUMS samples as well as other samples from the 1850-1880 censuses have been used by many economic historians.[24]

Table 2a reports summary statistics on initial wealth, prior to running for congress – i.e., figures for wealth and servants in 1850 are for candidates who ran for office in the 1850s, figures for 1860 are for individuals who ran for congress in the 1860s, and figures for 1870 are for individuals who ran for congress between 1870 and 1875. Given the skewness of the wealth distribution, we report mean and median values for wealth but only the mean for number of servants (the median number of servants is 1 in all but one sub-group of candidates).

The first outstanding fact is that congressional candidates – especially those who actually served in congress – were very rich men. Not surprisingly, the average and median wealth of congressional candidates exceeds substantially that of the mean and the median citizen in the IPUMS samples. Congressional candidates were in the 99th percentile of the overall wealth distribution. Somewhat more surprisingly, congressional candidates were rich even when compared to other "elite" groups. The simplest comparison involves lawyers. Law was by far the most common occupation of candidates in our sample – nearly half of all candidates were lawyers. On average, congressional candidates who were lawyers were more than three times as wealthy as the average lawyer in the country in 1860. The gap was even larger in 1870. Also, the gap is larger in relative terms in all decades if we compare medians. The median congressional winner was located in the top decile of the wealth distribution of lawyers nationwide. Thus, during this period congressional nominations were restricted to a rich elite. The average (median) wealth of a congressman during the 1860s was about \$43,000 (\$17,000). To put these numbers in context and bring them to present values, we use a multiplier of 50.[25] This would imply that the average wealth of congressmen during the 1860s was more than \$2,000,000 in present values, and the median was over \$800,000. Groseclose and Milyo (1999) estimate that in 1992 the average wealth of congressional incumbents was \$997,000 while the median was \$356,000. Thus, congressmen in the mid-1800's were relatively rich

[24]The list includes Soltow (1975), Williamson and Lindert (1980), Atack and Bateman (1981), Kearl and Pope (1984), Steckel (1988, 1989, 1990, 1994), Shammas (1993), Ferrie (1996), and Stewart (2006).

[25]The daily wage of a carpenter in Massachusetts is \$1.45 in 1850 and \$1.70 in 1860. The median daily wage of a carpenter today is about \$160. This suggests a multiplier of about 100 to put 1850-1860 dollars in today's dollars. If we use the CPI then the multiplier is about 30. The correct multiplier probably lies somewhere between these two, so we use 50 as a rough guess.

even when compared with congressmen today.

A comparison of different types of candidates reveals that those who won and actually served tended to be slightly richer than candidates who ran for congress but never served – though, the differences are not overwhelming. This suggests that some of the selection issues discussed in section 3.1 may be relevant in our context, since initial wealth, plausibly correlated with traits important for economic and political success, is correlated with winning elections and serving in congress.

A comparison of wealth levels across parties does not reveal any robust patterns. In the 1850s, Democratic candidates were poorer than Whigs. However, in the 1860s Republican and Democratic candidates were similarly wealthy, and in the 1870s Republicans were richer than Democrats.

Finally, Table 2b presents some descriptive statistics on *changes* in wealth and in the number of servants. The samples correspond to candidates who ran during the respective decades. Overall, the wealth of congressional candidates roughly doubled between 1850 and 1860, and increased by about 80% between 1860 and 1870. These rates of wealth accumulation are similar to the values exhibited by synthetic cohorts constructed from the IPUMS samples.[26] Of course, given the much larger initial wealth of congressional candidates the same change in percentage terms corresponds to a much larger increase in absolute terms.

In addition, there is some evidence that those who won and served in congress became richer than those who lost – particularly during the 1860s.[27] Column 4 reveals that on average, those who served during the 1860s became about 11 percentage points richer than losers between 1860 and 1870, using the median change in column 3, this corresponds to an additional $5,000. The figures for servants in column 5 exhibit a similar pattern. In the 1850s, winners did only slightly better than losers in terms of wealth, and about equally well in terms of servants. Figures for the 1870-1880 period in the last column do not suggest any abnormal wealth accumulation by winners relative to losers. A preview of the RDD results can be seen by comparing wealth accumulation of close winners and close losers in Table 2b, i.e. candidates who won or lost their first election with a vote share between 47% and 53%.

[26] We do not have enough observations to construct synthetic cohorts for lawyers in the IPUMS sample.

[27] More generally, the differences between winners and losers reflect a positive and statistically significant relationship between a candidate's vote share and wealth accumulation during each decade that suggests that electoral success may be correlated with other traits that are also correlated with wealth accumulation. For our baseline sample, an OLS regression of the change in the log of total wealth between 1860 and 1870 against the vote share in the first election reveals that an increase in one percentage point of the vote share is associated with an increase in total wealth of 7%. The coefficient is statistically significant at the .05 level. This confirms, once again, the importance of the RDD described in section 3.2.

There is no evidence that close winners accumulated more wealth than close losers during the 1850s or 1870s – if anything, close winners did slightly worse. However, there is evidence that those who won their first election in the 1860s by a small margin experienced a change in wealth between 1860 and 1870 that was 37 percentage points larger than that of those who ran but lost by a small margin. We explore this more systematically in Section 5.

Overall, while the differences between winners and losers are all positive, they are hardly overwhelming. This is already suggestive of the effectiveness of political institutions during this period in preventing systematic rent-seeking by politicians. As we show below however, some of these averages mask a large amount of heterogeneity.

There is also an interesting partisan pattern. During the 1850s, when Democrats controlled congress, Democratic candidates (both winners and losers) accumulated about 40% more wealth than Whigs. This pattern is reversed in the 1860s. During this decade Republicans took control of congress, and their candidates accumulated about 20% more wealth than Democrats. This suggests that candidates from the party in control became richer than candidates from the minority party. Since it is not the focus of our paper, we leave a thorough investigation of this for future work.

5 Results

In this section we present the main results of the paper. First, for reference, we present standard OLS estimates that illustrate average differences in wealth accumulation by congressmen and those who ran but lost. Next we present the RDD estimates for the different decades and some evidence on the underlying mechanisms that may explain the patterns in the data.

In all our tables we consider the following samples. The samples for the 1850-1860 period include candidates who ran for the 31st-36th Congresses; these are candidates who ran between 1848 and 1859 and, if they won, served at some point between 1850 and 1860. The samples for the 1860-1870 period include candidates who ran for the 36th-41st Congresses. Those who won during the 1860's served in congress under very different political, fiscal, and economic environments. The first half of the decade, the 37th-39th Congresses, coincided with the Civil War and immediate post-war years. As discussed in the introduction, federal spending rose to unprecedented levels, and most of the attention of government, indeed of the whole society, focused on the war. During the non-war years – i.e. the 36th Congress just before the war and the 40th and 41st Congresses afterward – the situation was more normal and federal spending after 1866 returned to levels roughly similar to those of the 1850s. Thus, throughout our analysis, we report separate estimates for those who ran during the Civil War and the non-war years. Finally, the sample for the 1870-1880 period corresponds to those who ran for the 41st-43th Congresses (between 1869 and 1873) and, if they won, served at some point between 1870 and 1875.

In addition, all regressions include the following set of controls: initial wealth (for regressions with wealth as dependent variable), initial number of servants (for regressions with number of servants as a dependent variable), age, age^2, occupation dummies for lawyer, farmer and manufacturer/merchant/banker, and a full set of state fixed effects.[28]

5.1 OLS Estimates

In Table 3 we present standard OLS estimates of equation (1) on the full sample of candidates where we drop the control function $f(\cdot)$ altogether. We present estimates of β_2 for our three dependent variables *Ending Wealth*, *Log Ending Wealth* and *Ending Servants* in the first, second and third columns respectively. Estimates in the first column correspond to median regressions while those in columns 2 and 3 correspond to OLS estimates.

The estimates in columns 1 and 2 of panel A, provide some suggestive evidence that those who served during the 1850s accumulated more wealth than those who ran but never served. However, this is not robust across dependent variables as the estimate in column 3 based on number of servants as a dependent variable is very close to zero and is not statistically significant. The estimates in panel B provide evidence that those who served during the Civil War years accumulated more wealth than whose who ran during the 1860s but never served. On the contrary, estimates for those who served during the non-war years are very small and statistically insignificant. These patterns in panel B are robust across the different dependent variables. Finally, there is no evidence that those who served between 1870 and 1875 accumulated more servants between 1870 and 1880 than those who ran during this period but did not serve. The estimate in panel C is very small and it is not statistically significant.

The patterns revealed by the OLS estimates already anticipate the main results of the paper. However, these estimates must be interpreted with caution as they confound the effect of serving in congress with other characteristics of the candidates correlated with winning elections and economic success (wealth accumulation). In order to isolate the effect of serving

[28]In all our median regressions with wealth in levels as dependent variable, we include *region* fixed effects.

in congress and estimate the rents from office we must turn to the regression discontinuity design (RDD) discussed in section 3.

5.2 Basic RDD Estimates

In this section we present the main results of our regression discontinuity analysis based on equation (1). We begin by providing some validity checks that give us confidence in our identification strategy. Next, we report our main regression estimates for the different decades.

5.2.1 RDD Preliminaries

If the outcome of close elections is as good as random, then we should observe relative "balance" across various characteristics of those who win and lose in these close elections. In our benchmark specifications we will focus on candidates whose first vote share lay in a 3% window around the 50% threshold. This is the bandwidth suggested by the Imbens and Kalyanaraman (2009) optimal bandwidth procedure. An important question is whether candidates in this "close election" sample resemble the quasi-random assignment that underlies the identification strategy based on close elections.

Table 4 presents differences in various covariates observed in the census records across candidates in the "close election" sample. We present differences for the different decades. Reassuringly, we find no systematic evidence of any major difference across winners and losers in any of the covariates in our main samples. Differences in initial wealth and the initial number of servants across winners and losers are small and statistically insignificant. This is perhaps the most important piece of evidence, since one potential concern is that richer candidates might be able to influence the outcome of elections – even close elections – in their favor. There is also no systematic evidence of any statistical differences in age or occupation groups across winners and losers, indicating that these factors do not influence election outcomes in the close election sample.

Recent papers by Snyder (2005), Caughey and Sekhon (2010) and Carpenter et al. (2011) criticize RDD studies that rely on close elections, arguing that there are anomalies even very near the 50% threshold. They show that in U.S. House elections, incumbents win noticeably more than 50% of the very close races – especially those where the winning margin was less than 1% – and that candidates from the party in control of state offices, such as the governorship, secretary of state and state house and senate, hold a systematic advantage in extremely close elections. These papers do not analyze the time period we study, so we provide some evidence here (a more detailed description is available in the data appendix). We find no statistical evidence of "sorting at the threshold" for incumbents or for candidates from the governor's party using a 1% or a 2% bandwidth. Similarly, for a 3% bandwidth, we find no evidence of sorting for candidates who ran in the 1850's or during the Civil War years (our main sub-sample of interest).[29]

5.2.2 RDD Regressions

Table 5 presents our main results for the 1850s (panel A), 1860s (panel B) and 1870s (panel C). We focus on local linear regressions in a 3% bandwidth. The first column – for *Ending Wealth* – reports estimates based on median regressions while regressions in columns 2 and 3 are estimated by OLS.

Consider the estimates for the 1850-1860 period. The estimates are *negative* and statistically insignificant across the various specifications and dependent variables. A similar pattern is revealed by the estimate in panel C for the 1870-1880 period: the point estimate is small and statistically insignificant. This is a remarkable result. In spite of some anecdotal evidence and claims made by historians and observers at the time about corruption during the pre-war decade and during the Grant administration in the 1870s (recall the discussion in section 2) our estimates indicate that, overall, congressmen during this period were not able to accumulate wealth at an abnormal rate relative to those who ran but lost by a small margin. A plausible interpretation is that democratic political institutions during these two decades were effective at preventing systematic and widespread rent-seeking by members of congress. Moreover, estimates for these decades stand in contrast with evidence for other countries and different periods where the value of political connections has been found to be large.

In panel B we report the results for the 1860-1870 period where we allow a different coefficient for those who served during the Civil War years and those who served in the non-war years.[30] At the bottom of the panel we report the p-value of an F-test for whether the

[29] We do find some evidence of sorting for elections in the post-Civil War years using a 3% bandwidth. However, these outcomes are not too surprising, since as Folke et al. (2011) show in districts with a "normal vote" different from .5 we actually *expect* candidates from the favored party to win more than 50% of the time except in extremely small windows around the threshold. Incumbents, as well as candidates whose party won the governorship, tend to be from the favored party rather than the disadvantaged party in a district and thus it is hard to disentangle the incumbency or party-control effect from the effect of the "normal vote" in the district.

[30] We do not report results from a fully saturated regression where we allow all controls, forcing variable and the state fixed effects to vary across the Civil War and non-war sample. Estimating a fully saturated model does not affect the point estimates noticeably, but naturally causes the standard errors to increase

coefficients for the Civil War and non-war samples are equal. The estimates provide evidence of a large effect of serving in congress during the Civil War on wealth accumulation. The point estimates for *Ending Log Wealth* in column 2 suggests that those who served during the Civil War accumulated about 40% more wealth than those who ran and lost by a small margin. Using average wealth values in 1860, the estimated coefficient implies that Civil War congressmen accumulated an additional $17,000 dollars (approximately $850,000 in current values) relative to those who ran but did not serve. This is remarkably similar to the point estimate in column 1 produced by the median regression using *Ending Wealth* as the dependent variable that suggests that congressmen who served during the Civil War accumulated an additional $18,000 (approximately $900,000 in current values) relative to those who ran but did not serve. The estimate using *Ending Servants* confirms the same pattern, indicating that Civil War winners accumulated about 0.6 more servants than losers. This is comforting since the number of servants, although a coarse measure of wealth, was harder to misreport to census enumerators.[31]

By contrast, the estimates for congressmen who served during the non-war years are always small and are never statistically significant. The estimate for *Ending Wealth* in column 2 is very close to zero while the estimates in columns 1 and 3 are negative and statistically insignificant. The F-tests in columns 1 and 3 always reject the null hypothesis that coefficients for Civil War and non-war years are the same at the .05 level. The F-test for *Ending Log Wealth* also rejects this hypothesis but only at the .10 level.[32]

Figure 3 illustrates these results graphically. It shows RDD plots for two variables:

due to the small sample sizes. Moreover, the inclusion of the state fixed effects does not affect the point estimates substantially, and these fixed effects are rarely if ever jointly statistically significant.

[31] Congressional salaries are unlikely to explain these differences. Until 1856 congressmen did not receive a salary but a per-diem of $8 that produced an average annual payment of $880. From 1856 to 1865 congressmen received an annual salary of $3,000, and from 1866 to 1871 they received an annual salary of $5,000. This was a large salary for the time, and congressmen during this period were not obliged to resign their existing jobs. They did, however, have to forego income they could have earned in their regular jobs during the time devoted to the congressional sessions. This was about one half of a year over the course of a typical congress. In addition, congressmen had to set up a second residence in Washington D.C., a large expense which consumed a large portion of their salary, according to contemporary reports. Note that using the multiplier of 50, the $5,000 salary corresponds to about $250,000 in present value. This compares favorably to the annual salary of $162,100 received by present congressmen.

[32] Our estimates do not simply capture a more general "early vs. late half of the decade" effect. The point estimate for those who served in the early half of the 1850s is not statistically different from the estimate for those who served in the late half of the decade. Moreover, the estimate in panel C for the 1870-1880 period corresponds precisely to those who served in the early half of the decade (1870-1875), as we have only collected wealth data for these individuals. That estimate is small and statistically insignificant providing additional evidence that our results do not simply capture larger wealth accumulation by those who serve in the early half of a decade. Our "before-and-after" analysis in section 6 will provide further evidence of this.

change in wealth (top row) and change in servants (bottom row). We compute binned averages based on vote share of the first election, and plot these averages against the vote share. We also plot locally-linear regression curves, which are allowed to differ on each side of the 50% threshold, together with 95% confidence intervals.[33] We show plots separately for the Civil War and non-war years. The plots confirm the same patterns of Table 5; for both dependent variables there is a sizable and statistically significant jump at the threshold for Civil War years (first column), but not for the non-war years (second column).

In panel D of Table 5 we report estimates of "placebo regressions" where we look at wealth accumulation between 1850 and 1860 for candidates who ran during the 1860s. If the estimated coefficients in panel B are actually due to service in congress, rather than unobserved characteristics of the winners, then serving in congress during the 1860s should have no effect on wealth accumulation in the previous decade. Reassuringly, "placebo" estimates for Civil War congressmen are very small and never statistically significant at conventional levels. Estimates for non-war congressmen are also small and insignificant with the exception of the estimate in column 3 that is *positive* and statistically significant. These estimates suggest that our estimates in panel B do not simply reflect pre-existing trends in wealth accumulation across close winners and close losers.

Finally, our results are also robust to the choice of bandwidth and control function $f(\cdot)$. In Appendix A, we report robustness checks of our main results. We report our results focusing on a 2% and 3% window around the threshold ("close elections sample") where we drop the control function $f(\cdot)$ altogether (see, e.g., Angrist and Lavy, 1999). Intuitively, for a small enough window h, our variable of interest $Winner_i^t$ is as good as randomly assigned across the individuals, and thus our estimate of β_2 in regression (1) corresponds to the causal effect of holding a seat in congress on wealth accumulation. This approach may be inefficient in small samples but has the advantage of not relying on the correct specification of the functional form of the control function. It also resembles more closely the quasi-experimental assignment provided by close elections. The assumption of random assignment should be even more convincing in this narrower 2% window, even if it may come at the expense of noisier estimates. We also report robustness checks where we focus on a larger sample by using a bandwidth of 20% and include a third order polynomial in the vote share

[33] Plots for change in wealth show the median regression fitted line while those for change in servants show the least squares regression line. To be consistent with our regression analysis, in the median regression fitted lines in the top row we allow a different intercept for Civil War and non-war winners but impose the same slope – i.e. we do not estimate a fully saturated regression where we allow the effect of the forcing variable to vary across Civil War and non-war candidates.

of the first congressional race. This is consistent with the approach proposed in Angrist and Pischke (2009) but has the disadvantage of relying crucially on the correct specification of the polynomial in the forcing variable.

Table A.1 shows the robustness checks for the regressions reported in Table 5. The estimates in columns 1-9 confirm the same overall pattern: no evidence of a statistically significant effect of serving in congress during the 1850s, the 1870s or during the non-war years in the 1860s, and evidence of large rents for those who served during the Civil War.

Finally, although we do not show this in a table, we can compare the winner against the loser in each close race, on a race by race basis. The results are as follows. For the Civil War years, the median difference in *Ending Wealth* between the winner and the loser is $10,300, the average difference in *Log Ending Wealth* between the winner and the loser is 0.32, and the average difference in *Log Servants* between the winner and the loser is 0.34. For the non-war years, the corresponding differences between the winner and the loser are much smaller: $5,525, 0.11, and 0.02, respectively. This confirms the overall pattern illustrated in Table 5.[34]

5.3 Exploring Possible Mechanisms

What can explain our evidence of rent seeking by Civil War congressmen? A first element of central importance in our interpretation is the size of federal government spending. Figure 1A illustrates the evolution of nominal government spending by the federal government between 1850 and 1880. There was a dramatic increase in nominal government spending during the Civil War years from about $60 million just before the outbreak of the war, to almost $1,200 million at the highest point during the war, followed by an equally sharp fall in spending after the war ended. This was driven by the need to mobilize, equip, feed, and move armies on a scale never before seen in U.S. history. The sudden spike in government spending might have made it easier for incumbent congressmen (and other politicians) to collect rents as there was a larger pool of resources on which to prey. Similarly, rent extraction would have been more difficult to detect during the Civil War than during the 1850s or 1870s because the same dollar amount of rents represented a much smaller fraction of total government spending. Using the point estimates from Table 5, the total amount of rents extracted by all incumbent congressmen serving during the Civil War would have represented less than 0.1% of total federal wartime spending. However, this would have represented almost 2% of

[34] For the 1850s, the differences between the winner and the loser are all slightly negative – -$2,000, -0.09, and -0.27, respectively.

the average level of pre-war spending (over four years), and about 1% of post-war spending. Thus, rent extraction comparable in scale to what we estimate for the Civil War years would have been much easier to detect during "normal" times.

A second possibility – beyond the increase in federal spending – is that as a consequence of the war, the effectiveness of the various checks and balances and political institutions set in place to oversee and control the behavior of politicians was undermined. During the Civil War years the federal government, the media, and the electorate were mainly focused on fighting the war, and thus oversight might have been lax relative to "normal" times. This would have allowed incumbent congressmen to channel contracts towards firms in which they had an interest, or collect side-payments or legal fees from contractors in exchange for favorable treatment. Procurement was especially frantic and disorganized during the first half of the war, as an army of almost 700,000 men was built essentially from scratch (see Wilson, 2006a). Under severe pressure, and focused on the gloomy military situation in the east, it is unlikely that the agencies of the federal government were capable of carefully overseeing and auditing much of the contracting. In Figure 1B we show some evidence consistent with this hypothesis. Reporting on corruption by local newspapers fell precisely during the Civil War years and started increasing again right after the end of the war, when most of the political turmoil and battles that had captured the media's attention disappeared.

Next, we address our hypothesis that wartime activity – especially military contracting and membership on key congressional committees – might have been the source of some of the rents extracted by congressmen during the Civil War.

5.3.1 Military Contracting States

First, we consider military contracts. Wilson (2006a) provides a list of the major military contractors in the most important industries during the Civil War (Appendix B of his book). These contractors were all located in Connecticut, Delaware, Massachusetts, Maine, Maryland, Missouri, New Jersey, New York, Ohio, Pennsylvania, Rhode Island and Vermont.[35] Using this list we define a dummy variable that is equal to 1 for the states with large military contracts, and 0 for the remaining states. We then re-estimate the regressions reported in panel B of Table 5, but add terms that interact this dummy variable with the variables indicating which candidates won during the Civil War and non-war years. An important caveat is that large contracting states differ from non-contracting states along other dimensions other than the magnitude of military contracts during the war. Large contracting states

[35] We exclude Delaware, Maryland and Missouri because they were slave states.

are richer and more consolidated states and thus our interaction terms may confound many other characteristics of these states.

Panel A of Table 6 presents the results of this analysis. The estimates provide robust evidence of large returns for congressmen from large contracting states who served during the Civil War. The point estimates for congressmen from these states – reported in the first row – are the largest and are always statistically significant. The difference is particularly striking for the estimates in column 1 – based on ending wealth – that suggest that congressmen representing large contracting states who served during the Civil War accumulated up to $28,000 (approximately $1,400,000 in present values) more wealth than those who ran but never served. Moreover, in this specification we can reject the hypothesis that the coefficient for congressmen from contracting states during the Civil War is equal to that of congressmen from other states during the war. The estimates in columns 2 and 3 using ending log wealth and number of servants as the dependent variable exhibit the same overall pattern. However, the estimates for congressmen who represented other states during the Civil War, though not statistically significant, are rather large in columns 2 and 3. In these specifications we cannot reject the null hypothesis that the effect for congressmen who served during the Civil War in contracting and other states is equal.

On the other hand, the estimates in the third row reveal that congressmen from large contracting states did not become abnormally richer in the non-war years once spending in war supplies in these states declined dramatically. The estimates are relatively small and are never statistically significant in the different specifications. There is also no evidence of abnormal wealth accumulation from congressmen who represented non-contracting states during the non-war years. Estimates in the fourth row of panel A are always small and not significant. Moreover, in columns 1 and 3 we can always reject the hypothesis that the coefficients for congressmen who served during the non-war years are equal to the coefficient of congressmen from contracting states who served during the Civil War. Perhaps more important, we cannot reject the hypothesis that the coefficients of congressmen representing contracting and non-contracting states were equal to each other during the non-war years. This suggests that it was only during high military spending war-times, when congressmen from large contracting states were able to accumulate wealth at an abnormal rate relative to those who ran but never served in congress.

In Panel B of Table 6 we report placebo regressions where we look at wealth accumulation by those who represented Contracting and other states during the 1860s, in the decade *prior* to serving in Congress. A potential concern with the results in panel A is that they may confound the effect of representing a large contracting state during the Civil War years with an overall higher ability of congressmen from these states. The estimates in the first row for those who represented contracting states during the Civil War are small and are not statistically significant. This is comforting and suggests that evidence of large rents for congressmen who represented these states during the Civil War does not reflect pre-existing characteristics or patterns of wealth accumulation of these individuals. Moreover, from the F-tests in panel B we cannot reject the null hypothesis that wealth accumulation between 1850 and 1860 was equal for congressmen who represented large contracting states and other states during the 1860s.

An additional concern has to do with the fact that the results in panel A may reflect the overall advantage that individuals representing large contracting states enjoy in extracting rents in any period other than the 1860s. To address this possibility, in Table 8 we explore whether individuals who represented large contracting states during the 1850s (panel A) or during the 1870s (panel B) accumulated more wealth relative to those who ran and never served or those who represented other states during this period. The results are, once again, reassuring. The estimates in panels A and B are, if anything, negative and are often small and statistically insignificant. Most importantly, there is no evidence of a statistical difference between the estimates for congressmen representing contracting states and other states in any of these decades.

Finally, in appendix table A.2 we report robustness checks for the regressions in panel A of Table 6. The overall patterns are the same when we focus on a 2% or 3% bandwidth and drop the control function altogether (columns 1-6) or when we use a much wider bandwitdh (h=20%) and control for a third order polynomial in the vote share of the first election (columns 7-9). Those who represented contracting states during the Civil War years accumulated the largest amounts of wealth between 1860-1870.

5.3.2 Military Spending Committees

Next, we study the role of committee assignments. We use the *Daily Journal* of the House of Representatives as well as the *Congressional Globe* to identify the committees most often cited as responsible for large bills on military appropriations during the 37th and 38th Congresses during the Civil War.[36] These committees are the Ways and Means (responsible for many appropriations bills including many for army and navy funding), Military Affairs

[36] The *House Journal* reports the proceedings of all legislative activity for the house and provides details on the committees responsible for every bill. The *Congressional Globe* records full debates.

and Militia, Expenditures of the War Department, Naval Affairs, Expenditures of the Navy Department, and Roads and Canals (important for military railroads during the war).

In Table 7 we explore whether congressmen who served on these key committees accumulated more wealth between 1860 and 1870 than congressmen who served on other committees, as well as candidates who ran and never served. The relevant variation of interest in this exercise is committee assignment. Thus, we do not report regression discontinuity estimates since these provide us with random assignment in access to office and not to different specific committees. Instead, we consider the whole sample of candidates and report difference-in-difference estimates for the effect of serving in a military spending committee in Civil War and in non-war years. The estimates in the first four rows of panel A report wealth accumulation between 1860 and 1870 by members of military spending committees and other committees who served during the Civil War and during the non-war years, relative to those who ran during this period but did not serve.

The estimates provide evidence of especially large returns for congressmen who served on the important military spending committees during the Civil War. The estimates in the first row are large and statistically significant across the different dependent variables. The estimate in column 1 implies that congressmen from these committees accumulated and additional \$20,000 (approximately \$1,000,000 in current values) relative to those who ran but did not serve. The estimates for congressmen from other committees who served during the Civil War are also positive and statistically significant in columns 1 and 3 but are smaller than those for congressmen in top military committees. Most importantly, the difference-in-difference estimates reported at the bottom of panel A are positive for all dependent variables though they are only statistically significant in columns 1 and 3 (in the latter case at the .06 level). This provides suggestive evidence that assignment to these key committees was particularly profitable during the Civil War years, when military spending was abnormally high.

In panel B of Table 7 we report placebo regressions that look at wealth accumulation between 1850 and 1860, *prior* to serving in Congress, by those who served in military spending committees and other committees between 1860 and 1870. The estimates in panel B are small and are never statistically significant suggesting that those who served between 1860 and 1870 did not accumulate more wealth in the 1850s relative to those who ran but did not serve. Moreover the "placebo" difference-in-difference estimates reported at the bottom of panel B are, if anything, negative and are never statistically significant. This suggests that the patterns reported in panel A of Table 7 do not simply capture pre-existing differences across individuals serving in these committees.

An additional validity check is reported in Table 8 that looks at wealth accumulation by those who served in top military spending committees and other committees during "normal times" in the 1850s (panel C) and 1870s (panel D) when military spending was not at unusually high levels. The estimates in panel C suggest that those who served in military spending committees during the 1850s may have accumulated more wealth between 1850 and 1860 than those who ran but never served, though this result is not robust across the different dependent variables (estimates in column 3 are small and statistically insignificant). The estimates in panel D are very close to zero and statistically insignificant. Most importantly, we cannot reject the hypothesis that the estimates for those who served in military spending committees and other committees in the 1850s and 1870s are equal to each other. Thus, our estimates in panel A of Table 7 do not simply capture an overall advantage in rent-seeking for those serving in military committees relative to those in other committees. Assignment to military spending committees was only profitable during the war.

Finally, the difference-in-difference estimates in Table 7 are based on the full sample of candidates. This can raise concerns that the patterns in Table 7 partly confound the change in the sample of candidates considered relative to the sample in Table 5 (that focuses on a bandwidth of 3% around the 0.5 threshold). For robustness, in appendix table A.3 we report estimates equivalent to those in Table 7 but focusing on the same 3% sample used in Table 5. The patterns remain essentially unchanged; the difference-in-difference estimates in panel A are positive (and larger than those reported in Table 7) but only statistically significant in columns 1 and 3 (in the latter case at the .09 level). The "placebo" difference in difference estimates in panel B are, if anything, negative and are not statistically significant.

Overall, the evidence in Tables 6-8 is consistent with the hypothesis that congressmen who served during the Civil War got richer than they would have otherwise, and that this was due in part to the unusually high levels of wartime spending. Members of congress from the mainly industrial states that were home to the largest federal contractors, and members who served on the committees that were responsible for most military appropriations, tended to accumulate more wealth between 1860 and 1870 than other members of congress, and noticeably more than the individuals who ran for congress but lost. These results highlight the importance of increased *opportunities* associated to the spike in government spending and contracting during the Civil War. Increased spending and opportunities for self-enrichment may need to interact with lower control and oversight by state institutions, the voters and the media to generate higher levels of rent-seeking. While we currently do not have data that allows us to disentangle the role of increased *opportunities* from the effect of reduced *control* we are currently gathering additional data to address this in future research.

5.3.3 Alternative Mechanisms

Next we explore some additional potential interpretations of the results reported in Tables 5-8. First, we examine whether members of Congress accumulate more wealth than election losers in the decades after serving in Congress. Next, we explore the hypothesis that the results in Table 5 reflect the fact that the Civil War may have attracted more venal candidates. Finally, we discuss whether the lack of evidence on rents for the 1850s and 1870s may be driven by lack of power – in particular, the inability to detect smaller levels of rent-seeking during this period.

Wealth Accumulation *After* Serving in Congress The results in Table 5 provide no systematic evidence of abnormal wealth accumulation *while serving in office*, at least for those who served during the 1850s and 1870s. One possibility is that members of congress are able to benefit from the connections and networks established while in office, *after* they leave congress. This may include both legal activities – e.g., lawyers in congress may meet and attract as clients a larger and richer set of individuals than they knew before serving – as well as shadier dealings such as preferential treatment for contracts and land grants that former congressmen receive from those inside government after leaving congress. Similarly, politicians may prefer to receive side-payments or bribes after they leave office when they are less under the scrutiny of voters and of political institutions. For the British case, Eggers and Hainmueller (2008) find that Conservative MPs profited from office largely through lucrative outside employment they later acquired as a result of their political positions.

To explore this possibility, in Table 9 we look at whether those who served in congress accumulated more wealth than those who ran but lost in the decade *after* they were out of office. In panel A we explore wealth accumulation between 1850 and 1860 for those who served in the 29th and 30th Congresses (between 1845 and 1850) but did not serve during the 1850s. Similarly, in panel B we consider wealth accumulation between 1860 and 1870 for those who served during the 1850s (31st-36th Congresses) but did not serve during the 1860s. Finally, in panel C we consider wealth accumulation between 1870 and 1880 for those who served during the 1860s (36th-41st Congresses) but not during the 1870s.

Overall, the estimates in Table 9 provide no robust evidence of abnormal returns in the decade after serving in congress. Estimates in columns 1 and 2 of panel A are *negative*, small and statistically insignificant. The estimate in column 3 is positive and rather large but it is not statistically significant. The estimates in panel B are positive but they are not statistically significant at conventional levels (even though the estimate in column 3 is rather large). Finally, the estimates in panel C for those who served during the Civil War and non-war years in the 1860s are also statistically insignificant.

Attraction of Venal Candidates Another possibility is that the political environment during this period attracted more venal candidates, who anticipated that federal politicians would have greater opportunities for war profiteering than others. Greater wealth accumulation by congressmen during this period may partly reflect a change in the *type* of individuals who ran for congress. However, using the information recorded in the census schedules, this hypothesis receives only limited support in the data. As the validity checks in Table 4 show, there is no evidence of any systematic difference across winning and losing candidates who ran during this period. There is also little evidence of a change in the occupational background of the overall pool of candidates – winners and losers – who ran for congress during the Civil War years. For the Civil War congresses, 58% of the candidates were lawyers, 19% were farmers and 14% were merchants, manufacturers or bankers. These figures are not very different for candidates who ran during the 1850s or during the non-war years in the 1860s – 53% of candidates who ran during the 1850s were lawyers, 20% were farmers and 14% were merchants, manufacturers or bankers. The corresponding figures for the non-war years during the 1860s were 57%, 16% and 15% respectively. Of course, we cannot rule out that candidates differed in terms of characteristics that we cannot observe, so this hypothesis should be explored in more detail by future research.

Rents during Normal Times A final potential interpretation of the results in Table 5 is that due to the relatively small size of the federal budget during the 1850s, 1870s and second half of the 1860s, we may be unable to detect in the data rates of rent extraction similar to those observed during the Civil War. In other words, political institutions may have been equally effective during the Civil War in monitoring and keeping the levels of rent extraction at the same proportion as during the non-war years, but we can only detect systematic rents during the Civil War years due to the much larger size of federal spending. This interpretation, while plausible, is unlikely to explain our results. Recall from our previous discussion that the total value of rents accumulated by congressmen during the Civil War years accounts to approximately 0.1% of total federal spending over the 4 years in this period. A similar rate or fraction of rent extraction during the 1850s or 1870s would have corresponded to rents of approximately $2,000 or $5,500 respectively per congressman over the 10-year period. Our analysis of the Lacrosse and Milwaukee railroad scandal revealed that we can detect bribes of $5,000 using census wealth data (this was the most common bribe amount in that scandal). Most importantly, there is no special reason why we may care more about rents as a fraction of total federal spending as opposed to their absolute value. Even if the fraction of federal spending appropriated by congressmen remained constant during the Civil War years, the dollar amount of rents was substantially larger during this period.

6 Another Empirical Strategy: Before-and-After Design

A common concern with RDD estimates is that they provide *local* average treatment effects (LATE) for observations or individuals around the given threshold and thus may be uninformative regarding the effect of a given treatment on observations further away from the discontinuity (see Angrist and Pischke, 2009). This is often referred to as the lack of external validity of regression discontinuity estimates.

In the context of our analysis, one could think of several reasons why estimates for the returns to congress based on individuals whose first election was decided by a small margin may lack external validity. One possible argument is that those who win by a narrow margin will tend to be more disciplined and less likely to engage in rent extraction since they are less safe and any minor wrongdoing may lead them to lose their seat in an upcoming election.[37] In this case, our regression discontinuity estimates may *underestimate* the actual rents from office enjoyed by congressmen who are safe in their seats and won their first election by a very large vote margin. However, it is easy to think of reasons why the regression discontinuity estimates *overestimate* the rents from congress enjoyed by those individuals who did not enter congress following a close election. If the electoral outcome of their first race was due to chance – say, an unusual partisan tide – then "close winners" may decide to make the most of their time in congress and engage in rent-seeking, anticipating that they are unlikely to win reelection in any case.

We cannot directly assess how informative are the local treatment RDD estimates for the broader set of candidates. As an alternative, we report evidence based on a different

[37]However, the fact that there were few career congressmen during this period makes this case unlikely.

empirical strategy – a simple "before-and-after" design – first introduced in Querubin and Snyder (2009). This approach does not rely on the comparison of winners and losers in close elections, but relies solely on data for individuals who actually won and served.

Figure 4 below illustrates the approach.[38] Suppose we can observe the wealth of members of congress at two different years $t-10$ and t. In Figure 4 we show this for $t = 1860$ (panel A), $t = 1870$ (panel B) and $t = 1880$ (panel C). We can then create indicator functions to classify all members of congress who served in the years around this period. Let N_{EARLY} be an indicator function that takes a value of 1 for all members of congress that served only during the 5 years preceding $t-10$ and zero otherwise. Similarly, T_{EARLY} takes a value of 1 for members of congress that served only during the 5 years following $t-10$ and zero otherwise. We can also define similar indicator functions for congressmen who served around t. That is, T_{LATE} takes a value of 1 for all those who served only in the 5 years preceding t and zero otherwise while N_{LATE} takes a value of 1 for congressmen who served only during the 5 years after t and zero otherwise. We can use these indicator functions to get a rough estimate of the returns to serving in congress in the early and late part of the decade under consideration. For example, to get an estimate of the returns to congress in the post-war years in the second half of the 1860s we can compare the accumulation of wealth between 1860 and 1870 for representatives that only served during the five years just before 1870 (i.e. all congressmen for which $T_{LATE}=1$) with those that only served during the five years just after 1870 (i.e. all congressmen for which $N_{LATE} = 1$). The first group was "treated" by politics – had access to congressional rents that would appear in their 1870 wealth – while the latter group was not. Similarly, we can get an estimate of the returns from a seat in congress during the Civil War years (early 1860s) by comparing the accumulation of wealth between 1860 and 1870 for those individuals that only served during the five years just after 1860 (i.e. those for which $T_{EARLY}=1$) with those that only served during the 5 years just before 1860 (those for which $N_{EARLY}=1$). In this case, only the latter group was treated by politics between 1860 and 1870. We can compare the different treatment and control groups around the different census years through a simple regression of the form:

$$Wealth_i^t = \beta_0 + \beta_1 Wealth_i^{t-1} + \beta_2 T_i + \beta_3' \mathbf{X}_i + \epsilon_i^t \tag{3}$$

where $Wealth_i^t$ is the wealth of congressman i in year t, $Wealth_i^{t-1}$ is the wealth of congressman i in the previous census year, T_i corresponds to one of the "treatment" dummies defined above, and $\mathbf{X}_i$ corresponds to a set of control variables, including age and age^2, occupation

[38] See Querubin and Snyder (2009) for a more detailed discussion.

dummies, and state fixed-effects.

The specific sample on which the above regression should be estimated depends on whether we are estimating the returns to a seat in congress in the early or late half of the decade under consideration. In order to estimate the returns for the late part of the decade, we should estimate the regression on the sample of individuals that served only in the five years preceding or following year t (i.e. those for which either T_{LATE} or N_{LATE} equals 1). In this case, T_i will just correspond to the indicator function T_{LATE}. If we want to estimate the returns in the early half of the decade, the estimation sample should consist of all those who only served in the 5 years preceding and following year $t-10$ (i.e. all those for which either T_{EARLY} or N_{EARLY} equals 1). Notice that for the 1870-1880 period we can only estimate the effect on the early half of the decade as we have only collected data for those who served between 1870 and 1875.

Table 10 presents the estimates of the main coefficient of interest – i.e., β_2 in equation (3), the coefficient on T_i. Each panel reports the results for the different decades under consideration. The results are straightforward. First, and consistent with the RDD estimates, we find no evidence of a large positive return to serving in congress during the 1850s. Point estimates for this period are small and statistically insignificant with the exception of the estimate in column 6 for the second half of the decade that is in fact negative and statistically significant. Similarly, and consistent with the results of Table 5, we find no evidence of abnormal wealth accumulation for those who served in the first half of the early 1870s (panel C). The point estimate is small and statistically insignificant. Finally, results for the 1860s – reported in panel B – confirm the same patterns of Table 5. There is no evidence of abnormal wealth accumulation for those who served in the post-war years during the second half of the decade; estimates in columns 2, 4 and 6 are small and are not statistically significant. However, we do find evidence of a relatively large return to serving in congress during the Civil War years in the early part of the 1860s. The point estimate for *Ending Log Wealth* reported in column 3 is 0.4, and the coefficient is highly statistically significant. The point estimates for *Ending Wealth* and *Ending Servants* in columns 1 and 5, respectively, are also large and the latter is also statistically significant at the 5% level.[39]

[39]In order to assess the validity of our approach, we also test for pre-existing differences in congressmen who served before and after the different census years.We do not show these results to save space but they are available upon request. Not surprisingly, congressmen who serve prior to a given census year are, on average, older than those who serve after the census year. To control for this difference, in our regressions we always include the age and squared age of the congressman to capture the (possibly non-linear) effect that age may have on wealth accumulation. Most importantly, treated congressmen do not differ by their initial wealth, a variable that plausibly captures other relevant characteristics such as ability, education, or

The point estimates from this analysis reveal a similar pattern as that for the RDD estimates reported in Table 5. This analysis, which relies on a completely different source of variation, confirms our main findings and gives us further confidence on the relevance of our RDD estimates.

7 Conclusions

The results of this paper suggest that the returns to a seat in the U.S. House were low during "normal" times in the mid-19th century. This is a remarkable result that stands in contrast to evidence for other relatively weakly institutionalized countries where political connections are valuable and often an important source of wealth accumulation. A plausible interpretation for this result is that democratic political institutions during this period were effective at controlling politicians and preventing the abuse of power for personal gain.

However, we report evidence of substantial returns to a seat in congress between 1861 and 1866 when federal government spending expanded sharply to unprecedented levels in order to fund the war. Our point estimates suggest that congressmen who served during the Civil War accumulated about 40% more wealth between 1860 and 1870 (nearly $1,000,000 in present values) than those who ran but never served.

We hypothesize that such dramatic increase in government spending may have made it easier for incumbent congressmen to accumulate rents due to a surge in *opportunities* associated, amongst others, to contracts for war supplies. In addition, the focus and attention of government institutions during this period were probably centered on the affairs of the war, diminishing the auditing and oversight capacity that may have been exercised by government agencies during normal times. This might have been particularly important in a context in which significant rent extraction would have represented a much small fraction of government spending than at other times. Similarly, politicians during this period may have been less accountable to the voters and the media who were also focused on the political and military events of the war, rather than on the dealings of politicians and the allocation of federal expenditure. We also show that wealth accumulation was particularly significant by representatives who represented states that played an important role providing supplies during the war and by congressmen who served in top military committees.

Conceptually, rent-seeking during the Civil War may have been triggered by increased

occupation. In addition – just as one example – we find that treated congressmen are no more or less likely to be lawyers. These similarities give us some confidence that the main difference between politicians at either side of the census year is their exposure to politics.

opportunities, reduced control and/or the interaction of these two forces. The results reported in the paper emphasize the important role of increased spending and opportunities but we do not report direct evidence on the role of decreased control and oversight by the media and the voters. In this sense, we cannot rule out that it was only the increase in spending (and not its interaction with reduced control) that lead to higher levels of rent seeking. We are currently gathering additional data to disentangle the relative importance of these two forces. An alternative is to gather data on media coverage in different states during this period and establish whether rent-seeking was more systematic by representatives from states where coverage of corruption decreased more during the Civil War. In addition, we can identify episodes of spending increases in particular states that were not associated to any particular event that may have reduced the accountability and control of politicians. Evidence of rent-seeking during these episodes will allow us to assess the relative importance of decreased control relative to increased opportunities for enrichment. We have found the census records of many state and local politicians such as governors, state senators, state legislators and city mayors. We are also gathering data on spending by state governments during this period. This will allow us to analyze wealth accumulation by state politicians during other episodes of increased spending.

Our results point to a broader interpretation which suggests that corruption and rent extraction may be more likely to occur in episodes of crisis such as natural disasters, wars or other types of political and economic turmoil. During these periods government expenditure often increases substantially, increasing the amount of resources on which politicians might prey, and at the same time the effectiveness of political institutions, the voters and the media in controlling politicians can be heavily undermined. Future research should explore this hypothesis more systematically in other contexts. This might be particularly important in situations where politicians themselves are responsible for declaring states of emergency, or engaging in war, justifying increases in government expenditure and distracting the attention of the constituency, the media, and other public agencies responsible for supervising politician's behavior.

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Table 1: LaCrosse & Milwaukee Railroad Scandal			
	(1)	(2)	(3)
	Wealth 1860	Wealth 1860	Log Wealth 1860
Bribe Amount	1.000	1.923	
	(0.158)	(0.713)	
Log Bribe Amount			0.853
			(0.249)
Observations	66	66	66
R-square	0.199	0.487	0.215

Median regression estimates in column 1 and OLS estimates in columns 2 and 3.

Table 2a: Summary Statistics on Initial Wealth

	Real Wealth		Total Wealth				Servants		
	1850 Mean	1850 Median	1860 Mean	1860 Median	1870 Mean	1870 Median	1850 Mean	1860 Mean	1870 Mean
All candidates	16140	5500	41403	16500	85817	27450	0.64	1.12	1.19
Winners	17335	5508	43191	17025	99754	28485	0.64	1.13	1.26
Losers	15053	5250	39934	15000	71405	26000	0.65	1.11	1.12
Democrats	13458	4500	43731	15500	69668	25600	0.62	1.11	1.23
Republicans	7307	3000	36269	15500	107403	30000	0.37	1.08	1.18
Whigs	19559	7250	.	.	.	.	0.67	.	.
Lawyers	13969	4000	28342	13415	56611	21000	0.59	1.04	1.11
IPUMS All	927	0	1823	200	2716	200	.	0.20	0.18
IPUMS Law	6722	0	8357	1500	10532	2000	.	0.53	0.51

Table 2b: Summary Statistics on Changes in Wealth

	Δ Log Real 1850-1860	Δ Servants 1850-1860	Δ Total 1860-1870	Δ Log Total 1860-1870	Δ Servants 1860-1870	Δ Servants 1870-1880
All candidates	0.93	0.53	14925	0.84	0.24	-0.03
Winners	0.96	0.52	17550	0.90	0.37	-0.02
Losers	0.90	0.54	12790	0.79	0.14	-0.03
Winners w/Margin < .03	0.82	0.36	21750	0.98	0.38	0.03
Losers w/Margin < .03	0.95	0.57	11000	0.61	0.13	-0.00
Democrats	1.04	0.54	11000	0.77	0.19	-0.08
Republicans	1.04	0.34	19000	0.96	0.32	0.03
Whigs	0.68	0.56	.	.	.	.
Lawyers	1.06	0.51	11450	0.79	0.21	0.02
IPUMS All	1.00			0.97	-0.00	0.01
IPUMS Law						

In Table 2b all figures are means except those in the Δ *Total 1860-1870* column, which are medians.

Table 3: OLS Estimates of Wealth vs. Serving in Congress			
	(1)	(2)	(3)
	Ending Wealth	Ending Log Wealth	Ending Servants
Panel A: 1850-1860			
Winner 1850s	2775	0.191	-0.017
	(1061)	(0.075)	(0.079)
Obs.	690	690	802
Panel B: 1860-1870, Civil War vs. Non-War			
Winner Civil War	8574	0.203	0.441
	(2218)	(0.080)	(0.082)
Winner Non-War	1038	0.104	-0.070
	(2203)	(0.080)	(0.079)
p-value of F-test	0.03	0.44	0.00
Obs.	757	757	917
Panel C: 1870-1880			
Winner 1870s	–	–	0.044
			(0.081)
Obs.			688

Median regression estimates for *Ending Wealth* dependent variable (column 1). OLS estimates for *Ending Log Wealth* and *Ending Servants* dependent variables (columns 2-3). The p-values are for F-tests of H0: β for Winner Civil War = β for Winner Non-War.

Table 4: Balance on Covariates in RDD Samples (3% margin)

1850-1860	Winner Mean	Loser Mean	Difference	p-Value
Log Initial Real Wealth	8.61	8.61	0.00	0.99
Log Initial Servants	0.72	0.65	0.07	0.65
Age	51.01	50.52	0.49	0.65
Lawyer Dummy	0.71	0.64	0.06	0.25
Manuf/Merch/Banker	0.16	0.22	-0.05	0.24
Farmer Dummy	0.21	0.20	0.02	0.72
1860-1870, Civil War Years				
Log Initial Total Wealth	9.98	9.76	0.22	0.32
Log Initial Servants	1.21	1.44	-0.23	0.25
Age	42.71	42.51	0.19	0.89
Lawyer Dummy	0.65	0.69	-0.04	0.57
Manuf/Merch/Banker	0.21	0.19	0.02	0.80
Farmer Dummy	0.23	0.12	0.11	0.08
1860-1870, Non-War Years				
Log Initial Total Wealth	9.57	9.57	0.00	1.00
Log Initial Servants	1.09	1.02	0.07	0.64
Age	40.22	40.53	-0.30	0.81
Lawyer Dummy	0.74	0.67	0.07	0.25
Manuf/Merch/Banker	0.15	0.24	-0.09	0.13
Farmer Dummy	0.14	0.09	0.05	0.25
1870-1880				
Log Initial Servants	1.17	1.09	0.08	0.60
Age	35.07	36.32	-1.25	0.26
Lawyer Dummy	0.64	0.52	0.12	0.05
Manuf/Merch/Banker	0.22	0.28	-0.06	0.25
Farmer Dummy	0.13	0.26	-0.13	0.01

Samples restricted to candidates who won between 47% and 53% of the vote in their first race.

Table 5: RDD Estimates of Effect of Winning First Race for Congress on Wealth			
	(1)	(2)	(3)
	Ending Wealth	Ending Log Wealth	Ending Servants
Panel A: 1850-1860			
Winner 1850s	-6472	-0.339	-0.462
	(5580)	(0.244)	(0.261)
Obs.	230	230	252
Panel B: 1860-1870, Civil War vs. Non-War			
Winner Civil War	17731	0.382	0.568
	(4260)	(0.172)	(0.170)
Winner Non-War	-2148	0.034	-0.272
	(3987)	(0.161)	(0.168)
p-value of F-test	0.00	0.10	0.00
Obs.	235	235	283
Panel C: 1870-1880			
Winner 1870s			0.145
	–	–	(0.250)
Obs.			252
Panel D: 1850-1860, Civil War vs. Non-War, Placebo Regressions			
Winner Civil War	153	-0.028	-0.015
	(5512)	(0.293)	(0.253)
Winner Non-War	-58	0.312	0.600
	(5353)	(0.274)	(0.249)
p-value of F-test	0.97	0.28	0.04
Obs.	141	141	186

RDD Estimates – Local Linear Regressions with 3% Bandwidth. Median regression estimates for *Ending Wealth* dependent variable (column 1). OLS estimates for *Ending Log Wealth* and *Ending Servants* dependent variables (columns 2-3). The p-values are for F-tests of H0: β for Winner Civil War = β for Winner Non-War.

Table 6: Contracting vs. Other States			
	(1)	(2)	(3)
	Ending Wealth	Ending Log Wealth	Ending Servants
Panel A: RDD Estimates, 1860-1870			
Civil War, Contract State	28028	0.413	0.637
	(5567)	(0.194)	(0.191)
Civil War, Other State	7034	0.310	0.386
	(7298)	(0.269)	(0.281)
Non-War, Contract State	569	0.029	-0.287
	(5014)	(0.179)	(0.183)
Non-War, Other State	322	0.051	-0.226
	(7767)	(0.289)	(0.293)
p-value of F-test 1	0.01	0.73	0.42
p-value of F-test 2	0.00	0.12	0.00
p-value of F-test 3	0.00	0.26	0.01
Obs.	235	235	283
Panel B: Placebo Regressions, 1850-1860			
Civil War, Contract State	552	-0.195	-0.076
	(6742)	(0.311)	(0.277)
Civil War, Other State	-309	0.493	0.095
	(9135)	(0.439)	(0.391)
Non-War, Contract State	109	0.357	0.716
	(6481)	(0.291)	(0.273)
Non-War, Other State	-675	0.306	0.241
	(10037)	(0.492)	(0.427)
p-value of F-test 1	0.92	0.11	0.68
p-value of F-test 2	0.96	0.13	0.02
p-value of F-test 3	0.90	0.32	0.48
Obs.	141	141	186

RDD Estimates – Local Linear Regressions with 3% Bandwidth. Median regression estimates for *Ending Wealth* dependent variable (column 1). OLS estimates for *Ending Log Wealth* and *Ending Servants* dependent variables (columns 2-3). The p-values are for F-tests of the hypothesis that the β for Civil War, Contract State Winners is equal to: (1) β for Civil War, Other State Winners, (2) β for Non-War, Contract State Winners, and (3) β for Non-War, Other State Winners.

Table 7: Military Committees vs. Other Committees			
	(1)	(2)	(3)
	Ending Wealth	Ending Log Wealth	Ending Servants
Panel A: Difference-in-Difference Estimates, 1860-1870			
Civil War, Military Comm	19756	0.394	0.598
	(4429)	(0.153)	(0.151)
Civil War, Other Comm	6187	0.147	0.440
	(2564)	(0.089)	(0.090)
Non-War, Military Comm	-314	0.125	-0.268
	(3400)	(0.118)	(0.120)
Non-War, Other Comm	479	0.034	-0.022
	(2647)	(0.092)	(0.090)
Diff-in-Diff Estimate	14362	0.156	0.403
p-value	0.02	0.47	0.06
Obs.	757	757	917
Panel B: Placebo Difference-in-Differences, 1850-1860			
Civil War, Military Comm	-5462	0.143	0.159
	(4797)	(0.212)	(0.169)
Civil War, Other Comm	-1280	0.071	0.060
	(2791)	(0.124)	(0.102)
Non-War, Military Comm	4362	0.121	0.146
	(3968)	(0.174)	(0.146)
Non-War, Other Comm	-2827	-0.102	0.046
	(3047)	(0.134)	(0.114)
Diff-in-Diff Estimate	-11371	-0.151	-0.001
p-value	0.12	0.63	1.00
Obs.	511	511	664

Difference-in-Difference = (Civil War, Military Comm − Civil War, Other Comm) - (Non-War, Military Comm − Non-War, Other Comm). Median regression estimates for *Ending Wealth* dependent variable (column 1). OLS estimates for *Ending Log Wealth* and *Ending Servants* dependent variables (columns 2-3). The p-value is for an F-test of the hypothesis that the difference-in-difference estimate is equal to zero.

Table 8: Contracting States and Military Committees in Other Decades			
	(1)	(2)	(3)
	Ending Wealth	Ending Log Wealth	Ending Servants
Panel A: 1850-1860, Contracting States vs. Other States			
Contract State	-7027	-0.340	-0.442
	(5317)	(0.251)	(0.268)
Other State	520	-0.333	-0.553
	(7343)	(0.348)	(0.373)
Obs.	230	230	252
Panel B: 1870-1880, Contracting States vs. Other States			
Contract State	–	–	0.118
			(0.271)
Other State	–	–	0.189
			(0.301)
Obs.			252
Panel C: 1850-1860, Military Committees vs. Other Committees			
Military Comm	4308	0.202	-0.046
	(1717)	(0.117)	(0.119)
Other Comm	2567	0.187	-0.005
	(1200)	(0.081)	(0.086)
Obs.	690	690	802
Panel D: 1870-1880, Military Committees vs. Other Committees			
Military Comm	–	–	0.005
			(0.116)
Other Comm	–	–	0.065
			(0.089)
Obs.			688

RDD Estimates in Panels A and B – Local Linear Regressions with 3% Bandwidth. Median regression estimates for *Ending Wealth* dependent variable (columns 1). OLS estimates for *Ending Log Wealth* and *Ending Servants* dependent variables (columns 2-3). In all cases we do not reject the hypothesis that the coefficient for Contract State (Military Comm) is equal to the coefficient for Other State (Other Comm) even at the .10 level.

Table 9: RDD Estimates of Effect of Serving in Congress on Wealth After Leaving Congress

	(1)	(2)	(3)
	Ending Wealth	Ending Log Wealth	Ending Servants
Panel A: 1850-1860			
Winner 1840s	-5167	-0.058	0.587
	(19296)	(0.339)	(0.466)
Obs.	121	121	123
Panel B: 1860-1870			
Winner 1850s	-122	0.156	0.480
	(14452)	(0.318)	(0.306)
Obs.	157	157	168
Panel C: 1870-1880			
Winner Civwar			0.352
	–	–	(0.277)
Winner Non-War			-0.306
	–	–	(0.253)
p-value of F-test	–	–	0.08
Obs.			

Median regression estimates for *Ending Wealth* dependent variable (column 1). OLS estimates for *Ending Log Wealth* and *Ending Servants* dependent variables (columns 2-3).

Table 10: Effect of Serving in Congress on Wealth Before and After Analysis						
	Ending Wealth		Ending Log Wealth		Ending Servants	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 1850-1860	Served Early	Served Late	Served Early	Served Late	Served Early	Served Late
Served in Period	3565	607	0.098	0.063	-0.062	-0.365
	(2383)	(2941)	(0.127)	(0.151)	(0.185)	(0.117)
Obs.	194	243	194	243	221	296
Panel B: 1860-1870	Served Early	Served Late	Served Early	Served Late	Served Early	Served Late
Served in Period	8024	1670	0.382	-0.026	0.354	0.213
	(6955)	(3143)	(0.145)	(0.122)	(0.157)	(0.135)
Obs.	251	283	251	283	293	319
Panel C: 1870-1880	Served Early	Served Late	Served Early	Served Late	Served Early	Served Late
Served in Period					-0.042	
	–	–	–	–	(0.182)	()
Obs.					274	

Median regression estimates for *Ending Wealth* dependent variable (columns 1-2). OLS estimates for *Ending Log Wealth* and *Ending Servants* dependent variables (columns 3-6).

Figure 1A

Federal Government Spending Before, During and After the Civil War

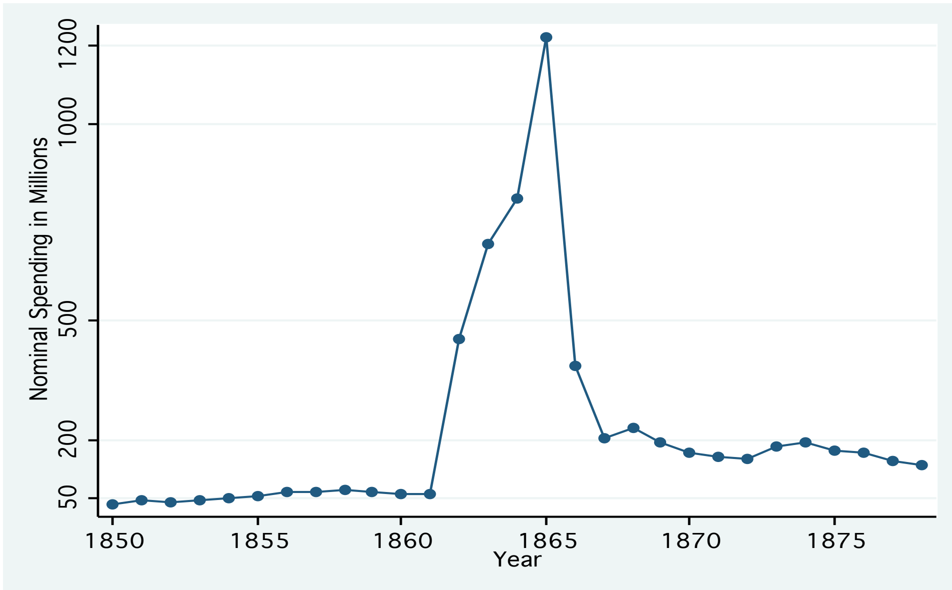


Figure 1B

Reporting on Corruption, During and After the Civil War

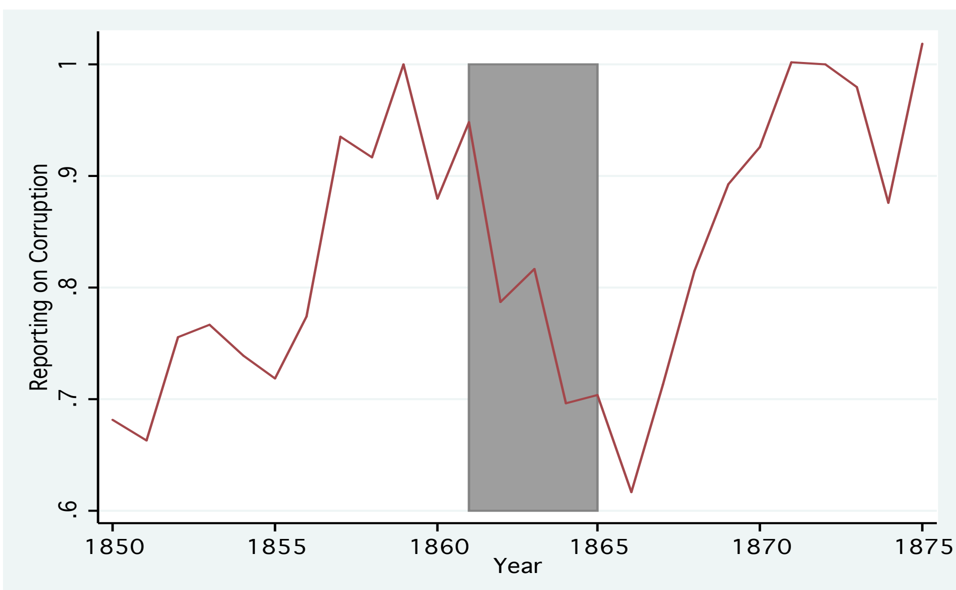
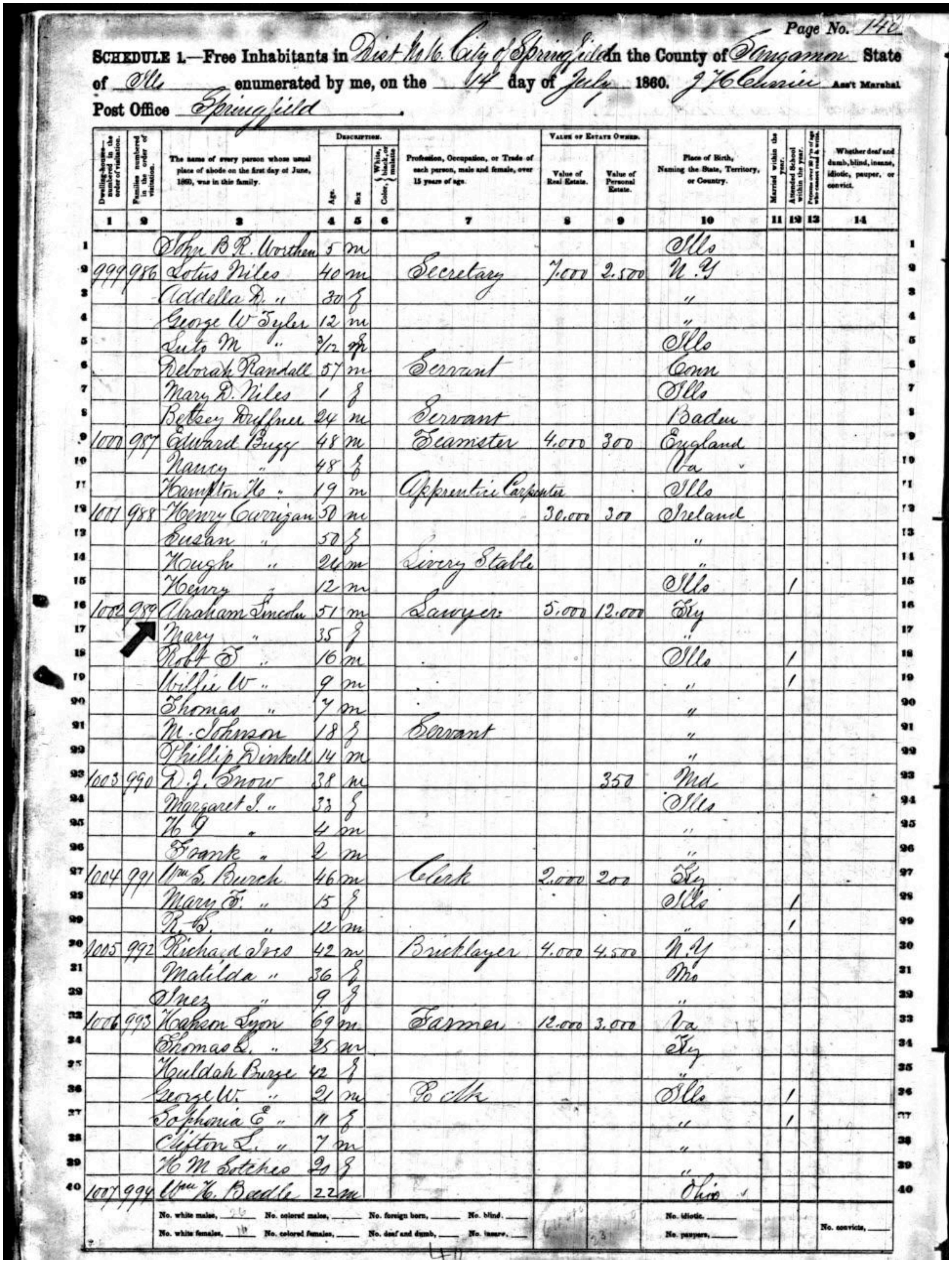


Figure 1B shows number of newspaper pages containing the words "corrupt*" or "fraud*" divided by the number of pages containing the word January (a neutral word), for a sample of local newspapers available in Ancestry.com. Source: Glaeser and Goldin (2004)

Figure 2: Sample Census Page (with Abraham Lincoln)



Page No. 140

SCHEDULE 1.—Free Inhabitants in Dist No 6 City of Springfield in the County of Sangamon State of Ills enumerated by me, on the 14 day of July 1860. J H Curran Ass't Marshal.

Post Office Springfield

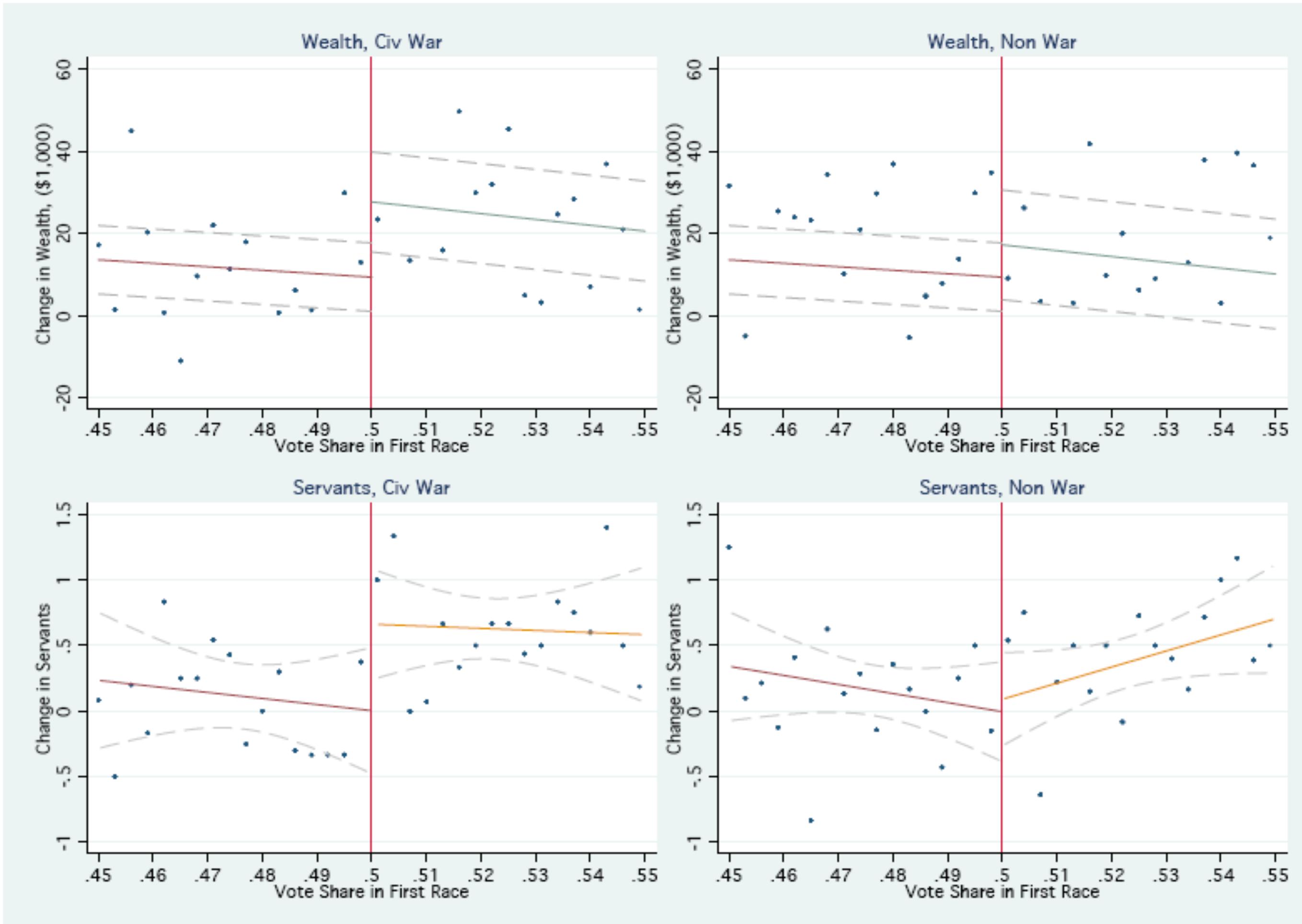
Dwelling-houses numbered in the order of visitation (1)	Families numbered in the order of visitation (2)	The name of every person whose usual place of abode on the first day of June, 1860, was in this family (3)	Age (4)	Sex (5)	Color (6)	Profession, Occupation, or Trade of each person, male and female, over 15 years of age (7)	Value of Real Estate (8)	Value of Personal Estate (9)	Place of Birth, Naming the State, Territory, or Country (10)	Married within the year (11)	Attended School within the year (12)	Persons over 20 y'rs of age who cannot read & write (13)	Whether deaf and dumb, blind, insane, idiotic, pauper, or convict (14)
		John B R Worthen	5	m					Ills				
999	986	Lotus Niles	40	m		Secretary	7,000	2,500	N.Y				
		Addella D. "	30	f					"				
		George W Tyler	12	m					"				
		Suty M "	1/12	f					Ills				
		Deborah Randall	57	m		Servant			Conn				
		Mary D. Niles	1	f					Ills				
		Betsey Duffner	24	m		Servant			Baden				
1000	987	Edward Bugg	48	m		Teamster	4,000	300	England				
		Nancy "	48	f					Va				
		Hampton H. "	19	m		Apprentice Carpenter			Ills				
1001	988	Henry Carrigan	50	m			30,000	300	Ireland				
		Susan "	50	f					"				
		Hugh "	26	m		Livery Stable			"				
		Henry "	12	m					Ills		1		
1002	989	Abraham Lincoln	51	m		Lawyer	5,000	12,000	Ky				
		Mary "	35	f					"				
		Robt T. "	16	m					Ills		1		
		Willie W. "	9	m					"		1		
		Thomas "	7	m					"				
		M. Johnson	18	f		Servant			"				
		Phillip Dinkell	14	m					"				
1003	990	R. J. Snow	38	m				350	Md				
		Margaret J. "	33	f					Ills				
		H. J. "	4	m					"				
		Frank "	2	m					"				
1004	991	Wm S. Burch	46	m		Clerk	2,000	200	Ky				
		Mary F. "	15	f					Ills		1		
		R. B. "	12	m					"		1		
1005	992	Richard Ives	42	m		Bricklayer	4,000	4,500	N.Y				
		Matilda "	36	f					Mo				
		Inez "	9	f					"				
1006	993	Hapen Lyon	69	m		Farmer	12,000	3,000	Va				
		Thomas L. "	35	m					Ky				
		Huldah Burge	42	f					"				
		George W. "	21	m		P. Clk			Ills		1		
		Sophonia E. "	11	f					"		1		
		Clifton L. "	7	m					"				
		H M Sotches	20	f					"				
1007	994	Wm H. Beadle	22	m					Ohio				

No. white males, 26 No. colored males, No. foreign born, No. blind, No. idiotic, No. convicts,
No. white females, 14 No. colored females, No. deaf and dumb, No. insane, No. paupers,

This corresponds to the page where Abraham Lincoln was listed in the 1860 Census. He reports real estate wealth of $5,000, personal wealth of $12,000, and one servant living in his dwelling. His occupation is Lawyer.

Figure 3

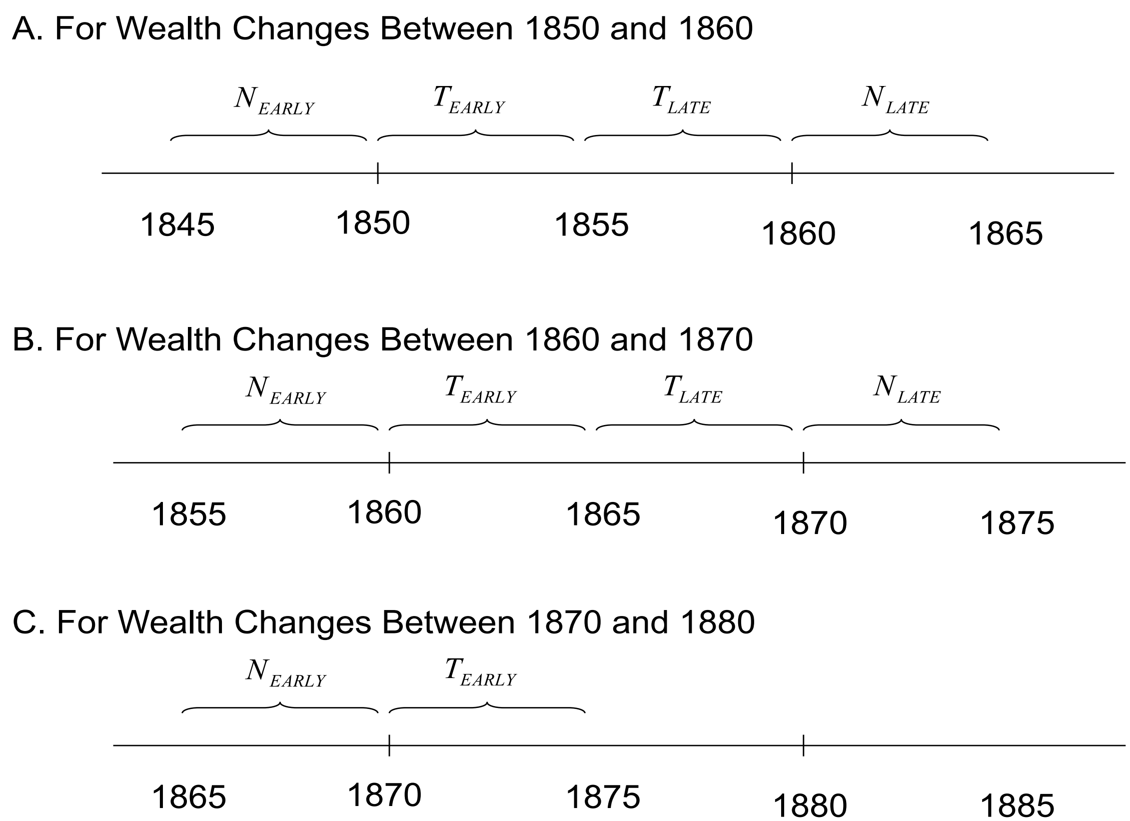
RDD Plots for Change in Wealth and Change in Servants



Figures in top row show fitted median regression line together with bootstrapped confidence intervals at the threshold. Figures in bottom row show OLS fitted line together with 95% confidence intervals.

Figure 4

Before and After Design



Appendix A: Robustness Checks

Table A.1: Robustness Checks on RDD Estimates in Table 5

	2% Bandwidth			3% Bandwidth			Polynomial		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Ending Wealth	Ending Log Wealth	Ending Servants	Ending Wealth	Ending Log Wealth	Ending Servants	Ending Wealth	Ending Log Wealth	Ending Servants
Panel A: 1850-1860									
Winner 1850s	-1702	-0.084	-0.292	-1513	-0.042	-0.243	298	-0.333	-0.433
	(3629)	(0.156)	(0.181)	(2679)	(0.131)	(0.144)	(1983)	(0.200)	(0.211)
Obs.	163	163	183	230	230	252	635	635	731
Panel B: 1860-1870, Civil War vs. Non-War									
Winner Civil War	16440	0.390	0.687	19586	0.397	0.634	13984	0.265	0.398
	(3680)	(0.198)	(0.185)	(3586)	(0.140)	(0.141)	(3198)	(0.118)	(0.116)
Winner Non-War	-3323	0.010	-0.277	152	0.050	-0.205	2922	0.191	-0.122
	(3386)	(0.190)	(0.183)	(3380)	(0.134)	(0.138)	(3258)	(0.119)	(0.122)
p-value of F-test	0.00	0.21	0.00	0.00	0.10	0.00	0.01	0.60	0.00
Obs.	162	162	183	235	235	283	714	714	854
Panel C: 1870-1880									
Winner 1870s			0.062			0.009			-0.120
	–	–	(0.172)			(0.131)			(0.224)
Obs.			155			252			644

Median regression estimates for *Ending Wealth* dependent variable (columns 1,4,7). OLS estimates for *Ending Log Wealth* and *Ending Servants* dependent variables (columns 2,3,5,6,8,9).

Table A.2: Robustness Checks on RDD Estimates in Table 6

Contracting State vs. Other States

	2% Bandwidth			3% Bandwidth			Polynomial		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Ending Wealth	Ending Log Wealth	Ending Servants	Ending Wealth	Ending Log Wealth	Ending Servants	Ending Wealth	Ending Log Wealth	Ending Servants
Civil War, Contract State	37866	0.460	0.868	27771	0.426	0.703	15703	0.282	0.454
	(7450)	(0.233)	(0.208)	(3132)	(0.167)	(0.166)	(3675)	(0.132)	(0.130)
Civil War, Other State	-76	0.249	0.137	6487	0.331	0.455	5591	0.210	0.270
	(10551)	(0.361)	(0.367)	(4471)	(0.248)	(0.262)	(4780)	(0.176)	(0.177)
Non-War, Contract State	-348	0.044	-0.236	203	0.049	-0.229	1283	0.114	-0.127
	(6604)	(0.212)	(0.199)	(2922)	(0.157)	(0.160)	(3627)	(0.130)	(0.133)
Non-War, Other State	-2425	-0.113	-0.438	-1844	0.057	-0.130	3064	0.381	-0.093
	(11535)	(0.453)	(0.438)	(4678)	(0.270)	(0.267)	(4919)	(0.179)	(0.180)
p-value of F-test 1	0.00	0.62	0.08	0.00	0.74	0.42	0.05	0.71	0.34
p-value of F-test 2	0.00	0.23	0.00	0.00	0.13	0.00	0.00	0.31	0.00
p-value of F-test 3	0.00	0.26	0.01	0.00	0.25	0.01	0.02	0.60	0.00
Obs.	162	162	183	235	235	283	714	714	854

Median regression estimates for *Ending Wealth* dependent variable (columns 1,4,7). OLS estimates for *Ending Log Wealth* and *Ending Servants* dependent variables (columns 2,3,5,6,8,9). The p-values are for F-tests of the hypothesis that the β for Civil War, Contract State Winners is equal to: (1) β for Civil War, Other State Winners, (2) β for Non-War, Contract State Winners, and (3) β for Non-War, Other State Winners.

Table A.3: Robustness Checks on Diff-in-Diff Estimates in Table 7
Military Committees vs. Other Committees, Restricted to 3% Sample

	(1)	(2)	(3)
	Ending Wealth	Ending Log Wealth	Ending Servants
Panel A: Difference-in-Difference Estimates, 1860-1870			
Civil War, Military Comm	45182	0.716	0.806
	(7244)	(0.248)	(0.249)
Civil War, Other Comm	9606	0.364	0.533
	(4399)	(0.148)	(0.149)
Non-War, Military Comm	8486	0.270	-0.419
	(5677)	(0.188)	(0.205)
Non-War, Other Comm	-1159	0.064	-0.050
	(4520)	(0.154)	(0.155)
Diff-in-Diff Estimate	25931	0.146	0.643
p-value	0.02	0.68	0.09
Obs.	235	235	283
Panel B: Placebo Difference-in-Differences, 1850-1860			
Civil War, Military Comm	1019	-0.049	0.021
	(6634)	(0.320)	(0.321)
Civil War, Other Comm	1256	0.038	-0.188
	(4557)	(0.216)	(0.202)
Non-War, Military Comm	80	0.233	0.389
	(6168)	(0.307)	(0.304)
Non-War, Other Comm	-2349	-0.010	0.112
	(5321)	(0.249)	(0.241)
Diff-in-Diff Estimate	-2666	-0.329	-0.067
p-value	0.81	0.53	0.90
Obs.	141	141	186

Difference-in-Difference = (Civil War, Military Comm − Civil War, Other Comm) - (Non-War, Military Comm − Non-War, Other Comm). Median regression estimates for *Ending Wealth* dependent variable (column 1). OLS estimates for *Ending Log Wealth* and *Ending Servants* dependent variables (columns 2-3). The p-values are for F-tests of the hypothesis that the difference-in-difference estimates are equal to zero.

Appendix B: Data Appendix

In this section we discuss various issues associated with our data sources and data collection process. First, we describe in more detail some characteristics of the political environment during this period. Then, we provide an in-depth discussion of the reliability of the census wealth, as well as a detailed description of the process for matching politicians to their census records, and for the coding of servants.

Electoral Data

The electoral data consists of election results for each election to the U.S. House of Representatives between 1845 and 1875. These data were collected by the Inter-University Consortium for Political and Social Research (ICPSR), and we revised and updated the ICPSR dataset using Dubin (1998).[40]

Additional information on the winners of each election is available from a biographical dataset compiled by the ICPSR, as well as the *Biographical Directory of the U.S. Congress*.[41] These provide information on the year and place of birth, profession and career, and the county of residence at different points in time. We use Martis (1982) to match counties and cities to congressional districts. This biographical and geographical information was useful for cleaning the electoral database (e.g., finding cases in which the election winner did not serve in congress), and also for matching candidates to census records.

It is important to describe several features of the electoral and political environment of the mid-19th century, because they are relevant for our analysis and quite different from the environment today. As shown in Table B.1, there were about 2,400 races to the House of Representatives between 1845 and 1875, involving about 3,000 distinct candidates. Congressional elections were quite competitive compared to today. Approximately 50% of all winners received less than 55% of the vote (as a percentage of the top two candidates' votes), and about 33% received less than 53% of the vote.

Also, there were very few "career congressmen" compared to today.[42] Fewer than 24% of those who won their first race ran for congress in more than two elections, and only 16%

[40] ICPSR Number 1. Dubin (1998) is essential not only for providing more complete and accurate election returns, but also for providing the first and middle names or initials of many candidates for which the ICPSR data provides only the last name, or the last name plus the first initial. Dubin (1998) also provides information on many special elections that are not included in the ICPSR data set.

[41] ICPSR Number 7428, and http://bioguide.congress.gov.

[42] However, many of those who served in congress served in other offices, both before and after their congressional service, so a larger number of men were "career politicians."

served three or more terms. The numbers are similar for those who won their first race by a narrow margin – only 21% ran more than twice for congress and only about 12% served three or more terms. In sum, few congressmen seem to have been interested in long careers in congress, irrespective of whether they first entered this office by a small or large margin.

Those who lost their first race almost never served in congress – 80% of those who lost their first race never even ran again, and fewer than 9% served in congress during our period. As one would expect, those who lost their first race by a small margin were more likely to try again and succeed in the future: about 28% ran again and about 16% ended up serving in congress.[43] Thus, as mentioned earlier, we focus on what happens in a candidate's first race, since this is the election that determines whether or not the "typical" candidate serves in congress.[44] Moreover, this suggests that "non-compliers" are not a major concern, and therefore our "intention to treat" estimates based on (1) provide a reasonable estimate of the causal effect of holding a congressional seat on wealth accumulation.

Close Elections and Sorting at the Threshold

Recent papers by Snyder (2005), Caughey and Sekhon (2010) and Carpenter et al. (2011) criticize RDD studies that rely on close elections, arguing that there are anomalies even very near the 50% threshold. They show that in U.S. House elections, incumbents win noticeably more than 50% of the very close races – especially those where the winning margin was less than 1% – and that candidates from the party in control of state offices, such as the governorship, secretary of state and state house and senate, hold a systematic advantage in extremely close elections.

These papers do not analyze the time period we study, so we provide some evidence here. In races where the winner's margin was 1% or less and an incumbent was running, exactly 50% of the incumbents won and 50% lost. In the 2% window, 56% of the incumbents won, but this figure is not statistically different from 50% at the .05 level. In the wider 3% window 60% of incumbents won, and the percentage is statistically significant. We check the hypothesis explored in Carpenter et al. (2011), by studying outcomes from the point of view

[43] As noted above, there were also a few cases of candidates who did not serve in congress despite winning the election, due to reasons such as being disqualified, death, and election contests. However, this only happened in 37 cases.

[44] Some candidates – about 5% of our sample – ran more than once, for non-consecutive congresses. In these cases we define a "spell" as a set of consecutive election attempts separated by at least one congress in which they did not run. We treat the spells as separate "quasi-experiments" and consider the vote share in the first election of each spell. In other words, we count as "close winners" or "close losers" those who won or lost the first race of any of their election spells by a small margin.

of the party controlling the governor's office at the time of the election. In races where the winner's margin was 1% or less, the candidate whose party controlled the governor's office won 53% of the time, but this figure is not statistically different from 50% at the .05 level. In the 2% window, the candidate from the governor's party won 54%, and again the percentage is not statistically different from 50% at the .05 level. In the wider 3% window, 56% of the candidates from the governor's party won, and the percentage is statistically different from 50%.[45]

Interestingly, the "sorting" at the threshold is concentrated in the post-Civil War congresses. In the congresses of the 1850s (31st-36th Congresses) there is no significant evidence of sorting. The figures for the percentage of close races won by the incumbent for the various windows are as follows: 46% for the 1% window, 57% for the 2% window, and 57% for the 3% window. Similarly, the figures for the percentage of close races won by the governor's party are as follows: 47% for the 1% window, 52% for the 2% window, and 53% for the 3% window. None of these are statistically different from 50% at the .05 level. We find the same patterns – no significant evidence of sorting – for our main sub-sample of interest, the Civil War years (37th-39th Congresses). The figures for the percentage of close races won by the incumbent for the various windows are as follows: 50% for the 1% window, 51% for the 2% window, and 55% for the 3% window. Similarly, the figures for the percentage of close races won by the governor's party are as follows: 51% for the 1% window, 53% for the 2% window, and 56% for the 3% window. Again, none of these are statistically different from 50% at the .05 level.

Census Wealth Data

In this section we provide evidence by previous authors and new evidence compiled by ourselves that suggest that census wealth data can be trusted and does not introduce any major biases for the purposes of our analysis. Next we discuss our census records matching procedure and address any potential selection issues that may arise from our inability to find the census record of every single candidate in every census year.

[45]In fact, these outcomes are not too surprising, since as Folke et al. (2011) show, in districts with a "normal vote" different from .5 we actually *expect* candidates from the favored party to win more than 50% of the time except in extremely small windows around the threshold. And incumbents, as well as candidates whose party won the governorship, tend to be from the favored party rather than the disadvantaged party in a district.

Reliability of the Census Data

The wealth data provided in census records was self-reported by the respondents, and was not checked for accuracy in other ways by government officials. Given this, it is important to discuss the possible problems associated with these data, as well as work done by previous authors and ourselves to establish its reliability.

All censuses were administered in person, by U.S. assistant marshals. The exact instructions given to the enumerators for the 1860 census were as follows. For the value of real estate:

> "Under heading 8, insert the value of the real estate owned by each individual enumerated. You are to obtain this information by personal inquiry of each head of a family, and are to insert the amount in dollars, be the estate located where it may. You are not to consider any question of lien or encumbrance; it is simply your duty to enter the value as given by the respondent."

Similarly, for the value of personal estate:

> "Under heading 9, insert (in dollars) the value of personal property or estate. Here you are to include the value of all the property, possessions, or wealth of each individual which is not embraced in the column previous consist of what it may; the value of bonds, mortgages, notes, slaves, live stock, plate, jewels or furniture; in fine, the value of whatever constitutes the personal wealth of individuals. Exact accuracy may not be arrived at, but all persons should be encouraged to give a near and prompt estimate for your information. Should any respondent manifest hesitation or unwillingness to make a free reply on this or any other subject, you will direct attention to Nos. 6 and 13 of your general instructions and the 15th section of the law."

According to Wright (1970b), there was some concern at the time regarding the "suspicions of the interviewees about the intentions of the enumerators and about the uses to which the information divulged would be put" (p. 38). Also, some observers expressed fears that the information might be used for tax purposes, although much of this was probably political posturing. For instance, a southern journal inquired whether "this Federal prying into the domestic economy of the people was not a precursor to direct taxes" (p. 38). Williamson and Lindert (1991) discuss the reliability of the 1860 census returns, in particular the possibility that respondents gave casual, and therefore inaccurate, answers to the census takers. They note that "a large number of [households] may have reported zero wealth in order to avoid the bother of estimating asset value (in fact 38% of free adult males reported property less than $100 in the 1860 census sample) but it is hard to tell what share of these actually reported zero wealth" (p. 41). They add that "at the other end of the wealth spectrum, one might speculate that the very rich overstated their wealth in the 1860 and 1870 censuses, but this is a hard conjecture to sustain [and] we know of no clear bias in the estimates, either for the 1774 or for 1860 [censuses]" (p. 41).

There are, however, several reasons to believe that these issues are not a major concern for our purposes. First, the information collected by census officials was, as a matter of policy, strictly confidential. The U.S. Secretary of the Interior stated the policy as follows:

> "... all marshals and assistants are expected to consider the facts intrusted to them as if obtained exclusively for the use of the Government, and not to be used in any way to the gratification of curiosity, the exposure of any man's business or pursuits, or for the private emolument of the marshals or assistants, who, while employed in this service, act as agents of the Government in the most confidential capacity."

This policy was reinforced for the 1870 census, with the following:

> "No graver offense can be committed by assistant marshals than to divulge information acquired in the discharge of their duty. All disclosures should be treated as strictly confidential, with the exception hereafter to be noted in the case of the mortality schedule [where professional review by a local physician was authorized]. Information will be solicited of any breach of confidence on the part of assistant marshals. The [Department of Interior] is determined to protect the citizen in all his rights in the present census."

Moreover, Wright (1970b) adds that "enumerators were instructed to approach every family 'with civil and conciliatory manners' and to 'secure confidence and good will."' They were to warn recalcitrant respondents of the penalties for refusal to answer or for giving false information. Above all, the information received was to be kept strictly confidential, and interviewees were to be assured that this was true. By 1860 one would suppose that much of the apprehension of the earlier years would have been dissipated by virtue of experience" (p. 38). Steckel (1990) notes that although the census did not verify self-reported wealth figures, it would have been difficult to conceal real estate holdings and thus these figures were probably reported reliably.

Second, even if some respondents were worried that the information provided would not in fact be kept confidential, there was no clear incentive for under-reporting or over-reporting wealth. There was no federal tax on wealth at the time, and no estate tax. Personal vanity, however, might have lead to some over-reporting.

Several previous studies have assessed the reliability of the census data in different ways. Soltow (1975) used random samples from the 1850-1870 censuses to analyze the evolution of wealth inequality in the U.S. He found that "wealth averages for the samples in the years 1850-1870 are generally in line with estimates made by various authorities on wealth distribution. Growth rates are similar to those found for GNP per worker by Kusnetz and commodity output per worker by Gallman" (p. 6). He also found evidence that the census wealth figures were consistent with aggregates obtained by county assessors. The fact that patterns of wealth and wealth inequality suggested by the census data were in line with those described by other sources provides evidence in favor of the reliability of census wealth.

Another group of studies compared wealth reported in the census sheets with taxable wealth. In an early paper, Gallman (1969) used a random sample from a 1 percent sample of census sheets from Baltimore, New Orleans, St. Louis, Maryland (excluding Baltimore) and Louisiana (excluding New Orleans), and found that "the aggregate value of property reported on the population schedules exceeded the value of property assessed for tax purposes by more than 50% and the estimated true value of taxable property by almost 20%" (p. 17). The discrepancies could be explained by the fact that not all property was subject to tax, and also that some of the property listed in the tax records belonged to corporations and other institutions (not enumerated in the population census). Moreover, individuals owning personal property worth less than \$100 were apparently not obliged to list their property in the census but presumably were obligated to list for tax purposes. Gallman concludes that "the large value of property reported on the population schedule, relative to the estimated true value of taxed property, is good evidence that the enumerators and respondents met their obligations" (p. 17).

One of the most relevant studies for our purpose is Steckel (1994), who matched 20,000 households from the federal census of Massachusetts and Ohio with real and personal property tax records from 1820 to 1910. Simple scatter plots of taxable wealth against census wealth reveal that for Massachusetts most observations line up around the 45 degree line, indicating an average coincidence of census and taxable wealth. The data from Ohio shows that census wealth tends to exceed taxable wealth, in line with the findings of Gallman (1969). Steckel explains the discrepancies by pointing to the "old-fashioned practice of valuing property at about one-half or two-thirds of what it was worth" (p. 79), differences in the dates of the wealth valuations relative to the census enumerations, and the fact that some property exempt from taxation, particularly personal property, may have been included in wealth totals reported by the census. In addition, some individuals may have owned wealth in taxing jurisdictions outside their place of residence. Finally, one cannot ignore the fact that some individuals evaded taxes. In addition, the census may have reported family or household property, including that owned by children or by a spouse, with the head, whereas taxable property included only that owned personally by the head. In order to establish any systematic discrepancies between census and taxable wealth, Steckel (1994) ran regressions of taxable wealth on census wealth and characteristics of the household head, for every census year. The results suggest no systematic associations between the discrepancies and any of the variables with the exception of gender status (taxable wealth is well below census wealth for women). This, however, is easily explained by the fact that widows received favorable tax treatment. Moreover, and despite the discrepancies between the sources of data pointed out above, inequality measures calculated with both census and taxable wealth are remarkably similar. Steckel concludes by stating that "these data [wealth from census schedules] are particularly valuable for analyzing patterns of wealth holding." (p. 84).

Even more important for our purposes, however, is whether politicians are more likely to misreport the true value of their wealth. In order to explore this issue, we found the 1850 and 1860 census records for all of the individuals in *The Rich Men of Massachusetts*, a book that purports to give the wealth of (most of) the richest 1,500 men in Massachusetts as of about 1851 as reported by independent parties.[46] We matched the individuals in this book to lists of mayors, state legislators and congressmen who served during the period in order to explore any systematic discrepancies between both sources by politicians, relative to non-politicians. As can be seen in Appendix Table B.2, the correlation between wealth reported in this book and the wealth recorded in the censuses of 1850 and 1860 is relatively high. More importantly, there is no evidence of significant under-reporting or over-reporting of politicians compared to non-politicians. This provides further confidence in the reliability of the census data.

Another measurement issue concerns the fact that it is sometimes difficult to distinguish between respondents with zero wealth and respondents who refused to provide any information to the census marshall, or instances where the marshal did not request the information.[47]

[46]The book provides information on total wealth while the 1850 census, as note above, reported only real estate wealth. Thus we matched individuals in the book with the 1860 census as well as the 1850 census, in order to have a measure of total wealth despite the fact that the 1860 census measure is 9 years later.

[47]Steckel (1994) notes that the incidence of "zero" wealth responses suggests that "some census enumerators failed to acquire accurate information on the value of wealth holdings through lack of diligence,

In both situations census marshals left the census record fields blank, which makes it hard to distinguish "zero" wealth from "wealth figure not available." It is clear that in most cases an empty wealth field corresponds to zero or very low wealth, since they are in the census records of very young individuals, and individuals with low-paying occupations such as laborers and domestic servants. However, one also finds census records of individuals known to be wealthy at the time, such as Fernando Wood, who despite reporting being rich in 1850 and 1860, did not report any wealth figure in 1870. For these individuals it is clear that the missing wealth figure did not correspond to zero wealth as can be inferred from the fact that there were various servants working for them.

The potential measurement error introduced by this issue should only be a concern for our purposes if there is a differential likelihood of not reporting any wealth by close winners and close losers. To explore this, in Appendix Table B.3 we focus on the close election sample – i.e. candidates who won or lost their first election by a margin smaller than 3% – and report in the first two columns, linear probability estimates for a dummy variable that indicates whether the candidate failed to report any wealth in 1860 and 1870 as a function of whether the individual served in congress in the decade prior to that census year, reported wealth in the previous census and the interaction of these two terms. All regressions include state fixed effects, age and age^2 as well as the same occupational dummies included in our main analysis. The results show that election winners, or those originally richer, are not more likely to fail to report their wealth. This gives us further confidence that this phenomenon does not introduce any systematic bias in our results.

Coding of Domestic Servants

As an alternative measure of wealth, we also collected information on the number of servants living with each individual in every census year. Servants living in every dwelling had to be reported to the enumerator and were, naturally, harder to hide and misreport than real or personal wealth figures. Servants were typically reported at the bottom of each household's record, following the enumeration of the relatives of the household head. We classified as servants all individuals who had a variation of one of the following occupations: servant, domestic servant, cook, coachman, nurse, gardener, laundress, seamstress, washwoman, waiter, hostler, or butler. In 1850 most servants' occupations were not listed. Inspection of the 1860 and 1870 records revealed that the majority of servants were young, foreign-born women, or were non-white. Therefore, for 1850 we classified as servants all individuals that did not

non-compliance of the household, or ignorance of the respondent" (p. 80).

share the same family name of the household head and had no occupation listed, but were Irish, German, Scandinavian, or non-white women aged 30 or younger. Moreover, the number of servants is strongly correlated with reported wealth in the census: a regression of total wealth against the number of servants reveals that an additional servant was associated with approximately \$20,000 of additional wealth in 1860 and with \$40,000 of additional wealth in 1870. The correlation is highly statistically significant, with a t-statistic over 10. We also used information on servants to detect cases in which reported wealth figures appear to be unreliable. Consider all candidates with 1 servant. We compute the 10th percentile of the distribution of wealth for these individuals, and recode the wealth as missing for candidates whose reported wealth is below this threshold. We repeat this for all other values of the number of servants. We also assign a value of total wealth of \$100 to all individuals who do not report any wealth and do not have any servants living with them. In 1870, census enumerators were instructed not to record personal wealth values below \$100. None of our results change substantially as a result of these transformations.

Matching of Candidates to their Census Records

We attempted to find the census record in each census year of every candidate for the House of Representatives during our period that obtained at least 25% of the vote. To do so we initially used PERL scripts to automatically match candidates to census records using the first and last name, as well as geographic information based on the county or counties located in the congressional seat sought. In the case of winners we used information provided in the biographical databases on the year of birth, county, and town of residence to further narrow the search. In the case of losers, for which more precise biographical information was not always available, we matched candidates by first and last name and verified that they were living in a county contained in the congressional district in which they were running. Despite the automated matching done by the scripts, the data collection process was still very labor intensive since we had to manually enter wealth figures and occupations. Also, the scripts only found 47% of the cases, due to typographical errors in the information provided by ancestry.com or to candidates who moved. We had to locate other cases by searching manually, checking alternative spellings and miss-spellings of names, checking miss-coded birth years, and searching in other counties and states for candidates who moved.

We successfully located and entered data on about 10,000 census records, out of a universe of about 12,000 cases.[48] This corresponds to an overall success rate of about 80%. We

[48]The biographical information allowed us to know the year of death of those who served. Naturally, we

matched approximately 98% of the winners to at least one census year, and nearly 80% to all three census years. We matched nearly 90% of the losers to at least one census year, and about 60% to all three census years. The lower success rate for losers is not surprising, since we did not always have detailed biographical information that allowed us to perform a more detailed search. Our success rate was relatively uniform across the three census years. Overall, our success rate is very satisfactory. This matching success rate compares with a 59% success rate reported by Steckel (1988) when trying to match over 1,800 household heads from 300 different counties in the 1850 and 1860 censuses and with a success rate of only 19% reported by Ferrie (1996) who tried to match a sample of over 25,000 males included in the IPUMS sample for 1850 to the 1860 census.

We were not able to find the census record of every single candidate in every single year. This could lead to concerns of selection bias in our sample. One encouraging fact is that our overall success rates were similar across the different census years – we found 75% of the census records in 1850, 78% of the records in 1860 and 75% of the records in 1870. Failure to match a congressional candidate to its census record in a given census year could happen for a variety of reasons. First, there is the possibility of underenumeration. Evidence reported by Steckel (1988) suggests enumeration rates were around 85%.[49] In addition, there were frequent typos in the transcription of the original census records which made it harder to find some of the candidates. Steckel (1988) examined a sample from the 1860 census, and found that 8.8% of the transcriptions were searchable errors (minor mistakes or typos), while 15.8% constituted non-searchable errors (that is, errors that would have made it impossible to find an individual). Migration and death were additional factors which complicated the matching of individuals, though this was less of a problem for winners for which we had not only their exact year of death but also some information on migration reported in the congressional biographies. For the case of the losers however, it is likely that some of the candidates we failed to match in the later census years had already passed away.

A large fraction of the candidates we failed to match were those individuals with too common names and for which we could find two or more matches in the census records with the exact same first, middle and last name in the same congressional district and of the

did not attempt to find the census record of those who were dead in a given census year.

[49]However, as mentioned by Wright (1970b), there were large efforts toward avoiding underenumeration and the instructions on coverage were explicit and italicized in the government circulars: "The assistant marshall shall make the enumeration by actual inquiry at every dwelling house, or by personal inquiry of the head of every family, and not otherwise" (p. 149). Also, the assistant marshals were sworn to carry out their instructions and violations were subject to penalties. Another incentive for avoiding the undercounting of individuals was provided by the fact that compensation for assistant marshals was on a per-entry basis.

relevant age. In order to minimize our type I error, we decided to exclude these candidates with very common names that we could not match. However, we are confident that this should not introduce any systematic bias in our sample. In fact, for their 1850 and 1860 samples Steckel (1988) and Ferrie (1996) ran logit regressions of a "common name" dummy against characteristics such as location of residence (region and city size) and other personal characteristics such as real and personal wealth, ethnicity, illiteracy and occupation. Their results show that while common names occur less often in southern states and in cities with less than 75,000 inhabitants, having a common name is not correlated with real or personal wealth. In order to explore in greater detail all these matching issues Steckel (1988) ran a logit regression of a "failure to match" dummy against different personal and geographic characteristics and found that people in the North Central and Mountain and Pacific regions, those in cities larger than 75,000 inhabitants, those foreign born and those illiterate were less likely to be matched while those with large real estates and living in smaller cities were easier to match (though the coefficient on wealth is very small for practical purposes). Something very similar was done by Ferrie (1996) who found that the probability of a successful match was higher for households in the northeastern states, for married individuals, for household heads involved in farming activities and it was lower for foreign-born and older individuals.

For our purposes, the only concern would be if we were differentially likely to find the census records of close winners or of individuals with different wealth levels. Thus, in columns 3 and 4 of Appendix Table B.3 we report linear probability models on the close election sample where we regress a "failure to find" dummy in 1860 and 1870 as a function of whether the individual served in congress in the prior decade, log of wealth reported in the previous census year and the interaction of these two. The point estimates reveal that we were not more or less likely to find the census records of those who won these close races or those who were originally richer. This suggests that failure to find some census records should not have introduced any systematic bias in our analysis.

Table B.1: Summary Statistics on Congressional Races

# of Races	= 2355			
# of Races w/Margin < 55%	= 1117			
# of Races w/Margin < 53%	= 752			
# of Candidates	= 2946			
# of Democrats	= 1539			
# of Whigs	= 490			
# of Republicans	= 734			
All Candidates				
	Won 1st Race		Lost 1st Race	
# who run 1 time	609	38.0%	1939	79.6%
# who run 2 times	618	38.6%	291	12.0%
# who run 3+ times	376	23.5%	205	8.4%
# who win 0 times	0	0.0%	2230	91.6%
# who win 1 time	862	53.8%	103	4.2%
# who win 2 times	482	30.1%	62	2.5%
# who win 3+ times	259	16.2%	40	1.6%
Candidates with Close First Race				
	Won 1st Race		Lost 1st Race	
# who run 1 time	237	38.0%	481	71.7%
# who run 2 times	255	40.9%	91	13.6%
# who run 3+ times	131	21.0%	99	14.8%
# who win 0 times	0	0.0%	560	83.5%
# who win 1 time	374	60.0%	55	8.2%
# who win 2 times	177	28.4%	33	4.9%
# who win 3+ times	72	11.6%	23	3.4%

Appendix Table B.2: Census Wealth vs. Wealth in ***Rich Men of Massachusetts***

	(1)	(2)	(3)
	Log Real 1850	Log Real 1860	Log Total 1860
RMM Wealth	0.79	0.81	1.01
	(.05)	(.07)	(.06)
Politician	0.08	0.13	0.12
	(.12)	(.14)	(.12)
Constant	0.71	0.81	-0.64
	(.62)	(.81)	(.68)
R-square	.30	.27	.46
N	505	368	356
Correlation with RMM Wealth	.54	.52	.68

Appendix Table B.3:
Assessing the Reliability of the Census Data

	(1)	(2)	(3)	(4)
	No Report 1860	No Report 1870	Not Found 1860	Not Found 1870
Winner	-0.007	0.025	-0.005	0.025
	(0.019)	(0.034)	(0.034)	(0.034)
Log(Wealth^{t-10})	0.016	-0.005	0.001	-0.005
	(0.015)	(0.017)	(0.018)	(0.017)
Winner $\times$ Log(Wealth^{t-10})	0.006	0.008	-0.021	0.008
	(0.019)	(0.026)	(0.022)	(0.026)
Observations	290	309	290	309
R-square	0.077	0.069	0.071	0.069