Wealth Accumulation by U.S. Congressmen, 1845-1875: Were the Civil War Years Exceptional(ly Good)?*

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Abstract

In this paper we use historical census data from the U.S. to estimate the pecuniary returns to holding a seat in the U.S. House of Representatives during the 1850s and 1860s. We employ a regression discontinuity design (RDD) based on close elections and compare wealth accumulation in the decades between 1850 and 1870 among those who won or lost their first congressional race by a small margin. We find no evidence of large returns to congressional seats for the 1850s or the second half of the 1860s. However, we do find evidence of significant returns for the first half of the 1860s, during the Civil War. Those who won their first election by a narrow margin and served during the period 1861-1866 (37th-39th Congresses) accumulated, on average, 33-55% more wealth between 1860 and 1870 than candidates who lost the election and did not serve – for the median congressman this corresponds to an additional $700,000-$1,200,000 in present values. We hypothesize that the sudden spike in government spending during the war and the decrease in oversight from government agencies might have made it easier for incumbent congressmen – and probably other politicians – to collect rents. We find evidence that wealth accumulation was particularly large for congressmen who represented states that were home to the major military contractors during the war, and for congressmen who served during the Civil War in committees that were responsible for most military appropriations – the latter accumulated up to 70% more wealth relative to those who never served. These results are robust to the inclusion of state fixed effects, and to the inclusion of a broad set of controls including age, initial wealth and occupation dummies. Placebo regressions reveal that these results are not driven by pre-existing differences in wealth accumulation or other covariates prior to serving in congress. We also show that all of the main results hold when we use the number of domestic servants – a good proxy for wealth – as the dependent variable. Finally, we show that a simple "before-and-after" design using only winning candidates yields surprisingly similar estimates to the RDD.

1 Introduction

An extensive literature in political economy stresses the importance of 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 redistribute resources to themselves or to favor certain interest groups in return for bribes or campaign contributions. The models in this literature generally predict inefficient and/or distorted policies.[1] Such rents may also be inconsistent with the protection of property rights and a level playing field that provide correct incentives for innovation and investment –features at the heart of institutional theories of comparative development. Several papers, including Mauro (1995), Knack and Keefer (1995), Olken (2007) and Reinikka and Svensson (2004), document the detrimental effects of corruption on development, and recently the World Bank identified corruption as one of the greatest obstacles to economic development. Even if the rents accruing to politicians do not imply any specific inefficiency or distortion, estimates of these rents may help assess arguments about the quality of politicians and the effects of quality on policy, as in Caselli and Morelli (2004), Messner and Polborn (2004), and Mattozzi and Merlo (2006, 2007, 2008).

A major empirical question in this context is understanding the environments under which rent extraction by politicians is more likely to occur. The magnitude of such rents may crucially depend on the nature of the political environment and institutions. Rent extraction may be limited if the political environment is highly competitive and the existing institutions provide an appropriate level of checks and oversight on politicians behavior. Similarly, a free and independent media may allow voters to monitor their representatives. On the other hand, in weakly institutionalized environments, politicians may be able to capture the political system and extract rents without punishment from oversight institutions

[1]The literature includes Barro (1973), Ferejohn (1986), Banks and Sundaram (1993, 1998), 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), Grossman and Helpman (1994, 1996, 2001), and Persson and Tabellini (2000).

or their constituencies.

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 estimate the pecuniary returns to holding a seat in the U.S. House of Representatives during the 1850s and 1860s. We focus on representatives who served during the period 1845 to 1875, as well as individuals who ran for a seat in the U.S. House but lost their election. 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 well as the number of slaves from the census slave schedules, 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 below.) It was difficult to misreport the number of servants, since census enumerators visited each home in person. When studying the 1860 to 1870 period we focus on "free" states where slavery was prohibited, because prior to emancipation slaves were counted as part of personal wealth. Thus, comparing wealth figures before and after the abolition of slavery may be misleading for former slave owners. We analyze wealth and slave holding in slave states during the 1850 to 1860 period.

We employ a regression discontinuity design (RDD) based on close elections to estimate the causal effect of serving in Congress on wealth accumulation during this period.[2] We compare wealth accumulation in the decades between 1850 and 1870 among those who won or lost their first congressional race by a small margin. The outcome of close elections provides us with quasi-random assignment of political power. It therefore allows us to isolate

[2]See Hahn, Todd and Van der Klaauw (2001) for a general discussion of regression discontinuity designs and Lee (2008) for a concrete application to close elections.

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.

We find no evidence of large returns to congressional seats for the 1850s or the second half of the 1860s. However, we do find evidence of significant returns for the first half of the 1860s, during the Civil War. Those who won their first election by a narrow margin and served during the 37th and 38th Congresses (1861-1865) accumulated, on average, about 30% more wealth between 1860 and 1870 than candidates who lost the election and did not serve. For the median congressman this corresponds to about $15,000 in additional wealth – roughly $750,000 in present values. Thus, our results indicate that the returns to a seat in the House were low during "normal" times in the mid-19th century, suggesting that U.S. institutions appear to have been effective at controlling politicians' behavior. However, the returns to office increased between 1861 and 1865, when federal government spending expanded sharply to unprecedented levels in order to fund the war.

Figure 1 illustrates the evolution of nominal spending by the Federal government between 1845 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 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 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, 2006). 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 addition, rent extraction would have been more difficult to detect during the Civil War than during the 1850s because the same dollar amount of rents represented a much smaller fraction of total government spending. Using the $15,000 figure from above, 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 the average level of pre-war spending (over four years), and about 0.5% 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.

We also find evidence that wealth accumulation was particularly significant by representatives who represented states that played an important role providing supplies during the war. Congressmen from these states accumulated 40-50% more wealth than similar individuals who never served. Moreover, 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 that candidates who ran but never served. These individuals accumulated almost 70% more wealth relative to those who never served. This, together with additional anecdotal evidence give us further confidence in our interpretation.

Our main results are robust to the inclusion of state fixed effects, and to the inclusion of a broad set of controls including age, initial wealth and occupation dummies. Placebo regressions reveal that these results are not driven by pre-existing differences in wealth accumulation or other covariates prior to serving in congress. We also test whether politicians appear to accumulate additional rents after leaving congress – which would be consistent with the idea that politicians benefit in the long run from the connections and networks established while in office – but do not find significant evidence of such returns. We also show that all of the main results hold when we use the number of domestic servants as the dependent variable. In fact, the results for servants are even more robust than those using wealth.

As always with an RDD approach, external validity is a concern. Do the estimates that rely on comparing individuals who won or lost their first race by a small margin apply to other politicians? We address this by employing an alternative estimation approach that relies solely on winners, not on the comparison of winners and losers. This is based on a simple "before-and-after" design. 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 (i.e., they were "treated"), while the second group did not. The estimates are surprisingly similar to using the RDD, despite the different samples and methodologies. Finally, we provide additional evidence that the wealth data reported in the census is reliable for the purposes of our study.

Our paper contributes to a small but recently growing literature on estimating the value of public office. In another historical paper, Acemoglu, Bautista, Querubin and Robinson (2008) find that in the Colombian state of Cundinamarca, between 1879 and 1890 an additional year in power was associated with an additional 50 percent increase in the value of land owned by incumbents. Eggers and Hainmueller (2009) collect probate records of candidates to the British parliament, 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. However, their data sources do not allow them to control for initial wealth, an important determinant of wealth accumulation. Three papers study congress in the current era. 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. Using different methodologies, Groseclose and Milyo (1999) and Diermeier, Keane and Merlo (2005) 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 lose. Finally, in a study of the Ukraine, Gorodnichenko and Peter (2007) examine the difference between consumption expenditures and income for public sector employees relative to the difference for similar private sector employees, and estimate that public officials receive bribes of at least 1 percent of GDP. Our paper improves on these in some respects, but, of course, it has other limitations.

Our paper also contributes to a growing empirical literature that studies corruption under different electoral and political institutions such as Ferraz and Finan (2010) and Olken (2007) among others. While we focus on one country during two decades, the evidence we present may apply more broadly to other situations. It 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, especially when government expenditure increases substantially and when oversight by the media and other institutions may be less effective than in normal times.

The rest of the paper is organized as follows. Section 2 provides some brief background 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. Section 6 provides evidence consistent with our interpretation of rent extraction during the Civil War. In Section 7 we perform various robustness checks and address concerns regarding the external validity of our RDD estimates. In Section 8 we provide substantial evidence for the reliability of the wealth data reported in the census. Section 9 concludes.

2 Historical and Political Background

In the second half of the 19th century, the United States was a "developing" nation, or at least an industrializing one. And by most accounts, U.S. politics at the time was highly corrupt. Railroads paid bribes for massive land grants and loans, steamship companies paid for lucrative mail routes, construction companies paid for canal contracts, and manufacturers and public utilities of all sorts paid for high tariffs and monopoly privileges. Politicians helped war profiteers sell shoddy goods to the government at inflated prices during the Civil War. Gross conflicts of interest involving officeholders were common and unpunished. Public officials sold a wide variety of services, including aid in obtaining appointments to military academies, assistance in lobbying for war claims and Indian claims, and tips about when the government was planning to sell gold. The spoils system dictated the distribution of government jobs. Electoral fraud was widespread. The press was partisan or bought off or both. Bosses increasingly dominated politics in major cities and some states. Simon Cameron summed up the political ethics of the era nicely with his famous line: "An honest politician is one who, when he is bought, will stay bought."

Reformers at the time identified two key problems: (1) politicians were no longer drawn from the pool of "the best men," and (2) as a result they treated politics simply as a way to make money for themselves and their friends. For example, Harper's Weekly lamented that "men of property and intelligence" had surrendered power "to men inferior in every proper recommendation... who follow politics just as any other money-making business." The magazine went on to criticize "the pecuniary corruption omnipresent in our Legislative Halls, which controls land grants and steamer contracts, and is incarnated in that gigantic corruption-fund, the public printing." The Cincinnati Enquirer described politicians as a "class of inferior men who have come out of public stations far richer than they went into them." Even Ralph Waldo Emerson railed against the "class of privileged thieves who infest our politics... those well dressed well-bred fellows... who get into government and rob without stint and without disgrace."[3]

Many later scholars agree with these claims. Summers (1987) 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" (page 14).[4] Writing about the events of 1857, Stampp (1990) notes, "Corruption was not a new

[3]James Bryce's description in The American Commonwealth is even more colorful: "A statesman of this type [ward politician] usually begins as a saloon or barkeeper, an occupation which enables him to form a large circle of acquaintances, especially among the 'loafer' class who have votes but no reason for using them one way more than another... But he may have started as a lawyer of the lowest kind, or lodging-house keeper, or have taken to politics after failure at store-keeping... They are usually vulgar, sometimes brutal, not so often criminal... Above them stand... the party managers, including the members of Congress and chief men in the state legislatures, and the editors of influential newspapers... What characterizes them as compared with the corresponding class in Europe is that their whole time is more frequently given to political work, that most of them draw an income from politics and the rest hope to do so, and that they come more largely from the poorer and less cultivated than from the higher ranks of society" (page 64-66).

[4]Summers goes on to argue that corruption was a factor leading to secession. In particular, it helped

phenomenon in American politics... but corruption had become distressingly common in this period of accelerating commercialization and industrial growth" (page 30). He explains the growth as follows: "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" (page 28). The administration of Ulysses S. Grant is considered by many historians to be the most corrupt in U.S. history, and the post-Civil War period has been dubbed "the era of good stealings." In his discussion of the scandals of the Grant administration, Josephson (1938) 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" (page 127).[5] Sproat (1968) argues that most liberal reformers in the late 1860s longed for a bygone era when politicians were statesman and gentlemen – "men of unbending integrity, 'sturdy independence,' and unimpeachable honesty" (page 50). They viewed the typical politician of the post-civil war era as "a slave to organizational tyranny and a pawn of special interest" (page 51). Less has been written about corruption during the Civil War. Several scholars discuss the case of Simon Cameron, who was Lincoln's first Secretary of War but was dismissed in part for showing too much favoritism in awarding military contracts.

Interestingly, much 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,

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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" (page 290). Greenberg (1985) makes similar arguments.

[5]For a revisionist view, see Summers (1993).

strategies and tactics, actions by military leaders, and so on – corruption by politicians was a relatively minor affair given that it did not noticeably impact the war effort of either side. Contemporary observers and historians have written extensively about war profiteering and the problems of military contracting.[6] For example, Keeney (2007, page 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, page 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, page 17) notes, "most of these regulation were only loosely enforced and soon of necessity went by the wayside." Some historical works, especially 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 focuses on the activities of private businessmen. This probably makes sense, since this is probably where the bulk of the wartime profits were.

In any case, despite the many claims about political corruption during the 19th Century, there is no systematic evidence regarding how widespread and pervasive the corruption actually was. We do not know whether large numbers of politicians during this period routinely abused political power for their own economic benefit, or whether the anecdotes cited by contemporary observers and historians actually constitute most of the actual cases

[6]See, e.g., Nagle 1999), LeDuc (2004), Wilson (2006), and Keeney (2007).

of corruption.

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 theoretical 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 RDD based on close elections in order to estimate the *causal* effect of serving in congress on wealth accumulation.

3.1 Theoretical Issues

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 create already 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 like talent or ability that are plausibly correlated with other personal outcomes such as economic success. On the one hand, more talented individuals 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. 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. In other words, there is an issue of selection into politics and this makes it very hard to disentangle the effect of politics and of other personal characteristics on the pattern of wealth accumulation of a politician. The direction and magnitude of the bias from a simple naive comparison of politicians and non-politicians is not trivial to measure.

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 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).

Even more relevant for our paper, Casselli and Morelli (2001) develop a model in which "low quality" – i.e., less competent and more dishonest – citizens have a "comparative advantage" in pursuing politics. This is because their market wages are lower than those of high quality individuals, and thus the opportunity cost of holding office is lower; in addition, their dishonesty allows them to reap greater benefits from the rents available in office. Equilibria exist in which only "bad" types run for office, and this situation reproduces itself. 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.

3.2 Empirical Specifications

The above 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. As shown in section 4, our data indeed suggest 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.

An alternative is to restrict ourselves to the sample of individuals who ran for office and compare winners against losers. Narrowing the analysis to this sample could potentially partial out most of the unobserved characteristics that lead some individuals to run for congress in the first place. However, it is likely that *winning* an election is also associated with unobserved characteristics such as talent that are also correlated with economic success. An individual who was able to win 90% of the vote in a given district is probably quite different from one who only managed to capture 10%. Thus, a naive comparison of winners and losers will not allow us to estimate the causal effect of politics on wealth accumulation.

To estimate a causal effect of political office-holding on wealth accumulation we employ an 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 over the loser, provide us with an empirical counterpart of the above counterfactual. If we believe that the outcome in 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. The underlying identification assumption is that the winning and losing candidates in close elections are similar in terms of unobserved characteristics. This is plausible, since both decided to run for office at the same time under the same circumstances, and both received approximately the same share of votes. However, only one of them is actually "treated" with a political office due to a random shock not correlated with the candidates' characteristics. In a sense, close losers show us what would have happened to the close winners had they not won.

In the empirical analyses 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.[7]

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

$$Wealth_i^t = \beta_0 + \beta_1 Wealth_i^{t-10} + \beta_2 Winner_i^t + \beta_3' \mathbf{X}_i + f(VoteShare_i) + \epsilon_i^t$$
$$\forall i \text{ such that } |0.5 - VoteShare_i| < h \tag{1}$$

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 the election and served in the U.S. House during the period between the two census years, and $Wealth_i^{t-10}$ 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.

In the most basic analysis we restrict attention to a small window h that determines a "close elections sample," and 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, while providing an unbiased estimate of the rents from political office, may be inefficient since it relies on a small sample. An alternative is to choose a larger window h and control flexibly for an nth-order polynomial on each side of the discontinuity. The idea underlying this approach is that a flexible functional form of the vote share should adequately control for the unobserved characteristics of candidates correlated with electoral success and wealth, allowing the estimate of β_2 to capture exclusively the effect of winning the election. A weakness of this approach is that it relies crucially on having correctly specified the functional form of $f(\cdot)$, which may be sensitive to observations far away from

[7] 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.

the discontinuity. Angrist and Pischke (2009) suggest decreasing the order of the polynomial as the window h becomes smaller, and Imbens and Lemieux (2008) propose estimating local linear regressions on each side of the discontinuity for a specially chosen value of h. Most importantly, the estimates should be relatively robust to the choice of window h and to the choice of the functional form of $f(\cdot)$. Thus, in our subsequent analysis we report our results for various values of h (5%, 3% and 2% windows) and different control functions.

The different estimates based on equation (1) correspond to the reduced form effect of winning the first race by a small margin on wealth accumulation – that is, 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 election. As we discuss below, this is not a major concern in our case, because a substantial majority of candidates (95%) who lose their first election never serve in Congress, and thus the number of "non-compliers" is small.[8]

We report the regression results using wealth in levels and log(wealth) as our dependent variable. 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.

Finally, we can also include interaction terms in the specifications above, in order to explore differences between close winners and losers under different political environments and institutions. For example, we explore whether the rents from political office are affected by the degree of power politicians exercise, as measured by the key committee posts they hold.

4 Data and Descriptive Statistics

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, and 1870, which provide us with the wealth, occupation and other

[8]Imbens and Lemieux (2008) propose a Wald estimator to address the potential bias introduced by non-compliers. Given that non-compliers are not a major concern in our setting we do not report these. The Wald estimates would be larger than our reported "intention to treat" estimates.

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. 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).[9]

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*.[10] 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. Finally, we use Canon, Nelson and Stewart (1998) to construct measures of congressmen's party leadership and committee positions.

In all of our analyses we limit attention to "serious" candidates, which we define as candidates who received at least 25% of the vote in at least one election.

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 1, there were about 2,400 races to the House of Representatives between 1845 and 1875, involving about 3,500 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),

[9] 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.

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

and about 33% received less than 53% of the vote. This will be important for our empirical analysis that will rely mainly on these close races.

Also, there were very few "career congressmen" compared to today.[11]. 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. 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. We revisit this point when discussing the external validity of our RDD estimates.

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.[12] 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.[13] 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.

Finally, as shown in Appendix Table A.1, there was a clear switch in party fortunes in about 1858. The Democrats controlled congress for most of the period 1850-1858, and the Republicans controlled congress for the entire period 1860-1868. We will present some suggestive data that the switch in party control is correlated with the economic fortune of

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

[12] 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.

[13] Some candidates – about 5% of our sample – run 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.

candidates from the different parties.

4.2 Census Wealth Data

The wealth data are from the 1850, 1860 and 1870 Federal U.S. censuses. These are the only years in which the Federal census collected information on people's wealth. The census reported real estate wealth in 1850, 1860 and 1870, and personal wealth in 1860 and 1870. In addition, separate slave schedules in 1850 and 1860 reported the number and age of all slaves owned by each slaveholder.

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."

In addition, census records provide information on year and place of birth, county and town of residence and occupation. 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 provides a search engine to locate records by first, middle and last name as well as year and place of birth and place of residence.

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 12,300 cases.[14] This corresponds to an overall success rate of about 80%. 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

[14] 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.

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. In Section 8 we discuss in more detail the reasons we could not find the census records for some individuals, and we provide evidence that this does not introduce any important biases to our analysis.

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 responded 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. We discuss the reliability of the census wealth data for our purposes in detail in Section 6.2. 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.[15] 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 dollars of additional wealth in 1860 and with 40,000 dollars 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.[16] For robustness, we report all our results using both reported wealth and the

[15] 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 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.

[16] 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.

number of servants as dependent variables.

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.

To compare the politicians and candidates in this period to other groups of the population, we use the Integrated Public Use Microdata Series (IPUMS) census samples for 1850, 1860 and 1870 collected by the Minnesota Population Center. These constitute representative 1% samples from each population census and provide information on every single variable collected in the census. This allows us to compare the politicians 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 1850-1870 censuses have been used by many economic historians.[17]

Tables 2a and 2b present some basic descriptive statistics of wealth levels and changes between congressional candidates and other individuals in the IPUMS sample. We present our results for free states (upper panel) and slave states (lower panel) separately. Table 2a reports summary statistics on initial wealth, prior to running for congress – i.e., figures for wealth, servants and slaves 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 and slaves (the median number of servants is 1 in all but one sub-group of candidates). In the *Slaves/Servants* panel, the figures are for slaves in 1850 and 1860, and servants in 1870.

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 citizens

[17]The list includes Gallman (1969, 1970), Soltow (1975a, 1975b), Lindert and Williamson (1980), Battalio and Kagel (1970), Foust and Swan (1970), Wright (1970a, 1970b), Schaefer (1987), Bateman and Foust (1974), Atack and Bateman (1981), Kearl and Pope (1984), Steckel (1988a, 1988b, 1987, 1989, 1990, 1994, 2001), Conley and Galenson (1994), Shammas (1993), Ferrie (1996, 2004), and Stewart (2006).

in the IPUMS sample. Congressional candidates were in the 99th percentile of the overall wealth distribution. Somewhat more surprisingly, congressional candidates were even rich 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 both 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 free-state 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.[18] 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 even when compared with congressmen today.

A comparison of different types of candidates reveals that those who won and actually served tended to be richer than candidates who ran for congress but never served. 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 further economic and political success, is correlated with winning elections and serving in congress. Comparing individuals in free and slave states may be misleading for several reasons, and should be limited to the 1850 figures. The 1860 figures are not strictly comparable because slave state candidates ran for congress during the 1860s only after the Civil War – in the first half of the decade the southern states had seceded and organized the Confederate Congress. The

[18]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 todays 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.

figures for 1870 in the slave states are even less comparable, since the abolition of slavery implied a large decrease in personal wealth for previous slave owners that did not occur in the free states. Due to these factors, most of our subsequent analysis focuses on the sample of free states. In any case, the descriptive statistics reveal that candidates in both regions were similarly rich in 1850.

A comparison of wealth levels across parties does not reveal any robust patterns. Consider the free states. In the 1850s, Democratic candidates were poorer than Whigs. However, in the 1860s Republican and Democratic candidates were similarly wealthy (Democrats have a larger mean but Republicans have a larger median), 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 or slaves in free and slave states (upper and lower panels, respectively). The samples correspond to candidates who ran during the respective decades – i.e., candidates who ran between 1850 and 1860 in columns 1 and 2, and those who ran between 1860 and 1870 in columns 3-5. Overall, the wealth of congressional candidates in the free states 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 sample.[19] 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. The wealth of congressional candidates in the slave states grew faster than that of their free-state counterparts during the 1850s, but stagnated during the 1860s, due to the emancipation of slaves, and, perhaps, wartime losses.

In addition, there is some evidence that those who won and served in congress became richer than those who lost their election, at least in the free states. Column 4 reveals that on average winners became about 15 percentage points richer than losers during the 1860s – using the median change in column 3, this corresponds to about $5,000. The figures for servants in column 5 exhibits a similar pattern. In the 1850s, winners did only slightly better

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

than losers in terms of wealth, and about equally well in terms of servants. Overall, while the differences between winners and losers are all positive, they are hardly overwhelming. As we show below however, these averages mask a large amount of heterogeneity.

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 of free states, 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. 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%. There is no evidence that closers winners accumulated more wealth than close losers during the 1850s – if anything, close winners did 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% larger than that of those who ran but lost by a small margin. We explore this more systematically in Section 5.

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 23% 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 estimate the causal effect of serving in congress on wealth accumulation, following the RDD based on close elections presented in Section 3.2.

5.1 Preliminaries: Balance across Covariates in Close Elections

Before presenting our main regression estimates we provide evidence that helps justify the identification strategy based on close elections. 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 most of our detailed analyses, we focus on a 3% window around the 50% threshold. We call this the "close election" sample. Although this window is fairly wide, we use it because we often need to cut the sample in various ways and explore interaction effects, and our sample sizes become too small if we use windows such as 1% or even 2%. Where our sample sizes are large enough, we present results for a 2% window as well as the 3% window.

Table 3 presents differences in various covariates observed in the census records across candidates in the "close election" sample. We focus on the free states and present differences for various sub-samples that will become important in our subsequent analysis – in particular we split the 1860s into the Civil War and non-war years. 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 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. 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 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 significant.[20]

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 the main sub-sample of interest below, the

[20]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.

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.

5.2 Regression Discontinuity Results

In this section we present the main results of our regression discontinuity analysis based on equation (1) introduced in section 3.2.

Table 4 presents our main results for the sample of free and slave states during the 1850s (first two panels) and for the free states during the 1860s (bottom panel). The layout of the table is somewhat non-standard, but straightforward. Each row corresponds to a different regression, and we only report the estimated coefficient of β_2 associated with the $Winner$ dummy, together with its standard error (in parentheses) and the number of observations used in the estimation (in brackets). The panels for wealth changes in the 1850-1860 period report median regressions with total wealth in 1860 as the dependent variable in the first three columns and OLS regressions with the log of total wealth in 1860 as the dependent variable in the middle three columns. Given the skewness of the wealth distribution in our sample, we always report median regression estimates when using wealth in levels as the dependent variable. The last three columns report OLS estimates using the number of servants (slaves) in the free (slave) states in 1860. 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 between 1851 and 1860.The bottom panel, for the 1860-1870 period, reports regressions for total wealth in 1870 in the first three columns, log of total wealth in the middle three columns and the number of servants in the last three columns. The samples for the 1860-1870 period include candidates who ran for the 36th-41st congresses between; these are candidates who ran between 1858 and 1869 and, if they won, served between 1861 and 1870. All regressions include the following set of controls: initial wealth (for regressions with wealth as dependent variable), initial number of servants/slaves (for regressions with number of servants/slaves as a dependent variable), age, age^2, occupation dummies for lawyer, farmer and manufacturer/merchant/banker, and a full set of state fixed effects.

The first row of each panel reports the OLS or median regression estimates of equation (1) on the full sample of candidates, with no control function. These estimates provide a useful benchmark for the RDD estimates; however, for the reasons discussed in section 3.2 they do not correspond to the causal estimate of serving in congress. The second row of each panel reports estimates for the full sample of candidates, and includes two 3rd order polynomials in the vote share of the first election, one on each side of the 50% threshold. The third row restricts the sample to those who obtained a vote share between 45% and 55% in their first election, and includes two linear control functions in the vote share, one on each side of the threshold (the local linear approach described by Imbens and Lemieux, 2008). Finally, the fourth and fifth rows of each panel focus on the "close election" sample based on 3% and 2% windows, respectively, and do not include a control function in the forcing variable.

Consider the estimates for the 1850-1860 period. In free states, the estimate for *Ending Wealth* in the first row suggests that the change in total wealth was about $3,000 larger for candidates who won their first race relative to those who lost. The baseline estimate for *Ending Log Wealth* is similar, suggesting that the change in wealth was 17 percentage points larger for winners relative to losers. These estimates are statistically significant at the .05 level. The various RDD estimates shown in rows 2-5 present a mixed pattern. They are often small and statistically insignificant, or even negative – especially those based on the close election sample. This suggests that the positive coefficients in the first row may confound the effects of winning with other characteristics of candidates. The overall patterns for the slave states, shown in the middle panel, are similar and the estimates are small and are never statistically significant. In sum, the RDD results for the 1850s do not provide any robust evidence of abnormal wealth accumulation by members of congress during this period, relative to those who lost their first election by a small margin. This contradicts the claims made by many observers and historians regarding rampant corruption in the 1850s (recall the discussion in Section 2), at least with respect to congress.

The estimates for the 1860-1870 period are reported in the bottom panel of Table 4, and focus exclusively on the free states.[21] The estimates in the first row are similar to those for the 1850-1860 period. For *Ending Wealth*, the estimate implies that the change in total wealth was about $4,000 larger for candidates who won their first race relative to those who lost, but the estimate is not statistically significant at the .05 level. For *Ending Log Wealth*, the estimate implies that the change in wealth was 22 percentage points larger for winners relative to losers and the estimate is statistically significant at the .05 level. For *Ending Servants* the estimate implies that winners hired an additional 0.22 of a servant relative to losers and is also statistically significant at the .05 level.

The RDD estimates are noticeably different than those for the 1850-1860 period. Consider the middle panels, with log wealth as the dependent variable. The estimates in rows 4 and 5 indicate that in the close election samples those who served during the 1860s accumulated about 40-50% more wealth than those who lost. The RDD estimates using a control function, in rows 2 and 3, are similar in magnitude. However, the evidence is not robust across dependent variables. Using ending wealth or servants as the dependent variable, the RDD estimates are positive but not statistically significant. Overall, then the estimates for the 1860s provide some suggestive evidence of rent accumulation during the 1860s. In the next section we explore the results for this decade in more detail.

[21] As discussed earlier, the analysis of the slave states is problematic during the 1860-1870 period since (a) candidates in these states ran only in the second half of the decade after the Civil War and (b) total wealth figures in slave states are affected by war destructions and by the fact that the abolition of slavery implied a substantial reduction in the personal wealth of previous slave owners.

5.3 Wealth Accumulation during the Civil War

The results for the period 1860-1870 reported in Table 4 confound the effects of serving 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 above, 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, at least in the north.[22] As shown in Figure 1 above, federal spending returned to levels roughly similar to those of the 1850s after 1866.

In order to address these differences, in Table 5 we report the results for the 1860-1870 period but allowing a different coefficient for those who served during the Civil War years and for those who served in the non-war years. We do not run separate regressions for the Civil War and non-war sub-samples, but estimate the coefficients in the same regression in order to test whether they are statistically different. Thus, the estimates of the corresponding rows from each panel are from the same regression; however, we report them separately for presentation purposes.[23] The bottom panel reports the p-value of an F-test for whether the coefficients for the Civil War and non-war samples are equal.

The estimates in the first row reveal that congressmen who served during the Civil War experienced an increase in total wealth that was $10,000, or 23 percentage points, larger than those who ran but did not serve during this period. The estimates for the number of servants reveal a similar pattern. By contrast, none of the corresponding estimates in the bottom panel provide evidence of abnormal wealth accumulation by those who served in the non-war years, relative to those who ran but lost in their first attempt to run during this period.

[22]In the south, the period of Reconstruction was probably anything but "normal."

[23]We do not report results from a fully saturated regression where we allow all controls 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 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.

The different RDD estimates reported in rows 2-5 of both panels confirm this general pattern, and provide evidence of an even larger effect of serving in congress during the Civil War. The point estimates for *Ending Log Wealth* suggest that those who served during the Civil War accumulated about 33-55% 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 $14,000-$24,000 dollars (approximately $700,000-$1,200,000 in current values) relative to those who ran but did not serve. This is very similar to the point estimates produced by the median regressions using *Ending Wealth* as the dependent variable. The estimates for Civil War congressmen are always statistically significant, and relatively robust across our different specifications and dependent variables. The estimates using *Ending Servants* show a similar pattern, indicating that Civil War winners accumulated up to 0.5 of a servant more than losers. This is comforting since the number of servants, although a coarse measure of wealth, was harder to misreport to census enumerators.[24]

By contrast, the estimates for congressmen who served during the non-war years are almost always small and statistically insignificant. For *Ending Wealth* and *Ending Servants*, the F-tests always reject the null hypothesis that coefficients for Civil War and non-war years are the same at the .1 level, and usually at the .05 level. Note, however, that for *Ending Log Wealth* the F-tests fail to reject the null hypotheses that the coefficients for Civil War and non-war congressmen are equal to each other.

Figure 3 illustrates these results graphically. It shows RDD Plots for two variables, log wealth and servants. More precisely, we first regress *Ending Log Wealth* (*Ending Servants*)

[24]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 to1865 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.

on *Initial Log Wealth* (*Initial Servants*) and compute the residuals. We then compute equal-sized binned averages based on vote shares, and plot these averages against the vote shares. We also plot locally-smoothed polynomial curves, which are allowed to differ on each side of the 50% threshold, together with 95% confidence intervals. We show plots for the Civil War and non-war years. The graphs tell the same story as Table 5 – for both dependent variables there is a sizable jump at the threshold for Civil War years (top panels), but not for the non-war years.

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. Note that these are similar to the lower-end RDD estimates reported in Table 5. 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.[25]

Evidence of abnormal wealth accumulation between 1860 and 1870 by congressmen who served during the Civil War, but not by those who served during the pre-war or post-war years, constitutes the main finding of our paper. Moreover, the bottom panels of Table 3 reveal that this effect is not driven by pre-existing differences in the characteristics of those who served during this period. What can explain our evidence of rent accumulation by Civil War congressmen? A first element of central importance in our interpretation is the size of federal government spending. Figure 1 illustrates the evolution of nominal government spending by the federal government between 1845 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. A second consideration

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

is that 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.

Another potential explanation of our results 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. The descriptive statistics in Table 3 do not provide 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.

6 More on the Civil War Years

We now explore the 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.

First, we consider military contracts. Wilson (2006) 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.[26] 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 free states. We then re-estimate the regressions reported in 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.

Table 6 presents the results of this analysis. As in Table 5, we do not run separate regressions for the various sub-samples, but estimate the coefficients in the same regression in order to test whether they are statistically different. Thus, again, the estimates of the corresponding rows from each panel are from the same regression, and we report them separately for presentation purposes. The top two panels report the estimates for those who served during the Civil War years in large contracting states (top panel) and the remaining free states (second panel). The third and fourth panels report the estimates for those who served during the non-war years of the 1860s in the large contracting states (third panel) and in the remaining free states (bottom panel). In each row in panels 2-4, we report the p-value of an F-test of the hypothesis that the coefficient reported in the row is equal to the corresponding coefficient in the top panel (i.e, the corresponding estimate for winners from large contracting states who served during the Civil War).

The estimates provide robust evidence of large returns for congressmen from these states who served during the Civil War. The RDD estimates suggest that congressmen from these states accumulated 40-50% more wealth than those who ran and lost the election by a small margin. Based on average wealth values in 1860, these estimates imply that Civil War congressmen in states with large contracts accumulated an additional \$17,000-22,000 (\$850,000-\$1,100,000 in current values) relative to the close losers from those states who "almost" served during the Civil War. This is similar to the point estimates of \$18,000-\$28,000 produced by the median regressions using *Ending Wealth* as the dependent variable. The estimates using number of servants as the dependent variable exhibit the same overall

[26] We exclude Delaware, Maryland and Missouri because they were slave states. Including it does not affect the results.

pattern, and all of the estimates are statistically significant at conventional levels. On the other hand, the third panel of Table 6 reveals that congressmen from the large contract states did not become abnormally richer in the non-war years once spending in war supplies in these states declined dramatically. In all but two cases – using the 5% window for the wealth variables – the estimates are small and statistically insignificant. For the regressions with *Ending Wealth* and *Ending Servants* as the dependent variables, the F-tests always reject the hypothesis that the coefficients for Civil War years and non-war years are equal. This is not the case for the regressions with *Ending Log Wealth* as the dependent variable.

For the free states that were not home to any of the largest military contractors, the results are noticeably different. For the *Ending Wealth* and *Ending Log Wealth* variables, the estimated coefficients are small and statistically insignificant in all specifications. Moreover, there are no systematic differences between the Civil War and non-war years. For *Ending Servants*, on the other hand, the pattern of coefficients during the Civil War years is similar to that for the states with large contractors. Also, for *Ending Servants* and *Ending Log Wealth* the coefficients for the Civil War years are always larger than those for the non-war years, but the opposite is often true for *Ending Log Wealth*.

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.[27] These committees are: Ways and Means (responsible for many appropriations bills including many for army and navy funding), Military Affairs 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. Naturally, these results must be interpreted

[27]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.

with caution, since committee assignments are not random, and selection onto different committees might be correlated with other characteristics of congressmen. Thus we are not estimating the *causal* effect of belonging to a top committee but rather, we are assessing the extent to which our interpretation for the Civil War years is consistent with the correlations observed in the data.

Table 7 has the same structure as Table 6. Again, the estimates in the corresponding rows of each panel are from one regression. The top two panels report the estimates for those who served during the Civil War years on the military funding committees (top panel) and those who served on other committees (second panel). The third and fourth panels report the estimates for those who served during the non-war years of the 1860s on military funding committees (third panel) and other committees (bottom panel). Panels 2-4 report the p-value of an F-test for whether the respective coefficient is equal to the coefficient in the top panel (corresponding to those from in top committees who served during the Civil War).

The estimates provide evidence of especially large returns for congressmen who served on the important military spending committees during the Civil War. The RDD estimates for *Log Ending Wealth* suggest that congressmen on these committees accumulated 50-70% more wealth than those who ran and lost the election by a small margin. These translate into absolute dollar figures similar to the median regression estimates for *Ending Wealth*, which range from $25,000 to $45,000. Again, the estimates using the number of servants as the dependent variable exhibit a qualitatively similar pattern, and all but one of the estimates are statistically significant at conventional levels. On the other hand, the third panel of Table 7 reveals a mixed pattern for congressmen who served on key military spending committees, but not during the Civil War years. The estimates for *Log Ending Wealth* are about half the size of those in the top panel and they are statistically insignificant. The estimates for *Ending Wealth* are also small and generally statistically insignificant. The estimates for *Ending Servants* are even negative.

The coefficients in the bottom panel – for those who did not serve on military spending committees and also did not serve during the Civil War are in all cases small in magnitude and statistically insignificant.

Overall, the evidence in Tables 5-7 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.

7 Robustness Checks

In this section we perform further checks on the RDD specification, to verify that the divergent patterns of wealth accumulation across winners and losers during the 1860-1870 period were not driven by pre-existing differences in wealth accumulation during the 1850-1860 period, before serving in congress. We also assess the extent to which congressmen were able to accumulate additional wealth *after* they left office, relative to those who ran but never served. Finally, we discuss the external validity of our regression discontinuity estimates and propose an alternative empirical method that confirms our main findings.

7.1 Placebo Regressions

We begin with "placebo" regressions, which are reported in Table 8. This table has the same structure as Table 5, but focuses on total wealth, log wealth, and servants in 1860 as the dependent variables. If the estimated coefficients in Table 5 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, i.e. wealth accumulation between 1850 and 1860.

Reassuringly, in Table 8 the estimates for both Civil War and non-war congressmen are small and never statistically significant at conventional levels, suggesting that our main results in Table 5 do not simply reflect pre-existing trends in wealth accumulation across close winners and close losers. Moreover, the differences between those who served in the Civil War and non-war years are small and never statistically significant.

7.2 Contract States and Top Military Committees in the 1850s

A potential concern with the results in Tables 6 and 7 is that they reflect an overall higher ability of congressmen serving in large contract states, or in top military committees to accumulate wealth in periods other than the 1860s when the Civil War took place. To address this concern, in Table 9 we explore whether congressmen who served in large contract states or top military committees during the 1850s accumulated more wealth between 1850 and 1860. In panel 2 we report the p-value of an F-test of the hypothesis that the coefficient reported in the row is equal to the corresponding coefficient in panel 1; analogously, in panel 4 we report the p-value of an F-test of the hypothesis that the coefficient reported in the row is equal to the corresponding coefficient in panel 2.

The results show that congressmen who served in large contract states during the 1850s did not become abnormally richer between 1850 and 1860; the point estimates for all the dependent variables are small, especially when compared to those reported in Table 6, and are rarely statistically significant. Moreover, we can never reject the hypothesis that the point estimates for those who served in large contract states are equal to the estimates for those who served in other free states. The bottom panel reveal a similar pattern for congressmen who served in top military committees during the 1850s. The point estimates are small and are rarely statistically significant, and again we can never reject the hypothesis that the estimates for congressmen serving in top military committees are equal to the estimates for congressmen serving in other committees.

7.3 Returns After Leaving Congress

Finally, in this section we estimate the extent to which congressmen were able to accumulate additional wealth after they left office, relative to those who ran but never served. This helps us assess some of the possible mechanisms underlying the accumulation of rents from office. Previous research suggests that politicians may be able to establish contacts and networks while in office that they will only be able to exploit for personal benefit once they leave congress and return to their regular business activities. 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, land grants, that former congressmen receive from those inside government after leaving congress. If political institutions during normal times are effective at controlling the accumulation of rents by those in office, then we may only observe unusual wealth accumulation after congressmen leave congress and are no longer watched as carefully by the media and government officials. We can perform this analysis for candidates who ran between 1845 and 1850 (but not during the 1850s) by studying wealth accumulation between 1850 and 1860, as well as for those who ran during the 1850s (but not during the 1860s) by studying wealth accumulation between 1860 and 1870.

Table 10 presents the estimates from regressions analogous to those reported for the free states in Table 4, but where now the dependent variables correspond to log wealth in the decade after our individuals left office and did not run or serve in congress any longer. The results from the upper panel suggest that those who served between 1845 and 1850 (but not during the 1850s) did not become richer between 1850 and 1860 than those who ran between 1845 and 1850 but never served. The point estimates of the RDD regressions are always small and statistically insignificant. The bottom panel reports the corresponding estimates on ending wealth in 1870 for those who served (or ran) during the 1850s but not during the 1860s. Again, the results reveal no evidence of abnormal wealth accumulation after leaving office by congressmen who served during the 1850s – the point estimates of the RDD regressions are generally small and never statistically significant.

7.4 Threats to External Validity: Before-and-After Analysis

A common concern with RDD estimates is that they provide *local treatment effects* for observations or individuals around the given threshold and thus they 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. 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. In Section 4.1 we provided evidence for the relatively short careers of congressmen during this period which suggests that this may not be such a major concern (congressmen may not have been constrained in their rent-seeking behavior by the goal of building a long congressional career). In fact, Table 1 illustrates that nearly 40% of congressmen serve only for 1 term and never attempt to run again. This is true whether they won their first race by a small or large margin. Nearly 80% percent of congressmen run at most 2 times and this is also independent of whether they won their first election by a large or small margin.

However, it is easy to think of reasons why the regression discontinuity estimates *over-estimate* 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. Indeed, Table 1 reveals that while those who won their first race by a small margin were as likely to run for reelection as those who won their first election by a large margin, the latter were more likely to win a subsequent reelection bid. In this section, we address some of these concerns and propose a simple "before-and-after" design, first introduced in Querubin and Snyder (2009). This 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.[28] 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 $t = 1860$ (top figure) and $t = 1870$ (bottom figure). 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 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

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

the different census years through a simple regression of the form:

$$Wealth_i^t = \beta_0 + \beta_1 Wealth_i^{t-10} + \beta_2 T_i + \beta_3' \mathbf{X}_i + \epsilon_i^t \tag{2}$$

where $Wealth_i^t$ is the wealth of congressman i in year t, $Wealth_i^{t-10}$ is the wealth of congressman i in year $t-10$, 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 dummies, and state or regions 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 (i.e. all those for which either T_{EARLY} or N_{EARLY} equals 1).

In order to assess the validity of our approach, in Table A.2 we test for pre-existing differences in congressmen who served before and after the different census years. 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, the table shows that treated congressmen do not differ by their initial wealth, a variable that plausibly captures other relevant characteristics such as ability, education, or occupation. In addition – just as one example – the table shows 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.

Table 11 presents the estimates of the main coefficients of interest – i.e., β_2 in equation

(2), the coefficient on T_i. 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. Second, the same is true for the second half of the 1860s. Third, we do find evidence of a relatively large return to serving in congress during the Civil War years. For *Ending Log Wealth* the point estimate is 0.41, and the coefficient is highly statistically significant. For *Ending Servants* the coefficient is 0.39 and also highly significant. For *Ending Wealth* the median regression point estimate is $12,400, but not statistically significant at the .05 level.

These point estimates are remarkably similar to the RDD estimates reported in Table 5. Moreover, notice that wealth accumulation between 1850 and 1860 was not any different between those who served in the late 1850s and in the early 1860s, which suggests that the large returns we find for the latter group do not correspond to pre-treatment differences.

This analysis, which relies on a completely different source of variation, confirms findings of Section 5. This gives us further confidence regarding the external validity of our regression discontinuity estimates. Moreover, the estimates from this two different methods coincide roughly on a return of approximately 40% to a seat in congress during the Civil War years.

8 Assesing the Reliability of the Data

In this section we discuss various issues associated with our data sources and data collection process. First 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 address any potential selection issues that may arise from our inability to find the census record of every single candidate in every census year.

8.1 Census Wealth 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.

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.

Finally, with respect to wealth acquired by politicians, almost all of the behavior that allowed elected officials to benefit from their office was perfectly legal. There was virtually no regulation of "conflicts of interest" of members of congress (or any other officeholders) at the time. Thus, politicians who managed to increase their wealth substantially as a result of political connections would not have any special reasons to hide or misreport it.

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 both 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.[29] 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 A.3, 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 informa-

[29]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.

tion to the census marshall, or instances where the marshal did not request the information.[30] 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 A.4 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 winners, or those originally richer are not more likely to fail to report their wealth which gives us further confidence that this phenomenon does not introduce any systematic bias in our results.

8.2 Failure to Find/Match a Candidate to its Census Record

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

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

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%.[31] In addition, there were frequent typos in the transcription of the original census records which made it harder to find some of the candidates. For a sample from the 1860 census, Steckel (1988) 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 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

[31]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.

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 A.4 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.

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

The evidence presented in the previous sections suggests that the census wealth data (as well as issues associated with its reporting) does not introduce any systematic bias in our analysis. A final question, particularly relevant 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.

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. 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.[32] 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."

Especially useful for our purposes, the special 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 representa-

[32]Bashford was later forced to leave the state and eventually went to Arizona; he was then appointed the territory's first Attorney General, and President Grant later appointed him as the territorial Secretary of State.

tives each 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.[33] 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:

$$Wealth_i^{1860} = \beta_0 + \beta_1 Wealth_i^{1850} + \beta_2 BribeAmount_i + \beta_3' \mathbf{X}_i + \epsilon_i \tag{3}$$

where $\mathbf{X}_i$ includes age and age^2 as controls. The coefficient of interest is β_2, which measures the extent to which larger bribes in 1856 are reflected in higher census wealth in 1860.

In Appendix Table A.5 we report estimates of β_2 in equation (3). 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 OLS estimates and column 2 shows median regression estimates. Column 3 reports the estimate when the logs of these variables are used.[34] 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. This also gives us further confidence that the results for the 1850s reported in Table 4 – that is, lack of evidence of abnormal wealth accumulation by congressmen during this

[33] 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.

[34] 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.

period – are not driven solely by lack of power and measurement error in the 1850 and 1860 census wealth.

9 Conclusions

The results of this paper suggest that the returns to a seat in the House were low during "normal" times in the mid-19th century (1850s and second half of the 1860s) when U.S. institutions appear to have been effective at controlling politicians' behavior. However, such returns increased 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 than those who ran but never served. Given average wealth in 1860, these returns correspond to an additional $17,500 between 1860 and 1870, or approximately $875,000 in present values.

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. 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. Congressmen from these states accumulated up to 70% more wealth than similar individuals who never served. This gives us further confidence in our interpretation. Our results are also consistent with the hypothesis that the political environment during this period attracted more venal candidates, who anticipated that a seat in congress would lead to greater opportunities for war profiteering.

More broadly, our results suggest 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 oversight by the media and other state institutions may be less effective than in normal times. Future research should explore this hypothesis more systematically in other contexts. It 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: Summary Statistics on Congressional Races, Free States

# of Races	= 2373
# of Races w/Margin < 55%	= 1123
# of Races w/Margin < 53%	= 756
# of Candidates	= 2973
# of Democrats	= 1551
# of Whigs	= 490
# of Republicans	= 746

All Candidates

	Won 1st Race		Lost 1st Race	
# who run 1 time	608	37.9%	1943	79.7%
# who run 2 times	618	38.6%	291	11.9%
# who run 3+ times	377	23.5%	203	8.3%
# who win 0 times	0	0.0%	2232	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%	92	13.7%
# who run 3+ times	131	21.0%	98	14.6%
# 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%

Table 2a: Summary Statistics on Initial Wealth									
Free States									
	1850 Real		1860 Total		1870 Total		Servants		
	Mean	Median	Mean	Median	Mean	Median	1850	1860	1870
All candidates	16129	5500	41673	16500	83788	26600	0.64	1.11	1.20
Winners	17318	5500	43308	17025	99470	28235	0.63	1.14	1.26
Losers	15053	5500	40323	15000	68029	26000	0.65	1.09	1.15
Democrats	13474	4500	44381	16050	68413	25000	0.62	1.10	1.27
Republicans	7307	3000	36012	15500	104083	30000	0.37	1.07	1.16
Whigs	19559	7250	.	.	.	.	0.67	.	.
Lawyers	13994	4000	27669	13350	57661	21000	0.60	1.04	1.12
IPUMS HH Heads	927	0	1823	200	2716	200	.	0.14	0.11
IPUMS Lawyers	6722	0	8357	1500	10532	2000	.	0.47	0.45
Slave States									
	1850 Real		1860 Total		1870 Total		Slaves/Servants		
	Mean	Median	Mean	Median	Mean	Median	1850	1860	1870
All candidates	14693	6000	56028	27000	48062	14280	17.14	17.78	1.02
Winners	16137	7000	60530	33038	60644	16570	20.89	21.97	1.15
Losers	13390	5000	50377	22000	34288	13500	13.60	12.59	0.90
Democrats	14799	5000	63649	34000	42142	15990	17.14	12.92	1.19
Republicans	.	.	30510	10488	57674	11875	.	6.16	0.84
Whigs	17094	7300	13200	13200	.	.	15.26	4.00	.
Lawyers	12488	5000	46398	24000	29021	14000	11.07	14.79	1.21
IPUMS HH Heads	988	0	3931	300	1164	0			0.10
IPUMS Lawyers	6384	0	13204	2410	6216	1650			0.28

Figures in the *Servants* and *Slaves/Servants* panels are means. In the *Slaves/Servants* panel, the figures are for slaves in 1850 and 1860, and servants in 1870.

Table 2b: Summary Statistics on Changes in Wealth					
Free States					
	Δ Log Real 1850-1860	Δ Servants 1850-1860	Δ Total 1860-1870	Δ Log Total 1860-1870	Δ Servants 1860-1870
All candidates	0.94	0.53	14700	0.82	0.25
Winners	0.97	0.52	17980	0.90	0.36
Losers	0.92	0.54	12250	0.75	0.16
Winners w/Margin < .03	0.81	0.37	21750	0.98	0.36
Losers w/Margin < .03	0.95	0.58	11000	0.61	0.19
Democrats	1.06	0.54	10700	0.73	0.21
Republicans	1.04	0.34	19000	0.96	0.32
Whigs	0.67	0.56	.	.	.
Lawyers	1.08	0.51	11500	0.78	0.21
IPUMS HH Heads	1.00			0.97	-0.00
IPUMS Lawyers					
Slave States					
	Δ Log Real 1850-1860	Δ Slaves 1850-1860	Δ Total 1860-1870	Δ Log Total 1860-1870	Δ Servants 1860-1870
All candidates	1.23	8.84	0	0.05	
Winners	1.14	9.28	1125	0.08	
Losers	1.32	8.41	-628	0.03	
Winners w/Margin < .03	1.26	10.07	-1250	0.06	
Losers w/Margin < .03	1.03	11.62	850	0.25	
Democrats	1.33	8.16	-1250	-0.04	
Republicans	.	.	4300	0.76	
Whigs	0.83	4.12	-7700	-0.88	
Lawyers	1.41	11.11	-250	-0.01	
IPUMS HH Heads	1.20			-0.67	0.00
IPUMS Lawyers					

All figures are means except those in the Δ *Total 1860-1870* column, which are medians.

Table 3: Balance on Covariates in RDD Samples, Free States (3% margin)				
1850-1860 Period				
	Winner Mean	Loser Mean	Difference	p-Value
Log Initial Real Wealth	8.70	8.50	0.20	0.31
Age	51.18	49.85	1.33	0.27
Lawyer Dummy	0.67	0.68	-0.01	0.94
Manuf/Merch/Banker	0.18	0.21	-0.04	0.49
Farmer Dummy	0.21	0.18	0.03	0.55
1860-1870 Period				
	Winner Mean	Loser Mean	Difference	p-Value
Log Initial Total Wealth	9.70	9.73	-0.03	0.86
Log Initial Servants	1.05	1.08	-0.03	0.83
Age	41.52	41.87	-0.36	0.76
Lawyer Dummy	0.67	0.69	-0.02	0.73
Manuf/Merch/Banker	0.17	0.22	-0.05	0.38
Farmer Dummy	0.13	0.11	0.02	0.69
1860-1870, Civil War Years				
	Winner Mean	Loser Mean	Difference	p-Value
Log Initial Total Wealth	10.01	10.03	-0.03	0.94
Log Initial Servants	1.26	1.55	-0.03	0.23
Age	43.33	43.66	-0.36	0.85
Lawyer Dummy	0.57	0.71	-0.02	0.16
Manuf/Merch/Banker	0.24	0.20	-0.05	0.61
Farmer Dummy	0.20	0.13	0.02	0.37
1860-1870, Non-War Years				
	Winner Mean	Loser Mean	Difference	p-Value
Log Initial Total Wealth	9.35	9.55	-0.03	0.50
Log Initial Servants	0.89	0.78	-0.03	0.55
Age	38.63	40.57	-0.36	0.25
Lawyer Dummy	0.69	0.69	-0.02	0.99
Manuf/Merch/Banker	0.16	0.21	-0.05	0.43
Farmer Dummy	0.06	0.10	0.02	0.42

All samples are restricted to candidates who obtained a vote-share between 47% and 53% in their first race for Congress.

Table 4: Effect of Serving in Congress on Wealth Accumulation

1850-1860 Period, Free States									
	Ending Wealth			Ending Log Wealth			Ending Servants		
Specification	Coeff	S.E.	N	Coeff	S.E.	N	Coeff	S.E.	N
All, no cntrl funct	2848	(1233)	[690]	0.166	(0.075)	[690]	-0.032	(0.079)	[799]
RDD, all, polyn cf	1525	(1786)	[690]	0.196	(0.144)	[690]	-0.135	(0.149)	[799]
RDD, 5%, linear cf	-1334	(2519)	[349]	-0.047	(0.195)	[349]	-0.080	(0.181)	[378]
RDD, 3% margin	-2165	(2729)	[230]	-0.055	(0.131)	[230]	-0.193	(0.147)	[251]
RDD, 2% margin	-3015	(2875)	[164]	-0.115	(0.154)	[164]	-0.182	(0.182)	[184]
1850-1860 Period, Slave States									
	Ending Wealth			Ending Log Wealth			Ending Slaves		
Specification	Coeff	S.E.	N	Coeff	S.E.	N	Coeff	S.E.	N
All, no cntrl funct	4074	(4934)	[324]	0.103	(0.114)	[324]	0.072	(2.643)	[290]
RDD, all, polyn cf	8058	(6925)	[324]	-0.010	(0.218)	[324]	-2.367	(4.947)	[290]
RDD, 5%, linear cf	8129	(15965)	[169]	-0.070	(0.235)	[169]	-2.210	(6.658)	[135]
RDD, 3% margin	-3301	(14606)	[126]	-0.120	(0.179)	[126]	-7.776	(5.272)	[101]
RDD, 2% margin	1106	(15592)	[90]	-0.129	(0.220)	[90]	-8.855	(4.557)	[75]
1860-1870 Period, Free States									
	Ending Wealth			Ending Log Wealth			Ending Servants		
Specification	Coeff	S.E.	N	Coeff	S.E.	N	Coeff	S.E.	N
All, no cntrl funct	3660	(2322)	[747]	0.224	(0.073)	[747]	0.230	(0.075)	[903]
RDD, all, polyn cf	5632	(3290)	[747]	0.510	(0.167)	[747]	0.002	(0.180)	[903]
RDD, 5%, linear cf	9255	(6546)	[355]	0.454	(0.193)	[355]	0.019	(0.211)	[416]
RDD, 3% margin	5137	(3762)	[235]	0.388	(0.125)	[235]	0.222	(0.139)	[281]
RDD, 2% margin	5808	(6226)	[162]	0.471	(0.174)	[162]	0.199	(0.188)	[183]

Quantile regression estimates for *Ending Wealth* dependent variable (columns 1-3). OLS estimates for other dependent variables (columns 4-9). Results not reported for samples with fewer than 50 observations.

Table 5: Effect of Serving in Congress on Wealth Accumulation 1860-1870 Period, Free States									
	Ending Wealth			Ending Log Wealth			Ending Servants		
Civil War Years									
	Coeff	S.E.	N	Coeff	S.E.	N	Coeff	S.E.	N
All, no cntrl funct	10010	(2671)	[747]	0.233	(0.081)	[747]	0.402	(0.085)	[903]
RDD, all, polyn cf	10434	(2722)	[747]	0.334	(0.106)	[747]	0.307	(0.111)	[903]
RDD, 5%, linear cf	12694	(3838)	[355]	0.326	(0.146)	[355]	0.393	(0.156)	[416]
RDD, 3% margin	20307	(3238)	[235]	0.492	(0.134)	[235]	0.497	(0.147)	[281]
RDD, 2% margin	18503	(3977)	[162]	0.556	(0.192)	[162]	0.503	(0.196)	[183]
Non-War Years									
	Coeff	S.E.	p-val	Coeff	S.E.	p-val	Coeff	S.E.	p-val
All, no cntrl funct	486	(2656)	*0.02*	0.122	(0.081)	*0.38*	-0.057	(0.083)	*0.00*
RDD, all, polyn cf	2818	(2799)	*0.04*	0.229	(0.107)	*0.41*	-0.140	(0.114)	*0.00*
RDD, 5%, linear cf	5771	(3615)	*0.12*	0.351	(0.140)	*0.88*	-0.175	(0.152)	*0.00*
RDD, 3% margin	385	(3139)	*0.00*	0.175	(0.130)	*0.10*	-0.108	(0.147)	*0.00*
RDD, 2% margin	-3463	(3665)	*0.00*	0.182	(0.179)	*0.17*	-0.255	(0.198)	*0.01*

Quantile regression estimates for *Ending Wealth* dependent variable (left panel). OLS estimates for other dependent variables (middle and right panels). Results not reported for samples with fewer than 50 observations. The p-values in panel 2 are for F-tests of the hypothesis that the effect of winning during the Non-War years is equal to the effect of winning during the Civil War years.

Table 6: Wealth Accumulation in 1860-1870 Period, Variation by Military Contract Status

	Ending Wealth			Ending Log Wealth			Ending Servants		
Civil War Years, States with Large Military Contracts									
Variable	Coeff	S.E.	N	Coeff	S.E.	N	Coeff	S.E.	N
All, no cntrl funct	16061	(3060)	[747]	0.278	(0.098)	[747]	0.444	(0.103)	[903]
RDD, all, polyn cf	16609	(3463)	[747]	0.375	(0.118)	[747]	0.353	(0.125)	[903]
RDD, 5%, linear cf	27346	(3370)	[355]	0.427	(0.167)	[355]	0.445	(0.180)	[416]
RDD, 3% margin	29527	(3026)	[235]	0.523	(0.160)	[235]	0.529	(0.175)	[281]
Civil War Years, Other States									
Variable	Coeff	S.E.	p-val	Coeff	S.E.	p-val	Coeff	S.E.	p-val
All, no cntrl funct	5397	(4334)	*0.04*	0.124	(0.145)	*0.38*	0.312	(0.152)	*0.47*
RDD, all, polyn cf	4558	(4618)	*0.02*	0.230	(0.163)	*0.41*	0.207	(0.169)	*0.43*
RDD, 5%, linear cf	6845	(4151)	*0.00*	0.134	(0.217)	*0.22*	0.285	(0.237)	*0.55*
RDD, 3% margin	4302	(4435)	*0.00*	0.423	(0.240)	*0.73*	0.391	(0.272)	*0.67*
Non-War Years, States with Large Military Contracts									
Variable	Coeff	S.E.	p-val	Coeff	S.E.	p-val	Coeff	S.E.	p-val
All, no cntrl funct	-883	(2981)	*0.00*	0.072	(0.096)	*0.17*	-0.056	(0.099)	*0.00*
RDD, all, polyn cf	2695	(3487)	*0.00*	0.175	(0.119)	*0.19*	-0.133	(0.126)	*0.00*
RDD, 5%, linear cf	7490	(3224)	*0.00*	0.360	(0.162)	*0.74*	-0.182	(0.172)	*0.00*
RDD, 3% margin	3004	(2950)	*0.00*	0.183	(0.154)	*0.13*	-0.161	(0.172)	*0.01*
Non-War Years, Other States									
Variable	Coeff	S.E.	p-val	Coeff	S.E.	p-val	Coeff	S.E.	p-val
All, no cntrl funct	3537	(4458)	*0.02*	0.246	(0.148)	*0.86*	-0.053	(0.151)	*0.01*
RDD, all, polyn cf	3635	(4811)	*0.02*	0.357	(0.164)	*0.92*	-0.149	(0.171)	*0.01*
RDD, 5%, linear cf	2545	(4233)	*0.00*	0.345	(0.220)	*0.74*	-0.144	(0.242)	*0.03*
RDD, 3% margin	-2065	(4518)	*0.00*	0.162	(0.256)	*0.23*	0.055	(0.287)	*0.16*

Quantile regression estimates for *Ending Wealth* dependent variable (left panel). OLS estimates for other dependent variables (middle and right panels). Results not reported for samples with fewer than 50 observations. The p-values in panels 2-4 are for F-tests of the hypothesis that the respective coefficient is equal to the coefficient in the top panel, i.e., the coefficient for those from large contracting states during the Civil War years.

Table 7: Wealth Accumulation 1860-1870 Period, Variation by Military Committee Membership									
	Ending Wealth			Ending Log Wealth			Ending Servants		
Civil War Years, Served on Military Spending Committees									
Variable	Coeff	S.E.	N	Coeff	S.E.	N	Coeff	S.E.	N
All, no cntrl funct	20205	(4587)	[747]	0.407	(0.154)	[747]	0.566	(0.156)	[903]
RDD, all, polyn cf	24559	(4096)	[747]	0.470	(0.167)	[747]	0.476	(0.171)	[903]
RDD, 5%, linear cf	29100	(5216)	[355]	0.504	(0.252)	[355]	0.479	(0.262)	[416]
RDD, 3% margin	43821	(6787)	[235]	0.704	(0.246)	[235]	0.795	(0.268)	[281]
Civil War Years, Not on Military Spending Committees									
Variable	Coeff	S.E.	p-val	Coeff	S.E.	p-val	Coeff	S.E.	p-val
All, no cntrl funct	7963	(2672)	*0.01*	0.181	(0.089)	*0.16*	0.394	(0.094)	*0.30*
RDD, all, polyn cf	9383	(2536)	*0.00*	0.249	(0.112)	*0.17*	0.297	(0.116)	*0.28*
RDD, 5%, linear cf	10969	(3152)	*0.00*	0.228	(0.154)	*0.26*	0.428	(0.162)	*0.84*
RDD, 3% margin	10185	(4141)	*0.00*	0.405	(0.147)	*0.25*	0.452	(0.161)	*0.24*
Non-War Years, Served on Military Spending Committees									
Variable	Coeff	S.E.	p-val	Coeff	S.E.	p-val	Coeff	S.E.	p-val
All, no cntrl funct	1429	(3504)	*0.00*	0.130	(0.119)	*0.21*	-0.241	(0.124)	*0.00*
RDD, all, polyn cf	578	(3160)	*0.00*	0.186	(0.131)	*0.20*	-0.308	(0.141)	*0.00*
RDD, 5%, linear cf	8777	(3607)	*0.00*	0.315	(0.175)	*0.55*	-0.318	(0.197)	*0.02*
RDD, 3% margin	8441	(5191)	*0.00*	0.286	(0.186)	*0.21*	-0.414	(0.220)	*0.00*
Non-War Years, Not on Military Spending Committees									
Variable	Coeff	S.E.	p-val	Coeff	S.E.	p-val	Coeff	S.E.	p-val
All, no cntrl funct	70	(2766)	*0.00*	0.057	(0.093)	*0.06*	-0.010	(0.094)	*0.00*
RDD, all, polyn cf	2022	(2761)	*0.00*	0.142	(0.121)	*0.08*	-0.107	(0.126)	*0.00*
RDD, 5%, linear cf	5351	(3291)	*0.00*	0.280	(0.164)	*0.41*	-0.089	(0.172)	*0.04*
RDD, 3% margin	-599	(4254)	*0.00*	0.073	(0.153)	*0.03*	-0.015	(0.169)	*0.01*

Quantile regression estimates for *Ending Wealth* dependent variable (left panel). OLS estimates for other dependent variables (middle and right panels). Results not reported for samples with fewer than 50 observations. The p-values in panels 2-4 are for F-tests of the hypothesis that the respective coefficient is equal to the coefficient in the top panel, i.e., the coefficient for those on military spending committees during the Civil War years.

Table 8: Wealth Accumulation in 1850-1860 Period, Free States (Placebos)									
	Ending Wealth			Ending Log Wealth			Ending Servants		
Civil War Years									
	Coeff	S.E.	N	Coeff	S.E.	N	Coeff	S.E.	N
All, no cntrl funct	-1320	(2330)	[508]	0.073	(0.112)	[508]	0.092	(0.093)	[651]
RDD, all, polyn cf	-98	(2625)	[508]	0.221	(0.170)	[508]	0.188	(0.145)	[651]
RDD, 5%, linear cf	-4017	(6426)	[225]	-0.012	(0.244)	[225]	0.086	(0.238)	[290]
RDD, 3% margin	30	(3766)	[141]	0.021	(0.188)	[141]	-0.011	(0.188)	[186]
RDD, 2% margin	-4527	(5514)	[97]	-0.007	(0.249)	[97]	0.062	(0.235)	[120]
Non-War Years									
	Coeff	S.E.	p-val	Coeff	S.E.	p-val	Coeff	S.E.	p-val
All, no cntrl funct	634	(2526)	*0.61*	0.025	(0.120)	*0.79*	0.079	(0.102)	*0.93*
RDD, all, polyn cf	1578	(2830)	*0.64*	0.184	(0.164)	*0.84*	0.142	(0.139)	*0.76*
RDD, 5%, linear cf	-2865	(5871)	*0.85*	0.174	(0.224)	*0.44*	0.014	(0.221)	*0.76*
RDD, 3% margin	-633	(4367)	*0.91*	0.217	(0.209)	*0.50*	0.192	(0.215)	*0.48*
RDD, 2% margin	3527	(6054)	*0.34*	0.175	(0.262)	*0.63*	0.093	(0.267)	*0.93*

Quantile regression estimates for *Ending Wealth* dependent variable (left panel). OLS estimates for other dependent variables (middle and right panels). Results not reported for samples with fewer than 50 observations. The p-values in panel 2 are for F-tests of the hypothesis that the effect of winning during the Non-War years is equal to the effect of winning during the Civil War years.

Table 9: Wealth Accumulation, 1850-1860, Variation by Military Contract Status and Service on Military Spending Committees

	Ending Wealth			Ending Log Wealth			Ending Servants		
States with Large Military Contracts									
Variable	Coeff	S.E.	N	Coeff	S.E.	N	Coeff	S.E.	N
All, no cntrl funct	2848	(2089)	[690]	0.183	(0.085)	[690]	-0.011	(0.089)	[799]
RDD, all, polyn cf	2984	(2346)	[690]	0.216	(0.150)	[690]	-0.122	(0.155)	[799]
RDD, 5%, linear cf	-1582	(2997)	[349]	-0.038	(0.205)	[349]	-0.090	(0.192)	[378]
RDD, 3% margin	-2761	(4614)	[230]	-0.000	(0.152)	[230]	-0.133	(0.173)	[251]
Other States									
Variable	Coeff	S.E.	p-val	Coeff	S.E.	p-val	Coeff	S.E.	p-val
All, no cntrl funct	5079	(2791)	*0.39*	0.104	(0.161)	*0.66*	-0.106	(0.167)	*0.62*
RDD, all, polyn cf	2640	(3055)	*0.90*	0.125	(0.203)	*0.62*	-0.177	(0.209)	*0.77*
RDD, 5%, linear cf	-459	(3658)	*0.73*	-0.069	(0.260)	*0.90*	-0.054	(0.252)	*0.88*
RDD, 3% margin	-1947	(5835)	*0.89*	-0.208	(0.254)	*0.48*	-0.342	(0.272)	*0.51*
Serving on Military Spending Committees									
Variable	Coeff	S.E.	N	Coeff	S.E.	N	Coeff	S.E.	N
All, no cntrl funct	4259	(1951)	[690]	0.155	(0.118)	[690]	-0.082	(0.120)	[799]
RDD, all, polyn cf	3459	(2354)	[690]	0.166	(0.165)	[690]	-0.199	(0.171)	[799]
RDD, 5%, linear cf	-1893	(3452)	[349]	0.049	(0.220)	[349]	-0.105	(0.214)	[378]
RDD, 3% margin	-4624	(2907)	[230]	0.008	(0.202)	[230]	-0.272	(0.216)	[251]
Not on Military Spending Committees									
Variable	Coeff	S.E.	p-val	Coeff	S.E.	p-val	Coeff	S.E.	p-val
All, no cntrl funct	2484	(1353)	*0.38*	0.170	(0.082)	*0.91*	-0.012	(0.086)	*0.58*
RDD, all, polyn cf	1037	(1802)	*0.24*	0.212	(0.151)	*0.71*	-0.101	(0.155)	*0.44*
RDD, 5%, linear cf	-1100	(3164)	*0.76*	-0.110	(0.207)	*0.35*	-0.068	(0.189)	*0.83*
RDD, 3% margin	-2478	(1979)	*0.46*	-0.075	(0.139)	*0.68*	-0.164	(0.158)	*0.62*

Quantile regression estimates for *Ending Wealth* dependent variable (left panel). OLS estimates for other dependent variables (middle and right panels). Results not reported for samples with fewer than 50 observations. The p-values in panels 2-4 are for F-tests of the hypothesis that the effects of representing a large contract state (serving on a military spending committee) and representing another state (serving on other committees) are equal.

Table 10: Wealth Accumulation After Leaving Congress									
Changes During 1850-1860, Candidate in 1840's, Free States									
	Ending Wealth			Ending Log Wealth			Ending Servants		
Specification	Coeff	S.E.	N	Coeff	S.E.	N	Coeff	S.E.	N
All, no cntrl funct	2390	(2243)	[395]	0.177	(0.104)	[395]	0.256	(0.111)	[423]
RDD, all, polyn cf	1496	(3126)	[395]	0.077	(0.338)	[395]	-0.261	(0.392)	[423]
RDD, 5%, linear cf	-3191	(10591)	[194]	-0.135	(0.382)	[194]	-0.585	(0.540)	[201]
RDD, 3% margin	-2892	(8505)	[121]	0.001	(0.177)	[121]	0.285	(0.243)	[123]
Changes During 1860-1870, Candidate in 1850's, Free States									
	Ending Wealth			Ending Log Wealth			Ending Servants		
Specification	Coeff	S.E.	N	Coeff	S.E.	N	Coeff	S.E.	N
All, no cntrl funct	1251	(3068)	[459]	0.040	(0.095)	[459]	0.271	(0.100)	[516]
RDD, all, polyn cf	2681	(4087)	[459]	-0.048	(0.201)	[459]	0.308	(0.198)	[516]
RDD, 5%, linear cf	6942	(5480)	[233]	0.090	(0.263)	[233]	0.405	(0.234)	[253]
RDD, 3% margin	7256	(5933)	[156]	0.149	(0.169)	[156]	0.285	(0.176)	[165]

Quantile regression estimates for *Ending Wealth* dependent variable (columns 1-3). OLS estimates for other dependent variables (columns 4-9). Results not reported for samples with fewer than 50 observations.

Table 11: Effect of Serving in Congress on Wealth Accumulation Before and After Analysis									
1850-1860 Period, Free States									
	Ending Wealth			Ending Log Wealth			Ending Servants		
When Served	Coeff	S.E.	N	Coeff	S.E.	N	Coeff	S.E.	N
Early in Decade	3560	(2955)	[193]	0.042	(0.127)	[193]	-0.085	(0.188)	[221]
Late in Decade	-847	(2534)	[240]	0.063	(0.151)	[240]	-0.410	(0.117)	[291]
1850-1860 Period, Slave States									
	Ending Wealth			Ending Log Wealth			Ending Slaves		
When Served	Coeff	S.E.	N	Coeff	S.E.	N	Coeff	S.E.	N
Early in Decade	-1795	(12037)	[113]	0.228	(0.205)	[113]	10.547	(6.225)	[99]
Late in Decade	-5178	(7345)	[165]	-0.144	(0.152)	[165]	-2.569	(4.216)	[160]
1860-1870 Period, Free States									
	Ending Wealth			Ending Log Wealth			Ending Servants		
When Served	Coeff	S.E.	N	Coeff	S.E.	N	Coeff	S.E.	N
Early in Decade	12412	(8443)	[248]	0.405	(0.147)	[248]	0.377	(0.161)	[284]
Late in Decade	751	(4403)	[279]	-0.010	(0.123)	[279]	0.154	(0.146)	[317]

Quantile regression estimates for *Ending Wealth* dependent variable (columns 1-3). OLS estimates for other dependent variables (columns 4-9). Results not reported for samples with fewer than 50 observations.

Appendix Table A.1: Dates of Congresses and Partisan Composition

Cong	Dates	Senate	House of Reps.	President
27	5/31/41 - 3/3/1843	29W, 22D (W)	142W, 98 D + 2 (W)	John Tyler (np)
28	12/4/43 - 3/3/1845	29W, 23D (W)	72W, 147 D + 4 (D)	John Tyler (np)
29	12/1/45 - 3/3/1847	22W, 34D (D)	79W, 142 D + 6 (D)	James K. Polk (D)
30	12/6/47 - 3/3/1849	21W, 38D + 1 (D)	116W, 110 D + 4 (W)	James K. Polk (D)
31	12/3/49 - 3/3/1851	25W, 35D + 2 (D)	108W, 113 D + 11 (D)	Z.Taylor/Fillmore (W)
32	12/1/51 - 3/3/1853	23W, 36D + 3 (D)	85W, 127 D + 7 (D)	Millard Fillmore (W)
33	12/5/53 - 3/3/1855	22W, 38D + 2 (D)	71W, 157 D + 6 (D)	Franklin Pierce (D)
34	12/3/55 - 3/3/1857	39D + 23 (D)	83 D + 151 (?)	Franklin Pierce (D)
35	12/7/57 - 3/3/1859	20R, 41D + 2 (D)	90R, 132 D + 15 (D)	James Buchanan (D)
36	12/5/59 - 3/3/1861	26R, 38D + 2 (D)	116R, 83 D + 39 (R)	James Buchanan (D)
37	7/4/61 - 3/3/1863	31R, 15D + 3 (R)	108R, 44 D + 30 (R)	Abraham Lincoln (R)
38	12/7/63 - 3/3/1865	33R, 10D + 5 (R)	86R, 72 D + 27 (R)	Abraham Lincoln (R)
39	12/4/65 - 3/3/1867	39R, 11D + 3 (R)	136R, 38 D + 19 (R)	Andrew Johnson (U)
40	3/4/67 - 3/3/1869	57R, 9D (R)	173R, 47 D + 4 (R)	Andrew Johnson (U)
41	3/4/69 - 3/3/1871	62R, 12D (R)	171R, 67 D + 5 (R)	Ulysses S. Grant (R)
42	3/4/71 - 3/3/1873	55R, 16D (R)	144R, 93 D + 4 (R)	Ulysses S. Grant (R)
43	3/4/73 - 3/3/1875	50R, 19D + 3 (R)	189R, 91 D + 10 (R)	Ulysses S. Grant (R)

Average number of days in session per congress = 186
#D =# of Democrats, #R = # of Republicans, # W = # of Whigs, + # = # in other parties
(D) = Democratic control, (R) = Republican control, etc.
in the 34th Congress, no party controlled the House

Table A.2: Balance on Covariates, Before and After Analysis				
1850-1860, Early in Decade				
	Winner Mean	Loser Mean	Difference	p-Value
Log Initial Real Wealth	8.89	9.16	-0.26	0.18
Age	54.36	58.30	-3.94	0.00
Lawyer Dummy	0.63	0.55	0.09	0.24
Manuf/Merch/Banker	0.20	0.16	0.04	0.54
Farmer Dummy	0.20	0.32	-0.12	0.07
1850-1860, Late in Decade				
	Winner Mean	Loser Mean	Difference	p-Value
Log Initial Total Wealth	10.18	10.02	0.16	0.34
Log Initial Servants	1.13	1.28	-0.15	0.35
Age	47.76	44.93	2.83	0.01
Lawyer Dummy	0.62	0.59	0.03	0.62
Manuf/Merch/Banker	0.24	0.25	-0.01	0.90
Farmer Dummy	0.12	0.17	-0.05	0.29
1860-1870, Early in Decade				
	Winner Mean	Loser Mean	Difference	p-Value
Log Initial Total Wealth	9.96	10.10	-0.14	0.43
Log Initial Servants	1.31	1.24	0.07	0.70
Age	43.38	48.44	-5.07	0.00
Lawyer Dummy	0.66	0.59	0.07	0.27
Manuf/Merch/Banker	0.22	0.30	-0.08	0.17
Farmer Dummy	0.16	0.16	-0.00	0.92
1860-1870, Late in Decade				
	Winner Mean	Loser Mean	Difference	p-Value
Log Initial Total Wealth	9.72	9.16	0.56	0.00
Log Initial Servants	1.05	0.92	0.13	0.36
Age	41.88	36.64	5.24	0.00
Lawyer Dummy	0.63	0.58	0.05	0.36
Manuf/Merch/Banker	0.22	0.26	-0.04	0.47
Farmer Dummy	0.13	0.14	-0.01	0.83

Appendix Table A.3: Census Wealth vs. Wealth in ***Rich Men of Massachusetts***

	Real 1850	Real 1860	Total 1860
Correlation with RMM Wealth	.54	.52	.68
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

Appendix Table A.4:
Assessing the Reliability of the Census Data

	No Report 1860	No Report 1870	Not Found 1860	Not Found 1870
Winner	-0.007	0.026	-0.009	0.026
	(0.019)	(0.033)	(0.034)	(0.033)
Log(Wealth^{t-10})	0.016	-0.005	0.007	-0.005
	(0.015)	(0.017)	(0.019)	(0.017)
Winner × Log(Wealth^{t-10})	0.006	0.004	-0.026	0.004
	(0.019)	(0.026)	(0.022)	(0.026)
Observations	290	310	290	310
R-square	0.078	0.073	0.079	0.073

Appendix Table A.5: LaCrosse & Milwaukee Railroad Scandal			
	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

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

Figure 1

Federal Government Spending Before, During and After the Civil War

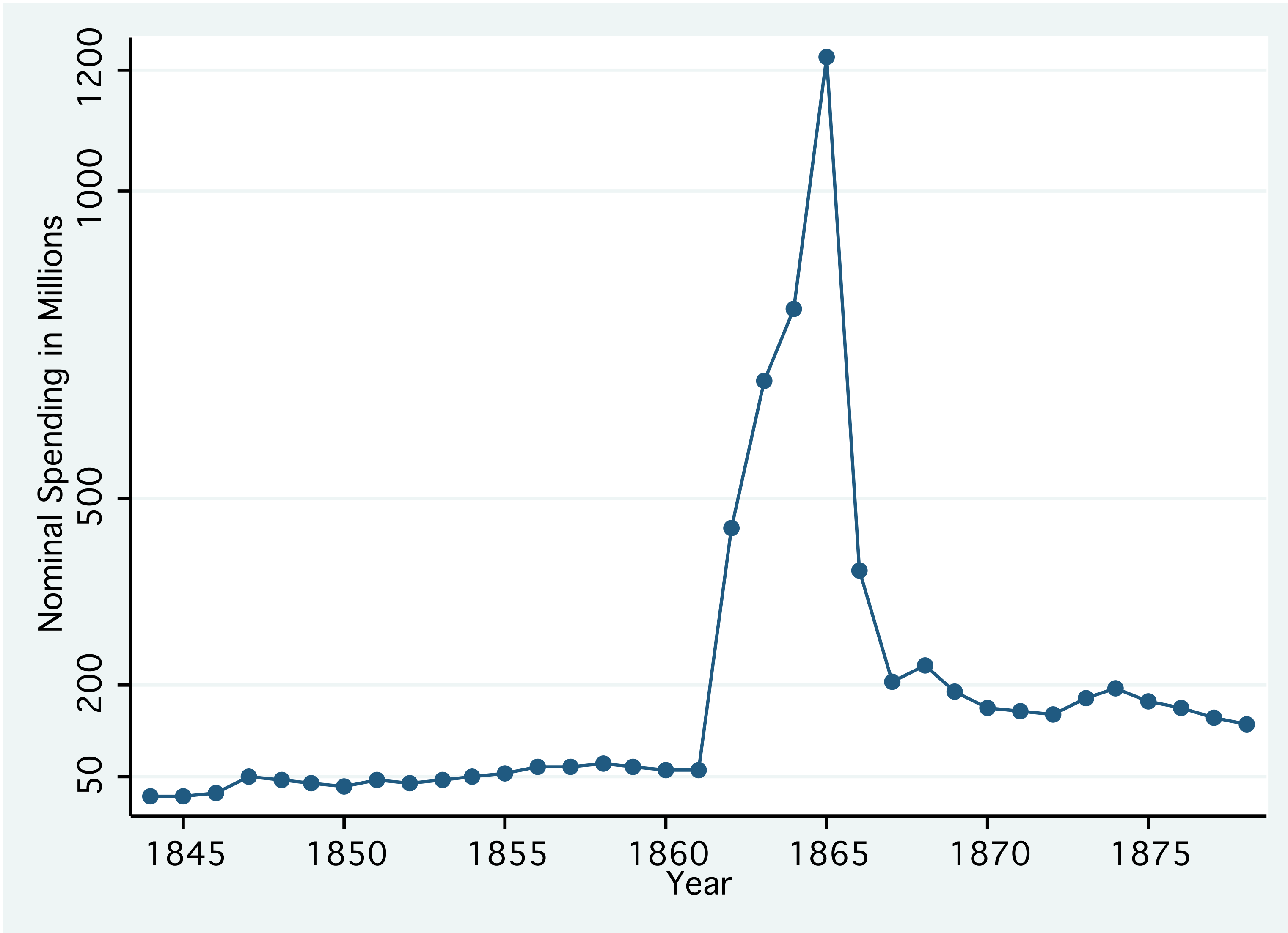
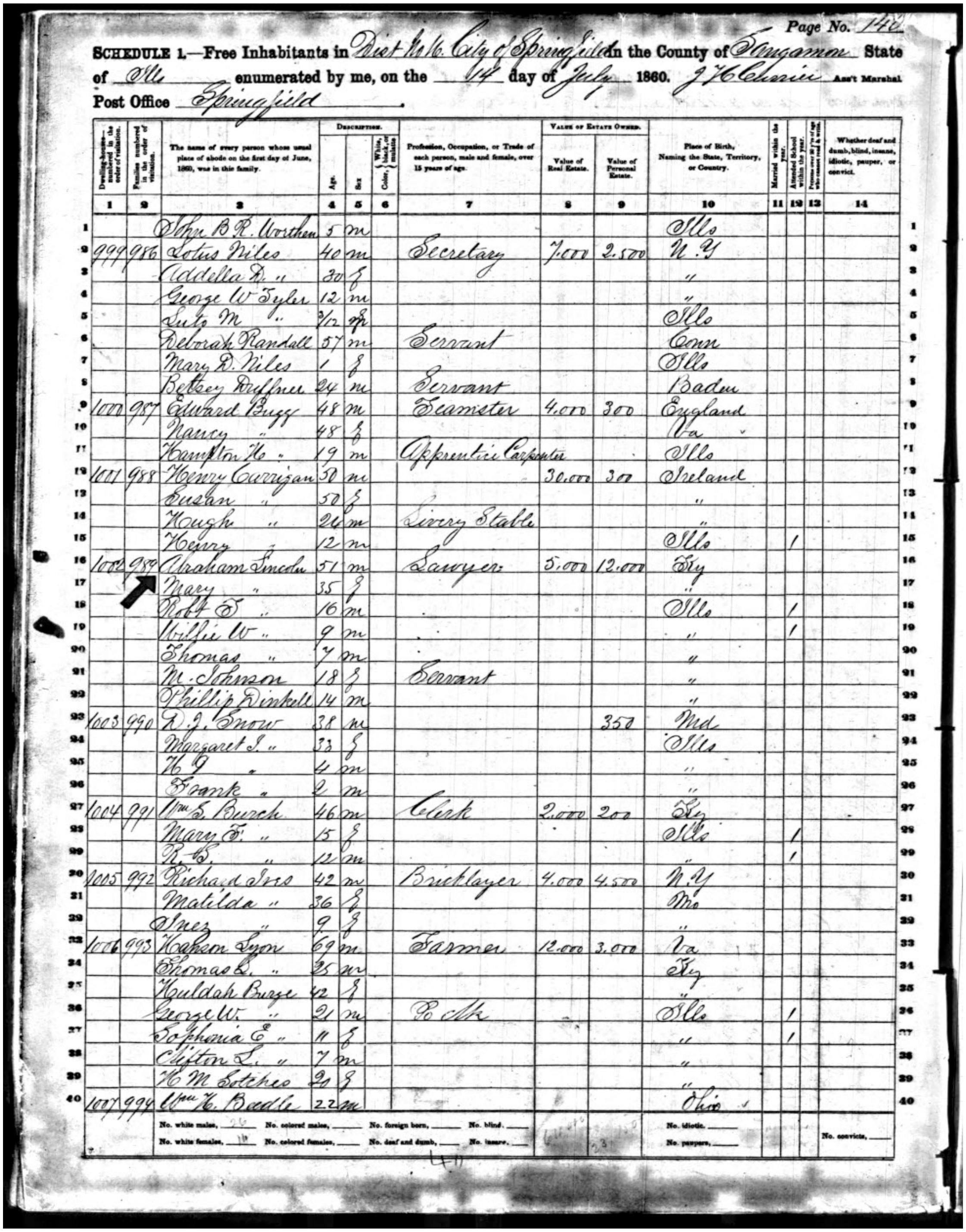


Figure 2: Sample Census Page (with Abraham Lincoln)



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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 Clemin Ass't Marshal.

Post Office Springfield

	Dwelling-houses numbered in the order of visitation.	Families numbered in the order of visitation.	The name of every person whose usual place of abode on the first day of June, 1860, was in this family.	Age.	Sex.	Color, white, black, or mulatto.	Profession, Occupation, or Trade of each person, male and female, over 15 years of age.	Value of Real Estate.	Value of Personal Estate.	Place of Birth, Naming the State, Territory, or Country.	Married within the year.	Attended School within the year.	Persons over 20 y'rs of age who cannot read & write.	Whether deaf and dumb, blind, insane, idiotic, pauper, or convict.
	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1			John B R Worthen	5	m					Ills				
2	999	986	Lotus Niles	40	m		Secretary	7.000	2.500	N.Y				
3			Addella D. "	30	f					"				
4			George W Tyler	12	m					"				
5			Lute M	3/12	f					Ills				
6			Deborah Randall	57	m		Servant			Conn				
7			Mary D. Niles	1	f					Ills				
8			Betsey Duffner	24	m		Servant			Baden				
9	1000	987	Edward Buzz	48	m		Teamster	4.000	300	England				
10			Nancy "	48	f					Va				
11			Hampton H. "	19	m		Apprentice Carpenter			Ills				
12	1001	988	Henry Corrigan	50	m			30.000	300	Ireland				
13			Susan "	50	f					"				
14			Hugh "	26	m		Livery Stable			"				
15			Henry "	12	m					Ills		1		
16	1002	989	Abraham Lincoln	51	m		Lawyer	5.000	12.000	Ky				
17			Mary "	35	f					"				
18			Robt T. "	16	m					Ills		1		
19			Willie W. "	9	m					"		1		
20			Thomas "	7	m					"				
21			M. Johnson	18	f		Servant			"				
22			Phillip Dinkell	14	m					"				
23	1003	990	D. J. Snow	38	m				350	Md				
24			Margaret J. "	33	f					Ills				
25			H. G. "	4	m					"				
26			Frank "	2	m					"				
27	1004	991	Wm S. Burch	46	m		Clerk	2.000	200	Ky				
28			Mary F. "	15	f					Ills		1		
29			R. B. "	12	m							1		
30	1005	992	Richard Ives	42	m		Bricklayer	4.000	4.500	N.Y				
31			Matilda "	36	f					Mo				
32			Inez "	9	f									
33	1006	993	Hanson Lyon	69	m		Farmer	12.000	3.000	Va				
34			Thomas L. "	25	m					Ky				
35			Huldah Burge	42	f					"				
36			George W. "	21	m		Po Clk			Ills		1		
37			Sophonia E. "	11	f					"		1		
38			Clifton L. "	7	m					"				
39			H M Sotcher	20	f					"				
40	1007	994	Wm H. Beadle	22	m					Ohio				

No. white males, 20 No. colored males, ___ No. foreign born, ___ No. blind, ___ No. idiotic, ___

No. white females, 19 No. colored females, ___ No. deaf and dumb, ___ No. insane, ___ No. paupers, ___ No. convicts, ___

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

Figure 3

RDD Plots of Residuals from Regressions of Ending Log Wealth on Initial Log Wealth

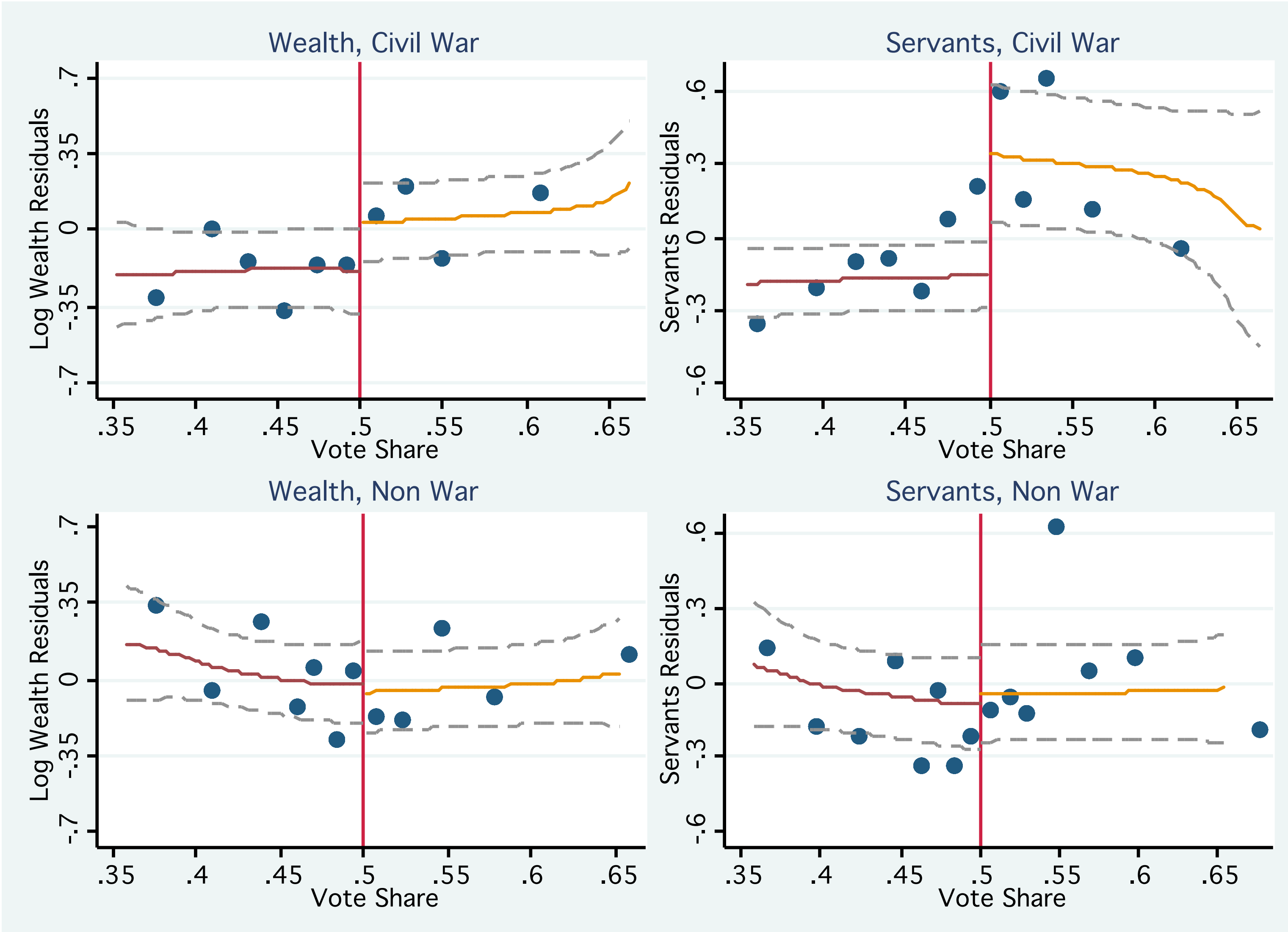
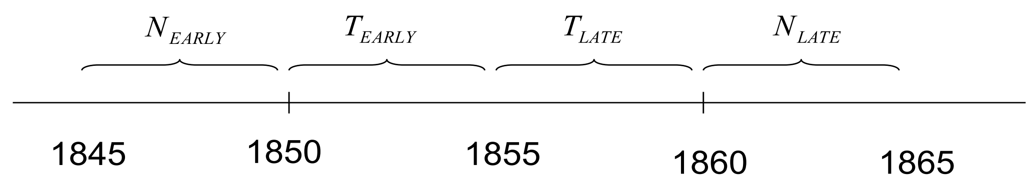


Figure 4

Before and After Design

A. For Wealth Changes Between 1850 and 1860



B. For Wealth Changes Between 1860 and 1870