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The Control of Politicians in Normal Times and Times of Crisis: Wealth Accumulation by U.S. Congressmen, 1850–1880*

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ABSTRACT

We employ a regression discontinuity design (RDD) based on close elections to estimate the rents from a seat in the U.S. Congress between 1850 and 1880. Using census data, we compare wealth accumulation among those who won or lost their first race by a small margin. We find

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evidence of significant returns for the first half of the 1860s, during the Civil War, but not for other periods. Those who won their first election by a narrow margin and served during the period 1861–1866 accumulated, on average, almost 40% more wealth between 1860 and 1870 (roughly $800,000 in present-day values) relative to those who ran but did not serve. We also find that wealth accumulation was particularly large for congressmen who represented states most involved in military contracting and those who served during the Civil War in committees that were responsible for most military appropriations. We hypothesize that increased opportunities from the sudden spike in government spending during the war and the decrease in control by the media might have made it easier for incumbent congressmen to collect rents.

Keywords: Corruption; accountability; Congress; elections; political institutions.

1 Introduction

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

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

and other political institutions in providing incentives and regulating the behavior of politicians.[2]

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

One possible way to study the degree to which democratic political institutions prevent the abuse of power for personal gain is to establish the extent of systematic rent-seeking by politicians.[3] 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. While not a ‘‘smoking gun,'' this type of evidence is widely cited by academics, journalists, and other observers as indicative of corruption. Moreover, as shown in Appendix Table B.4, in the case of a particular Wisconsin scandal, politicians known to have accepted bribes accumulated substantially more wealth than politicians who did not.

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[2] The literature is huge. For an overview see Persson and Tabellini (2000), Grossman and Helpman (2001), and Besley (2006).

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

In this paper, we use historical census data from the United States to address these questions and estimate the magnitude of political rents for members of the U.S. House of Representatives during the period 1850–1880. We compare wealth accumulation by members of the U.S. House of Representatives relative to candidates who ran but lost the election. To address selection and endogeneity concerns that arise from comparing wealth accumulation of election winners and losers, we employ a regression discontinuity design (RDD) based on close elections. That is, we compare wealth accumulation in the decades between 1850 and 1880 among those who won or lost their first congressional race by a small margin. This allows us to estimate the *causal* effect of serving in Congress on wealth accumulation during this period.[4] The outcome of close elections provides us with quasi-random assignment of political power. It therefore allows us to isolate the effect of serving in Congress from the effect of other characteristics of these individuals — such as talent, connections, or charisma — that are correlated with serving in Congress and wealth accumulation.

There are several reasons why the United States during this period provides an ideal setting for exploring some of these ideas. First, the U.S. census recorded wealth in 1850, 1860, and 1870, and we have found the individual census records for a large sample of candidates. We also collected information on the number of domestic servants in each candidate's household as reported in the 1850, 1860, 1870, and 1880 censuses as other proxies for wealth. Second, the United States was by most accounts a consolidated democracy by this time. Its average polity score during this period was 8.8 — exceeded only by New Zealand and Switzerland — and it had the maximum possible score in most of its components. If democratic institutions succeed at regulating the behavior of politicians, then the United States is a natural setting to explore this hypothesis. Most importantly, this period exhibits important variation in the political environment that will allow us to hypothesize about the environments under which rent-seeking may be more prevalent.

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

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[4] See Lee (2008) for a seminal application of RDD to close elections.

Our results can be summarized as follows. We find no evidence of abnormal wealth accumulation by congressmen who served during the 1850s, during the second half of the 1860s, or during the 1870s. This is a remarkable result and stands in contrast with evidence for other countries. One plausible interpretation is that U.S. political institutions during "normal times" were effective at controlling politicians and preventing the abuse of power for personal gain. However, we do find evidence of significant rent-seeking for congressmen who served during the first half of the 1860s, which coincided with the Civil War. Those who won their first election by a narrow margin and served during the 37th–39th Congresses (1861–1866) accumulated, on average, nearly 40% more wealth between 1860 and 1870 than candidates who lost the election and did not serve. For the average congressman this corresponds to about $20,000 in additional wealth — nearly $800,000 in present-day values.

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

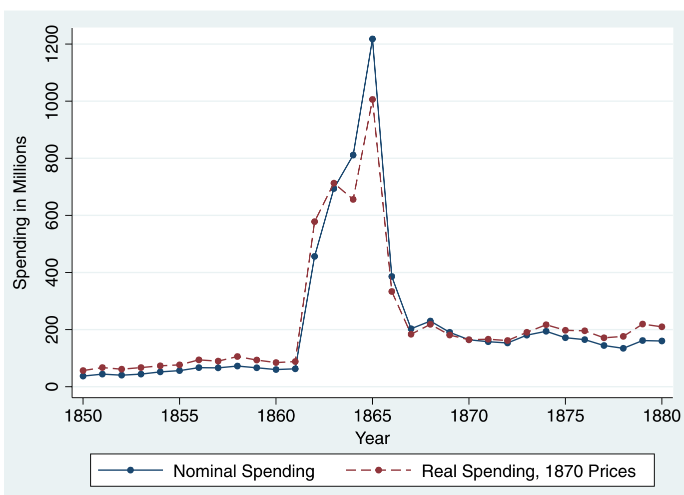

Figure 1A. Federal government spending before, during, and after the Civil War.

government spending during the Civil War years, from about $60 million just before the outbreak of the war to more than $1,200 million in 1865 — an increase of almost 2,000% in nominal terms and of more than 1,100% in real terms. This was driven by the need to mobilize, equip, feed, and move armies on a scale never before seen in U.S. history. The sudden spike in government spending might have made it easier for incumbent congressmen to collect rents. For example, they could channel contracts toward firms in which they had an interest, or collect side-payments or legal fees from contractors in exchange for favorable treatment. Procurement was especially frantic and disorganized during the first half of the war, as an army of almost 700,000 men was built essentially from scratch (see Wilson, 2006a). The potential decrease in *control* and oversight are illustrated in Figure 1B, which shows reporting on corruption by local newspapers during this period. Remarkably, reporting on corruption decreased during the Civil War years, and started increasing again in 1866 right after the end of the war. A plausible interpretation of this pattern is that the war battles and other political events during this period distracted the media from covering the dealings of politicians.

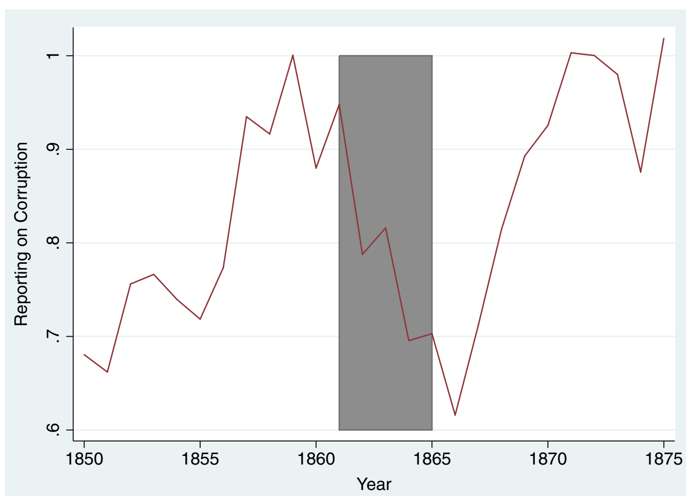

Figure 1B. Reporting on corruption, before, during and after the Civil War.

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

Something similar may have happened to voters and other state institutions who, focused on the immediate events of the war, may have decreased their oversight over the allocation of expenditure by congressmen and other politicians. This interaction of increased opportunities for self-enrichment and reduced control by the media and the voters, may explain the higher levels of rent seeking during the first half of the 1860s that we observe.

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

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

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

Our paper also contributes to a small but recently growing literature on estimating the value of political connections. Several papers have established the unusual economic returns of individuals and firms with political connections in countries with relatively weak political institutions (e.g., Fisman, 2001; Acemoglu *et al.*, 2008; Ferguson and Voth, 2008). Finally, in the context of a more strongly institutionalized democracy, Eggers and Hainmueller (2009) collect probate records of candidates to the British parliament in the period 1950–1970, and use an RDD to estimate the effect of holding a seat in parliament on wealth at death. They find significant positive effects for Conservative MPs but not for Labor MPs. Our finding that there did not appear to be significant returns to a seat in Congress in the United States for the non-war years stands in contrast to this literature, and suggests that American political institutions were relatively effective in controlling politicians during "normal times."

Other papers have explored the value of political connections in the United States in the current era.[5] These papers confirm the absence of returns from political connections in the United States during ‘‘normal times’’ that we report. Perhaps more closely related to our paper, Acemoglu *et al.* (2010) find that in the context of the recent financial crisis, financial firms connected to Timothy Geithner, experienced an abnormal return of 15% after his nomination as Treasury Secretary. This is consistent with the broader interpretation of our results, namely, the potential weakening of political institutions and an increase in the value of political connections during times of crisis.

2 Historical and Political Background

In the second half of the 19th century, several features of the United States might have served to check corruption and allow the state, the voters and the media to control politicians against the abuse of power. First, the political system introduced checks and balances to prevent the concentration of power. Political power was diffusely distributed, at least as measured by taxes and spending. Except during the Civil War, the federal government raised far less in taxes and spent far less than state and local governments overall. State governments probably also did more in terms of economic regulation. Even though comprehensive civil service reform had not been introduced by this time, federal statutes and local legislation prohibited the bribery of public officials. Voters also had mechanisms to keep politicians accountable. Throughout much of this period the political competition between the two major parties was fierce. And, while electoral fraud existed at some level, election outcomes were ‘‘fair’’ in the sense that both parties had a reasonable chance of winning a large share of the offices in most states. For example, in about half of the congressional races during our period of study, the winning candidate won with less than 55% of the vote. At least in theory, in such a competitive environment voters could have voted the most highly corrupt candidates out of office. Finally, the media was expanding substantially during this period. By 1860 there were approximately 2,000 local newspapers in circulation and by 1870 this number had almost doubled (see Rowell, 1869). This would have allowed the voters to learn about

[5] See, e.g., Groseclose and Milyo (1999), Diermeier *et al.* (2005), Fisman *et al.* (2006), Lenz and Lim (2009).

the dealings of politicians and learn about uncovered corruption scandals. For example, the bribery scandal of Tammany Hall's "Boss Tweed" was uncovered by newspapers in 1871.

Several scholars have written about corruption during this period. However, most accounts of political corruption focus on the *pre-war* years and the *post-war* years. A few scholars argue that corruption was rampant during the 1850s. Summers (1987, 14) writes: "In every way the decade before the Civil War was corrupt. The 1850s were as depraved as any other age, and, at least from the evidence available to historians, far more debauched than the 1840s." Writing about the events of 1857, Stampp (1990, 30) notes, "Corruption was not a new phenomenon in American politics... but corruption had become distressingly common in this period of accelerating commercialization and industrial growth."[6] For the post-war years, most historians probably consider the years of Ulysses S. Grant's presidency, from 1869 to 1877, to be the most corrupt in U.S. history. This period has been dubbed "the era of good stealings."[7]

Interestingly, less has been written about political corruption during the Civil War. This is perhaps not surprising given the overwhelming importance and scale of the war itself. Compared to the war — the massive mobilization and casualties, campaigns and battles, strategies and tactics, actions by military leaders, and so on — corruption by politicians was a relatively minor affair that did not receive much attention by the media or historians. A notable exception is Wilson (2006b, 45) for whom: "the apogee of the spoils system occurred during the Civil War, when party leaders took advantage of the war emergency to reward party supporters with reckless abandon.... The economic mobilization of the Union to defeat the Confederacy — by far the largest government spending project in the United States during the 19th Century — was nothing more than an outsized pork-barrel project for a party machine."

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

[7] The list of scandals includes Black Friday (Gold Panic), the Whiskey Ring, the Star Route Postal Ring, the New York Custom House Ring, the Trading Post Ring, the Delano Affair, and the Credit Mobilier scandal. For a revisionist view, see Summers (1993).

There is also anecdotal evidence on corruption during this period. The case of Simon Cameron, Lincoln's first Secretary of War who was dismissed in part for showing too much favoritism in awarding military contracts, is the most well-known. Also, some biographies describe cases where congressmen, senators, and other politicians profited from the war — e.g., Oakes Ames and his family obtained lucrative contracts to supply shovels, swords and other equipment to the Union army, and Thurlow Weed engaged in a variety of money-making schemes. But most of the literature, both by contemporary observers and historians, focuses on *private* war profiteering by businessmen, and the general problems of military contracting on a massive new scale.[8] This emphasis probably makes sense, since this is where the bulk of the wartime profits were.

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

3 Methodology

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

[8] See, e.g., Nagle (1999), Wilson (2006a), and Keeney (2007). For example, Keeney (2007, 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, 177) describes the wartime years as a time when suppliers charged exorbitant prices, sold shoddy blankets, uniforms, and boots; 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, 17) notes, "most of these regulations were only loosely enforced and soon of necessity went by the wayside."

on close elections in order to estimate the *causal* effect of serving in Congress on wealth accumulation.

3.1 Selection into Politics

The main problem underlying the estimation of the rents of a seat in Congress is the fact that congressmen are different from other citizens with respect to various characteristics that may be difficult to measure or observe. In many societies, basic literacy, age, and wealth restrictions that must be satisfied by anyone attempting to hold a political position already create a wedge between politicians and the remainder of the population. More generally however, the decision to become a politician is influenced by a series of personal characteristics that are plausibly correlated with other personal outcomes such as economic success. On the one hand, individuals more talented at accumulating wealth may find holding office more costly, since they must sacrifice high returns in the private sector. If this were true, then a simple comparison of wealth accumulated by politicians and non-politicians would tend to *underestimate* the rents from politics. On the other hand, if only the most talented individuals, who would have been very successful in the private sector anyway, manage to win elections and become politicians, then a naive comparison of politicians and non-politicians will tend to *overestimate* the rents from holding office. There is, in other words, selection into politics which makes it very hard to disentangle the effect of access to office from other personal characteristics. The direction and magnitude of the bias from a simple, naive comparison of politicians and non-politicians is not trivial to measure.[9]

3.2 Empirical Specifications

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

[9] See Besley and McLaren (1993), Caselli and Morelli (2004), and Mattozzi and Merlo (2008) for models with predictions about the selection of different types into politics.

other citizens in other ways that are correlated with the fact that they were significantly richer.

To estimate a causal effect of political office-holding on wealth accumulation, we employ a RDD. We must consider the following counterfactual: how much wealth would politician i have accumulated had he not been elected? Close elections, i.e., elections where the winner won by a very small margin, provide us with an empirical counterpart of the above counterfactual. If we believe that the outcome of close elections is as good as random, then we can assume that any differences in wealth accumulation between close winners and close losers can be attributed to holding the political office sought. In defining close elections, we focus on each candidate's *first* race for Congress in a given decade. The first election provides the cleanest quasi-experimental assignment to office.[10]

In our empirical approach we follow Hahn *et al.* (2001) and Imbens and Lemieux (2008), and estimate regressions of the form:

$$\text{Wealth}_i^t = \beta_0 + \beta_1 \text{Wealth}_i^{t-1} + \beta_2 \text{Winner}_i^t + \beta_3' \mathbf{X}_i + f(\text{Vote Share}_i) + \epsilon_i^t$$
$$\text{for all } i \text{ such that } |0.5 - \text{Vote Share}_i| < h \qquad (1)$$

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 his first race for Congress in the decade prior to census year t, and Wealth_i^{t-1} corresponds to the initial value of wealth in the preceding census year (10 years earlier). The vector X_i includes a battery of controls such as age, occupation dummies, and state fixed effects. The term $f(\cdot)$ corresponds to an nth order polynomial of the forcing variable, i.e., each candidate's vote share in their first race for Congress. We allow a different polynomial on either side of the 0.5 threshold. The choice of bandwidth h implies a sensitive trade-off. A small bandwidth more closely resembles the quasi-experimental assignment of close elections, but comes at the expense of efficiency due to small sample sizes. Large

[10] During the period we study, 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. 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. Another minor issue involves candidates who win in more than one decade. For example, a few candidates won their first race by a close margin in the 1850s, then continued serving into the 1860s. These candidates are included in the sample of close winners for both decades.

bandwidths are undesirable because they make use of observations far away from the threshold, posing a threat to the identification assumption.[11]

In our benchmark specifications, we follow Imbens and Lemieux (2008) and estimate local linear regressions using a 3% bandwidth. With this bandwidth we observe balance across covariates for winners and losers of close races. Moreover, this value of h is consistent with the optimal selection procedure proposed by Imbens and Kalyanaraman (2009).[12] Following previous work we also report the robustness of our results to alternative values of h and to the inclusion of higher order polynomials in the vote share.

The estimates based on Equation (1) correspond to the reduced form effect of winning the first race by a small margin on wealth accumulation (also known as the ‘‘intention to treat’’). However, some candidates who lose their first election run again and eventually serve in the decade. Also, some losing candidates successfully contest the outcome of their first election and eventually serve.[13] Finally, a few candidates do not serve in Congress despite winning their first election, due to deaths or voluntary resignations. That is, we have some ‘‘non-compliers.’’ This is not, however, a major concern in our context. Almost 85% of candidates who lose their first election by a narrow margin never serve in Congress. Similarly, 97.0% of those who win their first race actually serve in a specific decade.[14] An alternative approach to deal with the relatively small number of non-compliers is to use the outcome of

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[11] See, e.g. Angrist and Lavy (1999).

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

[13] While scholars such as Polsby (1968) note that there were a large number of contested elections during our period of study, it turns out that most of these were in slave states or territories and therefore not in our sample. Moreover, for a contested election to affect the RDD analysis, it must be a candidate's first race. This happens in only 22 cases in our 1850–1875 sample (1.0% of the total number of cases and 3.1% of the cases within the 3% bandwidth). In the case of contested elections, we use the first official vote count as certified by the relevant state election official as reported in Dubin (1998), not any of the subsequent vote counts as determined by the U.S. House. This is important because the crucial identifying assumption of the RDD — the “as if random assignment” in close races — is most clearly justified for electoral outcomes (rather than, say, decisions made later in the U.S. House in the case of contested elections). Thus our variable of interest Winner_i^t is not influenced by the outcome of contested elections. In any case, contested elections lead to only 2 cases of non-compliance in our sample.

[14] The few non-compliers are mainly due to cases where a candidate wins in, say, 1858 (1850s decade) and then runs again and loses in 1860 (1860s decade).

the first race, Winner_i^t, as an instrument for whether the candidate actually served in the decade, Served_i^t. For conciseness, we do not report instrumental variable (IV) estimates, but focus on the reduced form estimates. The patterns revealed by IV estimates are the same as those we report below (but, as expected, the estimated coefficients are slightly larger).

In all our analysis we report the regression results using wealth in levels and logs as our dependent variable. This allows us to capture different forms of rents from office. If rents correspond mostly to bribes or side-payments to politicians, then specifications using wealth in levels may be preferred. On the other hand, if rents correspond to returns on initial wealth — as might be the case, for example, of access to privileged information regarding investment opportunities — then the log functional form may be more appropriate. For regressions using wealth in levels as a dependent variable we estimate median regressions in order to reduce the influence of outliers on our estimates.

4 Data and Descriptive Statistics

We rely on several data sources for our analyses. Most are standard, including the sources for congressional election returns (for all elections between 1845 and 1875), biographical information, and committee assignments.[15] One novel source of data — novel at least in the study of congressional candidates — is the U.S. census. We use the censuses of 1850, 1860, 1870, and 1880 to collect key information about congressional candidates — wealth, number of servants, occupation, year and place of birth, and county and town of residence. The census reported real estate wealth in 1850, 1860, and 1870, and personal wealth (all assets other than real estate) in 1860 and 1870.[16] As an alternative measure of wealth, we also collected information on the number of domestic servants in the 1850, 1860, 1870, and 1880 censuses. Since the census wealth data is new, we provide a lengthy discussion of the data collection process, as well as an assessment of its reliability in Appendix B. Among the analyses in this appendix are comparisons of census wealth against wealth reported in other sources, and a study of a particular scandal in Wisconsin, where we show that politicians known to

[15] We use Dubin (1998) for election results, the *Congressional Biographical Directory*, and Canon *et al.* (1998) for committee assignments.

[16] These are the only years in which the Federal census collected information on wealth.

have accepted bribes accumulated substantially more census wealth than politicians who did not.

4.1 Descriptive Statistics

In this section, we present some basic descriptive statistics of wealth levels and changes during the period 1850 to 1880. Throughout the paper, we define real estate and total wealth in constant 1870 U.S. dollars.[17] To compare candidates in this period to other groups in the population, we use the Integrated Public Use Microdata Series (IPUMS) census samples for 1850, 1860, 1870, and 1880 collected by the Minnesota Population Center. These are representative 1% samples, and provide information on every variable collected in the relevant census. Thus, we can compare our sample of politicians to the population as a whole, and also to individuals with similar occupations.[18]

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

The first outstanding fact is that congressional candidates — especially those who actually served in Congress — were very rich men. Not surprisingly, the average and median wealth of congressional candidates exceeds substantially that of the mean and the median citizen in the IPUMS samples. Congressional candidates were in the 99th percentile of the overall wealth distribution. Somewhat more surprisingly, congressional candidates were rich even when compared to other "elite" groups. The simplest comparison involves lawyers. Law was by far the most common occupation of candidates

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[17] Although there was a considerable amount of inflation during the Civil War, there was deflation after the war, so the changes in price levels between decades are not too large. Based on the CPI, inflation was 8% between 1850 and 1860, 41% between 1860 and 1870, and −24% between 1870 and 1880. The CPI figures are from the website of the Federal Reserve Bank of Minneapolis.

[18] The IPUMS samples have been used by many economic historians, including Soltow (1975), Williamson and Lindert (1980), Atack and Bateman (1981), Steckel (1990), Shammas (1993), and Ferrie (1996).

Table 1A. Summary statistics on initial wealth.

	Real Estate Wealth		Total Wealth				Servants		
	1850		1860		1870		1850	1860	1870
	Mean	Median	Mean	Median	Mean	Median	Mean	Mean	Mean
All candidates	22171	6080	58076	22519	82261	25000	0.63	1.13	1.17
Winners	23747	6080	60551	23944	95597	25000	0.63	1.13	1.23
Losers	20719	6080	56110	21111	69552	25000	0.64	1.12	1.12
Democrats	18562	6080	60922	21111	65528	24000	0.60	1.11	1.19
Republicans	10034	3952	51171	21815	105250	29000	0.37	1.10	1.17
Whigs	27134	9120	—	—	—	—	0.67	—	—
Lawyers	19327	4560	39991	18940	54442	20000	0.58	1.05	1.08
Ipums All	1409	0	2565	281	2716	200	—	0.15	0.15
Ipums Lawyers	10217	0	11762	2111	10532	2000	—	0.46	0.49

in our sample — nearly half of all candidates were lawyers. On average, congressional candidates who were lawyers were more than three times as wealthy as the average lawyer in the country in 1860. The gap was even larger in 1870. Also, the gap is larger in relative terms in all decades if we compare medians. The median congressional winner was located in the top decile of the wealth distribution of lawyers nationwide. Thus, during this period congressional nominations were restricted to a rich elite. The average (median) wealth of a congressman during the 1860s was about $60,000 ($24,000) in 1870 dollars. To put these numbers in context and bring them to present-day values, we use a multiplier of 40.[19] This implies that the average wealth of congressmen during the 1860s was more than $2,000,000 in present-day values, and the median was over $900,000. 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-1800s were relatively rich compared to congressmen today.

A comparison of different types of candidates reveals that those who won and actually served tended to be slightly richer than candidates who ran for Congress but never served, at least with respect to average wealth. This suggests that some of the selection issues discussed in Section 3.1 may be relevant in our context, since initial wealth — plausibly correlated with traits important for economic and political success — is correlated with winning elections and serving in Congress. In terms of partisan differences, in the 1850s Democratic candidates were noticeably poorer than Whigs. In the 1860s Democrats had slightly higher average wealth than Republicans, but slightly lower median wealth. In the 1870s Republicans were substantially richer than Democrats.

Finally, Table 1B presents some descriptive statistics on *changes* in wealth and in the number of servants. The samples correspond to candidates who ran during the respective decades. Overall, the wealth of congressional candidates increased by over 80% between 1850 and 1860 and by about 50% between 1860 and 1870. These rates of wealth accumulation are slightly lower than the rates exhibited by synthetic cohorts constructed from the

[19] The average daily wage of a carpenter in 1870 was about $2.77. The median daily wage of a carpenter today in 2010 was about $152. This suggests a multiplier of about 54.7 to put 1870 dollars in today's dollars. If we use the CPI then the multiplier is about 18.4. The correct multiplier probably lies somewhere between these two. We use 40 as a rough guess, leaning slightly toward the multiplier based on wages since our study deals with wealth and income rather than consumption.

Table 1B. Summary statistics on changes in wealth.

	Δ Log Real Estate 1850–1860	Δ Servants 1850–1860	Δ Total 1860–1870	Δ Log Total 1860–1870	Δ Servants 1860–1870	Δ Servants 1870–1880
All candidates	0.78	0.54	7333	0.47	0.24	0.00
Winners	0.85	0.56	8926	0.56	0.36	0.01
Losers	0.71	0.53	5778	0.40	0.15	−0.01
Democrats	0.89	0.57	5345	0.38	0.20	−0.03
Republicans	1.05	0.32	10716	0.61	0.32	0.03
Whigs	0.58	0.57	—	—	—	—
Lawyers	0.89	0.53	5778	0.43	0.21	0.04
Ipums All	0.93			0.63	0.00	0.01
Ipums Lawyers	0.84			0.38	0.04	0.05

In Table 1B all figures are means except those in the Δ *Total 1860–1870* column, which are medians.

IPUMS samples.[20] Of course, given the much larger initial wealth of congressional candidates, the same percentage change corresponds to a much larger increase in absolute terms.

In addition, there is some suggestive evidence that those who won congressional races became richer than those who lost — particularly during the 1860s. Column 4 reveals that on average, those who won during the 1860s became about 15 percentage points richer than losers between 1860 and 1870. The figures for servants in column 5 exhibit a similar pattern. In the 1850s, winners did somewhat better than losers in terms of wealth accumulation, but about equally well in terms of servants. Figures for the 1870–1880 period in the last column do not suggest any abnormal wealth accumulation by winners relative to losers. Overall, however, while the differences between winners and losers are all positive, they are hardly overwhelming. This is already suggestive of the effectiveness of political institutions during this period in preventing systematic rent-seeking by politicians. As we show below however, some of these averages mask a large amount of heterogeneity.

There is also a suggestive partisan pattern. During the 1850s, when Democrats controlled congress, Democratic candidates (both winners and losers) accumulated about 30 percentage points more wealth than Whigs. This pattern is reversed in the 1860s. During this decade Republicans took control of congress, and their candidates accumulated about 20 percentage points more wealth than Democrats. This suggests that candidates from the majority party became richer than candidates from the minority party. For the 1860s, the median increase in total wealth for winners from the majority party was $10,300, while the increase for winners from the minority party was $6,500. For majority party losers, the median increase was $10,000, and for minority party losers the increase was $5,300. For the 1850s, the corresponding figures (for changes in real estate wealth) are $10,000, $8,300, $8,000, and $5,500, respectively. Thus, in both decades majority party winners did best, while minority party losers did worst. Comparisons of minority party winners and majority party losers show a mixed pattern — in the 1860s the former did better, while in the 1850s the latter did slightly better. In any case, as shown below, we do not find robust differences between the majority and minority party candidates in the RDD analysis. Moreover, it is not clear whether any overall differences are caused by majority party

[20] The synthetic cohorts for lawyers in the IPUMS samples are small, so the last row in Table 1B must be treated with caution.

status or by other factors correlated with this status (e.g., the majority party can attract more talented or ambitious politicians). We leave a thorough investigation of partisan effects for future work.

5 Results

In this section, we present the main results of the paper. First, for reference, we present standard OLS estimates that illustrate average differences in wealth accumulation by congressmen and those who ran but never served. Next we present the RDD estimates for the different decades and some evidence on the underlying mechanisms that may explain the patterns in the data.

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

In addition, all regressions include the following set of controls: initial wealth (for regressions with wealth as dependent variable), initial number of servants (for regressions with number of servants as a dependent variable), age, age^2, occupation dummies for lawyer, farmer and manufacturer/merchant/banker, and a full set of state fixed effects.[21]

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

5.1 OLS Estimates

In Table 2 we present descriptive OLS estimates of Equation (1) on the full sample of candidates, without the control function $f(\cdot)$. Also, for this

Table 2. OLS estimates of wealth vs. serving in Congress.

	(1) Ending Wealth	(2) Ending Log Wealth	(3) Ending Servants
Panel A: 1850–1860			
Served 1850s	4268	0.186	0.017
	(1426)	(0.074)	(0.078)
Obs.	756	652	794
Panel B: 1860–1870			
Served 1860s	5869	0.223	0.236
	(2504)	(0.072)	(0.072)
Obs.	775	741	905
Panel C: 1860–1870, Civil War vs. Non-War			
Served Civil War	9039	0.192	0.421
	(2482)	(0.081)	(0.083)
Served Non-War	2781	0.145	−0.065
	(2431)	(0.080)	(0.081)
p-value of F-test	0.109	0.714	0.000
Obs.	775	741	905
Panel D: 1870–1880			
Served 1870s			0.036
	—	—	(0.084)
Obs.			652

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

exercise we use Served_i^t — a dummy that takes a value of one for those who served at any point during the corresponding decade irrespective of whether they won their first race — rather than Winner_i^t. We present estimates of β_2 for our three dependent variables *Ending Wealth*, *Ending Log Wealth*, and *Ending Servants* in the first, second, and third columns respectively. Estimates in the first column correspond to median regressions, while those in columns 2 and 3 correspond to OLS estimates.

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

The patterns revealed by the OLS estimates anticipate the main results of the paper. However, these estimates cannot be given any causal interpretation, since they confound the effect of serving in Congress with any other characteristics of the candidates correlated with winning elections and economic success (wealth accumulation). In order to isolate the effect of serving in Congress and estimate the rents from office we must turn to the RDD discussed in Section 3.

5.2 Basic RDD Estimates

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

5.2.1 RDD preliminaries

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

Table 3 presents differences in various covariates observed in the census records across candidates in the ‘‘close election’’ sample. We present differences for the different decades. Reassuringly, we find no systematic evidence of any major difference across winners and losers in any of the covariates in our main samples. Differences in initial wealth and the initial number of servants across winners and losers are small and statistically insignificant. This is perhaps the most important piece of evidence, since one potential concern is that richer candidates might be able to influence the outcome of elections — even close elections — in their favor. There is also no evidence of systematic and statistically significant differences in age or occupation groups across winners and losers.[22,23]

5.2.2 RDD regressions

Table 4 presents our main RDD results for the 1850s (panel A), 1860s (panels B and C), and 1870s (panel D). In all cases we include a local linear control function and use a 3% bandwidth. The first column — for *Ending Wealth* — reports estimates based on median regressions while regressions in columns 2 and 3 are estimated by OLS.

Consider the estimates for the 1850–1860 period. The estimates are *negative* and statistically insignificant for all dependent variables. For the 1870–1880 period the point estimate is negative — but small — and statistically insignificant. These are surprising results. They indicate that, overall,

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[22] There are statistically significant differences for two of the occupation dummies in some periods — farmers and lawyers — but the differences go in different directions in different periods. In any case, we control for these variables in all of the regressions.

[23] Recent papers by Snyder (2005), Carpenter *et al.* (2011), and Caughey and Sekhon (2012) criticize RDD studies that rely on close elections, arguing that there is “sorting” even very near the 50% threshold. We show that these critiques are not relevant for our period in Appendix B.

Table 3. Balance on covariates in RDD samples (3% margin).

	Winner Mean	Loser Mean	Difference	p-Value
1850–1860				
Log Initial Real Estate Wealth	9.16	9.20	−0.04	0.83
Initial Servants	0.63	0.66	−0.03	0.79
Age	51.00	50.43	0.57	0.60
Lawyer Dummy	0.72	0.64	0.08	0.16
Manuf/Merch/Banker	0.15	0.23	−0.08	0.10
Farmer Dummy	0.22	0.19	0.02	0.61
1860–1870				
Log Initial Total Wealth	10.19	10.10	0.09	0.60
Initial Servants	1.11	1.22	−0.11	0.43
Age	41.31	41.44	−0.13	0.90
Lawyer Dummy	0.67	0.68	−0.01	0.81
Manuf/Merch/Banker	0.19	0.24	−0.04	0.37
Farmer Dummy	0.20	0.11	0.09	0.03
1860–1870, Civil War Years				
Log Initial Total Wealth	10.33	10.26	0.07	0.73
Initial Servants	1.21	1.45	−0.24	0.23
Age	42.73	42.48	0.25	0.86
Lawyer Dummy	0.66	0.69	−0.03	0.65
Manuf/Merch/Banker	0.20	0.20	0.00	0.98
Farmer Dummy	0.24	0.12	0.11	0.06
1860–1870, Non-War Years				
Log Initial Total Wealth	10.06	9.95	0.11	0.60
Initial Servants	1.09	1.02	0.07	0.66
Age	40.40	40.54	−0.14	0.91
Lawyer Dummy	0.75	0.68	0.07	0.27
Manuf/Merch/Banker	0.15	0.23	−0.09	0.12
Farmer Dummy	0.15	0.09	0.06	0.20
1870–1880				
Initial Servants	1.14	1.07	0.07	0.67
Age	34.85	36.21	−1.36	0.23
Lawyer Dummy	0.66	0.52	0.14	0.02
Manuf/Merch/Banker	0.22	0.28	−0.07	0.23
Farmer Dummy	0.12	0.27	−0.16	0.00

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

Table 4. RDD estimates of wealth vs. serving in Congress.

	(1) Ending Wealth	(2) Ending Log Wealth	(3) Ending Servants
Panel A: 1850–1860			
Winner 1850s	−8373	−0.402	−0.389
	(9146)	(0.262)	(0.261)
Obs.	248	220	249
Panel B: 1860–1870			
Winner 1860s	92	0.036	0.528
	(8065)	(0.242)	(0.249)
Obs.	242	233	286
Panel C: 1860–1870, Civil War vs. Non-War			
Winner Civil War	17883	0.358	0.575
	(5475)	(0.175)	(0.176)
Winner Non-War	−989	−0.009	−0.177
	(5316)	(0.167)	(0.175)
p-value of F-test	0.004	0.086	0.001
Obs.	242	233	286
Panel D: 1870–1880			
Winner 1870s			−0.018
	—	—	(0.275)
Obs.			236
Panel E: 1850–1860, Civil War vs. Non-War, Placebo Regressions			
Winner Civil War	1836	−0.080	0.019
	(7341)	(0.314)	(0.255)
Winner Non-War	657	0.152	0.622
	(7575)	(0.317)	(0.257)
p-value of F-test	0.885	0.506	0.040
Obs.	156	118	188

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

congressmen during this period were not able to accumulate wealth at an abnormal rate relative to those who ran but lost by a small margin. A plausible interpretation is that democratic political institutions during these two decades were effective at preventing systematic and widespread rent-seeking by congressmen. Of course, the results for the 1870s in panel D are based only on servants, and the standard error is relatively large. We should be especially cautious, because the point estimate is inconsistent with the widespread view that the 1870s were a highly corrupt era in U.S. politics. On the other hand, the solid historical evidence underlying this view consists of a relatively small number of well-known cases, and these cases do not involve large numbers of congressmen. Thus, our findings for the 1870s suggest the need for further empirical studies to assess how widespread and systematic corruption actually was during this period. Since the point estimates for the 1850s are negative and fairly large, we are more confident arguing that there is little evidence of systematic wealth accumulation by congressmen in the 1850s. Note also that the estimates for these decades stand in contrast with evidence for other countries and different periods where the value of political connections has been found to be large.

In panel B, we report RDD results for the 1860–1870 period. Evidence from these estimates is inconclusive — the coefficients in columns 1 and 2 are positive but small and statistically insignificant, while the coefficient in column 3 is relatively large and statistically significant. To explore the variation in the political and economic environment during this decade, in panel C we report the results for specifications with separate coefficients for the Civil War years and non-war years.[24] At the bottom of the panel we report the p-value of an F-test for the null hypothesis that the coefficients for the Civil War and non-war samples are equal. The estimates provide evidence of large effects for Civil War winners, but not for non-war winners. The point estimates for *Ending Log Wealth* in column 2 suggests that Civil War winners accumulated almost 40% more wealth than those who ran and

[24] More specifically, *Winner Civil War* is a dummy variable equal to one if candidate i won his first race for Congress and ran during the 37th, 38th, or 39th Congress, while Winner Non-War is a dummy variable equal to one if candidate i won his first race for Congress and ran during the 36th, 40th, or 41st Congress. We do not report results from a fully saturated regression where we allow all controls, the forcing variable, and the state fixed effects to vary across the Civil War and non-war sample. Estimating a fully saturated model does not affect the point estimates noticeably, but naturally causes the standard errors to increase 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.

lost. Using average wealth values in 1860, the estimated coefficient implies that Civil War winners accumulated an additional \$21,000 dollars (roughly \$850,000 in current values) relative to losers. This is similar to the point estimate in column 1 produced by the median regression using *Ending Wealth* as the dependent variable, which indicates that Civil War winners accumulated an additional \$18,000 (roughly \$720,000 in current values) relative to losers. The estimate for *Ending Servants* confirms the same pattern, indicating that Civil War winners accumulated almost 0.6 more servants than losers. This is comforting since the number of servants, although a coarse measure of wealth, was harder to misreport to census enumerators.[25]

By contrast, the estimates for non-war winners are always small and statistically insignificant (in fact, the point estimates are negative). The F-tests in columns 1 and 3 always reject the null hypothesis that coefficients for Civil War and non-war years are the same at the 0.05 level. The F-test for *Ending Log Wealth* also rejects this hypothesis but only at the 0.10 level.[26]

Figure 2 illustrates these results graphically. This figure shows RDD plots for two variables: change in wealth (top row) and change in servants (bottom row). We compute binned averages based on vote share of the first election, and plot these averages against the vote share. We also plot locally-linear regression curves, which are allowed to differ on each side of the 50% threshold, together with 95% confidence intervals.[27] We show plots separately for the Civil War and non-war years. The plots confirm the same patterns of Table 4; for both dependent variables there is a sizable and statistically significant jump at the threshold for Civil War years (first column), but not for the non-war years (second column).

In panel E of Table 4 we report estimates of "placebo regressions," where we examine wealth accumulation between 1850 and 1860 for candidates who

[25] Congressional salaries are unlikely to explain these differences, since congressmen had to pay for a second residence in Washington D.C. while serving. According to contemporary reports, this consumed a large portion of their congressional salary.

[26] Our estimates do not simply capture a more general "early vs. late half of the decade" effect. The point estimate for those who served in the early half of the 1850s is not statistically different from the estimate for those who served in the late half of the decade. The before-and-after analysis, presented in Appendix Table C.1 provides further evidence of this.

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

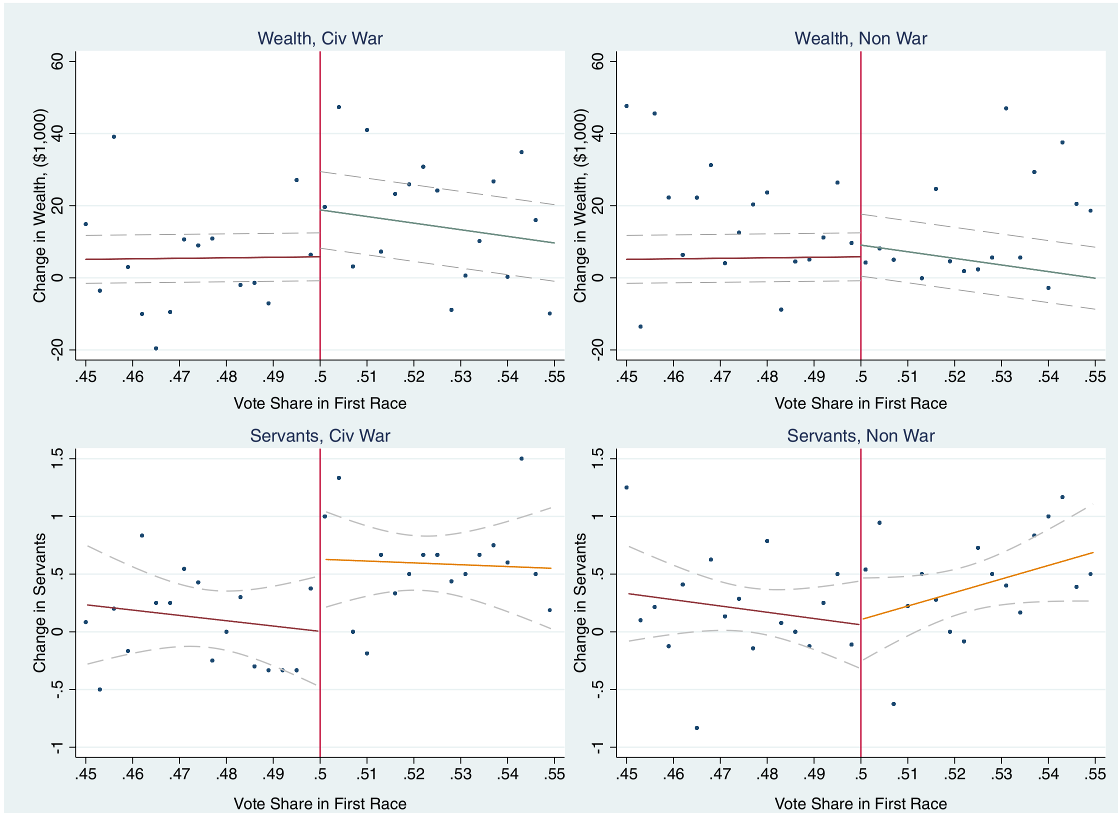

Figure 2. RDD plots for change in wealth and change in servants.

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

Table 5. Contracting vs. other states.

	(1) Ending Wealth	(2) Ending Log Wealth	(3) Ending Servants
Panel A: RDD Estimates, 1860–1870			
Civil War, Contract State	31162	0.401	0.628
	(5397)	(0.194)	(0.195)
Civil War, Other State	5890	0.244	0.434
	(7346)	(0.278)	(0.288)
Non-War, Contract State	−975	−0.014	−0.202
	(5062)	(0.183)	(0.190)
Non-War, Other State	−2757	−0.008	−0.107
	(7396)	(0.302)	(0.304)
p-value of F-test 1	0.002	0.598	0.531
p-value of F-test 2	0.000	0.098	0.002
p-value of F-test 3	0.000	0.210	0.025
Obs.	242	233	286
Panel B: Placebo Regressions, 1850–1860			
Civil War, Contract State	−1125	−0.325	−0.036
	(10455)	(0.326)	(0.277)
Civil War, Other State	2448	0.832	0.119
	(14788)	(0.502)	(0.393)
Non-War, Contract State	5536	0.267	0.742
	(10354)	(0.330)	(0.280)
Non-War, Other State	−694	−0.278	0.261
	(16278)	(0.593)	(0.429)
p-value of F-test 1	0.808	0.023	0.701
p-value of F-test 2	0.612	0.125	0.023
p-value of F-test 3	0.979	0.938	0.509
Obs.	156	118	188

Independent variables defined as follows: Civil War, Contract State = 1 for those who won their first race in the 1860s, ran during the Civil War, and were from contracting states, etc. RDD Estimates — Local Linear Regressions with 3% Bandwidth. Median regression estimates for *Ending Wealth* dependent variable (column 1). OLS estimates for *Ending Log Wealth* and *Ending Servants* dependent variables (columns 2–3). The p-values are for F-tests of the hypothesis that the β for Civil War, Contract State Winners is equal to: (1) β for Civil War, Other State Winners, (2) β for Non-War, Contract State Winners, and (3) β for Non-War, Other State Winners.

ran during the 1860s. If the estimated coefficients in panel C are actually due to service in congress, rather than unobserved characteristics of the winners, then serving in Congress during the 1860s should have no effect on wealth accumulation in the previous decade. Reassuringly, the placebo estimates for Civil War congressmen are small and statistically insignificant. Estimates for non-war congressmen are also small and insignificant with the exception of the estimate in column 3, which is large and statistically significant. These estimates indicate that our estimates in panel C do not simply reflect unobserved characteristics, or pre-existing trends in wealth accumulation across close winners and close losers.

Finally, we conducted a series of robustness checks. In the interest of space we report these in Appendix Table A.1. We first consider 2% and 3% bandwidths around the threshold to define close elections, and drop the control function $f(\cdot)$. We also report robustness checks where we focus on a larger sample by using a bandwidth of 5% in the vote share of the first congressional race, with a local-linear control function. The estimates in Table A.1 generally exhibit the same patterns as those in Table 4. There is no evidence of a statistically significant effect of winning a congressional seat during the 1850s, the 1870s or during the non-war years in the 1860s. Also, in almost all cases the estimates imply significant rents for Civil War winners. For the 1860s, the results for *Ending Wealth* and *Ending Servants* are quite robust to alternative specifications. The estimates for *Ending Log Wealth* remain essentially the same in the smaller 2% bandwidth, but the Civil War vs. non-war differences disappear when we use the wider 5% bandwidth.[28]

Another robustness check addresses the concern that RDD estimates only provide *local* average treatment effects for observations or individuals around the given threshold, and may therefore be uninformative regarding the effect of a given treatment on observations further away from the discontinuity. This is often referred to as the lack of external validity of regression discontinuity estimates. We cannot directly assess how informative the local treatment RDD estimates are regarding the broader set of candidates. As an alternative, in Appendix C we report evidence based on a different empirical strategy — a simple "before-and-after" design — first introduced in

[28] Ideally, we would also like to compare the winner against the loser in each close race, on a race by race basis — equivalently, we would include race-specific fixed-effects. Unfortunately, we cannot do this due to sample size. The samples are too small because each observation requires us to measure the wealth for both candidates in the race in two adjacent censuses, and we often are missing one of the four numbers required.

Querubin and Snyder (2009). This approach does not rely on the comparison of winners and losers in close elections, but relies solely on data for individuals who actually won and served. The point estimates from this analysis reveal a similar pattern as that for the RDD estimates reported in Table 4. More specifically, the only statistically significant coefficients are for those who served in the first half of the 1860s — the Civil War years. These congressmen accumulated more wealth (and servants) than the congressmen who served in the second half of the 1850s. Thus, the before-and-after analysis, which relies on a completely different source of variation, confirms our main findings and also gives us further confidence on the relevance of the RDD estimates.

5.3 Exploring Possible Mechanisms

What can explain our evidence of rent seeking by Civil War congressmen? A first element of central importance in our interpretation is the size of federal government spending. Figure 1A illustrates the evolution of nominal government spending by the federal government between 1850 and 1880. There was a dramatic increase in nominal government spending during the Civil War years from about $60 million just before the outbreak of the war, to almost $1,200 million at the highest point during the war, followed by an equally sharp fall in spending after the war ended. This was driven by the need to mobilize, equip, feed, and move armies on a scale never before seen in U.S. history. The sudden spike in government spending might have made it easier for incumbent congressmen (and other politicians) to collect rents as there was a larger pool of resources on which to prey. Similarly, rent extraction would have been more difficult to detect during the Civil War than during the 1850s or 1870s because the same dollar amount of rents represented a much smaller fraction of total government spending. Using the point estimates from Table 4, the total amount of rents extracted by all incumbent congressmen serving during the Civil War would have represented only about 0.2% of total federal wartime spending. However, this would have represented more than 2% of the average level of pre-war spending, and nearly 1% of post-war spending. Thus, rent extraction comparable in scale to what we estimate for the Civil War years would have been much easier to detect during "normal" times.

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

Other factors might also have played a role. First, after the southern states seceded the total number of seats in the House of Representatives fell by about 22%, so the rent-seeking ‘‘value’’ of each seat may have risen. Second, the share of Democratic representatives in the House also fell substantially, and this may have weakened the internal checks provided by a strong minority party. It is unlikely that the second factor accounts for the differences between the Civil War and non-war years, since the Republicans commanded even more overwhelming majorities in the House during the second half of the 1860s.[29] Another possibility is that those ‘‘loyal to the union,’’ especially Republicans, could justify rent-seeking during the Civil War using arguments related to nationalism — they were defending the country and deserved to be rewarded. As noted below, however, we do not find significant differences between Republican and Democratic Civil War winners.

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

[29] When the southern states were re-admitted during Reconstruction, they elected solid Republican delegations — the Democratic “solid south” did not emerge until after 1870.

5.3.1 Military contracting states

First, we consider military contracts. Wilson (2006a) provides lists 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, Massachusetts, Maine, New Jersey, New York, Ohio, Pennsylvania, Rhode Island, and Vermont.[30] It is likely that local knowledge and connections would have given congressmen from these states a relative advantage in benefiting from these contracts — e.g., as lawyers representing the contracting firms, or as partners in the firms themselves.[31] Using this list, we define a dummy variable that is equal to one for the states with large military contracts, and zero for the remaining states. We then re-estimate the regressions reported in panel C of Table 4, but add terms that interact this dummy variable with the variables indicating which candidates won during the Civil War and non-war years. An important caveat is that large contracting states differ from non-contracting states along other dimensions other than the magnitude of military contracts during the war. Large contracting states are richer, older, and more eastern, so our interaction terms may capture other characteristics of these states.

Panel A of Table 5 presents the results of this analysis. The estimates provide robust evidence of large returns for Civil War winners from large contracting states. The point estimates for congressional winners from these states — reported in the first row — are the largest and are always statistically significant. The point estimate in column 1 for *Ending Wealth* is particularly striking. It indicates that Civil War winners from large contracting states accumulated about \$31,000 more total wealth than those who ran but did not win (approximately \$1,200,000 in present-day values). Also, we can reject the hypothesis that the coefficient for congressmen from contracting states during the Civil War is equal to that for congressmen from other states during the war. The estimates in columns 2 and 3 using *Ending Log Wealth* and *Ending Servants* as dependent variables exhibit similar overall patterns. However, the estimates for congressmen who represented other states during the Civil War, though not statistically significant, are rather large in columns 2 and 3. In these specifications we cannot reject the

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[30] The lists in Wilson (2006a) also include Delaware, Maryland, and Missouri, but these are not in our analyses because they were slave states.

[31] Much investing in the 1860s was "local" since stock markets were still developing — e.g., almost all of the firms listed on the NYSE were railroads, banks, and coal companies.

null hypothesis that the coefficients for Civil War winners from contracting states and other states are equal.

On the other hand, the estimates in the third row reveal that in the non-war years — when federal military spending was many times smaller — winners from large contracting states did not become abnormally richer than losers. The estimates are small, negative, and statistically insignificant for all three dependent variables. The patterns are similar for non-war winners from non-contracting states during the non-war years (see the fourth row of panel A). Note also that we cannot reject the hypothesis that the coefficients for winners from contracting and non-contracting states were equal during the non-war years. On the other hand, in columns 1 and 3 we can always reject the hypothesis that the coefficients for Civil War winners from large contracting states are equal to the coefficients for the non-war winners either from contracting states or other states. This suggests that congressmen from large contracting states were able to accumulate wealth at an abnormally high rate relative to losers only during the years of extraordinarily high military spending.

In Panel B of Table 5 we report placebo regressions, where we examine wealth accumulation between 1850 and 1860 for different types of candidates in the 1860s. As in the placebo regressions reported in panel E of Table 4, if the estimated coefficients in panel A of Table 5 are actually due to service in congress, rather than unobserved characteristics of winners, then serving in Congress during the 1860s — whether from large contracting states or other states — should have no effect on wealth accumulation in the previous decade. The estimates in the first row, for Civil War winners from contracting states, are small and are statistically insignificant. This is reassuring, because it suggests that the evidence of large rents in the first row of Panel A does not simply reflect unobserved characteristics.

An additional concern has to do with the fact that the results in panel A may reflect the overall advantage that individuals representing large contracting states enjoy in extracting rents in any period other than the 1860s. To address this possibility, in Appendix Table A.4 we explore whether individuals who represented large contracting states during the 1850s or during the 1870s accumulated more wealth relative to those who ran and never served or those who represented other states during this period. Once again, the results are reassuring. The estimates in panels A and B are mostly negative and always statistically insignificant. Most importantly, there is no evidence that winners from contracting states accumulated significantly more wealth than winners from other states in other decades.

Finally, in Appendix Table A.2 we report robustness checks for the regressions in panel A of Table 5. The overall patterns are essentially the same when we use a 2% or 3% bandwidth and drop the control function, or when we use a wider 5% bandwidth and a local-linear control function.

5.3.2 Military spending committees

Next, we study the role of committee assignments. We use the *Daily Journal* of the House of Representatives as well as the *Congressional Globe* to identify the committees most often cited as responsible for large bills on military appropriations during the 37th and 38th Congresses during the Civil War.[32] These committees are the 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 6, we explore whether congressmen who served on these key committees accumulated more wealth between 1860 and 1870 than congressmen who served on other committees, as well as candidates who ran and never served. The relevant variation of interest in this exercise is committee assignment. Since the RDD only provides random assignment of access to office, and not to specific committees, the estimates in Table 6 are *not* RDD estimates. Instead, the identification of committee effects are based on a difference-in-difference design. For consistency with the analyses in previous tables, however, we continue to restrict attention to the RDD 3% sample.

The estimates in panel A provide evidence of especially large returns for congressmen who served on the important military spending committees during the Civil War. The estimates in the first row are large and statistically significant across the different dependent variables. The estimate in column 1 implies that congressmen from these committees accumulated an additional \$51,000 (approximately \$2,000,000 in current values) relative to those who

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[32] 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.

Table 6. Military committees vs. other committees.

	(1)	(2)	(3)
	Ending Wealth	Ending Log Wealth	Ending Servants
Panel A: Difference-in-Difference Estimates, 1860–1870			
Civil War, Military Comm	51386	0.710	0.812
	(7348)	(0.247)	(0.251)
Civil War, Other Comm	12139	0.358	0.506
	(4268)	(0.147)	(0.150)
Non-War, Military Comm	338	0.271	−0.332
	(5233)	(0.188)	(0.203)
Non-War, Other Comm	−523	0.071	−0.063
	(4408)	(0.153)	(0.156)
Diff-in-Diff Estimate	38386	0.151	0.575
p-value	0.000	0.675	0.129
Obs.	242	233	286
Panel B: Placebo Difference-in-Differences, 1850–1860			
Civil War, Military Comm	−1222	−0.163	0.028
	(3729)	(0.321)	(0.317)
Civil War, Other Comm	1319	0.117	−0.200
	(2447)	(0.230)	(0.200)
Non-War, Military Comm	2018	0.237	0.421
	(3149)	(0.328)	(0.301)
Non-War, Other Comm	−2856	−0.254	0.263
	(2683)	(0.291)	(0.240)
Diff-in-Diff Estimate	−7414	−0.772	0.070
p-value	0.200	0.178	0.891
Obs.	156	118	188

Independent variables defined as follows: Civil War, Military Comm = 1 for those who served during the Civil War, and served on committees with military-related jurisdictions. Difference-in-Difference = (Civil War, Military Comm — Civil War, Other Comm)–(Non-War, Military Comm — Non-War, Other Comm). Median regression estimates for *Ending Wealth* dependent variable (column 1). OLS estimates for *Ending Log Wealth* and *Ending Servants* dependent variables (columns 2–3). The p-values are for F-tests of the hypothesis that the difference-in-difference estimates are equal to zero.

ran but did not serve. The estimates for congressmen from other committees who served during the Civil War are also positive and statistically significant, but are smaller than those for congressmen on the top military committees. The difference-in-difference estimates reported at the bottom of panel A are positive for all dependent variables, although only statistically significant in column 1. This provides suggestive evidence that assignment to these key committees was particularly profitable during the Civil War years, when military spending was abnormally high.

In panel B of Table 6 we report placebo regressions that examine wealth accumulation between 1850 and 1860, *prior* to serving in Congress, by those who served in military spending committees and other committees between 1860 and 1870. The estimates in panel B are small and always statistically insignificant. Moreover the placebo difference-in-difference estimates reported at the bottom of panel B are sometimes negative, and never statistically significant.

Appendix Table A.4 provides an additional validity check. There, we examine wealth accumulation by those who served on top military spending committees and other committees during ‘‘normal times’’ in the 1850s (panel C) and 1870s (panel D), when military spending was not at unusually high levels. The estimates in panel C suggest that those who served in military spending committees during the 1850s may have accumulated more wealth between 1850 and 1860 than those who ran but never served, although this result is not robust across the different dependent variables (estimates in column 3 are small and statistically insignificant). The estimates in panel D are close to zero and statistically insignificant. Most importantly, we cannot reject the hypothesis that the estimates for those who served in military spending committees and other committees in the 1850s and 1870s are equal to each other. Thus, the estimates in panel A of Table 6 do not appear to capture an overall advantage in rent-seeking for those serving in military committees relative to those in other committees. Assignment to military spending committees was only clearly profitable during the war.

As noted above, the difference-in-difference estimates in Table 6 are based on the RDD sample. As a further robustness check, in Appendix Table A.3 we report estimates of the same specifications as those in Table 6, but using the full sample of candidates (rather than only the 3% RDD sample). The patterns are similar to those in Table 6 — in particular, the estimates for congressmen who served on important military committees during the Civil

War are always the largest — although the difference-in-difference estimates are smaller.

Overall, the evidence in Tables 4–6 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 congressmen, and noticeably more than the individuals who ran for Congress but lost. These results highlight the importance of increased *opportunities* associated to the spike in government spending and contracting during the Civil War. Increased spending and opportunities for self-enrichment may need to interact with lower control and oversight by state institutions, the voters and the media to generate higher levels of rent-seeking. We do not yet have data that allows us to disentangle the role of increased *opportunities* from the effect of reduced *control*, but this is clearly an interesting avenue for future research.

Finally, in Appendix A we explore other possible interpretations of the results reported in Tables 4–6. In Appendix Table A.5 we investigate the hypothesis that members of the majority party — who control the main levers of power — could enrich themselves more easily than those in the minority. In Appendix Table A.6 we study wealth accumulation after leaving office — i.e., whether those who served in Congress accumulated more wealth than those who ran but lost in the decade *after* they were out of office. We find no significant evidence for either of these hypotheses.

6 Conclusions

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

However, we report evidence of substantial returns to a seat in Congress between 1861 and 1866 when federal government spending expanded sharply to unprecedented levels in order to fund the war. Our point estimates suggest that congressmen who served during the Civil War accumulated almost 40% more wealth between 1860 and 1870 (nearly $800,000 in present-day values) than those who ran but never served. Note also that our estimates probably represent a lower bound on the overall rents from officeholding in the United States during this period, since many of the losing congressional candidates went on to serve in other offices — e.g., in state legislatures, local offices, and in a few cases higher state offices including governor.

We hypothesize that the dramatic increase in government spending may have made it easier for incumbent congressmen to accumulate rents due to a surge in *opportunities* associated with, say, contracts for military supplies. In addition, the attention of government institutions during this period was probably focused on the affairs of the war, reducing the oversight and auditing 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 smaller fraction of government spending than at other times. Similarly, politicians during this period may have been less accountable to the voters and the media who were also focused on the political and military events of the war, rather than on the dealings of politicians and the allocation of federal expenditure. We also show that wealth accumulation was particularly significant by congressmen who represented states that played an important role providing supplies during the war, and by congressmen who served in top military committees.

Conceptually, rent-seeking during the Civil War may have been triggered by increased opportunities, reduced control and/or the interaction of these two forces. The results reported in the paper emphasize the important role of increased government spending and rent seeking opportunities, but we do not have direct evidence on the role of decreased control and oversight by the media and the voters. Moreover, we only have one case of extraordinarily high spending in our study. Thus, we cannot be confident that the increased wealth accumulation by congressmen was due only to the increase in spending.

We are currently gathering additional data to disentangle the relative importance of these two forces. An alternative is to gather data on media coverage in different states during this period and establish whether rent-seeking was more systematic by representatives from states where coverage of corruption decreased more during the Civil War. In addition, we can identify episodes of spending increases in particular states that were not associated to any particular event that may have reduced the accountability and control of politicians. Evidence of rent-seeking during these episodes will allow us to assess the relative importance of decreased control relative to increased opportunities for enrichment. We have found the census records of many state and local politicians such as governors, state senators, state legislators and city mayors. We are also gathering data on spending by state governments during this period. This will allow us to analyze wealth accumulation by state politicians during other episodes of increased spending. Studying bureaucrats — e.g., quartermasters and others directly handling or paying for military supplies and contracts — would also be interesting, and this could be done using the before-and-after analysis applied in Querubin and Snyder (2009).

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

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