

# The Returns to U.S. Congressional Seats in the Mid-19th Century<sup>1</sup>

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## **Abstract**

In many political economy models, endogenous “rents” from office can cause large distortions in policy and even reduce economic growth. However, there is little systematic evidence about the actual magnitude of such rents. We have collected data on wealth for a large sample of individuals who served in the U.S. House of Representatives during the period 1845-1875, using the censuses of 1850, 1860 and 1870. We use this data to estimate the monetary returns to holding office. We find no evidence of large returns for the 1850’s or late 1860’s. We do find evidence of significant returns for the early 1860’s. This pattern suggests that although the returns to a seat in the House were low during “normal” times, they increased during the Civil War years – perhaps not coincidentally, a period of dramatically higher federal spending.

# 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. These models tend to predict inefficient and/or distorted policies. 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 (arguments at the heart of institutional theories of comparative development).<sup>1</sup>

In these models incumbent politicians typically capture some of the rents in equilibrium. The rents might be small if the political environment is highly competitive and politicians do not have any special information, but otherwise they should be substantial. Thus, 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.

Even if the returns accruing to politicians do not imply any specific inefficiency or distortion, estimates of these returns 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, 2007a, 2007b). Finding that those with political power tend to accumulate wealth more than others would also help us understand the persistence of elites and the reproduction of political and economic power (e.g., Dal Bo, Dal Bo and Snyder, 2007).

In this paper we use historical census data from the U.S. to estimate the returns to holding a seat in the U.S. House of Representatives during the 1850’s and 1860’s. We focus

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<sup>1</sup>The literature includes Barro (1973), Ferejohn (1986), Banks and Sundaram (1993, 1998), Harrington (1993), Persson, Rolland and Tabellini (1997, 2000), Fearon (1999), Barganza (2000), Hindriks and Belleflamme (2001), Le Borgne and Lockwood (2001a, 2001b), 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).

on the northern states, and on representatives who served during the period 1845 to 1875. The U.S. census recorded wealth in 1850, 1860, and 1870, and we have found the individual census records of a large sample of representatives. We then employ 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, while the second group did not. We describe this in more detail below, and also discuss possible weaknesses in the approach.

We find no evidence of large returns to congressional seats for the 1850’s or late 1860’s. We do find evidence of significant returns for the early 1860’s. We are tempted to speculate that the returns to a seat in the House were low during “normal” times in the mid-19th century, but increased when federal government spending expanded sharply during the Civil War years. At a minimum, the *absence* of any evidence of large returns during the 1850’s and late 1860’s calls into question the frequent claims by politicians, journalists, and reformers at the time, as well as some later historians, that this was an extraordinarily corrupt era in U.S. politics.

We are in the process of extending this work to the south, and to other political offices – U.S. Senators, governors, and mayors, as well as top bureaucratic posts. We are also collecting data on individuals who run for office but *lose*. These provide an excellent control group, especially those who lose by a small margin. In future work we will employ a regression-discontinuity design that compares those who narrowly won office to those who narrowly lost. This will yield estimates that are arguably subject to less bias than those reported here. This comes at a cost of course – we must collect much more data. Moreover, finding records of census wealth for losers is more difficult than for winners, because there is less biographical information to help in matching.

Our paper contributes to a small but recently growing literature on estimating the value of public office. In another historical paper, Acemoglu, et al. (2007) 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. However, given that politicians may differ from non-politicians in many other respects, a naive comparison of politicians and non-politicians may confound the causal effect of politics with the effects of unobserved characteristics. Eggers and Hainmueller (2008) collect probate records of candidates to the British parliament, and use a regression-discontinuity design 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. Three papers study congress in the current era. Lenz (n.d.) uses reported assets of U.S. members of congress, matched with a sample from the Panel Study of Income Dynamics, and finds 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 sources of value, 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.

## 2. A Corrupt Era?

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.”<sup>2</sup>

Many later scholars agree with these claims. Summers (1987) writes, “In every way the decade before the Civil War was corrupt. The 1850’s were as depraved as any other age, and, at least from the evidence available to historians, far more debauched than the 1840s” (page

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<sup>2</sup>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).

14).<sup>3</sup> Writing about the events of 1857, Stamp (1990) 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” (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 spoils 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).<sup>4</sup> Sproat (1968) argues that most liberal reformers in the late 1860’s 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).

In spite of these widespread claims about political corruption, there is little systematic evidence that politicians in this period did indeed abuse political power for their own economic benefit. We will now provide some.

### 3. Data and Estimation Strategy

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<sup>3</sup>Summers goes on to argue that corruption was a factor leading to secession. In particular, it helped bolster the arguments of both abolitionists and Southern Rights men. The former argued that corruption enabled the “Slave Power” to dominate the national government. It achieved its goals, especially the extension of slavery into the territories, by bribing weak and venal northerner politicians. The latter argued that “only disunion could keep the South from being infected with Northern corruption, just as revolution had freed the colonists from the contagion of British practice in 1776” (page 290). Greenberg (1985) makes similar arguments.

<sup>4</sup>For a revisionist view, see Summers (1993).

### 3.1. Theoretical and Methodological Issues

The main problem underlying the estimation of the returns of a seat in congress is self-selection into politics. 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. Hence, a naive comparison of wealth accumulation by politicians and non-politicians will confound the causal effect of politics and the effect of other personal characteristics we may be unable to measure or observe<sup>5</sup>.

To estimate the causal effect of political office-holding on wealth accumulation one could use a regression discontinuity design based on close elections. The identifying assumption is that the outcome of very close elections is random and hence we can assume that any differences in wealth accumulation between close winners and close losers can be attributed to politics. This approach however, requires detailed information on both the winners *and* losers of congressional races. We are currently collecting data on all candidates to the U.S. congress between 1840 and 1875 and will report the results of the regression discontinuity approach in future work. In this paper however, we report the results of a simple “before-and-after” design that relies solely on data for individuals who actually won and served.

Figure 1 below illustrates our approach. Suppose we can observe the wealth of members of congress at two different years  $t$  and  $t + 10$ . 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  $t$  and zero otherwise. Similarly,  $T_{early}$  takes a value of 1 for members of congress that served only during the 5 years following  $t$  and zero otherwise. We can also define similar indicator function for congressman who served around  $t + 10$ . That is,  $T_{late}$  takes a value of 1 for all those who served only in the 5 years preceding  $t+10$  and

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<sup>5</sup>On the one hand, highly talented individuals may find holding office especially costly since they must sacrifice high returns in the private sector. If so, 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.



zero otherwise while  $N_{late}$  takes a value of 1 for congressman who served only during the 5 years after  $t+10$  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 late 1860's 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 congressman for which  $T_{late} = 1$ ) with those that only served during the five years just after 1870 (i.e. all congressman 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 early 1860's 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 however, while only the latter group was treated by politics between 1860 and 1870, we need to consider the possibility that politicians obtain returns from Congress not only while in office but also after they have served.

To motivate our regression framework and have a better understanding of the magnitudes we are estimating consider the following process for the accumulation of wealth of a given individual  $i$  in at time  $t$ :

$$\frac{dW}{dt} = [r + r_d d(t) + r_a a(t) + X'\beta] W(t) + [R_d d(t) + R_a a(t)] \quad (1)$$

Equation (1) distinguishes between two types of returns to serving in congress – those that increase the returns on existing initial wealth, and more direct payoffs in monetary units that are independent of the politician's initial wealth. The first term shows the different factors that may affect the returns on *initial* wealth. The  $r$  corresponds to the market rate of return to which all individuals have access. The  $r_d$  corresponds to the additional return that politicians get during the time in which they are holding office. An individual only enjoys these additional returns when  $d(t)=1$ , where  $d(t)$  is an indicator function for whether the individ-

ual is holding office at time  $t$ . The  $r_d$  may be related to the better investment opportunities to which congressman may have access while in office due to privileged information on the financial or real estate markets. The  $r_a$  corresponds to the additional return that politicians may enjoy *after* leaving office. This may reflect networks or connections that politicians are able to enjoy once they leave office. A congressman only enjoys this additional return when  $a(t) = 1$ , where  $a(t)$  is an indicator function for whether the individual is out of office at time  $t$  (after having served). Finally,  $X'\beta$  captures all other individual characteristics that can affect the returns an individual gets on his stock of wealth, such as occupation, age, and initial wealth (if we believe there is mean reversion). The second term in (1) captures direct payoffs that congressman may get – such as direct bribes or side-payments – that increase their wealth directly and are independent of their initial wealth. The  $R_d$  corresponds to the direct payoffs a politician receives at time  $t$  while in office, while  $R_a$  correspond to the direct payoffs politicians may enjoy after leaving office.

Dividing both sides of equation (1) by  $W(t)$  yields:

$$\frac{dW}{dt}/W(t) = \frac{d\ln(W(t))}{dt} = [r + r_d d(t) + r_a a(t) + X'\beta] + [R_d d(t) + R_a a(t)]/W(t) \quad (2)$$

In the above analysis we modeled the evolution of wealth in continuous time for illustration purposes. However, we can do the “before-and-after” analysis described above by estimating a discrete time version of equation (2) above. More concretely, we can estimate:

$$\log(W_{i,t+10}) - \log(W_{i,t}) = \alpha + \gamma T_i + \phi \frac{T_i}{W_{i,t}} + X_i'\beta + \varepsilon_i \quad (3)$$

where  $W_{i,t}$  ( $W_{i,t+10}$ ) is the wealth of congressman  $i$  in year  $t$  ( $t+10$ ),  $T_i$  corresponds to one of the “treatment” indicator functions defined above, and  $X_i$  corresponds to a series of individual characteristics that may influence wealth accumulation between the two years.

The specific sample on which the above regression should be estimated as well as the interpretation of the coefficients 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  $t+10$  (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  $T_{late}$ . In terms of interpreting the coefficients, in this case  $\gamma$  corresponds to the estimate of  $r_d$ ,  $\phi$  corresponds to an estimate of  $R_d$  while the constant  $\alpha$  captures the market rate of return,  $r$ . In this case, we do not have to worry about returns to congress *after* serving since our control group (those who served in the 5 years after  $t + 10$ ) did not serve between  $t$  and  $t+10$ .

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 this case,  $T_i$  will correspond to  $T_{early}$  and again,  $\gamma$  will provide an estimate of  $r_d$  and  $\phi$  an estimate of  $R_d$ . However, in this case the estimate of  $\alpha$  will now confound both the market return  $r$  as well as the returns after serving congress  $r_a$  and  $R_a/W_t$ .

However, it is important to mention some potential drawbacks of the above framework. First, notice that in our definition of  $T_i$  we ignore the fact that some congressman start serving at different times and serve for a different number of years within the 5-year period (that is, some “treated” congressmen may have been “treated” for more years than others). Most importantly however, our estimates of  $\gamma$  and  $\phi$  (i.e. of the returns and payoffs from office while serving) can be biased if congressional winners in the five years just before or just after a given year are different with respect to various characteristics that we are unable to control for and are correlated with wealth accumulation between  $t$  and  $t + 10$ . Selection into politics is unlikely to be a concern here since our sample consists of members of congress (individuals that *ran* and *won* elections). Moreover, we are comparing winners in a relatively small time window across a given year which gives us further confidence that they are similar. However, there can still be some underlying differences we cannot observe, and hence, we cannot be certain that our estimates are unbiased.

In the analysis that follows we estimate equation (3) above for  $t = 1850$  and  $t = 1860$ . This will allow us to get an estimate of the returns to serving in congress in the early and

late part of the 1850's and 1860's.<sup>6</sup>

### 3.2. Data

We obtained the names of all members of congress serving between 1840 and 1875 from ICPSR and McKibbin (1997) and the *Biographical Directory of the United States Congress*. These sources also provided additional information on congressmen, including the year and place of birth, profession and career, and place of residence at different points in time. We also used Martis (1982) to match congressional districts to counties and cities. This information was useful for matching each representative to his census records.

The wealth data are from the Federal U.S. censuses of 1850, 1860 and 1870. These are the only years in which the census collected information on people's wealth. The census recorded real estate wealth in 1850, 1860 and 1870, and personal wealth in 1860 and 1870. In addition, the census recorded information on year and place of birth, county of residence and occupation. The census records in these years are available in Ancestry.com, a genealogical website that provides images of the original census records as well as a search engine that helps locate every single individual recorded in these censuses by first, middle and last name as well as year and place of birth and place of residence.

We proceeded to find the census record in each census year of every member of the House of Representatives during our period. We initially used PERL scripts to automatically locate the census records of as many congressmen as possible, using the first and last name, year of birth and county of residence. Despite the automated matching done by the scripts, the data collection process was still very labor intensive since wealth figures and occupation had to be typed manually. In addition, many records must be found by careful manual searching, because names and birth years are sometimes miss-recorded in the Ancestry.com search engine, many census records include only first and middle initials rather than full first names, some birth years are incorrect in the census, and some congressmen move to different counties or states. In some cases, we were unable to match congressmen with very common

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<sup>6</sup>In each of the before-and-after analyses we drop congressmen who also served in the U.S. Senate during the relevant period.

names, or unable to find them for unknown reasons.<sup>7</sup>

There were a total of 1,968 different non-southern congressmen in this period. So far, we have managed to find two or more census records for 1,431 of them.

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, there could be concerns associated with the reliability of these data. There are, however, several reasons to believe that these data are useful for our purposes. Most importantly, the information collected by census officials was, as a matter of policy, strictly confidential.<sup>8</sup>

Moreover, several previous studies have assessed the reliability of the census data in different ways. Soltow (1975) 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” (page 6). Another group of studies compared wealth reported in the census sheets with taxable wealth. Particularly relevant for our purposes 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. While the data from Ohio suggests that census wealth tends to exceed taxable wealth, his analysis suggests no systematic associations between the discrepancies and any individual characteristics.

Finally, even more important for our purposes, 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 the richest 1,500 men in Massachusetts as of about

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<sup>7</sup>There could be concerns on whether dropping congressmen with common names will introduce any bias in the analysis. Steckel (1988) and Ferrie (1996) ran, for their 1850 and 1860 samples, logit regressions of a “common name” dummy against characteristics such as location of residence (region and city size) and other personal characteristics such as real and personal wealth, ethnicity, illiteracy and occupation. Their results show that while common names occur less often in southern states and in cities with less than 75,000 inhabitants having a common name is not correlated with real or personal wealth.

<sup>8</sup>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.

1851.<sup>9</sup> Our analysis (not reported) indicates that 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 for our purposes.

A final measurement issue concerns the fact that it is sometimes difficult to distinguish between respondents with zero wealth and respondents who refused to provide any information to the census marshal (or instances where the marshal did not request the information).<sup>10</sup> 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.

## 4. Results

To assess the validity of our approach, in Table 1 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 will always include the age and age<sup>2</sup> 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.<sup>11</sup>

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<sup>9</sup>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 1860 census measure is 9 years later.

<sup>10</sup>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” (page 80).

<sup>11</sup>This is true for the other major occupation groups as well.

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 2 presents the estimates of the main coefficients of interest, i.e.,  $\gamma$  in equation (3), the coefficient on  $T_i$ . In these specifications we omit the variable  $T_i/W_{i,t}$ , and set  $\phi = 0$ . We estimated models that included the variable  $T_i/W_{i,t}$ , and also experimented with other specifications that allowed the effect of treatment in congress to vary according to initial wealth, but these interaction terms were never statistically significant. So we focus on the simpler specification here.

The results are straightforward. First, we find no evidence of a large positive return to serving in congress during the 1850's. Second, the same is true for the second half of the 1860's. Third, we do find evidence of a relatively large return to serving in congress during the first half of the 1860's. Moreover, notice that wealth accumulation between 1850 and 1860 was similar for those congressmen who served in the late 1850's and in the early 1860's. This suggests that the additional returns we find for the latter group do not correspond to pre-treatment differences.<sup>12</sup>

For the first half of the 1860's, the point estimate is .36, which implies that serving in congress during this period yielded an additional 36 percent in total wealth accumulation between 1860 and 1870. The average growth in wealth between 1860 and 1870 of the control group – those who served in the second half of the 1850's but not the first half of the 1860's – was only 39 percent. So, the returns to a seat in congress during the period were quite large in relative terms.

## 5. Conclusion

How do we reconcile our findings with the claims of widespread corruption that were so common during this period? Perhaps the claims, at least for the 1850s and late 1860s, were exaggerated or mainly political rhetoric. Another possibility is that the action was elsewhere, in state and local governments. After all, throughout the 19th century (except

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<sup>12</sup>There was little inflation between 1850 and 1860, but prices were about 40 percent higher in 1870 than in 1860.

during the Civil War) combined state and local spending exceeded federal spending. The patterns we identify suggest that this is worth further study.

Another possible lesson is that politicians can sometimes exploit extraordinary circumstances. We find evidence that congressmen used their offices for personal gain during the early 1860s. This coincides with the Civil War, a period of extraordinarily large federal government spending. In the 1861 fiscal year (July 1860-June 1861), just before the Civil War, the federal government spent only about \$67 million, about \$2 per capita. Expenditures exploded during the war, to \$475 million in 1862, \$715 million in 1863, \$865 million in 1864, and \$1,298 million in 1865.<sup>13</sup> Spending shrank sharply after the war, though not to its pre-war levels even in real terms – the average was \$292 million over the period 1867-1871. Moreover, much of the spending at the beginning of the war was done frantically under an emergency situation, with relatively little oversight and considerable chaos. There were many opportunities to make money, and politicians were well placed to take advantage of them. Perhaps they did.

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<sup>13</sup>All spending figures are from the *Statistical Abstract of the U.S., 1878*, Table 1. This excludes debt payments.



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<b>Table 1: Differences in Means for Observables</b>				
	Early Treatment		Late Treatment	
Variable	Untreated	Treated	Untreated	Treated
Initial Real Wealth, 1850	8.96	8.96	8.64	8.56
Age, 1850	47.99	42.88*	36.77	37.35
Lawyer Dummy,1850	0.53	0.60	0.60	0.64
Initial Total Wealth, 1860	10.03	10.04	9.23	9.52
Age, 1860	47.40	43.36*	37.25	39.39*
Lawyer Dummy,1860	0.64	0.64	0.59	0.64

Entries are cell means.

\* = difference between Treated and Untreated is significant at the .05 level.

Wealth is in natural logs.

<b>Table 2: Before and After Estimates of the Returns to a Seat in Congress</b>				
	Early Treatment		Late Treatment	
Dependent Variable	Basic Controls	All Controls	Basic Controls	All Controls
$\Delta$ Real Wealth 1850-1860	-0.03 (0.14)	0.05 (0.14)	0.09 (0.14)	0.11 (0.15)
Observations	193	193	257	257
Ending Total Wealth, 1860	-0.08 (0.13)	-0.06 (0.14)	-0.08 (0.13)	-0.05 (0.14)
Observations	198	198	262	262
$\Delta$ Total Wealth 1860-1870	0.36* (0.16)	0.36* (0.17)	-0.07 (0.11)	-0.04 (0.12)
Observations	236	236	308	308

Entries give estimated coefficient on variables as described in text. Robust standard errors in parenthesis.

Basic controls = *Initial Wealth*,  $(Initial\ Wealth)^2$ ,  $(Initial\ Wealth)^3$ , *Age* and  $Age^2$ .

All controls = Basic controls plus *Occupation Dummies* and *State Fixed Effects*.

\* = coefficient is significant at the .05 level.

Figure 1  
Before-and-After Timing

