Returns to Politics Under A Changing Political System

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Abstract:

Several studies have shown evidence that politicians profit financially from holding office. The mechanisms allowing them to do so, however, remain unclear. Based on newly-collected data from probate inventories, I obtain a measure of wealth for a sample of just-elected politicians and their losing contenders in Dutch district-level elections (1860-1917). Using a dynamic regression discontinuity design, I derive estimates of the returns to a political career. I also investigate whether just-elected politicians are more likely to take up certain careers after having been elected, and I investigate whether the establishment of political parties and suffrage extensions influence the relationship between political office and personal wealth. The results show that the returns to politics are primarily accrued in the first periods of holding office, and that political parties succeed in constraining politicians' rent-seeking ability. Being elected to national politics, however, does not make one more likely to obtain a high-earnings position afterwards.

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1   Introduction

In the majority of modern constitutions around the world, it is stipulated that the people have the power to decide what happens to their country (Persson and Tabellini, 2005). In practice, however, national governance usually involves some delegation of power from the people to representatives, politicians, who are in turn expected to act in the interest of those who elected them. In many cases, this turns out to be only partially true. Politicians are often suspected to use and abuse their political position for private gain, or otherwise pursue policies that are counter to the interests of their constituents. Economists have made many attempts to investigate representatives' incentives and the corresponding opportunities to prioritize private gain. Barro (1973) and Ferejohn (1986) are pioneering studies that focus on the role of the electoral process and its various elements, such as term limits for politicians. Acemoglu et al. (2005) ignited a literature based on the premise that electoral accountability in itself is not enough to solve politicians' commitment problems. Later studies focused on the role of interest groups (Snyder Jr and Ting, 2008) based on religion or ethnicity (Padró i Miquel, 2007), the role of the media (Gehlbach and Sonin, 2014) and government audits (Avis et al., 2018) to keep politicians in check.

Despite the many attempts that been made to regulate politicians (see e.g. Djankov et al., 2010, for a survey), empirically, several studies have shown the existence of particular forms of returns to politics, that is to say, benefits accruing to politicians beyond their formal compensation. These rents can come in monetary forms, as in (Svaleryd and Vlachos, 2009; Eggers and Hainmueller, 2009; Amore and Bennedsen, 2013; Fisman et al., 2014), in the forms of prioritizing one's ideology over electoral preferences (Peltzman, 1984; Mian et al., 2010) but also in the forms of nepotism (Dal Bó et al., 2009) and can even be channeled to other individuals, e.g. relatives (Fafchamps and Labonne, 2017). Many empirical studies highlight a great diversity of possible mechanisms at work. Specifically, Eggers and Hainmueller (2009) hint that party organization might be a significant determinant of the extent to which politicians can prioritize their own interests, motivated by a sharp difference in political rents between Conservative and Labor MPs in Post-WWII Britain. Fisman et al. (2014) find a differential effect in various Indian states that have different levels of corruption. Querubin et al. (2011) hint at the influence of government size and monitoring by the media as possible determinants of returns to a political career.

This study addresses takes a long-term perspective and investigates the institutional determinants of returns to politics using a sample of close elections in the Netherlands from 1848-1917. I make use of close elections to establish the existence and magnitude of returns to politics using a regression discontinuity strategy. This setting enables me to to tie the returns to politics to several explanations. First, the Netherlands employed a district system (De Jong, 1999). In each district, a small number of candidates took part, and these were frequently hotly contested, so that close elections are a relatively frequent phenomenon. Second, this period is marked by the establishment of political parties (de Jong, 2001). I empirically investigate whether political parties are able to curb the returns to politics for individual politicians, by making use of *newspaper recommendations* for politicians, which allow me to identify political allegiance before political parties were established. Third, this period was marked by several franchise extensions, suddenly making the electorate much larger in each district (Van den Berg and Vis, 2013), which I use to directly estimate the influence of suffrage extensions and increased electoral turnout on the returns to politics. Finally, I also collect extensive data on the career paths of politicians, allowing me to investigate whether

politicians who continued their career in business or other positions are responsible for the effect.

It is also important to understand the magnitude of the returns to politics in historical perspective. The Netherlands, in parallel to other European countries, underwent various important changes in the late 19th and early 20th centuries (Przeworski, 2009): in particular, the country started out as a country under absolute monarchy in the early 19th century, but switched to constitutional monarchy and parliamentary control following liberal reforms in 1848 (Aerts, 2018). There were severe restrictions to suffrage in the most important governmental bodies: one had to be male, and pay a minimum amount of taxes to be accorded the right to vote, although eligibility was (formally) completely unconstrained (van der Kolk et al., 2018). Throughout the late 19th and early 20th centuries, politicians and activists have campaigned for, and ultimately achieved, universal suffrage. A better understanding of the interplay between politicians' personal interests at hand and their decision-making might shed new light on explanations regarding politicians' decisions to extend the franchise (Lizzeri and Persico, 2004; Besley, 2005; Becker and Hornung, 2020).

Similarly, the same period also saw the development and rise in popularity of political parties. As the differences between liberal and confessional (Christian) factions of parliament mounted, politicians and politically conscious citizens began to organize themselves into electoral associations (*Kiesvereenigingen*), the existence of which was quickly superseded by political parties (De Jong, 1999). The first political party, the Anti-Revolutionary Party, was founded in 1879 and its liberal counterpart, the Liberal Union, in 1885 (de Jong, 2003; Voerman, 1989). The Catholic electoral associations united themselves somewhat later, in 1897. Before this era, candidates who aligned with a particular political agenda were usually supported by newspapers (De Jong, 1999). It is widely recognized that political parties exert party discipline and party affiliation is an important determinant of political voting behavior, thereby possibly constraining financial returns to politics (see e.g. Aidt and Franck, 2015, 2019; Becker and Hornung, 2020). The staggered establishment of political parties thus allows me to empirically identify the influence of party discipline while keeping political affiliation constant, and thereby shed light on how political parties changed the political landscape.

Methodologically, I use a dynamic regression discontinuity design (Cellini et al., 2010) to estimate parameters that can be interpreted as an average treatment effect on the treated population (ATT) of being elected into national politics on individuals' personal wealth. The regression discontinuity results hinge on the assumption that nearly-elected candidates are good counterfactuals for politicians. I make this plausible by gathering a sizeable dataset containing information about the candidates' background, origin, political orientation and demographics, as well as the district characteristics in which close elections took place. The results show firstly that politicians who marginally won elections are significantly wealthier at the end of their life than politicians who marginally lost. These results are robust to many parameter choices of the RD analysis, and also pass various placebo tests. Secondly, the results show convincing evidence for some of the explanations I bring forward: electoral competition and political parties had a disciplining effect on politics and successfully curbed the returns to politics.

The remainder of this study is structured as follows. First, in section 2, I discuss the historical background by focusing on the history of eligibility and suffrage restrictions and their evolution over time (section 2.1), and on the political playing field, including electoral associations, political party formation (section 2.2). In section 3, I introduce the data. In section 4, I describe the empirical strategy, and in

section 5, I show the main regression discontinuity results. In section 6, I investigate various alternative explanations. In section 7, I provide various robustness checks and in section 8, I conclude.

2   Institutional Background

2.1   Democratization in the Netherlands, 1848-1917

In the period 1848-1917, all elections to the lower house were organized in the framework of a district system. Before 1848, the year in which constitutional reforms liberalized the electoral system and political institutions of the country, delegates to the Lower house were elected indirectly: the enfranchised electorate elected delegates to an intermediary assembly called the Provincial Estates, which then elected delegates to the lower house. Delegates to the upper house were elected in a similar way, and in contrast to the lower house elections, the 1848 constitution left this system intact for the elections to the upper house, whereas the elections to the lower house were subject to reform, effectively rendering them direct, and more democratic (Blok, 1987). From 1849 onward, lower house elections took place biannually, in which every two years, half of the seats were up for contest. In almost all cases, districts features two seats, and hence, in each election, one seat was up for election. This also meant that a lower house member was elected for four years.

The precise mapping from municipality (the lowest-level administrative unit of the Netherlands) to district was stipulated in the electoral law (*Kieswet*), in which the stated objective was that each district, and consequently each representative, represent about 45,000 inhabitants (De Jong, 1999). Accordingly, after the constitutional revision in 1848, the lower house had 68 seats, corresponding roughly to the representation of 45,000 inhabitants by each of those seats. In the meantime, however, population growth had taken off, meaning that it was more and more difficult to apply this rule. The lawmakers responded to this issue by increasing the number of seats, creating and changing the composition of districts: the number of lower house seats raised from 68 to 86 in about 10 years. However, because of the stakes involved (issues related to gerrymandering), it became more and more difficult to agree upon a given composition, effectively delaying any reform from 1878 to a constitutional revision in 1887, after it was capped at 100. At the same time, with population growth not stalling, and compromise aimed at the reallocation of districts being difficult, the district system saw imbalances between districts become more and more salient. This particularly favored sparsely over densely populated districts. Even the electoral law reforms of 1896, which encompassed, among other reforms, a partition of the largest cities into various districts, effectively increasing their representation, could not change the imbalance that disfavored them.

Issues like these also caused the district system to come under fire from various sides of the political spectrum (van der Kolk et al., 2018). One frequently expressed criticism focused on the strong deviation from proportional representation caused by the district system: in the population, it was argued, confessional (Protestant and Catholic) parties could count on a majority, of which they were deprived during the district system. In the first election under proportional representation (1918), this was indeed the case. Another issue was related to the set of rules that stipulated under which circumstances an election was won. Significantly, throughout the entire period, elections required that an absolute majority of votes be obtained. This also applied to elections which were contested by more than two candidates: in case no candidate obtained an absolute majority, a second round was organized in which the two candidates who obtained the highest amount of votes competed against each other. As mentioned, this first-past-the-post-like system particularly favored the liberal party over confessional politicians, because even though

confessional politicians were usually the most popular in the first round, a liberal-socialist coalition block could still beat the confessional candidate in the second.

Lower house members were also compensated for their political activity. The 1815 Constitution stipulated that lower house members were entitled to a retribution of expenses of 2500 guilders per year, aiming to cover the costs of living in the Hague, in addition to traveling reimbursements at the rate of 1,50 per kilometer (Elzinga, 1985). If we compare these numbers to the work of van Zanden (1983) and van Riel (2018), who provide wage data for different professions in the Netherlands from 1819-1913, we find that the lump sum amounts to approx. 9 times the yearly wage of an average worker in 1850. The reimbursement of 1,50 per kilometer equaled about twice the average wage in 1850. After the 1848 Constitution, politicians sought legitimacy partly by decreasing the lump sum to 2000 guilders per year and the traveling reimbursements at 1,50 per travelled kilometer. Rising wages made this sum equal to about 5 times the average wage in 1890. In 1917, these numbers were raised again, this time to 5,000 guilders. The workers' wage, however, had not yet doubled, but only increased by a factor of about 1.5, enlarging the gap again. With respect to the reimbursement of traveling expenses, from then on, members of parliament were awarded free public transportation, attenuating the need to look for a place of residence in the Hague, and decreasing the gap between politicians who lived close and far from the Hague. In addition, (former) members of parliament were awarded a pension (Kan, 1916) of 100 guilders for each active year in parliament, with a maximum total pension of 2,000 guilders.

Both before and after 1848, politics was generally considered (by politicians themselves) an honorary function, unlike a job. Many politicians objected to paying or retributing the costs associated with being a representative, fearing it would incentivize politicians with seeking votes, thereby compromising the representative's independence, and it would attract politicians who would be prone to doing so (see e.g. Aerts, 2009). With time, more and more politicians, principally liberals and socialists, started to change their views for a variety of reasons, the most important of which being that working class individuals might be discouraged to take part in the country's representative institutions because of financial vulnerability. This view gradually became more mainstream, especially as politicians with a working class background became more frequent in parliament (ref to myself) and lead to the incorporation of the raise of the retribution in the 1917 constitutional revision.

In terms of international comparability, these trends closely paralleled developments in e.g. France, Germany and Great Britain. In Germany, the 1871 *Reichsverfassung* explicitly forbade to compensate delegates to the *Reichstag* in any way, but in 1906, a limited and imperfect system of retribution was instated (Lindeboom, 1916; Edinger, 2009). In France, parliamentary compensation had been the object of parliamentary struggle since the revolution, and a 1906 hike caused widespread indignation (Monier and Portalez, 2020). In Great Britain, members of parliament were nonsalaried until 1911, after a scandal within the Labor Party sparked parliament to legislate parliamentary compensation (Madden and McKeown, 2012).

[I want to talk about reforms of the *Kieswet*, what and why - and how eventually universal suffrage came to existence] The district system for lower house was put in place after the 1848 Constitution, in which the King, frightened by events elsewhere in Europe, had ordered the most influential liberal politician of the time, J.R. Thorbecke, to draft a new Constitution, on which several reforms were based. At the time, it was widely known that this marked an era that broke with constitutional monarchy and set a

precedent for further democratization, but both active and passive suffrage were still severely restricted: about 85,000 men on a population of over 2.5 million had the right to active suffrage for both upper and lower houses. However, whereas eligibility for the upper house was limited to those who surfaced on the so-called *Lijst Verkiesbaren voor de Eerste Kamer*, a list on which figured a certain number of the highest tax-paying individuals. The list was assembled on a per-province basis, and based on the rule of thumb that for each 3,000 inhabitants, 1 individual would be eligible for election (REF Moes, Broncommentaar). Voting was therefore restricted explicitly on the basis of wealth. Additionally, suffrage was also restricted to males over 30.

Entry to the lower house, however, was formally unrestricted, so that formally, every citizen aged 30 or higher could be a candidate in the lower house elections. In practice, however, citizens were understood to be only the male members of the population, which was formalized by a constitutional revision in 1887.

[Talk about active and passive suffrage and the changing requirements over time]

The first elections under proportional representation were also the first elections in which electoral lists were printed according to party membership, nudging the voters to vote for a party, rather than a particular candidate.

2.2 Political Landscape and Political Party Formation

The electoral system in the Netherlands after 1848 was centered on individual delegates, not political parties. Politicians were supposed to be independent, not least with respect to their own delegates, and to promote the common interests of the country (de Jong, 2003). Political parties were preceded by *Kiesvereenigingen*, electoral unions, of eligible individuals with (generally) the same political orientation, intending to coordinate their voting behavior. These electoral unions were partly a response to rising and increasing awareness of ideological differences between various factions, but also partly to increase information about elections: oftentimes, the electorate was not aware of what candidates' political positions were (Aerts et al., 2002) and diffusion of political views, for instance through newspapers, was limited. Faced with this nontransparent environment, De Jong (1999) argues that the electorate often based their opinions on those of individuals of high societal standing: burgomasters, notaries, clerics and similar individuals.

[I want to write about the path leading to the existence of political parties from Kiesvereenigingen]

[Here I want to write about campaigning, campaign financing]

[I want to write about the ideological basis underlying various parties]

3 Data and Sources

3.1 Close Elections

The *Repositorium Tweede Kamerverkiezingen 1848-1917* (Repository Lower House Elections) is a repository containing information about all elections to the Dutch lower house over the period 1848-1917, in which elections were organized at the district-level. As mentioned, any candidate who met the requirements of passive suffrage could be voted for - there was no predetermined list of candidates. Theoretically, therefore, there was no cap on the number of contestants in a given elections. Practically, however, local newspapers diffused who would be the contestants in upcoming elections, which frequently went hand in hand with an endorsement by the editorial board of a particular candidate (Oud, 1997; De Jong, 1999). In practice, elections were almost always contested by two candidates. There is also a small number of elections in which one candidate ran uncontested. Other exceptions include the more densely populated cities, such as Amsterdam and Rotterdam, in which a relatively large number of candidates, sometimes up to 10, would contest for several (up to three) seats.

[Figures 1 and 2 here]

The *Repositorium* contains data on virtually all these elections, including the names of the candidates who contested in a given election, the amount of votes they obtained, the number of enfranchised individuals in this district, voter turnout, and also some metadata, including the amount of seats that are contested in the particular election, the type of election, and the election date. Based on this data, we also define the set of winners in each election e, $\{Winners\}_e$, which consists of candidates whose rank in terms of the amount of votes $\leq$ the amount of seats that were contested.

In total, there are about 2400 unique elections in the district system over the period 1860-1917. In line with other studies using close elections (e.g. Lee, 2008), we use a vote margin-based approach to identify which elections are close: in particular, we first find the *marginal winner (MW)* in the election, which is defined as a winning candidate with the lowest number of votes from all winning candidates. In the vast majority of cases, this amounts to the only winner, because the election had only one seat up for election, but in a significant minority of the cases, this yields a different candidate. Then, at the candidate-district level (candidate i, district e), we define and compute vote margins as follows:

$$\text{Margin}_i = \begin{cases} \frac{\text{Amount of Votes}_i - \text{Amount of Votes}_{MW}}{\text{Amount of Votes}_e} & \text{if } i \in \{\text{Winners}\} \\ \frac{\text{Amount of Votes}_{MW} - \text{Amount of Votes}_i}{\text{Amount of Votes}_e} & \text{if } i \notin \{\text{Winners}\} \end{cases} \tag{1}$$

This way of defining the margin ensure that winners end up with a margin ≥ 0, whereas losers have a negative margin. Then, a close election is an election in which there is at least a candidate with a margin $\in [-x, 0)$, where x is an arbitrary bandwidth. After data collection, elaborated on in the next section, we chose bandwidth $x = 20\%$, meaning that we identify elections as close if there is at least one candidate who obtained at most 20 percentage points fewer votes than the marginal winner. This yields about 500 close elections, in which there are at least 500 candidates that lost the election with a margin that is less in absolute value than $x\%$. There are a significant amount of elections in which some of the close

candidates are candidates who either already were active in politics, or were elected in the lower house at a later moment. We then proceed to identify close losers who did not end up in politics later by requiring that the close candidates not be in the union of all Winners-sets, and we end up with candidates that (i) marginally lost an election and (ii) did not end up in the lower house later, or were not active in the lower house already.

3.2 Personal Wealth

[Talk about the Memories van Successie, problem of time-comparability (time to death as a covariate), evasion and limited availability of data]

The *Memories van Successie* are archives primarily containing documents specifying the appraisal of a deceased individual's assets and liabilities with the purpose of levying inheritance taxes. The first inheritance tax that targeted lineal descendants was instated in 1877, and from that year onward, every individual whose estimated wealth was higher than some minimum threshold (in practice, almost everyone) was subjected to an appraisal of their assets and liabilities, and depending on the particular composition of the inheritance, a specific inheritance tax (Bos, 1990). Citizens were initially expected to voluntarily declare their assets and liabilities within a year after the decease, but the authorities were by no means depending on voluntary action: civil servants provided the tax authorities with lists of deceased individuals, after which descendants could be summoned to declare. In all cases, descendants had to declare under oath that the list of assets and liabilities they submitted was truthful in a court (Moes, 2012) Several miscellaneous documents containing internal correspondence within the tax agency also indicate that taxation was approached with care and legal requirements were paid attention to.

It is not generally known precisely how the Dutch tax agency appraised all asset classes, in particular, real estate, but most financial assets were appraised with eye for detail: listed stock and bond prices were quoted from the *Prijscourant*, a publication administered by the Amsterdam stock exchange, which contained accurate data about contemporaneous stock prices. The value of foreign assets were without exception denoted in Dutch guilders. The *Memories* are publicly available from 1877-1927 in all Dutch provincial archives. After 1927, the *Memories* are still part of the internal administration of the Dutch tax agency, hence, they are by and large inaccessible to the public. Any particular document contains the name, place and date of death of the individual, followed by an initial statement of an individual's assets, liabilities and net wealth. Afterwards, an entire detailed inventory describing all their assets and liabilities, including financial claims can be found. Finally, the assets, liabilities and net wealth are again stated at the end of the *Memories*. By default, I use the net wealth that is first stated, and although sometimes slight differences can be found, the correlation between these two statements is 0.99.

3.3 Other Covariates

I retrieve a proprietary dataset from the *Politiek Documentatiecentrum* (PDC), a think-tank focused on Dutch politics. The data encompass various demographic and other variables related to a politicians' life, including their birth and death date and place, which allow me to find the politicians in the *Memories* archives, as well as information about their political allegiance and their professional career. For

non-politicians, I make use of the search engines of provincial archives, a historical newspaper search engine *Delpher*, and various genealogy websites to find the dates and places of decease. For these non-politicians, there is generally less information about their professions, and the level of detail is generally lower, but whereever possible, this information is also retained. Political party membership or allegiance is also found through these means.

4 Method

4.1 A Dynamic Regression Discontinuity Design

The analysis of returns to politics is complicated by two features: first, comparing candidates who ran for office more frequently with candidates who did not exert the same effort might result in endogenous estimates to the extent the effort undertaken in getting elected is correlated with wealth-accumulating capacity, even if there is no discontinuity at the cut-off point. Secondly, even when comparing candidates who exerted a similar amount of effort, the estimate contains not only the ceteris paribus effect of being elected for the first time on end-of-life wealth, but also the dynamic effects of having an altered probability of being re-elected and accruing returns to a prolonged stay in the lower house. Hence, I follow an approach similar to Cellini et al. (2010) to disentangle these effects. More precisely, consider the following model[1], which incorporates the possibility that politicians who are first elected at different tries can realize different initial wealth effects:

$$w_i = \sum_{\tau=1}^{\infty} \theta_\tau b_{i,\tau} + \sum_{t=2}^{\infty} \gamma_t c_{i,t} + u_i \tag{2}$$

where w_i is a candidate's end-of-life wealth, $b_{i,\tau}$ is an indicator reflecting whether candidate i is first elected at their τ'th try. In this model, θ_τ represents the ceteris paribus impact on wealth after being elected *for the first time* after trying τ times. This ensures that similar candidates in terms of effort are compared. Note that in this setup, this effect is independent of actual calendar time. In section **??**, I investigate whether suffrage extensions represent a structural break in this relationship. Secondly, $c_{i,t}$ is an indicator reflecting whether a politician is elected for the t'th time after having been elected initially. I restrict the structure such that γ_t does not depend on the number of tries τ. Consequently, γ_t represents the effect on wealth effect of being elected for the t'th time *after* having been elected once. I detail how I estimate the parameters γ_t in section 4.2. Differentiating both sides of equation 2 with respect to a particular $b_{i,\tau}$ then gives the so-called "intent-to-treat" (ITT) effect of being elected once at the τ'th try:

$$\theta_\tau^{ITT} = \frac{dw_i}{db_{i,\tau}} = \frac{\partial w_i}{\partial b_{i,\tau}} + \left(\sum_{t=2}^{\infty} \frac{dc_{i,t}}{db_{i,\tau}} \cdot \gamma_t \right) \tag{3}$$

$$= \theta_\tau^{ATT} + \left(\sum_{t=2}^{\infty} \pi_t \cdot \gamma_t \right)$$

where $dc_{i,t}/db_{i,\tau}$ represents the incumbency advantage (Lee, 2008), the change in the probability of being elected on the probability of being reelected. In the last line, I make the assumption that this fraction $\pi_{\tau,t} = \pi_t$ for all τ, indicating that the incumbency advantage in the t'th election after having won

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[1]This model is a model for close elections, making sure that $\mathbf{E}[u_i b_\tau] = 0$, so that the parameters θ_τ can be estimated consistently.

once is the same for candidates elected for the first time at different tries τ and τ'. [2] In other words, the estimand for the effect of being elected once (at the τ'th try) on end-of-life wealth contains a combination of the ceteris paribus effect θ_τ^{ATT} and the probability-weighted *wealth effects of increased tenure*, reflected by the γ_t.

First, I set out by estimating the θ_τ^{ITT} for different τ. I do this by employing a regression discontinuity approach similar to Eggers and Hainmueller (2009), Fisman et al. (2014) and Fafchamps and Labonne (2017). The basic specification that I use, for a particular τ, is:

$$g(w_i) = \alpha + \theta_\tau^{ITT} \cdot 1_{\text{Margin}_i > 0} + \eta \cdot f(\text{Margin}_i) + X_i \beta + \epsilon_i \tag{4}$$

where $g(.) = \{\text{ihs}(.), \log(.)\}$. I estimate θ_τ^{ITT} using local linear polynomial regression on each side of the threshold, following Gelman and Imbens (2019) and Cattaneo et al. (2019).

In terms of interpretation, these θ_τ^{ITT}'s are likely an overestimate for the θ_τ^{ATT}, given a hypothesized positive incumbency advantage and returns to political activity. Afterwards, I investigate whether the θ_τ^{ITT} are different for different τ's, i.e. whether there are notable differences in returns to politics between politicians elected who tried hard and those who had it easy. In order to retrieve estimates of θ_τ^{ATT}, I also need to estimate the γ_t and π_t, which I describe in the next section. The effects γ_t are also of theoretical interest, as they describe the marginal return curve to a political career.

4.2 Estimating Incumbency Advantage and Returns to Tenure

Estimating the incumbency advantages π_t is relatively straightforward, using the following specification for the k'th election after an election e for candidate i:

$$\mathcal{I}[c_{i,k} = 1] = \alpha + \pi_{i,k} \cdot 1_{\text{Margin}_{i,e} > 0} + \eta \cdot f(\text{Margin}_{i,e}) + X_i \beta + \epsilon_i \tag{5}$$

where the dependent variable is 1 if candidate i won an election k, 0 if a candidate loses. I include a constant term, and focus on close elections to identify the ceteris paribus influence of winning on the probability of winning the k'th election afterwards. I also include various covariates at the individual level. The estimation procedure is described in section 4.3. Estimating equation 5 for each $k \in \{2, 3, \dots\}$ then gives estimates for the incumbency advantages for the $k - 1$'th election in the future.

Estimating the returns to tenure in the lower house is somewhat more challenging. Conditional on being elected $t - 1$ times (where $t > 1$), the structure for end-of-life wealth is as follows:

$$w_i = \sum_{k=t}^{\infty} \gamma_k c_{i,k} + u_i \tag{6}$$

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[2] I also assume that the incumbency advantage is independent of calendar time, and that there are no dynamic incumbency advantages, i.e., there is no *additional* incumbency advantage after being elected twice in a row, as opposed to an incumbency advantage in the third election after initially having won one (the latter of which is among the π_t I estimate).

Again, focusing on an RD-implementation so that $\mathbb{E}[u_i \gamma_t] = 0$, and differentiating equation 6 with respect to the independent variable $c_{i,k}$ makes clear the same issue as in section 4.1 is at hand:

$$\gamma_k^{ITT} = \frac{dw_i}{dc_{i,k}} = \frac{\partial \gamma_{i,k}}{\partial c_{i,k}} + \sum_{t'>t} \gamma_{t'} \cdot \frac{\partial c_{i,t'}}{\partial c_{i,k}} \tag{7}$$
$$= \gamma_k^{ATT} + \sum_{t'>t} \gamma_{t'}^{ATT} \cdot \pi_{(t'-k)}$$

Unlike Cellini et al. (2010), I do not have a panel data dependent variable, and cannot identify one t for which the estimand $\gamma_t^{ITT} = \gamma_t^{ATT}$. This means that the ceteris paribus tenure effects are only identified under the assumption that for some acceptably large t^*, $\gamma_{t*}^{ITT} = \gamma_{t*}^{ATT}$. In the analysis, I employ this assumption and test its sensitivity for the estimates of γ_t^{ATT} and θ_τ^{ATT}. Additionally, for sufficiently precise estimation of the γ_t^{ITT}, conditionally on being elected $t-1$ times in the lower house, politicians must have participated in close elections afterwards (and a certain share of them must win). I identify that indeed a sufficient number of politicians is eligible in section **??**. I then use these politicians who have been elected $t-1$ times to estimate γ_t^{ITT} as follows:

$$g(w_i) = \alpha + \gamma_t^{ITT} \cdot 1_{\text{Margin}_i > 0} + \eta \cdot f(\text{Margin}_i) + X_i \beta + \epsilon_i \tag{8}$$

Hence, under the condition that after some t^* the incumbency advantage is statistically not different from zero, and the assumption that $\gamma_t^{ITT} = \gamma_t^{ATT}$ for some t, I can recursively estimate the γ_t^{ATT} using equation 7, and compute standard errors using the delta method. These estimates in turn allow me to estimate the θ_τ^{ATT} in equation 3.

4.3 Regression Discontinuity Parameters

All of the estimands in equations 4, 5 and 8 are estimated using a regression discontinuity-based estimation procedure. I follow Lowes and Montero (2021), by requiring that bandwidth selection be effectuated according to the MSE-minimizing procedure in Cattaneo et al. (2019), where I force the bandwidth to be equal at both sides of the cut-off point. I use a triangular kernel in the baseline specification, and I report standard errors based on bias-corrected confidence intervals (Calonico et al., 2015). In robustness analyses, I use other types of kernels, and use similar fixed as well as flexible bandwidths, e.g. the bandwidth selection procedure in Imbens and Kalyanaraman (2012).

The regression discontinuity approach implies that the treatment, being elected, is essentially randomly allocated close to the threshold. An implication of this is that the characteristics of pre-treatment variables should be roughly equal in treatment (politician) and control (non-politician) groups. I compare means for both groups at different election margins, following concerns raised by Caughey and Sekhon (2011). I use the same logic as do Lowes and Montero (2021), who estimate the RD-effect on pre-treatment characteristics at the cut-off. The equality in pre-treatment covariates also implies that the inclusion of covariates in the RD regression should not substantially impact the estimates of the treat-

ment effect. However, the inclusion of covariates does help in obtaining a more precise estimate of the local treatment effect (Lee, 2008), which is why by default, I report estimates conditional on the inclusion of covariates.

Finally, the RD design assumes there is no bunching, or selective sorting, around the threshold. Failing to meet this assumption would result in attributing the effect from the variables responsible for sorting to the treatment (McCrary, 2008). There is anecdotal evidence that this is unlikely. Most importantly, the electoral institutions are such that there are only a fixed number of seats (Blok, 1987; De Jong, 1999), and that there were no significant controversies over who had actually won the elections.

4.4 Other Sources of Returns to Politics

4.4.1 Career Paths

In addition to increased personal wealth, holding office might also bring about returns of a different kind. Several studies (e.g Amore and Bennedsen, 2013; Fafchamps and Labonne, 2017; Folke et al., 2017; Cruz et al., 2017) investigate the existence and magnitude of various benefits accruing to family members of politicians. It is therefore plausible that politicians, by virtue of being elected into national politics and having acquired a network, are themselves also more likely to end up in certain positions. Inspired by Amore and Bennedsen (2013) and Folke et al. (2017), I investigate whether just-elected politicians are more likely to become a mayor later in their career than their nearly-elected counterparts. A mayor (Dutch: *Burgemeester*) is the executive of a municipal administration in the Netherlands, an influential position which is not up for democratic election, and the position is also without substantial oversight and monitoring. For example, municipalities had the discretion to determine the mayor's salary (Kaal, 2008).

Secondly, I investigate whether just-elected politicians are more likely to end up in the colonial administration or colonial business in the Dutch Indies. After the abolition of the *Cultuurstelsel* (1870), private enterprise in the Dutch Indies was allowed by the Dutch government, and markets were opened to both Dutch and foreign investors. However, private enterprise was still characterized by an extremely coercive environment, and the economy was still primarily focused on rent extraction, which was now carried out by private firms rather than the government (Lindblad et al., 1993; Steegh et al., 2016; Taselaar, 1914), the benefactors of which were likely individuals at positions in the colonial administration and colonial business.

Thirdly, I investigate whether a political career gives individuals more access to a career in finance and business in the metropolitan. The contemporary literature (e.g. Fisman et al., 2014) documents that political connections, and thus politicians, are valuable to firms. Everything else equal, then, politicians might be more likely to take up a position in finance and business than nearly-elected non-politicians.

I estimate whether being elected has an influence on the probability of taking up a career path in one of these three settings using the following specification, for each $j \in J = \{\text{Mayor, Colonial, Business}\}$:

$$\mathcal{I}[j_i = 1] = \alpha + \delta \cdot 1_{\text{Margin}_i > 0} + \eta \cdot f(\text{Margin}_i) + X_i \beta + \epsilon_i \qquad (9)$$

where $\mathcal{I}$ is an indicator indicating whether a candidate worked in j after taking part in an election.

4.4.2 Party Discipline

As mentioned, the period under investigation (1860-1917) also features the rise of party politics. In particular, all the major political parties came into existence in the two decades following the establishment of the first major political party, the anti-revolutionary party (de Jong, 2001). Virtually all politicians immediately joined the parties that catered to their ideology after formation. It is well known, however, that political parties can exert discipline (e.g Grossman and Helpman, 2005), and thus limit the choice space of politicians, whereas before, politicians were virtually unencumbered by party discipline, which likely made it easier for them to accrue returns. Hence, I expect that after the formation of political parties, the returns to politics decrease.

Empirically, I can identify the influence of party discipline by exploiting newspaper recommendations. Newspaper recommendations were a way in which national newspapers, which had a clearly delineated political constituency, found a way to identify the political orientation of politicians and to signal this to the electorate. In practice, there was a near one-to-one correspondence between newspapers and political allegiance. These allow me to identify e.g. liberal politicians before and after the liberal party came into existence. For simplicity, I only focus only on "intent-to-treat" effects, and only on returns accrued after the *first* election into politics, by estimating the following specifications for each $h \in \mathcal{H} = \{$Before Party Formation, After Party Formation$\}$:

$$g(w_i) = \alpha + \delta \cdot 1_{\text{Margin}_i > 0} + \eta \cdot f(\text{Margin}_i) + X_i \beta + \epsilon_i \tag{10}$$

Politician i is in {Before Party Formation} if the election took place before the party was formed, and is in {After Party Formation} otherwise.

In addition to differences in returns to politics before and after party formation, there might also be differences in returns to politics for candidates belonging to different parties, because parties exert a different degree of party discipline. For this reason, I also estimate equation 4 separately for each party.

5   Analysis

5.1   Decriptive Statistics and Covariate Balance

To investigate the validity of the RD design, I first show descriptive statistics of the pooled data in table **??**, and then show various pre-determined potential covariates relating to pre-treatment characteristics in table 2. I confine the analysis of covariate balance to a dataset with only first try individuals, who have never been elected before. In section 7, I also provide covariate balance tables for different subsamples.

[Tables 1 and 2 here]

The validity of the regression discontinuity design hinges on several assumptions that are made more plausible in table 2. Most significantly, there has to be some randomness in the treatment assignment mechanism conditional on the subjects having exerted effort to end up with some score (McCrary, 2008; Lee and Lemieux, 2010). In this setting, this means that politicians should have incomplete control over the amount of votes that they obtain: no matter what amount of effort they have exerted, what characteristics they have, conditional on those factors, the allocation of the treatment (i.e. winning the election) should be random. Lee (2008), who also uses a close elections design, brings forth two arguments in favor of this: first, the turnout on election day might vary, for instance because of the weather. Secondly, vote counting is error prone and uncertain, thereby giving precise election outcomes inherent uncertainty.

Caughey and Sekhon (2011) identify a number of covariates that, despite the nature of close elections, tend to differ sharply between winning candidates and losing candidates: for example, the winning party in the district in the previous election tends to be the same as the winning party in the current election, and the winners tends to spend more on the campaign than the losers. Furthermore, in their context (US Post-WWII Senate elections), there are marked differences in incumbency between winners and runners-up. In this context, it is also possible that incumbency has a causal effect on politicians' wealth. In the analysis, we account for this using three different strategies: (i) limiting the analysis to close elections of first-time politicians, (ii) explicitly investigating the influence of tenure in politics, and (iii) limiting the analysis to districts in which the incumbent retired.

Of course, there are also a number of covariates that are determined after the treatment was administered, and there are no *a priori* reasons why these should be balanced. This discussion therefore closely relates to the interpretation of the findings, which we discuss in section **??**. Plausible covariates that could differ between groups, and could be correlated with end-of-life wealth are the number of years lived after treatment, number of times participated in elections after close election, active working period after treatment. In table **??**, I summarize the plausible covariates which we investigate in the empirical analysis.

In table 2, I show the distribution of the covariates in the treatment (Politician) and control (Non-politician) groups. The first rows show the running variable and variables associated with the running variable, i.e. variables where a difference between groups is expected. Indeed, for all these variables, Margin (the running variable), Turnout, and Amount of Votes, there is a statistically significant difference between Politicians and Non-politicians, which is unsurprising, as politicians have won these elections, and should have garnered more votes on average. Secondly, there is also a statistically significant difference between the amount of seats that were contested in a close election from which I have sampled

politicians and from which I have sampled non-politicians. On average, politicians have won in larger districts, whereas non-politicians have lost in slightly smaller districts, and politicians are also more likely to be sampled from regular (scheduled) elections rather than second rounds or other types of elections. On average, politicians also win in districts with a higher electoral threshold than non-politicians. All of these imbalances are likely to be an artifact of the asymmetric nature of sampling, i.e. the filtering of close elections to close elections in which an individual who never became a politician, lost. It is unlikely that any of these have any confounding impact on politicians' wealth.

5.2   Total Returns to First Election

[Table 3 here]

[Figure 3 here]

5.3   Returns to Tenure

In table 4, I display coefficient estimates and significance levels ($\alpha = 0.05$) for so-called ITT (total) effects, and average treatment effects on the treated (ATT) using the recursion defined in equation 3. In the first panel, I display the ITT effects, and in the second panel, I focus on the ATT effects. Standard errors for the estimates of the ATTs are calculated by the delta method.

In figure 4, I explore the robustness of the estimates for the ATT to t^*, the stint for which the estimated ATTs are equal to the ITTs. The results show that the estimated ATT's are very similar to the estimated ATTs, in part due to the incumbency advantages being small.

The results show that there is a convincing positive ceteris paribus effect of a first (4-year) stint in politics on personal wealth. The effect of a second (4-year) stint is less certain, as it is not robust to the inclusion of covariates. The point estimates for all other (ATT) effects are insignificantly different from zero, as are their ITT counterparts. This indicates that the benefits to a political career are accrued in one stint, and that crystallizing a network over more periods does not increase the returns to a political career.

[Table 4 here]

[Figure 4 here]

6 What Determines Returns to Politics?

6.1 Party Organization

Political parties are potentially an example of an institution potentially determining returns to politics. Eggers and Hainmueller (2009) suggest that political parties and associated party discipline can serve as an additional constraint on elected politicians: political party membership can help an individual with political aspirations get elected by providing a platform, whereas in return, the politician must adhere to a certain degree of party discipline.

Empirically, I establish whether, and in what parties, party discipline effects the magnitude of political rents politicians can accrue. Since section 5 established that ATT's and ITT's are very close, I focus this analysis on easier to identify (and arguably more relevant) ITT's only. I do this in two ways. First, I make use of the staggered introduction of political parties: I compare estimates of political rents for politicians before and after political parties for their affiliation were established, and by isolating the wealth premium for politicians according to party by estimating specification 4 separately for politicians who were elected before and after the political party of their affiliation was established. As most politicians chose to join a political party after they were established, I can identify, in almost all cases by revealed preference, which party politicians would have joined if these existed. To do this, I define a variable *Party*, which takes on the value 1 if an individual is elected after 1879 for Protestant-affiliated politicians, after 1885 for liberal politicians, after 1897 for Catholic politicians. The RD coefficient in the two groups then gives an estimate of lifetime political rents for politicians who have entered politics in the era of political parties, rather than before. I repeat this procedure for the politicians who were elected before the political party of their choice was established. In table 5, I show RD estimates for both groups under various sets of covariates, and compute the difference between the two estimators. As the control group for both estimates is shared, the estimates are not independent, and I bootstrap the estimator 1000 times to derive the distribution of the difference, which I use to calculate *p*-values.

[Table 5 here]

Second, I compare political rents between political parties by splitting up the sample according to politicians' political party, and use the non-politicians as a whole as a control group. As non-politicians' political affiliation is unknown in many cases, I use newspaper recommendations as control variables. To statistically investigate the difference in political rents per party, I make use of bootstrapped standard errors and compute test statistics on that basis.

[Table 6 here]

[Figure 5 here]

6.2 Suffrage Extensions and Turnout

Many attempts have been made by politicians to regulate themselves in order to minimize or altogether root out abuse of power, the most prominent and often-used being of course regular elections. Regular

elections are assumed to ensure at least some degree of accountability by providing politicians with an incentive to act in such a way as to increase their chances of being reelected. Elections, however, are not a panacea. Under many circumstances, elections fail to adequately reduce abuse of power by politicians, for example, in the case of failure of relevant information about politicians' performance reaching the general public. Elections are not the only mechanisms that politicians, political theorists and economists have come up with: several other mechanisms aimed at ensuring accountability include term limits, to prevent the same individuals from holding power too long, asset disclosure laws, to force politicians to disclose information about their wealth, its origin and its evolution, the institution of a publicly accessible debate, for example in an assembly or lower house, or a free press to disseminate relevant and trustworthy information. Furthermore, constitutions itself can be thought of as a device to enact constraints on the behavior of politicians as well as the general public. Other institutions that are present in a large number of countries include a senate, or other independent judicial organs that yield various degrees of power to ensure judicial coherence of laws. In many countries, there are also various restrictions on eligibility: it is often the case that a member of parliament cannot simultaneously serve as an executive. Finally, supranational institutions can also be thought of attempts at constraining national politicians' behavior and at ensuring that the rights of certain constituencies are respected.

All of the aforementioned instruments are often thought to play an important role in reducing corruption and improving democratic accountability, but politicians have also used the very same instruments at their disposal to entrench themselves or have otherwise distorted those institutions. Some examples involve delaying, annulling or falsifying elections, or constraining the elections so as to place certain restrictions on candidates belonging to a certain gender or religion, or on a minimum amount of wealth. Propaganda can also be thought of as interference with the purpose of the press, that is, disseminating relevant and trustworthy information.

To empirically investigate whether electoral competition and monitoring of politicians is a key driver of the results, I estimate specification 4 while separating the sample into two windows, where I vary the window over time. This way, I estimate the difference of political rents in elections before significant suffrage expansions, elections after a partially liberalized regime (between 1887 and 1896) and elections after a regime strongly resembling universal suffrage (after 1896). I then use the delta method to compute the distribution of the difference and to test whether coefficients δ are statistically different from each other. The results are displayed in table 7.

[Table 7 here]

In addition, I investigate the temporal pattern of (first-stint) returns to politics in a graphical way. In figure 6, panel A, I plot the returns before and after a cut-off point. These estimates show that rents have a tendency to decrease over time, but that there is no sudden change following the suffrage extension of 1887. Arguably, the suffrage extension in 1896 coincides with the sharp drop in after-period rents from 1897 onward, even though these estimates are never statistically significantly different from each other. In panel B, I also estimate the returns from a moving average of ± 15 years around a particular date. This graph confirms the pattern in panel A, i.e. declining rents over time, until about 1885, following which the estimate stabilizes.

[Figure 6 here]

6.3 Career Paths

I make use of a dataset containing descriptions of the career paths of politicians. These descriptions allow me to map the heterogeneous descriptions of positions politicians held after being elected to three simple categories. Firstly, I first address whether there is a differential political rent effect between politicians who worked in business after having worked in politics and politicians who did not. Secondly, I address whether being active in the colonies, be it in the administration or in business, after one's political career is a determinant of the magnitude of political rents. Thirdly, I inspect whether politicians who continue their career elsewhere in politics garner more political rents than those that do not, thus explicitly identifying through which functions political rents are accumulated.

To this end, I repeat the procedure in the preceding sections. In table **??**, I show the difference in estimated political rents for politicians who (i) were active in business after their political career vs. all others, (ii) were active in the colonies after their political career vs. all others, and (iii) who were active in politics after first being elected in the lower house vs. all others.

[Table 8 here]

In particular, the rent premium might be due to politicians who became a *burgemeester* after being active in national politics. A *burgemeester* is the principal executive of a municipality (a city or several villages), and has autonomy over city and municipality politics, including the budget, without having much oversight. The difference between this function and many other political functions is that this is an explicitly low-level function, whereas most other functions are at the national level.

7 Robustness Checks

7.1 Placebo Tests

[Figure 8 here]

In figure 8, I plot the effect of first-time pooled rents (irrespective of the number of times) as a function of the cut-off point, where 0.0 is the actual estimate. The estimates make clear that the actual effect is the highest in magnitude, and statistically different from zero at the 95% significance level. The plot shows that the placebo estimates, which use a fictional cut-off point in the range of $[-0.15, 0.15]$, are lower in all cases, and are never statistically significant at the 95% level. Most significantly, the plots that switch the cut-off point to a number very close to zero show radically different effects in magnitude, and are statistically insignificantly different from zero. This adds support to the conjecture that the actual estimates reflect the causal impact of a political career on personal end-of-life wealth.

7.2 Sensitivity to RD Parameters

I estimate the results in table 3 using flexible bandwidth and different covariates in table 9.

[Table 9 here]

I estimate the results in table 4 using flexible bandwidths. The results are displayed in table 10.

[Table 10 here]

I estimate the results in table 7 using flexible (2-sided) bandwidths and a different set of covariates.

[Table 11 here]

I display figure 4, but now using flexible bandwidths and including covariates.

[Figure 7 here]

7.3 Covariate Balance

In table 12, I show the covariate balance for the RD analyses of second period rents. Nearly all variables are balanced around the margin, indicated by the absence of significant RD estimates, except for the estimates of political allegiance: after already having been elected once, politicians are more likely to have received a recommendation from a socialist or liberal-oriented newspaper than their runners-up. Even though balanced in the first stint, in the second stint, so conditional on having been elected already, socialists and liberals have an increased tendency to be reelected. As for implications for the analysis of personal wealth, differences in wealth between politicians of different political allegiances are controlled for in all concerned analyses.

[Table 12 Here]

8 Conclusion

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Figures and Tables

Tables

Table 1: Descriptive Statistics

	Mean	SD	Min	Max	N
Panel A: Newspaper Recommendations					
Rec.: Protestant	0.16	0.37	0.00	1.00	6197
Rec.: Liberal	0.19	0.39	0.00	1.00	6197
Rec.: Socialist	0.06	0.24	0.00	1.00	6197
Rec: Catholic	0.18	0.38	0.00	1.00	6197
Panel B: Demographic Characteristics Politicians					
Lifespan	22.41	14.55	−47.36	129.79	4993
Age at Election	49.35	12.33	−61.09	125.06	4711
Year of Death	1902.32	23.31	1837.00	1986.00	4993
Year of Election	1880.61	19.88	1848.00	1918.00	6197
Panel C: Election Characteristics					
Log Turnout	7.98	0.92	5.70	11.85	6197
Log Turnout Previous	7.88	0.92	5.81	11.85	5747
Log Population 1859	9.43	1.87	0.00	12.03	4058
Panel D: Birthplace Characteristics					
Share Protestant	0.62	0.25	0.00	1.00	3879
Share Catholic	0.35	0.26	0.00	1.00	3879
Labor Force Share Agricul.	0.06	0.12	0.00	0.62	4022
Labor Force Share Industry	0.19	0.10	0.00	0.59	4022
Taxes Per Capita 1859	4.06	1.60	0.37	7.27	4008
Taxes Per Capita 1889	4.95	1.61	0.67	10.34	4022
Distance to the Hague	91.17	65.26	0.00	250.00	4700
Panel E: District Characteristics					
Share Protestant	0.64	0.26	0.00	1.00	5780
Share Catholic	0.33	0.27	0.00	1.00	5780
Labor Force Share Agricul.	0.06	0.09	0.00	0.47	5916
Labor Force Share Industry	0.22	0.10	0.00	0.60	5916
Panel F: Ex-Post Characteristics					
Log Deflated Wealth	11.17	2.25	0.00	15.05	2893
Age of Death	71.45	10.27	38.04	99.80	4709
Panel G: Party and Career Characteristics					
Elected After ARP	0.56	0.50	0.00	1.00	6197
Elected After RK	0.30	0.46	0.00	1.00	6197
Elected After Lib	0.56	0.50	0.00	1.00	6197
Liberal	0.30	0.46	0.00	1.00	6197
Protestant	0.24	0.43	0.00	1.00	6197
Catholic	0.09	0.29	0.00	1.00	6197
Panel H: Career Paths					
Profession: Business	0.01	0.11	0.00	1.00	4711
Profession: Mayor	0.05	0.21	0.00	1.00	4711
Profession: Colonial	0.02	0.14	0.00	1.00	4711

Note:

This table shows descriptive statistics for all observations. In panel A, I show newspaper recommendations for each major political faction. Panel B discusses demographic characteristics, and panel C discusses characteristics related to elections. Panels D and E contain birthplace and district characteristics. Panel F contains ex-post variables and Panel G and H contain several variables related to party and career characteristics.

Table 2: Covariate Balance - First Attempts - First Stint

	Margin < 0.2			Margin < 0.05			
	Politicians	Non-Politicians	p-val.	Politicians	Non-Politicians	p-val.	RD Estimate (SD)
Panel A: Newspaper Recommendations							
Rec.: Protestant	0.08	0.07	0.529	0.10	0.09	0.758	-0.176 (0.094)
Rec.: Liberal	0.18	0.17	0.839	0.19	0.17	0.707	0.172 (0.114)
Rec.: Socialist	0.04	0.02	0.164	0.06	0.02	0.184	-0.015 (0.020)
Rec: Catholic	0.11	0.09	0.435	0.12	0.15	0.558	-0.211 (0.103)
Panel B: Demographic Characteristics							
Lifespan	28.04	28.17	0.937	28.99	27.05	0.541	-3.423 (3.809)
Age at Election	44.12	42.67	0.350	43.38	41.75	0.541	4.346 (3.434)
Year of Death	1904.81	1906.75	0.532	1908.83	1913.53	0.435	-4.058 (5.858)
Year of Election	1878.67	1879.55	0.668	1881.43	1880.60	0.816	-3.173 (4.026)
Panel C: Election Characteristics							
Log Turnout	7.90	7.81	0.324	7.94	7.79	0.388	-0.904 (0.297)
Log Turnout Previous	7.81	7.79	0.816	7.87	7.72	0.351	-0.473 (0.231)
Panel D: Birthplace Characteristics							
Log Population 1859	9.56	9.03	0.147	9.79	8.83	0.032**	-0.316 (0.518)
Share Protestant	0.59	0.55	0.465	0.62	0.35	0.013**	0.023 (0.084)
Share Catholic	0.38	0.42	0.440	0.35	0.63	0.010**	-0.006 (0.081)
Labor Force Share Agricul.	0.05	0.03	0.033**	0.05	0.03	0.450	0.019 (0.023)
Labor Force Share Industry	0.20	0.22	0.318	0.20	0.21	0.932	-0.013 (0.034)
Taxes Per Capita 1859	3.95	3.77	0.512	4.28	3.26	0.073*	-0.138 (0.638)
Taxes Per Capita 1889	4.78	4.71	0.785	5.02	4.05	0.073*	0.171 (0.573)
Distance to the Hague	90.58	103.75	0.214	83.13	118.47	0.112	26.572 (17.568)
Panel E: District Characteristics							
Share Protestant	0.57	0.58	0.735	0.59	0.54	0.384	0.053 (0.036)
Share Catholic	0.41	0.40	0.752	0.39	0.45	0.316	-0.034 (0.036)
Labor Force Share Agricul.	0.07	0.07	0.746	0.08	0.09	0.905	0.005 (0.013)
Labor Force Share Industry	0.22	0.22	0.833	0.22	0.23	0.540	-0.013 (0.018)

Note: The table contains means for various sets of variables conditioned on the absolute margin being < 0.2 (left panel) and <0.05 (right panel). The first two columns represent the means for politicians and non-politicians respectively, and the third column shows the p-value of a Welch two-sample t-test. The last column shows the local non-parametric RD estimate, estimated by the procedure in Cattaneo et al. (2019). HC-Robust standard errors are shown between brackets. Significance is indicated by *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table 3: Main RD Estimates - 1st Stint

	First Triers				Second Triers		All Triers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coefficient (ITT)	1.731	1.860	1.992	2.044	1.197	1.322	1.035	0.774
SE (BC)	(0.713)**	(0.540)***	(0.769)**	(0.603)***	(0.934)	(0.733)**	(0.436)***	(0.333)***
Mean DV Treated (1%)	12.849	12.849	12.849	12.849	11.057	11.057	12.214	12.214
Mean DV Control (1%)	10.193	10.193	10.193	10.193	10.795	10.795	10.576	10.576
N (Politicians)	103	103	86	86	65	65	295	295
N (Non-Politicians)	172	172	158	158	182	182	774	774
Bandwidth	Optimal	2 x Optimal	Optimal	2 x Optimal	Optimal	2 x Optimal	Optimal	2 x Optimal

Note: Table showing Bias-corrected standard errors clustered at the Birthplace-level. The first two columns show univariate regressions under the optimal MSE bandwidth, and twice the optimal bandwidth. In columns 3 and 4, selected covariates are added, in particular, covariates that seemed to be unbalanced at the 2% cutoff. In particular, the regression controls for lifespan, times participated in election, birthplace population, birthplace characteristics, age at election, and socialist recommendations. In addition, I control for politicians' lifespan. Columns 5 and 6 focus on second-triers and columns 7 and 8 pool all attempts. *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

Table 4: ATT estimates for different t*

	t* = 4	t* = 5	t* = 6	t* = 7
Panel A: Without Control Variables				
ATT_1	1.054**	1.087**	0.973*	0.877
ATT_2	1.449*	1.482*	1.366	1.268
ATT_3	-0.008	0.022	-0.082	-0.170
ATT_4	-0.329	-0.301	-0.398	-0.479
ATT_5	0	0.256	0.169	0.095
ATT_6	0	0	-1.220	-1.289
ATT_7	0	0	0	-0.872
Panel B: With Control Variables				
ATT_1	1.167**	1.265**	1.296**	1.234**
ATT_2	0.623	0.723	0.756	0.692
ATT_3	-0.174	-0.085	-0.056	-0.113
ATT_4	-0.569	-0.486	-0.459	-0.512
ATT_5	0	0.759	0.784	0.736
ATT_6	0	0	0.341	0.297
ATT_7	0	0	0	-0.569

Note: Table showing effects of stints {1, ..., 7} under different t*. All the ATT coefficients are derived and recursively computed from ITT coefficients, which are in turn estimated using the methodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors are calculated using the delta method. The estimates in panel A are without control variables and the estimates in panel B control for birthplace population, birthplace characteristics, age at election, socialist newspaper recommendations and politicians' lifespan. *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

Table 5: RD Estimates of Political Rents according to Party Establishment

	No Covariates		With Covariates	
	After	Before	After	Before
	(1)	(2)	(3)	(4)
Panel A: First-try, first-period returns				
Coefficient	0.744	1.100	0.543	0.689
SE (BC)	(0.551)*	(0.431)***	(0.499)	(0.407)*
N Treated	196	308	165	254
N Control	385	803	343	732
Mean Treated (1%)	12.336	12.311	12.405	12.506
Mean Control (1%)	10.770	10.666	10.937	11.004
Covariates	No	No	Yes	Yes
Panel B: Second period returns				
Coefficient	0.475	1.366	0.549	0.399
SE (BC)	(0.443)	(0.712)**	(0.425)	(0.617)
N Treated	152	232	124	194
N Control	86	162	73	129
Mean Treated (1%)	11.947	11.935	11.612	11.709
Mean Control (1%)	11.214	9.935	11.033	10.698
Covariates	No	No	Yes	Yes

Note:

The table shows RD estimates using the MSE-optimal bandwidth (Cattaneo et al., 2019). The Dependent Variable is Log(1+Personal Wealth). I report bias-corrected standard errors. Panel A estimates the returns for the first-triers for the first stint, panel B estimates the returns for the second stint, irrespective of the number of tries. Columns (1) and (3) contain estimates for the post-party period, and columns (2) and (4) for the pre-party period. Columns (1) and (2) contain estimates with no covariates, and columns (3) and (4) control for potential imbalances in lifespan, age, newspaper recommendations and a time trend. *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table 6: RD Estimates by Party

	Protestants		Catholics		Liberals	
	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient	1.663	2.325	0.148	0.403	1.604	1.406
SE (BC)	(0.826)**	(0.791)***	(0.629)	(0.642)	(0.545)***	(0.499)***
N Treatment	81	81	56	56	184	184
Covariates	No	Yes	No	Yes	No	Yes

Note: Table showing Bias-corrected and Robust standard errors clustered at the Birthplace-level, estimated under the optimal MSE bandwidth per party. Columns (1), (3) and (5) are without covariates, whereas in the remaining columns, I control for age, lifespan and newspaper recommendations. *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

	Without Covariates		With Covariates		Second Rents	
	(1)	(2)	(3)	(4)	(5)	(6)
Before and After Suffrage Extension - 1887						
Coefficient (ITT)	0.994	0.914	0.835	1.109	1.710	0.617
SE (BC)	(0.428)***	(0.961)	(0.462)**	(1.062)	(1.103)*	(0.687)
Mean DV Treated (1%)	12.187	12.300	12.313	12.576	11.933	11.939
Mean DV Control (1%)	10.562	10.677	10.939	11.918	8.701	10.962
N (Politicians)	222	127	189	106	169	91
N (Non-Politicians)	649	176	612	162	118	57
Period	Before	After	Before	After	Before	After
Before and After Suffrage Extension - 1896						
Coefficient (ITT)	1.113	-0.342	1.124	-0.350	1.391	1.820
SE (BC)	(0.400)***	(0.967)	(0.396)***	(0.940)	(0.780)**	(1.548)
Mean DV Treated (1%)	12.370	10.734	12.370	10.734	12.006	11.650
Mean DV Control (1%)	10.649	9.436	10.649	9.436	9.796	11.320
N (Politicians)	267	82	267	82	201	59
N (Non-Politicians)	728	97	728	97	144	31
Period	Before	After	Before	After	Before	After
High and Low Turnout						
Coefficient (ITT)	1.493	0.612	1.210	0.485	2.383	-0.220
SE (BC)	(0.563)***	(0.727)	(0.542)**	(0.809)	(1.106)***	(0.738)
Mean DV Treated (1%)	12.032	12.303	12.122	12.673	12.394	11.685
Mean DV Control (1%)	10.465	10.650	10.987	10.650	9.114	11.451
N (Politicians)	177	128	149	109	131	104
N (Non-Politicians)	428	302	396	290	93	70
Turnout	High	Low	High	Low	High	Low

Note: Table showing Bias-corrected and Robust standard errors clustered at the Birthplace-level. The dependent variable in all cases is Log(1+Wealth). All effects are estimated under the MSE-optimal bandwidth. Panel A shows the differences in political rents before and after the suffrage extension in 1887. Panel B shows the differences in returns to politics before and after the suffrage extension in 1896. Panel C shows the differences in returns between elections with a positive turnout difference vis-a-vis elections with a negative turnout differences in comparison to the preceding general election. In panels A and C, I control for age, lifespan, newspaper recommendations and economic composition of the district and the politicians' birthplace. In panel B, due to data constraints, I control for age, lifespance and newspaper recommendations. *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

Table 8: RD Estimates of Effect on Career Paths

	Business	Politics	Colonial	Business	Politics	Colonial
	Without Covariates			With Covariates		
	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient (ITT)	0.006	-0.008	0.004	0.011	-0.021	-0.005
SE (BC)	(0.023)	(0.029)	(0.026)	(0.029)	(0.029)	(0.027)
Mean DV Treated (1%)	0.081	0.027	0.054	0.100	0.033	0.033
Mean DV Control (1%)	0.022	0.022	0.043	0.025	0.000	0.050
N (Politicians)	635	635	635	531	531	531
N (Non-Politicians)	1190	1190	1190	1052	1052	1052

Note: Table showing the effect of being elected into politics on three future career paths: taking up a position in finance (business), continuing in non-lower house politics (as a mayor), and taking up a career in the colonies. Bias-corrected and Robust standard errors clustered at the Birthplace-level. All effects are estimated under the MSE-optimal bandwidth. I control for age, lifespan, newspaper recommendations and economic composition of the district and the politicians' birthplace. *: p < 0.10, **: p < 0.05, ***: p < 0.01.

Table 9: Robustness to Main RD Estimates - 1st Stint

	First Triers				Second Triers		All Triers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coefficient (ITT)	1.731	1.860	2.085	2.089	2.135	1.483	1.021	0.719
SE (BC)	(0.713)**	(0.540)***	(0.784)**	(0.610)***	(0.939)***	(0.737)**	(0.453)***	(0.349)**
Mean DV Treated (1%)	12.849	12.849	12.849	12.849	11.057	11.057	12.214	12.214
Mean DV Control (1%)	10.193	10.193	10.193	10.193	10.795	10.795	10.576	10.576
N (Politicians)	103	103	84	84	59	59	277	277
N (Non-Politicians)	172	172	148	148	168	168	721	721
Bandwidth	Optimal	2 x Optimal	Optimal	2 x Optimal	Optimal	2 x Optimal	Optimal	2 x Optimal

Note: Table showing Bias-corrected standard errors clustered at the Birthplace-level. The first two columns show univariate regressions under the optimal MSE bandwidth with the option *msecomb2*, and twice the optimal bandwidth. In columns 3 and 4, selected covariates are added, an alternative selection to the covariates in the main results. In particular, the regression controls for district religious share, birthplace population, birthplace religious share, district GDP, lifespan and birthplace labor force composition. Columns 5 and 6 focus on second-triers and columns 7 and 8 pool all attempts. *: p < 0.10, **: p < 0.05, ***: p < 0.01.

Table 10: ATT estimates for different t*

	t* = 4	t* = 5	t* = 6	t* = 7
Panel A: Without Control Variables				
ATT_1	1.054**	1.087**	0.973*	0.877
ATT_2	1.449*	1.482*	1.366	1.268
ATT_3	-0.008	0.022	-0.082	-0.170
ATT_4	-0.329	-0.301	-0.398	-0.479
ATT_5	0	0.256	0.169	0.095
ATT_6	0	0	-1.220	-1.289
ATT_7	0	0	0	-0.872
Panel B: With Control Variables				
ATT_1	0.978*	1.059*	1.091*	1.007*
ATT_2	0.632	0.714	0.747	0.662
ATT_3	-0.198	-0.124	-0.095	-0.171
ATT_4	-0.659	-0.590	-0.563	-0.634
ATT_5	0	0.624	0.648	0.584
ATT_6	0	0	0.341	0.282
ATT_7	0	0	0	-0.758

Note:

 Table showing coefficients effects of stints {1, ..., 7} under different t*. All the ATT coefficients are derived and recursively computed from ITT coefficients, which are in turn estimated using the methodology in (Cattaneo et al., 2019) using the *msecomb2* bandwidth selector. Standard errors are calculated using the delta method. The estimates in panel A are without control variables and the estimates in panel B control for birthplace population, birthplace characteristics, age at election, socialist newspaper recommendations and politicians' lifespan. *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

Table 11: Robustness to RD Estimates - Suffrage Extension and Turnout

	Without Covariates		With Covariates		Second Rents	
	(1)	(2)	(3)	(4)	(5)	(6)
Before and After Suffrage Extension - 1887						
Coefficient (ITT)	1.050	0.946	0.958	0.672	1.699	0.655
SE (BC)	(0.430)***	(0.931)	(0.487)**	(1.065)	(1.102)*	(0.693)
Mean DV Treated (1%)	12.187	12.300	12.015	12.300	11.933	11.939
Mean DV Control (1%)	10.562	10.677	10.468	10.677	8.701	10.962
N (Politicians)	222	127	178	99	169	91
N (Non-Politicians)	649	176	578	143	118	57
Period	Before	After	Before	After	Before	After
Before and After Suffrage Extension - 1896						
Coefficient (ITT)	1.187	-0.293	1.199	-0.319	1.367	1.565
SE (BC)	(0.415)***	(0.983)	(0.415)***	(0.940)	(0.766)**	(1.394)
Mean DV Treated (1%)	12.370	10.734	12.370	10.734	12.006	11.650
Mean DV Control (1%)	10.649	9.436	10.649	9.436	9.796	11.320
N (Politicians)	267	82	267	82	201	59
N (Non-Politicians)	728	97	728	97	144	31
Period	Before	After	Before	After	Before	After
High and Low Turnout						
Coefficient (ITT)	1.488	0.495	1.235	0.333	2.282	-0.267
SE (BC)	(0.550)***	(0.761)	(0.557)**	(0.884)	(1.045)***	(0.739)
Mean DV Treated (1%)	12.032	12.303	12.032	12.303	12.394	11.685
Mean DV Control (1%)	10.465	10.650	10.465	10.650	9.114	11.451
N (Politicians)	177	128	144	107	131	104
N (Non-Politicians)	428	302	377	269	93	70
Turnout	High	Low	High	Low	High	Low

Note:

Table showing Bias-corrected and Robust standard errors clustered at the Birthplace-level. The dependent variable in all cases is Log(1+Wealth). All effects are estimated under the MSE-optimal bandwidth under the option *msecomb2*. Panel A shows the differences in political rents before and after the suffrage extension in 1887. Panel B shows the differences in returns to politics before and after the suffrage extension in 1896. Panel C shows the differences in returns between elections with a positive turnout difference vis-a-vis elections with a negative turnout differences in comparison to the preceding general election. In panels A and C, I control for economic and religious composition of the politicians' district and the politicians' birthplace, as well as for lifespan. In panel B, due to data constraints, I control for age, lifespan and newspaper recommendations. *: p < 0.10, **: p < 0.05, ***: p < 0.01.

Table 12: Covariate Balance - Second Stint

	Margin < 0.2			Margin < 0.05			
	Politicians	Non-Politicians	p-val.	Politicians	Non-Politicians	p-val.	RD Estimate (SD)
Panel A: Newspaper Recommendations							
Rec.: Protestant	0.19	0.17	0.538	0.22	0.11	0.058*	0.062 (0.101)
Rec.: Liberal	0.17	0.23	0.151	0.13	0.16	0.682	0.247 (0.100)**
Rec.: Socialist	0.04	0.05	0.646	0.03	0.05	0.500	0.054 (0.030)*
Rec: Catholic	0.23	0.20	0.605	0.22	0.13	0.168	0.107 (0.094)
Panel B: Demographic Characteristics							
Lifespan	22.69	21.75	0.504	23.54	23.17	0.868	-1.520 (3.400)
Age at Election	47.70	49.61	0.086*	46.76	50.24	0.038**	0.008 (2.659)
Year of Death	1901.67	1900.21	0.580	1901.08	1896.84	0.328	2.597 (5.257)
Year of Election	1879.00	1878.58	0.842	1877.82	1874.05	0.278	3.186 (3.696)
Panel C: Election Characteristics							
Log Turnout	7.94	7.86	0.441	7.95	7.84	0.456	0.042 (0.189)
Log Turnout Previous	7.80	7.77	0.705	7.75	7.64	0.490	0.011 (0.263)
Panel D: Birthplace Characteristics							
Log Population 1859	9.40	9.06	0.193	9.23	9.14	0.836	0.860 (0.696)
Share Protestant	0.58	0.60	0.550	0.56	0.61	0.338	0.052 (0.060)
Share Catholic	0.38	0.37	0.691	0.42	0.36	0.310	-0.049 (0.066)
Labor Force Share Agricul.	0.05	0.05	0.600	0.06	0.07	0.574	0.025 (0.023)
Labor Force Share Industry	0.19	0.18	0.870	0.19	0.19	0.773	0.010 (0.033)
Taxes Per Capita 1859	3.93	4.02	0.648	3.64	4.23	0.055*	-0.039 (0.396)
Taxes Per Capita 1889	4.84	4.82	0.924	4.62	5.17	0.074*	-0.058 (0.415)
Distance to the Hague	91.71	82.95	0.203	100.53	76.70	0.040**	-18.075 (15.643)
Panel E: District Characteristics							
Share Protestant	0.62	0.65	0.375	0.60	0.67	0.177	-0.011 (0.040)
Share Catholic	0.35	0.33	0.445	0.38	0.32	0.266	0.011 (0.042)
Labor Force Share Agricul.	0.06	0.06	0.906	0.06	0.08	0.090*	0.000 (0.015)
Labor Force Share Industry	0.22	0.24	0.061*	0.23	0.24	0.735	-0.037 (0.018)

Note: The table contains means for various sets of variables conditioned on the absolute margin being < 0.2 (left panel) and <0.05 (right panel). The first two columns represent the means for subsequent politicians and non-politicians respectively, and the third column shows the p-value of a Welch two-sample t-test. The last column shows the local non-parametric RD estimate, estimated by the procedure in Cattaneo et al. (2019). HC-Robust standard errors are shown between brackets. Significance is indicated by *: p < 0.1, **: p < 0.05, ***: p < 0.01.
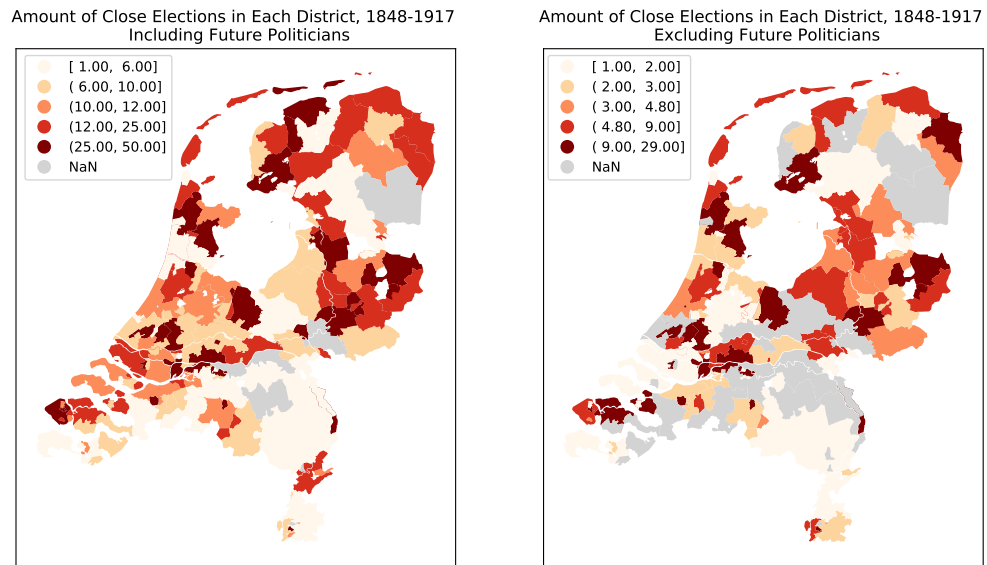
Figures
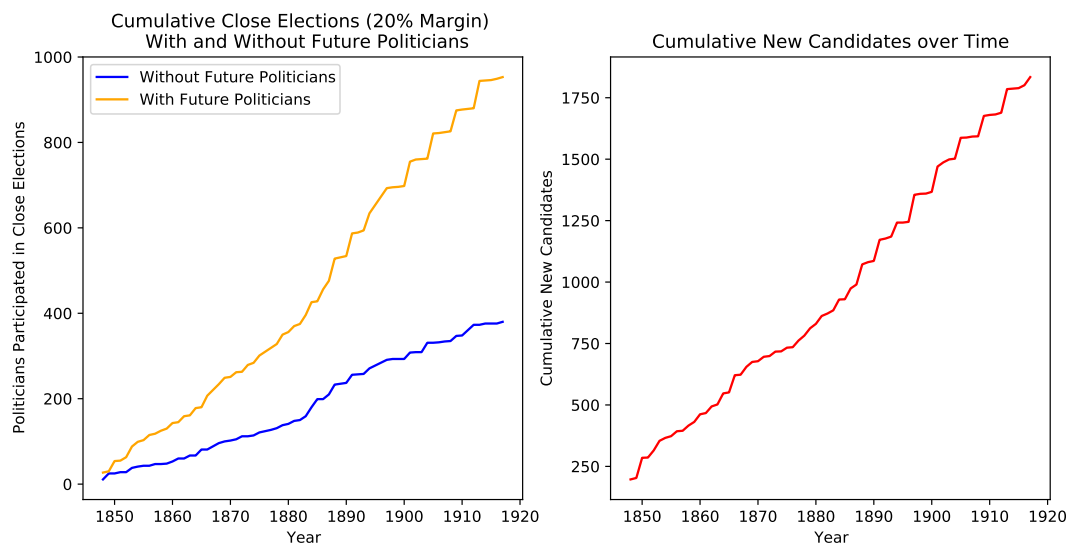

Figure 1: Close Elections Per District
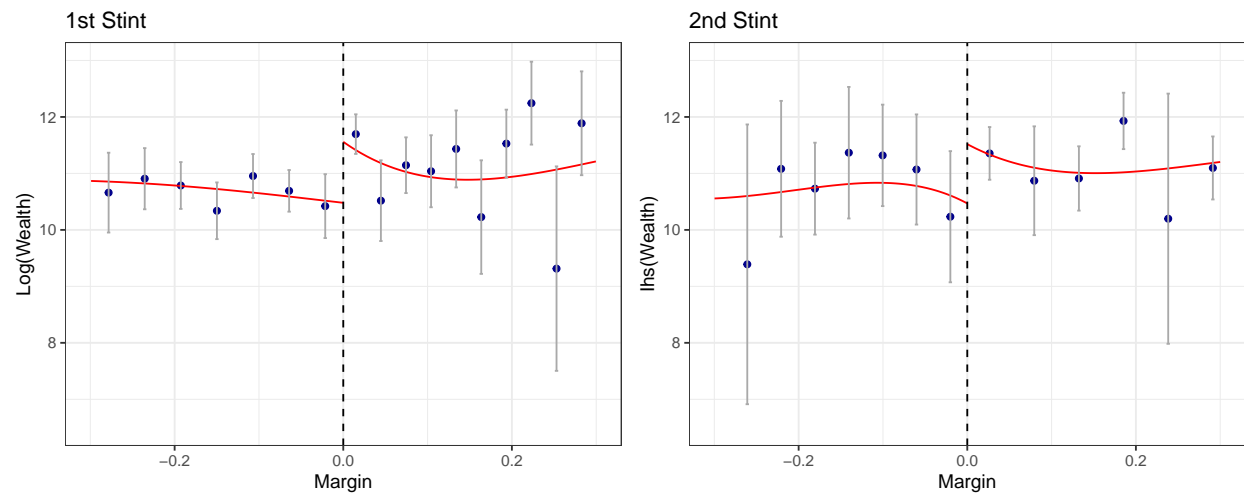
Figure 2: Close Elections over Time

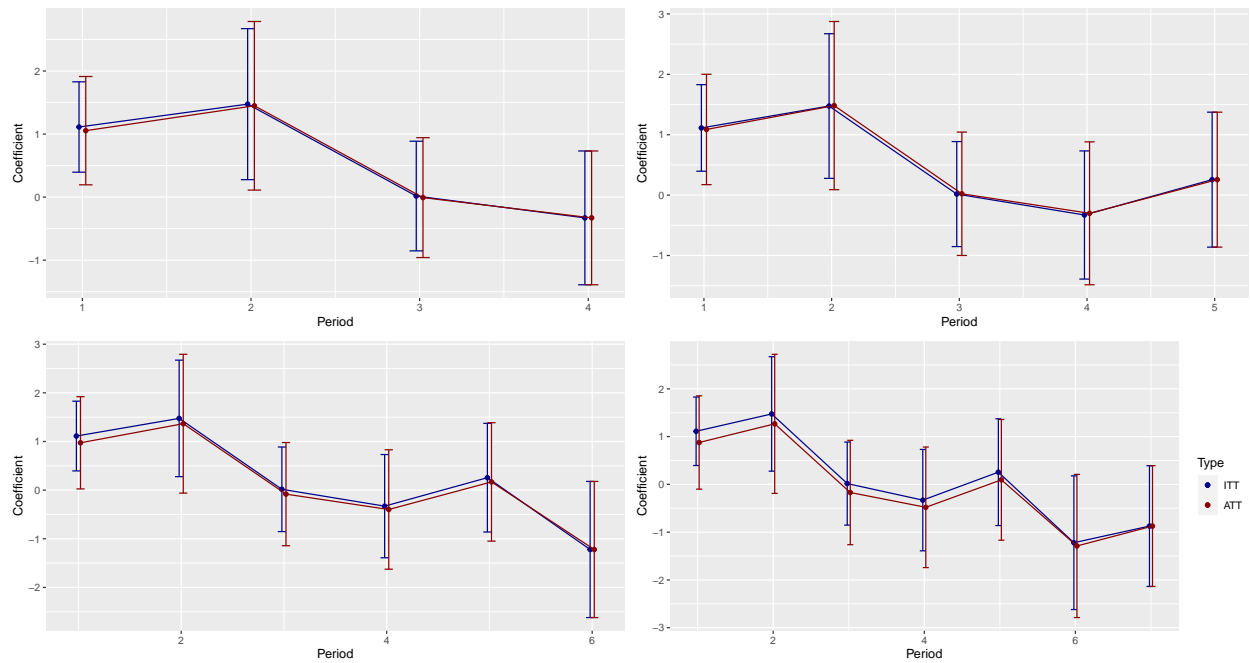Figure 3: Estimates of Returns to Politics
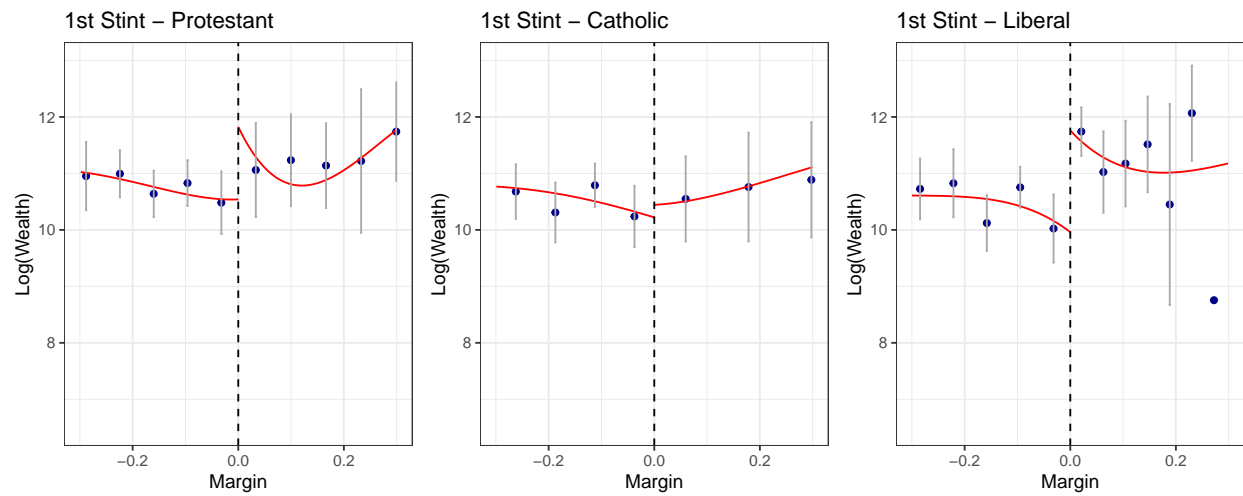
Figure 4: ITTs and ATTs for different t^*
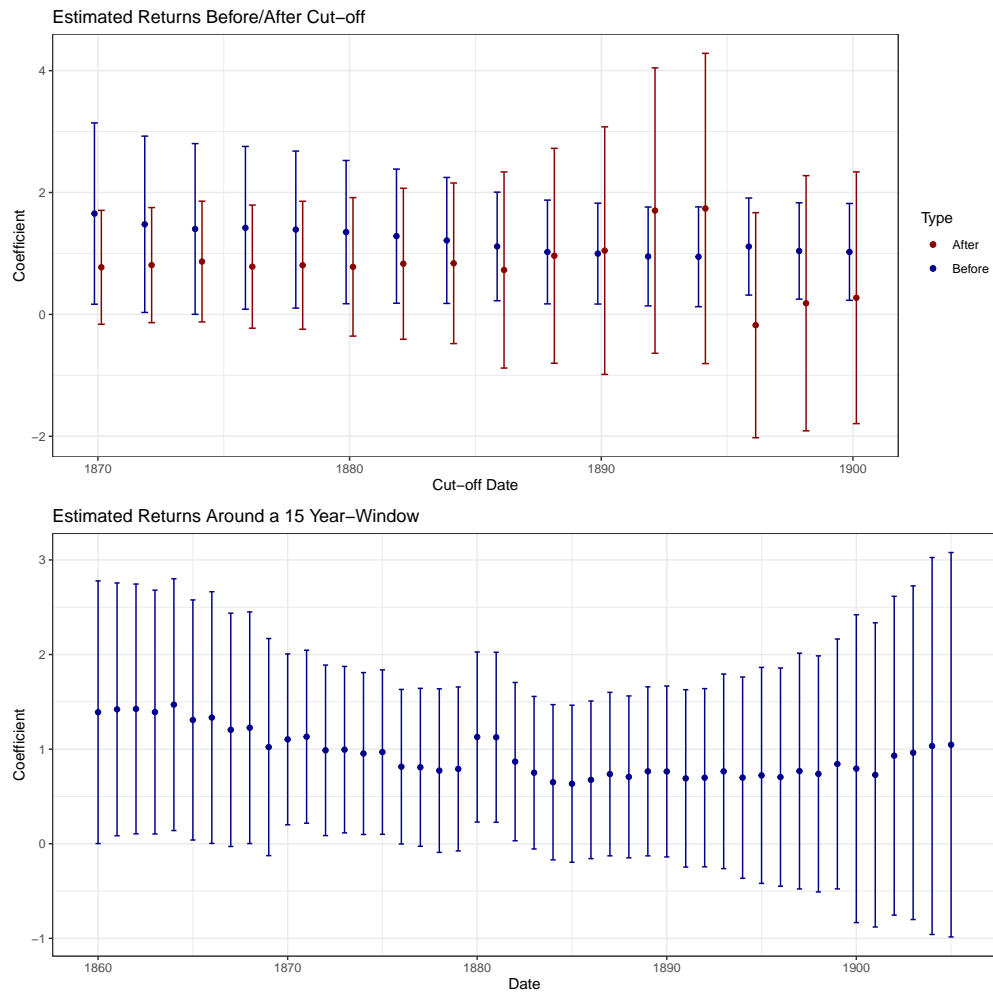
Figure 5: Estimates of Returns per Party
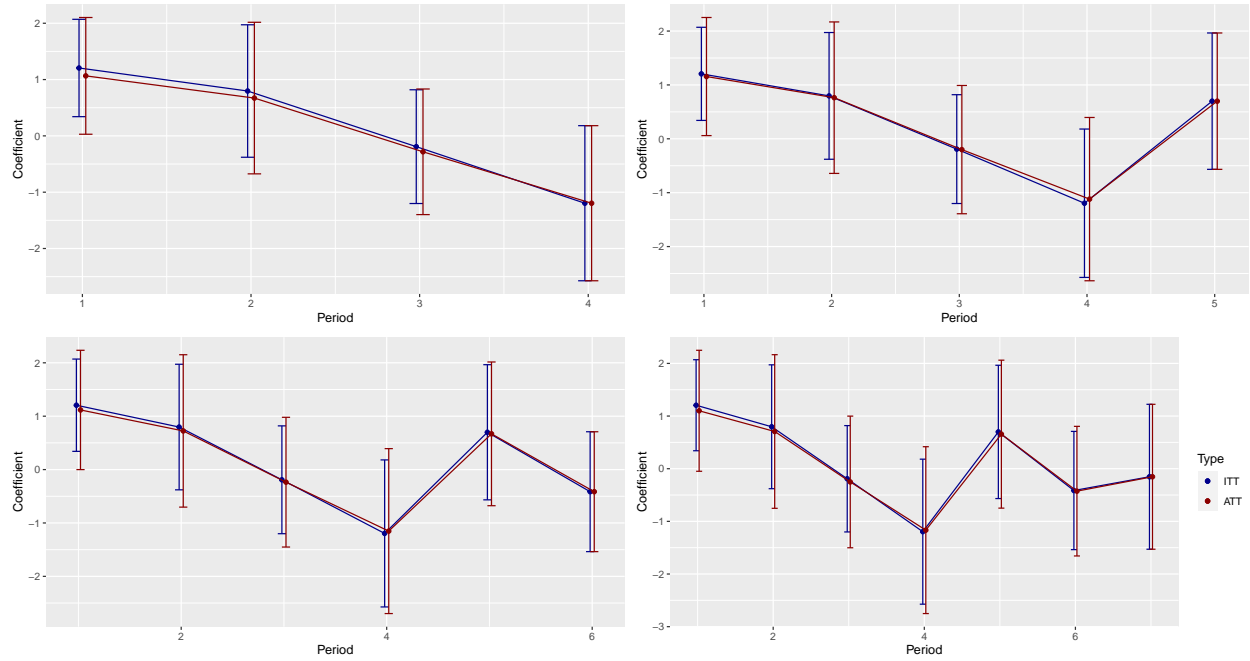
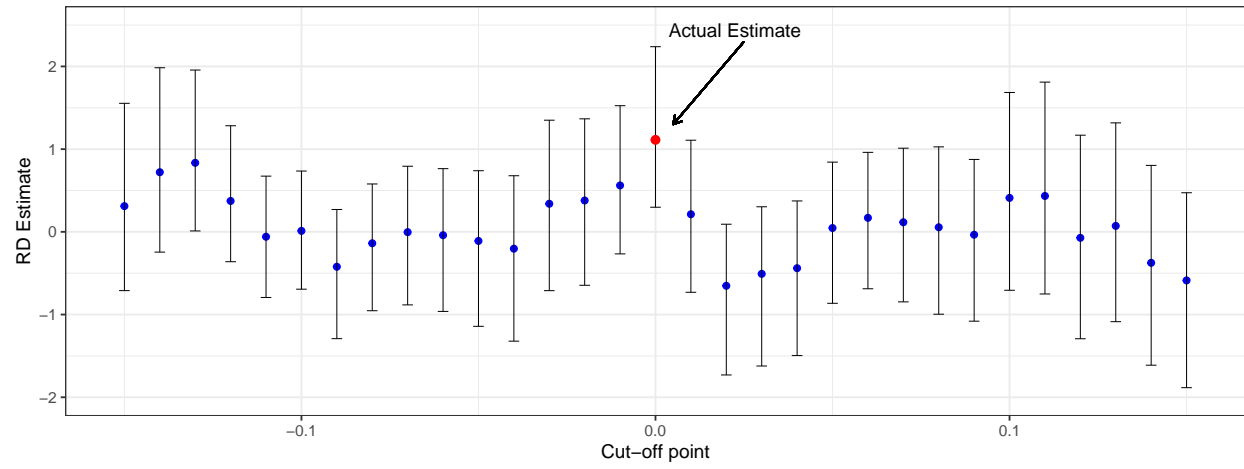Figure 6: Estimates of Returns - Temporal Patterns

Figure 7: Robustness to t^*, flexible bandwidth and with covariates

Figure 8: Placebo Test for 1st Stint