Dynamic Returns to Political Tenure*

Bas Machielsen†

December 20, 2023

For the latest version of the paper, please click here.

Abstract:

Economists frequently assert that politicians derive financial returns from a political career, but these returns can be obscured by the varying duration of political careers. In this study, I estimate the returns associated with successive mandates in the Lower House, capitalizing on the repetitive treatment assignment in Dutch district-level elections from 1848-1917. Based on newly-acquired data from probate inventories, I construct a measure of personal wealth for a subset comprising newly-elected politicians and their unelected contenders. Employing a dynamic regression discontinuity framework, I establish that the financial benefits accruing to politicians are predominantly concentrated within the initial phase of their political activity. The findings underscore that politicians elected for a single term exhibit an accelerated wealth accumulation rate of about 5 percentage points per annum higher than their narrowly defeated counterparts. I also explore various potential mechanisms, revealing a significant role played by political parties in curbing politicians' capacity to derive financial gains from their political trajectories.

JEL Classifications: N14, D72, H71

*I am grateful to Oscar Gelderblom, Abe De Jong, Matthijs Korevaar, Kim Oosterlinck, Pierre-Guillaume Méon and Bob Rijkers for many useful comments and suggestions. I am also indebted to seminar participants at the Université Libre de Bruxelles, Erasmus University Economic History Seminar, the 2022 European Historical Economics Society Conference, the 2022 KSV New Paper Sessions and the 2023 Utrecht Applied Economics Seminar for many helpful remarks and suggestions on an earlier draft of this paper. I also want to thank Jesper van der Most and Margot Hoogerwerf for useful research assistance.

†Utrecht University School of Economics, Utrecht University, Kriekenpitplein 21-22, 3584 EC Utrecht, the Netherlands; e–mail: a.h.machielsen@uu.nl

1 Introduction

Elected officials are conventionally presumed to prioritize the interests of their constituents (Persson and Tabellini, 2002; Duggan and Martinelli, 2017). However, this assumption is often only partially accurate in practical scenarios. There exists a pervasive suspicion that politicians may exploit their positions for personal gain or enact policies that run contrary to the interest of those they represent. Over time, numerous endeavors have been undertaken to regulate the conduct of politicians[1]. Despite these efforts, empirical evidence from multiple studies attests to the persistence of politicians pursuing self-serving objectives. The body of literature extensively documents distinct forms of benefits accruing to politicians, extending beyond their formal remuneration. While the majority of studies focus on delineating private gains in monetary terms (Svaleryd and Vlachos, 2009; Eggers and Hainmueller, 2009; Amore and Bennedsen, 2013; Fisman et al., 2014), other scholarly works identify subtler forms of personal returns. These encompass instances where politicians prioritize their ideological beliefs over electoral preferences (Peltzman, 1984; Mian et al., 2010) or exhibit favoritism toward family members in decision-making processes (Folke et al., 2017).

Nevertheless, there is no clear consensus in interpreting these empirical observations. Some scholars argue that the benefits reaped from a political career predominantly materialize during the tenure itself (Amore and Bennedsen, 2013; Fisman et al., 2014; Bourveau et al., 2021). Conversely, an opposing perspective asserts that these benefits might crystallize over an extended timeframe (Querubin and Snyder Jr, 2009). In addition to financial returns to politics, the crystallization of benefits might manifest itself over a longer timeframe as nepotism (Dal Bó et al., 2009) extending to other individuals, such as relatives (Fafchamps and Labonne, 2017; Folke et al., 2017). Moreover, the factors influencing the magnitude of returns to political engagement remain ambiguous. Eggers and Hainmueller (2009) propose that the organizational structure of political parties could significantly influence the degree to which politicians prioritize personal interests. Contrarily, Fisman et al. (2014) discern differential returns to politics across various Indian states characterized by distinct levels of corruption. Additionally, Querubin et al. (2011) suggest that the size of the government and scrutiny by the media might influence the returns associated with a political career. However, validating these explanations poses challenges as most existing research relies on static frameworks to estimate the returns to politics.

This study takes a dynamic perspective and explicitly investigates the temporal component of the returns to politics using the case of the Netherlands from 1848-1917. This approach facilitates the comprehension of how these benefits materialize and potentially evolve over time, shedding light on the shifting nature of political incentives and behaviors across different stages of a politician's career trajectory. I make use of close elections to establish the existence and magnitude of financial returns to politics using a dynamic regression discontinuity strategy (Cellini et al., 2010). The Netherlands employed a district

[1] (see, for instance, Djankov et al., 2010, for a comprehensive overview)

system (De Jong, 1999). In each district, a small number of candidates took part, and these elections were frequently hotly contested. This setting enables me to to tie the returns to politics to several changing institutions, most notably, the establishment of political parties (De Jong, 2001). Exploring mechanisms, I exploit detailed data on the career paths of politics, concentrating on whether Lower House politics serves to facilitate the appearance of 'career politicians' and 'political careers' in the spirit of Mattozzi and Merlo (2008). I also focus on whether political parties are able to curb the returns to politics for individual politicians, by making use of data on newspaper recommendations for candidates, which allow me to identify political allegiance before political parties were established. Finally, I focus on the role of electoral monitoring in disciplining politicians' ability to extract rents (Barro, 1973; Ferejohn, 1986; Duggan and Martinelli, 2017).

Methodologically, I employ a dynamic regression discontinuity design to leverage the repeated quasi-random treatment assignment stemming from close electoral outcomes. This design considers not only individuals elected for the first time but also those re-elected an equivalent number of times previously. To ascertain the validity of treatment assignment, I compile a substantial dataset encompassing comprehensive details regarding candidates' backgrounds, origins, political inclinations, demographics, and the characteristics of districts where closely contested elections occurred. This meticulous approach enables a robust estimation of the returns associated with successive tenures in political office. However, interpreting the analysis is complicated by the presence of incumbency advantages (Lee, 2008). The estimated overall impact of being elected on personal wealth encompasses both an immediate (*ceteris paribus*) effect, and the incumbency advantages multiplied by subsequent *ceteris paribus* effects. Employing a methodology akin to that of Cellini et al. (2010), I derive iterative estimates of the *ceteris paribus* effects from the overall estimated effects across each political term, alongside the incumbency advantages. These derived estimates offer an interpretation akin to a 'marginal return curve' depicting the successive benefits associated with holding multiple terms in political office.

The analysis shows that the benefits reaped from engaging in politics primarily manifest during the initial term of office. Politicians securing their first mandate by a narrow margin exhibited significantly greater wealth by the end of their lives compared to politicians who narrowly lost their inaugural election. This additional wealth amounted to 100,000 guilders, a sum equivalent to eight times the salary of a cabinet minister. In relative terms, this translated to an additional five percentage points in wealth accumulation per annum for winners of closely contested elections—an effect size close to the effect size found by Fisman et al. (2014) in contemporary India. These findings remain robust upon integrating covariates, exploring various parameter specifications, and enduring scrutiny through multiple placebo tests. As the analysis extends into returns to subsequent terms, the results lose statistical significance. The point estimates hover around zero in numerous instances, suggesting minimal or negligible returns associated with second or subsequent tenures in the Lower House. This outcome aligns with the perspective positing that engagement in politics might furnish exhaustive human capital, while also endorsing the notion that politicians engaging in rent-

seeking behavior may amass rents exclusively during a single term. These results challenge theories advocating for a consistent marginal return curve associated with political tenure (Persson and Tabellini, 2002; Caselli and Morelli, 2004; Baltrunaite, 2020; Bourveau et al., 2021). Instead, they underscore the dynamic nature of the benefits derived from political office, emphasizing a concentration of returns within the initial term rather than a uniform trajectory across successive tenures.

Afterwards, I explore potential mechanisms leading to this result. The analysis reveals a noteworthy trend: the inception of political parties significantly diminishes the returns associated with political office, to the extent that the estimated effect approaches zero. Notably, these outcomes are not propelled by shifts in individuals' decisions to enter politics (Besley, 2005), as there exists no discernible correlation between electoral success and such decisions. Similarly, there is no indication of an elevated likelihood for individuals to pursue more financially rewarding business careers post-politics, either pre- or post-establishment of political parties. Excluding an increased propensity for entry into the more technically oriented Upper House, there is scant evidence supporting the notion that service in the Lower House serves as a springboard to distinct, potentially more lucrative political roles. This negation dismisses explanations suggesting that returns to politics are accrued only indirectly, subsequent to a political career. Moreover, this implies that political careers did not hold substantial value for prospective employers, thereby undermining the plausibility of a human capital-based rationale (cf. Diermeier et al., 2005; Mattozzi and Merlo, 2008). The results also seem to be driven by districts and observations in which electoral monitoring is generally lower.

In tandem with several European nations, the Netherlands underwent significant transformations during the late 19th and early 20th centuries (Przeworski, 2009). Initially an absolute monarchy in the early 19th century, the country transitioned to constitutional monarchy and parliamentary oversight following liberal reforms in 1848 (Aerts, 2018). However, suffrage in vital governmental bodies during this period was severely restricted—limited to males meeting specific tax payment criteria, despite nominal eligibility being unconstrained (Van Der Kolk et al., 2018). Over subsequent decades, relentless campaigns by politicians and activists culminated in the achievement of universal suffrage. Understanding how politicians' self-interests interacted with their decision-making processes could offer fresh insights into the rationale behind extending the franchise (Lizzeri and Persico, 2004; Besley, 2005; Becker and Hornung, 2020).

Simultaneously, and also in tandem with international developments, the era witnessed the emergence and ascent of political parties. As schisms between liberal and Christian parliamentary factions widened, politicians and politically aware citizens coalesced into electoral associations (*Kiesvereenigingen*), swiftly evolving into formal political parties (De Jong, 1999). The inaugural political party, the Anti-Revolutionary Party, was established in 1879, followed by its liberal counterpart, the Liberal Union, in 1885 (De Jong, 2001; Voerman, 1989). The amalgamation of Catholic electoral associations occurred somewhat later, in 1904. Preceding this period, candidates aligned with specific political agendas usually gar-

nered support from newspapers (De Jong, 1999). The influence of political parties, evident in their ability to enforce party discipline, might significantly impact political voting behavior, potentially restraining financial returns tied to politics (see e.g. Aidt and Franck, 2015, 2019; Becker and Hornung, 2020). The sequential establishment of political parties allows for an empirical disentanglement of the effects of party discipline while holding constant political affiliation. This offers an opportunity to illuminate the transformative role of political parties in reshaping the political landscape.

The remainder of this study is structured as follows. First, in section 2, I discuss the historical background by focusing on the development of the district system and political party formation. 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 5.2, I investigate various alternative explanations. After concluding in section 6, I provide various robustness checks in Appendix A.

2 Historical Background

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 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. Every two years, half of the seats were up for contest. In almost all cases, districts featured two seats, and in each election, one seat was up for election (De Jong, 1999). Thus, a Lower House member was elected for four years.

Candidacy was individual-based: initially, political parties were wholly absent. After political differences became more salient in the 1860's and 1870's (De Jong, 2001), electoral associations (Dutch: *Kiesvereenigingen*) started to play a role: these associations were the precursors of political parties. Gradually, these associations formed explicit political parties with a clear ideology, based around the cultural-religious landscape of the Netherlands: Protestant, Catholic, Liberal parties became the largest political actors of the country.

The elections themselves were determined following an absolute majority logic. When no candidate in the first round obtained an absolute majority, a second round would be organized, with the two candidates with the highest amount of votes (De Jong, 1999). Candidates would remain in office for a four year term, but a constitutional provision, which remained in force for the entire period, stipulated that members of parliament who would accept a second remunerated function in government lost membership by default. They

could, however, stand for reelection (De Jong, 1999; Loots, 2004). Apart from untimely death of a Lower House member, this was the principal reason that some elections occurred at times other than the officially stipulated election moments. In addition, there was a population-dependent electoral threshold, and elections were nullified in case of insufficient turnout, irrespective of the outcome.

The precise mapping from municipality (the lowest-level administrative unit of the Netherlands) to district was stipulated in the electoral law (*Kieswet*), with the stated objective that each district, and consequently each representative, represented 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, making it more and more difficult to apply this rule. The lawmakers responded by increasing the number of seats, creating and changing the composition of districts: the number of Lower House seats increased 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 to 1887, when it was fixed at 100. The constitutional revision in 1887 also implied that the Lower House members were elected at the same time, while keeping intact the 4-year term, and that there would be one district for one representative, implying the break-up of previously large districts into various smaller ones, e.g. Amsterdam or Rotterdam. At the same time, as the population continued to grow, the reallocation of districts became more difficult, and 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 (De Jong, 1999).

While in principle, candidacy was open to any male aged thirty or older throughout the period, suffrage rights were severely restricted. The 1848 Constitution left suffrage and eligibility requirements to the electoral law *Kieswet*, which in turn stipulated that men who paid more taxes than a certain threshold, called a *census* (De Vries, 1971; De Haan, 2003). This census, in turn, was determined on a municipal level. In some municipalities, such as Amsterdam, where the population was relatively rich, the threshold was higher, and the censuses were generally coordinated to be such that about 1 in 3,000 individuals was enfranchised. Van Der Kolk et al. (2018) note that about 85,000 men on a population of over 2.5 million had the right to active suffrage for both upper and Lower Houses. The constitutional changes and changes in the electoral law in 1887 in effect encompassed a lowering of census requirements, which was the principal mechanism through which a larger share of the population was enfranchised (about 25% after 1887 according to Van Der Kolk et al. (2018)), although besides taxes, there were various other means of acquiring the right to vote. The changes in the electoral law in 1896 added many more grounds other than income as a criterion to be enfranchised, such as having a particular set of degrees, paying a certain amount of rent or having a savings account. De Jong (1999) notes that about 48,6%

of all Dutch men aged 25 and over were enfranchised by 1900.

Throughout the period from 1848 to 1917, 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, 2001). Political parties were preceded by *Kiesvereenigingen*, electoral unions, of enfranchised individuals with (generally) the same political orientation, intending to coordinate their voting behavior. *Kiesvereenigingen* were a way to improve the dissemination of information and aggregate electoral preferences in a more effective way. A special role in information provision was taken up by national newspapers: the editorial boards of several large national newspapers with a clear ideological background regularly endorse candidates they thought reflected their politics best (De Jong, 1999).

These ideological backgrounds also served as the basis for the party landscape that was arising. The first player to take the initiative towards party formation was the Protestant politician Abraham Kuyper, who founded the Anti-Revolutionary Party (ARP) in 1879 after British model (Koch, 2020). His program centered on obtaining autonomy for the country's different religions, particularly in education (De Jong, 2001), but also in other social, economic and political institutions. Parties soon proved to be the natural means of coordination, both between politicians with a similar ideology, and between politicians and electorates: the liberal counterpart to the ARP was founded in 1885, and the Catholic union of electoral associations was founded in 1891. An overwhelming majority of incumbent politicians joined political parties, and, since it was nearly impossible to be elected without the support of a party, after the formation of parties, there were almost no unaffiliated politicians. The strong ideology-based political landscape was also the reason why there very few cases of politicians switching political parties. (e.g. De Haan and Te Velde, 1996; De Jong, 1997)

3 Data and Sources

3.1 Electoral Data

The *Repositorium Tweede Kamerverkiezingen 1848-1917* (Repository Lower House Elections) encompasses comprehensive records of Dutch Lower House elections held between 1848 and 1917, a period characterized by district-level electoral organization. This dataset systematically documents crucial election details, including district demarcations, dates of elections, and election types categorized as regular, intermediate, or second round elections. Additionally, it furnishes candidate particulars such as their names alongside the vote count secured, the total number of eligible voters in the respective district, voter turnout, and pertinent metadata such as the number of contested seats in the election, election type, and date. The focus of this analysis is directed towards elections culminating in a definitive victor. A refined subset is created by excluding elections that did not immediately result in

a clear winner, specifically first rounds necessitating subsequent rounds or nullified elections failing to meet the electoral threshold. This refinement process identifies approximately 2100 distinct elections within the district system between 1848 and 1917.

In line with other studies using close elections (e.g. Lee, 2008; Fisman et al., 2014), this study employs a vote margin-based methodology to discern elections exhibiting marginal differences. The marginal winner (MW) in each election is identified as the winning candidate securing the lowest count of votes among all victorious candidates. In the majority of instances, this corresponds to the sole winner in elections featuring a single contested seat. However, in a minority of cases, this criterion may reveal a different candidate. The grouping denoted as Winners_e encompasses all victorious candidates in a given election e. Subsequently, at the candidate-district level (candidate i, district e), vote margins are defined and computed as follows:

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

This way of defining the margin ensures symmetry and simplifies to the conventional definition of margin in case of two candidates.

3.2 Politician Data

I retrieve a proprietary dataset from the *Politiek Documentatiecentrum* (PDC)[2], a think-tank focused on Dutch politics. The data encompass various demographic variables related to a politicians' life, including their birth and death date and place, and detailed data about career paths they have undertaken over the course of their life. I use these data to match politicians to candidate-election pairs in the election data using a rule-based approach (Abramitzky et al., 2021) based on active period and fuzzy string matching. In addition to election-candidate specific information, I also collect newspaper recommendations for individual i in election e from the *Repositorium*. Local newspapers reported 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).

3.3 Non-Politician Data

Similar to the politicians, i.e. individuals who were elected at least once in their lifetime, I also retrieve data for non-politicians, whose data are not collected by the PDC due to them never being elected into politics. Hence, I make use of online genealogical sources, such as *genealogieonline.nl*, *Geni.com*, the historical newspaper search engine *Delpher*, local provincial archives to identify the birth date and place and date and place of decease for non-politicians and *Wikipedia*. In addition, I collect information on their career paths,

[2]Information about the PDC is accessible here

where specifically, I look for information whether they have worked in politics, business or the colonies after being a candidate.

3.4 Personal Wealth

I use archival data from probate inventories that contain the personal wealth of candidates at time of decease from provincial archives, called the *Memories van Successie* (MVS). The MVS primarily contain documents specifying the appraisal of a deceased individual's assets and liabilities with the purpose of levying inheritance taxes (Bos, 1990). This source is generally regarded as a highly reliable source of individuals' net worth. Descendants had to declare under oath in court that the list of assets and liabilities they submitted was truthful (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. The MVS are publicly available from 1877 to 1927. There are various studies outside of the Netherlands that use similar sources. Eggers and Hainmueller (2009) use a very similar source for their study about British MPs, and Fisman et al. (2014) use mandatory asset declaration forms for Indian MPs, and Bottomley (2019) uses probate inventories to investigate the returns to inventions.

Since I am focusing on close elections, I have prioritized collecting wealth data for candidates whose margins were closer to zero. In total, out of 6,197 candidate-election pairs, I collected probate inventories for 2,893 candidate-election pairs. These pertain to 515 unique candidates, whereas in total, there are 1,590 unique candidates. There are 2,877 candidate-election pairs who took place in relatively close elections, for 1,527 of which I collected their personal wealth (53%). The main reason of absence is the aforementioned limited availability of the archival data. Machielsen (2021) shows that there is no relationship between many characteristics and the likelihood of finding a probate inventory, implying that the inventories I wasn't able to find are missing at random. Out of the 1,590 unique candidates, 620 of them succeeded in getting elected at least once. I was able to collect the personal wealth for 371 out of these individuals (55%). Out of the 970 unique candidates that were never elected, I was able to collect the personal wealth for 144 out of them. Out of the 382 non-politicians who were not elected with a margin of at most 20%, I collected the personal wealth for 123 candidates. Finally, the election dynamics are such that out of 620 politicians who have been elected at least once, 467 of them succeeded in getting elected twice, 356 three times, 297 four times, and 254 more than four times.

3.5 Other Covariates

I obtain control variables at the district-level from *HDNG*, a database containing information about Dutch municipalities. I use a dynamic mapping to aggregate data on the municipality-level to the district-level, contingent on the year in which the election took place, after which I construct variables that measure the religious composition (% Catholic and Protestant),

the composition of the labor force (% in industry, services, agriculture) and the share of taxes per capita in two available years, 1859 and 1889 as a proxy for district economic activity.

4 Method

4.1 A Dynamic Regression Discontinuity Design

I use quasi-random variation induced by close elections to estimate the effect of being politically active on end-of-life wealth. The analysis of these returns to politics is complicated by two features: first, because individuals can be elected multiple times, I have to take into account the dynamic nature of the treatment assignment to individuals. Concretely, an estimate of the effect of being elected for the first time on end-of-life wealth contains not only the *ceteris paribus* effect, 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. Secondly, comparing candidates who ran for office more frequently with candidates who did not exert the same effort might result in biased 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.

I follow an approach similar to Cellini et al. (2010) to disentangle these effects. More precisely, consider the following model[3], which incorporates the possibility that politicians who are first elected at different tries can realize different initial wealth effects:

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

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.[4] 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 1 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:

[3]This model is estimated using a RD-strategy with close elections, making sure that $\mathbf{E}[u_i b_{i,\tau}] = 0$, so that the parameters θ_τ can be estimated consistently.

[4]In this setup, this effect is independent of actual calendar time.

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

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 once is the same for candidates elected for the first time at different tries τ and τ'. [5] 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:

$$\log(w_i) = \alpha + \theta_\tau^{ITT} \cdot 1_{\text{Margin}_i > 0} + \eta \cdot f(\text{Margin}_i) + X_i\beta + \epsilon_i \tag{3}$$

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), and describe the default choice of parameters in section 4.3.

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 t'th period ATT returns γ_t and the incumbency advantages 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 Politics

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

[5] 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).

$$\mathcal{I}[c_{i,k}=1] = \alpha + \pi_{i,k} \cdot 1_{\text{Margin}_{i,e}>0} + \eta \cdot f(\text{Margin}_{i,e}) + X_i\beta + \epsilon_i \tag{4}$$

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 4 for each $k \in \{2, 3, \dots\}$ then gives estimates for the incumbency advantages for the k'th election in the future.

Estimating the returns to subsequent periods in the Lower House is somewhat more challenging. Conditional on being elected $t-1$ times, and on choosing to stand as a candidate again, the structure for end-of-life wealth is as follows:

$$w_i = \sum_{k=t}^{\infty} \gamma_k c_{i,k} + u_i \tag{5}$$

Again, focusing on an RD-implementation so that $\mathbb{E}[u_i c_{i,k}] = 0$, and differentiating equation 5 with respect to the independent variable $c_{i,k}$ makes clear the same issue as in section 4.1 is at hand:

$$\begin{aligned} \gamma_k^{ITT} = \frac{dw_i}{dc_{i,k}} &= \frac{\partial w_i}{\partial c_{i,k}} + \sum_{t'>t} \gamma_{t'} \cdot \frac{\partial c_{i,t'}}{\partial c_{i,k}} \\ &= \gamma_k^{ATT} + \sum_{t'>t} \gamma_{t'}^{ATT} \cdot \pi_{(t'-k)} \end{aligned} \tag{6}$$

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 period 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}. Furthermore, the recursion in equation 6 is valid if politicians choose to participate in subsequent elections without ever skipping one. If incumbency advantages are monotonically decreasing, the resulting estimates for the γ_t^{ATT} are lower bounds. 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 then use these politicians who have been elected $t-1$ times to estimate γ_t^{ITT} as follows:

$$\log(w_i) = \alpha + \gamma_t^{ITT} \cdot 1_{\text{Margin}_i>0} + \eta \cdot f(\text{Margin}_i) + X_i\beta + \epsilon_i \tag{7}$$

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 6, and compute standard errors using the delta method. These estimates in turn allow me to estimate the θ_τ^{ATT} in equation 2.

4.3 Regression Discontinuity Parameters

All of the estimands in equations 3, 4 and 7 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). These results are reported in Appendix A.

5 Analysis

5.1 Dynamic Returns to Politics

5.1.1 Descriptive Statistics and Covariate Balance

The regression discontinuity approach implies a random allocation of politician status close to the threshold with respect to pre-treatment variables, meaning that these pre-treatment characteristics should be roughly equal in treatment (politician) and control (non-politician) groups. Following concerns raised about the possible non-randomness of close elections 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 as well as within different margins, to investigate patterns of convergence. To investigate the validity of the RD design, I first show descriptive statistics of the pooled data in Table 7.1, and then show various pre-determined potential covariates relating to pre-treatment characteristics in Table 7.2. For brevity, I confine the analysis of covariate balance to a dataset with candidates who have never been elected before. In Appendix A, I also investigate covariate balance tables for different subsamples.

[Tables 7.1 and 7.2 here]

Table 7.1 shows the descriptive statistics of the dataset. In panel A, I show the newspaper recommendations. It shows that Catholic, Liberal and Protestant newspaper recommendations are comparable in frequency, whereas recommendations by Socialist newspapers were

less frequent. These shares correspond roughly to the balance of power in the Dutch political system. A significant fraction of the candidates, about 40%, was not backed by a (politically-oriented) newspaper. In panel B, I show demographic characteristics: politicians are on average 49.4 years old when elected, and live another 22.4 years after an election. The average turnout in a district was about 2,500, and the average size of the electorate in 1859 was about 12,500. In panel D, the birthplace characteristics, I show certain demographic factors. The religious denominations roughly represent those of the country as a whole: on average 62% of the average politicians' birthplace are Protestants, 35% are Catholic. Similar numbers apply not only to the birthplaces, but also to the districts they are running for office in. The average wealth at death of a candidate was about 70,000 guilders, which is equal to about 6 times a Minister's salary in 1900, and is about equal to 1 million euros in present-day terms[6].

Table 7.2 shows the distribution of several covariates in the treatment and control groups for all candidates who have never been elected before. The second to fourth columns show the sample means, conditional on the absolute value of the margin being < 0.2. The fifth to seventh columns show sample means conditional on a tighter margin, 0.05. In panel A, the results show that there is no difference in political affiliation between politicians and non-politicians, as evidenced by a balance in newspaper recommendations. Similarly, elected politicians and their runners-up have comparable demographic characteristics (panel C). The turnout in the districts is statistically indistinguishable, and so are other district characteristics (panel E). Some birthplace characteristics, the share of the labor force working in agriculture and taxes per capita, seem to differ somewhat between politicians and non-politicians (panel D). However, at the margin, these imbalances between politicians and runners-up vanish. In Appendix A, I repeat this analysis for other terms.

5.1.2 Returns to a Political Career

In Table 7.3, I show the estimates of equation 3. These estimates correspond to the "Intent-to-Treat" (ITT) effect of being elected on personal wealth, implying these are the total returns to a political career of least one period. The first four columns focus on the candidates who run for office for the first time. In the first two columns, I show estimates without covariates under the optimal, and twice the optimal bandwidth. In the third and fourth column, I add covariates. In the fifth and sixth column, I focus on all candidates who tried for a second time (after failing the first time), and in columns 7 and 8, I pool all candidates that, if elected, would be elected for the first time, irrespective of the number of tries. Columns 5 to 8 include several covariates.

The point estimates are all very similar in magnitude. In column 1, for example, the point estimate of 1.731 implies that politicians who had just been elected are almost 100,000 1900 guilders wealthier than if they had not been elected. That number is equal to approx. 8 minister's salaries, and equal to about 1.5 million present-day euros. This was not be-

[6] According to the IISG currency conversion tool

cause politicians were well-paid: it is significantly more than can be explained by wealth accumulation through politicians' formal remuneration. After the 1848 Constitution, politicians received remuneration of 2000 guilders per year (Elzinga, 1985).[7] 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. These numbers are still far from being able to explain the much higher wealth accumulation among politicians. The results also approximately match the results obtained in Fisman et al. (2014), who report an asset growth premium of 5% for politicians relative to their nearly-elected counterparts. The estimates in column 7, for example, also imply a yearly asset growth premium of about 5%, given that politicians live for another twenty years on average. The results are in the same order of magnitude as those of Eggers and Hainmueller (2009), who report a coefficient estimate of around 0.65 for a sample comparable to the sample in column 7 and 8 in Table 7.3.

[Table 7.3 here]

The differences in wealth accumulation between elected politicians and runners-up can also be shown to good effect graphically in Figure 1. I show the estimated conditional expectation function left and right of the cut-off point for two of the estimates in Table 7.3. The results are conditional on the inclusion of the same covariates as in Table 7.3 and show two settings, one for first triers, and one for all triers, who, if elected, would be elected for the first time. It becomes clear that the conditional expectation function itself is volatile, meaning there is no clear relationship between the margin a candidate obtained at elections and end-of-life wealth in general, as is expected for various reasons. However, at the cut-off point, there is an evident jump in the conditional expectation function, such that nearly-elected politicians end up much wealthier than their non-elected counterparts.

[Figure 1 here]

I then decompose these total wealth effect of a political career into various average treatment effects of being elected for the τ'th time, everything else equal. These results are displayed in Table 7.4. In these analyses, I notably control for the number of elections a candidate has already participated in before. I first report coefficient estimates for ITT effects, and then report the estimate for the average treatment effect on the treated (ATT), using the recursion defined in equation 2, for $t^* = \{4, 7\}$[8]. Standard errors for the estimates of the ATTs are obtained by the delta method. The obtained estimates are remarkably consistent for different t^*: in both reported cases, as well as in the unreported intermediate cases, the

[7] 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. Rising wages made this sum equal to about 5 times the average wage in 1890. In Appendix B, I describe politicians' compensation in more detail.

[8] The parameter t^* is the term for which the estimated ATT is equal to the ITT

point estimates for the ATT in the first period are statistically significant and hover around 1.1. This number represents the *ceteris paribus* effect of being elected once on end-of-life wealth. The effect size corresponds to about 60,000 guilders, equaling 5 minister's salaries and the equivalent of about 850,000 contemporary euros. For all other periods, the estimate of the ATT is close to zero, and never statistically significant, implying the absence of a discontinuity around the cut-off point.

Strikingly, the ATT effect is insignificantly different from zero for all subsequent elections, no matter the t^*. This means that the returns to politics found in Table 7.3 are principally due to the returns in the first period: politicians do not gain any financial advantage by being elected two or more times. In Figure 2, I graphically show the robustness of these estimates for the ATT to t^*. This figure shows the estimated ATTs and ITTs for being elected for the τ'th time. These results corroborate that the estimated ATT's are very similar to the estimated ITTs, and that the total effects reported in Table 7.3 are mostly due to the effect of being elected once. This is due to the incumbency advantages being rather small, minimizing the relevance of future *ceteris paribus* returns in estimating the present. Thus, after the first term, any additional terms after a first term do not increase politicians' end-of-life wealth. In Appendix A, Tables E.3 and E.4, I confirm that these results are invariant to RDD parameters such as the kernel or bandwidth chosen. I additionally show invariance to the definition of the dependent variable in table E.5. I also show the full version of Table 7.4 in Table E.6 and show the irrelevancy to bandwidth choice algorithm in Table E.7. In the remainder of the analysis, I focus on the ITT effect from being elected for the first time, corresponding to the *total* return to a political career, and I provide evidence making it more plausible that these returns are indeed accrued in-office. In what follows, I argue that the establishment of political parties caused the returns to politics to decrease notably, and I also consider several alternative explanations.

[Table 7.4 here]

[Figure 2 here]

5.2 Explanations

5.2.1 The Influence of Political Party Formation

Political parties potentially determine 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. Several theoretical studies also model the ability of the party to control its members in terms of voting for the position favored by the party (e.g. Eguia, 2011; Iaryczower, 2008; Curto-Grau and Zudenkova, 2018).

Empirically, I can identify the influence of party discipline by exploiting newspaper recommendations to find out politicians' affiliation, irrespective of whether parties were already established. The connection between newspaper recommendations and political allegiance is so clear that a newspaper recommendation from a particular newspaper is interpreted as party membership if that particular party had existed at that point in time. In practice, there was a near one-to-one correspondence between newspapers and political allegiance.[9] I estimate the following specifications for each $h \in \mathcal{H} = \{\text{Before Party Formation, After Party Formation}\}$:

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

Candidate i is in { Before Party Formation } if the election took place before the candidate's party, as indicated by a newspaper recommendation, was formed, and is in { After Party Formation } otherwise. In the vector X_i, I include newspaper recommendation indicators, so that the estimates are conditional on candidates being recommended by the same newspaper, thereby relying on variation between individuals of the same political allegiance.

[Table 7.5 here]

In Table 7.5, I report the estimates of specification 8. I again focus on the ITT effect of being elected into politics for the first time, as the ITT is very close to the ATT-effect, as per the results in the previous section. In the first two columns, I focus on the first try for the first period, and in the second two columns, I focus on candidates that already tried at least once, but if elected, would be elected for the first time. The last two columns contain estimates irrespective of the number of tries. The results show that the point estimate for the subsample with candidates before party formation is much higher than the point estimate for the subsample after party formation in all cases. Unsurprisingly, the point estimate for the subsample under electoral institutions without political parties is somewhat higher than the point estimates in Table 7.3. The point estimate for the subsample within political parties is much lower, and again in all cases, fails to attain statistical significance. The difference between the two point estimates is statistically significant in most cases, including in the pooled model, and in the first model for first triers. The effects for first triers are somewhat lower than the effect for other triers. The pooled results (columns 5 and 6) represent an average of those two effects.

The results are consistent with a vision that political parties are able to constrain politicians, as suggested in Eggers and Hainmueller (2009). The results here show that party discipline, rather than only serving the party leadership, can also serve another purpose: to constrain politicians from using their discretion to engage in rent-seeking voting behavior, or cater their voting behavior to interest groups. However, unlike in Eggers and Hainmueller (2009), the results in Table 7.5 seem to come from a combination of political parties, and is

[9] In Appendix C, I describe the connection between newspapers and political parties in detail.

not due to the particular organization of one political party, which I show in Appendix A, Table E.15. In Appendix A, Figure 6, I show placebo tests, estimating the party effect by artificially varying the year of party establishment and conducting the analysis in equation 8 again. The results show that the effect is the highest and most significant for the actual year of party establishment. Finally, in Appendix Tables E.16 and E.17, I decompose the ITT effects described here to the dynamic effects using equation 6. I find that the results are consistent with the analysis in this section: there seem to be positive returns concentrated in the first period for politicians outside a political party. On the other hand, the dynamic returns inside political parties are slightly more complex: while there are no first term returns apparent for this subsample, there is evidence that politicians can accrue returns in the second and third periods. Because the incumbency advantages for subsequent periods are relatively small, these effects are subsumed by the null effects in the first period.

5.2.2 In-office rents

The results in section 5.1.2 make it plausible that politicians are able to extract in-office rents from them holding political office, but only if they have enough discretion, not limited by a political party. The estimates suggest, however, that they are only able to do so in the first period, and not in later periods, as politicians who are just-elected for a second time are not systematically wealthier than politicians who just fail to be elected for a second time. There are various pieces of anecdotal evidence that support these quantitative results. In 1862, during his first term, liberal MP van der Maesen de Sombreff had to step down after he was implicated in a plot to exempt the province of the district he was representing from a tax hike. De Jong and Rutjes (2015) document a plot by the local Catholic clergy and Catholic MP Haffmans, involving the clergy checking whether parishioners voted for him. In 1874, a law aimed at ending child labor was accepted (Van Den Berg and Vis, 2013). However, a parliamentary inquiry in 1886 showed that the law was not observed. Observers blamed this partially on the corruption of politicians themselves having a stake in firms exploiting child labor (Van Den Berg and Vis, 2013; Wartena, 2003). In 1909, the leadership of the Protestant ARP was implicated in a scandal involving the award of royal decorations in exchange for monetary gifts to the party (De Bruijn, 2005). In 1915, in his first term as a Lower House member, liberal MP De Jong was accused of using his Lower House function and membership of a committee on the rationing of vegetables to use inside knowledge to gain personal pecuniary advantages (Kroeze, 2013). An investigation conducted by the liberal party concluded that De Jong had used his function illegitimately, although refrained from concluding he had engaged in corruption. About the affair, socialist MP Sannes was quoted as saying "we live in an atmosphere which, let me put it mildly, is not very fresh; there is no man which isn't convinced that [...] there is being tampered with [...]. Private individuals [...] always indulge in tampering."

5.2.3 Indirect Benefits and Selection

It is possible that politicians do not accrue in-office rents, but use politics as a gateway to more lucrative professions. Several studies (e.g Eggers and Hainmueller, 2009; Amore and Bennedsen, 2013; Fafchamps and Labonne, 2017; Folke et al., 2017; Cruz et al., 2017) investigate the existence and magnitude of various other benefits accruing to politicians. It is therefore plausible that politicians, by virtue of being elected into national politics, are themselves also more likely to end up in certain positions. Inspired by Amore and Bennedsen (2013) and Folke et al. (2017), I first investigate whether just-elected politicians are more likely to undertake certain career paths later in their life compared to their nearly-elected counterparts. Secondly, I investigate whether the relationship between holding political office and these career paths changes following party formation.

My empirical strategy aims to find differences in the likelihood of occupying three different positions: mayor, working in the financial sector, and working in the colonies. Firstly, 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 and Others, 1993; Steegh et al., 2016; Taselaar, 1998), 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 $j \in J = \{\text{Mayor, Colonial, Finance}\}$:

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

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

In Table 7.6, I show the RD estimates for the probabilities of candidates for becoming active (i) in business after their political career vs. all others, (ii) 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. In panel A, I show the unconditional results, and in panel B, I contrast the results before party formation with the results after party formation. The results show no evidence for indirect benefits for politicians after a political career: politicians are not more likely to pursue a career in either business, politics or colonial occupations. The point estimates are all close to zero, and none of them is statistically significant. In this respect, the results differ markedly from Eggers and Hainmueller (2009), who document large career advantages for politicians in a post World War II setting. The results also contradict a particular kind of incumbency advantage (Lee, 2008), in that politicians are not more likely to become a mayor afterwards than just-losing candidates. Even though the mean difference is always positive, there is no evidence of a discontinuous jump around the threshold determining whether a candidate is elected or not. There is also no discernible change in this relationship after political party formation. Hence, politicians aren't able to find new ways of accumulating returns to politics, after constraints on in-office behavior were established by political parties.

These results can also be interpreted as absence of selection-based trends in the candidate pool following political party formation: there is no evidence that candidates are more likely to pursue any of these three career paths after political party formation. This runs counter to a selection-based explanation of the findings in section 5.2.1, and indicates that politicians with similar aspirations and abilities were in the candidate pool before and after political party formation. I also offer more explicit evidence on the stationarity of the candidate pool around the moments of party formation in Appendix A, Table E.14, and confirm that there are little to no systematic differences between the candidate pool for several years before and after party formation. Altogether, this implies that the candidate pool before and after party formation was roughly similar in terms of pre-treatment characteristics, but also in terms of choices and opportunities for a post-politics career, and that selection-related issues are unlikely to be responsible for the observed differences.

[Table 7.6 here]

To further investigate the pattern of returns, I also consider differences between returns to politics for young versus old politicians. If the indirect benefits channel is active, younger politicians must have been able to accrue more returns over their lifetime than relatively older politicians. According to this explanation, the total effects of a political career should be larger for younger politicians than for older politicians. I investigate this issue in Appendix A, Table E.8. These results show that if anything, the opposite is true: the returns seem to be stronger for politicians who are older than the median age, although they seem to be somewhat spread out over the entire age range. The results can however be rendered spurious if younger politicians chose to accumulate wealth in different ways, for instance, by consuming more. In Appendix Table E.9, I investigate this by comparing young politicians who died young and who did not. I find that there is no difference between these groups, implying

that such an explanation is unlikely, and that the observed difference between young and old politicians is likely genuine. I also verify that there is no difference in expected lifespan for politicians and non-politicians at the discontinuity for different attempts in Table E.10. In any case, there is no evidence that younger politicians have been accruing systematically more benefits than older politicians. I conclude that there is no evidence that younger politicians obtain more returns from politics than older politicians, and hence, there is no evidence for a mechanism implying indirect benefits to a political career.

Finally, there is also a concern that the pattern of results may be due to selection in electoral dynamics. Concretely, if the electorate can (partially) detect rent-seeking type politicians (Besley and Case, 1995), then, after observing their activity for one period, this type of politician may be voted out, such that only 'honest' politicians remain in the political arena. I address this concern in Appendix A, Tables E.11, E.12, and E.13. This type of explanation implies that the correlation between personal wealth and the probability of reelection, candidacy or recommendation given candidacy, is negative. In fact, empirically, these correlations are mostly positive after the first and second periods, and insignificantly different from zero for others, making it unlikely that these dynamics play a role. In short, this shows that selection concerns towards honest or non-rent-seeking politicians, coming from either the electorate, political parties, or candidates themselves, are unlikely to play a large role.

5.2.4 Suffrage Extensions

In the period of investigation, suffrage extension played a central role in the political debate (Van Der Kolk et al., 2018). After a failed attempt to extend the franchise in 1872, it became increasingly clear that the coupling of suffrage to taxation excluded too high a proportion of the electorate. The attempt was hampered by the fact that Protestant and Catholic politicians required the position of Christian education to be taken into account into a new Constitutional revision, whereas the liberals wanted to only extend the franchise and decouple suffrage from taxation (Van Den Berg and Vis, 2013). In 1887, following a constitutional revision, the criterion based on taxes paid were augmented by a host of other criteria, including the notoriously vague stipulations of "fitness" and "societal standing" (Van Der Kolk et al., 2018). After again a failed attempt in 1892, an attempt in 1896 have turned out to be more fertile. The proposals introduced two new criteria for suffrage: paying direct a certain amount of income, land or wealth taxation, and a miscellaneous category called 'declaration', which included paying a certain amount of rent, passing certain exams, or having savings or a pension. As the incomes of the Dutch population steadily rose, while the franchise requirements remained static, this also made that more and more inhabitants were enfranchised (Van Der Kolk et al., 2018). In the elections of 1897, about 575,000 men were enfranchised. This number rose to close to 1 million men in 1913, i.e. 50% of the male population. In 1917, universal male suffrage was implemented, and in 1918 universal suffrage.

Suffrage extensions could have impacted the equilibrium returns to politics in various ways. There are theoretical and empirical studies (Lizzeri and Persico, 2004; Persson and Tabellini, 2004; Aidt and Mooney, 2014) that imply that suffrage extension can reduce rent-seeking behavior of politicians, mainly because politicians face stronger electoral incentives from a broader share of the population. To empirically investigate whether and to what extent suffrage extensions have been a key driver of the results, I estimate specification 3 while splitting the sample into before and after the various suffrage extensions. This way, I estimate the difference of political rents in elections before significant suffrage expansions, elections after a partially liberalized regime (between 1887 and 1897) and elections after a regime strongly resembling universal suffrage (after 1897). The results are displayed in Table 7.7.

[Table 7.7]

The results show that there are significant and positive returns to politics in the first period. Between the first and the second periods, there is no discernible difference between estimated returns to politics before and after the suffrage extensions, although both point estimates for the period before 1887 are significant, whereas the point estimates for the period between 1887 and 1896 are not. In all of the first four models, however, the point estimates hover around unity, and are not statistically significantly different from each other. The point estimates are comparable in magnitude with the point estimates shown in previous sections. In Table 7.3, I implicitly took this differential into account by estimating the results conditional on suffrage regime (1848-1887, 1887-1896, 1896-1917). In so far as an increase in suffrage extension implies an increase in monitoring on the part of the (enlarged) electorate, these results contradict the hypotheses posed by Querubin et al. (2011), who argue that increased monitoring is primarily responsible for rent extraction. On the other hand, the results in the last two column show a statistically significant negative effect for being politically active after 1897. The results, however, could be due to the fact that political parties were already in existence, implying a reduced possibility to obtain in-office returns. The relatively low salary then, would make it that there are positive opportunity costs to working in politics as opposed to elsewhere.

I investigate graphically whether this change in equilibrium returns to politics is driven by the expansion of the franchise, or whether it is an artifact of the aforementioned political party effects. If the change in returns is due to franchise extension, then the results should show a sharp drop in equilibrium rents following the 1897 expansion. I investigate whether the temporal pattern of equilibrium returns around the introduction of the 1897 franchise expansion in Figure 3. I plot the estimate of the "ITT" returns after a variable cut-off point. These serve as placebo tests for a possible structural break in the treatment effect centered around 1897.

These estimates show that the returns have stayed more or less stable over a long period of time, and that there is no sudden change following the suffrage extension of 1887. On the

other hand, there is some evidence that the suffrage extension in 1897 coincides with the sharp drop in returns to politics from 1897 onward. The estimates are strongly indicative of the conjecture that the increase in the electorate after the 1897 franchise extension made it even more difficult for politicians to accrue returns to politics, pushing the point estimate consistently down to zero, even though these estimates are not statistically different from zero at the 95% level. Strictly speaking, the estimates show a drop after I confine the dataset to elections that took place from 1894 onward, but the effect is strongest after the suffrage extension in 1897, and stabilizes afterwards. On the other hand, 1894 represents the moment at which all major political parties had been formed. It is therefore difficult to conclude that these results are exclusively due to franchise extension.

[Figure 3 here]

5.2.5 Constant Marginal Returns

The results in the previous sections show that politicians are only able to accrue returns to politics in the first period of political activity, after which a political career does not help in obtaining a financial advantage relative to careers outside of politics. In other words, the marginal returns to politics are likely diminishing. This result in itself contradicts various explanations of the returns to politics found in the literature. For example, in a present-day context, there is evidence that politicians can obtain rents by using insider information (Bourveau et al., 2021) or influencing public procurement (Baltrunaite, 2020). These and similar mechanisms imply that politicians can do this in principle at any moment in their career, not just in the first period. Hence, the results shown above are inconsistent with these explanations.

A possible reconciliation of these mechanisms with the regression discontinuity results described above could be that the regression discontinuity estimates are interpretable as local average treatment effects (Angrist and Imbens, 1995), rather than global effects. Recall that the estimated effects are for politicians with potential outcomes such that they won or lost with a small margin. Suppose a politician has only limited political capital to engage in rent-seeking activities (à la Curto-Grau and Zudenkova (2018)), and has the possibility to deplete this over multiple periods if elected again, but it is uncertain whether they will be elected a second time (indicated by the small margin the first time). Then, it makes sense to deplete the bulk of that capital during the first period. Moreover, the results in Appendix Tables E.16 and E.17 also suggest that the amount of discipline parties exert over politicians might not be constant: these results suggest that within political parties, politicians can accrue personal returns in the second and third period of political activity. This might indicate that relatively new politicians are effectively disciplined, but as soon as their influence increases, they might wield more power vis-à-vis parties, allowing them to exercise discretion again in subsequent periods. Similarly, politicians who anticipate the end of their Lower House career (see e.g. Besley and Case, 1995; Lopez, 2003) might no longer be disciplined by political parties. Finally, I cannot rule out that statistical power could be

an issue: given the lower sample size of second-term or third-term candidates, it becomes progressively more difficult to identify effects of further terms.

6 Conclusion

This study investigated the returns to politics in a context of changing political institutions. I find that there is a convincing and robust causal effect of becoming politically active on end-of-life wealth, corroborating several other studies (Eggers and Hainmueller, 2009; Fisman et al., 2014). Using the methodology of Cellini et al. (2010), I then investigate the *pattern* of these returns by exploiting the repeated quasi-random assignment of political office among candidates being elected once, twice, and more often. This allows me to obtain a marginal return curve to additional term of political office. I find that politicians can only accrue returns from political office in their first term. These returns are of a significant magnitude. They are equal to about 6-8 times a minister's salary, depending on the point estimate, and cannot be explained by the formal remuneration of politicians. In the second term and later terms, the end-of-life wealth of politicians is insignificantly different from candidates who failed to be elected by a small margin, indicating that these returns are due to politicians' being elected for the first time.

Next, I turn to the question of how changing political institutions change the equilibrium returns to politics. I firstly focus on an explanation implied in Eggers and Hainmueller (2009), who hint that the existence of political parties (not) being able to discipline their members might be an important determinant of political rents. By exploiting newspaper recommendations, allowing me to identify a candidate's allegiance before political parties actually existed, I contrast the returns to politics within and outside the regime of political parties. I find that the results show up chiefly in the periods in which parties aren't formed. In contrast to Eggers and Hainmueller (2009), the results do not come from one particular party. These results show that political parties, by quickly monopolizing the political arena, leaving very little space for independent candidates, and subsequently introducing party discipline, have successfully constrained politicians' rent-seeking behavior.

The combined findings imply that returns to politics are in-office rents, and show that party discipline is the primary determinant. This view is supported by anecdotal evidence of corruption cases documented by historians (Kroeze, 2013). Most of these cases feature members of parliament in their first term. I also consider alternative explanations to the in-office rents explanation. Apart from anecdotal evidence, this is evident from their professional careers after political office. In particular, I find no evidence that the returns are accrued out-of-office by an increased probability to work as a mayor, in the colonies, or in finance after holding office (cf. Mattozzi and Merlo, 2008). Similarly, I investigate whether the result is due to dynamic selection (Besley, 2005), a different pool of candidates following the establishment of political parties. Judging by ex-ante characteristics as well as by career paths, I find there is no evidence for selection playing a role. Finally, I investigate whether

suffrage extensions, potentially confounding the estimates of the effect of political parties, plays an important role. I find that the returns to political office do not change as a result of suffrage extensions, and that the returns to politics are more or less stationary. I also argue against explanations that imply a constant marginal return curve to politics, e.g. insider trading (Bourveau et al., 2021).

The results strongly suggest that politicians were able to realize returns to a political career within office, but that this is contingent on there being no political parties. Whereas economists and political historians usually interpret political parties as incarnations of political groups with similar ideologies or aggregators of policy preferences (De Jong, 2001; De Rooy, 2014; Persson and Tabellini, 2002; Ferreira and Gyourko, 2009), this paper is consistent with a complementary rationale for political parties: they served as mechanisms to constrain rent-seeking behavior. Plausibly, political parties have enough leverage over politicians to discipline their voting behavior (Grossman and Helpman, 2005), thereby limiting catering to interest groups. The results furthermore suggest that returns to politics are realized in the first period of political activity. Although I cannot exclude the results reflect an absence of statistical power, this is unlikely to be an issue since the point estimates are also very close to zero. All of this seems to imply decreasing returns to a political career.

The findings confirm widespread views about nineteenth-century European politics as being dominated by a wealthy, oligarchical elite, subject to few constraints. However, despite many studies arguing that politicians were subject to constraints from the electorate, for example in the form of the threat of revolution or other unrest (e.g. Acemoglu and Robinson, 2000; Aidt and Franck, 2019), this paper finds no evidence for a strong effect of suffrage extensions and increases in the size of the electorate on politicians' rent-seeking behavior. In comparison to these electoral repsonsiveness-hypotheses, the results of this paper show that party discipline was much more important in curbing politicians' behavior.

This study raises several issues for future research. First, it is unclear why there are only returns to a first term in political office, and these returns seem to disappear for later terms. Second, an interesting question is whether there can be found direct evidence for catering to interest groups in a historical setting, as was shown in contemporary settings (Baltrunaite, 2020; Bourveau et al., 2021). Third, given the important role of political parties in both democratization and in disciplining politicians, both theoreticians and empiricists could focus on what allowed political parties to obtain enough leverage over politicians to be able to discipline them, and whether this helped political parties in obtaining more votes.

References

Abramitzky, Ran, Leah Boustan, Katherine Eriksson, James Feigenbaum, and Santiago Pérez (2021) "Automated linking of historical data," *Journal of Economic Literature*, 59 (3), 865–918.

Acemoglu, Daron and James A Robinson (2000) "Why did the West extend the franchise? Democracy, inequality, and growth in historical perspective," *The Quarterly Journal of Economics*, 115 (4), 1167–1199.

Aerts, Remieg (2009) "Nationale beginselen? Een transnationale geschiedenis van politiek en grondwet in de negentiende eeuw," *BMGN-Low Countries Historical Review*, 124 (4), 580–598.

——— (2018) *Thorbecke wil het: Biografie van een staatsman*: Prometheus.

Aerts, Remieg, Janny De Jong, and Henk Te Velde (2002) *Het persoonlijke is politiek: egodocumenten en politieke cultuur*: Uitgeverij Verloren.

Aidt, Toke S. and Raphaël Franck (2015) "Democratization under the threat of revolution: Evidence from the Great Reform Act of 1832," *Econometrica*, 83 (2), 505–547.

Aidt, Toke S and Raphaël Franck (2019) "What Motivates an Oligarchic Elite to Democratize? Evidence from the Roll Call Vote on the Great Reform Act of 1832," *The Journal of Economic History*, 79 (3), 773–825.

Aidt, Toke S. and Graham Mooney (2014) "Voting suffrage and the political budget cycle: Evidence from the London Metropolitan Boroughs 1902–1937," *Journal of Public Economics*, 112, 53–71.

Amore, Mario Daniele and Morten Bennedsen (2013) "The value of local political connections in a low-corruption environment," *Journal of Financial Economics*, 110 (2), 387–402.

Angrist, Joshua and Guido Imbens (1995) "Identification and estimation of local average treatment effects," *National Bureau of Economic Research*.

Baltrunaite, Audinga (2020) "Political contributions and public procurement: evidence from Lithuania," *Journal of the European Economic Association*, 18 (2), 541–582.

Barro, Robert J (1973) "The control of politicians: an economic model," *Public Choice*, 19–42.

Becker, Sascha O and Erik Hornung (2020) "The political economy of the Prussian three-class franchise," *The Journal of Economic History*, 80 (4), 1143–1188.

Bellemare, Marc F and Casey J Wichman (2020) "Elasticities and the inverse hyperbolic sine transformation," *Oxford Bulletin of Economics and Statistics*, 82 (1), 50–61.

Besley, Timothy (2005) "Political selection," *Journal of Economic Perspectives*, 19 (3), 43–60.

Besley, Timothy and Anne Case (1995) "Does electoral accountability affect economic policy choices? Evidence from gubernatorial term limits," *The Quarterly Journal of Economics*, 110 (3), 769–798.

Blok, Lodewijk (1987) *Stemmen en kiezen: het kiesstelsel in Nederland in de periode 1814-1850*, 47: Wolters-Noordhoff/Forsten.

Bos, N. J. P. M. (1990) "Vermogensbezitters en bevoorrechte belastingbetalers in de negentiende eeuw," *BMGN-Low Countries Historical Review*, 105 (4), 553–577.

Bottomley, Sean (2019) "The returns to invention during the British industrial revolution," *The Economic History Review*, 72 (2), 510–530.

Bourveau, Thomas, Renaud Coulomb, and Marc Sangnier (2021) "Political Connections and White-Collar Crime: Evidence from Insider Trading in France," *Journal of the European Economic Association*, 19 (5), 2543–2576.

Calonico, Sebastian, Matias D Cattaneo, and Rocio Titiunik (2015) "rdrobust: An R Package for Robust Nonparametric Inference in Regression-Discontinuity Designs.," *R Journal*, 7 (1), 38.

Caselli, Francesco and Massimo Morelli (2004) "Bad politicians," *Journal of Public Economics*, 88 (3-4), 759–782.

Cattaneo, Matias D., Nicolás Idrobo, and Rocío Titiunik (2019) *A practical introduction to regression discontinuity designs: Foundations*: Cambridge University Press.

Caughey, Devin and Jasjeet S Sekhon (2011) "Elections and the regression discontinuity design: Lessons from close US house races, 1942–2008," *Political Analysis*, 19 (4), 385–408.

Cellini, Stephanie Riegg, Fernando Ferreira, and Jesse Rothstein (2010) "The value of school facility investments: Evidence from a dynamic regression discontinuity design," *The Quarterly Journal of Economics*, 125 (1), 215–261.

Cruz, Cesi, Julien Labonne, and Pablo Querubin (2017) "Politician family networks and electoral outcomes: Evidence from the Philippines," *American Economic Review*, 107 (10), 3006–37.

Curto-Grau, Marta and Galina Zudenkova (2018) "Party discipline and government spending: Theory and evidence," *Journal of Public Economics*, 164, 139–152.

Dal Bó, Ernesto, Pedro Dal Bó, and Jason Snyder (2009) "Political dynasties," *The Review of Economic Studies*, 76 (1), 115–142.

De Bruijn, Jan (2005) *Het boetekleed ontsiert de man niet. Abraham Kuyper en de Lintje-saffaire (1909-1910)*: Bert Bakker.

De Haan, I. and H. Te Velde (1996) "Vormen van politiek. Veranderingen van de openbaarheid in Nederland 1848-1900," *BMGN-Low Countries Historical Review*, 111 (2), 167–200.

De Haan, Ido (2003) *Het beginsel van leven en wasdom: de constitutie van de Nederlandse politiek in de negentiende eeuw*: Wereldbibliotheek Amsterdam, the Netherlands.

De Jong, R (2001) "Antirevolutionaire partijvorming, 1848-1879: een afwijkende visie," *Jaarboek Documentatiecentrum Nederlandse Politieke Partijen 2001*, 213–226.

De Jong, Ron (1997) "Katholieken en verkiezingen, 1850-1900. Oude en nieuwe vormen van politiek," *Groniek*, 1 (137).

——— (1999) *Van standspolitiek naar partijloyaliteit: verkiezingen voor de Tweede Kamer 1848-1887*: Uitgeverij Verloren.

De Jong, Ron and Mart Rutjes (2015) "Slecht gekozen: Politieke vertegenwoordiging en electorale wanpraktijken in Nederland, 1795-1917," *Tijdschrift voor Geschiedenis*, 128 (4), 579–598.

De Rooy, Piet (2014) *Ons stipje op de waereldkaart: de politieke cultuur van Nederland in de negentiende en twintigste eeuw*: Wereldbibliotheek.

De Vries, J. (1971) "Het censuskiesrecht en de welvaart in Nederland 1850-1917," *Economisch en Sociaal-historisch Jaarboek*, 34, 178–231.

Diermeier, Daniel, Michael Keane, and Antonio Merlo (2005) "A political economy model of congressional careers," *American Economic Review*, 95 (1), 347–373.

Djankov, Simeon, Rafael La Porta, Florencio Lopez-de silanes, and Andrei Shleifer (2010) "Disclosure by politicians," *American Economic Journal: Applied Economics*, 2 (2), 179–209.

Duggan, John and César Martinelli (2017) "The political economy of dynamic elections: Accountability, commitment, and responsiveness," *Journal of Economic Literature*, 55 (3), 916–84.

Edinger, Michael (2009) *Profil eines Berufsstandes: Professionalisierung und Karrierelogiken von Abgeordneten im vereinten Deutschland*: Nomos, 177–216.

Eggers, Andrew C. and Jens Hainmueller (2009) "MPs for sale? Returns to office in postwar British politics," *American Political Science Review*, 103 (4), 513–533.

Eguia, Jon X (2011) "Voting blocs, party discipline and party formation," *Games and Economic Behavior*, 73 (1), 111–135.

Elzinga, Douwe Jan (1985) *De financiële positie van de leden der Staten-Generaal*: Wolters-Noordhoff.

Fafchamps, Marcel and Julien Labonne (2017) "Do politicians' relatives get better jobs? evidence from municipal elections," *The Journal of Law, Economics, and Organization*, 33 (2), 268–300.

Ferejohn, John (1986) "Incumbent performance and electoral control," *Public Choice*, 50 (1), 5–25.

Ferreira, Fernando and Joseph Gyourko (2009) "Do political parties matter? Evidence from US cities," *The Quarterly Journal of Economics*, 124 (1), 399–422.

Fisman, Raymond, Florian Schulz, and Vikrant Vig (2014) "The private returns to public office," *Journal of Political Economy*, 122 (4), 806–862.

Folke, Olle, Torsten Persson, and Johanna Rickne (2017) "Dynastic political rents? Economic benefits to relatives of top politicians."

Gelman, Andrew and Guido Imbens (2019) "Why high-order polynomials should not be used in regression discontinuity designs," *Journal of Business & Economic Statistics*, 37 (3), 447–456.

Grossman, Gene M. and Elhanan Helpman (2005) "Party discipline and pork barrel politics," *National Bureau of Economic Research*.

Iaryczower, Matias (2008) "Contestable leadership: Party leaders as principals and agents," *Quarterly Journal of Political Science*, 3 (3), 203–225.

Imbens, Guido and Karthik Kalyanaraman (2012) "Optimal bandwidth choice for the regression discontinuity estimator," *The Review of Economic Studies*, 79 (3), 933–959.

Kaal, Harm (2008) *Het hoofd van de stad: Amsterdam en zijn burgemeester tijdens het interbellum*: Amsterdam University Press.

Kan, J. B. (1916) *Handelingen over de herziening der grondwet*: Belinfante.

Koch, Jeroen (2020) *Abraham Kuyper: een biografie*: Boom.

Kroeze, Ronald (2013) *Een kwestie van politieke moraliteit: politieke corruptieschandalen en goed bestuur in Nederland, 1848-1940*: Uitgeverij Verloren.

Lee, David S (2008) "Randomized experiments from non-random selection in US House elections," *Journal of Econometrics*, 142 (2), 675–697.

Lindblad, J. Th and Others (1993) "Ondernemen in Nederlands-Indie c. 1900-1940," *BMGN-Low Countries Historical Review*, 108 (4), 699–710.

Lindeboom, Lucas (1916) *Parlementaire schadeloosstelling* Ph.D. dissertation, Vrije Universiteit Amsterdam.

Lizzeri, Alessandro and Nicola Persico (2004) "Why did the elites extend the suffrage? Democracy and the scope of government, with an application to Britain's "Age of Reform"," *The Quarterly Journal of Economics*, 119 (2), 707–765.

Loots, Jasper (2004) *Voor het volk, van het volk. Van districtenstelsel naar evenredige vertegenwoordiging*: Amsterdam: Wereldbibliotheek.

Lopez, Edward J (2003) "Term limits: Causes and consequences," *Public Choice*, 114 (1), 1–56.

Lowes, Sara and Eduardo Montero (2021) "Concessions, violence, and indirect rule: evidence from the Congo Free State," *The Quarterly Journal of Economics*, 136 (4), 2047–2091.

Machielsen, Bas (2021) "The Wealth of The Dutch Political Elite (1870-1920)," *Working Paper*.

Madden, Cathy and Deirdre Mckeown (2012) *Parliamentary remuneration and entitlements*: Department of Parliamentary Services, Parliamentary Library.

Mattozzi, Andrea and Antonio Merlo (2008) "Political careers or career politicians?" *Journal of Public Economics*, 92 (3-4), 597–608.

Mian, Atif, Amir Sufi, and Francesco Trebbi (2010) "The political economy of the US mortgage default crisis," *American Economic Review*, 100 (5), 1967–98.

Moes, Jaap (2012) *Onder aristocraten: over hegemonie, welstand en aanzien van adel, patriciaat en andere notabelen in Nederland, 1848-1914*: Uitgeverij Verloren.

Monier, Frédéric and Christophe Portalez (2020) "Une norme disputée: l'indemnité parlementaire en France (1789-1914)," *Cahiers Jaures*, 1 (1), 15–36.

Olive, David J (2008) "Applied robust statistics," *Preprint M-02-006*.

Oud, Pieter Jacobus (1997) *Staatkundige vormgeving in Nederland*, 1: Uitgeverij Van Gorcum.

Peltzman, Sam (1984) "Constituent interest and congressional voting," *The Journal of Law and Economics*, 27 (1), 181–210.

Persson, Torsten and Guido Tabellini (2002) *Political economics: explaining economic policy*: MIT Press.

——— (2004) "Constitutions and economic policy," *Journal of Economic Perspectives*, 18 (1), 75–98.

Przeworski, Adam (2009) "Conquered or granted? A history of suffrage extensions," *British Journal of Political Science*, 39 (2), 291–321.

Querubin, Pablo, James M. Snyder, and Others (2011) "The control of politicians in normal times and times of crisis: Wealth accumulation by US congressmen, 1850-1880,"Technical report, National Bureau of Economic Research.

Querubin, Pablo and James M Snyder Jr (2009) "The returns to US congressional seats in the mid-19th century," *The Political Economy of Democracy*, 225–40.

Steegh, Frans, Leo Dalhuisen, and Mariëtte Van Selm (2016) *Geschiedenis van Indonesië*: Amsterdam University Press.

Stuurman, Siep (1983) *Verzuiling, kapitalisme en patriarchaat: aspecten van de ontwikkeling van de moderne staat in Nederland*: Sun.

Svaleryd, Helena and Jonas Vlachos (2009) "Political rents in a non-corrupt democracy," *Journal of Public Economics*, 93 (3-4), 355–372.

Taselaar, Arjen (1998) "De Nederlandse koloniale lobby," *Ondernemers En De Indische Politiek*, 1940, 107.

Van Den Berg, J. Th J. and Jacob Jan Vis (2013) *De eerste honderdvijftig jaar: parlementaire geschiedenis van Nederland, 1796-1946*: Prometheus.

Van Der Kolk, Henk, Patrick Van Schie, Fleur De Beaufort, Peter Van Den Berg, and Ron De Jong (2018) *Tussen geschiktheid en grondrecht: de ontwikkeling van het Nederlandse kiesrecht vanaf 1795*: Amsterdam University Press.

Van Riel, Arthur (2018) *Trials of Convergence: Prices, markets and industrialization in the Netherlands, 1800-1913* Ph.D. dissertation, Utrecht University.

Van Zanden, Jan Luiten (1983) "Lonen en arbeidsmarkt in Amsterdam, 1800-1865," *Tijdschrift Voor Sociale Geschiedenis*, 9, 1–27.

Van Zanden, Jan Luiten and Arthur Van Riel (2004) *The strictures of inheritance: The Dutch economy in the nineteenth century*, 14: Princeton University Press.

Voerman, Gerrit (1989) "Liberalen op een tweesprong," *Jaarboek Documentatiecentrum Nederlandse Politieke Geschiedenis*.

Wartena, Bert (2003) *H. Goeman Borgesius (1847-1917): vader van de verzorgingsstaat: een halve eeuw liberale en sociale politiek in Nederland*: Het Spinhuis.

7 Tables and Figures Main Text

7.1 Tables

Table 7.1: 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	19.82	10.42	0.06	39.99	4389
Age at Election	49.32	11.35	1.41	106.51	4690
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					
Election After ARP	0.56	0.50	0.00	1.00	6197
Election After RK	0.30	0.46	0.00	1.00	6197
Election After Lib	0.46	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 7.2: Covariate Balance - First Term

	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	0.13	0.12	0.855	0.12	0.11	0.759	-0.175 (0.043)
Rec.: Liberal	0.14	0.10	0.036**	0.14	0.06	0.012**	0.034 (0.053)
Rec.: Socialist	0.08	0.07	0.760	0.07	0.13	0.106	0.007 (0.035)
Rec: Catholic	0.11	0.11	0.844	0.11	0.09	0.563	-0.163 (0.046)
Panel B: Demographic Characteristics							
Lifespan	21.55	21.92	0.669	22.55	20.79	0.286	1.915 (1.520)
Age at Election	45.93	45.08	0.349	44.93	44.92	0.998	2.246 (1.572)
Year of Death	1904.22	1899.64	0.015**	1905.69	1900.02	0.108	4.047 (3.617)
Year of Election	1880.31	1876.81	0.009***	1881.05	1879.42	0.529	-0.204 (2.495)
Panel C: Election Characteristics							
Log Turnout	7.88	7.81	0.178	7.84	7.83	0.917	-0.568 (0.133)
Log Turnout Previous	7.82	7.70	0.042**	7.84	7.81	0.790	-0.424 (0.118)
Panel D: Birthplace Characteristics							
Log Population 1859	9.52	9.63	0.586	9.33	9.70	0.319	-0.153 (0.335)
Share Protestant	0.63	0.63	0.858	0.63	0.55	0.125	0.019 (0.040)
Share Catholic	0.34	0.33	0.783	0.34	0.41	0.189	-0.013 (0.042)
Labor Force Share Agricul.	0.05	0.04	0.019**	0.06	0.03	0.002***	0.007 (0.017)
Labor Force Share Industry	0.20	0.19	0.173	0.20	0.19	0.796	-0.011 (0.016)
Taxes Per Capita 1859	4.03	4.36	0.018**	3.68	4.57	0.001***	-0.040 (0.277)
Taxes Per Capita 1889	4.89	5.26	0.007***	4.71	5.42	0.008***	-0.001 (0.247)
Distance to the Hague	95.24	89.69	0.325	106.59	90.60	0.148	6.476 (9.331)
Panel E: District Characteristics							
Share Protestant	0.63	0.62	0.774	0.60	0.55	0.190	-0.004 (0.032)
Share Catholic	0.34	0.35	0.697	0.37	0.43	0.182	0.014 (0.033)
Labor Force Share Agricul.	0.06	0.07	0.206	0.06	0.05	0.178	0.020 (0.014)
Labor Force Share Industry	0.21	0.22	0.218	0.20	0.21	0.577	-0.004 (0.012)

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 *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table 7.3: Main RD Estimates - 1st Term

	First Triers				Second Triers		All Triers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coefficient (ITT)	1.731	1.861	2.041	2.123	1.446	1.256	0.995	0.754
SE (BC)	(0.716)*	(0.539)***	(0.784)**	(0.600)***	(0.910)	(0.716)*	(0.496)**	(0.377)**
Mean DV Treated (1%)	12.849	12.849	12.901	12.901	11.059	11.059	12.375	12.375
Mean DV Control (1%)	10.193	10.193	10.887	10.887	9.759	9.759	10.706	10.706
N (Politicians)	103	103	86	86	65	65	295	295
N (Non-Politicians)	172	172	158	158	182	182	774	774
Bandwidth	Optimal	2x Optimal	Optimal	2x Optimal	Optimal	2x Optimal	Optimal	2x Optimal

Note: Table showing Bias-corrected standard errors clustered at the individual-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 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 7.4: ATT estimates for different t^*

	t=1	t=2	t=3	t=4	t=5	t=6	t=7
Panel A: t* = 4							
Coefficient (ITT)	1.062	0.342	0	-0.685			
SE (ITT)	(0.399)***	(0.611)	(0.613)	(0.633)			
Coefficient (ATT)	0.997	0.283	-0.053	-0.685			
SE (ATT)	(0.492)**	(0.704)	(0.661)	(0.633)			
N Treated	295	219	172	141			
N Control	774	145	98	78			
Mean DV Treated	12.375	11.709	11.594	12.224			
Mean DV Control	11.004	10.505	11.944	12.677			
Panel B: t* = 7							
Coefficient (ITT)	1.062	0.342	0	-0.685	0.746	-0.129	-0.771
SE (ITT)	(0.399)***	(0.611)	(0.613)	(0.633)	(0.937)	(0.562)	(0.83)
Coefficient (ATT)	0.997	0.282	-0.054	-0.686	0.672	-0.189	-0.771
SE (ATT)	(0.574)*	(0.785)	(0.762)	(0.769)	(1.016)	(0.627)	(0.83)
N Treated	295	219	172	141	101	75	52
N Control	774	145	98	78	43	42	23
Mean DV Treated	12.375	11.709	11.594	12.224	11.657	12.194	12.112
Mean DV Control	11.004	10.505	11.944	12.677	11.997	13.187	13.103

Note: Table showing coefficients effects of stints {1, ..., t^*} under different $t^* \in \{4, 7\}$. 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 both panels control for birthplace population, birthplace characteristics, age at election, newspaper recommendations (party). *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

Table 7.5: Estimates In and Out-Party

	First Triers		Other Triers		All Triers	
	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient (Without Party)	1.167	1.186	1.493	1.504	1.282	1.304
SE (Without Party)	(0.573)**	(0.568)**	(0.912)*	(0.913)*	(0.527)***	(0.531)***
Coefficient (Within Party)	-0.694	-0.577	0.007	0.053	-0.259	-0.160
SE (Within Party)	(0.745)	(0.735)	(0.727)	(0.756)	(0.543)	(0.570)
p-value Difference	0.058	0.074	0.272	0.458	0.09	0.122
Mean DV Treated	12.123	12.123	12.002	12.002	12.086	12.086
Mean DV Control	10.355	10.355	10.727	10.727	10.494	10.494
N Treated	207	210	120	120	327	330
N Control	485	491	286	292	771	783
Bandwidth	Optimal	Optimal	Optimal	Optimal	Optimal	Optimal

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. The first two columns show estimates of the returns for the first-triers for the first stint, the second two estimates the returns for the second stint, and the third pair shows the results for all triers. Columns (1), (3) and (5) contain estimates with covariates including party, lifespan, number of votes, age, and number of candidates. Columns (2), (4) and (6) control for number of tries, party, district economic composition and total amount of votes. *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table 7.6: RD Estimates of Being Elected on Career Paths

	Finance		Colonial		Mayor	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Unconditional Estimates						
Coefficient	0.002	0.003	0.001	0.000	-0.007	-0.020
SE (BC)	(0.021)	(0.021)	(0.030)	(0.029)	(0.031)	(0.030)
Mean DV Treated (1%)	0.062	0.059	0.062	0.059	0.000	0.000
Mean DV Control (1%)	0.028	0.028	0.056	0.056	0.042	0.042
N (Politicians)	587	593	587	593	587	593
N (Non-Politicians)	1112	1126	1112	1126	1112	1126
Bandwidth	Optimal	Optimal	Optimal	Optimal	Optimal	Optimal
Panel B: Before and After Party Establishment						
Coefficient (Before Party)	0.017	0.018	0.002	0.004	-0.001	-0.003
SE (Before Party)	(0.035)	(0.035)	(0.044)	(0.045)	(0.034)	(0.034)
Coefficient (After Party)	-0.031	-0.023	0.005	0.000	-0.023	-0.049
SE (After Party)	(0.027)	(0.028)	(0.026)	(0.025)	(0.054)	(0.053)
Mean DV Treated (1%)	0.062	0.059	0.062	0.059	0.000	0.000
Mean DV Control (1%)	0.028	0.028	0.056	0.056	0.042	0.042
N (Politicians)	587	593	587	593	587	593
N (Non-Politicians)	1112	1126	1112	1126	1112	1126
Bandwidth	Optimal	Optimal	Optimal	Optimal	Optimal	Optimal

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 individual-level. All effects are estimated under the MSE-optimal bandwidth. I use two sets of covariates: first, I control for total amount of votes, age, newspaper recommendations and economic and demographic composition of the district. Second, I control for newspaper recommendations, the number of tries, and the economic and demographic composition of the candidate's birthplace. *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

Table 7.7: RD Estimates of Being Elected on Personal Wealth Before/After Suffrage Extensions

	Before 1887		Between 1887-1897		After 1897	
	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient (ITT)	1.376	1.328	1.395	1.440	-1.471	-0.849
SE (BC)	(0.562)***	(0.575)**	(1.181)	(1.338)	(0.789)**	(0.874)
Mean DV Treated (1%)	12.342	12.342	12.780	12.780	10.274	10.274
Mean DV Control (1%)	10.904	10.904	9.792	9.792	11.572	11.572
N Treated	147	148	48	48	64	64
N Control	431	436	117	117	73	73
Bandwidth	Optimal	Optimal	Optimal	Optimal	Optimal	Optimal

Note: Table showing the effect of being elected into politics on personal end-of-life wealth. The dependent variable is Log(1+Wealth at Death). The estimates show Bias-corrected and Robust standard errors clustered at the individual-level. All effects are estimated under the MSE-optimal bandwidth. I use two sets of covariates: in columns (1), (3) and (5) I control for birtplace population, and demographics, and newspaper recommendations (party). In columns (2), (4) and (6) I control for number of tries, birthplace demographics, district demographics and number of tries. *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

7.2 Figures

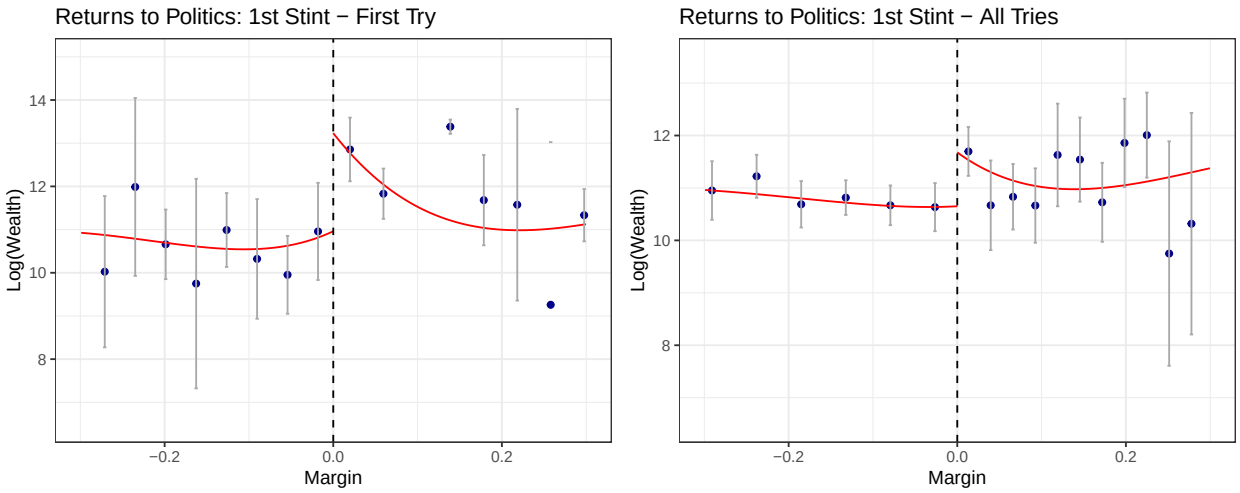

Figure 1: Estimates of Returns to Politics

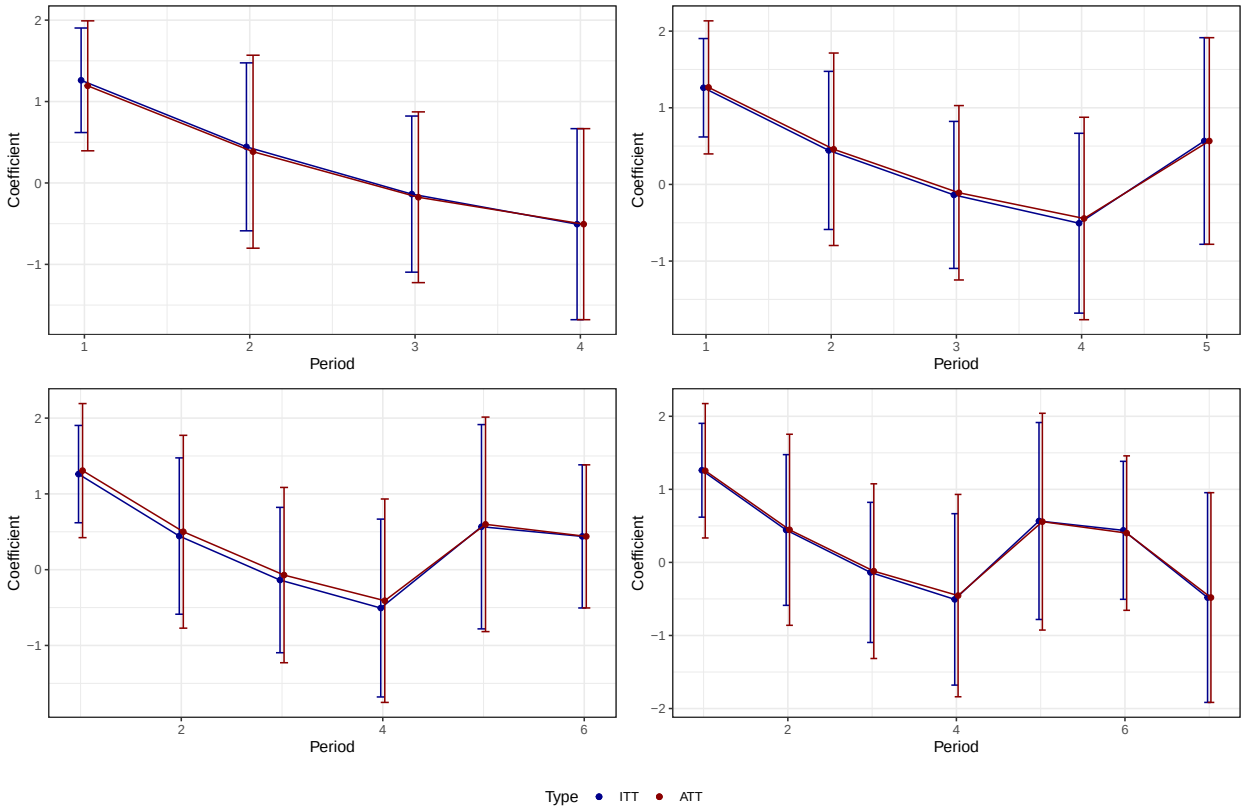

Figure 2: ITTs and ATTs for different t^*

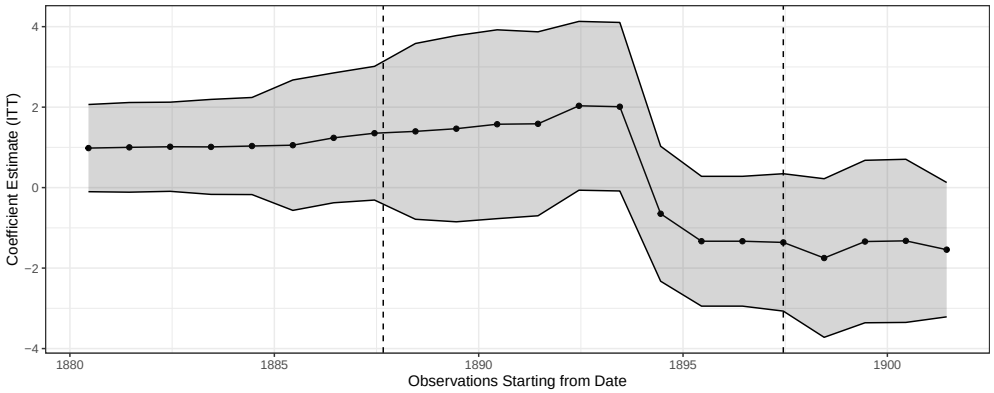

Figure 3: Estimates of Returns Around Suffrage Extensions

A Robustness Checks

A.1 Covariate Balance

In Table E.1, I show the covariate balance, but now only for the individuals who attempted their first try. This table is qualitatively very similar to the results in the main text: there seems to be an imbalance on various characteristics far away from the cut-off point, as there is no reason politicians and non-politicians are elected randomly with respect to these characteristics. At the margin, however, the RD estimates show that there is no jump in any of these covariates, as evidenced by the lack of statistical significance of the RD estimates. Hence, covariate balance also holds in this subgroup.

[Table E.1 Here]

In Table E.2, 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 term, in the second term, 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 E.2 Here]

A.2 Sensitivity to RD Parameters

I estimate the results in Table 7.3 using flexible bandwidth and different covariates and report the results in Table E.3. The results are qualitatively extremely similar to the results in the main text, and show significance in all cases. The magnitude of the effect is also very similar. I thus conclude that the results are invariant to the specific choice of the bandwidth parameter chosen.

[Table E.3 here]

I also estimate the results in Table 7.3 using different kernel choices. The default kernel is a triangular kernel, but I also estimate the results using the Yepanechnikov and uniform kernels in Table E.4. The results are again extremely similar to the results in the main text. The estimates are therefore independent of the precise kernel used.

[Table E.4 here]

In addition, I investigate the sensitivity of the main results to the dependent variable definition. In particular, I use the inverse hyperbolic sine, as suggested by Bellemare and Wichman (2020). The results, displayed in table E.5 are insensitive to this specification and very similar to the main results.

[Table E.5 here]

Similarly, I display the results similar to Table 7.4 but for all $t^* \in \{4, 5, 6, 7\}$. In the main text, I included an excerpt from this Table, for only $t^* \in \{4, 7\}$. This table shows the full results. The full results corroborate that the average treatment effect is only statistically distinguishable from zero in the first period. This is confirmed, irrespective of the actual value of t^*.

[Table E.6 here]

I also estimate these results using flexible bandwidths. The results using flexible bandwidths are in Table E.7.

[Table E.7]

The results are displayed in Table E.7. These results are also qualitatively very similar to the results in the main text, indicating that the results are not an artifact of the RDD parameters. According to these results, just-elected politicians accumulate about 130,000 guilders more wealth than nearly-elected losing contenders, a magnitude very comparable to the magnitude of the effect in the main text. The results show the familiar pattern in that there is a significant first-period effect, and the effects for all the other periods however around the zero, while never being statistically significant.

Graphically, I also display Figure 4, but now using flexible bandwidths and a different set of covariates. The results of this analysis show the same pattern as in the figure in the main text: there is a significant *ceteris paribus* effect in the first period, but not in the other periods, irrespective of what t^* is used to identify the estimates. The shape of the figure is also very similarly qualitatively, in that the results seem to hover around zero for all periods after the first period, and never attain significance.

[Figure 4 here]

A.3 Young vs. Old Politicians

In Table E.8, I investigate the difference in estimates between young (e.g. aged younger than the median) and old (older than the median) politicians. The idea is if the returns are not collected in office, but outside of office, younger politicians should have longer to accrue the benefits, and hence the total (ITT) returns from a political career should be higher. The results show that if anything, the opposite is the case: the effects seem to be concentrated among the part of the sample that is aged above the median age when taking part in elections.

[Table E.8 here]

These results could potentially still be due to benefits if young politicians have chosen to consume more of their income or rents as a result of being elected into political office, rendering the end-of-life estimates spurious. To this end, I use quasi-exogenous variation in the timing of death to investigate whether this is the case. A human capital-based explanation should find that young politicians who died young, and did not change their consumption pattern, should have had higher returns than young politicians who died old, who might have. Table E.9 investigates this issue.

[Table E.9 here]

Even though the point estimates for the candidates who died young are consistently lower than the point estimates for the candidates who died later, the estimates are not statistically significant. This means that a consumption-based explanation of the findings in table E.8 is unlikely, and consequently, that a human capital-based explanation is unlikely.

A.4 Selection of Non Rent-Seeking Politicians

In section 5.2.3, I argued against selection of non-corrupt politicians as an explanation for the observed pattern of dynamic returns. In Table E.11, I estimate the correlation between personal wealth and the probability of being election in the n'th election, after having been elected $n-1$ times. According to this explanation, the correlation between personal wealth and being elected for the n'th time after having been elected $n-1$ times should be negative, since after observing politicians' corruption, the electorate is able to filter out corrupt politicians, as in several asymmetric information and moral hazard-based models (Besley and Case, 1995).

[Table E.11 here]

The actual results show a positive correlation for the first two elections, whereas the subsequent correlation is insignificant. Only in the fourth election after having been elected three times, there is a significant negative relationship between the personal wealth of the candidate and the probability of getting reelected. The results are likely to be an upper bound on the true correlation, as existing wealth differences due to the returns to political rent-seeking accumulating over time and increasing wealth differences between corrupt and non-corrupt politicians.

I repeat the same exercise, but instead of investigating election wins, I investigate the probability of election candidacy, and the probability of candidacy and being recommended by a newspaper. The intuition is that selection might also occur from the side of political parties. In anticipation that parties supposedly filter out rent-seeking politicians, candidates might not attempt to run for office again. Alternatively, parties might not accord rent-seeking candidates a recommendation again, making them less-likely to be elected (or even

to be closely elected). These explanations imply a negative correlation between personal wealth and the probability of either of these events occurring. The results are shown in tables E.12 and E.13.

[Tables E.12 and E.13 here]

The results show either no, or a positive correlation between wealth and the probability of candidacy, again indicating that a selection-explanation is unlikely, be it selection by the electorate, or selection by political parties, or selection by rent-seeking candidates themselves.

A.5 No Change In Candidate Pool Before and After Party Formation

In Table E.14, I compare the candidate pool before and after the introduction of political parties. I focus on candidates that were recommended by newspapers, so as to know their potential party status if there were political parties. The results show that on the whole, there is no difference between the candidate pools 5 years before and after the introduction of the political party of their respective allegiance.

[Table E.14 here]

There are some differences in the groups, most notably with respect to socialist candidacy, which is occurring more frequently after parties have been formed. Apparently, this goes at the detriment of Catholic candidacy, which occurs less after parties have been introduced. Similarly, there are some minor differences in demographic characteristics, and to a lesser degree, average characteristics of the district in which the elections are organized. All of these, however, are unlikely to have an influence on the results established in section 5.2.1, partially because I control for many of these imbalances in the analysis of post and pre-party returns to politics.

A.6 Party Formation Effect Per Party

In Table E.15, I show the within-without party effect reported in Table 7.5 separately for every party $\in$ {Catholic, Liberal, Protestant}. The results show that the result in the main text is mainly due to Protestant and Liberal parties, whereas the estimates for returns to politics for Catholic politicians are negative in the period without parties, and very uncertain afterwards. The latter is likely an artefact of the relatively small sample size.

[Table E.15 here]

The magnitude of the effects are consistent under two different set of covariates, indicating that covariate imbalance is unlikely to be a problem. Compared to the main text,

the effects are somewhat larger, consistent with the intuition that the result is a weighted average of these per-party results, where the estimates for Liberal and Protestant returns are counterweighted by the (negative) returns for Catholic politicians. The results might also have to do with the particular form of party organization among Catholics: unlike protestant and liberals, who had formal parties modeled after the English model, Catholics have adhered to a looser form of party organization until relatively late in the nineteenth century, in part due to internal divisions among Catholic politicians.

A.7 Dynamic Effects In- and Out-Party

In Tables E.16 and E.17, I show the dynamic results for the observations in a without-party regime and a within-party regime. The results for the without-party regime are very similar to the results focusing on the ITT effect in the main text. As in the main text, the ITT results show a significant and positive effect for the first term in the Lower House. The ATT effects, however, border on statistical significance, due to noisy estimates for further terms, but show the expected sign and are very similar in magnitude compared to the ITT effect.

Focusing on the dynamics after political parties have been established, the results surprisingly show that there is a significant and positive effect of being politically active on personal wealth, but not in the first term. The effects are concentrated in the second and third term, and are robust to changing t^*. These effects are comparable in terms of magnitude to the first-term effects for politicians unconstrained by political parties. The existence of these effects calls into question the aforementioned conclusion that politicians are not able to amass personal returns within a party regime: it seems that *on the whole*, politicians within political parties are not able to amass returns, but politicians who are able to be elected a second or a third time might be.

[Tables E.16 and E.17 here]

These results can still be consistent with politicians being disciplined by political parties, but only to a certain extent. It is likely that the result has to do with bargaining power of politicians versus parties. Politicians who are elected for the first time, and who are popular, are likely to have enough leverage against the political party to engage in their own interest. These are also likely to be the politicians who are reelected. But, at the margin, these politicians' popularity should be roughly equal to the popularity of just-losers. On the other hand, it might have to do with within-party political influence. Politicians who have been member of a party long enough can accrue enough influence within their party, and only then afford the autonomy to engage in self-serving behavior.

A.8 Placebo Tests

[Figure 5 here]

In Figure 5, 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.

In Figure 6 I also estimate the difference in coefficient before and after party formation, while artificially changing the threshold of the party formation indicators from $[-8, 8]$ years before/after the appropriate party was actually formed. The estimates again make clear that the actual effect is the highest effect, increasing the likelihood of party formation actually being responsible for the curbing of the returns to politics.

[Figure 6 here]

B Compensation for Politicians

Lower House members were 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).

C Party System

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, 2001). Political parties were preceded by *Kiesvereenigingen*, electoral unions, of enfranchised 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 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. *Kiesvereenigingen* were a way to improve the dissemination of information and aggregate electoral preferences in a more effective way. A special role in information provision was taken up by national newspapers: the editorial boards of several large national newspapers with a clear ideological background regularly endorse candidate(s) they thought reflected their politics best (De Jong, 1999).

The main issues that separated politicians of different allegiance were schooling, franchise extension and taxation. There were also differences in economic and colonial policy positions, but the most salient issues surrounding state funding of religious schools and the extent to which the state should interfere in the economy (Van Zanden and Van Riel, 2004). The funding of education was one of the aspects that accompanied the rise of religious tensions

in the Netherlands throughout the nineteenth century. These religious tensions culminated in a system frequently dubber pillarization (Dutch: *Verzuiling*), meaning the segregation of the Dutch population into a Protestant and Catholic pillar, with separate societies for both, and coordination between these pillars through elites, including in national politics. The liberals formed a more loosely-defined third pillar (Stuurman, 1983).

These pillars also served as the basis for the party landscape that was arising. The first player to take the initiative towards party formation was the Protestant politician Abraham Kuyper, who founded the Anti-Revolutionary Party (ARP) in 1879 after British model (Koch, 2020). His program centered on obtaining autonomy for the country's different religions, particularly in education (De Jong, 2001), but also in other social, economic and political institutions. Parties soon proved to be the natural means of coordination, both between politicians with a similar ideology, and between politicians and electorates: the liberal counterpart to the ARP was founded in 1895, and the Catholic union of electoral associations was founded in 1891. Additionally, and afterwards, there were also a number of Socialist parties. An overwhelming majority of incumbent politicians joined political parties, and, since it was nearly impossible to be elected without the support of a party, after the formation of parties, the number of unaffiliated politicians was negligible.

The links between political parties and newspaper were as follows: a recommendation from the *Algemeen Handelsblad* was considered an endorsement for a liberal candidate, a recommendation from *De Tijd*, a Catholic newspaper, endorsed Catholic candidates, and a recommendation from *De Standaard* can be considered as an ideological affiliation to Protestant politics.

D Selection Bias

D.1 Truncated Wealth

The results in section 5 can be influenced by sampling mechanisms. Several concerns that have been mentioned include observing a truncated version of wealth, tax evasion that is proportional to wealth, and differential sampling of "wealthier" and "poorer" candidates. In this section, I argue that under a broad range of parameters, these concerns bias my results downwards. I do so using a very simple setting: instead of using the Calonico et al. (2015) estimator lacking a clear functional form, I use a naive difference between means estimator to analyze the direction of the bias in each of these settings. In many tables, I show that this "naive" estimator is fairly close to the non-parametric RD estimate.

Firstly, consider the data generating process at the margin to be:

$$W_i^* = \theta \cdot \mathbf{1}_{\{Politician_i\}} + \epsilon_i \tag{10}$$

where $\mathbf{1}$ is an indicator taking the value of 1 when individual i is elected, 0 otherwise. I take the error term to be $\mathcal{N}(0, \sigma^2)$. This specification is without loss of much generality,

since at the margin, the influence of covariates is partialled out, including the influence of the running variable, Margin. Hence, the mean-zero assumption does not lose generality. The normal distribution allows me to obtain tangible, closed-form results for an expression of the bias.

The first possibility to bias the results is truncated sampling. Suppose that instead of W_i^*, I observe:

$$W_i = \begin{cases} W_i^* & \text{if } W_i^* > c \\ NA & \text{if } W_i^* \leq c \end{cases} \tag{11}$$

Meaning that W_i is a truncated version of the actual wealth variable W_i^*, only observed when wealth exceeds a threshold c. In the main text, it is mentioned that several sources thought that a *Memorie* is administered only when an individual is suspected to have enough assets, although I have found numerous examples of the contrary. Now, W_i is distributed as a truncated normal with $(\mu, \sigma^2, a, b) = (\theta \cdot \mathbf{1}_{Politician_i}, \sigma^2, c, \infty)$. Then, the expected value of W_i equals (see e.g. Olive, 2008, for a derivation):

$$\mathbb{E}[W_i] = \theta \cdot \mathbf{1}_{\{Politician_i\}} + \sigma \cdot \left[\frac{\phi\left(\frac{c-\theta\cdot\mathbf{1}_{\{Politician_i\}}}{\sigma}\right)}{1-\Phi\left(\frac{c-\theta\cdot\mathbf{1}_{\{Politician_i\}}}{\sigma}\right)} \right]$$

with ϕ, Φ respectively denoting the density and cdf for the standard normal distribution. The expected value of the "naive" estimator is then $\mathbb{E}[\hat{\theta}] = \mathbb{E}[W_i|Politician] - \mathbb{E}[W_i|Non-Politician]$:

$$\mathbb{E}[\hat{\theta}] = \theta + \sigma \cdot \left[\frac{\phi\left(\frac{c-\theta}{\sigma}\right)}{1-\Phi\left(\frac{c-\theta}{\sigma}\right)} - \frac{\phi\left(\frac{c}{\sigma}\right)}{1-\Phi\left(\frac{c}{\sigma}\right)} \right]$$

Hence, if:

$$\frac{\phi\left(\frac{c-\theta}{\sigma}\right)}{1-\Phi\left(\frac{c-\theta}{\sigma}\right)} < \frac{\phi\left(\frac{c}{\sigma}\right)}{1-\Phi\left(\frac{c}{\sigma}\right)} \tag{12}$$

Then, $\mathbb{E}[\hat{\theta}] < \theta$. Sufficient conditions for this are:

- $\theta > 2c$ so that $\phi(\frac{c}{\sigma}) > \phi(\frac{-c-\epsilon}{\sigma})$, with ϵ reflecting the extent to which θ is greater than $2c$.
- $\frac{c}{\sigma}$ to be relatively small, or σ very large for a given c, so that $\Phi(\frac{c-\theta}{\sigma})$ and $\Phi(\frac{c}{\sigma})$ are similar in magnitude.

Condition 12 is very likely to be met, as c is anecdotally suggested to be close to about 300, and θ is to be of the order of 100,000. Furthermore, σ is also of the order of 100,000, so that this condition is likely to be satisfied in empirically plausible settings. I confirm

this in the replication package, where I show that for large ranges of parameter values, this condition holds.

D.2 Tax evasion

Tax evasion can plausibly occur. The main concern focuses on differential tax evasion, because the wealthy have a stronger incentive to engage in tax evasion than the poor. In this regard, consider the same dgp as before, and consider the following relationship between actual and poor wealth:

$$W_i = \begin{cases} p \cdot W_i^* & \text{if } W_i^* > c \\ W_i^* & \text{if } W_i^* \leq c \end{cases}$$

with $0 < p < 1$, reflecting the extent to which wealthier candidates engage into taxation. In this case, the expected value of observed wealth is:

$$\mathbb{E}[W_i] = \Pr(W_i^* > c) \cdot p \cdot \mathbb{E}[W_i^*] + \Pr(W_i^* \leq c) \cdot \mathbb{E}[W_i^*]$$

Calculating these probabilities and then evaluating $\mathbb{E}[\hat{\theta}]$, defined as before, gives:

$$\begin{aligned} \mathbb{E}[\hat{\theta}] &= \left[1 - \Phi(\frac{c-\theta}{\sigma})\right] \cdot p \cdot \theta + \Phi(\frac{c-\theta}{\sigma}) \cdot \theta - 0 \\ &= \theta \left[p(1 - \Phi(\frac{c-\theta}{\sigma}) + \Phi(\frac{c-\theta}{\sigma})\right] < \theta \text{ if } 0 < p < 1 \end{aligned}$$

Hence, this result shows that tax evasion unambiguously biases the results downward.

D.3 Differential Sampling

The final concern focuses on differential sampling, meaning, the expectation of observed wealth is disproportionally skewed to "wealthy" candidates rather than poor individuals. The potential bias comes from the supposition that wealthier candidates are also more likely to be politicians, potentially biasing the result. To analyze this, I introduce a parameter p, presumably greater than 0.5 reflecting the dominance of wealthier candidates in the sample:

$$W_i = p \cdot W_i^* \cdot \mathbf{1}_{W_i^* > c} + (1-p) \cdot W_i^* \cdot W_i^* \cdot \mathbf{1}_{W_i^* \leq c}$$

And the expected value of observed wealth looks like:

$$\mathbb{E}[W_i] = p \cdot \mathbb{E}[W_i^* | W_i^* > c] + (1-p) \cdot \mathbb{E}[W_i^* | W_i^* \leq c]$$

This reflects a weighted average of "wealthy" and "poor" candidates potentially skewed from their frequency in the population.

Deriving the expected value of these truncated normal distributions (derivations again in Olive (2008)), and then evaluating the expected value of the "naive" estimator gives:

$$\begin{aligned}\mathbb{E}[\hat{\theta}] &= p \cdot \theta + (1-p) \cdot \theta + p \cdot \sigma \cdot \left[\frac{\phi(\frac{c-\theta}{\sigma})}{1 - \Phi(\frac{c-\theta}{\sigma})} \right] - (1-p) \cdot \sigma \cdot \frac{\phi(\frac{c-\theta}{\sigma})}{\Phi(\frac{c-\theta}{\sigma})} - \\ & p \cdot \sigma \cdot \left[\frac{\phi(\frac{c}{\sigma})}{1 - \Phi(\frac{c}{\sigma})} \right] + (1-p) \cdot \sigma \cdot \left[\frac{\phi(\frac{c}{\sigma})}{\Phi(\frac{c}{\sigma})} \right] \\ &= \theta + p\sigma \left[\frac{\phi(\frac{c-\theta}{\sigma})}{1 - \Phi(\frac{c-\theta}{\sigma})} - \frac{\phi(\frac{c}{\sigma})}{1 - \Phi(\frac{c}{\sigma})} \right] + (1-p)\sigma \left[\frac{\phi(\frac{c}{\sigma})}{\Phi(\frac{c}{\sigma})} - \frac{\phi(\frac{c-\theta}{\sigma})}{\Phi(\frac{c-\theta}{\sigma})} \right]\end{aligned}$$

Requiring that the estimator be unbiased also implicitly determines the value for p to be in accordance with the distribution of wealth in the population. Two sufficient conditions for $\mathbb{E}[\hat{\theta}] < \theta$ are then:

- $$\frac{\phi(\frac{c-\theta}{\sigma})}{1 - \Phi(\frac{c-\theta}{\sigma})} < \frac{\phi(\frac{c}{\sigma})}{1 - \Phi(\frac{c}{\sigma})}$$
- $$\frac{\phi(\frac{c}{\sigma})}{\Phi(\frac{c}{\sigma})} < \frac{\phi(\frac{c-\theta}{\sigma})}{\Phi(\frac{c-\theta}{\sigma})}$$

In the replication package, I explore for what parameter values these conditions hold, and I find that for virtually all plausible empirical values, these conditions hold. Hence, this kind of bias also likely causes the estimate to be biased downwards.

E Tables and Figures Appendix

E.1 Tables

Table E.1: Covariate Balance - First Attempts - First Term

	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	22.75	25.56	0.052*	24.12	23.67	0.867	1.036 (2.796)
Age at Election	44.13	42.67	0.319	43.38	41.75	0.541	4.955 (3.494)
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 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 *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table E.2: Covariate Balance - Second Term

	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	20.46	20.14	0.800	20.96	21.20	0.903	-0.931 (2.456)
Age at Election	47.30	49.61	0.031**	46.76	50.24	0.038**	0.312 (2.029)
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.

Table E.3: Robustness to Main RD Estimates - 1st Period: BW Selector

	First Triers				Second Triers		All Triers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coefficient (ITT)	1.730	1.842	2.253	2.089	2.051	1.344	0.940	0.555
SE (BC)	(0.709)*	(0.534)***	(0.909)**	(0.702)***	(1.087)*	(0.846)	(0.588)*	(0.447)
Mean DV Treated (1%)	12.849	12.849	12.901	12.901	11.059	11.059	12.225	12.225
Mean DV Control (1%)	10.193	10.193	10.577	10.577	9.277	9.277	10.660	10.660
N (Politicians)	103	103	70	70	54	54	244	244
N (Non-Politicians)	172	172	120	120	145	145	579	579
Bandwidth	Optimal	2x Optimal	Optimal	2x Optimal	Optimal	2x Optimal	Optimal	2x 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 E.4: Robustness to Main RD Estimates - 1st Period: Kernel

	First Triers				Second Triers		All Triers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Uniform Kernel								
Coefficient (ITT)	1.774	1.981	2.341	1.830	2.270	1.217	0.748	0.496
SE (BC)	(0.746)**	(0.593)***	(0.861)**	(0.649)***	(1.121)**	(0.839)	(0.524)	(0.393)
Mean DV Treated (1%)	12.849	12.849	12.901	12.901	11.059	11.059	12.225	12.225
Mean DV Control (1%)	10.193	10.193	10.577	10.577	9.277	9.277	10.660	10.660
N (Politicians)	103	103	70	70	54	54	244	244
N (Non-Politicians)	172	172	120	120	145	145	579	579
Bandwidth	Optimal	2x Optimal	Optimal	2x Optimal	Optimal	2x Optimal	Optimal	2x Optimal
Panel B: Yepanechnikov Kernel								
Coefficient (ITT)	1.681	1.865	2.301	2.244	2.228	1.474	0.884	0.488
SE (BC)	(0.686)*	(0.519)***	(0.925)**	(0.724)***	(1.178)*	(0.888)	(0.583)	(0.436)
Mean DV Treated (1%)	12.849	12.849	12.901	12.901	11.059	11.059	12.225	12.225
Mean DV Control (1%)	10.193	10.193	10.577	10.577	9.277	9.277	10.660	10.660
N (Politicians)	103	103	70	70	54	54	244	244
N (Non-Politicians)	172	172	120	120	145	145	579	579
Bandwidth	Optimal	2x Optimal	Optimal	2x Optimal	Optimal	2x Optimal	Optimal	2x 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 E.5: Robustness to Main RD Estimates - 1st Period: Ihs

	First Triers				Second Triers		All Triers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coefficient (ITT)	1.294	1.417	1.607	1.556	0.421	0.484	0.618	0.495
SE (BC)	(0.575)*	(0.455)***	(0.588)**	(0.474)***	(0.656)	(0.527)	(0.350)*	(0.276)*
Mean DV Treated (1%)	13.542	13.542	13.594	13.594	11.752	11.752	13.068	13.068
Mean DV Control (1%)	12.019	12.019	12.357	12.357	11.672	11.672	12.113	12.113
N (Politicians)	102	102	85	85	65	65	292	292
N (Non-Politicians)	167	167	153	153	176	176	761	761
Bandwidth	Optimal	2x Optimal	Optimal	2x Optimal	Optimal	2x Optimal	Optimal	2x Optimal

Note: Table showing Bias-corrected standard errors clustered at the individual-level. The dependent variable is ihs(Personal Wealth). 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 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 E.6: ATT estimates for different t^*: Full Table

	t=1	t=2	t=3	t=4	t=5	t=6	t=7
Panel A: t* = 4							
Coefficient (ITT)	1.062	0.342	0	-0.685			
SE (ITT)	(0.399)***	(0.611)	(0.613)	(0.633)			
Coefficient (ATT)	0.997	0.283	-0.053	-0.685			
SE (ATT)	(0.492)**	(0.704)	(0.661)	(0.633)			
N Treated	295	219	172	141			
N Control	774	145	98	78			
Mean DV Treated	12.375	11.709	11.594	12.224			
Mean DV Control	11.004	10.505	11.944	12.677			
Panel B: t* = 5							
Coefficient (ITT)	1.062	0.342	0	-0.685	0.746		
SE (ITT)	(0.399)***	(0.611)	(0.613)	(0.633)	(0.937)		
Coefficient (ATT)	1.094	0.381	0.035	-0.604	0.746		
SE (ATT)	(0.545)**	(0.757)	(0.73)	(0.735)	(0.937)		
N Treated	295	219	172	141	101		
N Control	774	145	98	78	43		
Mean DV Treated	12.375	11.709	11.594	12.224	11.657		
Mean DV Control	11.004	10.505	11.944	12.677	11.997		
Panel C: t* = 6							
Coefficient (ITT)	1.062	0.342	0	-0.685	0.746	-0.129	
SE (ITT)	(0.399)***	(0.611)	(0.613)	(0.633)	(0.937)	(0.562)	
Coefficient (ATT)	1.082	0.369	0.024	-0.614	0.737	-0.129	
SE (ATT)	(0.555)*	(0.766)	(0.741)	(0.747)	(0.977)	(0.562)	
N Treated	295	219	172	141	101	75	
N Control	774	145	98	78	43	42	
Mean DV Treated	12.375	11.709	11.594	12.224	11.657	12.194	
Mean DV Control	11.004	10.505	11.944	12.677	11.997	13.187	
Panel D: t* = 7							
Coefficient (ITT)	1.062	0.342	0	-0.685	0.746	-0.129	-0.771
SE (ITT)	(0.399)***	(0.611)	(0.613)	(0.633)	(0.937)	(0.562)	(0.83)
Coefficient (ATT)	0.997	0.282	-0.054	-0.686	0.672	-0.189	-0.771
SE (ATT)	(0.574)*	(0.785)	(0.762)	(0.769)	(1.016)	(0.627)	(0.83)
N Treated	295	219	172	141	101	75	52
N Control	774	145	98	78	43	42	23
Mean DV Treated	12.375	11.709	11.594	12.224	11.657	12.194	12.112
Mean DV Control	11.004	10.505	11.944	12.677	11.997	13.187	13.103

Note: Table showing coefficients effects of stints $\{1, ..., t^*\}$ under different $t^* \in \{4, 5, 6, 7\}$. 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 both panels control for birthplace population, birthplace characteristics, age at election, newspaper recommendations (party). *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

Table E.7: ATT estimates for different t^*: Different BW Selector

	t=1	t=2	t=3	t=4	t=5	t=6	t=7
Panel A: t* = 4							
Coefficient (ITT)	1.287	0.583	-0.126	-0.803			
SE (ITT)	(0.45)***	(0.665)	(0.683)	(0.708)			
Coefficient (ATT)	1.193	0.499	-0.187	-0.803			
SE (ATT)	(0.555)**	(0.768)	(0.737)	(0.708)			
N Treated	295	219	172	141			
N Control	774	145	98	78			
Mean DV Treated	12.375	11.709	11.594	12.224			
Mean DV Control	11.004	10.505	11.944	12.677			
Panel B: t* = 5							
Coefficient (ITT)	1.287	0.583	-0.126	-0.803	0.711		
SE (ITT)	(0.45)***	(0.665)	(0.683)	(0.708)	(0.995)		
Coefficient (ATT)	1.284	0.592	-0.103	-0.725	0.711		
SE (ATT)	(0.61)**	(0.823)	(0.808)	(0.817)	(0.995)		
N Treated	295	219	172	141	101		
N Control	774	145	98	78	43		
Mean DV Treated	12.375	11.709	11.594	12.224	11.657		
Mean DV Control	11.004	10.505	11.944	12.677	11.997		
Panel C: t* = 6							
Coefficient (ITT)	1.287	0.583	-0.126	-0.803	0.711	-0.158	
SE (ITT)	(0.45)***	(0.665)	(0.683)	(0.708)	(0.995)	(0.578)	
Coefficient (ATT)	1.269	0.577	-0.117	-0.738	0.699	-0.158	
SE (ATT)	(0.619)**	(0.833)	(0.819)	(0.829)	(1.036)	(0.578)	
N Treated	295	219	172	141	101	75	
N Control	774	145	98	78	43	42	
Mean DV Treated	12.375	11.709	11.594	12.224	11.657	12.194	
Mean DV Control	11.004	10.505	11.944	12.677	11.997	13.187	
Panel D: t* = 7							
Coefficient (ITT)	1.287	0.583	-0.126	-0.803	0.711	-0.158	-0.906
SE (ITT)	(0.45)***	(0.665)	(0.683)	(0.708)	(0.995)	(0.578)	(0.848)
Coefficient (ATT)	1.17	0.476	-0.208	-0.822	0.623	-0.229	-0.906
SE (ATT)	(0.637)*	(0.851)	(0.84)	(0.851)	(1.076)	(0.644)	(0.848)
N Treated	295	219	172	141	101	75	52
N Control	774	145	98	78	43	42	23
Mean DV Treated	12.375	11.709	11.594	12.224	11.657	12.194	12.112
Mean DV Control	11.004	10.505	11.944	12.677	11.997	13.187	13.103

Note: Table showing coefficients effects of stints $\{1, ..., t^*\}$ under different $t^* \in \{4, 5, 6, 7\}$. 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 *certwo* bandwidth selector. Standard errors are calculated using the delta method. The estimates in both panels control for birthplace population, birthplace characteristics, age at election, newspaper recommendations (party). *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

Table E.8: RD Estimates For Young & Old Politicians

	Median		30 vs. 70		20 vs. 80	
	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient (Young)	-0.175	0.459	-0.056	0.689	-0.922	-0.062
SE (Young)	(0.712)	(0.597)	(1.168)	(1.023)	(1.528)	(1.240)
Coefficient (Old)	1.521	1.786	1.618	1.552	1.835	1.464
SE (Old)	(0.679)**	(0.652)***	(0.724)**	(0.685)**	(0.897)**	(0.883)
Mean DV Treated	12.225	12.214	12.644	12.791	12.393	12.714
Mean DV Control	10.666	10.497	10.954	11.114	10.650	10.775
N Treated	283	342	159	194	95	122
N Control	733	814	444	492	296	328
Bandwidth	Optimal	Optimal	Optimal	Optimal	Optimal	Optimal

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 clustered at the individual level. The first two columns show estimates of the returns for individuals aged above and below the median age, the second two estimates the results for individuals aged above the 70th quantile and below the 30th quantile, and the third pair shows the results for individuals aged above the 80th quantile and below the 20th quantile. Columns (1), (3) and (5) contain estimates with covariates including district characteristics, number of tries, number of votes, nd number of candidates. Columns (2), (4) and (6) control for number of tries, party, and district population. *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table E.9: RD Estimates For Young Politicians Who Died Young vs. Died Old

	Median Cut-Off		40q Cut-Off		30q Cut-Off	
	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient (Died Young)	-0.748	-0.116	-1.151	-0.279	-1.463	-0.287
SE (Died Young)	(0.948)	(0.899)	(1.162)	(1.079)	(1.715)	(1.430)
Coefficient (Died Old)	0.331	0.721	0.566	0.889	0.494	0.772
SE (Died Old)	(0.754)	(0.564)	(0.698)	(0.562)	(0.538)	(0.449)
Mean DV Treated	11.598	11.520	11.598	11.520	11.598	11.520
Mean DV Control	10.920	10.433	10.920	10.433	10.920	10.433
N Treated	151	177	151	177	151	177
N Control	369	407	369	407	369	407
Bandwidth	Optimal	Optimal	Optimal	Optimal	Optimal	Optimal

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 clustered at the individual level. The first two columns show estimates of the returns for individuals with a below and above-median lifespan after election, the second two estimates the results for individuals with a lifespan after election below and above the 40th quantile, and the third pair shows the results for individuals with a lifespan after election below and above the 30th quantile. Columns (1), (3) and (5) contain estimates with covariates including district characteristics, number of tries, number of votes, nd number of candidates. Columns (2), (4) and (6) control for number of tries, party, and district population. *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table E.10: Robustness Check - Lifespan

	First Triers				Second Triers		All Triers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coefficient (ITT)	1.032	1.301	1.742	1.239	3.186	3.793	0.510	1.115
SE (BC)	(2.427)	(1.973)	(2.639)	(2.137)	(2.942)	(2.408)	(1.522)	(1.215)
Mean DV Treated (1%)	23.934	23.934	24.316	24.316	24.813	24.813	23.619	23.619
Mean DV Control (1%)	17.092	17.092	18.770	18.770	21.443	21.443	21.630	21.630
N (Politicians)	150	150	122	122	114	114	447	447
N (Non-Politicians)	258	258	167	167	201	201	842	842
Bandwidth	Optimal	2x Optimal	Optimal	2x Optimal	Optimal	2x Optimal	Optimal	2x Optimal

Note: Table showing Bias-corrected standard errors clustered at the individual-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 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 E.11: Correlation between Wealth and Probability of Election

	(1)	(2)	(3)	(4)	(5)	(6)
Personal Wealth	0.016***	0.021**	−0.015	−0.031**	−0.002	−0.024
	(0.006)	(0.010)	(0.011)	(0.013)	(0.018)	(0.020)
N	1002	361	251	199	150	114
Adj. R2	0.25	0.10	0.11	0.03	−0.02	0.23
Party Controls	Yes	Yes	Yes	Yes	Yes	Yes
Electoral Controls	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes

* p < 0.1, ** p < 0.05, *** p < 0.01

Note: Robust standard errors in parentheses. Analysis show the correlation between end-of-life wealth and probability of election in the 1st election in (1). Then, in the second election given that the first election was won, in (2), etc. Estimates are conditional on party controls, electoral controls, and district fixed effects. *: p<0.1, **: p<0.05, ***:p<0.01.

Table E.12: Correlation between Wealth and Probability of Candidacy

	(1)	(2)	(3)	(4)	(5)	(6)
Personal Wealth	−0.002	−0.002	0.013	0.040***	0.002	0.044**
	(0.007)	(0.010)	(0.013)	(0.013)	(0.015)	(0.020)
N	1002	361	251	199	150	114
Adj. R2	0.10	0.10	0.07	0.11	0.13	0.05
Party Controls	Yes	Yes	Yes	Yes	Yes	Yes
Electoral Controls	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes

* p < 0.1, ** p < 0.05, *** p < 0.01

Note: Robust standard errors in parentheses. Analysis show the correlation between end-of-life wealth and probability of candidacy in the 1st election in (1). Then, in the second election given that the first election was won, in (2), etc. Estimates are conditional on party controls, electoral controls, and district fixed effects. *: p<0.1, **: p<0.05, ***:p<0.01.

Table E.13: Correlation between Wealth and Probability of Candidacy and Recommendation

	(1)	(2)	(3)	(4)	(5)	(6)
Personal Wealth	0.006	0.004	0.000	0.023**	−0.008	0.048**
	(0.005)	(0.009)	(0.010)	(0.011)	(0.014)	(0.023)
N	1002	361	251	199	150	114
Adj. R2	0.16	0.17	0.18	0.27	0.24	0.05
Party Controls	Yes	Yes	Yes	Yes	Yes	Yes
Electoral Controls	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes

* p < 0.1, ** p < 0.05, *** p < 0.01

Note: Robust standard errors in parentheses. Analysis show the correlation between end-of-life wealth and probability of candidacy and recommendation in the 1st election in (1). Then, in the second election given that the first election was won, in (2), etc. Estimates are conditional on party controls, electoral controls, and district fixed effects. *: p<0.1, **: p<0.05, ***:p<0.01.

Table E.14: Change in Candidate Composition After/Before Party Formation

	(0,5]		[-5,0)			
	Mean	SD	Mean	SD	t-stat.	p-value
Panel A: Newspaper Recommendations						
Rec.: Protestant	0.25	0.43	0.19	0.39	1.552	0.121
Rec.: Liberal	0.33	0.47	0.30	0.46	0.739	0.460
Rec.: Socialist	0.08	0.28	0.02	0.14	3.224	0.001***
Rec: Catholic	0.23	0.42	0.15	0.36	2.085	0.038**
Panel B: Demographic Characteristics						
Lifespan	22.23	11.00	19.78	10.93	1.904	0.058*
Age at Election	44.82	9.77	47.15	11.31	-1.997	0.047**
Year of Death	1912.28	16.36	1909.78	19.86	1.327	0.185
Year of Election	1887.40	7.53	1884.79	8.16	3.531	0.000***
Panel C: Election Characteristics						
Log Turnout	8.05	0.70	7.88	0.74	2.564	0.011**
Log Turnout Previous	7.89	0.69	7.82	0.72	1.149	0.251
Panel D: Birthplace Characteristics						
Log Population 1859	9.34	1.56	9.51	1.97	-0.810	0.418
Share Protestant	0.67	0.23	0.70	0.20	-1.334	0.183
Share Catholic	0.31	0.23	0.27	0.20	1.700	0.090*
Labor Force Share Agricul.	0.05	0.09	0.04	0.07	1.345	0.180
Labor Force Share Industry	0.20	0.10	0.20	0.09	0.356	0.722
Taxes Per Capita 1859	4.04	1.46	4.06	1.47	-0.120	0.905
Taxes Per Capita 1889	5.06	1.44	5.07	1.41	-0.052	0.959
Distance to the Hague	101.65	63.84	102.90	68.26	-0.171	0.864
Panel E: District Characteristics						
Share Protestant	0.61	0.28	0.64	0.25	-1.000	0.318
Share Catholic	0.36	0.28	0.34	0.25	0.987	0.324
Labor Force Share Agricul.	0.07	0.09	0.06	0.09	0.361	0.718
Labor Force Share Industry	0.22	0.10	0.21	0.09	0.679	0.498

Note: Table shows means and standard deviations for candidates who have not been elected before in two groups: from 0 to 5 years after party formation, and from 5 to 0 years before party formation. I then conduct Welch t-tests and show the p-value. Significance is indicated as follows: *: p<0.1, **: p<0.05, ***: p<0.01.

Table E.15: Estimates In and Out-Party, Per Party

	Catholic		Liberal		Protestant	
	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient (Without Party)	-1.709	-1.661	1.112	1.103	1.074	1.360
SE (Without Party)	(0.996)	(1.034)	(0.840)	(0.879)	(0.858)	(0.948)*
Coefficient (Within Party)	9.729	-2.402	-0.975	-0.838	0.569	0.563
SE (Within Party)	(11.637)	(16.103)	(0.886)	(0.894)	(1.160)	(1.065)
p-value Difference	0.11	1.75	0.066	0.104	1.31	1.204
Mean DV Treated	10.274	10.274	12.560	12.560	12.082	12.082
Mean DV Control	10.227	10.227	10.549	10.549	11.051	11.051
N Treated	47	49	173	174	73	73
N Control	79	84	254	259	296	298
Bandwidth	Optimal	Optimal	Optimal	Optimal	Optimal	Optimal

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. The first two columns show estimates of the returns for the first-triers for the first stint, the second two estimates the returns for the second stint, and the third pair shows the results for all triers. Columns (1), (3) and (5) contain estimates with covariates including party, lifespan, number of votes, age, and number of candidates. Columns (2), (4) and (6) control for number of tries, party, district economic composition and total amount of votes. *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table E.16: ATT estimates for different t^* - Before Party Formation

	t=1	t=2	t=3	t=4	t=5	t=6	t=7
Panel A: t* = 4							
Coefficient (ITT)	1.315	1.505	0.364	-0.378			
SE (ITT)	(0.496)***	(1.219)	(0.791)	(0.641)			
Coefficient (ATT)	1.277	1.518	0.322	-0.378			
SE (ATT)	(0.666)*	(1.383)	(0.863)	(0.641)			
N Treated	190	150	113	88			
N Control	527	107	50	44			
Mean DV Treated	12.008	11.801	10.868				
Mean DV Control	10.47	7.903	11.635	12.633			
Panel B: t* = 5							
Coefficient (ITT)	1.315	1.505	0.364	-0.378	-0.077		
SE (ITT)	(0.496)***	(1.219)	(0.791)	(0.641)	(1.003)		
Coefficient (ATT)	1.27	1.51	0.315	-0.383	-0.077		
SE (ATT)	(0.69)*	(1.408)	(0.901)	(0.716)	(1.003)		
N Treated	190	150	113	88	64		
N Control	527	107	50	44	23		
Mean DV Treated	12.008	11.801	10.868		10.101		
Mean DV Control	10.47	7.903	11.635	12.633	12.403		
Panel C: t* = 6							
Coefficient (ITT)	1.315	1.505	0.364	-0.378	-0.077	-0.317	
SE (ITT)	(0.496)***	(1.219)	(0.791)	(0.641)	(1.003)	(0.815)	
Coefficient (ATT)	1.238	1.478	0.288	-0.408	-0.1	-0.317	
SE (ATT)	(0.704)*	(1.423)	(0.921)	(0.741)	(1.063)	(0.815)	
N Treated	190	150	113	88	64	48	
N Control	527	107	50	44	23	25	
Mean DV Treated	12.008	11.801	10.868		10.101	12.194	
Mean DV Control	10.47	7.903	11.635	12.633	12.403		
Panel D: t* = 7							
Coefficient (ITT)	1.315	1.505	0.364	-0.378	-0.077	-0.317	-3.646
SE (ITT)	(0.496)***	(1.219)	(0.791)	(0.641)	(1.003)	(0.815)	(2.791)
Coefficient (ATT)	1.031	1.264	0.105	-0.573	-0.253	-0.459	-3.646
SE (ATT)	(0.743)	(1.462)	(0.969)	(0.795)	(1.132)	(0.924)	(2.791)
N Treated	190	150	113	88	64	48	27
N Control	527	107	50	44	23	25	11
Mean DV Treated	12.008	11.801	10.868		10.101	12.194	
Mean DV Control	10.47	7.903	11.635	12.633	12.403		13.103

Note: Table showing coefficients effects of stints $\{1, ..., t^*\}$ under different $t^* \in \{4, 5, 6, 7\}$ before party formation. 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 both panels control for birthplace population, birthplace characteristics, age at election, newspaper recommendations (party). *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

Table E.17: ATT estimates for different t^* - After Party Formation

	t=1	t=2	t=3	t=4	t=5	t=6	t=7
Panel A: t* = 4							
Coefficient (ITT)	0.275	1.253	1.13	0.128			
SE (ITT)	(0.617)	(0.465)***	(0.624)*	(0.727)			
Coefficient (ATT)	0.361	1.322	1.136	0.128			
SE (ATT)	(0.666)	(0.513)**	(0.655)*	(0.727)			
N Treated	131	94	82	76			
N Control	238	60	51	36			
Mean DV Treated	12.27	12.053	13.045	12.078			
Mean DV Control	10.541	11.096	11.848	12.742			
Panel B: t* = 5							
Coefficient (ITT)	0.275	1.253	1.13	0.128	0.767		
SE (ITT)	(0.617)	(0.465)***	(0.624)*	(0.727)	(0.558)		
Coefficient (ATT)	0.485	1.444	1.251	0.239	0.767		
SE (ATT)	(0.714)	(0.561)**	(0.712)*	(0.808)	(0.558)		
N Treated	131	94	82	76	51		
N Control	238	60	51	36	33		
Mean DV Treated	12.27	12.053	13.045	12.078	13.214		
Mean DV Control	10.541	11.096	11.848	12.742	11.817		
Panel C: t* = 6							
Coefficient (ITT)	0.275	1.253	1.13	0.128	0.767	0.267	
SE (ITT)	(0.617)	(0.465)***	(0.624)*	(0.727)	(0.558)	(0.64)	
Coefficient (ATT)	0.509	1.468	1.274	0.26	0.786	0.267	
SE (ATT)	(0.729)	(0.577)**	(0.728)*	(0.825)	(0.603)	(0.64)	
N Treated	131	94	82	76	51	38	
N Control	238	60	51	36	33	17	
Mean DV Treated	12.27	12.053	13.045	12.078	13.214		
Mean DV Control	10.541	11.096	11.848	12.742	11.817	13.187	
Panel D: t* = 7							
Coefficient (ITT)	0.275	1.253	1.13	0.128	0.767	0.267	-2.952
SE (ITT)	(0.617)	(0.465)***	(0.624)*	(0.727)	(0.558)	(0.64)	(1.623)*
Coefficient (ATT)	-0.032	0.934	0.768	-0.224	0.362	-0.129	-2.952
SE (ATT)	(0.878)	(0.726)	(0.88)	(0.98)	(0.784)	(0.857)	(1.623)*
N Treated	131	94	82	76	51	38	30
N Control	238	60	51	36	33	17	18
Mean DV Treated	12.27	12.053	13.045	12.078	13.214		12.112
Mean DV Control	10.541	11.096	11.848	12.742	11.817	13.187	

Note: Table showing coefficients effects of stints {1, ..., t^*} under different $t^* \in \{4, 5, 6, 7\}$ after party formation. 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 both panels control for birthplace population, birthplace characteristics, age at election, newspaper recommendations (party). *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

E.2 Figures

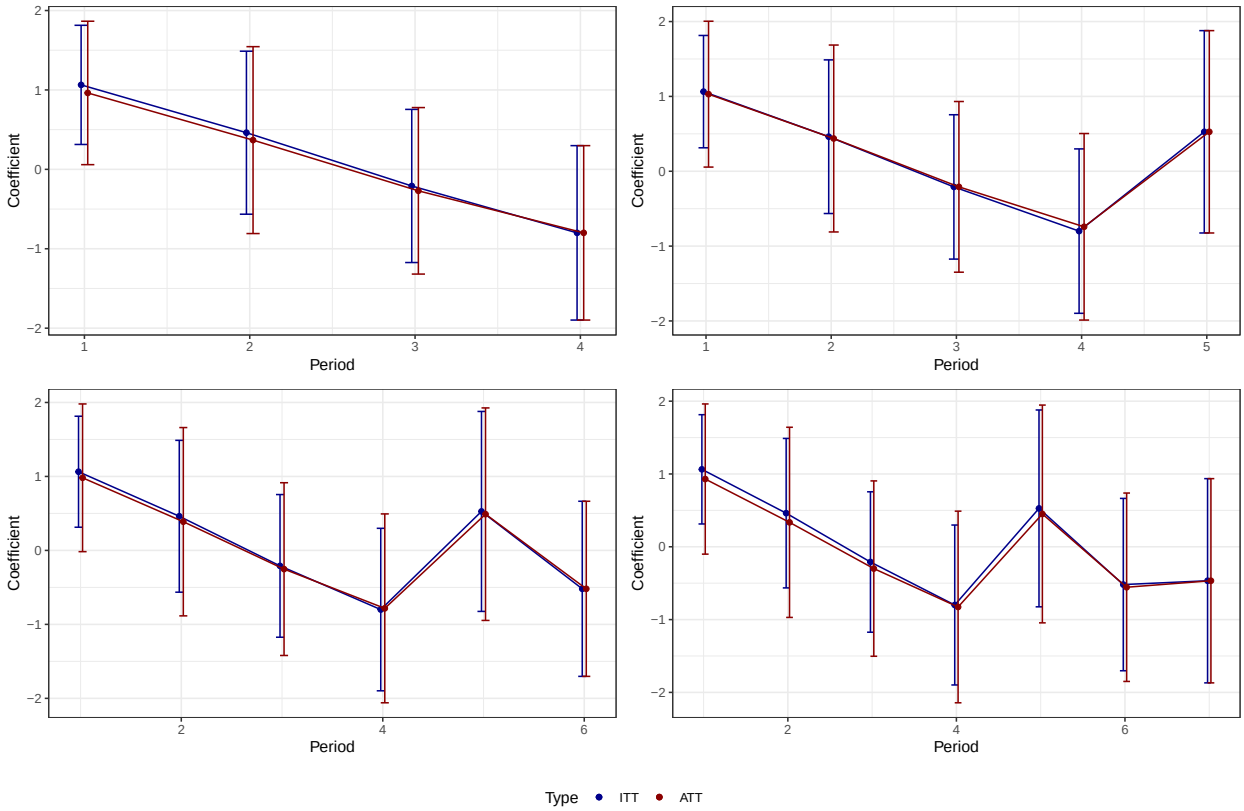

Figure 4: Robustness to t^*, flexible bandwidth and with covariates

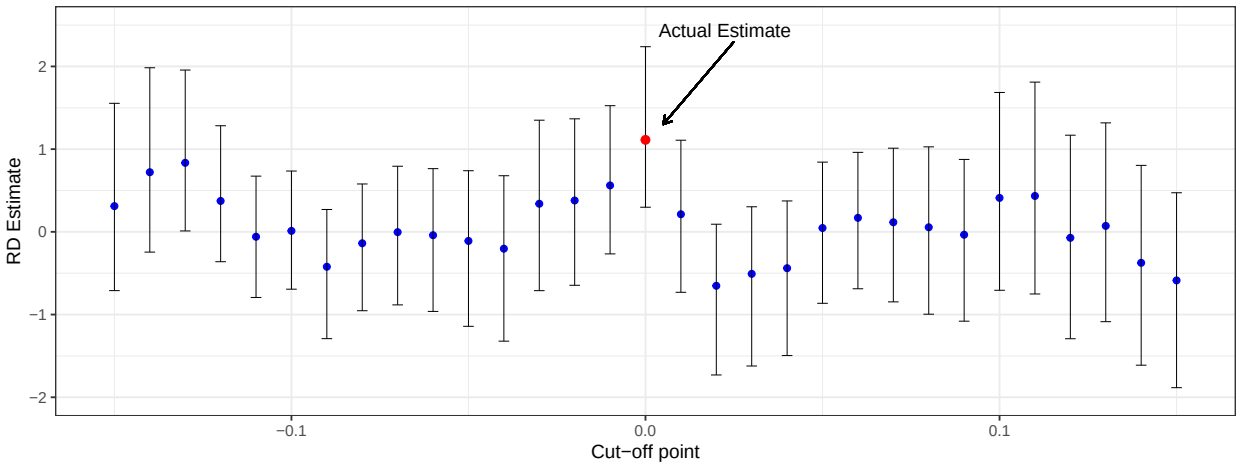

Figure 5: Placebo Test for First Term

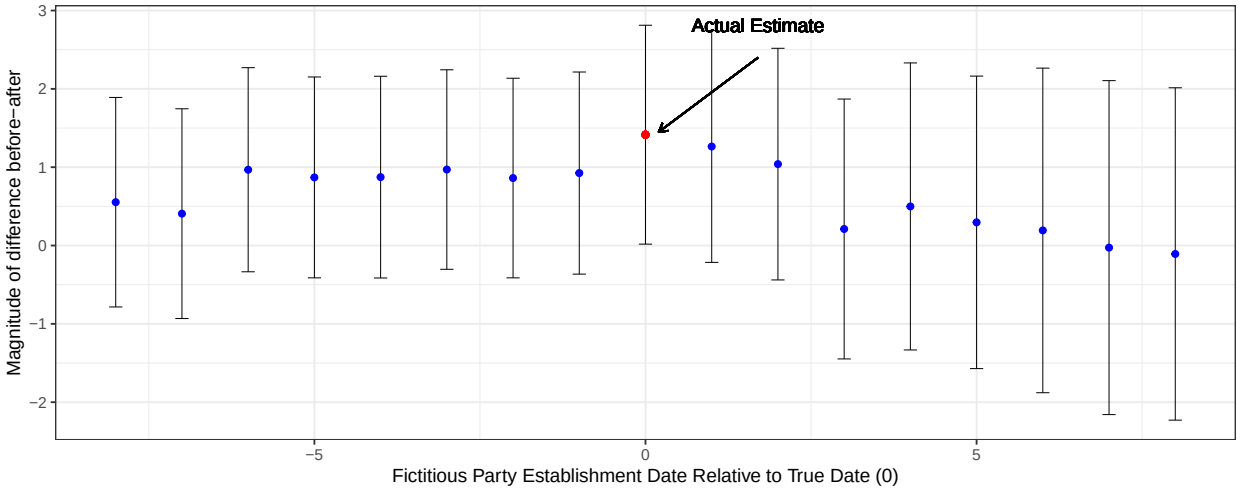

Figure 6: Placebo Test Party Formation