Returns to Politics Under A Changing Political System

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Abstract:

Economists frequently assert that politicians derive financial returns from a political career, but it is unclear how they manage to do so. In this paper, I derive estimates of the returns to consecutive lower house mandates exploiting the repeated treatment assignment resulting from Dutch district-level elections (1860-1917) using a dynamic regression discontinuity methodology. Based on newly-collected data from probate inventories, I obtain a measure of wealth for a dynamic sample of just-elected politicians and their losing contenders. I document that politicians' returns to politics are concentrated in the first period of political activity, strongly favoring an in-office explanation. I also shed light on the role of political parties in constraining politicians' ability to benefit financially from their political career: after the establishment of political parties, there is only limited evidence of politicians being able to profit from their political career, suggesting political parties discipline politicians' in-office behavior. I investigate several contending explanations, and find that just-elected politicians are not more likely to take up certain careers after having been elected, and that suffrage extensions do not influence the relationship between political office and personal wealth.

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1 Introduction

Politicians are generally expected to act in the interest of those who elected them (Persson and Tabellini, 2002; Duggan and Martinelli, 2017). In many real-life cases, this turns out to be only partially true. Politicians are often suspected to use and abuse their political position for private gain, or otherwise pursue policies that are counter to the interests of their constituents. Despite the many attempts that been made to regulate politicians (see e.g. Djankov et al., 2010, for a survey), empirically, several studies have shown the existence of particular forms of returns to politics, that is to say, benefits accruing to politicians beyond their formal compensation. Most authors documents the existence of returns to politics in monetary forms (Svaleryd and Vlachos, 2009; Eggers and Hainmueller, 2009; Amore and Bennedsen, 2013; Fisman et al., 2014) or in the form of prioritizing one's ideology over electoral preferences (Peltzman, 1984; Mian et al., 2010)

However, there is no clear consensus when it comes to explaining these empirical findings. Some authors argue that the benefits of a political career are mostly accrued during a political career (Amore and Bennedsen, 2013; Fisman et al., 2014; Bourveau et al., 2021), whereas others argue that the benefits can be cristallized over a longer period of time (Querubin and Snyder Jr, 2009), in the forms of nepotism (Dal Bó et al., 2009), or can even be channeled to other individuals, e.g. relatives (Fafchamps and Labonne, 2017; Folke et al., 2017). Furthermore, it is not clear what determines the magnitude of returns to politics. Eggers and Hainmueller (2009) hint that party organization might be a significant determinant of the extent to which politicians can prioritize their own interests, motivated by a sharp difference in political rents between Conservative and Labor MPs in Post-WWII Britain. Fisman et al. (2014) find a differential effect in various Indian states that have different levels of corruption. Querubin et al. (2011) hint at the influence of government size and monitoring by the media as possible determinants of returns to a political career. These explanations are difficult to verify, as most research exploits a static setting to identify the magnitude of returns to politics, leaving the institutional environment constant.

This study takes a long-term perspective and explicitly investigates the institutional determinants of returns to politics in the Netherlands from 1848-1917. I make use of close elections to establish the existence and magnitude of returns to politics using a dynamic regression discontinuity strategy (Cellini et al., 2010). The Netherlands employed a district system (De Jong, 1999). In each district, a small number of candidates took part, and these elections were frequently hotly contested, so that close elections are a relatively frequent phenomenon. 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). I empirically investigate whether political parties are able to curb the returns to politics for individual politicians, by making use of data on *newspaper recommendations* for politicians, which allow me to identify political allegiance before political parties were established. This period was also marked by several franchise extensions, suddenly making the electorate much larger in each district (Van den Berg and Vis, 2013). Finally, this period saw the appearance of 'career politicians' and 'political careers' in the spirit of Mattozzi and Merlo (2008).

Methodologically, I use a dynamic regression discontinuity design, exploiting repeated quasi-random treatment assignment of close elections between individuals who were never elected before, but also between individuals who have all been elected once, twice, or more often. I investigate whether treatment

assignment is as good as random by gathering a sizeable dataset containing information about the candidates' background, origin, political orientation and demographics, as well as the district characteristics in which close elections took place. This situation allows me to reliably estimate the returns to consecutive stints of holding political office. The interpretation of the analysis is complicated by the presence of incumbency advantages (Lee, 2008). Any estimated total effect of being elected on personal wealth contains a *ceteris paribus* effect, but also the incumbency advantages times the future *ceteris paribus* effects. Using a procedure similar to Cellini et al. (2010), I retrieve recursive estimates of the *ceteris paribus* effects from the estimated total effects to each political stint, and the incumbency advantages. These estimates can be interpreted as a 'marginal return curve' to consecutive stints of political office.

The results of this analysis show that the returns to political office are primarily concentrated in the first period of political office. They show that politicians who marginally won their first election are significantly wealthier at the end of their life than politicians who marginally lost. These results are robust to the inclusion of covariates, many parameter choices, and also pass various placebo tests. There are little to no returns to second or longer stays in the lower house. This finding is consistent with the view that politics provides (exhaustive) human capital, but also with a view of rent-seeking politicians being able to accrue rents in only one stint. The result challenges explanations that imply a constant marginal return curve to political office (Persson and Tabellini, 2002; Caselli and Morelli, 2004; Baltrunaite, 2020; Bourveau et al., 2021).

Afterwards, I set out to find the institutional determinants of the returns to politics. I find that the establishment of political parties decreases the returns to politics significantly, to the point that the point estimate is close to zero. These results are not driven by a change in individuals deciding to run for office (Besley, 2005), as there is no relationship between being elected into politics, and the likelihood of a lucrative business career after politics, either before or after the establishment of political parties. Neither is there any evidence of lower house politics being a stepping stone to different, potentially more lucrative political functions, thus ruling out explanations that imply returns to politics are collected only indirectly, after a political career. This also implies that political careers were not valuable to potential future employers, making a human capital-based explanation (cf. Diermeier et al., 2005; Mattozzi and Merlo, 2008) less plausible. I also investigate whether suffrage extensions, bringing about a substantial increase in monitoring, influence political rents, but I find no substantial evidence of their influence.

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

The same period also saw the development and rise in popularity of political parties. As the dif-

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

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 5.3, I investigate various alternative explanations. After concluding in section 6, I provide various robustness checks in appendix A.

2 Historical Background

2.1 Dutch Politics 1848-1917

In the period 1848-1917, all elections to the lower house were organized in the framework of a district system. Before 1848, the year in which constitutional reforms liberalized the electoral system and political institutions of the country, delegates to the Lower house were elected indirectly: the enfranchised electorate elected delegates to an intermediary assembly called the Provincial Estates, which then elected delegates to the lower house. Delegates to the upper house were elected in a similar way, and in contrast to the lower house elections, the 1848 constitution left this system intact for the elections to the upper house, whereas the elections to the lower house were subject to reform, effectively rendering them direct, and more democratic (Blok, 1987). From 1849 onward, lower house elections took place biannually, in which every two years, half of the seats were up for contest. In almost all cases, districts features two seats, and in each election, one seat was up for election (De Jong, 1999). This also meant that 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 secondary remunerated function in government lost membership by default. They could, however, stand for reelection (De Jong, 1999; Loots, 2004). Apart from preliminary decease of 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*), in which the stated objective was that each district, and consequently each representative, represent about 45,000 inhabitants (De Jong, 1999). Accordingly, after the constitutional revision in 1848, the lower house had 68 seats, corresponding roughly to the representation of 45,000 inhabitants by each of those seats. In the meantime, however, population growth had taken off, meaning that it was more and more difficult to apply this rule. The lawmakers responded to this issue by increasing the number of seats, creating and changing the composition of districts: the number of lower house seats raised from 68 to 86 in about 10 years. However, because of the stakes involved (issues related to gerrymandering), it became more and more difficult to agree upon a given composition, effectively delaying any reform from 1878 to a constitutional revision in 1887, after it was capped at 100.

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 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, with population growth not stalling, and compromise aimed at the reallocation of districts being difficult, the district system saw imbalances between districts become more and more salient. This particularly favored sparsely over densely populated districts. Even the electoral law reforms of 1896, which encompassed, among other reforms, a partition of the largest cities into various districts, effectively increasing their representation, could not change the imbalance that disfavored them.

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* (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% according to van der Kolk et al. (2018)), although next to taxes, there were also 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.

3 Data and Sources

3.1 Electoral Data

The *Repositorium Tweede Kamerverkiezingen 1848-1917* (Repository Lower House Elections) contains information about all elections to the Dutch lower house over the period 1848-1917, in which elections were organized at the district-level. This dataset contains the district, date, and type of election (regular, intermediate, second round), as well as the names of the candidates. In addition, the dataset contains the amount of votes they obtained, the number of enfranchised individuals in this district, voter turnout, and also some metadata, including the amount of seats that are contested in the particular election, the type of election, and the election date. Based on these data, I first exclude elections that did not lead directly to a winner (i.e. first rounds of elections which had second rounds, or nullified elections that did not reach the electoral threshold). In total, there are about 2100 unique elections in the district system over the period 1860-1917. In line with other studies using close elections (e.g. Lee, 2008), I use a vote margin-based approach to identify which elections are close: in particular, I first find the *marginal winner (MW)* in the election, which is defined as a winning candidate with the lowest number of votes from all winning candidates. In the vast majority of cases, this amounts to the only winner, because the election had only one seat up for election, but in a minority of the cases, this yields a different candidate. The set $\{\text{Winners}\}_e$ then consists of all election winners in election e. Then, at the candidate-district level (candidate i, district e), I define and compute vote margins 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. In figures 1 and 2, I show the geographical distribution of close elections, taken to be elections where one or more candidates were elected with a margin of less than 20%. Close elections seem to be balanced across the country.

[Figures 1 and 2 here]

3.2 Politician Data

I retrieve a proprietary dataset from the *Politiek Documentatiecentrum* (PDC), 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 career activity 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 diffused who would be the contestants in upcoming elections, which frequently went hand in hand with an endorsement by the editorial board of a particular candidate (Oud, 1997; De Jong, 1999).

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 *Wikipedia*, *genealogieonline.nl*, *Geni.com*, the historical newspaper search engine *Delpher*, and local provincial archives to identify the birth date and place and date and place of decease for non-politicians. In addition, I collect information on their career paths, 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 newly collect archival data on 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.

I have prioritized collecting wealth data for candidates whose margins were closer to zero. In total, out of 6,197 candidate-election pairs, I have been able to collect the wealth for 2,893 candidate-election pairs. These pertain to 515 unique candidates, whereas in total, there are 1,590 candidates. There are 2,877 candidate-election pairs who took place in relatively close elections, for 1,527 of which I have been able to collect personal wealth (53%). The main reason of absence is the aforementioned limited availability of the archives. 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 20%, I was able to collect the 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[1], which incorporates the possibility that politicians who are first elected at different tries can realize different initial wealth effects:

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

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. Note that in this setup, this effect is independent of actual calendar time. In section 5.3.3, I investigate whether suffrage extensions represent a structural break in this relationship. Secondly, $c_{i,t}$ is an indicator reflecting whether a politician is elected for the t'th time after having been elected initially. I restrict the structure such that γ_t does not depend on the number of tries τ. Consequently, γ_t represents the effect on wealth effect of being elected for the t'th time *after* having been elected once. I detail how I estimate the parameters γ_t in section 4.2. Differentiating both sides of equation 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:

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

where $dc_{i,t}/db_{i,\tau}$ represents the incumbency advantage (Lee, 2008), the change in the probability

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

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

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

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

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 γ_t and π_t, which I describe in the next section. The effects γ_t are also of theoretical interest, as they describe the marginal return curve to a political career.

4.2 Estimating Incumbency Advantage and Returns to Tenure

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

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

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

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

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

Again, focusing on an RD-implementation so that $\mathbb{E}[u_i \gamma_t] = 0$, 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:

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

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 tenure effects are only identified under the assumption that for some acceptably large t^*, $\gamma_{t*}^{ITT} = \gamma_{t*}^{ATT}$. In the analysis, I employ this assumption and test its sensitivity for the estimates of γ_t^{ATT} and θ_τ^{ATT}. Additionally, for sufficiently precise estimation of the γ_t^{ITT}, conditionally on being elected $t-1$ times in the lower house, politicians must have participated in close elections afterwards (and a certain share of them must win). I 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).

5 Analysis

5.1 Dynamic Returns to Politcs

5.1.1 Decriptive Statistics and Covariate Balance

The regression discontinuity approach implies random allocation of politician status close to the threshold with respect to pre-treatment variables, which 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 1, and then show various pre-determined potential covariates relating to pre-treatment characteristics in table 2. I confine the analysis of covariate balance to a dataset with only first try individuals, who have never been elected before. In appendix A, I also provide covariate balance tables for different subsamples.

[Tables 1 and 2 here]

Table 1 shows the descriptive statistics of the dataset. In the first panel, 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. A significant fraction of the electorate was also not backed by a (politically-oriented) newspaper. In panel B, I show demographic characteristics: politicians on average life another 22.4 years after their first election, they are on average 49.4 years old when elected. The average turnout in a district was about 2,500, and the and the average population size in 1859 was about 12,500. The birthplace characteristics reflect roughly the demographics of the country: on average 62% of the average politicians' birthplace are Protestants, 35% are Catholic. Similar numbers apply to the districts they are running for office. The average wealth of death of a candidate is about 70,000 guilders, which is equal to about 5 times a Minister's salary in 1900, and is about equal to 1 million euros in present-day terms[3].

Table 2 shows the distribution of the covariates in the treatment and control groups for the all candidates that tried for the first time (and were never elected before). The second to fourth column show the sample means conditional on the absolute value of the margin being < 0.2, whereas 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 evidence by a balance in newspaper recommendations. Similarly, politicians and non-politicians have similar demographic characteristics (panel C). The turnouts in the districts are statistically indistinguishable, and so are other district characteristics (panel E). The birthplace characteristics, however, seem to differ somewhat between politicians and non-politicians (panel D). However, at the margin, these differences vanish. In appendix A, I repeat this analysis for other stints.

[3] According to the IISG currency conversion tool

5.1.2 Returns to a Political Career

In table 3, I show the estimates of equation 3. These estimates correspond to the "Intent-to-Treat" (ITT) effect of being elected on personal wealth. 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 the second time (and consequently weren't elected 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 are estimates including several covariates.

The point estimates are all very similar in magnitude. To interpret the estimates, I focus on column 1. The point estimate of 1.731 implies that politicians who had just been elected would be 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 is significantly more than could be explain by wealth accumulation through politicians' formal remuneration: after the 1848 Constitution, politicians were paid a renumeration of 2000 guilders per year (Elzinga, 1985).[4] 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. Hence, even if one assumes that losing candidates would have negligible earnings and wealth accumulation, these numbers are still far from being able to explain the much higher wealth accumulation among politicians.

[Table 3 here]

[Figure 3 here]

In table 4, I decompose these total effects from a political career into average treatment effects of being elected for the τ'th time. In these analyses, I notably control for the number of tries a candidate has already participated before the particular election. I show coefficient estimates for ITT effects, and the average treatment effects on the treated (ATT), using the recursion defined in equation 2, for $t^* = \{4, 7\}$[5]. Standard errors for the estimates of the ATTs are calculated by the delta method. The estimates are remarkably consistent for different t^*: in both reported cases, as well as in the unreported intermediate cases, the point estimates for the ATT in the first period are 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.

Strikingly, the ATT effects of being elected for any other time other than the first time are insignificantly different from zero, no matter the t^*. This means that the returns to politics are due to the returns in the first period: politicians do not gain any financial advantage of being elected two or more times. In figure 4, I graphically show the robustness of the estimates for the ATT to t^*. These results corroborate

[4] 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.

[5] The parameter t^* is the stint for which the estimated ATTs are equal to the ITTs

that the estimated ATT's are very similar to the estimated ITTs, and that the total effects reported in table 3 are mostly due to the effect of being elected once. Thus, any additional stints after a first stint do not increase politicians' end-of-life wealth. In the remainder of the analysis, I hence focus on the ITT effect from being elected for the first time, and I provide evidence making it more plausible that these returns are indeed accrued in-office. Afterwards, I argue that the establishment of political parties caused the returns to politics to decrease notably, and I also consider several alternative explanations.

[Table 4 here]

[Figure 4 here]

5.2 The Influence of Political Party Formation

The electoral system in the Netherlands after 1848 was centered on individual delegates, not political parties. Politicians were supposed to be independent, not least with respect to their own delegates, and to promote the common interests of the country (de Jong, 2003). Political parties were preceded by *Kiesvereenigingen*, electoral unions, of 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 1895, and the Catholic union of electoral associations was founded in 1893. 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.

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 et al., 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. In practice, there was a near one-to-one correspondence between newspapers and political allegiance.[6] I estimate

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

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 include newspaper recommendation indicators, so that the estimates are conditional on candidates being recommended by the same newspaper.

[Table 5 here, edit and show only first period, more covs]

In table 5, I report the estimates of specification 8. I again focus on the ITT effect of being elected into politics. In panel A, I focus on the first try for the first period, and in panel B, I focus on the first period 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. Unsurprisingly, the point estimate for the subsample without political parties is somewhat higher than the point estimates in table 3. The point estimate for the subsample within political parties is much lower, and depending on the model, fails to attain statistical significance. The difference between the two point estimates is statistically significant. In appendix A, table 10, I show that this result is not due to one particular party.

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 5 seem to come from a combination of political parties, and is not due to the particular organization of one political party.

5.3 Explanations

5.3.1 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 stint, 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 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 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 1915, in his first stint 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 legumes 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."

It is possible, however, that politicians do not accrue in-office rents, but use politics as a gateway to more lucrative professions (Mattozzi and Merlo, 2008). I pay attention to this *indirect benefits* explanation in section 5.3.2. In the same section, I also consider that party formation might have come with different incentives and thus a different candidate pool (Besley, 2005), which might be responsible for the effect. In section 5.3.3, I investigate empirically whether the effects related to the extension of the franchise might have been an institutional change confounding the effect of the introduction of political parties. Lastly, I argue against explanations that imply *constant marginal returns* to political office, among which are insider-information (Bourveau et al., 2021) and procurement-based (Baltrunaite, 2020), on the basis of earlier results.

5.3.2 Indirect Benefits and Selection

In addition to in-office rents, holding political office might also bring about returns of a different kind. 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 en-

vironment, and the economy was still primarily focused on rent extraction, which was now carried out by private firms rather than the government (Lindblad et al., 1993; Steegh et al., 2016; Taselaar, 1914), the benefactors of which were likely individuals at positions in the colonial administration and colonial business. Thirdly, I investigate whether a political career gives individuals more access to a career in finance and business in the metropolitan. The contemporary literature (e.g. Fisman et al., 2014) documents that political connections, and thus politicians, are valuable to firms. Everything else equal, then, politicians might be more likely to take up a position in finance and business than nearly-elected non-politicians.

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

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

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

In table 6, I show the difference in estimated political rents for politicians who (i) were active in business after their political career vs. all others, (ii) were active in the colonies after their political career vs. all others, and (iii) who were active in politics after first being elected in the lower house vs. all others. 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.

[Table 6 here]
[Table X with the interaction with party founded here]
[Table X2 t-test before after here]

In table X, I show the results before and after party formation. The results indicate that there is no difference in the pattern of occupations between politicians who have entered politics after political parties were formed, and those that entered politics before. This runs counter to a selection-based explanations of the findings in section 5.2, and indicates that politicians with similar aspirations and abilities were in the candidate pool before and after political party formation.

In table X2, I also show the results of analyses of pre-treatment characteristics of candidates before and after party formation. These analyses show that the results are not different. This implies that the candidate pool before and after party formation was roughly similar in terms of choices and opportunities for a post-politics career. To further investigate the candidate pool, I statistically compare the candidate pool before and after party formation in terms of pre-treatment characteristics. I find that on a large number of characteristics, there are no differences between these groups, indicating that the candidate pool did not change over time.

5.3.3 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 taxation, and a miscellaneous category called 'declaration', which included paying 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, close to 50%. 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 1896) and elections after a regime strongly resembling universal suffrage (after 1896). The results are displayed in table 7.

[Table 7 here, edit to delete 2nd rents]

The results show that there is no discernible difference between estimated returns to politics before and after various suffrage extensions. In all models, the point estimates from before and after aren't statistically significantly different from each other. The point estimates are comparable in magnitude with the point estimates shown in previous section. In table 3, I implicitly took this into account by estimating the results conditional on suffrage regime (1848-1887, 1887-1896, 1896-1917). In addition, I investigate the temporal pattern of (first-stint) returns to politics in a graphical way. In figure 6, panel A, I plot the returns before and after a cut-off point. These serve as placebo tests for a possible structural break in the treatment effect. These estimates show that rents have a tendency to decrease over time, but that there is no sudden change following the suffrage extension of 1887. Arguably, the suffrage extension in 1896 coincides with the sharp drop in after-period rents from 1897 onward, even though these estimates are never statistically significantly different from each other. In panel B, I also estimate the returns from a moving average of ± 15 years before a particular date. This graph confirms the pattern in panel A, i.e. declining rents over time, until about 1885, following which the estimate stabilizes.

[Figure 6 here]

5.3.4 Constant Marginal Returns

The results in the previous sections show that politicians are only able to engage in rent-seeking in the first period of political activity, after which a political career not gain financial advantage relative to career 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. If a politicians has only limited political capital to engage in rent-seeking activities (à la Curto-Grau and Zudenkova (2018)), 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), it makes sense to deplete that capital during the first period. Furthermore, statistical power could be an issue: given the lower sample size of second-stint or third-stint candidates, it becomes progressively more difficult to identify effects of further stints.

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 set out to 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 stints of political office. I find that politicians can only accrue returns from political office in their first stint. In the second and later stints, the end-of-life wealth of politicians is insignificantly different from candidates who failed to be elected by a small margin.

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 findings imply 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.

I proceed to provide evidence in favor of the view that the returns to politics are in-office rents, and undertake various analyses to show that party discipline is the primary determinant. I provide anecdotal evidence of corruption cases documented by historians (Kroeze, 2013). Most of these cases feature members of parliament in their first stint. I also consider alternative explanations to the in-office rents explanation. In particular, I consider whether the returns are accrued out-of-office by investigating whether just-elected candidates are more likely to work as a mayor, in the colonies, or in finance after holding office than nearly-elected contenders (Mattozzi and Merlo, 2008). I find no evidence of this. 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 are incarnations of political groups with similar ideologies or aggregators of policy preferences (de Jong, 2001; 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 political power. 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 arguging 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 stint in political office, and these returns seem to disappear for later stints. 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). Thirdly, 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.

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A Robustness Checks

A.1 Placebo Tests

[Figure 8 here]

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

A.2 Sensitivity to RD Parameters

I estimate the results in table 3 using flexible bandwidth and different covariates in table 8.

[Table 8 here]

I estimate the results in table 4 using flexible bandwidths. The results are displayed in table 9.

[Table 9 here]

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

[Table 11 here]

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

[Figure 7 here]

A.3 Covariate Balance

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

[Table 12 Here]

Figures and Tables

Tables

Table 1: Descriptive Statistics

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

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

Table 2: Covariate Balance - First Attempts - First Stint

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

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

Table 3: Main RD Estimates - 1st Stint

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

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

Table 4: ATT estimates for different t*

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

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

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

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

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

Table 6: RD Estimates of Effect on Career Paths

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

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

Table 7: RD Estimates - Suffrage Extension and Turnout

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

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

Table 8: Robustness to Main RD Estimates - 1st Stint

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

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

Table 9: ATT estimates for different t*

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

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

Table 10: RD Estimates by Party

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

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

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

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

Note:

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

Table 12: Covariate Balance - Second Stint

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

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

Figures

Figure 1: Close Elections Per District

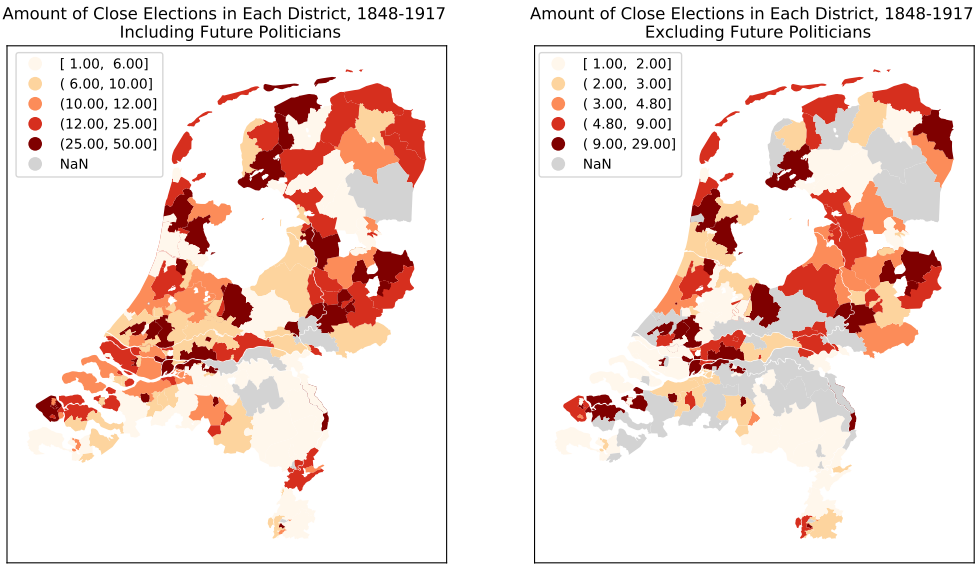
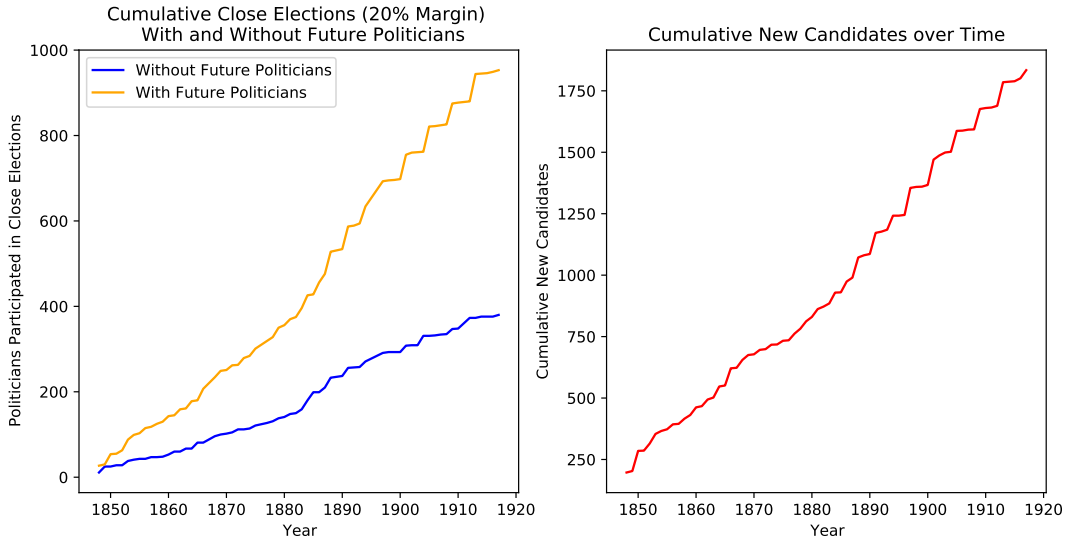


Figure shows the regional spread of elections for the full sample and for the full sample excluding politicians. Since district composition is not static, but changes over time, the data is aggregated to, and displayed as the situation in 1895.

Figure 2: Close Elections over Time



The left panel of the figure shows the count of close elections over time, indicating that they are distributed relatively evenly over time. The right panel shows the cumulative number of new (i.e. never seen before) candidates over time.

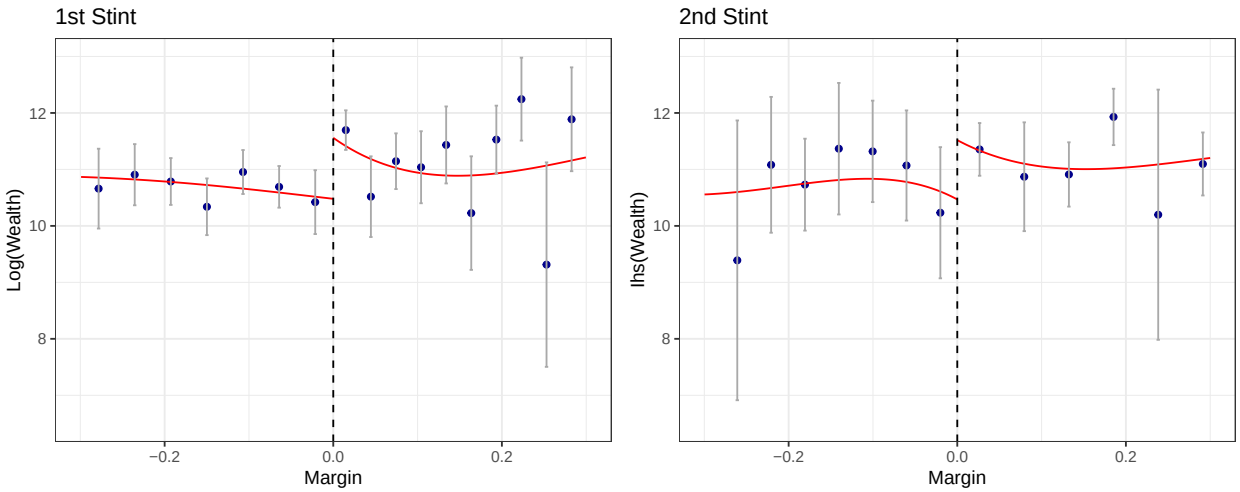


Figure 3: Estimates of Returns to Politics

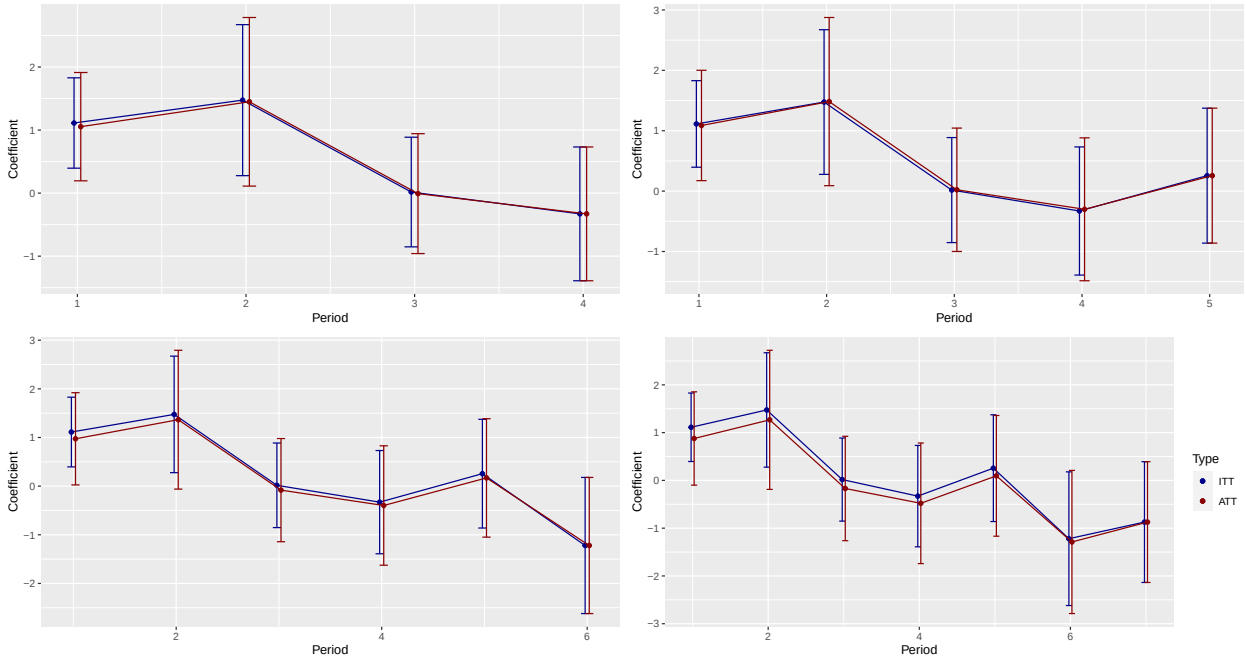


Figure 4: ITTs and ATTs for different t^*

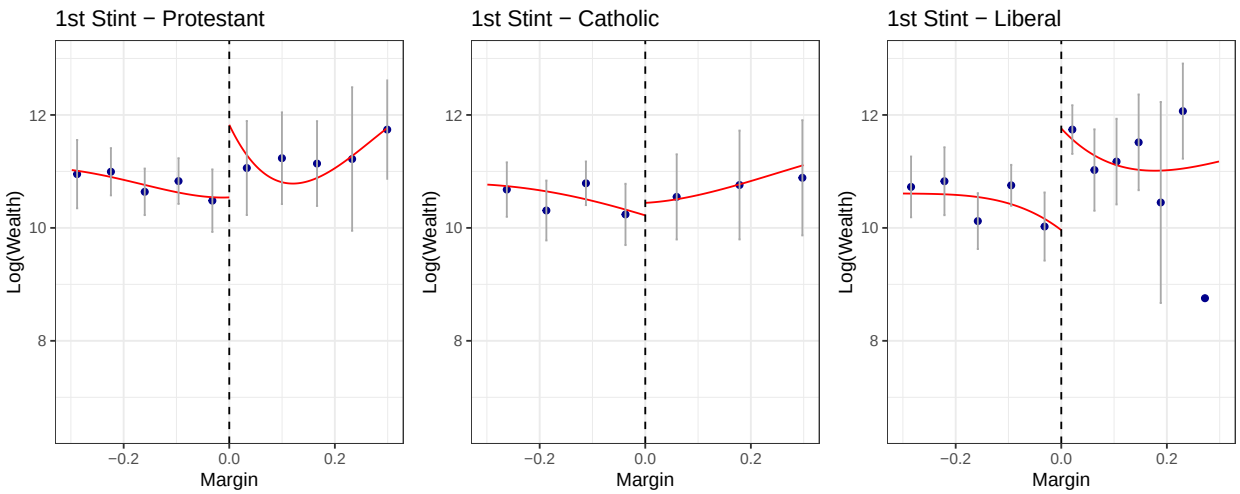


Figure 5: Estimates of Returns per Party

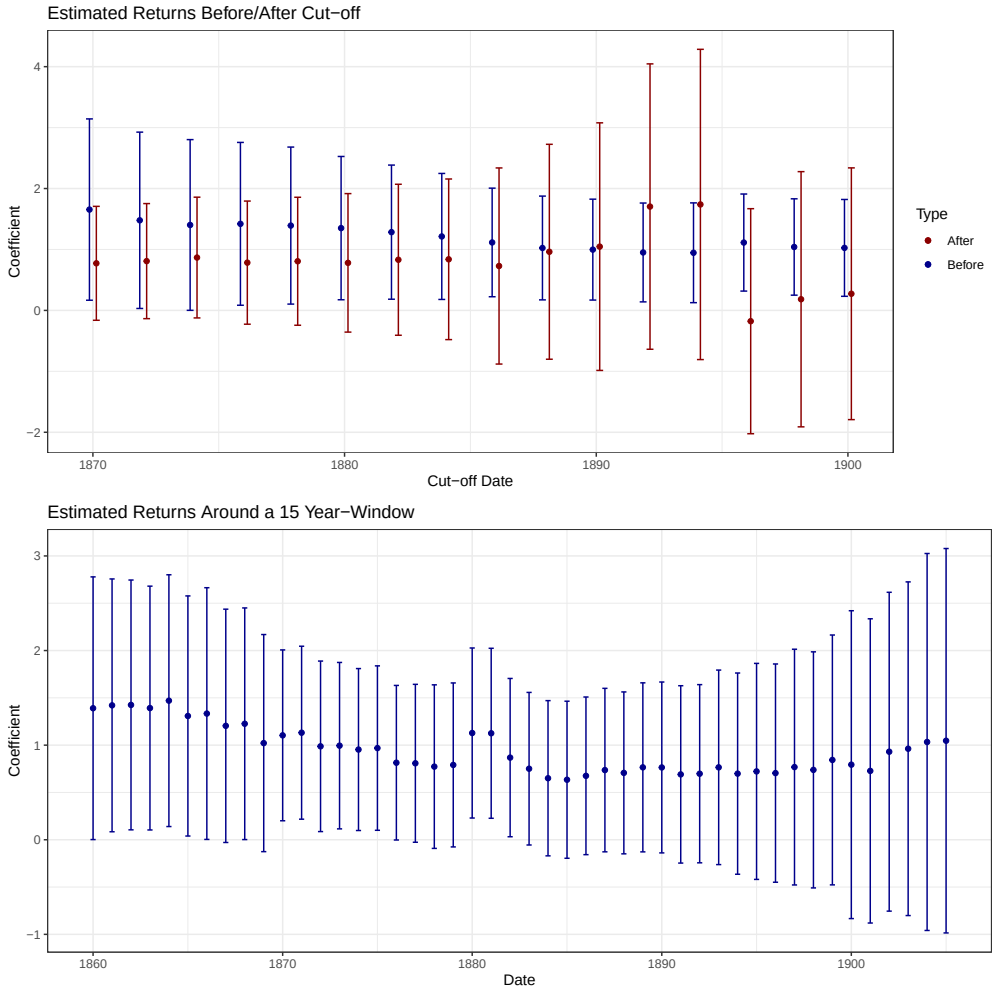


Figure 6: Estimates of Returns - Temporal Patterns

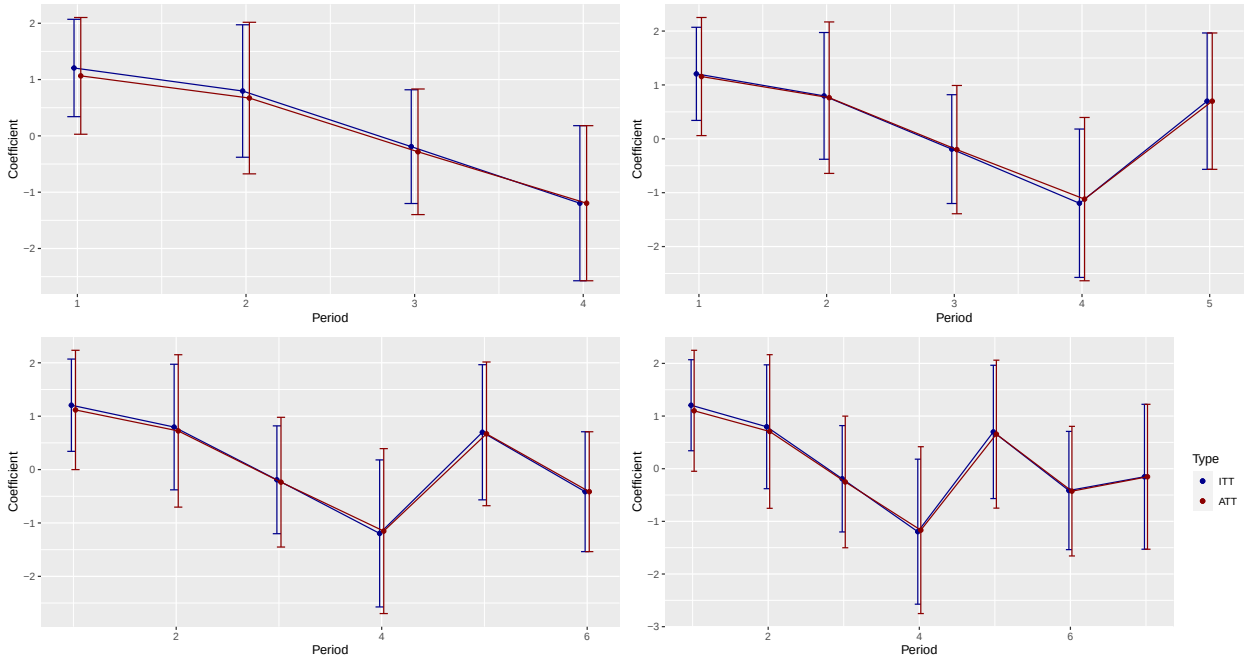


Figure 7: Robustness to t^*, flexible bandwidth and with covariates

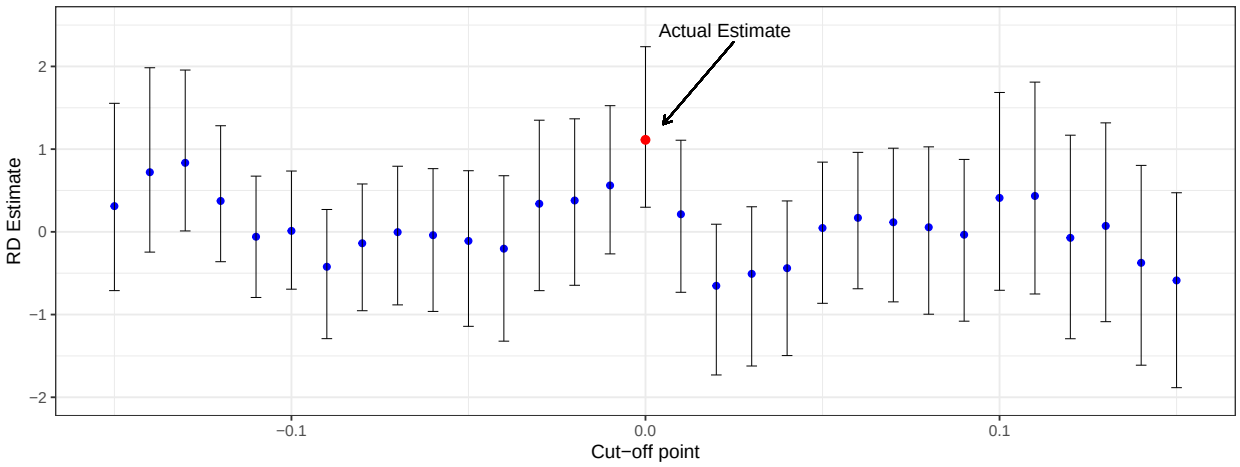


Figure 8: Placebo Test for 1st Stint

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, 2003). 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 1893. 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.