Dynamic Returns to Political Tenure*

Bas Machielsen†

January 12, 2026

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

Abstract:

Economists frequently assert that politicians derive financial returns from a political career, but these returns can be obscured by the varying duration of political careers. In this study, I estimate the financial returns associated with successive mandates in the Lower House, capitalizing on the repetitive treatment assignment through close elections in the Netherlands from 1848-1917. Employing a dynamic regression discontinuity framework, I establish that the financial benefits accruing to politicians are due to the first two periods of political tenure, but no substantial returns emerge during the more advanced career stages. These findings emphasize that politicians elected for a first and second term exhibit significantly higher end-of-life wealth than their losing counterparts, equivalent to several years' salaries. I also explore various potential mechanisms, providing evidence in favor of career-based explanations and against in-office returns.

JEL Classifications: N14, D72, H71

*I am grateful to Oscar Gelderblom, Abe De Jong for many constructive rounds of feedback, remarks, comments and suggestions. I thank Matthijs Korevaar, Kim Oosterlinck, Pierre-Guillaume Méon, Bob Rijkers, Coen Teulings, Raphael Franck (discussant), James Bisbee (discussant) and Daniel Waldenström for useful comments and suggestions. I am also indebted to seminar participants at the 2024 CBS-Princeton Money in Politics Conference, the 2024 Economic History Society Conference, the 2024 European Public Choice Society Conference, and the 2023 Utrecht Applied Economics Seminar, Université Libre de Bruxelles, Erasmus University Economic History Seminar, the 2022 European Historical Economics Society Conference, and the 2022 KVS New Paper Sessions for many helpful remarks and suggestions on an earlier draft of this paper. I also want to thank Jesper van der Most and Margot Hoogerwerf for useful research assistance.

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

1 Introduction

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

Nevertheless, there is no clear consensus in interpreting these empirical observations. Some scholars argue that the benefits reaped from a political career predominantly materialize during the tenure itself (Amore and Bennedsen, 2013; Fisman et al., 2014; Bourveau et al., 2021). Conversely, an opposing perspective asserts that these benefits might crystallize over an extended timeframe (Querubin and Snyder Jr, 2009). In addition to financial returns to politics, the crystallization of benefits might manifest itself over a longer timeframe as nepotism (Dal Bó et al., 2009) extending to other individuals, such as relatives (Fafchamps and Labonne, 2017; Folke et al., 2017). Other studies suggest the returns to politics may be accrued through human capital accumulation on the job (Mattozzi and Merlo, 2008). Moreover, the factors influencing the magnitude of returns to political engagement remain ambiguous. Eggers and Hainmueller (2009) propose that the organizational structure of political parties could significantly influence the degree to which politicians prioritize personal interests. Fisman et al. (2014) discern differential returns to politics across various Indian states characterized by different levels of corruption. Additionally, Querubin et al. (2011) suggest that the government size and scrutiny by the media might influence the returns associated with a political career. Most of these studies focus on a static setting, without considering the dynamic component inherent in the returns to politics.

This study adopts a dynamic perspective, aiming to derive estimates of the returns for each additional period of political activity, thereby tracing out a marginal return curve to political engagement. Using the historical context of the Netherlands from 1848 to 1917, this study exploits the repeated allocation of Lower House membership to estimate the financial returns accruing from successive periods of political office. The study employs close elections to establish the existence and magnitude of financial returns to politics, utilizing a dynamic

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

regression discontinuity strategy (Cellini et al., 2010; Hsu and Shen, 2024). During the 19th century, Dutch elections were structured under a district system (De Jong, 1999). In each district, a limited number of candidates typically participated, yet these elections were frequently intensely contested. The absence of term limits further contributed to a significant number of candidates repeatedly seeking election. This institutional setting facilitates the estimation of returns for subsequent periods of political activity and allows for an examination of the linkage between these returns and evolving political institutions. This approach enables an analysis of the role of monitoring in constraining politicians' ability to extract rents (Barro, 1973; Ferejohn, 1986; Duggan and Martinelli, 2017). Moreover, the study investigates the relationship between financial returns and potential alterations in candidates' career trajectories induced by Lower House membership. This involves an analysis of detailed data on candidate careers, concentrating on whether parliamentary service facilitates the emergence of 'career politicians' or 'political careers,' (Mattozzi and Merlo, 2008), and whether this phenomenon is correlated with the financial returns to political office.

Consistent with broader European trends, the Netherlands underwent profound political transformations during the late 19th and early 20th centuries (Przeworski, 2009). Following liberal reforms in 1848, the nation transitioned from an absolute to a constitutional monarchy with parliamentary oversight (Aerts, 2018). However, suffrage remained severely restricted to males meeting specific tax criteria, despite nominally open eligibility (Van Der Kolk et al., 2018). Subsequent decades witnessed campaigns by politicians and activists that eventually led to universal suffrage. Concurrently, this era saw the emergence of political parties, a political press, and a national political culture. Amidst widening schisms between liberal and Christian parliamentary factions, electoral associations (*Kiesvereenigingen*) formed, evolving into formal political parties (De Jong, 1999), such as the Anti-Revolutionary Party (1879), the Liberal Union (1885), and a Catholic amalgamation (1904) (De Jong, 2001; Voerman, 1989). Prior to this party formation, candidates typically garnered support from newspapers aligned with specific political agendas (De Jong, 1999). These developments provide a valuable setting to investigate how institutional changes, particularly those increasing the monitoring of politicians, might impact the magnitude of financial returns to political office (see e.g. Aidt and Franck, 2015, 2019; Becker and Hornung, 2020).

Methodologically, the dynamic regression discontinuity design leverages repeated quasi-random treatment assignment arising from close electoral outcomes, considering candidates with equal prior tenures. To validate this assignment, a comprehensive dataset is compiled, detailing candidate backgrounds, political leanings, demographics, and district characteristics for closely contested elections. However, interpretation is complicated by incumbency advantages (Lee, 2008). The overall estimated impact of an election encompasses both an immediate (*ceteris paribus*) effect and incumbency advantages that compound subsequent *ceteris paribus* effects. Following Cellini et al. (2010) and Hsu and Shen (2024), *ceteris paribus* effects are iteratively derived from the overall estimated effects and incumbency advantages for each political term. These derived estimates yield a marginal return curve, representing the successive *ceteris paribus* benefits of holding multiple terms in office.

The analysis reveals that significant financial returns to politics accrue primarily during the first two terms of office. Politicians narrowly securing a mandate for the first or second time exhibited substantially higher lifetime wealth—approximately 100,000 guilders, equivalent to eight times a cabinet minister's salary—compared to candidates narrowly losing an election. This translates to an additional five percentage points in annual wealth accumulation for winners of these closely contested second-term elections, an effect size comparable to findings in a contemporary setting by Fisman et al. (2014). The robustness of these findings is confirmed through the inclusion of covariates, various parameter specifications, and multiple placebo tests. Conversely, for third and subsequent terms, estimated returns lack statistical significance, with point estimates frequently near zero, suggesting negligible financial benefits. This overall pattern aligns with theories suggesting that rent-seeking opportunities may face diminishing marginal returns, or even be a depletable resource (Rosen, 1986; Tullock, 2008). These results challenge theories positing a constant marginal return to political tenure (Persson and Tabellini, 2002; Caselli and Morelli, 2004; Baltrunaite, 2020; Bourveau et al., 2021), instead underscoring the dynamic and non-uniform nature of benefits derived from political office, with returns concentrated within particular terms.

Subsequently, the analysis explores potential mechanisms underlying this result. Consistent with extensive literature, it scrutinizes the relationship between these financial returns and career trajectories (Dal Bó et al., 2009; i Vidal et al., 2012; Dal Bó et al., 2017; Wasserman, 2023), employing fine-grained data on politicians' and candidates' post-election career paths. The results show that other, less prestigious political functions, such as provincial-level representative, sometimes substitute for Lower House membership, partially explaining the observed financial returns.[2] The evidence also suggests that candidates accumulate earnings-relevant human capital in the Lower House that translates into altered subsequent career paths, for instance, in law. These changes in career paths temporally align with the financial returns to politics, supporting human capital-based explanations for the observed financial returns (Diermeier et al., 2005; Mattozzi and Merlo, 2008). Other evidence points against in-office explanations of the returns to politics: there does not seem to be any heterogeneity in the returns to politics related to the institutional context politicians were elected in.

The remainder of this study is structured as follows. In Section 2, I discuss the historical background by focusing on the development of the district system and political parties. In Section 3, I introduce the data sources used in this study. In Section 4, I describe the empirical strategy, and in Section 5, I show the main regression discontinuity results. In Section 6, I investigate various alternative mechanisms, and I conclude in Section 7.[3]

[2]Most of the other, lower-level political positions came with only a slightly lower or no salary, as detailed in Appendix A.2.

[3]Appendix A features a more extensive description of the historical background of the setting in this study. Appendix B focuses on several estimation issues. I provide various robustness checks and supplementary analyses in Appendix C. Appendix D is a data appendix and also contains instructions pertaining to the replication package, also available on the Harvard Dataverse and GitHub.

2 Historical Background

2.1 Electoral Institutions

During the period 1848-1917, all elections to the Lower House were conducted under a district system. Prior to 1848, the year constitutional reforms liberalized the nation's electoral system and political institutions, delegates to the Lower House were chosen indirectly: the enfranchised electorate selected representatives for the Provincial Estates, which, in turn, appointed members to the Lower House. A similar indirect method was used to elect delegates to the Upper House. After 1848, the Lower House electoral system underwent reforms that rendered them direct and, consequently, more democratic (Blok, 1987).[4] Starting in 1849, Lower House elections were held biannually, with half of the seats being contested every two years. Typically, districts comprised two seats and in each election, one seat was subject to contest (De Jong, 1999). Consequently, members of the Lower House served four-year terms. Candidacy was initially individual-based, as formal political parties were absent. As political differences became more salient during the 1860s and 1870s (De Jong, 2001), electoral associations (Dutch: *Kiesvereenigingen*) began to emerge, serving as precursors to political parties. These associations gradually evolved into explicit political parties with distinct ideologies, largely reflecting the cultural-religious landscape of the Netherlands; Protestant, Catholic, and Liberal parties subsequently became the country's dominant political actors.

The electoral law (*Kieswet*) stipulated the mapping of municipalities (the Netherlands' lowest-level administrative units) to electoral districts, with the stated objective that each district would correspond to approximately 45,000 inhabitants (De Jong, 1999). Accordingly, following the 1848 constitutional revision, the Lower House comprised 68 seats, aligning with this representational target. However, significant population growth made adherence to this rule increasingly challenging. Lawmakers responded by augmenting the number of seats and altering district compositions; for instance, the number of Lower House seats rose from 68 to 86 within approximately a decade. Achieving consensus on district configurations became progressively more difficult due to the high political stakes involved, including concerns about gerrymandering, which effectively postponed substantial reform until 1887, when the number of seats was fixed at 100. The 1887 constitutional revision also mandated that all Lower House members be elected simultaneously, while retaining the four-year term, and established a system of one representative per district. This latter change necessitated the division of previously large multi-member districts into smaller constituencies. As the population continued to expand, the reallocation of districts became more arduous, and representational imbalances between districts became increasingly salient. This disparity particularly favored sparsely populated districts over their densely populated counterparts. Even the electoral law reforms of 1896, which included, among other measures, the partitioning of the largest cities into multiple districts (thereby ostensibly increasing their representation), failed to rectify

[4] In contrast to the Lower House, the 1848 constitutional reforms preserved this indirect system for the Upper House.

the underlying imbalance that continued to disadvantage these urban areas (De Jong, 1999).

Although candidacy was, in principle, open to any male aged thirty or older, suffrage rights remained restricted. The 1848 Constitution delegated the determination of suffrage and eligibility requirements to the electoral law. This law stipulated that suffrage was granted to men who paid taxes that exceeded a certain threshold, known as the *census* (De Vries, 1971; De Haan, 2003). This *census* was determined at the municipal level; consequently, in wealthier municipalities such as Amsterdam, the requirement was higher. Census levels were generally calibrated so that approximately 1 in 3,000 inhabitants was enfranchised. Van Der Kolk et al. (2018) note that approximately 85,000 men, out of a total population exceeding 2.5 million, possessed active suffrage rights for both the Upper and Lower Houses. Constitutional and electoral law amendments in 1887 effectively entailed a lowering of *census* requirements, which served as the principal mechanism for enfranchising a larger proportion of the male population, reaching approximately 25% after 1887 (Van Der Kolk et al., 2018), although alternative pathways to suffrage also existed. Subsequent reforms in 1896 further expanded the criteria for enfranchisement, introducing qualifications such as possessing specific academic degrees, paying a certain level of rent, or holding a savings account. De Jong (1999) observes that by 1900, approximately 48.6% of all Dutch men aged 25 and older were enfranchised.

2.2 Party Landscape

Between 1848 and 1917, the Dutch electoral system centered on individual delegates rather than political parties; politicians were expected to maintain independence and promote national interests (De Jong, 2001). Preceding formal parties, electoral unions (*Kiesvereenigingen*) emerged, comprising enfranchised individuals with shared political orientations, aimed at coordinating voting behavior, enhancing information dissemination, and aggregating electoral preferences. National newspapers with distinct ideological stances played a significant role by endorsing candidates aligned with their views (De Jong, 1999). These ideological foundations underpinned the nascent party landscape. The Protestant politician Abraham Kuyper initiated party formation by establishing the Anti-Revolutionary Party (ARP) in 1879, modeled on British lines, with a program centered on religious autonomy, particularly in education, but also extending to other socio-economic and political institutions (Koch, 2020; De Jong, 2001). Parties rapidly proved effective coordination mechanisms, both among ideologically similar politicians and between politicians and electorates: the liberal counterpart to the ARP was founded in 1885, and the Catholic union of electoral associations followed in 1891. Consequently, an overwhelming majority of incumbent and aspiring politicians joined these organizations, as election without party support became exceedingly difficult, resulting in few unaffiliated politicians post-party formation. This strong, ideologically-rooted political landscape also contributed to the rarity of politicians switching parties (e.g. De Haan and Te Velde, 1996; De Jong, 1997). Appendix A.1 provides a more extensive historical background on party formation and their relationship with leading

national newspapers.

2.3 Formal Compensation for Politicians

Members of the Lower House received formal compensation for their political activities. The 1815 Constitution stipulated an annual expense retribution of 2,500 guilders, intended for living costs in The Hague, supplemented by travel reimbursements at 1.50 guilders per kilometer (Elzinga, 1985). Comparative wage data (Van Zanden, 1983; Van Riel, 2018) indicate this sum in 1850 was approximately nine times an average worker's annual wage, or five times that of a medium-sized town's mayor (Provinciale Verslagen, 1860). Following constitutional amendments in 1848 aimed at enhancing political legitimacy, this annual sum was reduced to 2,000 guilders, while travel reimbursements remained unchanged.

By 1890, rising general wages meant parliamentary compensation equated to roughly five times the average wage (Elzinga, 1985), comparable to an engineer's income or two to five times that of a medium-sized town's mayor (Polak, 1908). Given the findings presented later in this study regarding the transition of politicians into the legal profession, it is instructive to benchmark parliamentary compensation against the legal sector. Public sector wage structures reveal that the annual parliamentary allowance was roughly equivalent to the salary of a cantonal judge (*kantonrechter*), the lowest tier of the judiciary, who earned approximately 2,500 guilders. However, significant economic stratification existed above this level, likely mirroring the private legal market. For instance, a district judge in Amsterdam earned 4,000 guilders, while a colleague in a Second Class court (such as Alkmaar) earned only 2,500 guilders (Government of the Netherlands, 1906). This 60% premium for comparable roles in major cities was likely necessary to compete with the private sector; Amsterdam and Rotterdam were centers of trade where private lawyers handling high-value commercial disputes commanded fees significantly exceeding public salaries. Since judgeships were often career capstones offering prestige and security rather than maximum earnings (topping out around 8,000 guilders), the earning potential for successful private lawyers in these hubs was likely substantially higher than both judicial and parliamentary compensation.

Subsequent post-1917 adjustments increased annual compensation to 5,000 guilders. As average worker wages had only risen approximately 1.5-fold, this widened the disparity. Concurrently, members received free public transportation, mitigating accommodation needs in The Hague and reducing residential proximity-based disparities. Furthermore, former Lower House members became entitled to a pension after age 60: 100 guilders per year of service, capped at 2,000 guilders (Van Welderen Rengers and Romeijn, 1916).[5]

[5]Appendix A.2 details Lower House compensation, remuneration for other representative and executive bodies, the legal profession, and offers a comparative analysis of these wages relative to other politicians and average earnings.

3 Data and Sources

3.1 Electoral Data

The *Repositorium Tweede Kamerverkiezingen 1848-1917* (Repository Lower House Elections) provides comprehensive records of Dutch Lower House elections conducted under a district system between 1848 and 1917. This dataset systematically documents election details such as district demarcations, dates, types (regular, intermediate, or second round), candidate names, vote counts, eligible voter numbers, turnout, and metadata including contested seats. This study focuses on elections yielding a definitive victor, excluding first rounds requiring subsequent rounds or nullified elections due to unmet electoral thresholds. This refinement identifies 2,858 distinct elections. Based on this data, an electoral margin variable is constructed to identify close elections (cf. e.g. Lee, 2008; Fisman et al., 2014). Unlike most studies, this research involves a significant number of multi-candidate elections, necessitating a generalized definition for the margin, which serves as the running variable in the Regression Discontinuity (RD) strategy. The marginal winner (MW) in each election is the victorious candidate with the fewest votes among all winners, often the sole winner in single-seat contests. The marginal loser (ML) is defined analogously. With Winners_j denoting all victorious candidates in district j, vote margins at the candidate-district level (candidate i in district j) are computed as:

$$\text{Margin}_{ij} = \begin{cases} \frac{\text{Votes}_{ij} - \text{Votes}_{ML}}{\text{Votes}_j} & \text{if } i \in \text{Winners}_j \\ \frac{\text{Votes}_{MW} - \text{Votes}_{ij}}{\text{Votes}_j} & \text{if } i \notin \text{Winners}_j \end{cases}$$

This definition ensures symmetry and simplifies to the conventional margin in two-candidate elections.[6]

3.2 Candidate Data

From the *Politiek Documentatiecentrum* (PDC), an institution specializing in Dutch politics, I obtain a dataset including politicians' demographic variables (e.g., birth and death details) and comprehensive career path information (job descriptions, start/end dates). These data were used to match politicians to candidate-election pairs from the election records via a rule-based approach (Abramitzky et al., 2021) based on political activity periods and fuzzy string matching, with subsequent manual verification. In addition to candidate-election specifics, politician-specific newspaper endorsements were gathered from the *Repositorium*, as local newspapers frequently reported on electoral contestants and provided editorial endorsements (Oud, 1997; De Jong, 1999). For "non-politicians"—candidates never elected to the Lower House whose data are generally absent from the PDC corpus[7]—biographical information

[6] 1,287 out of 2,858 elections are elections between 2 candidates.

[7] With the exception of candidates who were never elected into the Lower House, but might have served in the Upper House or as a Minister or Provincial Executive, in which case their data is also collected by

(birth/death dates and locations) and career paths were compiled using online genealogical sources (e.g., *genealogieonline.nl*, *Geni.com*), the historical newspaper archive *Delpher*, local provincial archives, and *Wikipedia*.

3.3 Personal Wealth

I collected archival data on candidates' personal wealth at decease from provincial probate inventories, the *Memories van Successie* (MVS). These documents, compiled for inheritance tax purposes, detail individuals' assets and liabilities (Bos, 1990) and are generally considered highly reliable due to sworn declarations and meticulous tax agency procedures (Moes, 2012). The MVS are publicly accessible for the period 1877-1927. Similar probate or asset declaration data have been used in other studies (Eggers and Hainmueller, 2009; Fisman et al., 2014; Bottomley, 2019). The net wealth measure from these probate inventories is deflated to 1900 guilders using a CPI from Jordà et al. (2019). Appendix D provides examples and further details on this primary data source.

Data collection prioritized candidates involved in close elections. In total, probate inventories were collected for 4,065 out of 6,679 candidate-election pairs, representing 523 unique candidates from a total of 905. For the 2,618 candidate-election pairs in relatively close elections (absolute margin $\leq 20\%$), probate inventories were obtained for 1,652 (63%). The primary reason for data absence is the limited archival availability of the MVS.[8] Of the 905 unique candidates, 621 were elected at least once, with probate inventories found for 369 (59%). For the 284 never-elected candidates, 143 inventories were located, including 123 for those who lost at least one election by a margin of 20% or less. Among the 621 politicians elected at least once, 463 secured a second term, 342 a third, 278 a fourth, and 203 were elected more than four times.[9]

The net wealth measure from these probate inventories includes a small number of zero-valued observations. The net wealth is calculated as assets minus liabilities; therefore, a value of zero (or a negative value, which I code as zero for the transformation) is a true measure indicating that an individual's liabilities met or exceeded their assets at the time of death. These observations are different from missing values, which occur when a probate inventory for a candidate could not be located in the archives. In the sample for the first-term analysis, I observe 6 candidates with a recorded net wealth of zero.[10] The presence of these valid, non-negative values motivates the use of the transformation in Equation 1.

the PDC.

[8] Appendix Table C.29 confirms that there is no selection bias introduced by the unavailability of probate inventories, and Machielsen (2025) suggests that such missingness is likely random.

[9] Potential biases due to sampling mechanisms, such as wealth thresholds for probate inventory creation or differential tax evasion, are acknowledged. Analysis in Appendix B suggests these factors likely introduce a downward bias in the estimates.

[10] In the second-term analysis, I observe only 1 candidate with a net wealth of zero. In all subsequent analyses, all candidates have positive wealth.

3.4 District-level Control Variables

District-level control variables were obtained from the Historical Database of Dutch Municipalities (*HDNG*). Given slight temporal variations in district composition, a dynamic mapping aggregated municipality-level data to the district level, contingent on the election year. This process yielded variables measuring religious composition (percentage Catholic and Protestant), labor force composition (percentage in industry, services, agriculture), and per capita shares of various taxes for 1859 and 1889 as proxies for district economic activity. Additionally, district-level literacy data were derived from the Historical Sample of the Netherlands (*HSN*), aggregated from individual-municipality level information.

4 Method

4.1 Dynamic Regression Discontinuity

This study uses quasi-random variation induced by close elections to estimate the effect of political activity on end-of-life wealth. The analysis of these returns is complicated by the recursive nature of political office: individuals' multiple potential elections necessitate accounting for the dynamic nature of treatment assignment. Concretely, an estimate of an initial election's effect on wealth encompasses not only the *ceteris paribus* impact but also dynamic effects arising from altered re-election probabilities and the accrual of returns from prolonged Lower House tenure.[11]

I estimate the returns for different periods of political activity, denoted by $\tau \in \{1, \ldots, t^*\}$, by employing a regression discontinuity approach similar to Eggers and Hainmueller (2009), Fisman et al. (2014) and Fafchamps and Labonne (2017). The basic specification for a particular τ, is:

$$\log(1 + w_{ijp}) = \alpha_j + \gamma_p + \theta_\tau^{ITT} \cdot 1_{\text{Margin}_i > 0} + \eta \cdot f(\text{Margin}_i) + X_{ij}\beta + \epsilon_i \tag{1}$$

where w_{ijp} is the end-of-life net wealth for candidate i from party p competing in district j.[12] The parameters θ_τ^{ITT} are the coefficients of interest. Equation 1 is estimated on a subsample of candidates who have won exactly $\tau - 1$ elections. Hence, this strategy compares candidates who are closely elected for the τ'th time to their losing contenders, where both groups of candidates have won exactly $\tau - 1$ elections in the past. 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). I use a bandwidth of 0.10 in all estimations to ensure

[11] 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. In the analyses, I use only the first attempts. In robustness checks, I frequently condition the sample on candidates having tried a similar number of times, and use it as a control variable.

[12] In line with insights from Chen and Roth (2023), I perform a battery of robustness checks to assess the sensitivity of these estimates to scaling and different "log-like" transformations.

that differences in estimates are not driven by variations in bandwidth selection. In Appendix Tables C.12 and C.13, I test for the sensitivity of the reported results to the bandwidth by using various wider and smaller bandwidths[13] and by using an optimal per-period bandwidth estimator offered by Cattaneo et al. (2019), but the results do not appear to be sensitive to the bandwidth.[14]

Subsequently, to disentangle these dynamic effects, the analysis employs a modeling approach based on Cellini et al. (2010). The following model explicitly incorporates the possibility that the estimated return for an initial term of office may partially reflect anticipated future effects:[15]

$$w_i = \sum_{k=t}^{\infty} \theta_k c_{i,k} + u_i \tag{2}$$

where $c_{i,k}$ is an indicator whether candidate i has been elected for the k'th time. Whether a candidate wins the k'th election $c_{i,k}$ can in turn be a function of preceding wins. Differentiating Equation 2 with respect to the independent variable $c_{i,k}$ makes clear that the raw regression discontinuity estimates might contain feedback effects from effects arising from participating and winning in the future:

$$\begin{aligned} \theta_k^{ITT} \hat{=} \frac{dw_i}{dc_{i,k}} &= \frac{\partial w_i}{\partial c_{i,k}} + \sum_{t>k} \theta_t \cdot \frac{\partial c_{i,t}}{\partial c_{i,k}} \\ &= \theta_k^{ATT} + \sum_{t>k} \theta_t^{ATT} \cdot \pi_{(t-k)} \end{aligned} \tag{3}$$

.

The final line demonstrates that the parameters of interest—the *ceteris paribus* wealth effects of being elected for the k'th time (θ^{ATT})—are functions of the initially estimated ITT effects (θ^{ITT}) and subsequent *ceteris paribus* wealth effects. Furthermore, the partial derivative of being elected for the t'th time with respect to being elected for the k'th time represents an incumbency advantage, denoted as π_{t-k}. These π_{t-k} terms are henceforth referred to as $t-k$'th order incumbency advantages. Within this framework, the regression discontinuity estimates from Equation 1 are interpreted as "intent-to-treat" (ITT) effects, while the *ceteris paribus* estimates represent "average treatment effect on the treated" (ATT) effects. Once the ITT effects and incumbency advantages are estimated, Equation 3 facilitates the recursive computation of ATT effect estimates, contingent on a specific identification as-

[13] The results are essentially unchanged for bandwidths in the domain $[0.07, 0.35]$

[14] In most rounds, the optimal bandwidth is close to 0.10. This issue is explored in more detail in Section 5.4.

[15] This model is estimated using an RD strategy with close elections, ensuring that $\mathbf{E}[u_i c_{i,k}] = 0$, so that the parameters can be consistently estimated.

sumption.[16] For Equation 3 to contain a finite number of θ^{ATT} terms, I must impose one t^* for which the estimand $\theta^{ITT}_{t^*} = \theta^{ATT}_{t^*}$. I face a trade-off between plausibility by setting t^* as high as possible, and data availability and precision of the estimates by setting t^* to an earlier round. Since the variance of the estimates sharply increases after $t^* = 5$, I report the results using $t^* = 5$ in the main text. In various checks reported in Section 5.3, I confirm that this assumption has no bearing on the results. Standard errors for the ATT estimates are computed using the delta method.

5 Results

5.1 Covariate Balance

The validity of the regression discontinuity (RD) approach requires that, proximate to the electoral threshold, the assignment to politician status is quasi-random with respect to pre-treatment variables, which should therefore be balanced between treatment (politician) and control (non-politician) groups. Acknowledging concerns regarding the potential non-randomness of close elections (Caughey and Sekhon, 2011), this study employs a methodology similar to Lowes and Montero (2021), examining RD effects on pre-treatment characteristics at the cut-off and within varying margins to investigate patterns of convergence. Table 1 presents this analysis, with statistics conditional on party, district and decade fixed effects.

A key aspect of these candidate backgrounds involves their professional careers prior to the election. A significant portion of the candidates had a background in law (but not as a judge). The prevailing pattern in our data is that individuals completed their legal education and were established as lawyers before seeking national office. The covariate balance tests confirm that the propensity to have a pre-existing legal background is balanced at the electoral threshold. In addition, about 20% of the candidates are already active in politics at the provincial level, mostly as a Deputy in Provincial Estates. Deputies are responsible for their own policy area in a province, and formulate and implement policy in specific areas such as agriculture, infrastructure, the economy, or nature. They also perform tasks assigned by the national government.

Selection into politics is evidently non-random. Across a wide electoral margin, the elected (treated) group significantly differs from the non-elected (control) group across most measurable characteristics. Specifically, elected candidates are more frequently endorsed by newspapers, are more likely to have been active in local politics before, and represent

[16] Estimating the incumbency advantages π_n is achieved using the following specification for the n'th order incumbency advantage:

$$\mathcal{I}[c_{i,t+n} = 1] = \alpha_t + \pi_n \cdot 1_{\text{Margin}_{i,t}>0} + \eta \cdot f(\text{Margin}_{i,t}) + \epsilon_{it},$$

where the dependent variable is 1 if candidate i won an election $t+n$, 0 if a candidate loses. This Equation is again estimated using the methodology of Cattaneo et al. (2019) and uses the default parameter settings.

districts at times when they are slightly poorer. They are, however, identical regarding the demographic composition of their birthplaces. However, when restricting the analysis to a narrow margin, most differences between treated and control groups diminish. At a 5% margin, only slight imbalances persist in newspaper recommendations, birth year, and the likelihood of being active in law before the election. Crucially, at the discontinuity itself, and conditional on party fixed effects, virtually no statistically significant imbalances remain, with the exception of birth year and the distance of the birthplace to the Hague. A comparison between candidates with different birth years might lead to comparisons between candidates with different lifespans. In Appendix Table C.6, I analyze wealth per unit of lifespan, thereby controlling for eventual effects from comparing individuals born in different years. These results are identical to the results presented in the remainder of the text. Thus, the RD strategy compares treated individuals to control individuals who are statistically similar in terms of candidate backgrounds, district characteristics, and birthplaces.[17]

[Table 1 here]

Appendix C further investigates covariate balance for the second and third elections (Tables C.2 and C.3), revealing no evidence of a discontinuity in pre-treatment variables in these instances. Full descriptive statistics for all variables employed in this study are presented in Table C.4. Additionally, more extensive tests were conducted on subsamples for the first and second rounds, as detailed in Appendix Tables C.7 and C.8, to assess for discontinuities at the margin in pretreatment versions of several of the dependent variables utilized. These tests indicate no discontinuity in the past values of outcome variables used throughout this analysis.

5.2 Regression Discontinuity Results

Table 2 shows the results for some of the estimates of Equation 1. In the first column, I compare the end-of-life wealth of candidates, focusing on $\tau = 1$: candidates who, if elected, would be elected for the first time. In addition, I require that these be candidates who compete for the first time. In the second column, I focus on candidates who, if elected, would be elected for the second time, at their first attempt to be elected for the second time. In the subsequent columns, I focus on candidates who, if elected, would be elected for the τ'th time, at their first attempt after being elected $\tau - 1$ times. In all cases, standard errors are clustered at the candidate-level.

[Table 2 here]

The results reveal a distinct pattern: the estimated return of politics to end-of-life wealth is statistically significant for the first term. The coefficient estimate is positive, and implies

[17] Controlling for Electoral Threshold has no bearing on any of the results reported in this study. These balancing results hold also after condition only on party fixed effects, and party and decade fixed effects.

a difference in end-of-life wealth between winners and statistically similar losers of about 100,000 guilders. Similarly, for the second term (comparing candidates with exactly one prior electoral victory), point estimates are also positive and statistically significant at the 5% level. These estimates are similar in magnitude, again indicating an end-of-life wealth premium of approximately 100,000 guilders for a second term in the Lower House. Considering an average post-election lifespan of 22 years,[18] this equates to an annual wealth premium of roughly 5,000 guilders, or twice the formal yearly salary, a sum not explicable by official compensation alone. From an accumulation perspective, this implies an annual wealth premium of approximately 5 percentage points, a magnitude consistent with estimates by Fisman et al. (2014) in contemporary India and of a similar order of magnitude to findings by Eggers and Hainmueller (2009) for British MPs. However, a discontinuity is evident only when analyzing returns to the first and second terms of political activity; for all other periods, no apparent discontinuity in the conditional expectation function around the margin is observed. After $\tau = 5$, comparing candidates who win for the fifth time, and serve 20 years in the Lower House, with those who lose and served 16 years, the estimates become noisy and it is no longer possible to derive reliable estimates.

As detailed in Section 4, these estimates potentially incorporate future effects and endogenous participation, thus precluding their interpretation as the *ceteris paribus* effect of the τ'th period of political activity on end-of-life wealth. Therefore, the methodology from Section 4.1 is employed to derive *ceteris paribus* estimates for each additional term of political office.

5.3 Dynamic RD Results

Table 3 presents estimates of the *ceteris paribus* (ATT) effects derived from the ITT effects for each additional term of political office calculated using Equation 3. The Table employs the $t^* = 5$ assumption, implying there are no more returns to a Lower House career after 5 periods (20 years). The estimates align with the pattern observed in Table 2, suggesting a positive financial return to political office in the first two periods, but an absence of such return afterwards. The magnitude of these effects are consistent with previous findings: an end-of-life wealth premium of approximately 100,000 guilders. This sum is equivalent to roughly six times a Minister's salary, or an annual premium of approximately 5,000 guilders from initial entry into politics until death, which in turn is twice the typical yearly parliamentary salary during the period under investigation. These estimates of the ceteris paribus returns, after correcting for future office holding and associated rewards, are statistically significant at the 5% level. For the remaining periods, no discernible returns to office are evident: the point estimates are generally very close to zero and are statistically indistinguishable from zero, despite the roughly comparable sample size.

[Table 3 here]

[18] Appendix C, Table C.4

As indicated in Table 3, the ITT and ATT estimates do not differ substantially. This proximity is attributable to the relatively small incumbency advantages, which consequently limit the feedback effect from future terms. Although estimates for the first and second periods are theoretically more susceptible to bias from the compounding of future effects, Table 3 clearly shows that the *ceteris paribus* returns for a first and second term in office remain statistically significant at the 5% level. In contrast, estimates for all other periods of political activity are statistically indistinguishable from zero. Nevertheless, the ITT estimates, particularly for the first period, tend to be upwardly biased by the presence of larger returns in subsequent terms. For instance, the point estimate for the *ceteris paribus* return to a first period of political office decreases from 1.327 to 1.094, indicating that the ITT approach may significantly overestimate this initial return. This reiterates the core pattern observed after correcting the estimates in Table 2 for future returns. In Appendix Tables C.9, C.10 and C.11, I show that this pattern is not an artifact of a specific t^* choice; the general pattern remains consistent. This robustness is again due to the limited magnitude of incumbency advantages, which narrows the divergence between ITT and ATT estimates.

While this provides valid estimates for each term, a comparison of these estimates across terms requires an examination of how the pool of candidates evolves. The decision to run for re-election is non-random, and this selection may influence the observed pattern of returns. Hence, part of the effects could just be driven by sample composition. If this is true, the observed drop in the wealth premium after $\tau = 2$ does not occur because the value of the third term is low, but because the people competing for it are systematically different from those who competed for the first term. To gain insight into the extent to which this issue plays a role, I report the characteristics of the candidate pool (within a 5% margin) for each election in Table 4. In the final column, I report an F-test for equality of means across rounds.

[Table 4 here]

I find that candidates running for later terms are different in several ways from candidates participating in earlier terms. For example, candidates competing in a third or fourth term are indeed more likely to be liberal, have pre-existing careers in law and are more likely to have been involved in local politics, suggesting they are a more politically seasoned and professionally established group. This confirms that the samples across the columns of our main results tables are not directly comparable and represent different populations of political actors. However, these compositional differences do not undermine the core finding, but rather help to explain it. The fact that the financial premium disappears precisely when the candidate pool becomes more professionally established (e.g., more lawyers, more experienced politicians) is consistent with diminishing marginal returns. For a new candidate, a term in the Lower House provides a significant boost in human capital and access to new networks. For a seasoned politician who is already a successful lawyer or has extensive political connections, the marginal benefit of an additional four years is likely much smaller, as their

career and financial trajectory are already set. Therefore, the selection of more established candidates into later electoral rounds provides an explanation for why the marginal financial returns to political office diminish over time. In Section 6, I investigate this issue in more detail.

5.4 Robustness Checks

In Appendix C, I perform a battery of robustness checks, showing that these results remain robust over a large array of potential decisions that could influence the estimates.

Placebo test: In Appendix Figure C. 1, I graphically show the estimates of placebo tests involving artificially varying the cut-off point for being elected from $[-0.15, 0.15]$. I focus on the key estimates reported here, the *ceteris paribus* effects. These results show that the point estimates are highest and only significant for the first two estimates at the true cut-off ($c = 0$). Virtually all the results with a cut-off point slightly to the left or slightly to the right of the true cut-off show a point estimate close to zero and large confidence intervals.

Regression discontinuity parameters: The baseline estimates result from using the default parameter settings in the `rdrobust` package by Calonico et al. (2015) – a first-order polynomial to estimate the treatment effect, and a second-order polynomial to estimate the bias, which is used for a bias-corrected confidence interval. Furthermore, the local linear regression estimates are based on a triangular kernel. The only exception is a fixed bandwidth to render results comparable between rounds. To investigate whether the estimates are sensitive to the bandwidth used, in Appendix Tables C.12 and C.13, I use a wide range of bandwidths ($[0.07, 0.35]$) and the per-round MSE-optimal bandwidths as defined by Cattaneo et al. (2019).[19] Similarly, in Appendix Tables C.14 and C.15, I estimate the ITT effects using different kernels. Virtually all of the resulting estimates are very similar to the results reported in Table 3.[20] Finally, I also report estimates with standard errors clustered at the political party level (Appendix Table C.16). The results are again invariant to this decision.

Estimating the incumbency advantages: In the baseline analyses, the ATT estimates are usually fairly close to the ITT estimates. This is partially due to the estimated incumbency advantages being fairly small. In the baseline analysis, I use the same fixed effects to estimate incumbency advantages. However, incumbency advantages can also be estimated in a different way, for example, by conditioning on other control variables and fixed effects. In Appendix Table C.17, I report resulting estimates using unconditional incumbency advantages. The results are very similar to the results shown in Table 3. This rules out that an arbitrary estimation of incumbency advantages is responsible for the observed

[19] A bandwidth strategy that treats rounds as independent fails to exploit or mitigate the covariances between estimates, leading to suboptimal inference for the total effect. The slight sensitivity observed in Table C.10 is a consequence of applying a local, one-period bandwidth choice to a multi-period setting, which can result in significant bias.

[20] Notably, although according to some of the estimates reported here the ITT estimates are highly significant, the ATT effects are not. This illustrates why results exclusively focusing on ITT parameters such as the ones in Eggers and Hainmueller (2009) should be treated with caution.

pattern of results.

Alternative definitions and transformations of the DV: In the baseline analyses, I use deflated log wealth using a CPI from Jordà et al. (2019). To investigate the sensitivity of the estimates to this procedure, I use non-deflated log wealth as listed at the beginning of the probate inventory, and net wealth as listed at the end of the probate inventory. These can sometimes differ due to accounting inaccuracies. As another alternative, I use the inverse hyperbolic sine transformation of wealth instead of the log transformation. These robustness checks are performed in Appendix Tables C.18, C.19 and C.20, and all show estimates with the same pattern as in Table 3.

Furthermore, Chen and Roth (2023) argue that when the outcome variable is weakly positive, there is no treatment effect parameter that is an average of individual-level treatment effects, unit invariant, and point identified at the same time. Focusing on unit variance, they find that the effects found in various studies change radically depending on the units of measurement of the dependent variable. In Appendix Tables C.21 and C.22, I explore the effects of using different scales on the estimates. Although the implied effect sizes vary, they are still comparable to the originally reported effect size. Moreover, exactly the same pattern is found as in Table 3. Following the recommendations by Chen and Roth (2023), I also estimate the ITT effects using a Poisson QMLE procedure and a standard linear regression-based regression discontinuity estimate. The resulting unit-independent treatment effects (Appendix Table C.23) are also very close to the estimates reported in the main text and the pattern is identical.

Other control variables and fixed effects: The baseline estimates are estimates within-party, within-district, and within-decade. In Appendix Table C.24, I rely exclusively on within-district variation by estimating Equation 1 and the derived ATT effects using district and decade fixed effects, but omit party fixed effects. The disadvantage is a potential increase in bias. In practice, the pattern is exactly the same as in Table 3. The statistical significance is also unaffected. I also estimate the results with party and district effects, without decade fixed effects (Appendix Table C.25). The results are also invariant to this decision. Finally, I use a full set of control variables, consisting of all variables significant at the 5%-level in the balancing Table 1, in addition to district, decade and party fixed effects. The results (Appendix Table C.26) are also invariant to the inclusion of this complete set of controls.

Extreme values: To assess the sensitivity of the findings to extreme values, the analysis was repeated after winsorizing the top 5% of the observations. The resulting estimates, presented in Appendix Table C.27, are quantitatively very similar to the baseline results, indicating that the findings are not driven by outliers.

6 Mechanisms

6.1 Career Paths

To investigate the mechanisms driving the financial returns documented previously, this section examines how election to the Lower House influences subsequent career trajectories. The existing literature distinguishes between two primary channels for political rent accrual: gains realized while in-office (Fisman, 2001; Fisman et al., 2014; Baltrunaite, 2020) and advantages materializing post-tenure (Eggers and Hainmueller, 2009; Fafchamps and Labonne, 2017; Querubin et al., 2016; Folke et al., 2017; Geys, 2017). To test the latter, I estimate Equation 4 to analyze changes in career paths following the election:

$$y_{i,t^+} - y_{i,t^-} = \alpha_j + \gamma_p + \theta_\tau^{ITT} \cdot 1_{\text{Margin}_i>0} + \eta \cdot f(\text{Margin}_i) + X_{ij}\beta + \epsilon_i, \tag{4}$$

where y_{i,t^+} is an indicator for whether candidate i pursues a given career path at any point after the election at time t, and y_{i,t^-} indicates involvement in that same career prior to the election. The variation in this measure is thus coming from politicians who switch their career from not being involved in career path j before participating in an election to being involved in career path j after participating in that election, thereby controlling for pre-existing career choices, and isolating the change attributable to electoral success. The methodology described in Equation 3 is then employed to compute *ceteris paribus* estimates. The reported results use the same sample as the analyses in Section 5. Since the data on career paths are more widely available than the wealth data, in Appendix Table C.28, I show that the results are virtually identical using either sample.[21]

Table 5 presents the estimates for four key career outcomes.[22] Panel A reveals a striking pattern regarding lower-level political office: winning candidates are significantly less likely to subsequently enter provincial politics during their first two terms. This implies that the control group—narrow losers—disproportionately pursues provincial office. This could be interpreted as winners having superior "outside options" that make provincial roles unattractive, or simply that narrow losers remain deeply committed to a political career and accept provincial seats as a second-best outcome. To distinguish between these mechanisms, I conduct a heterogeneity analysis on the first-term election, splitting the sample between candidates who were already lawyers and those who were not. If the outside option hypothesis were the primary driver, the negative effect of winning on entering provincial politics should be significantly larger for non-lawyers (who gain the most relative human capital). The results (reported in Appendix Table C.31) show no evidence of this. This favors the commitment hypothesis: losers, regardless of their prior professional status, persist in public service by securing provincial mandates.

If winners are pivoting away from the provincial political track, where do they go? Panel

[21] In Appendix Table C.29, I also show that the wealth sample and the more widely available sample are statistically identical.

[22] These results are also robust to larger bandwidths, as reported in Appendix Table C.30.

B identifies a specific, lucrative exit path that drives the wealth results: Law. During the first term, winners are significantly more likely than losers to transition into the legal profession. This confirms that access to the Lower House enhances human capital, providing a bridge to high-earning private sector careers. While the "commitment" channel in Panel A explains why losers end up in lower-paying provincial roles, Panel B clarifies the source of the winners' financial premium. The combined evidence suggests the wealth gap is driven by a divergence in career tracks: losers remain in lower-level politics, while winners leverage their tenure to access the legal market. Consistent with this market-based explanation, I find that the wealth premium is driven by politicians elected in the major commercial hubs (Amsterdam, Rotterdam, The Hague), where the private returns to legal expertise were highest (see Appendix Table C.32).

Notably, while the effect on entering Law appears in later periods, the corresponding wealth premium does not. This discrepancy is likely due to timing. The average first-time candidate (aged 45) has ample time to capitalize on a legal career. In contrast, candidates serving a third or fourth term are significantly older; for these veteran politicians, there is insufficient time before retirement or death for a career switch to generate a statistically detectable impact on end-of-life wealth.

A potential concern regarding the interpretation of the wealth results is that the long-term careers of winners and losers might eventually converge. If narrow losers in early rounds frequently win subsequent elections, or if winners in later rounds exit politics quickly, the distinction between the "treatment" and "control" groups would fade over a lifetime. Panel C addresses this by estimating the effect of winning the current election on the candidate's total lifetime tenure in the Lower House.[23] The results show a large, positive, and statistically significant effect across all rounds. This confirms that winning a close election is a pivotal event that creates a permanent divergence in political careers; losers do not, on average, "catch up" by winning later. Consequently, the disappearance of the financial return after the second term cannot be attributed to a convergence in the political trajectories of winners and losers. The treatment group consistently serves longer in the Lower House, yet after the second term, this additional service yields no marginal financial benefit.

Panel D further characterizes these career trajectories by examining the duration of service in other (non-Lower House) political functions. The results indicate that the substitution between national and local political careers is complex and varies by seniority. For the first term—where the wealth premium is most pronounced—there is no statistically significant difference in the duration of other political service. This reinforces the finding that the wealth gap is driven by a shift into the private sector (Law), rather than by differential accumulation of salaries from other political posts. In contrast, by the fourth term, winners spend significantly fewer years in other political roles compared to losers (a reduction of roughly 4 years). This sharp "exit" effect for veteran politicians suggests that while late-stage winners remain in the Lower House (Panel C), they actively shed other political responsibilities,

[23] The dependent variable is log(1+days in Lower House).

whereas late-stage losers appear to retreat into, or remain stuck in, other political functions.

[Table 5 here]

In summary, the combined evidence from career paths supports a human-capital-based explanation for the financial returns to politics (Mattozzi and Merlo, 2008; Keane and Merlo, 2010; Geys, 2017). During the critical first two terms, winning candidates pivot away from lower-status provincial politics (Panel A) and toward high-earning legal professions (Panel B), creating a distinct wealth premium. Panel C confirms that these elections mark a permanent divergence in parliamentary careers, while Panel D suggests that for early-career politicians, this wealth accumulation is not driven by the duration of non-Lower House public service. In contrast, losing candidates are characterized by a "consolation prize" trajectory: they are effectively barred from the lucrative legal pivot and instead accumulate tenure in provincial or other political roles associated with lower lifetime earnings.

In Appendix Table C.34, I focus on several other plausible career paths, such as the Upper House, Ministerial positions, entrepreneurship, and city-level politics. Although there is some evidence that Lower House election alters candidates' career possibilities or trajectories, these findings show no systematic relationship between being elected and career paths that align temporally with the pattern of returns to politics observed in Table 3.

6.2 In-office Returns

An alternative explanation for the observed wealth premium is that politicians accrue returns while in office, a mechanism widely discussed in the literature (Fisman, 2001; Fisman et al., 2014). Such gains could stem from leveraging insider knowledge of forthcoming legislation to inform asset trades or from monetizing political influence sought by corporate interests (Tahoun, 2014; González et al., 2020). The in-office rent-seeking hypothesis implies that the magnitude of returns should be sensitive to the institutional context and a politician's position of power. To test this, a heterogeneity analysis was conducted, distinguishing between candidates whose party formed the incumbent government post-election and those from opposition parties. During the period studied, Dutch governance operated under a majority-rule system in which either Liberal or Confessional parties typically held an absolute parliamentary majority. Consequently, the power to initiate and amend legislation was predominantly a prerogative of incumbent party members (Van Den Berg and Vis, 2013). If in-office rents were the primary mechanism, the financial premium should therefore be concentrated among politicians from the governing party.

The results, presented in Table 6, show no such heterogeneity.[24] The financial returns for governing and opposition party members are statistically indistinguishable. This finding is

[24]Due to the sample size reduction from splitting the data, this analysis could only be reliably estimated using $t^* = 4$.

inconsistent with the in-office returns hypothesis, as opposition members, who lacked comparable opportunities to influence legislation for personal benefit, accrued the same wealth premium as their counterparts in government.[25]

[Table 6 here]

The in-office rent-seeking explanation is further weakened by a series of additional heterogeneity analyses. First, the effect is not driven by candidates who die shortly after leaving office, which would be expected if there were little time to accumulate gains post-tenure. This result also casts doubt on alternative explanations related to induced thriftiness or enhanced financial literacy, as the benefits of such behavioral changes would likely require more time to materialize. Second, the wealth premium does not vary significantly with key institutional or political factors, including the local turnout, or with district literacy rates. The lack of an effect related to turnout provides no support for theories suggesting that monitoring disciplines politicians' self-interested behavior (Acemoglu and Robinson, 2001; Duggan and Martinelli, 2017). There is also little evidence of heterogeneity between early periods (in which politicians' wages were relatively high compared to the average wage, which should decrease in-office rent-seeking) and later periods. A final test considered potential differences across political parties. Given that the Liberal Party was more loosely organized than its Christian counterparts, one might expect different opportunities for rent extraction (Van Den Berg and Vis, 2013). However, the analysis reveals no significant inter-party differences; the established pattern of returns holds consistently across subsamples for individual parties.[26]

In summary, the evidence provides little support for the hypothesis that the documented financial returns are generated through the in-office accumulation of wealth. The wealth premium is remarkably insensitive to a politician's capacity to influence legislation and to a range of contemporaneous institutional conditions that should theoretically mediate rent extraction, such as electoral monitoring or party discipline (Duggan and Martinelli, 2017). Nevertheless, these findings must be interpreted with caution, as the subsample analyses necessarily rely on small sample sizes, and anecdotal evidence suggesting that rent-seeking and illicit gains were nonetheless features of political life.[27]

7 Conclusion

This study investigated the financial returns to politics from a dynamic perspective. Unlike previous studies (Eggers and Hainmueller, 2009; Fisman et al., 2014), this paper's method

[25]The subsamples on which these were conducted are also roughly statistically comparable (Appendix Table C.33).

[26]These results are reported, in order, in Appendix Tables C.35, C.36, C.37, C.38, and C.39.

[27]Historical records point to specific instances of corruption, such as the 1909 scandal involving royal decorations for party funding (De Bruijn, 2005), insider trading allegations regarding vegetable rationing (Kroeze, 2013), and conflicts of interest where MPs held stakes in firms violating child labor laws (Van Den Berg and Vis, 2013). See Appendix A.3 for a more detailed background on these and other anecdotes.

explicitly estimates marginal returns for each additional period of political activity. A robust causal effect of political activity on end-of-life wealth is found, corroborating other research (Eggers and Hainmueller, 2009; Fisman et al., 2014). Crucially, this effect manifests itself only during the first two terms of office, suggesting that there is little financial gain to a prolonged political career as opposed to a shorter one. This finding is robust to variations in regression discontinuity parameters, incumbency advantage estimation, variable definitions, control variables, outlier exclusion and placebo tests.

The estimated returns are both statistically significant and plausible in magnitude, totaling approximately six times a Minister's annual salary. This implies a wealth premium equivalent to roughly twice the formal salary of a Lower House member (Elzinga, 1985), a figure that cannot be attributed to official remuneration alone. Instead, evidence from subsequent career trajectories suggests that newly elected politicians substantially enhanced their marketable human capital, gaining access to new and influential positions, particularly within the legal profession (cf. Mattozzi and Merlo, 2008). Membership in the Lower House expanded politicians' outside options, enabling them to secure higher lifetime earnings. The evidence does not support in-office rent-seeking as the primary source of these financial returns. This alternative explanation was investigated through a series of heterogeneity analyses. Under a rent-seeking hypothesis, the wealth premium should be concentrated among politicians of the incumbent governing party, as they would have been best positioned to leverage the legislative process for personal gain (cf. Van Den Berg and Vis, 2013). However, the analysis reveals no significant heterogeneity in returns based on governing party status or variations in electoral institutions, which substantially weakens the in-office rent-accrual hypothesis.

For terms of office beyond the second, the end-of-life wealth of long-serving politicians is statistically indistinguishable from that of their narrowly defeated opponents, indicating an absence of a further wealth premium. This suggests that for these later contests, the control group—candidates who had already served multiple terms before being narrowly defeated—possessed comparable levels of political, business, or legal capital, thus obscuring any marginal premium from an additional term. This dynamic approach allows for a refinement of previous findings: the estimates from Eggers and Hainmueller (2009) parallel this study's first-term Intent-to-Treat estimates, whereas the results from Fisman et al. (2014) likely conflate the returns from initial and later terms of office.

These findings open several avenues for future research. Although this study finds no direct financial returns beyond the second term, it would be premature to conclude that prolonged political office offers no financial benefits. Future research could investigate the social and professional networks of both winning and losing candidates, as well as the interplay between different political roles, to develop a more holistic understanding of the political class (cf. Dal Bó et al., 2009; Fafchamps and Labonne, 2017; Dal Bó and Finan, 2018). Furthermore, the front-loaded pattern of returns documented here challenges models that assume constant marginal returns from activities like insider trading (Bourveau et al., 2021) or embezzlement (Baltrunaite, 2020), which necessitates further inquiry into the diverse

determinants of financial gains from a political career.

From a historical perspective, these results are consistent with characterizations of nineteenth-century European politics as dominated by a wealthy, oligarchical elite (Van Den Berg and Vis, 2013; De Rooy, 2014). While the findings support the concept of "career politicians" who leverage public office for private advancement (Mattozzi and Merlo, 2008), they provide limited evidence for theories emphasizing the power of institutional reforms, such as suffrage extensions, to discipline the financial behavior of political actors (Aidt and Franck, 2019; Lacroix, 2023; Marcucci et al., 2023). Future work could seek more direct historical evidence of politicians strategically pursuing financial self-interest, thereby providing a historical counterpart to the rich evidence from contemporary settings (Tahoun and Van Lent, 2019; Baltrunaite, 2020; Bourveau et al., 2021).

References

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

Acemoglu, Daron and James A. Robinson (2001) "A theory of political transitions," *American Economic Review*, 91 (4), 938–963.

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

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

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

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

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

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

Angrist, Joshua D. and Jörn-steffen Pischke (2008) *Mostly harmless econometrics: An empiricist's companion*: Princeton university press.

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

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

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

Ministerie van Binnenlandse Zaken, Min (1893) *Raming van Kosten voor het Departement van Binnenlandse Zaken*: Handelingen der Staten-Generaal: Bijlagen 1892-1893.

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

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

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

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

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

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

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

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

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

Chen, Jiafeng and Jonathan Roth (2023) "Logs with Zeros? Some Problems and Solutions*," *The Quarterly Journal of Economics*, qjad054, 10.1093/qje/qjad054.

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

Dal Bó, Ernesto and Frederico Finan (2018) "Progress and perspectives in the study of political selection," *Annual Review of Economics*, 10, 541–575.

Dal Bó, Ernesto, Frederico Finan, Olle Folke, Torsten Persson, and Johanna Rickne (2017) "Who becomes a politician?" *The Quarterly Journal of Economics*, 132 (4), 1877–1914.

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

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

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

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

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

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

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

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

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

Deursen, A.Th. Van (1994) *Overheid en Publiek Belang: Jhr. Mr. A.F. de Savornin Lohman, Christelijk-Historische Unie en reformatorische politiek*: Onbekend.

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

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

Domela Nieuwenhuis, Ferdinand (1910) "Van Christen tot Anarchist."

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

Dynan, Karen E., Jonathan Skinner, and Stephen P. Zeldes (2002) "The importance of bequests and life-cycle saving in capital accumulation: A new answer," *American Economic Review*, 92 (2), 274–278.

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

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

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

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

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

Fisman, Raymond (2001) "Estimating the value of political connections," *American Economic Review*, 91 (4), 1095–1102.

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

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

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

Geys, Benny (2017) "Political dynasties, electoral institutions and politicians' human capital," *The Economic Journal*, 127 (605), F474–F494.

González, Felipe, Mounu Prem, Francisco Urzúa et al. (2020) "The privatization origins of political corporations: Evidence from the Pinochet regime," *The Journal of Economic History*, 80 (2), 417–456.

Government of the Netherlands (1906) *Wettelijke maatregelen tot regeling der administratieve rechtspraak: Wetsontwerpen I–III en Toelichtende Memoriën, Advies van den Raad van State benevens de daarbij behoorende afzonderlijke adviezen, Rapport aan H. M. de Koningin*, 's-Gravenhage: Boekhandel vh. Gebr. Belinfante, https://resolver.kb.nl/resolve?urn=MMSFUBA02:000014520:pdf, Digital version via Koninklijke Bibliotheek.

Hsu, Yu-Chin and Shu Shen (2024) "Dynamic regression discontinuity under treatment effect heterogeneity," *Quantitative Economics*, 15 (4), 1035–1064.

Jordà, Òscar, Katharina Knoll, Dmitry Kuvshinov, Moritz Schularick, and Alan M Taylor (2019) "The rate of return on everything, 1870–2015," *The Quarterly Journal of Economics*, 134 (3), 1225–1298.

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

Keane, Michael P and Antonio Merlo (2010) "Money, political ambition, and the career decisions of politicians," *American Economic Journal: Microeconomics*, 2 (3), 186–215.

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

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

Lacroix, Jean (2023) "Ballots instead of bullets? The effect of the voting rights act on political violence," *Journal of the European Economic Association*, 21 (2), 764–813.

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

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

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

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

——— (2025) "Democratization, personal wealth of politicians and voting behavior," *Explorations in Economic History*, 101733.

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

Marcucci, Andrea, Dominic Rohner, and Alessandro Saia (2023) "Ballot or Bullet: The Impact of the UK's Representation of the People Act on Peace and Prosperity," *The Economic Journal*, 133 (652), 1510–1536.

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

van der Meer, AJ (1984) *" Baron of werkman": BH Heldt, de eerste werkman in de Tweede Kamer 17 november 1885* Ph.D. dissertation.

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

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

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

Netscher, F (1890) "Uit ons parlement," *Portretten en schetsen uit de Eerste–en Tweede Kamer*.

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

Ministerie van Oorlog, Oorlog (1890) *Raming van Kosten voor het Departement van Justitie*: Handelingen der Staten-Generaal: Bijlagen 1889-1890.

——— (1906) *Raming van Kosten voor het Departement van Oorlog*: Handelingen der Staten-Generaal: Bijlagen 1905-1906.

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

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

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

Polak, A. (1908) *Bijna 200 Miljoen: Overzicht der Staatsuitgaven en Inkomsten*: J.B. Wolters Groningen.

Provinciale Verslagen, Drenthe (1920) *Provinciale Verslagen Drenthe*: Gedeputeerde Staten Drenthe.

Provinciale Verslagen, Groningen (1910) *Provinciale Verslagen Groningen*: Gedeputeerde Staten Groningen.

——— (1920) *Provinciale Verslagen Groningen*: Gedeputeerde Staten Groningen.

Provinciale Verslagen, Noord-Holland (1860) *Provinciale Verslagen Noord-Holland*: Gedeputeerde Staten Noord-Holland.

——— (1910) *Provinciale Verslagen Noord-Holland*: Gedeputeerde Staten Noord-Holland.

——— (1988) *Provinciale Verslagen Noord-Holland*: Gedeputeerde Staten Noord-Holland.

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

Querubin, Pablo et al. (2016) "Family and politics: Dynastic persistence in the Philippines," *Quarterly Journal of Political Science*, 11 (2), 151–181.

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

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

Rosen, Shwerwin (1986) "Prizes and Incentives in Elimination Tournaments.," *American Economic Review*, 76 (4).

Staatscommissie voor de salarisregeling van burgerlijke rijksambtenaren en beambten (1918) *Verslag van de Staatscommissie voor de salarisregeling van burgerlijke rijksambtenaren en beambten: Eerste Gedeelte*, 's-Gravenhage: Drukkerij Mouton & Co. https://resolver.kb.nl/resolve?urn=MMKB24:075736000:pdf.

Staten-Generaal (1893) *Uitgewerkte en toelichtende staat behoorende bij de raming van kosten voor de Hooge Colleges van Staat en het Kabinet der Koningin voor het dienstjaar 1893*: Handelingen der Staten-Generaal: Bijlagen 1892-1893.

Stutje, Jan Willem (2012) *Ferdinand Domela Nieuwenhuis: een romantische revolutionair*: Houtekiet/Amsab-Atlas Contact.

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

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

Tahoun, Ahmed (2014) "The role of stock ownership by US members of Congress on the market for political favors," *Journal of Financial Economics*, 111 (1), 86–110.

Tahoun, Ahmed and Laurence Van Lent (2019) "The personal wealth interests of politicians and government intervention in the economy," *Review of Finance*, 23 (1), 37–74.

Tullock, Gordon (2008) "The welfare costs of tariffs, monopolies, and theft," in *40 Years of Research on Rent Seeking 1*, 45–53: Springer.

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

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

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

Van Welderen Rengers, Wilco Julius and H. J. Romeijn (1916) *Schets eener parlementaire geschiedenis van Nederland*: M. Nijhoff.

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

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

i Vidal, Jordi Blanes, Mirko Draca, and Christian Fons-Rosen (2012) "Revolving door lobbyists," *The American Economic Review*, 102 (7), 3731.

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

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

Wasserman, Melanie (2023) "Gender differences in politician persistence," *The Review of Economics and Statistics*, 105 (2), 275–291.

Tables and Figures Main Text

Table 1: Covariate Balance - First Round

	Margin Within 0.2			Margin Within 0.05			
Variable	Mean (Treated)	Mean (Control)	p-val.	Mean (Treated)	Mean (Control)	p-val.	RD Estimate
Personal Characteristics							
Rec.: Liberal	0.21	0.21	0.941	0.17	0.25	0.593	0.007 (0.054)
Rec. Socialist	0.05	0.02	0.089*	0.06	0.04	0.278	0.031 (0.030)
Rec.: Protestant	0.13	0.19	0.061*	0.07	0.24	0.044**	-0.095 (0.053)
Rec. Catholic	0.06	0.07	0.711	0.09	0.08	0.566	0.003 (0.039)
Year of Birth Candidate	1831.78	1833.98	0.135	1825.20	1835.52	0.001***	-11.843 (1.964)***
Provincial Politics Before	0.25	0.13	0.011**	0.21	0.14	0.339	0.124 (0.071)
Local Politics Before	0.15	0.05	0.015**	0.13	0.11	0.302	0.109 (0.062)
Dummy Law Before	0.13	0.22	0.103	0.02	0.28	0.028**	-0.240 (0.071)
Dummy Business Before	0.01	0.02	0.708	0.03	0.01	0.738	-0.002 (0.022)
Electoral Characteristics							
Election Year	1882.13	1881.80	0.613	1881.10	1881.80	0.516	-1.099 (0.902)
Turnout	0.74	0.73	0.432	0.73	0.77	0.973	-0.004 (0.016)
Log (Electoral Threshold)	7.24	7.27	0.577	7.28	7.40	0.912	-0.147 (0.059)
Log(Electorate Size)	8.27	8.31	0.212	8.32	8.39	0.945	-0.136 (0.052)
District Characteristics							
District Population	11.53	11.57	0.155	11.59	11.65	0.803	-0.099 (0.038)
% Labor Force Industry District	0.40	0.40	0.236	0.39	0.39	0.550	0.000 (0.002)
% Labor Force Agriculture District	0.14	0.14	0.591	0.14	0.14	0.611	0.002 (0.004)
% Labor Force Services District	0.46	0.46	0.935	0.46	0.47	0.471	-0.002 (0.004)
% Paying Wealth Tax District	7.01	7.11	0.093*	6.99	7.11	0.347	-0.169 (0.091)
Income Tax Share District	8.04	8.12	0.079*	8.03	8.13	0.361	-0.124 (0.071)
% Catholic District	0.31	0.31	0.738	0.31	0.32	0.202	0.009 (0.006)
% Protestant District	0.63	0.63	0.743	0.63	0.63	0.344	-0.012 (0.007)
Birthplace Characteristics							
% Labor Force Industry Birth Place	0.36	0.35	0.323	0.34	0.33	0.899	-0.011 (0.021)
% Labor Force Agriculture Birth Place	0.07	0.07	0.880	0.07	0.07	0.965	0.020 (0.023)
% Labor Force Services Birth Place	0.57	0.59	0.504	0.59	0.60	0.976	-0.009 (0.041)
% Catholic Birth Place	0.33	0.32	0.733	0.41	0.35	0.268	-0.001 (0.041)
% Protestant Birth Place	0.64	0.65	0.566	0.56	0.63	0.238	-0.016 (0.039)
Distance to The Hague - BP	73.38	75.20	0.767	68.93	78.44	0.558	4.690 (8.141)*

Table contains means for various sets of variables conditioned on the absolute margin being lower than 0.2 (left panel) and lower than 0.05 (right panel). The sample is candidates who have never been elected so far. 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). Standard errors clustered at the district-level are shown between brackets. Significance is indicated by *: $p \leq 0.1$, **: $p \leq 0.05$, ***: $p \leq 0.01$.

Table 2: Estimates of the Total Financial Returns to Politics

Round τ:	1	2	3	4	5
Coefficient (ITT)	1.327**	1.438**	-0.024	-0.627	-0.777
SE (BC)	(0.392)	(0.565)	(0.441)	(0.330)	(0.428)
Mean DV Treated	11.863	11.766	11.860	12.115	12.579
Mean DV Control	11.349	10.805	11.842	12.261	11.056
Effective N (Treated)	68	59	31	29	23
Effective N (Control)	117	38	33	26	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes	Yes

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is log(1 + net wealth). The estimates represent the raw RD estimates at the τ-th election. Estimates are from a local linear RD with a fixed bandwidth of 0.10, conditional on party, decade, and district fixed effects. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 3: Estimates of the Returns to Politics

Round τ	1	2	3	4	5
Coefficient (ATT)	1.094***	1.344**	-0.004	-0.550	-0.777
SE (ATT)	(0.399)	(0.568)	(0.442)	(0.333)	(0.428)
Coefficient (ITT)	1.327**	1.438**	-0.024	-0.627	-0.777
SE (ITT)	(0.392)	(0.565)	(0.441)	(0.330)	(0.428)
Mean DV Treated	11.863	11.766	11.860	12.115	12.579
Mean DV Control	11.349	10.805	11.842	12.261	11.056
Effective N (Treated)	68	59	31	29	23
Effective N (Control)	117	38	33	26	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes	Yes

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is log(1 + net wealth). 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=5). Estimates are from a local linear RD with a fixed bandwidth of 0.10, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 4: Candidate Characteristics by Election Round

Variable/Round	1	2	3	4	5	p-val.
Panel A: Personal Characteristics						
Party: Liberal	0.320	0.482	0.581	0.610	0.661	0.000***
	(0.467)	(0.501)	(0.496)	(0.491)	(0.477)	
Party: Protestant	0.194	0.348	0.371	0.341	0.271	0.000***
	(0.396)	(0.478)	(0.486)	(0.477)	(0.448)	
Party: Catholic	0.082	0.098	0.029	0.049	0.068	0.222
	(0.275)	(0.298)	(0.167)	(0.217)	(0.254)	
Party: Socialist	0.046	0.061	0.019	0.000	0.000	0.041**
	(0.211)	(0.240)	(0.137)	(0.000)	(0.000)	
Year of Birth Candidate	1835.508	1834.226	1830.752	1830.720	1827.864	0.030**
	(22.878)	(19.626)	(18.676)	(16.722)	(16.598)	
Dummy Upper House Before	0.008	0.024	0.000	0.024	0.068	0.008***
	(0.090)	(0.155)	(0.000)	(0.155)	(0.254)	
Provincial Politics Before	0.216	0.335	0.390	0.476	0.390	0.000***
	(0.412)	(0.474)	(0.490)	(0.502)	(0.492)	
Local Politics Before	0.115	0.159	0.219	0.195	0.169	0.059*
	(0.319)	(0.366)	(0.416)	(0.399)	(0.378)	
Dummy Law Before	0.268	0.439	0.457	0.561	0.559	0.000***
	(0.443)	(0.498)	(0.501)	(0.499)	(0.501)	
Dummy Business Before	0.016	0.030	0.029	0.024	0.017	0.845
	(0.127)	(0.172)	(0.167)	(0.155)	(0.130)	
Panel B: Electoral Characteristics						
Election Year	1882.833	1882.427	1880.305	1880.939	1880.695	0.645
	(18.887)	(18.107)	(15.666)	(16.697)	(13.465)	
Log(Turnout)	7.744	7.817	7.787	7.841	7.827	0.667
	(0.704)	(0.456)	(0.543)	(0.574)	(0.576)	
Log (Electoral Threshold)	7.279	7.328	7.205	7.284	7.197	0.581
	(0.763)	(0.593)	(0.608)	(0.629)	(0.620)	
Log(Electorate Size)	8.293	8.352	8.282	8.290	8.310	0.892
	(0.747)	(0.557)	(0.576)	(0.623)	(0.658)	
Panel C: District Characteristics						
District Population	11.588	11.650	11.685	11.698	11.799	0.268
	(0.772)	(0.752)	(0.737)	(0.763)	(0.722)	
% Labor Force Industry District	0.402	0.402	0.387	0.401	0.388	0.537
	(0.097)	(0.094)	(0.090)	(0.098)	(0.088)	
% Labor Force Agriculture District	0.146	0.149	0.137	0.134	0.133	0.810
	(0.129)	(0.127)	(0.126)	(0.117)	(0.121)	
% Labor Force Services District	0.452	0.449	0.477	0.465	0.479	0.688
	(0.205)	(0.202)	(0.206)	(0.199)	(0.197)	
% Paying Wealth Tax District	7.031	7.127	7.055	7.192	7.045	0.933
	(1.658)	(1.646)	(1.749)	(1.774)	(1.751)	
Income Tax Share District	8.075	8.186	8.095	8.211	8.179	0.741
	(1.076)	(1.110)	(1.095)	(1.179)	(1.189)	
% Catholic District	0.323	0.312	0.297	0.287	0.316	0.546
	(0.213)	(0.193)	(0.179)	(0.185)	(0.211)	
% Protestant District	0.617	0.628	0.655	0.664	0.643	0.147
	(0.193)	(0.175)	(0.168)	(0.168)	(0.196)	
Panel D: Birthplace Characteristics						
% Labor Force Industry Birth Place	0.360	0.357	0.354	0.368	0.350	0.835
	(0.085)	(0.088)	(0.091)	(0.098)	(0.070)	
% Labor Force Agriculture Birth Place	0.074	0.064	0.062	0.062	0.086	0.664
	(0.098)	(0.090)	(0.107)	(0.100)	(0.129)	
% Labor Force Services Birth Place	0.566	0.579	0.584	0.570	0.564	0.928
	(0.162)	(0.155)	(0.177)	(0.169)	(0.190)	
% Catholic Birth Place	0.381	0.387	0.353	0.365	0.390	0.844
	(0.256)	(0.252)	(0.227)	(0.235)	(0.233)	
% Protestant Birth Place	0.598	0.594	0.628	0.608	0.589	0.844
	(0.245)	(0.241)	(0.219)	(0.221)	(0.225)	
Distance to The Hague - BP	80.153	75.087	70.454	70.910	78.611	0.540
	(55.030)	(52.853)	(52.926)	(55.287)	(48.121)	

The Table contains means and standard deviations (between brackets) for various candidate characteristics, separated by election round. The last column shows the p-value of an F-test for a statistical difference in means. Significance levels: *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table 5: Estimates of the Dynamics of Career Paths

Round τ	1	2	3	4	5
Panel A: Entry Provincial Politics					
Coefficient (ATT)	-0.713***	-0.486***	0.699***	-1.357***	-1.036***
SE (ATT)	(0.124)	(0.151)	(0.165)	(0.137)	(0.194)
Coefficient (ITT)	-0.490***	-0.170	0.702***	-1.416***	-1.036***
SE (ITT)	(0.120)	(0.147)	(0.164)	(0.136)	(0.194)
Mean DV Treated	-0.258	-0.152	0.000	-0.545	-0.583
Mean DV Control	0.034	-0.143	-0.500	0.000	0.333
Effective N (Treated)	68	59	31	29	23
Effective N (Control)	117	38	33	26	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
Panel B: Entry Law					
Coefficient (ATT)	0.262**	-0.241	0.566***	1.150***	-1.110*
SE (ATT)	(0.130)	(0.161)	(0.128)	(0.223)	(0.500)
Coefficient (ITT)	0.104	-0.113	0.719***	1.083***	-1.110*
SE (ITT)	(0.118)	(0.138)	(0.121)	(0.221)	(0.500)
Mean DV Treated	0.452	0.485	0.471	0.545	0.417
Mean DV Control	0.305	0.333	0.545	0.444	0.333
Effective N (Treated)	68	59	31	29	23
Effective N (Control)	117	38	33	26	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
Panel C: Duration Lower House					
Coefficient (ATT)	2.743***	0.942***	1.230***	0.738***	0.846***
SE (ATT)	(0.561)	(0.278)	(0.221)	(0.156)	(0.212)
Coefficient (ITT)	2.745***	1.001***	1.312***	0.843***	0.846***
SE (ITT)	(0.560)	(0.276)	(0.220)	(0.153)	(0.212)
Mean DV Treated	7.285	7.799	8.398	8.426	8.498
Mean DV Control	4.522	7.379	7.565	8.351	8.613
Effective N (Treated)	68	59	31	29	23
Effective N (Control)	117	38	33	26	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
Panel D: Duration Any Other Politics					
Coefficient (ATT)	-1.333	2.626**	3.078**	-4.293***	-0.802
SE (ATT)	(1.054)	(1.118)	(1.551)	(1.415)	(1.271)
Coefficient (ITT)	-0.651	3.267	2.895	-4.339*	-0.802
SE (ITT)	(1.014)	(1.089)	(1.546)	(1.413)	(1.271)
Mean DV Treated	3.600	3.917	4.129	3.608	1.884
Mean DV Control	3.294	2.976	2.348	4.530	8.459
Effective N (Treated)	68	59	31	29	23
Effective N (Control)	117	38	33	26	12
Bandwidth	0.100	0.100	0.100	0.100	0.100

Dynamic Regression Discontinuity (RD) estimates of winning an election for the τ-th term on four career path outcomes. Dependent Variables: entry into Provincial Politics (Panel A), entry into Law (Panel B), total lifetime years in the Lower House (Panel C), and total lifetime years in Other Politics (Panel D). 'ITT' is the raw RD estimate; 'ATT' is the ceteris paribus effect of the τ-th term, correcting for future incumbency effects. Estimates are from a local linear RD (bandwidth = 0.10) with party and district fixed effects. 'Effective N' is the sample size within the bandwidth. Standard errors for ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 6: Dynamic Results: Heterogeneity according to Party Incumbency

	Elected when Party Opposition				Elected when Party Incumbent			
Round τ:	1	2	3	4	1	2	3	4
Panel A: Personal Wealth								
Coefficient (ATT)	-0.677	1.867	-0.606	-0.718	-0.138	1.643**	1.141	-1.657
SE (ATT)	(0.965)	(2.845)	(0.723)	(0.946)	(1.220)	(0.631)	(1.070)	(1.442)
Effective N (Treated)	32	34	19	18	36	25	12	11
Effective N (Control)	96	15	12	13	21	23	21	13
Panel B: All Other Politics								
Coefficient (ATT)	0.000	-0.014	0.571	-0.090	-0.333	0.020	0.745*	-1.181
SE (ATT)	(0.313)	(0.590)	(0.468)	(0.655)	(0.349)	(0.389)	(0.428)	(0.481)
Effective N (Treated)	32	34	19	18	36	25	12	11
Effective N (Control)	96	15	12	13	21	23	21	13
Panel C: National Politics								
Coefficient (ATT)	0.059	-0.064	0.111	0.114	-0.516*	0.198	0.584	-0.748
SE (ATT)	(0.167)	(0.286)	(0.309)	(0.390)	(0.267)	(0.371)	(0.406)	(0.700)
Effective N (Treated)	32	34	19	18	36	25	12	11
Effective N (Control)	96	15	12	13	21	23	21	13
Panel D: Provincial Politics								
Coefficient (ATT)	-0.276	-0.284	0.841**	-0.402	0.111	-0.587**	1.203**	0.735
SE (ATT)	(0.314)	(0.651)	(0.361)	(0.329)	(0.252)	(0.278)	(0.464)	(0.667)
Effective N (Treated)	32	34	19	18	36	25	12	11
Effective N (Control)	96	15	12	13	21	23	21	13
Panel E: Municipal Politics								
Coefficient (ATT)	-0.029	-0.308	0.461*	0.279	-0.059	0.284	-0.199	-0.384
SE (ATT)	(0.227)	(0.537)	(0.249)	(0.426)	(0.292)	(0.190)	(0.362)	(0.670)
Effective N (Treated)	32	34	19	18	36	25	12	11
Effective N (Control)	96	15	12	13	21	23	21	13
Panel F: Business, Law, Entrepreneurship								
Coefficient (ATT)	0.301	-0.096	0.328	0.718***	0.008	0.062	0.132	0.393
SE (ATT)	(0.249)	(0.371)	(0.262)	(0.322)	(0.279)	(0.261)	(0.365)	(0.731)
Effective N (Treated)	32	34	19	18	36	25	12	11
Effective N (Control)	96	15	12	13	21	23	21	13

Dynamic Regression Discontinuity (RD) estimates of winning an election for the τ-th term on several career path outcomes. Dependent Variables: Personal Wealth (Panel A), Entry in All Other Politics except the Lower House (Panel B), Entry in National Politics (Panel C), Entry in Provincial Politics (Panel D), Entry in Municipal Politics (Panel E), and Entry in Business, Entrepreneurship & Law (Panel F). 'ITT' is the raw RD estimate; 'ATT' is the ceteris paribus effect of the τ-th term, correcting for future incumbency effects. Estimates are from a local linear RD (bandwidth = 0.10) with party and district fixed effects. 'Effective N' is the sample size within the bandwidth. Standard errors for ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Online Appendix for "Dynamic Returns to Political Tenure"

Anonymized
anonymized@anonymized.com

A Extensive Historical Background

A.1 Party System

The electoral system in the Netherlands after 1848 was centered on individual delegates, not political parties. Politicians were supposed to be independent, not least with respect to their own delegates, and to promote the common interests of the country (De Jong, 2001). Political parties were preceded by *Kiesvereenigingen*, electoral unions, of enfranchised individuals with (generally) the same political orientation, intending to coordinate their voting behavior. These electoral unions were partly a response to rising and increasing awareness of ideological differences between various factions, but also partly to increase information about elections: oftentimes, the electorate was not aware of what candidates' political positions were (Aerts et al., 2002) and diffusion of political views was limited. Faced with this nontransparent environment, De Jong (1999) argues that the electorate often based their opinions on those of individuals of high societal standing: burgomasters, notaries, clerics and similar individuals. *Kiesvereenigingen* were a way to improve the dissemination of information and aggregate electoral preferences in a more effective way. A special role in information provision was taken up by national newspapers: the editorial boards of several large national newspapers with a clear ideological background regularly endorse candidate(s) they thought reflected their politics best (De Jong, 1999).

The main issues that separated politicians of different allegiance were schooling, franchise extension and taxation. There were also differences in economic and colonial policy positions, but the most salient issues surrounding state funding of religious schools and the extent to which the state should interfere in the economy (Van Zanden and Van Riel, 2004). The funding of education was one of the aspects that accompanied the rise of religious tensions in the Netherlands throughout the nineteenth century. These religious tensions culminated in a system frequently dubber pillarization (Dutch: *Verzuiling*), meaning the segregation of the Dutch population into a Protestant and Catholic pillar, with separate societies for both, and coordination between these pillars through elites, including in national politics. The liberals formed a more loosely-defined third pillar (Stuurman, 1983).

These pillars also served as the basis for the party landscape that was arising. The first player to take the initiative towards party formation was the Protestant politician Abraham Kuyper, who founded the Anti-Revolutionary Party (ARP) in 1879 after British

model (Koch, 2020). His program centered on obtaining autonomy for the country's different religions, particularly in education (De Jong, 2001), but also in other social, economic and political institutions. Parties soon proved to be the natural means of coordination, both between politicians with a similar ideology, and between politicians and electorates: the liberal counterpart to the ARP was founded in 1895, and the Catholic union of electoral associations was founded in 1891. Additionally, and afterwards, there were also a number of Socialist parties. An overwhelming majority of incumbent politicians joined political parties, and, since it was nearly impossible to be elected without the support of a party, after the formation of parties, the number of unaffiliated politicians was negligible.

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

A.2 Compensation for Politicians

National Politics: *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.

Upper House members received no formal salary. However, they did receive a remuneration. I make use of Polak (1908), available online here, who analyzes the detailed version of the government expenditure and cost framing (*Staatsbegrooting*) of 1908. Unfortunately, these documents are not available online: they are available in the Dutch National Archives, entry 2.02.09.09, indices 890-956, and entry 2.08.41, indices 146-153. Polak (1908) men-

tions that in 1908, 25,400 guilders have been booked for the compensation of Upper House members. Considering the 50 members, this amounts to about 500 guilders per person, an amount considerably lower than for their colleages in the Lower House, but also in accordance with the lower workload of the Upper House (the Upper House only convened once or twice a week). Using a circumstantially available cost framing from 1893 available here, the exact same numbers are corroborated: the Upper House in its entirety get reimbursed for a total of 25,000 guilders, or about 500 guilders per person (Staten-Generaal, 1893).

Ministers received a considerable salary throughout our period. I use the Framing of Costs (*Raming van Kosten*) of several Ministries to sample the yearly salary of Ministers over time. In 1896, the Ministry of Justice reported a salary of 12,000 guilders for the Minister of Justice (Ministerie van Oorlog, 1890). Still, in 1906, the Ministry of War accorded the minister with a salary of again 12,000 guilders, that is, 6 times the salary of a Lower House member (Ministerie van Oorlog, 1906). The former document is available online here and the latter here.

Provincial Politics: Provincial politics are formed by a three functions: the main executive, called the King's Commissioner (*Commissaris van de Koning*), who forms the executive branch of the provincial government together with the Deputies (*Gedeputeerden*). Together, they are supervised by an assembly made up of Provincial Members of the Estates (*Provinciale Statenleden*). For the year 1893, I take the Cost Framing of the Ministry of Internal Affairs, available here (Ministerie van Binnenlandse Zaken, 1893), in which we can find an overview of the salaries of all the King's Commissioner's and Provincial executives (Deputies). The salary of a King's Commissioner in 1893 amounted to 7,000 or 8,000 guilders, whereas the salary of a Deputy amounted to 2,000 guilders, equal to the Lower House salary.

For a later period, I take the Provincial Records (*Provinciale Verslagen*) for the province of Groningen in 1910, available online here. In these Provincial Records, an annual report of provincial finances is provided, stating that the salary of the King's Commissioner (the main provincial executive) earned a salary of 7000 guilders in Groningen in 1910 (Provinciale Verslagen, 1910, p.215). In Noord-Holland, in the same year, the salary of the same position was 8000 guilders, as evidenced here (Provinciale Verslagen, 1910, p. 242). In virtually all Provincial Reports, it is difficult to calculate the salary of a Deputy due to limited transparency in accounting. The income statement of the province of Noord-Holland (Provinciale Verslagen, 1910, p. 247) states that the collective expenses for the Deputies together with support staff amounted to 76,500 guilders. On page 13 in the same document, there is an overview of all supportive personnel and their yearly salaries, amounting to 50,250 guilders. That leaves 26,250 guilders for the combined salary of the Deputies. Given that there were 7 Deputies in total, their salary over 1910 would amount to 4375 guilders per person. In Drenthe (Provinciale Verslagen, 1920, ch. 2, p. 56), arguably the poorest province, the salary for a Deputy amounted to 3500 guilders in 1920. Members of the Provincial Estates had no right to a formal salary, but instead received a small reimbursement for their efforts. In 1920, the Province of Groningen had 45 Members in the Provincial Estates (Provinciale Verslagen, 1920), which collectively received about 2000 guilders as a

reimbursement, amounting to about 40 guilders per person, a negligible amount inferior to a laborer's monthly salary.

Local Politics: Local politics consisted of three relevant functions: mayor (*Burgemeester*, alderman (*Wethouder*), which together form the daily executive branch of a municipality, and city councillors (*Gemeenteraadsleden*), who form the supervisory branch of municipal politics. The Provincial Records (*Provinciale Verslagen*) (for Groningen: Provinciale Verslagen, 1910, p. 232) (for Noord-Holland: Provinciale Verslagen, 1988, p. 26) (and Provinciale Verslagen, 1910, p. 40) contain information about the salaries of mayors. As an example, I take the provinces of Groningen and Noord-Holland in 1888 and 1910. The books are accessible online here for Groningen, and here for Noord-Holland. In 1888, the median salary for a small to medium-sized municipality equals 300-500 guilders, that is, about 4 to 8 times lower than a Lower House member's salary. In 1910, the median salary for a mayor hovers around 1000 guilders, that is about 40% of a salary of a Lower House mandate at the same time, although there are many outliers to the right. Salaries of aldermen vary from almost nothing (25 guilders) to a decent yearly wage of a skilled professional (750 guilders).

In the Provincial Records of Groningen (Provinciale Verslagen, 1910, p. 232), it is also reported that the median salary of city councillors ranges from 37,50 to about 100 guilders per year. In Drenthe in 1920 (Provinciale Verslagen, 1920, ch. 3, p. 8), the yearly salaries for the aldermen varied from about 250 guilders in the smallest municipalities to about 1000 guilders in the largest. For city councillers, the remuneration is not mentioned.

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 (Machielsen, 2021) and lead to the incorporation of the wage increase into 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).

Lawyer Salaries: To contextualize the findings regarding the transition of politicians

into the legal profession, we examine the structure of judicial salaries during the late 19th and early 20th centuries. Data derived from legislative overviews of administrative jurisdiction and Department of Justice salary proposals reveals a hierarchy that was significantly stratified by both rank and geography (Staatscommissie voor de salarisregeling van burgerlijke rijksambtenaren en beambten, 1918; Government of the Netherlands, 1906). Table C.1 summarizes the salary scales for members of the judiciary as recorded in Ministry of Justice proposals. A comparison with the remuneration of Lower House members—fixed at 2,000 to 2,500 guilders annually for the majority of this period—demonstrates that judicial roles consistently offered superior financial compensation. Even the lowest-ranking judicial officers, such as Cantonal Judges (*Kantonrechters*) in rural districts, earned salaries exceeding the parliamentary allowance. At the upper end of the spectrum, senior judgeships offered compensation four to five times that of a Member of Parliament.

Table C.1: Annual Salary Scales for Judicial Officials (Select Positions)

Position	**Annual Salary (Guilders)**
High Court (Hoge Raad)	
President	8,500 – 10,000
Vice-President	7,500 – 9,000
Councilor (*Raadsheer*)	6,500 – 8,000
Attorney General (*Procureur-Generaal*)	8,500 – 10,000
Courts of Appeal (Gerechtshoven)	
President (Amsterdam/The Hague)	7,000 – 8,500
Councilor (Amsterdam/The Hague)	6,000 – 7,500
Councilor (Rural/Other)	5,500 – 7,000
District Courts (Arrondissements-rechtbanken)	
President (Amsterdam/Rotterdam/The Hague)	5,500 – 7,000
President (2nd Class Courts)	4,500 – 6,000
Judge (Amsterdam/Rotterdam/The Hague)	4,500 – 6,000
Judge (2nd Class Courts)	3,000 – 4,500
Cantonal Courts (Kantongerechten)	
Cantonal Judge (Amsterdam/Rotterdam/The Hague)	4,500 – 6,000
Cantonal Judge (1st Class)	4,000 – 5,500
Cantonal Judge (2nd Class)	3,000 – 4,500
Cantonal Judge (3rd Class)	2,700 – 4,200

Source: Staatscommissie voor de salarisregeling van burgerlijke rijksambtenaren en beambten (1918), Department of Justice, items 70-110.

The wage structure reveals significant economic stratification that likely mirrors the private legal market of the time. The most striking feature of the data is the salary gap between the major commercial hubs (Amsterdam, Rotterdam, The Hague) and the rest of the country. For example, a District Judge in Amsterdam earned between 4,500 and 6,000 guilders,

while a colleague holding the same title in a Second Class court (such as Alkmaar) earned between 3,000 and 4,500 guilders—a difference of roughly 50% for arguably the same job description.

This premium was likely necessary to compete with the private sector. Amsterdam and Rotterdam were centers of trade and shipping; private advocates in these cities handled high-value commercial disputes and commanded significant fees. To attract competent jurists away from lucrative private practice in these cities, the State was compelled to offer significantly higher salaries than in rural areas, where private earning potential was lower.

While a salary of 8,000 guilders represented a very comfortable upper-class income for the era, top-tier private advocates in Amsterdam likely earned significantly more. In legal systems, judgeships are often viewed as a "capstone" to a career; the State relies on prestige, job security, and pension benefits to compensate for lower raw earnings compared to "star" private lawyers. The fact that public sector wages plateaued relatively quickly (reaching 4,000 to 5,000 guilders for most senior roles below the High Court) suggests that one did not enter the judiciary to maximize wealth, but to maintain status.

Therefore, if the parliamentary allowance (2,000–2,500 guilders) roughly equated to the salary of the lowest-ranking Cantonal Judge (approx. 2,700 guilders), the transition from politics to a high-ranking private legal career in a major city offered a massive financial upside. The data suggests that a successful private practice or a high-ranking judicial appointment in an urban center could yield an income multiple times that of a career politician.

A.3 Anecdotal Evidence of Rent-Seeking

Finally, in addition to indications that post-politics careers matter, there are various pieces of anecdotal evidence that could be consistent with both in-office and career paths explanations. In 1862, liberal MP Van Der Maesen de Sombreff had to step down after he was implicated in a plot to exempt the province of the district he was representing from a tax hike. De Jong and Rutjes (2015) document a plot by the local Catholic clergy and Catholic MP Haffmans, involving the clergy checking whether parishioners voted for him. In 1874, a law aimed at ending child labor was accepted (Van Den Berg and Vis, 2013). However, a parliamentary inquiry in 1886 showed that the law was not observed. Observers blamed this partially on the corruption of politicians themselves having a stake in firms exploiting child labor (Van Den Berg and Vis, 2013; Wartena, 2003). In 1909, the leadership of the Protestant ARP was implicated in a scandal involving the award of royal decorations in exchange for monetary gifts to the party (De Bruijn, 2005). In 1915, in his first term as a Lower House member, liberal MP De Jong was accused of using his Lower House function and membership of a committee on the rationing of vegetables to use inside knowledge to gain personal pecuniary advantages (Kroeze, 2013). An investigation conducted by the liberal party concluded that De Jong had used his function illegitimately, although it 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."

Additionally, there are several anecdotes which suggest that rent-seeking was an entrenched feature of political life, wherein newcomers effectively gain acceptance into the political establishment to fully integrate into its power structures. Over time, those who successfully navigate these informal norms appear to benefit from the system's rent-generating opportunities. One of the first working class MP's, Heldt, recalled: "While, after the opening of the meeting, the Minutes were read out as usual, there was certainly a bit of nervousness in the Chamber; they knew what had to be done. And what would they [the established MPs] see? A 'workman' [Heldt] who would possibly hesitate to take off his cap for the President, a smock, scenes, and God knows what else!" (Netscher, 1890). He was also refused an introductory handshake by about half of the parliament. However, his presence was quickly normalized, and later, he was even accused of being "a rentier" and "a baron" (van der Meer, 1984).

A.F. de Savorin Lohman, a strict Protestant aristocrat judge who was elected into both the Lower and Upper houses, was also unimpressed upon his arrival in the Lower House: the company he had encountered upon his arrival at the Binnenhof "had inspired him with no respect at all: an atmosphere of lies, where everything was teeming with intrigue" (Deursen, 1994). About Protestant leader Abraham Kuyper, who was first elected in 1874, it is noted that "in the political environment [of the] Netherlands [which] was still dominated by 'high gentlemen', by aristocrats and genteel bourgeoisie, Kuyper, whose roots lay in the middle class, was an exception" (Koch, 2020). Kuyper also motivated his followers to make financial sacrifices. However, with an annual summer holiday and sometimes a long stay abroad, "Kuyper led a life of luxury. He effortlessly reconciled this luxury with the Calvinist philosophy of life, with all its aversion to earthly pomp and vain ostentation" (Koch, 2020). The first socialist MP Domela Nieuwenhuis, upon his first entry in the Lower House, was refused a handshake by many of his colleagues, about which he later remarked that "the reception I received in the Lower House confirms the assertion (...) that there is no more disgusting parliament in the entire civilized world than the Dutch" (Domela Nieuwenhuis, 1910). Later, however, his presence was normalized and Domela Nieuwenhuis gained the respect of his colleagues (Stutje, 2012).

B Estimation Issues

B.1 Selection Bias

B.1.1 Truncated Wealth

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

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

$$W_i^* = \theta \cdot \mathbf{1}_{\{Politician_i\}} + \epsilon_i \tag{5}$$

where $\mathbf{1}$ is an indicator taking the value of 1 when individual i is elected, 0 otherwise. I take the error term to be $\mathcal{N}(0, \sigma^2)$. This specification is without loss of much generality, since at the margin, the influence of covariates is partialled out, including the influence of the running variable, Margin. Hence, the mean-zero assumption does not lose generality. The normal distribution allows me to obtain tangible, closed-form results for an expression of the bias.

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

$$W_i = \begin{cases} W_i^* & \text{if } W_i^* > c \\ NA & \text{if } W_i^* \leq c \end{cases} \tag{6}$$

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

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

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

Politician]:

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

Hence, if:

$$\frac{\phi\left(\frac{c-\theta}{\sigma}\right)}{1 - \Phi\left(\frac{c-\theta}{\sigma}\right)} < \frac{\phi\left(\frac{c}{\sigma}\right)}{1 - \Phi\left(\frac{c}{\sigma}\right)} \tag{7}$$

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

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

Condition 7 is very likely to be met, as c is anecdotally suggested to be close to about 300, and θ is to be of the order of $100,000$. Furthermore, σ is also of the order of $100,000$, so that this condition is likely to be satisfied in empirically plausible settings. I confirm this in the replication package, where I show that for large ranges of parameter values, this condition holds.

B.1.2 Tax evasion

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

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

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

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

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

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

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

C Robustness Checks & Supplementary Analyses

Table C.2: Covariate Balance - Second Round

	Margin Within 0.2			Margin Within 0.05			
Variable	Mean (Treated)	Mean (Control)	p-val.	Mean (Treated)	Mean (Control)	p-val.	RD Estimate
Personal Characteristics							
Rec.: Liberal	0.28	0.13	0.000***	0.32	0.24	0.040**	0.225 (0.059)
Rec. Socialist	0.04	0.01	0.063*	0.06	0.00	0.347	0.030 (0.023)
Rec.: Protestant	0.22	0.25	0.498	0.19	0.28	0.558	-0.045 (0.057)
Rec. Catholic	0.01	0.03	0.290	0.03	0.00	0.659	-0.019 (0.018)
Year of Birth Candidate	1830.02	1829.56	0.768	1822.59	1834.92	0.066*	-2.243 (1.940)
Provincial Politics Before	0.34	0.31	0.678	0.19	0.36	0.449	-0.002 (0.077)
Local Politics Before	0.22	0.14	0.190	0.15	0.24	0.984	-0.153 (0.077)
Dummy Law Before	0.45	0.47	0.723	0.38	0.48	0.555	0.125 (0.095)
Dummy Business Before	0.02	0.03	0.865	-0.04	0.08	0.368	-0.020 (0.038)
Electoral Characteristics							
Election Year	1877.80	1877.11	0.482	1875.62	1882.24	0.494	-1.524 (1.173)
Turnout	0.72	0.73	0.848	0.72	0.73	0.883	0.022 (0.019)
Log (Electoral Threshold)	7.19	7.18	0.770	7.10	7.51	0.635	-0.035 (0.076)
Log(Electorate Size)	8.25	8.23	0.551	8.17	8.56	0.630	-0.060 (0.066)
District Characteristics							
District Population	11.62	11.67	0.401	11.61	11.85	0.382	-0.082 (0.110)
% Labor Force Industry District	0.38	0.39	0.065*	0.38	0.39	0.024**	-0.004 (0.003)
% Labor Force Agriculture District	0.13	0.13	0.689	0.13	0.10	0.511	0.002 (0.005)
% Labor Force Services District	0.49	0.49	0.478	0.49	0.51	0.793	0.002 (0.005)
% Paying Wealth Tax District	7.16	7.11	0.563	7.03	7.03	0.320	-0.098 (0.098)
Income Tax Share District	8.21	8.16	0.433	8.09	8.25	0.292	-0.070 (0.083)
% Catholic District	0.30	0.30	0.222	0.30	0.32	0.780	0.004 (0.007)
% Protestant District	0.63	0.65	0.024**	0.65	0.63	0.391	-0.007 (0.006)
Birthplace Characteristics							
% Labor Force Industry Birth Place	0.35	0.36	0.656	0.37	0.35	0.807	-0.038 (0.022)
% Labor Force Agriculture Birth Place	0.09	0.05	0.047**	0.14	0.04	0.085*	0.024 (0.020)
% Labor Force Services Birth Place	0.56	0.59	0.345	0.49	0.61	0.221	0.014 (0.036)
% Catholic Birth Place	0.35	0.38	0.368	0.39	0.33	0.971	-0.038 (0.045)
% Protestant Birth Place	0.62	0.60	0.473	0.59	0.65	0.947	0.031 (0.044)
Distance to The Hague - BP	71.20	72.82	0.838	61.71	82.93	0.552	-32.874 (10.266)

The table contains means for various sets of variables conditioned on the absolute margin being lower than 0.2 (left panel) and lower than 0.05 (right panel). The sample is candidates who have been elected once so far. 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). Standard errors clustered at the district-level are shown between brackets. Significance is indicated by *: $p \leq 0.1$, **: $p \leq 0.05$, ***: $p \leq 0.01$.

Table C.3: Covariate Balance - Third Round

	Margin Within 0.2			Margin Within 0.05			
Variable	Mean (Treated)	Mean (Control)	p-val.	Mean (Treated)	Mean (Control)	p-val.	RD Estimate
Personal Characteristics							
Rec.: Liberal	0.39	0.30	0.090*	0.45	0.30	0.105	0.193 (0.067)*
Rec. Socialist	0.06	0.01	0.061*	0.01	0.00	NA	-0.003 (0.022)
Rec.: Protestant	0.21	0.26	0.344	0.32	0.19	0.530	-0.088 (0.072)
Rec. Catholic	0.04	0.04	0.976	0.02	0.07	0.778	-0.003 (0.051)
Year of Birth Candidate	1831.85	1831.52	0.872	1834.47	1827.81	0.417	-1.585 (2.494)
Provincial Politics Before	0.42	0.36	0.481	0.30	0.37	0.676	-0.025 (0.106)
Local Politics Before	0.23	0.15	0.285	-0.03	0.22	0.073*	-0.050 (0.083)
Dummy Law Before	0.50	0.44	0.509	0.43	0.52	0.965	-0.146 (0.118)
Dummy Business Before	0.02	0.04	0.387	0.00	0.07	0.567	-0.012 (0.042)
Electoral Characteristics							
Election Year	1881.56	1880.37	0.271	1881.39	1877.59	0.548	-2.439 (1.222)
Turnout	0.71	0.73	0.208	0.75	0.74	0.507	-0.035 (0.023)
Log (Electoral Threshold)	7.18	7.24	0.375	7.30	7.28	0.710	-0.118 (0.095)
Log(Electorate Size)	8.27	8.29	0.799	8.31	8.32	0.853	-0.068 (0.078)
District Characteristics							
District Population	11.61	11.56	0.347	11.68	11.69	0.257	0.152 (0.059)
% Labor Force Industry District	0.38	0.39	0.349	0.39	0.37	0.825	-0.001 (0.003)
% Labor Force Agriculture District	0.15	0.14	0.127	0.15	0.10	0.202	0.007 (0.005)
% Labor Force Services District	0.47	0.47	0.493	0.47	0.53	0.271	-0.006 (0.006)
% Paying Wealth Tax District	7.12	7.04	0.301	7.42	7.09	0.054*	0.400 (0.112)*
Income Tax Share District	8.11	8.05	0.276	8.36	8.15	0.041**	0.327 (0.083)**
% Catholic District	0.27	0.27	0.094*	0.28	0.32	0.195	-0.001 (0.008)
% Protestant District	0.67	0.68	0.650	0.66	0.65	0.198	0.003 (0.008)
Birthplace Characteristics							
% Labor Force Industry Birth Place	0.33	0.34	0.342	0.34	0.35	0.992	0.018 (0.015)
% Labor Force Agriculture Birth Place	0.04	0.07	0.498	0.05	0.06	0.765	-0.036 (0.023)
% Labor Force Services Birth Place	0.63	0.59	0.404	0.61	0.59	0.835	0.018 (0.036)
% Catholic Birth Place	0.35	0.36	0.908	0.29	0.39	0.337	0.001 (0.042)
% Protestant Birth Place	0.62	0.63	0.807	0.68	0.60	0.437	-0.003 (0.039)
Distance to The Hague - BP	66.65	74.59	0.434	53.93	76.34	0.136	-30.495 (11.176)*

The table contains means for various sets of variables conditioned on the absolute margin being lower than 0.2 (left panel) and lower than 0.05 (right panel). The sample is candidates who have been elected twice so far. 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). Standard errors clustered at the district-level are shown between brackets. Significance is indicated by *: $p \leq 0.1$, **: $p \leq 0.05$, ***: $p \leq 0.01$.

Table C.4: Descriptive Statistics

	Mean	SD	Min	Max	N
Panel A: Party Affiliation					
Party: Catholic	0.11	0.31	0.00	1.00	4899
Party: Protestant	0.32	0.47	0.00	1.00	4899
Party: Liberal	0.45	0.50	0.00	1.00	4899
Party: Socialist	0.06	0.24	0.00	1.00	4899
Panel B: Newspaper Recommendations					
Rec.: Protestant	0.15	0.36	0.00	1.00	4899
Rec.: Liberal	0.20	0.40	0.00	1.00	4899
Rec.: Socialist	0.06	0.23	0.00	1.00	4899
Rec.: Catholic	0.05	0.22	0.00	1.00	4899
Panel C: Candidate-Election Characteristics					
Age at Election	49.47	10.05	21.00	83.00	4899
Year of Election	1879.54	19.42	1848.00	1918.00	4899
Number of Tries Until Election	5.82	9.59	0.00	97.00	4899
Election HHI	0.46	0.19	0.05	1.00	4899
Electoral Threshold	7.00	0.72	4.91	9.06	4899
Electorate Size	8.16	0.65	5.83	10.22	4899
Turnout (% of Electorate)	0.66	0.17	0.16	0.98	4899
Turnout Previous Election	0.65	0.18	0.16	0.97	3933
Panel D: District Characteristics					
Log Population District	11.36	1.21	0.00	13.38	4899
Share Protestant District	0.60	0.24	0.00	0.97	4861
Share Catholic District	0.34	0.26	0.00	1.00	4861
Labor Force Share Agriculture District	0.17	0.12	0.00	0.43	4764
Labor Force Share Industry District	0.42	0.09	0.27	0.68	4764
Labor Force Share Services District	0.42	0.18	0.08	0.72	4764
Wealth Tax Revenue	3785.28	3407.46	0.00	13406.00	4899
District Paying Income Tax	6740.25	5923.28	0.00	26840.00	4899
Distance to The Hague - District	87.34	57.70	0.00	216.12	4898
Panel E: Birthplace Characteristics					
Labor Force Share Agriculture Birthplace	0.08	0.11	0.00	0.44	2948
Labor Force Share Industry Birthplace	0.36	0.09	0.26	0.73	2948
Labor Force Share Services Birthplace	0.56	0.18	0.06	0.72	2948
Share Protestant Birthplace	0.58	0.26	0.00	1.00	3682
Share Catholic Birthplace	0.40	0.27	0.00	1.00	3682
Distance to The Hague - Birthplace	78.60	55.36	0.00	218.85	4048
Panel F: Dependent Variables					
Lifespan	22.13	12.52	0.02	59.68	4899
Net Wealth (Deflated, Log)	10.54	3.50	0.00	15.09	3026
Career: Politics	0.43	0.50	0.00	1.00	4899
Career: Nat. Politics	0.28	0.45	0.00	1.00	4899
Career: Prov. Politics	0.17	0.38	0.00	1.00	4899
Career: Municipal Politics	0.09	0.28	0.00	1.00	4899
Career: Non-Politics	0.08	0.27	0.00	1.00	4899
Career: Law	0.07	0.26	0.00	1.00	4899

This table shows descriptive statistics for all observations. Panel A are party dummies. In panel B, I show newspaper recommendations for each major political faction. Panel C shows candidate-election characteristics, candidate age, year of election, number of tries of candidate until this election,a Herfindahl-Hirschmann index of competitiveness, electoral threshold, size of the electoral, turnout and past turnout. Panels D and E contain district and birthplace characteristics. Panel F shows various dependent variables used in this study.

Table C.5: Comparison of Candidate Characteristics: Full Sample vs. Wealth Subsample

Variable	Full Sample	Wealth Subsample	p-value
Personal Characteristics			
Party: Liberal	0.42	0.53	0.000***
Party: Protestant	0.32	0.27	0.005**
Party: Catholic	0.07	0.07	0.763
Party: Socialist	0.04	0.03	0.090*
Year of Birth Candidate	1831.85	1832.91	0.162
Dummy Upper House Before	0.04	0.03	0.147
Provincial Politics Before	0.29	0.32	0.156
Local Politics Before	0.15	0.18	0.051*
Dummy Law Before	0.43	0.48	0.018**
Dummy Business Before	0.02	0.02	0.777
Election Characteristics			
Election Year	1881.93	1882.70	0.245
Log(Turnout)	7.82	7.87	0.056*
Log (Electoral Threshold)	7.29	7.28	0.768
Log(Electorate Size)	8.32	8.32	0.814
District Characteristics			
District Population	11.64	11.67	0.481
% Labor Force Industry District	0.40	0.40	0.670
% Labor Force Agriculture District	0.14	0.14	0.161
% Labor Force Services District	0.46	0.47	0.290
% Paying Wealth Tax District	7.10	7.10	0.994
Income Tax Share District	8.15	8.16	0.836
% Catholic District	0.30	0.31	0.685
Birthplace Characteristics			
% Protestant District	0.64	0.64	0.768
% Labor Force Industry Birth Place	0.36	0.35	0.559
% Labor Force Agriculture Birth Place	0.07	0.06	0.022**
% Labor Force Services Birth Place	0.57	0.58	0.094*
% Catholic Birth Place	0.38	0.37	0.238
% Protestant Birth Place	0.60	0.61	0.231
Distance to The Hague - BP	73.63	70.73	0.242

The Table reports the results of a linear regression model estimating the conditional mean difference between the Full Sample and the Wealth Available subsample for a host of characteristics. The reported numbers are the mean for each variable in each respective sample. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

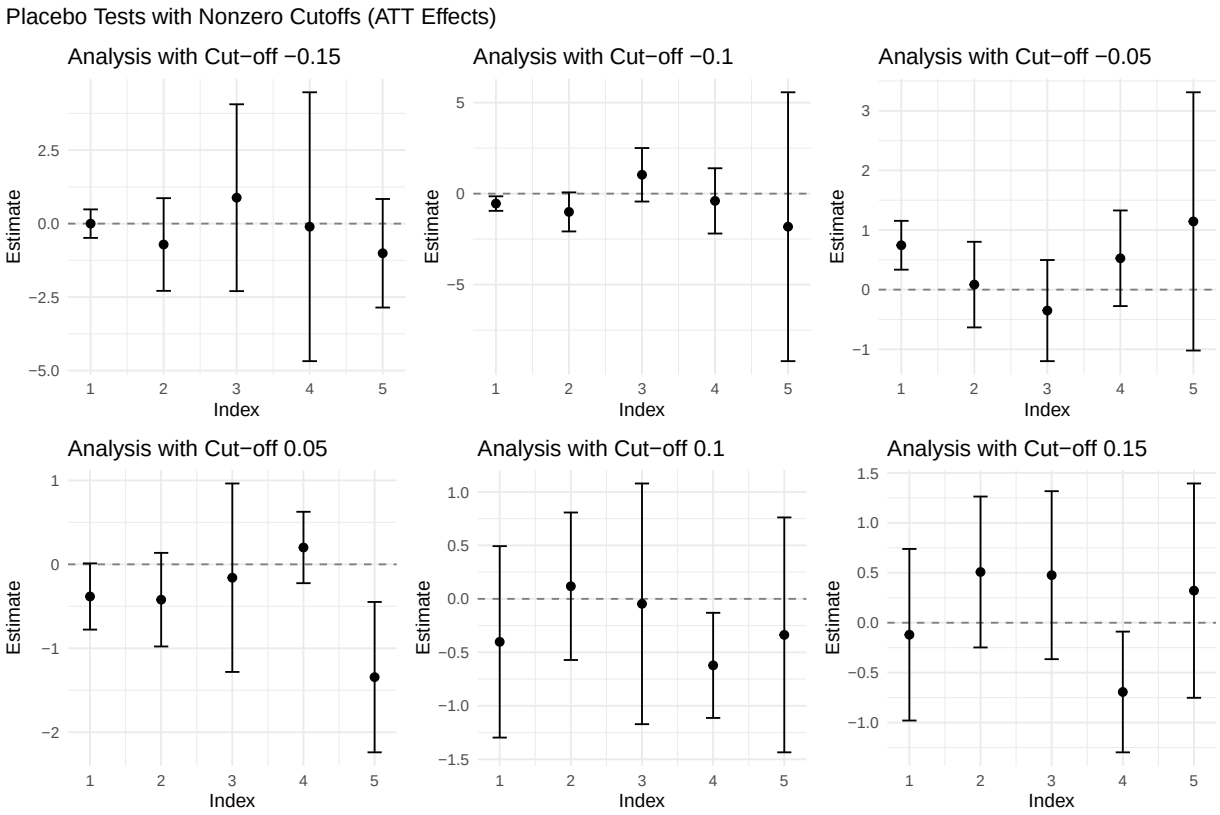

Figure C. 1: Placebo Test With Varying Cutoff

Table C.6: Estimates of the Returns to Politics

Round τ	1	2	3	4	5
Coefficient (ATT)	1.133***	1.965***	-0.742	-0.779**	0.211
SE (ATT)	(0.383)	(0.630)	(0.582)	(0.305)	(0.524)
Coefficient (ITT)	1.415**	1.894***	-0.831	-0.757*	0.211
SE (ITT)	(0.375)	(0.626)	(0.580)	(0.300)	(0.524)
Mean DV Treated	9.024	8.778	8.677	8.884	9.671
Mean DV Control	8.265	7.783	9.085	9.648	7.870
Effective N (Treated)	68	59	31	29	23
Effective N (Control)	115	38	33	26	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes	Yes

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is log(1 + net wealth)/Lifespan. 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=5). Estimates are from a local linear RD with the optimal bandwidth selection, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.7: Covariate Balance - First Round

	Margin Within 0.2			Margin Within 0.05			
Variable	Mean (Treated)	Mean (Control)	p-val.	Mean (Treated)	Mean (Control)	p-val.	RD Estimate
Dummy Minister Before	0.05	0.05	0.918	0.03	0.09	0.740	0.098 (0.049)
Dummy Mayor Before	0.11	0.03	0.019**	0.07	0.07	0.514	0.098 (0.052)
Dummy Alderman Before	0.06	0.03	0.259	0.07	0.04	0.506	0.015 (0.040)
Dummy Deputy Before	0.04	0.01	0.313	0.07	0.00	0.153	0.018 (0.029)*
Dummy Law Before	0.13	0.22	0.103	0.02	0.28	0.028**	-0.240 (0.071)
Dummy Business Before	0.01	0.02	0.708	0.03	0.01	0.738	-0.002 (0.022)
Dummy Municipal Rep. Before	0.13	0.14	0.849	0.03	0.22	0.284	-0.071 (0.073)

The table contains means for various sets of variables conditioned on the absolute margin being lower than 0.2 (left panel) and lower than 0.05 (right panel). The sample is candidates who have never been elected so far. 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). Standard errors clustered at the district-level are shown between brackets. Significance is indicated by *: $p \leq 0.1$, **: $p \leq 0.05$, ***: $p \leq 0.01$.

Table C.8: Covariate Balance - Second Round

	Margin Within 0.2			Margin Within 0.05			
Variable	Mean (Treated)	Mean (Control)	p-val.	Mean (Treated)	Mean (Control)	p-val.	RD Estimate
Dummy Minister Before	0.11	0.13	0.692	0.22	0.12	0.450	0.019 (0.074)
Dummy Mayor Before	0.15	0.08	0.126	0.12	0.12	0.674	-0.085 (0.061)
Dummy Alderman Before	0.09	0.08	0.727	0.03	0.16	0.655	-0.102 (0.057)
Dummy Deputy Before	0.04	0.03	0.536	0.09	0.04	0.321	0.080 (0.036)
Dummy Law Before	0.45	0.47	0.723	0.38	0.48	0.555	0.125 (0.095)
Dummy Business Before	0.02	0.03	0.865	-0.04	0.08	0.368	-0.020 (0.038)
Dummy Municipal Rep. Before	0.38	0.28	0.120	0.24	0.48	0.849	-0.063 (0.094)
Election Turnout Last Election Candidate	0.71	0.71	0.888	0.73	0.68	0.777	0.013 (0.032)
Vote Share Last Election Candidate	0.38	0.42	0.195	0.32	0.42	0.053*	-0.133 (0.037)

The table contains means for various sets of variables conditioned on the absolute margin being lower than 0.2 (left panel) and lower than 0.05 (right panel). The sample is candidates who have been elected once. 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). Standard errors clustered at the district-level are shown between brackets. Significance is indicated by *: $p \leq 0.1$, **: $p \leq 0.05$, ***: $p \leq 0.01$.

Table C.9: Estimates of the Returns to Politics

Round τ	1	2	3	4
Coefficient (ATT)	0.917**	1.458**	0.150	-0.627
SE (ATT)	(0.426)	(0.579)	(0.450)	(0.330)
Coefficient (ITT)	1.327**	1.438**	-0.024	-0.627
SE (ITT)	(0.392)	(0.565)	(0.441)	(0.330)
Mean DV Treated	11.863	11.766	11.860	12.115
Mean DV Control	11.349	10.805	11.842	12.261
Effective N (Treated)	68	59	31	29
Effective N (Control)	117	38	33	26
Bandwidth	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes

Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under $t^* = 4$. 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). Standard errors for the ATT estimates are derived using the delta method.The estimates are conditional on party, decade and district fixed effects. *: p < 0.10, **: p < 0.05, ***: p < 0.01.

Table C.10: Estimates of the Returns to Politics

Round τ	1	2	3	4	5	6
Coefficient (ATT)	0.939**	1.444**	0.107	-0.350	0.055	-3.004
SE (ATT)	(0.427)	(0.582)	(0.474)	(0.515)	(0.946)	(3.050)
Coefficient (ITT)	1.327**	1.438**	-0.024	-0.627	-0.777	-3.004
SE (ITT)	(0.392)	(0.565)	(0.441)	(0.330)	(0.428)	(3.050)
Mean DV Treated	11.863	11.766	11.860	12.115	12.579	12.503
Mean DV Control	11.349	10.805	11.842	12.261	11.056	13.045
Effective N (Treated)	68	59	31	29	23	14
Effective N (Control)	117	38	33	26	12	7
Bandwidth	0.100	0.100	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes	Yes	Yes

Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under $t^* = 6$. 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). Standard errors for the ATT estimates are derived using the delta method.The estimates are conditional on party, decade and district fixed effects. *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

Table C.11: Estimates of the Returns to Politics

Round τ	1	2	3	4	5	6	7
Coefficient (ATT)	0.900**	1.465**	0.108	-0.409	0.117	-2.165	-3.029*
SE (ATT)	(0.427)	(0.582)	(0.474)	(0.518)	(0.956)	(3.065)	(1.090)
Coefficient (ITT)	1.327**	1.438**	-0.024	-0.627	-0.777	-3.004	-3.029*
SE (ITT)	(0.392)	(0.565)	(0.441)	(0.330)	(0.428)	(3.050)	(1.090)
Mean DV Treated	11.863	11.766	11.860	12.115	12.579	12.503	11.369
Mean DV Control	11.349	10.805	11.842	12.261	11.056	13.045	11.996
Effective N (Treated)	68	59	31	29	23	14	15
Effective N (Control)	117	38	33	26	12	7	7
Bandwidth	0.100	0.100	0.100	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under $t^* = 7$. All the ATT coefficients are derived and recursively computed from ITT coefficients, which are in turn estimated using the methodology in (Cattaneo et al., 2019). Standard errors for the ATT estimates are derived using the delta method.The estimates are conditional on party, decade and district fixed effects. *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

Table C.12: Estimates of the Returns to Politics

Round τ	1	2	3	4	5
Panel A: Bandwidth 0.07					
Coefficient (ATT)	1.171***	2.198***	-0.627	4.658***	0.734
SE (ATT)	(0.396)	(0.567)	(0.499)	(0.385)	(0.878)
Coefficient (ITT)	0.943**	1.882***	-0.506	4.683***	0.734
SE (ITT)	(0.391)	(0.564)	(0.496)	(0.384)	(0.878)
Mean DV Treated	11.863	11.766	11.860	12.115	12.579
Mean DV Control	11.349	10.805	11.842	12.261	11.056
Effective N (Treated)	53	45	20	15	16
Effective N (Control)	82	30	25	17	5
Bandwidth	0.070	0.070	0.070	0.070	0.070
Panel B: Bandwidth 0.15					
Coefficient (ATT)	0.610*	1.494***	-0.152	-0.087	-0.334
SE (ATT)	(0.337)	(0.463)	(0.448)	(0.347)	(0.530)
Coefficient (ITT)	0.793**	1.505***	-0.156	-0.122	-0.334
SE (ITT)	(0.331)	(0.459)	(0.446)	(0.343)	(0.530)
Mean DV Treated	11.863	11.766	11.860	12.115	12.579
Mean DV Control	11.349	10.805	11.842	12.261	11.056
Effective N (Treated)	80	82	48	39	34
Effective N (Control)	157	47	37	31	14
Bandwidth	0.150	0.150	0.150	0.150	0.150
Panel C: Bandwidth 0.20					
Coefficient (ATT)	0.662**	1.479***	-0.203	0.446	1.561*
SE (ATT)	(0.337)	(0.396)	(0.406)	(0.337)	(0.452)
Coefficient (ITT)	0.663	1.304***	-0.239	0.591	1.561*
SE (ITT)	(0.331)	(0.392)	(0.404)	(0.335)	(0.452)
Mean DV Treated	11.863	11.766	11.860	12.115	12.579
Mean DV Control	11.349	10.805	11.842	12.261	11.056
Effective N (Treated)	98	98	61	60	45
Effective N (Control)	189	61	41	33	16
Bandwidth	0.200	0.200	0.200	0.200	0.200
Panel D: Bandwidth 0.30					
Coefficient (ATT)	0.871***	1.018***	-0.454	0.754**	1.246
SE (ATT)	(0.286)	(0.350)	(0.402)	(0.330)	(0.429)
Coefficient (ITT)	0.875*	0.859**	-0.399	0.898	1.246
SE (ITT)	(0.281)	(0.346)	(0.400)	(0.327)	(0.429)
Mean DV Treated	11.863	11.766	11.860	12.115	12.579
Mean DV Control	11.349	10.805	11.842	12.261	11.056
Effective N (Treated)	126	127	85	80	61
Effective N (Control)	231	69	47	37	16
Bandwidth	0.300	0.300	0.300	0.300	0.300
Panel E: Bandwidth 0.35					
Coefficient (ATT)	0.932***	0.802**	-0.357	-0.029	2.030**
SE (ATT)	(0.268)	(0.334)	(0.402)	(0.305)	(0.379)
Coefficient (ITT)	0.927**	0.641**	-0.403	0.221**	2.030**
SE (ITT)	(0.263)	(0.330)	(0.400)	(0.302)	(0.379)
Mean DV Treated	11.863	11.766	11.860	12.115	12.579
Mean DV Control	11.349	10.805	11.842	12.261	11.056
Effective N (Treated)	138	139	94	95	66
Effective N (Control)	253	73	48	40	19
Bandwidth	0.350	0.350	0.350	0.350	0.350

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is log(1 + net wealth). 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the -th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=5). Estimates are from a local linear RD with the optimal bandwidth selection, conditional on party, decade, and district fixed effects. Standard

Table C.13: Estimates of the Returns to Politics

Round τ	1	2	3	4	5
Coefficient (ATT)	0.348	1.588**	0.303	-0.944	1.379
SE (ATT)	(0.413)	(0.675)	(0.645)	(0.712)	(0.756)
Coefficient (ITT)	0.468	1.553**	0.152	-0.813	1.379
SE (ITT)	(0.400)	(0.669)	(0.640)	(0.708)	(0.756)
Mean DV Treated	11.863	11.746	11.870	12.126	12.576
Mean DV Control	11.346	10.823	11.849	12.313	11.056
Effective N (Treated)	90	58	51	18	40
Effective N (Control)	179	38	37	19	16
Bandwidth	0.181	0.096	0.157	0.078	0.179
FE	Yes	Yes	Yes	Yes	Yes

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is log(1 + net wealth). 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming $t^*=5$). Estimates are from a local linear RD with the optimal bandwidth selection, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.14: Estimates of the Returns to Politics

Round τ	1	2	3	4	5
Coefficient (ATT)	0.929**	1.723***	-0.256	0.670***	-0.907
SE (ATT)	(0.363)	(0.529)	(0.404)	(0.229)	(0.300)
Coefficient (ITT)	1.109**	1.749***	-0.149	0.580	-0.907
SE (ITT)	(0.357)	(0.527)	(0.403)	(0.227)	(0.300)
Mean DV Treated	11.863	11.766	11.860	12.115	12.579
Mean DV Control	11.349	10.805	11.842	12.261	11.056
Effective N (Treated)	68	59	31	29	23
Effective N (Control)	117	38	33	26	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes	Yes

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is log(1 + net wealth). 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=5). Estimates are from a local linear RD with the a bandwidth of 0.10 and a triangular or Epanechnikov kernel, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.15: Estimates of the Returns to Politics

Round τ	1	2	3	4	5
Coefficient (ATT)	0.996***	2.097***	-0.366	1.000***	-0.676
SE (ATT)	(0.343)	(0.520)	(0.369)	(0.181)	(0.233)
Coefficient (ITT)	1.174***	2.077***	-0.237	0.933***	-0.676
SE (ITT)	(0.338)	(0.518)	(0.369)	(0.179)	(0.233)
Mean DV Treated	11.863	11.766	11.860	12.115	12.579
Mean DV Control	11.349	10.805	11.842	12.261	11.056
Effective N (Treated)	68	59	31	29	23
Effective N (Control)	117	38	33	26	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes	Yes

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is log(1 + net wealth). 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=5). Estimates are from a local linear RD with the a bandwidth of 0.10 and a triangular or Epanechnikov kernel, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.16: Estimates of the Returns to Politics

Round τ	1	2	3	4	5
Coefficient (ATT)	1.113***	1.348***	-0.036	-0.537	-0.777*
SE (ATT)	(0.250)	(0.278)	(0.447)	(0.404)	(0.705)
Coefficient (ITT)	1.346**	1.438***	-0.055	-0.615	-0.777*
SE (ITT)	(0.241)	(0.267)	(0.445)	(0.398)	(0.705)
Mean DV Treated	11.857	11.773	11.863	12.142	12.579
Mean DV Control	11.335	10.820	11.842	12.353	11.056
Effective N (Treated)	68	59	31	29	23
Effective N (Control)	117	38	33	26	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes	Yes

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is log(1 + net wealth). 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the -th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=5). Estimates are from a local linear RD with a bandwidth of 0.10, conditional on party, decade, and district fixed effects. Standard errors for the ITT are clustered by party and standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.17: Estimates of the Returns to Politics - Incumbency

Round τ	1	2	3	4	5
Coefficient (ATT)	1.073***	1.433**	0.082	-0.490	-0.777
SE (ATT)	(0.405)	(0.570)	(0.445)	(0.338)	(0.428)
Coefficient (ITT)	1.327**	1.438**	-0.024	-0.627	-0.777
SE (ITT)	(0.392)	(0.565)	(0.441)	(0.330)	(0.428)
Mean DV Treated	11.863	11.766	11.860	12.115	12.579
Mean DV Control	11.349	10.805	11.842	12.261	11.056
Effective N (Treated)	68	59	31	29	23
Effective N (Control)	117	38	33	26	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes	Yes

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is log(1 + net wealth). 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=5). Estimates are from a local linear RD with a fixed bandwidth of 0.10, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.18: Estimates of the Returns to Politics - Net Wealth Start

Round τ	1	2	3	4	5
Coefficient (ATT)	0.925**	1.202**	-0.049	-0.403	-1.698***
SE (ATT)	(0.407)	(0.564)	(0.416)	(0.334)	(0.456)
Coefficient (ITT)	1.191*	1.368**	-0.014	-0.572	-1.698***
SE (ITT)	(0.401)	(0.561)	(0.415)	(0.331)	(0.456)
Mean DV Treated	12.096	11.958	12.022	12.288	12.705
Mean DV Control	11.563	10.977	12.014	12.421	11.095
Effective N (Treated)	68	59	31	29	23
Effective N (Control)	117	38	33	26	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes	Yes

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is log(1 + net wealth). 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=5). Estimates are from a local linear RD with a fixed bandwidth of 0.10, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.19: Estimates of the Returns to Politics - Net Wealth End

Round τ	1	2	3	4	5
Coefficient (ATT)	1.145***	1.266**	0.076	-0.346	-1.412**
SE (ATT)	(0.417)	(0.562)	(0.413)	(0.353)	(0.497)
Coefficient (ITT)	1.389***	1.416**	0.104	-0.486	-1.412**
SE (ITT)	(0.411)	(0.559)	(0.411)	(0.349)	(0.497)
Mean DV Treated	12.123	11.954	12.012	12.273	12.691
Mean DV Control	11.614	10.962	11.966	12.396	11.324
Effective N (Treated)	68	59	31	29	23
Effective N (Control)	65	38	33	26	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes	Yes

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is log(1 + net wealth). 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=5). Estimates are from a local linear RD with a fixed bandwidth of 0.10, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.20: Estimates of the Returns to Politics - Ihs Transform

Round τ	1	2	3	4	5
Coefficient (ATT)	1.742*	3.585***	0.611	-0.651**	-0.775
SE (ATT)	(1.029)	(1.089)	(1.157)	(0.316)	(0.428)
Coefficient (ITT)	2.000	3.734***	0.632	-0.691	-0.775
SE (ITT)	(1.023)	(1.086)	(1.157)	(0.315)	(0.428)
Mean DV Treated	11.517	11.493	11.325	12.808	13.273
Mean DV Control	11.320	10.822	11.548	12.508	11.750
Effective N (Treated)	76	64	33	30	23
Effective N (Control)	120	39	36	28	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes	Yes

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is log(1 + net wealth). 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=5). Estimates are from a local linear RD with a fixed bandwidth of 0.10, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.21: Estimates of the Returns to Politics - Scale Wealth 0.01

Round τ	1	2	3	4	5
Coefficient (ATT)	1.135	2.199***	0.366	-0.551*	-1.674***
SE (ATT)	(0.692)	(0.751)	(0.832)	(0.323)	(0.456)
Coefficient (ITT)	1.394	2.410***	0.456	-0.638	-1.674***
SE (ITT)	(0.687)	(0.748)	(0.831)	(0.322)	(0.456)
Mean DV Treated	6.826	6.736	6.640	7.689	8.108
Mean DV Control	6.596	6.114	6.786	7.313	6.492
Effective N (Treated)	76	64	33	30	23
Effective N (Control)	120	39	36	28	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes	Yes

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is a scaled version of log(1 + net wealth). 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=5). Estimates are from a local linear RD with a fixed bandwidth of 0.10, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.22: Estimates of the Returns to Politics - Scale Wealth 0.000001

Round τ	1	2	3	4	5
Coefficient (ATT)	0.214	0.856***	0.074	-0.389*	0.033
SE (ATT)	(0.169)	(0.212)	(0.237)	(0.198)	(0.316)
Coefficient (ITT)	0.287	0.884***	0.051	-0.387	0.033
SE (ITT)	(0.166)	(0.209)	(0.236)	(0.197)	(0.316)
Mean DV Treated	1.048	0.972	1.017	1.152	1.441
Mean DV Control	0.930	0.663	1.002	1.235	0.569
Effective N (Treated)	75	63	32	29	23
Effective N (Control)	120	39	36	28	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes	Yes

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is a scaled version of log(1 + net wealth). 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=5). Estimates are from a local linear RD with a fixed bandwidth of 0.10, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.23: Estimates of the Total Returns to Politics (QMLE)

Round τ:	1	2	3	4	5
Coefficient (ITT)	0.081**	0.167**	-0.023	-0.092	0.034
	(0.035)	(0.080)	(0.023)	(0.076)	(0.041)
Num.Obs.	504	268	189	148	105
District FE	Yes	Yes	Yes	Yes	Yes
Party FE	Yes	Yes	Yes	Yes	Yes

* p ¡ 0.1, ** p ¡ 0.05, *** p ¡ 0.01
Poisson Quasi-Maximum Likelihood estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is log(1 + net wealth). The estimates are derived using the equation Personal Wealth$_i$ = α_0+α_1Margin$_i$+α_2Margin$_i$× Winner$_i$ + δWinner$_i$ + ϵ_i using weights inversely proportional to the distance from zero. Standard errors are clustered at the politician-level. Models show the estimate of returns to each subsequent period of tenure (referred to as Round) for a subsample of candidates at their first try. The estimates are conditional on party and district fixed effects. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.24: Estimates of the Returns to Politics

Round τ	1	2	3	4	5
Coefficient (ATT)	1.240***	1.658***	0.043	-0.534	-0.400
SE (ATT)	(0.408)	(0.552)	(0.575)	(0.333)	(0.428)
Coefficient (ITT)	1.569**	1.651***	-0.078	-0.609	-0.400
SE (ITT)	(0.393)	(0.541)	(0.572)	(0.323)	(0.428)
Mean DV Treated	11.863	11.769	11.860	12.122	12.579
Mean DV Control	11.348	10.828	11.838	12.261	11.056
Effective N (Treated)	68	59	31	29	23
Effective N (Control)	117	38	33	26	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes	Yes

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is log(1 + net wealth). 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the -th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=5). Estimates are from a local linear RD with the a fixed bandwidth of 0.10, conditional on district x year fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.25: Estimates of the Returns to Politics

Round τ	1	2	3	4	5
Coefficient (ATT)	0.950**	1.374**	-0.184	0.661**	-0.903**
SE (ATT)	(0.401)	(0.540)	(0.453)	(0.331)	(0.433)
Coefficient (ITT)	1.082**	1.424**	-0.109	0.571	-0.903**
SE (ITT)	(0.396)	(0.537)	(0.452)	(0.328)	(0.433)
Mean DV Treated	11.863	11.750	11.860	12.126	12.579
Mean DV Control	11.350	10.838	11.841	12.344	11.056
Effective N (Treated)	68	59	31	29	23
Effective N (Control)	117	38	33	26	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes	Yes

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is log(1 + net wealth). 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the -th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=5). Estimates are from a local linear RD with the a fixed bandwidth of 0.10, conditional on party and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.26: Estimates of the Returns to Politics - Control Variables

Round τ	1	2	3	4	5
Coefficient (ATT)	1.087***	1.373**	-0.015	-0.544	-0.777
SE (ATT)	(0.400)	(0.567)	(0.443)	(0.333)	(0.428)
Coefficient (ITT)	1.327**	1.438**	-0.024	-0.627	-0.777
SE (ITT)	(0.392)	(0.565)	(0.441)	(0.330)	(0.428)
Mean DV Treated	11.863	11.766	11.860	12.115	12.579
Mean DV Control	11.349	10.805	11.842	12.261	11.056
Effective N (Treated)	68	59	31	29	23
Effective N (Control)	117	38	33	26	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes	Yes

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is log(1 + net wealth). 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=5). Estimates are from a local linear RD with a fixed bandwidth of 0.10, conditional on select controls, and party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.27: Estimates of the Returns to Politics - Winsorize

Round τ	1	2	3	4	5
Coefficient (ATT)	1.525**	3.219***	0.078	-0.571*	-0.701
SE (ATT)	(0.637)	(1.050)	(0.460)	(0.336)	(0.440)
Coefficient (ITT)	1.938**	3.315**	0.052	-0.641	-0.701
SE (ITT)	(0.627)	(1.048)	(0.458)	(0.333)	(0.440)
Mean DV Treated	11.904	11.820	11.860	12.151	12.586
Mean DV Control	11.036	10.111	11.830	12.261	11.056
Effective N (Treated)	68	59	31	29	23
Effective N (Control)	117	38	33	26	12
Bandwidth	0.100	0.100	0.100	0.100	0.100
FE	Yes	Yes	Yes	Yes	Yes

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The dependent variable is log(1 + net wealth). 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=5). Estimates are from a local linear RD with a fixed bandwidth of 0.10, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.28: Estimates of the Returns to Politics

Round τ	1	2	3	4	5
Panel A: Entry Provincial Politics					
Coefficient (ATT)	-0.249**	-0.355**	0.783***	0.077	-0.863***
SE (ATT)	(0.107)	(0.140)	(0.166)	(0.135)	(0.219)
Coefficient (ITT)	-0.268	-0.240**	0.851***	0.034	-0.863***
SE (ITT)	(0.104)	(0.138)	(0.165)	(0.134)	(0.219)
Mean DV Treated	-0.190	-0.161	-0.107	-0.350	-0.467
Mean DV Control	0.024	-0.219	-0.438	-0.167	0.143
Effective N (Treated)	126	102	53	49	36
Effective N (Control)	240	60	51	32	21
Bandwidth	0.100	0.100	0.100	0.100	0.100
Panel B: Entry Law					
Coefficient (ATT)	0.241***	-0.092	0.334**	0.607***	-0.186
SE (ATT)	(0.077)	(0.123)	(0.131)	(0.187)	(0.265)
Coefficient (ITT)	0.173	-0.100	0.380***	0.597**	-0.186
SE (ITT)	(0.072)	(0.120)	(0.129)	(0.187)	(0.265)
Mean DV Treated	0.310	0.446	0.429	0.400	0.467
Mean DV Control	0.210	0.375	0.438	0.500	0.429
Effective N (Treated)	126	102	53	49	36
Effective N (Control)	240	60	51	32	21
Bandwidth	0.100	0.100	0.100	0.100	0.100
Panel C: Duration Lower House					
Coefficient (ATT)	2.379***	0.946***	0.852***	0.323**	0.565**
SE (ATT)	(0.492)	(0.238)	(0.182)	(0.162)	(0.191)
Coefficient (ITT)	2.490***	1.047***	0.898***	0.399	0.565**
SE (ITT)	(0.491)	(0.236)	(0.181)	(0.159)	(0.191)
Mean DV Treated	7.139	7.955	8.319	8.399	8.580
Mean DV Control	3.687	7.369	7.785	8.293	8.241
Effective N (Treated)	126	102	53	49	36
Effective N (Control)	240	60	51	32	21
Bandwidth	0.100	0.100	0.100	0.100	0.100
Panel D: Duration Any Other Politics					
Coefficient (ATT)	-1.144*	-0.205	3.899***	0.056	-1.763
SE (ATT)	(0.690)	(1.051)	(1.120)	(1.331)	(1.593)
Coefficient (ITT)	-1.318*	0.151	4.033***	-0.031	-1.763
SE (ITT)	(0.660)	(1.034)	(1.111)	(1.329)	(1.593)
Mean DV Treated	3.552	3.531	3.804	3.793	2.088
Mean DV Control	2.787	3.308	2.316	4.173	7.173
Effective N (Treated)	126	102	53	49	36
Effective N (Control)	240	60	51	32	21
Bandwidth	0.100	0.100	0.100	0.100	0.100

Dynamic Regression Discontinuity (RD) estimates of winning an election for the τ-th term on four career path outcomes. Dependent Variables: entry into Provincial Politics (Panel A), entry into Law (Panel B), total lifetime years in the Lower House (Panel C), and total lifetime years in Other Politics (Panel D). 'ITT' is the raw RD estimate; 'ATT' is the ceteris paribus effect of the τ-th term, correcting for future incumbency effects. Estimates are from a local linear RD (bandwidth = 0.10) with party and district fixed effects. 'Effective N' is the sample size within the bandwidth. Standard errors for ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.29: Comparison of Candidate Characteristics: Full Sample vs. Wealth Subsample

Variable	Full Sample	Wealth Subsample	p-value
Personal Characteristics			
Party: Liberal	0.42	0.53	0.000***
Party: Protestant	0.32	0.27	0.005**
Party: Catholic	0.07	0.07	0.763
Party: Socialist	0.04	0.03	0.090*
Year of Birth Candidate	1831.85	1832.91	0.162
Dummy Upper House Before	0.04	0.03	0.147
Provincial Politics Before	0.29	0.32	0.156
Local Politics Before	0.15	0.18	0.051*
Dummy Law Before	0.43	0.48	0.018**
Dummy Business Before	0.02	0.02	0.777
Election Characteristics			
Election Year	1881.93	1882.70	0.245
Log(Turnout)	7.82	7.87	0.056*
Log (Electoral Threshold)	7.29	7.28	0.768
Log(Electorate Size)	8.32	8.32	0.814
District Characteristics			
District Population	11.64	11.67	0.481
% Labor Force Industry District	0.40	0.40	0.670
% Labor Force Agriculture District	0.14	0.14	0.161
% Labor Force Services District	0.46	0.47	0.290
% Paying Wealth Tax District	7.10	7.10	0.994
Income Tax Share District	8.15	8.16	0.836
% Catholic District	0.30	0.31	0.685
Birthplace Characteristics			
% Protestant District	0.64	0.64	0.768
% Labor Force Industry Birth Place	0.36	0.35	0.559
% Labor Force Agriculture Birth Place	0.07	0.06	0.022**
% Labor Force Services Birth Place	0.57	0.58	0.094*
% Catholic Birth Place	0.38	0.37	0.238
% Protestant Birth Place	0.60	0.61	0.231
Distance to The Hague - BP	73.63	70.73	0.242

The Table reports the results of a linear regression model estimating the conditional mean difference between the Full Sample and the Wealth Available subsample for a host of characteristics. The reported numbers are the mean for each variable in each respective sample. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Table C.30: Estimates of the Returns to Politics

Round τ	1	2	3	4	5
Panel A: Entry Provincial Politics					
Coefficient (ATT)	-0.326***	-0.295***	0.413***	-0.697***	-0.274
SE (ATT)	(0.082)	(0.107)	(0.127)	(0.126)	(0.182)
Coefficient (ITT)	-0.299**	-0.201	0.359***	-0.723***	-0.274
SE (ITT)	(0.079)	(0.105)	(0.126)	(0.125)	(0.182)
Mean DV Treated	-0.190	-0.161	-0.107	-0.350	-0.467
Mean DV Control	0.024	-0.219	-0.438	-0.167	0.143
Effective N (Treated)	191	173	106	98	73
Effective N (Control)	433	98	71	48	26
Bandwidth	0.200	0.200	0.200	0.200	0.200
Panel B: Entry Law					
Coefficient (ATT)	0.216***	-0.105	0.184	0.091	-0.030
SE (ATT)	(0.060)	(0.098)	(0.116)	(0.133)	(0.206)
Coefficient (ITT)	0.192*	-0.089	0.194	0.088	-0.030
SE (ITT)	(0.056)	(0.095)	(0.114)	(0.132)	(0.206)
Mean DV Treated	0.310	0.446	0.429	0.400	0.467
Mean DV Control	0.210	0.375	0.438	0.500	0.429
Effective N (Treated)	191	173	106	98	73
Effective N (Control)	433	98	71	48	26
Bandwidth	0.200	0.200	0.200	0.200	0.200
Panel C: Duration Lower House					
Coefficient (ATT)	3.481***	0.460***	0.255*	0.377**	0.336***
SE (ATT)	(0.386)	(0.175)	(0.144)	(0.158)	(0.133)
Coefficient (ITT)	3.553***	0.506***	0.329***	0.435	0.336**
SE (ITT)	(0.385)	(0.174)	(0.141)	(0.156)	(0.133)
Mean DV Treated	7.139	7.955	8.319	8.399	8.580
Mean DV Control	3.687	7.369	7.785	8.293	8.241
Effective N (Treated)	191	173	106	98	73
Effective N (Control)	433	98	71	48	26
Bandwidth	0.200	0.200	0.200	0.200	0.200
Panel D: Duration Any Other Politics					
Coefficient (ATT)	-1.290**	-0.903	1.776**	1.506	-1.640
SE (ATT)	(0.531)	(0.761)	(0.846)	(1.070)	(1.460)
Coefficient (ITT)	-1.471**	-0.670	1.996**	1.349	-1.640
SE (ITT)	(0.508)	(0.746)	(0.837)	(1.061)	(1.460)
Mean DV Treated	3.552	3.531	3.804	3.793	2.088
Mean DV Control	2.787	3.308	2.316	4.173	7.173
Effective N (Treated)	191	173	106	98	73
Effective N (Control)	433	98	71	48	26
Bandwidth	0.200	0.200	0.200	0.200	0.200

Dynamic Regression Discontinuity (RD) estimates of winning an election for the τ-th term on four career path outcomes. Dependent Variables: entry into Provincial Politics (Panel A), entry into Law (Panel B), total lifetime years in the Lower House (Panel C), and total lifetime years in Other Politics (Panel D). 'ITT' is the raw RD estimate; 'ATT' is the ceteris paribus effect of the τ-th term, correcting for future incumbency effects. Estimates are from a local linear RD (bandwidth = 0.10) with party and district fixed effects. 'Effective N' is the sample size within the bandwidth. Standard errors for ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.31: Heterogeneity in Provincial Career Path by Lawyer Status

Round τ	1	2	3	4
Panel A: No Lawyer Past				
Coefficient (ATT)	-0.087	0.040	0.791***	1.251***
SE (ATT)	(0.132)	(0.157)	(0.222)	(0.188)
Coefficient (ITT)	-0.230	-0.068*	0.839***	1.251***
SE (ITT)	(0.129)	(0.155)	(0.222)	(0.188)
Mean DV Treated	-0.125	-0.062	-0.067	-0.250
Mean DV Control	0.032	-0.263	-0.444	0.167
Effective N (Treated)	87	61	32	25
Effective N (Control)	185	37	30	16
Bandwidth	0.100	0.100	0.100	0.100
Panel B: Lawyer Past				
Coefficient (ATT)	-0.834***	0.017	0.765***	-0.139
SE (ATT)	(0.140)	(0.178)	(0.235)	(0.158)
Coefficient (ITT)	-0.848***	0.055	0.759***	-0.139
SE (ITT)	(0.139)	(0.178)	(0.234)	(0.158)
Mean DV Treated	-0.333	-0.280	-0.143	-0.500
Mean DV Control	0.000	-0.154	-0.400	-0.500
Effective N (Treated)	39	43	22	26
Effective N (Control)	55	23	22	16
Bandwidth	0.100	0.100	0.100	0.100

Dynamic Regression Discontinuity (RD) estimates of the effect on a provincial politics career path of serving additional terms in the Lower House. The dependent variable is entry into Provincial Politics. 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=4). Estimates are from a local linear RD with a fixed bandwidth of 0.10, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.32: Heterogeneity by District

Round τ	1	2	3	4
Panel A: Major Commercial Hub Districts				
Coefficient (ATT)	1.748**	1.769	-0.054	-0.967
SE (ATT)	(0.839)	(1.148)	(0.672)	(2.319)
Coefficient (ITT)	1.627	1.706	-0.007	-0.967
SE (ITT)	(0.833)	(1.137)	(0.662)	(2.319)
Mean DV Treated	12.431	11.664	12.379	9.928
Mean DV Control	11.363	11.070	11.951	12.615
Effective N (Treated)	21	23	18	15
Effective N (Control)	50	16	11	11
Bandwidth	0.150	0.150	0.150	0.150
Panel B: All Other Districts				
Coefficient (ATT)	-0.193	0.872	0.054	-0.469
SE (ATT)	(0.537)	(0.691)	(0.901)	(0.613)
Coefficient (ITT)	-0.004	0.901**	-0.022	-0.469
SE (ITT)	(0.520)	(0.674)	(0.895)	(0.613)
Mean DV Treated	11.734	11.583	11.324	11.872
Mean DV Control	11.310	10.584	11.940	12.325
Effective N (Treated)	59	62	31	27
Effective N (Control)	110	37	27	20
Bandwidth	0.150	0.150	0.150	0.150

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The analysis is split up between major and commercial hubs, based on the regional dispersion of judicial salaries. The dependent variable is Personal Wealth. 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=4). Estimates are from a local linear RD with a fixed bandwidth of 0.10, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.33: Comparison of Candidate Characteristics: Incumbent vs. Non-Incumbent Party

Variable	Non-Incumbent	Incumbent	p-value
Personal Characteristics			
Party: Liberal	0.34	0.54	0.000***
Party: Protestant	0.27	0.40	0.000***
Party: Catholic	0.07	0.06	0.482
Party: Socialist	0.07	0.00	0.000***
Year of Birth Candidate	1832.67	1830.83	0.088*
Dummy Upper House Before	0.04	0.04	0.452
Provincial Politics Before	0.25	0.35	0.000***
Local Politics Before	0.13	0.17	0.062*
Dummy Law Before	0.38	0.51	0.000***
Dummy Business Before	0.02	0.03	0.145
Election Characteristics			
Election Year	1882.69	1880.80	0.034**
Log(Turnout)	7.83	7.80	0.383
Log (Electoral Threshold)	7.32	7.24	0.020**
Log(Electorate Size)	8.35	8.28	0.035**
District Characteristics			
District Population	11.61	11.69	0.050*
% Labor Force Industry District	0.40	0.40	0.358
% Labor Force Agriculture District	0.15	0.14	0.031**
% Labor Force Services District	0.45	0.47	0.080*
% Paying Wealth Tax District	7.15	7.02	0.134
Income Tax Share District	8.18	8.10	0.155
% Catholic District	0.30	0.31	0.625
Birthplace Characteristics			
% Protestant District	0.64	0.64	0.631
% Labor Force Industry Birth Place	0.35	0.36	0.393
% Labor Force Agriculture Birth Place	0.07	0.07	0.936
% Labor Force Services Birth Place	0.57	0.57	0.641
% Catholic Birth Place	0.38	0.39	0.532
% Protestant Birth Place	0.60	0.60	0.626
Distance to The Hague - BP	74.13	73.03	0.719

The Table reports the results of a linear regression model estimating the conditional mean difference between Non-Incumbent Parties and Incumbent Parties. The sample is restricted to elections with a margin of less than 0.1. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Table C.34: Estimates of the Returns to Politics

Round τ	1	2	3	4	5
Panel A: Upper House (Duration)					
Coefficient (ATT)	-0.900*	1.308	0.876	1.129	-0.520
SE (ATT)	(0.501)	(0.859)	(1.088)	(1.330)	(2.105)
Coefficient (ITT)	-0.645**	1.545	1.062	1.031	-0.520
SE (ITT)	(0.469)	(0.829)	(1.053)	(1.270)	(2.105)
Mean DV Treated	0.770	1.845	2.117	2.022	1.107
Mean DV Control	0.962	0.946	1.083	2.449	2.437
Effective N (Treated)	126	102	53	49	36
Effective N (Control)	240	60	51	32	21
Bandwidth	0.100	0.100	0.100	0.100	0.100
Panel B: Upper House (Dummy)					
Coefficient (ATT)	-0.108*	0.145	0.124	0.020	-0.063
SE (ATT)	(0.063)	(0.096)	(0.153)	(0.189)	(0.228)
Coefficient (ITT)	-0.075**	0.172*	0.124	0.008	-0.063
SE (ITT)	(0.060)	(0.091)	(0.148)	(0.184)	(0.228)
Mean DV Treated	0.103	0.232	0.286	0.250	0.133
Mean DV Control	0.121	0.062	0.156	0.333	0.286
Effective N (Treated)	126	102	53	49	36
Effective N (Control)	240	60	51	32	21
Bandwidth	0.100	0.100	0.100	0.100	0.100
Panel C: Minister					
Coefficient (ATT)	-0.900*	1.308	0.876	1.129	-0.520
SE (ATT)	(0.501)	(0.859)	(1.088)	(1.330)	(2.105)
Coefficient (ITT)	-0.645**	1.545	1.062	1.031	-0.520
SE (ITT)	(0.469)	(0.829)	(1.053)	(1.270)	(2.105)
Mean DV Treated	0.770	1.845	2.117	2.022	1.107
Mean DV Control	0.962	0.946	1.083	2.449	2.437
Effective N (Treated)	126	102	53	49	36
Effective N (Control)	240	60	51	32	21
Bandwidth	0.100	0.100	0.100	0.100	0.100
Panel D: Provincial Politics					
Coefficient (ATT)	-0.216**	-0.399***	0.790***	0.125	-0.863***
SE (ATT)	(0.107)	(0.141)	(0.166)	(0.136)	(0.219)
Coefficient (ITT)	-0.268	-0.240**	0.851***	0.034	-0.863***
SE (ITT)	(0.104)	(0.138)	(0.165)	(0.134)	(0.219)
Mean DV Treated	-0.190	-0.161	-0.107	-0.350	-0.467
Mean DV Control	0.024	-0.219	-0.438	-0.167	0.143
Effective N (Treated)	126	102	53	49	36
Effective N (Control)	240	60	51	32	21
Bandwidth	0.100	0.100	0.100	0.100	0.100
Panel E: City Politics					
Coefficient (ATT)	-0.064	0.039	0.551***	-0.090	0.709**
SE (ATT)	(0.099)	(0.129)	(0.122)	(0.123)	(0.125)
Coefficient (ITT)	-0.120	0.035	0.502***	-0.015	0.709**
SE (ITT)	(0.097)	(0.128)	(0.121)	(0.122)	(0.125)
Mean DV Treated	-0.103	-0.071	-0.107	-0.200	-0.200
Mean DV Control	-0.008	-0.094	-0.125	-0.167	-0.286
Effective N (Treated)	126	102	53	49	36
Effective N (Control)	240	60	51	32	21
Bandwidth	0.100	0.100	0.100	0.100	0.100
Panel F: Non-Political Careers					
Coefficient (ATT)	0.163**	0.006	0.413***	0.398**	-0.457**
SE (ATT)	(0.082)	(0.147)	(0.145)	(0.171)	(0.208)
Coefficient (ITT)	0.127	0.071	0.480**	0.350**	-0.457**
SE (ITT)	(0.077)	(0.144)	(0.144)	(0.170)	(0.208)
Mean DV Treated	-0.379	-0.429	-0.429	-0.450	-0.533
Mean DV Control	-0.234	-0.406	-0.500	-0.583	-0.429
Effective N (Treated)	126	102	53	49	36
Effective N (Control)	240	60	51	32	21
Bandwidth	0.100	0.100	0.100	0.100	0.100

Dynamic Regression Discontinuity (RD) estimates of winning an election for the τ-th term on four career path outcomes. Dependent Variables: entry into Provincial Politics (Panel A), years in Other Politics (Panel B), entry into Law (Panel C), and total lifetime years in the Lower House (Panel D, duration). 'ITT' is the raw RD estimate; 'ATT' is the ceteris paribus effect of the τ-th term, correcting for future incumbency effects. Estimates are from a local linear RD (bandwidth = 0.10) with party and district fixed effects. 'Effective N' is the sample size within the bandwidth. Standard errors for ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.35: Heterogeneity by Lifespan

Round τ	1	2	3	4
Panel A: Short Lifespan				
Coefficient (ATT)	0.372	2.379**	1.937*	-1.508
SE (ATT)	(0.939)	(1.009)	(1.003)	(2.327)
Coefficient (ITT)	0.690	2.513**	1.812	-1.508
SE (ITT)	(0.926)	(1.005)	(0.985)	(2.327)
Mean DV Treated	11.811	11.906	12.463	11.220
Mean DV Control	11.397	10.502	11.639	11.985
Effective N (Treated)	37	29	15	15
Effective N (Control)	49	21	18	13
Bandwidth	0.100	0.100	0.100	0.100
Panel B: Long Lifespan				
Coefficient (ATT)	-0.026	0.255	-1.683	-1.188
SE (ATT)	(0.864)	(1.079)	(1.270)	(0.806)
Coefficient (ITT)	0.227	0.056	-1.868	-1.188
SE (ITT)	(0.840)	(1.060)	(1.263)	(0.806)
Mean DV Treated	11.951	11.319	11.075	11.849
Mean DV Control	11.293	11.290	12.217	12.784
Effective N (Treated)	31	31	17	16
Effective N (Control)	66	17	15	13
Bandwidth	0.100	0.100	0.100	0.100

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The analysis is split up between observations with a long and short lifespan, based on the median. The dependent variable is Personal Wealth. 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=4). Estimates are from a local linear RD with a fixed bandwidth of 0.10, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.36: Heterogeneity by Turnout

Round τ	1	2	3	4
Panel A: Low Turnout				
Coefficient (ATT)	-0.478	0.600	-1.556	-0.194
SE (ATT)	(1.333)	(1.956)	(1.622)	(1.571)
Coefficient (ITT)	-0.492	0.513	-1.566	-0.194
SE (ITT)	(1.328)	(1.954)	(1.620)	(1.571)
Mean DV Treated	11.896	11.383	11.038	11.655
Mean DV Control	11.041	11.111	12.129	12.057
Effective N (Treated)	18	20	11	17
Effective N (Control)	38	21	13	9
Bandwidth	0.100	0.100	0.100	0.100
Panel B: High Turnout				
Coefficient (ATT)	-0.181	1.142	1.273	-2.915**
SE (ATT)	(0.735)	(1.090)	(1.007)	(0.872)
Coefficient (ITT)	0.362	1.529	0.679	-2.915**
SE (ITT)	(0.691)	(1.070)	(0.992)	(0.872)
Mean DV Treated	11.914	11.814	12.024	11.203
Mean DV Control	11.498	10.970	11.933	12.940
Effective N (Treated)	36	26	18	10
Effective N (Control)	63	10	14	12
Bandwidth	0.100	0.100	0.100	0.100

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The analysis is split up between observations with a low and high turnout, based on the median. The dependent variable is Personal Wealth. 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=4). Estimates are from a local linear RD with a fixed bandwidth of 0.10, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.37: Heterogeneity by Literacy Rate

Round τ	1	2	3	4
Panel A: Low Turnout				
Coefficient (ATT)	-0.078	1.589	-0.423	-1.109
SE (ATT)	(0.834)	(1.425)	(1.079)	(1.259)
Coefficient (ITT)	0.280*	1.579	-0.571	-1.109
SE (ITT)	(0.797)	(1.417)	(1.066)	(1.259)
Mean DV Treated	11.387	12.006	11.731	12.067
Mean DV Control	10.945	10.559	12.154	12.909
Effective N (Treated)	21	19	11	12
Effective N (Control)	38	14	11	8
Bandwidth	0.100	0.100	0.100	0.100
Panel B: High Turnout				
Coefficient (ATT)	0.729	1.309	1.014	0.803
SE (ATT)	(0.984)	(0.989)	(0.746)	(0.968)
Coefficient (ITT)	0.744	1.334	1.038	0.803
SE (ITT)	(0.983)	(0.988)	(0.745)	(0.968)
Mean DV Treated	12.142	11.528	12.287	12.149
Mean DV Control	11.136	10.981	11.367	11.372
Effective N (Treated)	32	28	16	17
Effective N (Control)	50	19	14	13
Bandwidth	0.100	0.100	0.100	0.100

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The analysis is split up between districts with a low and high literacy rates, based on the median. The dependent variable is Personal Wealth. 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=4). Estimates are from a local linear RD with a fixed bandwidth of 0.10, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.38: Heterogeneity by Period

Round τ	1	2	3	4
Panel A: Before Party Formation				
Coefficient (ATT)	1.072	0.887	-0.290	-3.062
SE (ATT)	(0.981)	(1.482)	(1.973)	(0.975)
Coefficient (ITT)	1.200	0.872	-0.606	-3.062
SE (ITT)	(0.969)	(1.468)	(1.971)	(0.975)
Mean DV Treated	12.179	11.453	11.187	11.694
Mean DV Control	10.870	10.792	12.034	13.251
Effective N (Treated)	27	20	12	13
Effective N (Control)	58	16	12	9
Bandwidth	0.100	0.100	0.100	0.100
Panel B: After Party Formation				
Coefficient (ATT)	-0.374	1.385	0.279	-0.455
SE (ATT)	(0.747)	(0.989)	(0.964)	(1.223)
Coefficient (ITT)	-0.106	1.464	0.202	-0.455
SE (ITT)	(0.710)	(0.972)	(0.941)	(1.223)
Mean DV Treated	11.712	11.725	11.780	11.703
Mean DV Control	11.682	10.809	11.725	11.765
Effective N (Treated)	41	40	20	18
Effective N (Control)	59	22	21	17
Bandwidth	0.100	0.100	0.100	0.100

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The analysis is split up between early and late periods, based on the median year of election in the sample. The dependent variable is Personal Wealth. 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=4). Estimates are from a local linear RD with a fixed bandwidth of 0.10, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table C.39: Heterogeneity by Party

Round τ	1	2	3	4
Panel A: Confessional Politicians				
Coefficient (ATT)	-0.438	1.023	-0.232	-1.118
SE (ATT)	(0.801)	(1.481)	(1.051)	(1.086)
Coefficient (ITT)	-0.173	1.078	-0.302	-1.118
SE (ITT)	(0.772)	(1.478)	(1.049)	(1.086)
Mean DV Treated	11.506	11.877	12.196	12.612
Mean DV Control	11.402	10.974	12.075	12.820
Effective N (Treated)	21	20	11	7
Effective N (Control)	78	18	10	12
Bandwidth	0.100	0.100	0.100	0.100
Panel B: Liberal Politicians				
Coefficient (ATT)	0.836	1.563	0.153	-0.734
SE (ATT)	(0.757)	(0.984)	(1.176)	(1.243)
Coefficient (ITT)	1.111	1.548	0.029	-0.734
SE (ITT)	(0.735)	(0.961)	(1.157)	(1.243)
Mean DV Treated	12.216	11.601	11.352	11.219
Mean DV Control	11.166	10.695	11.715	11.813
Effective N (Treated)	44	38	20	24
Effective N (Control)	85	19	23	14
Bandwidth	0.100	0.100	0.100	0.100

Dynamic Regression Discontinuity (RD) estimates of the financial returns to serving additional terms in the Lower House. The analysis is split up between two parties (with sufficient observations). The dependent variable is Personal Wealth. 'ITT' (Intent-to-Treat) is the raw RD estimate at the τ-th election. 'ATT' (Average Treatment Effect on the Treated) is the ceteris paribus effect of the τ-th term, correcting for future incumbency advantages (following (Cellini et al., 2010), assuming t*=4). Estimates are from a local linear RD with a fixed bandwidth of 0.10, conditional on party, decade, and district fixed effects. Standard errors for the ATT are calculated via the delta method. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

D Replication Package and Data Appendix

D.1 Replication Package

This paper is accompanied by a replication package which is hosted on a Github repository, accessible through `https://github.com/basm92/retpol_new`, and also available on the Harvard dataverse (`https://doi.org/CHANGETHIS`). The replication package contains a README file with several instructions pertaining to the steps that need to be undertaken to replicate the findings presented in this paper. It contains the final dataset, under the directory `data/analysis/dataset_final.csv`. Notably, it also contains the code that achieved the data wrangling to arrive at the final dataset used in the paper.

The replication package contains all files needed to replicate the paper with the exception of one file (also detailed in the README document on Github/Dataverse), which is the HDNG database. The 2021 version of the HDNG database, available under a persistent identifier here, is used for this paper. In order for the replication package to function, the user needs to place the 'HDNG_v4.txt' file in the '~/data/hdng' folder, where ~ represents the directory into which the replication package is forked/downloaded (the working directory). In the root folder on the replication package repository (and on the Dataverse repository), there is code that accomplishes this ('download_necessary_data.R').

This replication package can serve two purposes: replication of the analysis on the basis of the assembled dataset. This is detailed in the README on the repository. The second purpose is to replicate the data collection and data wrangling process. The remainder of this manual is about this. It is structured in several steps, representing the way to proceed from the primary sources to the data set. In this manual, I describe this process in detail, and in tandem to the data collection process. The code follows the same structure as the text below: each step is saved in a different '.R' file.

Step 0: Scrape Elections: I start out with a family of URL's pertaining to the election data from the *Repositorium Tweede Kamerverkiezingen* (Repository of Lower House Elections). Each election is represented by a unique ID in a URL.[28] I then scrape the table on each respective page, containing the individual-level data candidate name, count of votes, percentage of votes, and newspaper recommendation (if any), and the election-level data electorate size, turnout, electoral threshold, number of seats, type of election, date of election and district name. The resulting data is defined on the individual level and saved as election_results_details_new.csv in ~ `/data/interim_data`. The file should have 8563 rows and 13 columns. In this script, I also scrape a Wikipedia page pertaining to the affiliation of all subsequent governments in the period of interest. This file is saved as government_affiliation.csv in data/election_data.

Step 1: Calculate Elections: In step 1, I first parse the aforementioned datasets and convert variables. I also solve a problem with the encoding of the candidate names, pertaining to several accents and non-standard Latin alphabet characters. Then, I use the list of elected

[28] URLs starting with this link, followed by an identifier (a number from 10 to about 2000).

individuals, also from the Repository, to find who wins which election, and on that bases, to calculate the margin for each candidate as defined in the paper. Finally, I also generate a broad margin, for losing candidates in the first round in elections that were only decided in the second round. I save this file as elections_with_margins.csv in `~data/interim_data`.

Step 2: Add Wealth Data In this step, I take the output of step 1 and add the wealth data from the *Memories van Successie* as defined in the main text, with the help of a hand made key mapping the candidate names to the identifiers used in the wealth dataset. I export this dataset to data/interim_data as elections_with_wealth.csv.

Step 3: Deflate Wealth Data: In this step, I deflate the wealth data using the CPI coming from Jordà et al. (2019). In addition, I augment the politician-data with data from the *Politiek Documentatiecentrum* to add information pertaining to the birth and death date of politicians. The same information was already present for non-politicians because they were contained in the same primary dataset in step 2. Adding death dates is required before deflating nominal net wealth because deflating requires knowing the year of death, i.e. the year in which the nominal net wealth coming from the probate inventories was registered. The resulting file is again saved in data/interim_data under elections_wealth_defl_birthdeath.csv.

Step 4: Add Election History: In this step, I proceed to create three variables for each candidate-election pair pertaining to the election history of that candidate: the number of tries until now, the number of wins until now and the number of tries since the last win. The resulting dataframe is exported to interim_data as elections_with_hisory.csv.

Step 5: Add Career Variables: In this script, I add variables mapping out the career of candidates. In particular, I generate a class of dummies, pertaining to whether candidate i ever becomes x after election j, where $x \subset \{$ Upper House, Minister, Provincial Deputy, Provincial Executive, Mayor, Alderman, Municipal Counciller, Businessperson, Lawyer, Judge, Landowner $\}$. Then, I create a similar set of dummies for whether a candidate has been any x before election j. I also add duration variables, counting the total duration spent in each of these functions. This way, I can track career switches, or control for potential path dependencies in career choices. I export the resulting file as elections_with_careers.csv in data/interim_data.

Step 6: Add District Characteristics: In this step, I add various district-level variables to the dataset. In particular, I augment the dataset by various variables coming from the *Historische Database van Nederlandse Gemeenten* (HDNG, Historical Database of Dutch Municipalities): labor force decomposition (% of labor force working in industry, services and agriculture, coming from professional censuses), district tax revenues, particular, the percentage and promillage of individuals paying wealth tax and income tax respectively. Using the *Historische Sample van Nederland* (HSN, Historical Sample of the Netherlands), I also construct a proxy for the district-level literacy rates by weightin municipality-specific proxies for the literacy rate. Finally, using the Dutch censuses, the HDNG also contains information about the religious decomposition of a district, for which I measure the per-

centage of *Hervormd* and *Gereformeerd* Protestants,[29] and Catholics. Finally, I include the Euclidian distance to the Hague from each district centroid. The output is saved as /elections_with_district_data.csv in the ***sim***data/interim_data folder. The file should have 8519 observations and 99 columns by now.

Step 7: Add Birthplace Characteristics: In step 7, I again use the HDNG to add several birthplace characteristics. In particular, I extract the professional composition, the religious decomposition, and the distance to The Hague measured from the birthplace centroid. The file is expoted as elections_with_birthplace_characteristics.csv

Step 8: Add Party Affiliation: In this step, I leverage the data from the *Politiek Documentatiecentrum* (PDC, Politics Documentation Center) to add two party classifications to the dataset: 1 simple and 1 more granular. The simple classification makes no distinction between Protestant and Catholic parties (under one moniker of "confessional"), whereas the granular classification does. The classification is derived from a heterogeneous party classification constructed by experts of Dutch 19th century political history. I use a mapping to convert this very heterogeneous classification to a mapping involving Protestant, Catholic, Liberal, Socialist, and another involving Confessional, Liberal, Socialist. The dataset is saved in interim_data as elections_with_party_affiliation.csv.

Step 9: Add Electoral Characteristics Person: In step 9, I leverage the elections dataset again to recover some variables describing candidate-election level variables for the current election candidate i participates in, and also, if available, the preceding election candidate i participates in. I collect: the turnout (already there) in election and the vote share,[30] a dummy socialist indicating a socialist candidate participated in the election in which candidate i also participated, the percentage socialist vote, a Herfindahl-Hirschmann index of votes and the number of candidates participating. The data is saved in data/interim_data as elections_with_electoral_characteristics.csv. It should have 139 columns and 8519 rows.

Step 10: Add Parental Wealth: In this step, for a small subsample of available candidates, I collect parental wealth, defined as average inheritance of both parents if available, otherwise, the inheritance of the available parent, dividing by the number of siblings + 1. This data is added to the dataset and exported as elections_with_parental_wealth.csv in interim_data.

Step 11: Add Lifespan And Misc.: In this step, I compute the lifespan of an individual measured from election j in years. I also compute the wealth per unit of lifespan, and I compute the age at election. In addition, I expand some categorical variables, such as the party classification to dummies to incorporate them in descriptive statistics more easily. I also create a couple of variables used in robustness checks and heterogeneity analysis, such as variables indicating next election participation or newspaper recommendation. I also add incumbent, indicating whether your party is, or will be, incumbent in the next (current) parliament. Finally, to filter out potential erroneous matches, I filter out observations for

[29] The two most numerous Protestant denominations.

[30] These variables were already there but are needed in the definition of other variables.

which the age of election is lower than 20. The final product of this contains 6679 rows and 145 columns and is saved in interim_data in the file elections_with_lifespan_and_misc.csv.

Step 12: Add Incumbency Advantages: In this final step, I add the data required to compute the incumbency advantages. These variables are only used in the dynamic analyses to estimate incumbency advantages, and correspondingly the ATT effects. I save this file in the folder `analysis` under the name final_dataset.csv.

D.2 Wealth Data

This study primarily relies on archival sources to collect probate inventories, *Memories van Successie* (MVS), to obtain a reliable measure of politicians' personal wealth (Bos, 1990). Probate inventories have many advantages: they provide a detailed appraisal of a politicians' wealth at the time of decease, and usually, also a detailed inventories consisting of their assets and liabilities, and a separate appraisal of each and every one of them. The completeness of the deceased's wealth had to be declared under oath, and regularly, the tax agency required descendants to file additional declarations of assets that were initially missing. This indicates that a significant amount of time was devoted to ensuring that an individual's full wealth served as the tax base.

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

Despite their apparent reliability, the MVS might also have several disadvantages. For one, it is possible that despite oversight, individuals are still able to hide assets in various ways. To the extent this happens systematically, this potentially biases the results, possibly introducing measurement error or selection bias, or making the estimates less efficient (Angrist and Pischke, 2008). If tax evasion is easier for wealthier individuals, however, this likely biases the results downward. In appendix B.1, I provide analyses showing this more formally. Secondly, the MVS provide an overview of an individual's assets at only one point in time, at the end of one's life. In view of life-cycle saving theories in finance, individuals might have various motives to systematically change consumption patterns, the composition

of their wealth, and anticipate bequests as they get older (Dynan et al., 2002).

Below is an example of one particular *Memorie van Successie* (figure C. 2). The particular example is a digitized version of the document, available at the website of the Utrecht Provincial Archive. The layout of a MVS is standardized. The first page, the front page, contains the last name and first name(s), and the place and date of death (top right). Afterwards, it contains various point relating to the administration, including the day at which the MVS was registered. It also contains references to various other administrative documents.

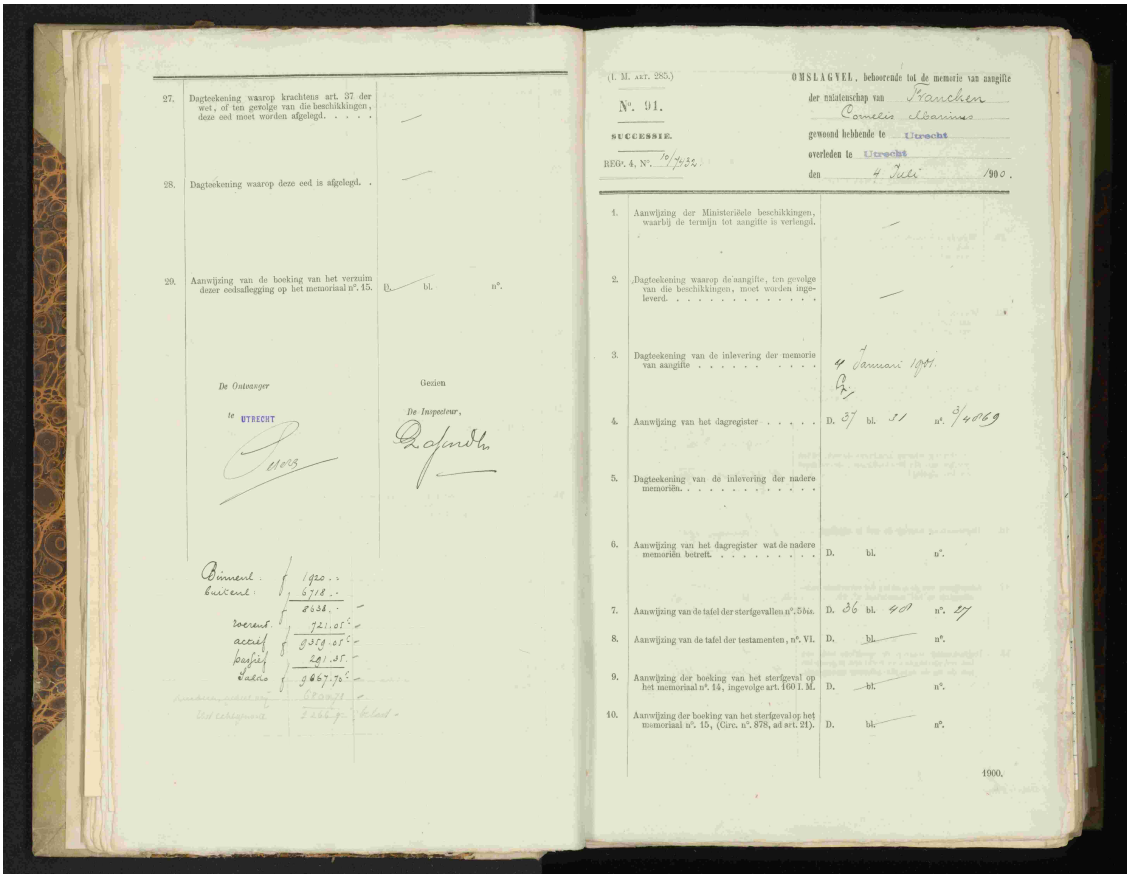

Figure C. 2: Front page of a MVS (on the right)

The second page of a MVS is depicted below (figure C. 3). The second page notably contains point 11. Point 11 is a resume of the remaining content of a MVS. Particularly, it contains the gross assets (*Baten*), gross liabilities (*Lasten*) and the net wealth (*Saldo*) of an individual at the time of death. Furthermore, point 12 contains the amount of the net wealth which is subject to taxation. Finally, again several metadata regarding several key dates in the administrative process of registering a MVS are given. Then, on the right page, an overview of an individual's assets and liabilities is given. First, the name and death date of the deceased is repeated, after which a recitation of the oath follows. Afterwards, an inventory of assets and liabilities is assembled. Each asset has a short description, followed

by a value. These values are added, first for all assets, then for all liabilities, and in the end, net wealth is obtained (not visible on this picture). Finally, on the basis of this net wealth, taxation is assembled. The MVS is closed by again providing several relevant references to other administrative sources, and a signature of the civil servant and the deceased's heirs (not visible on figure C. 3, but visible on figure C. 2 on the left).

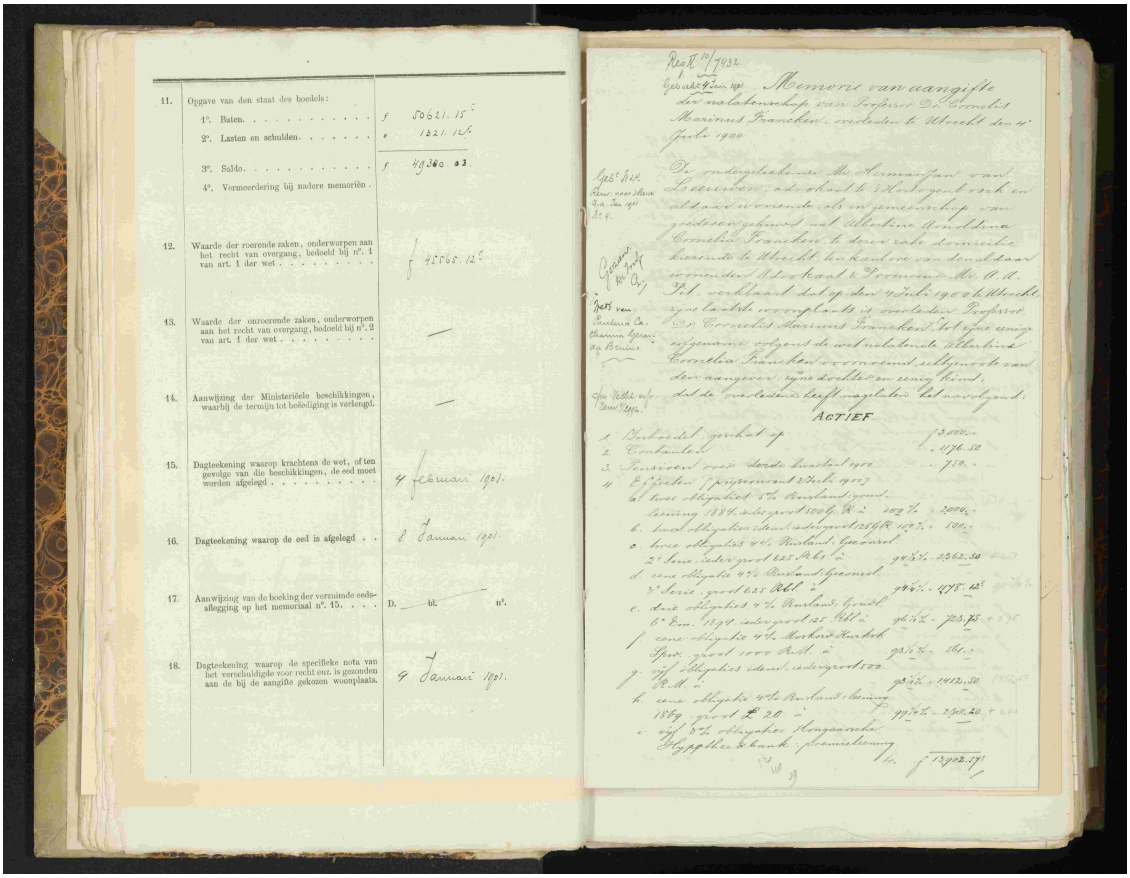

Figure C. 3: Second and further pages of a MVS

Although the MVS theoretically cover virtually the entire population, in practice, it is sometimes difficult to find specific individuals. Out of all active politicians who died within the period of archival accessibility, I have managed to find probate inventories for about 70% of them. In my opinion, missing observations occur principally because of two reasons. The law stipulates that individuals must file and register the MVS at the registration office managing the place of death. This principle is widely deviated from. For example, it is often difficult to find probate inventories of individuals who have died outside of the Netherlands, because there is no designated office. In addition, descendants of deceased individuals often do not file their declaration at the place of death, but rather, at the office close to the place in which they live, or with which they have a special cultural bonding. In this respect, biographical information about individuals to be found can help locate the likely place of the specific MVS.

The second reason why individuals might be difficult to find has to do with archival organization. Oftentimes, individuals' assets are transferred from generation to generation, leading the civil servants administering the probate inventories to use probate inventories from previously deceased parents to investigate the assets of the deceased children. These probate inventories are sometimes not put back, and hence, leaves open a range of possible locations for the parents' probate inventories. In practice, I believe that after having considered the place of death and possibly the place of bonding, it is generally not worth the risk of conducting more search activity for a probate inventory in potentially different archives and places.

D.3 All Other Data

PDC: The biographical archive of the *Politiek Documentatiecentrum* (Political Documentation Center) contains extensive data on members of parliament and government officials. It includes both personal information and details on their (personal) parliamentary activities. This digital archive now encompasses individuals who have played a role in national governance since 1796, such as members of parliament, government officials, members of the European Parliament, state councillors, members of the Audit Office, etc. The size, comprehensiveness, quality, independent composition, and timeliness of this archive make it a unique national and international resource. The data is available for scientific research and journalistic publications, subject to certain conditions. The data I use mainly concerns biographical data, as well as data on which districts politicians represented at different points in time. See here for a short introduction to the data source (Dutch).

HDNG: The Historische Database Nederlandse Gemeenten (Historical Database of Dutch Municipalities) is a repository containing many variables on a municipality-level over time. The information relevant to this paper is on population, professional and religious compositions, as well as on taxes. These are in turn derived from various primary sources. The database is available here.

Repository: The *Repositorium Tweede Kamerverkiezingen* (Repository Lower House Elections) is used to gather electoral data. The website is available here. This project aims to provide researchers with a comprehensive resource that serves as a reference tool and facilitates the analysis and interpretation of election outcomes. The publication consists of organized data for each electoral district and election, including details such as the type of election, size of the electorate, voter turnout, and the number of votes received by each candidate. Additionally, through newspaper research, the database contains the political affiliation of a candidate in the form of a newspaper recommendation.

HSN: The *Historische Steekproef Nederland* (Historical Sample of the Netherlands) is a database tracking about 85,000 Dutch individuals throughout their life history to study information such as marriage, religious affiliation, literacy, migration history, and social networks. The 77,000 individuals have been selected for representativeness. In this study, I use information from marriage records to find whether individuals are literate (signed their

marriage contract with a name or with a cross) and aggregate this to the district level to find a district-period specific literacy rate. The data is accessible here (after registration).