Dynamic Returns to Political Tenure*

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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 exhibit a distinct "gate-keeping" pattern: no financial gains are observed during the first period of political tenure, but substantial returns emerge during the second term. These findings emphasize that politicians elected for a 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 for in-office returns.

JEL Classifications: N14, D72, H71

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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 takes a dynamic perspective and explicitly sets out to derive estimates of the returns to each additional period of political activity, tracing out a marginal return curve to political activity. It uses the setting of the Netherlands from 1848-1917 and exploits the repeated allocation of Lower House membership to estimate the financial returns for each particular period of political office. I make use of close elections to establish the existence and magnitude of financial returns to politics using a dynamic regression discontinuity strategy

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

(Cellini et al., 2010). In the 19th century, Dutch elections were based on a district system (De Jong, 1999). In each district, a small number of candidates took part, but these elections were frequently hotly contested. The absence of term limits also made for a large number of candidates who ran for election a large number of times. This setting enables me to estimate the returns to subsequent periods of political activity, but also to tie the returns to politics to changing political institutions, e.g. suffrage extensions (De Jong, 2001), allowing me to focus on the role of monitoring in disciplining politicians' ability to extract rents (Barro, 1973; Ferejohn, 1986; Duggan and Martinelli, 2017). Moreover, I investigate the relation between financial returns and potential changes in candidates' career paths induced by Lower House membership: I exploit detailed data on the careers of candidates, concentrating on whether Lower House politics serves to facilitate the appearance of 'career politicians' and 'political careers' in the spirit of Mattozzi and Merlo (2008) and whether this is related to the financial returns to political office.

In tandem with several European nations, the Netherlands underwent significant transformations during the late 19th and early 20th centuries (Przeworski, 2009). Initially an absolute monarchy in the early 19th century, the country transitioned to constitutional monarchy and parliamentary oversight following liberal reforms in 1848 (Aerts, 2018). However, electoral institutions during this period were severely restricted—limited to males meeting specific tax payment criteria, despite nominal eligibility being unconstrained (Van Der Kolk et al., 2018). Over subsequent decades, campaigns by politicians and activists culminated in the achievement of universal suffrage. Simultaneously, and also in tandem with international developments, the era witnessed the emergence and ascent of political parties, a political press and a national political culture. As schisms between liberal and Christian parliamentary factions widened, politicians and politically aware citizens coalesced into electoral associations (*Kiesvereenigingen*), swiftly evolving into formal political parties (De Jong, 1999). The inaugural political party, the Anti-Revolutionary Party, was established in 1879, followed by its liberal counterpart, the Liberal Union, in 1885 (De Jong, 2001; Voerman, 1989). The amalgamation of Catholic electoral associations occurred somewhat later, in 1904. Preceding this period, candidates aligned with specific political agendas usually garnered support from newspapers (De Jong, 1999). These and other developments provide a laboratory to gain insight into whether and how changing institutions can impact the magnitude of the returns to politics: these changes and associated increases in monitoring of politicians might potentially restrain financial returns tied to politics (see e.g. Aidt and Franck, 2015, 2019; Becker and Hornung, 2020).

Methodologically, the dynamic regression discontinuity design leverages the repeated quasi-random treatment assignment stemming from close electoral outcomes. This design considers not only candidates elected for the first time but also those re-elected an equivalent number of times previously. To ascertain the validity of treatment assignment, I compile a substantial dataset encompassing comprehensive details regarding candidates' backgrounds, origins, political inclinations, demographics, and the characteristics of districts where closely contested elections occurred. This approach enables a reliable estimation of the returns as-

sociated with successive tenures in political office. However, interpreting the estimates is complicated by the presence of incumbency advantages (Lee, 2008). The estimated overall impact of being elected on personal wealth encompasses both an immediate (*ceteris paribus*) effect, and the incumbency advantages multiplied by subsequent *ceteris paribus* effects. Employing a methodology similar to that of Cellini et al. (2010), I derive iterative estimates of the *ceteris paribus* effects from the overall estimated effects across each political term, alongside the incumbency advantages. These derived estimates offer an interpretation akin to a 'marginal return curve' depicting the successive *ceteris paribus* benefits associated with holding multiple terms in political office.

The analysis shows that the financial returns to politics primarily manifest during the *second* term of office. Politicians securing their second mandate by a narrow margin exhibited significantly higher wealth by the end of their lives compared to politicians who won their first, but narrowly lost their second election. Compared to an untreated observation at the margin, this additional wealth amounted to about 100,000 guilders, a sum equivalent to eight times the salary of a cabinet minister. In per-year terms, this translated to an additional five percentage points in wealth accumulation per annum for winners of closely contested elections — an effect size close to the effect size found by Fisman et al. (2014) in contemporary India. These findings remain robust upon integrating covariates, exploring various parameter specifications, and enduring scrutiny through multiple placebo tests. As the analysis extends into returns to subsequent terms, the estimates lose statistical significance. The point estimates hover around zero in numerous instances, suggesting minimal or negligible returns associated with third or subsequent tenures in the Lower House. These patterns are robust to to a battery of robustness checks. The outcome aligns with the perspective positing that the possibility to engage rent-seeking might be subject to entry barriers, as there is no evidence for financial returns from the first period of political office, while also endorsing the notion that politicians engaging in rent-seeking behavior may amass rents exclusively during a single term. These results challenge theories and views implying a constant marginal return curve associated with political tenure (Persson and Tabellini, 2002; Caselli and Morelli, 2004; Baltrunaite, 2020; Bourveau et al., 2021). Instead, they underscore the dynamic nature of the benefits derived from political office, emphasizing a concentration of returns within one particular term rather than a uniform trajectory across successive tenures.

Afterwards, I explore potential mechanisms leading to this result. In particular, and in line with an extensive literature, I focus on 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). I employ fine-grained data on politicians' and candidates' subsequent career paths after having participated in elections. While I find that Lower House membership is sometimes a substitute for Upper House membership, this cannot explain the observed patterns of financial returns to politics.[2] I also find scant evidence that candidates accumulate

[2]The Upper House membership came without salary, only with a reimbursement of expenses, as detailed

earnings-relevant human capital in the Lower House and continue a potentially lucrative career in domains such as law and litigation. This implies that political careers did not hold substantial value for prospective employers, thereby undermining the plausibility of a human capital-based rationale of the financial returns to politics (Diermeier et al., 2005; Mattozzi and Merlo, 2008). Instead, I find suggestive evidence for in-office returns: the results are almost entirely driven by candidates whose party was currently in government, rather than in the opposition. I also identify that monitoring at the district level plays an important role in curbing politicians' self-interested behavior: as an example, I find that candidates elected in districts with a relatively low literacy rate experience higher returns to politics than those elected in districts with high literacy rates. These and similar explanations make it more likely that politicians' rent-seeking ability is determined by the surrounding institutions while in office. I also supplement this systematic evidence with more anecdotal evidence from the historical literature.

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 party formation. 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. Online Appendix A features a more extensive description of the historical background of the setting in this study. Online Appendix B analyzes formally the influence of selection bias in this study, and Online Appendix C is a data appendix and also contains instructions pertaining to the replication package, also available on the Harvard Dataverse and GitHub. Finally, I provide various robustness checks and supplementary analyses in the Online Appendix D which accompanies this paper.

2 Historical Background

2.1 Electoral Institutions

In the period 1848-1917, all elections to the Lower House were organized in the framework of a district system. Before 1848, the year in which constitutional reforms liberalized the electoral system and political institutions of the country, delegates to the Lower House were elected indirectly: the enfranchised electorate elected delegates to the Provincial Estates, which then elected delegates to the Lower House. Delegates to the upper house were elected in a similar way, and in contrast to the Lower House elections, the 1848 constitution left this system intact for the elections to the upper house, whereas the elections to the Lower House were subject to reform, effectively rendering them direct, and more democratic (Blok, 1987). From 1849 onward, Lower House elections took place biannually. Every two years, half of the seats were up for contest. In almost all cases, districts featured two seats, and in each

in Online Appendix A.1

election, one seat was up for election (De Jong, 1999). Thus, a Lower House member was elected for four years.

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

The elections themselves were determined following an absolute majority logic. When no candidate in the first round obtained an absolute majority, a second round would be organized, with the two candidates with the highest amount of votes (De Jong, 1999). Candidates would remain in office for a four year term, but a constitutional provision, which remained in force for the entire period, stipulated that members of parliament who would accept a second remunerated function in government lost membership by default. They could, however, stand for reelection (De Jong, 1999; Loots, 2004). Apart from untimely death of a Lower House member, this was the principal reason that some elections occurred at times other than the officially stipulated election moments. In addition, there was a population-dependent electoral threshold, and elections were nullified in case of insufficient turnout, irrespective of the outcome.

The precise mapping from municipality (the lowest-level administrative unit of the Netherlands) to district was stipulated in the electoral law (*Kieswet*), with the stated objective that each district, and consequently each representative, represented about 45,000 inhabitants (De Jong, 1999). Accordingly, after the constitutional revision in 1848, the Lower House had 68 seats, corresponding roughly to the representation of 45,000 inhabitants by each of those seats. In the meantime, however, population growth had taken off, making it more and more difficult to apply this rule. The lawmakers responded by increasing the number of seats, creating and changing the composition of districts: the number of Lower House seats increased from 68 to 86 in about 10 years. However, because of the stakes involved (issues related to gerrymandering), it became more and more difficult to agree upon a given composition, effectively delaying any reform to 1887, when it was fixed at 100. The constitutional revision in 1887 also implied that the Lower House members were elected at the same time, while keeping intact the 4-year term, and that there would be one district for one representative, implying the break-up of previously large districts into various smaller ones, e.g. Amsterdam or Rotterdam. At the same time, as the population continued to grow, the reallocation of districts became more difficult, and imbalances between districts become more and more salient. This particularly favored sparsely over densely populated districts. Even the electoral law reforms of 1896, which encompassed, among other reforms, a partition of the largest cities into various districts, effectively increasing their representation, could not change the imbalance that disfavored them (De Jong, 1999).

While in principle, candidacy was open to any male aged thirty or older throughout the period, suffrage rights were severely restricted. The 1848 Constitution left suffrage and

eligibility requirements to the electoral law *Kieswet*, which in turn stipulated that men who paid more taxes than a certain threshold, called a *census* (De Vries, 1971; De Haan, 2003). This census, in turn, was determined on a municipal level. In some municipalities, such as Amsterdam, where the population was relatively rich, the threshold was higher, and the censuses were generally coordinated to be such that about 1 in 3,000 individuals was enfranchised. Van Der Kolk et al. (2018) note that about 85,000 men on a population of over 2.5 million had the right to active suffrage for both upper and Lower Houses. The constitutional changes and changes in the electoral law in 1887 in effect encompassed a lowering of census requirements, which was the principal mechanism through which a larger share of the population was enfranchised (about 25% after 1887 according to Van Der Kolk et al. (2018)), although besides taxes, there were various other means of acquiring the right to vote. The changes in the electoral law in 1896 added many more grounds other than income as a criterion to be enfranchised, such as having a particular set of degrees, paying a certain amount of rent or having a savings account. De Jong (1999) notes that about 48,6% of all Dutch men aged 25 and over were enfranchised by 1900.

2.2 Party Landscape

Throughout the period from 1848 to 1917, the electoral system in the Netherlands after 1848 was centered on individual delegates, not political parties. Politicians were supposed to be independent, not least with respect to their own delegates, and to promote the common interests of the country (De Jong, 2001). Political parties were preceded by *Kiesvereenigingen*, electoral unions, of enfranchised individuals with (generally) the same political orientation, intending to coordinate their voting behavior. *Kiesvereenigingen* were a way to improve the dissemination of information and aggregate electoral preferences in a more effective way. A special role in information provision was taken up by national newspapers: the editorial boards of several large national newspapers with a clear ideological background regularly endorse candidates they thought reflected their politics best (De Jong, 1999).

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

De Jong, 1997). In Online Appendix A, I describe a more extensive historical background of 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 activity. According to the 1815 Constitution, Lower House members were entitled to receive a retribution of expenses amounting to 2500 guilders annually. This sum aimed to cover living costs in The Hague, along with reimbursements for travel expenses at a rate of 1.50 guilders per kilometer traveled (Elzinga, 1985). Comparing these figures to wage data provided by (Van Zanden, 1983) and (Van Riel, 2018) for various professions in the Netherlands spanning from 1819 to 1913, we find that this lump sum equated to approximately nine times the yearly wage of an average worker in 1850, or about 5 times the yearly wage of a mayor of a medium-sized town Provinciale Verslagen (1860).

Following amendments to the Constitution in 1848, aimed at enhancing political legitimacy, the lump sum was reduced to 2000 guilders per year, with travel reimbursements remaining at 1.50 guilders per kilometer traveled. By 1890, rising wages had reduced the compensation for politicians to around five times the average wage (Elzinga, 1985). By then, the salary of a politician was comparable to the income of an engineer, or about 2-5 times the income of a mayor of a medium-sized town (Polak, 1908). Subsequent adjustments after 1917 raised these figures again, with the annual compensation reaching 5000 guilders. However, the workers' wage had not doubled by this time but only increased by approximately 1.5 times, thus widening the disparity once more. Furthermore, starting from this period, members of parliament were provided with free public transportation for their travel needs, mitigating the necessity to seek accommodation in The Hague and reducing the disparity between politicians residing near and far from the city. Additionally, former Lower House members of parliament were entitled to a pension after the age of 60, receiving 100 guilders for each year of active service in parliament, with a maximum total pension capped at 2000 guilders (Van Welderen Rengers and Romeijn, 1916). In Online Appendix A, I describe the compensation for members of the Lower House, as well as the compensation for other representative and executive bodies in more detail. I also pay attention to the relative wages of Lower House members with the wages for other politicians and with the average wages.

3 Data and Sources

3.1 Electoral Data

The *Repositorium Tweede Kamerverkiezingen 1848-1917* (Repository Lower House Elections) encompasses comprehensive records of Dutch Lower House elections held between 1848 and 1917, a period characterized by district-level electoral organization. This dataset systematically documents crucial election details, including district demarcations, dates of

elections, and election types categorized as regular, intermediate, or second round elections. Additionally, it furnishes candidate particulars such as their names alongside the vote count secured, the total number of eligible voters in the respective district, voter turnout, and pertinent metadata such as the number of contested seats in the election, election type, and date. The focus of this paper is directed towards elections culminating in a definitive victor. A refined subset is created by excluding elections that did not immediately result in a clear winner, specifically, first rounds necessitating subsequent rounds or nullified elections failing to meet the electoral threshold. This refinement process identifies 2,858 distinct elections within the district system between 1848 and 1917.

3.2 Politician Data

I retrieve a proprietary dataset from the *Politiek Documentatiecentrum* (PDC)[3], a think-tank focused on Dutch politics. The data encompass various demographic variables related to a politicians' life, including their birth and death date and place, and detailed data about career paths they have undertaken over the course of their life, including a description, a start date and an end date for each job or activity they have undertaken. I use these data to match politicians to candidate-election pairs in the election data using a rule-based approach (Abramitzky et al., 2021) based on period of political activity and fuzzy string matching, and correct the results manually. In addition to election-candidate specific information, I also collect politician-election specific newspaper recommendations from the *Repositorium*. Local newspapers reported who would be the contestants in upcoming elections, which frequently went hand in hand with an endorsement by the editorial board of a particular candidate (Oud, 1997; De Jong, 1999).

3.3 Non-Politician Data

Similar to the politicians, i.e. candidates who were elected at least once, I also retrieve data for non-politicians, whose data are not collected by the PDC due to them never being elected into politics.[4] Hence, I make use of online genealogical sources, such as *genealogieonline.nl*, *Geni.com*, the historical newspaper search engine *Delpher*, and local provincial archives to identify the birth date and place and date and place of decease for non-politicians and *Wikipedia*. In addition, I trace and collect information on their career paths.

3.4 Personal Wealth

I use archival data from probate inventories that contain the personal wealth of candidates at time of decease from provincial archives, called the *Memories van Successie* (MVS). The

[3]Information about the PDC is accessible here

[4]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 the PDC.

MVS primarily contain documents specifying the appraisal of a deceased individual's assets and liabilities with the purpose of levying inheritance taxes (Bos, 1990). This source is generally regarded as a highly reliable source of individuals' net worth. Descendants had to declare under oath in court that the list of assets and liabilities they submitted was truthful (Moes, 2012). Several miscellaneous documents containing internal correspondence within the tax agency also indicate that taxation was approached with care and legal requirements were paid attention to. The MVS are publicly available from 1877 to 1927. There are various studies outside of the Netherlands that use similar sources. Eggers and Hainmueller (2009) use a very similar source for their study about British MPs, and Fisman et al. (2014) use mandatory asset declaration forms for Indian MPs, and Bottomley (2019) uses probate inventories to investigate the returns to inventions. In Online Appendix C, I show several examples of the primary data source and provide a more in-depth explanation.

Since I am focusing on close elections, I have prioritized collecting wealth data for candidates whose margins were closer to zero. In total, out of 6,679 candidate-election pairs, I collected probate inventories for 4,065 candidate-election pairs. These pertain to 515 unique candidates, whereas in total, there are 905 unique candidates. There are 2,618 candidate-election pairs who took place in relatively close elections,[5] for 1,652 of which I collected their probate inventory (63%). The main reason of absence is the aforementioned limited availability of the archival data. Machielsen (2021) shows that there is no relationship between many characteristics and the likelihood of finding a probate inventory, implying that the unfindable inventories are missing at random. Out of the 905 unique candidates, 621 of them succeeded in getting elected at least once. I was able to collect the probate inventory for 369 out of these individuals (59%). Out of the 284 unique candidates that were never elected, I was able to find the probate inventory for 143 out of them. Out of the candidates who were not elected, but did, at some point, lose with a margin of at most 20%, I found the probate inventories for 123 unique candidates. Finally, out of 621 politicians who have been elected at least once, 463 of them succeeded in getting elected twice, 342 three times, 278 four times, and 203 more than four times. [6]

3.5 Other Covariates

I obtain control variables at the district-level from the Historical Database of Dutch Municipalities (*HDNG*), a database containing information about Dutch municipalities. Since the composition of districts changes slightly over time, I use a dynamic mapping to aggregate data on the municipality-level to the district-level, contingent on the year in which the election took place, after which I construct variables that measure the religious com-

[5] Elections with an absolute margin smaller than 20%.

[6] I acknowledge the possibility that the results can be influenced by sampling mechanisms. For example, I might only observe observations with a net wealth high enough to assemble a probate inventory. Alternatively, due to disproportional tax evasion, the real net wealth of extremely wealthy individuals is underestimated. In Online Appendix B, I analyze the consequences of these processes for the estimates and find that all of these processes likely introduce a downward bias in my estimates.

position (% Catholic and Protestant), the composition of the labor force (% in industry, services, agriculture) and the share of various taxes per capita in two available years, 1859 and 1889 as a proxy for district economic activity. In addition, I use data from the Historical Sample of the Netherlands (*HSN*) for district-level data on literacy, aggregated from individual-municipality level data.

4 Method

4.1 Dynamic Regression Discontinuity

I use quasi-random variation induced by close elections to estimate the effect of being politically active on end-of-life wealth. The analysis of these returns to politics is complicated by the recursive nature of being elected into political office: because individuals can be elected multiple times, I have to take into account the dynamic nature of the treatment assignment to individuals. Concretely, an estimate of the effect of being elected for the first time on end-of-life wealth contains not only the *ceteris paribus* effect, but also the dynamic effects of having an altered probability of being re-elected and accruing returns to a prolonged stay in the Lower House.[7]

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

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

where w_{ijp} is the end-of-life net wealth for candidate i from party p competing in district j.[8] 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), and use the default set of parameters.

Subsequently, I follow an approach based on Cellini et al. (2010) to disentangle these effects. More precisely, consider the following model[9], which incorporates the possibility

[7] 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 frequently condition the sample on candidates having tried a similar number of times, or use it as a control variable.

[8] 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.

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

that the estimated return for candidates who are elected for the first time can partially reflect future effects:

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

where $c_{i,k}$ is an indicator whether candidate i has been elected for the k'th time. Focusing on an RD-implementation so that $\mathbb{E}[u_i c_{i,k}] = 0$, and 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 from participating and winning in the future:

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

where in the last line, I recognize that the parameters of interest, the ceteris paribus wealth effects of being elected for the k'th time θ^{ATT}, are functions of the original estimates, θ^{ITT} and of future ceteris paribus wealth effects. In addition, the partial derivative of being elected for the t'th time with respect to being elected for the k'th time is recognized to be an incumbency advantage, denoted as π_{t-k}. Henceforth, I refer to the π_{t-k} as the $t-k$'th order incumbency advantages. In the context of this structure, I interpret the regression discontinuity estimates from equation 1 as an "intent-to-treat" (ITT) effect, and the ceteris paribus estimates as the "average treatment effect on the treated" (ATT) estimates. After having estimated the ITT effects, and the incumbency advantages, equation 3 allows me to recursively compute estimates for the ATT effects, under one identification assumption. For equation 3 to contain a finite number of θ^{ATT} terms, I must impose one t^* for which the estimand $\theta_{t^*}^{ITT} = \theta_{t^*}^{ATT}$. In the analysis, I employ this assumption and test its sensitivity for the estimates of θ_t^{ATT}. Standard errors for the ATT estimates can be computed using the delta method.

4.2 Definition of Margin

In line with other studies using close elections (e.g. Lee, 2008; Fisman et al., 2014), this study employs a vote margin-based methodology to identify close elections with only marginal differences between two or more candidates. Most studies focus on elections with only two candidates. This study, however, features a significant number of multi-candidate elections, requiring a more general definition for margin, the running variable in the RD strategy. The marginal winner (MW) in each election is identified as the winning candidate securing the lowest count of votes among all victorious candidates. In a significant number of instances,

this corresponds to the sole winner in elections featuring a single contested seat. However, in several cases, this criterion may reveal a different candidate. The marginal loser (ML) is defined analogously. The set denoted as Winners_j encompasses all victorious candidates in a given district j. Consequently, at the candidate-district level (candidate i, district j), vote margins are defined and computed as follows:

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

This way of defining the margin ensures symmetry and simplifies to the conventional definition of margin in case of an election with only two candidates.[10]

4.3 Estimation of Incumbency Advantages

Estimating the incumbency advantages π_t is relatively straightforward, using the following specification for the n'th order incumbency advantage:

$$\mathcal{I}[c_{i,t+n} = 1] = \alpha_t + \pi_n \cdot 1_{\text{Margin}_{i,t}>0} + \eta \cdot f(\text{Margin}_{i,t}) + \epsilon_{it} \tag{4}$$

where the dependent variable is 1 if candidate i won an election $t+n$, 0 if a candidate loses. Again, in line with e.g. Lee (2008), I focus on close elections to identify the ceteris paribus influence of winning on the probability of winning the n'th election afterwards. Equation 4 is again estimated using the methodology of Cattaneo et al. (2019) and uses the default parameter settings.

5 Results

5.1 Covariate Balance

The validity of the regression discontinuity approach implies a random allocation of politician status close to the threshold with respect to pre-treatment variables, meaning that these pre-treatment characteristics should be roughly equal in treatment (politician) and control (non-politician) groups. Following concerns raised about the possible non-randomness of close elections by Caughey and Sekhon (2011), I use the same logic as do Lowes and Montero (2021), who estimate the RD-effect on pre-treatment characteristics at the cut-off as well as within different margins, to investigate patterns of convergence. Table 7.1 displays the results of this analysis. The presented statistics are conditional on party fixed effects. Selection into politics is clearly not random. Focusing on candidates who won and candidates who lost within a fairly wide margin, the elected group ("treated") differs in almost every

[10] 1,287 out of 2,858 elections are elections between 2 candidates.

measurable aspect from the non-elected group ("control"). In particular, elected candidates are much more likely to be recommended by a newspaper, are elected faster, and compete in elections with a higher turnout. They also differ from losing candidates in terms of the district make-up in which they compete, and they come from statistically different birthplaces. However, when conditioning this on a fairly narrow margin, almost all differences between the treated and control group disappear. At the 5% margin, only slight imbalances in terms of newspaper recommendations and district characteristics remain. Conditional on being a member of a particular party, at the discontinuity, no measurable imbalances remain: on average, my strategy compares treated observations from candidates, backgrounds, districts and birthplaces who are statistically similar to candidates from the control group.[11] In all baseline analyses that follow, I condition my estimates are control variables that are responsible for some imbalances at the 5% level (recommendations and the religious make-up of districts), and I also control for election year and the age at election.

[Table 7.1 here]

In Online Appendix D, I also investigate the covariate balance for the second and third elections (Tables D.1 and D.2). In these cases, there is no evidence for a discontinuity in any of the pre-treatment variables. I also show the full descriptive statistics of all variables used in this study (D.3). Next, I also show placebo tests for the subsample for the first and second election: I test whether there is a discontinuity at the margin for past outcomes of various dependent variables I used in this study in Online Appendix Figure D.1. These results indicate that there is no sign of a discontinuity in any of the past values of the outcome variables used throughout this analysis.

5.2 Regression Discontinuity Results

Table 7.2 shows the results for some of the estimates of Equation 1. In the first and second column, I focus on $\tau = 1$: on candidates who, if elected, would be elected for the first time. In the first column, I focus on candidates who, in addition, compete for the first time. In the second column, I focus on candidates who have competed less than three times. Because this subsample might include multiple observations of the same individual, I cluster standard errors at the individual candidate-level. In the second two columns, I focus on $\tau = 2$, candidates who, if elected, would be elected for the second time. Hence, these estimates compare candidates who have all been elected once. In column 3, I focus on candidates who have tried fewer than three times since last elected, and in column 4, I focus on the entire subsample. Finally, in columns 5 and 6, I focus on $\tau = 3$ and $\tau = 4$ respectively, that is, comparing candidates who have been elected twice (column 5) and three times (column 6) and run for their third (column 5) and fourth (column 6) election.

[Table 7.2 here]

[11] These results hold also if I condition on district fixed effects in addition to party fixed effects.

The results show a surprising pattern: the estimate of the returns of politics to end-of-life wealth is insignificant for the first period, irrespective of the number of tries. The coefficient estimate is positive, but does not attain statistical significance. In the second period, comparing candidates who have been elected exactly one, the point estimates are positive and statistically significant at a 5% level. These estimates imply that the end-of-life premium to politics is about 100,000 guilders. Considering that politicians live about 22 years on average after being elected,[12] that amounts to a wealth premium of about 5000 per year, or twice the formal salary. These results can therefore not be explained by a formal salary. From the perspective of accumulation, these estimates imply a wealth accumulation premium of about 5 percentage points per annum. This is roughly consistent with estimates by Fisman et al. (2014) found in contemporary India. It is also of the same order of magnitude as the effect found by Eggers and Hainmueller (2009). In Figure 7.1, I graphically show the estimates from Equation 1 for the first until the sixth period. It is also apparent visually that there is only a discontinuity when analyzing the returns to the second period of political activity: in all other periods, there is no apparent discontinuity in the conditional expectation function around the margin.

[Figure 7.1 here]

The problem with these estimates, as explained in section 4, is that each of these estimates might be tainted by future estimates, so it cannot be interpreted as a *ceteris paribus* estimate of the effect of the τ'th period of political activity on end-of-life wealth. Hence, in what follows, I employ the methodology from section 4.1 to provide *ceteris paribus* estimates to each additional term of political office.

5.3 Dynamic RD Results

In Table 7.3, I provide estimates of the ceteris paribus (ATT) effects on the basis of the ITT effects for each additional term of political office. In the two panels, I have varied the t^* assumption: in panel A, I use $t^* = 4$, indicating that for the fourth period, the ceteris paribus return should equal the total return. In Panel B, I set $t^* = 7$. The estimates for both Panels are generally consistent, and are consistent with the pattern shown in Table 7.2: there seems to be a gate-keeping pattern of returns to politics: in the first period, there are no discernible returns to office, whereas in the second period, the estimates are significant at the 5% level, and the magnitude is comparable to the magnitude sketched out before: a second period of political activity makes for a end-of-life wealth premium of about 100,000 guilders, equal to about six times the salary of a Minister, or about 5,000 per year from entry into politics until death, which is in turn twice the yearly salary for most of the period under investigation. In further periods, the ceteris paribus returns to politics are again indistinguishable from zero.

[Table 7.3 here]

[12] Online Appendix D, Table D.3

As can be seen from the Table, the estimates for the ITT and ATT effects do not differ radically. This is due to the incumbency advantages being relatively small, also rendering the feedback effect from future effects small. Nevertheless, the Table also shows that the ITT estimates, particular for the first period, tend to be biased upward by the presence of larger returns to future terms of political office. When $t^* = 7$, the point estimate for the returns to a first period of political office decrease from 0.511 to 0.141, indicating that the ITT approach significantly overestimates first-period return to political office.

In Figure 7.2, I graphically show the estimated pattern of returns to additional periods of political office, while varying the t^* assumption in each window. The Figure demonstrates that the pattern observed in Table 7.3 is not an artefact of a particular assumption for t^*: the pattern remains similar irrespective of t^*. The reason for this is the limited magnitude of the incumbency advantages, narrowing the gap between the ITT-estimate and the ATT-estimates. However, estimates for the first and second periods could theoretically be most affected by this bias, as these estimates are tainted by the highest number of feedback effects from future estimates. In Figure 7.2, however, it can be seen that the estimates for the ceteris paribus returns to political office for the second time are always statistically significant at the 5% level. Furthermore, estimates for all other periods of political activity are statistically indistinguishable from zero.

[Figure 7.2]

5.4 Robustness Checks

In Online Appendix D, 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: First, in Online Appendix Figure D.2, I graphically show the estimates of a placebo test involving artificially varying the cut-off point for being elected from $[-0.15, 0.15]$. I focus on the key estimates reported here, the returns to a second Lower House term. These results show that the point estimates are highest for the true cut-off ($c = 0$), and furthermore, the true estimate is the only statistically significant estimate. 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 a large confidence interval.

Regression discontinuity parameters: The baseline ITT estimates result from the default parameter settings in the `rdrobust` package by Calonico et al. (2015). In particular, they use the MSE-optimal bandwidth as defined by Cattaneo et al. (2019), they use 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.[13] To investigate whether the estimates are sensitive to the bandwidth used, I estimate the ITT effects with a fixed bandwidth of 0.15, considered wide, and a fixed bandwidth of 0.08, considered narrow. The

[13] All of these choices are the default parameters in the `rdrobust` package.

results are reported in Online Appendix Tables D.4 and D.5. Similarly, in Online Appendix Table D.6, I estimate the ITT effects using different kernels, the Epanechnikov and uniform kernels. Next, in Online Appendix Table D.7, I report the estimates resulting from varying the polynomial degree with which the effect and the bias are estimated. All of the resulting estimates are virtually identical to the results reported in Table 7.3.[14] Finally, I also report estimates with standard errors clustered at the unique election (district-year) level (Online Appendix Table D.8). 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 equation 4 to estimate incumbency advantages. However, incumbency advantages can also be estimated in a more refined way, for example, by conditioning on district and party fixed effects. In Online Appendix Table D.9, I report resulting ATT estimates, incorporating an estimation of incumbency advantages with district and party fixed effects. The results are very similar to the results shown in Table 7.3. This rules out that an arbitrary estimation of incumbency advantages is responsible for the observed pattern of results.

Other log-like transformations of the DV: 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. 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 Table D.10, I explore the effects of using different scales on the estimates. I find that while the effect sizes vary, they are still comparable to the originally reported effect size. Moreover, exactly the same pattern is found, and the statistical significance of all estimates is identical to the pattern found in Table 7.3. I also estimate the treatment effect using a Poisson QMLE procedure. The resulting unit-independent treatment effects (Online Appendix Table D.11) are also very close to the estimates reported in the main text.

Other control variables and fixed effects: The baseline estimates are estimates within-party, within-district. In Online Appendix Table D.12, I also rely on exclusively within-election variation by estimating Equation 1 and the derived ATT effects using district-year dummies. The inference in this case is based on close candidates within the same election. The disadvantage is a potential loss in statistical power and increase in bias, since the optimal bandwidth might be relatively high in this case. Although the magnitude of the effect in this case is decidedly smaller, the pattern is exactly the same as in Table 7.3. The statistical significance is also unaffected. I also estimate the effects based on only within-party variation, without district fixed effects (Online Appendix Table D.13). 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 7.1, in addition to district

[14] 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.

and party fixed effects, and age at election and year controls. The results (Online Appendix Table D.14) are also invariant to the inclusion of this full set of controls.

Extreme values: There is also a concern that the estimates might be driven by outliers in the wealth distribution. To investigate the effect of outliers, I winsorize the upper decile of the distribution of the dependent variable. The reported estimates are shown in Online Appendix Table D.15. These estimates are virtually identical to the baseline estimates, which are therefore not due to outliers.

Alternative definitions of the DV: In my 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 also 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. Additionally, on some probate inventories, net wealth is listed as negative. In the default analysis, these observations are omitted. However, I also analyze the results including these observations as a net wealth of zero. These three robustness checks are performed in Online Appendix Table D.16, and all show estimates with the same pattern as in Table 7.3.

Independent observations: Since in the baseline analysis, the same outcome is used more than once, there is a concern that inference may be biased. Even though the baseline estimates allow for a correlation between more observations of the same politician, I provide estimates with standard errors of the ITT estimates clustered at the election level in Online Appendix Table D.17. These results again show exactly the same pattern, and are still statistically significant for the second period despite the reduced sample size.

Pre-election wealth: Since the identification assumption of the analyses hinges on pre-treatment characteristics being balanced among winning and losing candidates, there might be a concern that just-elected candidates might be discontinuously wealthier at the time of election in comparison to their just-losing contenders. Even though there is evidence against the hypothesis that wealth might discontinuously influence the probability of election (Poulos, 2019), I conduct an analysis on a small subsample of observations for which I can construct a proxy of initial wealth. For a small subsample of observations, I have been able to find the probate inventories of parents and construct proxies of inheritance, defined as parental net wealth divided by number of offspring.[15] I report these analyses in Online Appendix Tables D.18 and D.19. Even though some of these analysis are too noisy to provide meaningful inference, the point estimates and patterns exactly match the pattern in the baseline analysis.

Other identification strategy: Finally, the results are also robust to a different identification strategy. I estimate the ITT effects using instrumental variable (IV) analysis, making use of the fact that candidates who are recommended by a newspaper are much more likely to be treated (to win an election) than candidates who aren't recommended. In addition, these analyses are conditioned on district and party fixed effects, use certain human-capital-

[15] The sample for this analysis is limited by the availability of probate inventories: for the probate inventory to be publicly available, an individual's date of decease has to be between 1877 and 1928.

relevant control variables (pre-treatment career paths) and control for the number of tries. Although this identification strategy is not necessarily valid, the resulting estimates replicate the same pattern of the baseline analysis.[16] The results are reported in Online Appendix Table D.20. The effect of political office for the second term has a significantly positive effect on end-of-life wealth, whereas for all other periods, the estimated effect of political office is close to, and statistically indistinguishable from zero.

6 Mechanisms

6.1 Career Paths

The results in the previous section may be due to various mechanisms. In the literature, there are ample papers implicitly and explicitly arguing that political rents might be accrued in-office (Fisman, 2001; Fisman et al., 2014; Baltrunaite, 2020). On the other hand, there is also a literature showing that political office might give various advantages that are only accrued after having acquired political office, or even after a political career (Eggers and Hainmueller, 2009; Fafchamps and Labonne, 2017; Querubin et al., 2016; Folke et al., 2017; Geys, 2017). To investigate the effect of Lower House membership on political career trajectories, I estimate equations of the form:

$$y_{i,t^+} - y_{i,t^-} = \alpha_j + \gamma_p + \theta_\tau^{ITT} \cdot 1_{\text{Margin}_i>0} + \eta \cdot f(\text{Margin}_i) + X_{ij}\beta + \epsilon_i \tag{5}$$

where y_{i,t^+} is an indicator variable equaling 1 when a candidate takes on career path k at any moment *after* the election at time t, and y_{i,t^-} is an indicator whether a candidate had taken on career path k before the election at time t. Even though the pre-treatment characteristics are statistically indistinguishable, this specification is robust to potential imbalances in career paths, and detects changes in career paths due to being elected in political office. I then use the method described in equation 3 to compute ceteris paribus estimates. The career paths I investigate are: careers in any kind of politics (panel B), careers in national politics (panel C), provincial politics (panel D) and municipal politics (panel E). Finally, I investigate careers in business, law and entrepreneurship (panel F). In panel A, I recapitulate the ceteris paribus results from equation 1.

In Table 7.4, I report the results from this analysis. The results in panel B show that there are no effects of being elected into the Lower House on future careers in politics: winners are not more likely to pursue a career in politics than losing candidates, irrespective of the term. Panel C reveals that this pattern might conceal considerable heterogeneity: when we focus specifically on national politics, it is revealed that winners are *less* likely to pursue careers in any (other) national political institution other than the Lower House. This reveals to some extent that the Lower House and other national political institutions are substitutes, since

[16] If newspaper recommendations can be bought, or otherwise aren't administered randomly, the instrument itself is endogenous.

losing candidates end up significantly more often in the Upper House or as a Minister.

[Table 7.4 here]

Looking at the other Panels, there is very little evidence that election into the Lower House causes candidate's career possibilities and trajectories to change drastically. More particularly, whereas the returns to political office have been identified as being due to the second period of political activity, there is no evidence of an accompanying change in career paths due to the same period of political activity. Panel F does seem to confirm that (first-time) politicians are more likely to engage in a business or entrepreneurial career due to their experience in the Lower House, but that in turn does not make for an end-of-life wealth premium, as evidenced by Panel A. In Online Appendix D, Table D.21, I analyse a more granular version of these career paths. This analysis reveals that just-losing candidates are specifically more likely to take up Ministerial positions, and that this is driving the effect on national politics. Given that Ministerial positions were extremely highly paid in terms of formal salary, this could explain the absence of tangible wealth effects in the first period of activity in the Lower House. Secondly, consistent with the content of Lower House activities, the effect of Lower House membership on Business & Entrepreneurship career paths is mainly driven by careers specifically in law. In both cases, however, these effects do not coincide with the effect of Lower House membership on end-of-life wealth in the second period. Hence, these results provide only limited support that career paths were substantially altered due to Lower House membership, and that this could be an explanation of the observed effects in Table 7.3. These results therefore present evidence against a human-capital based explanation of political rents (Mattozzi and Merlo, 2008; Keane and Merlo, 2010; Geys, 2017).

6.2 In-office Returns

Another possibility is that politicians can use their Lower House mandate to accrue returns during their political activity (Fisman, 2001; Fisman et al., 2014). For example, politicians may act with insider knowledge about laws affecting asset prices, or politicians' power might be bought by firms (Tahoun, 2014; González et al., 2020). In Table 7.5, I report the first results supporting this supposition: in two subsamples, I distinguish between (i) candidates whose party will be the incumbent party after the election,[17] and (ii) candidates whose parties form the opposition. In this period, governance was marked by majority-rule, and one of the parties (Liberals or Confessionals) had the absolute majority in parliament. Therefore, law-making and the initiative to amendments were usually ceded to members of the incumbent party (Van Den Berg and Vis, 2013). I find that the effects are entirely driven by observations from the incumbent party. This result makes it likely that elected candidates from the governing might have influenced law-making in such a way that it benefited them personally: losing candidates of the same incumbent party ended up with significantly lower end-of-life

[17] Or is the incumbent party in the case of a preliminary election.

wealth. Elected candidates from the opposition parties did not have such opportunities. The same "gate-keeping" pattern as presented in the main results is visible.

[Table 7.5 here]

At the same time, I find no significant heterogeneity in terms of the career path changes that are due to assuming political office, with the exception that the effect of political office on the likelihood of taking on other national political functions is solely due to members of incumbent political parties. In Online Appendix Table D.22, I also investigate whether there is heterogeneity between parties. This is not substantially the case: what matters is the incumbency status and not the party *per se*. All of the estimates on subsamples of observations from only one party show the same "gate-keeping" pattern as reported in the main text, but none of the parties is solely responsible for driving the effects.

In Online Appendix Table D.23, I also show that the effect is driven by other heterogeneity in electoral institutions at the time and place of being elected: I find that the results are driven by districts in which socialist candidates received a relatively high vote share in the preceding elections, as opposed to districts in which socialists received low vote shares. To account for the differential intensity of socialist candidates over time, I condition these estimates on decade fixed effects. Even though the sample size and statistical power are limited, these results show that the effects are coming from candidates in districts with a low socialist vote share, confirming findings from a literature about revolutionary threat disciplining politicians self-interested behavior (Acemoglu and Robinson, 2001; Aidt and Franck, 2019).

In Online Appendix Tables D.24 and D.25, I provide suggestive evidence of the effects depending on yet other types of institutional heterogeneity: the effects are mainly driven by districts with a low literacy rate, and by observations from before a major suffrage extension in 1896.[18] These results make it all the more likely that returns are accrued in-office, as they respond to contemporaneous institutional variation: if the returns to political office are only accrued later, the circumstances under which candidates are elected should not influence the magnitude of the returns to politics. At the same time, these results make it clear that increased monitoring of politicians could reduce the occurrence of rent-seeking behavior Duggan and Martinelli (2017).

An additional piece of evidence for why the returns are likely to be accrued in-office comes from a comparative analysis between a subsample of candidates who died relative shortly after being elected, and a subsample of candidates who die relatively late. A potential alternative explanation might be that political office induces increased thriftiness or higher financial literacy, and the increase accumulation coming from those might be responsible for the observed patterns. This implies that the observed pattern in Table 7.3 should not

[18] Because there is also considerable secular time-variation in literacy, and the variation around the suffrage extension is also time-variation, these estimates are also conditional on decade fixed effects in addition to district and party fixed effects.

be visible for candidates who die relatively shortly after having been elected. In Online Appendix Table D.26, I show the results of these two analyses. In fact, the results are driven almost exclusively by the subsample of candidates with a relatively shorter lifespan after being elected. To be sure, I also report the estimates of political office on career paths, but there is no evidence of a differential relationship between political office and career paths between the two subsamples. Hence, this result adds credibility to the in-office rent-seeking hypothesis.

Furthermore, an out-of-office explanation implies the results might be due to effects on longevity (Chetty et al., 2016): a political career may impact life expectancy treated candidates might live longer, and therefore have more time to accumulate wealth. In addition to finding no direct effect of any period of political office on lifespan, in Online Appendix Table D.27, I report an analysis of the effect of political office on wealth per unit of lifespan. The results that I find show exactly the same pattern as the baseline results and also imply the same order of magnitude for the returns to office.

There is also a concern that the pattern of results may be due to selection in electoral dynamics. Concretely, if the electorate can (partially) detect rent-seeking type politicians (Besley and Case, 1995), then, after observing their activity for some time, this type of politician may be voted out, such that only 'honest' politicians remain in the political arena. This selection may also come from political parties: they may chose to halt recommendations or sabotage these candidates. I address this concern in Online Appendix Tables D.28, D.29, and D.30. If this selection mechanism is responsible for the results, the correlation between personal wealth and the probability of reelection, candidacy or recommendation given candidacy should be negative. In fact, empirically, these correlations are mostly positive after the first and second periods, and insignificantly different from zero for others, making it unlikely that these dynamics play a role. In short, this shows that selection concerns towards honest or non-rent-seeking politicians, coming from either the electorate, political parties, or candidates themselves, are unlikely to play a role.

Finally, there are also various pieces of anecdotal evidence that support the existence of in-office rent-seeking behavior. 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 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, several anecdotes also clearly illustrate the existence of entry barriers to the Lower House. 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 (Stutje, 2012). One of the first working class MP's, Heldt, had a similar experience: "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). 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. That easily led to friction (...). Although the liberals officially honored the meritocratic ideal, neither the emphasis that was systematically placed on his academic education nor the acculturation of Dutch preachers to the learned class (...) could adequately compensate for his [modest] origins" (Koch, 2020).

7 Conclusion

This study investigated the financial returns to politics from a dynamic perspective. In contrast to previous studies (Eggers and Hainmueller, 2009; Fisman et al., 2014), the method in this paper explicitly sets out to derive estimates of the returns to each additional period of political activity, tracing out a marginal return curve to political activity. The method used in this study exploits close elections and the resulting allocation of Lower House membership using a historical sample of Dutch elections (1848-1917). The observed repeated treatment assignment can be used to construct *ceteris paribus* estimates of the returns to a particular period of political office. I find that there is a convincing and robust causal effect of political activity on end-of-life wealth, corroborating several other studies (Eggers and Hainmueller, 2009; Fisman et al., 2014). However, this effect only manifests itself during the *second* term of political office, hinting at the existence of entry-barriers to rent-seeking behavior. I find that this pattern is highly persistent, and remains robust to many alter-

native decisions: it is invariant to the regression discontinuity parameters, the method of estimation of the incumbency advantages, variable definitions and transformations, control variables, and the exclusion of outliers. It also passes placebo tests and the estimates from an unrelated identification strategy show the exact same pattern.

The estimated returns are of a significant, but plausible, magnitude. In total, they are equal to about six times a Minister's salary, depending on the point estimate, and cannot be explained by the formal remuneration of politicians. In per-year terms, the wealth premium of this second period in office is about twice the formal salary of Lower House members in this period (Elzinga, 1985). This fact also makes it unlikely that the results can be explained by formal salary differences between winning and losing candidates in any term. To be more precise about this claim, I also focus on the change in career patterns induced by holding political office for the τ'th time. I find that these patterns exhibit little changes, indicating that Lower House membership is (i) not responsible for an increase in marketable human capital that could potentially change career trajectories, and (ii) likely did not give access to a new network that could help candidates ending up at positions they wouldn't have ended up in the counterfactual (cf. Mattozzi and Merlo, 2008). These estimates, as well as anecdotal evidence (Van Den Berg, 1983; Van Den Braak, 1999) suggests that candidates who were close to being elected were already part of a network in which they competed with Lower House members. This is also evident in the high control group mean of other (non-Lower House) political careers after the election.

I provide evidence that the returns are likely due to in-office rent-seeking behavior, in contrast to explanations related to career paths and human capital benefits of Lower House membership. The results are driven by candidates who are elected when their party is the incumbent governing party. It is these candidates, who, if elected, likely have enough power to take the initiative to spin law-making procedures in their personal favor. Opposition politicians often did not have these opportunities (Van Den Berg and Vis, 2013). The results imply that these opportunities were concentrated with, and provided to, politicians after an initial burn-in period. There are also many pieces of anecdotal evidence consistent with this pattern of burn-in periods and rent-seeking behavior (Kroeze, 2013). More generally, heterogeneity in the effects according to the electoral institutions present in the district at the moment of the election supports the conjecture that rents are mainly accrued in-office. This heterogeneity is only present in the effect on end-of-life wealth, not in the effects on career paths.

In all other terms but the second term, the end-of-life wealth of politicians is insignificantly different from candidates who failed to be elected by a small margin, hinting at the absence of a wealth premium of political activity. As discussed, the reason for the absence of returns in the first-term might be a burn-in period. In later terms, the absence of a wealth premium for Lower House membership might have to do with the control group: candidates who compete in close elections after having been elected to the Lower House more than twice are likely to also have been employed elsewhere in politics, business, or law, such that there is no clear premium to Lower House membership. The results also give a more refined

view of the returns to political office. Whereas the estimates from Eggers and Hainmueller (2009) parallel the first period ITT estimates reported here, the estimates of Fisman et al. (2014) contain a mixture of the estimates, since candidates in their sample may run for different periods of political office. This paper explicitly recognizes the relationship between incumbency advantages and arising opportunities in future periods of incumbency.

The findings in this study raise a number of questions. Even though I find no evidence for returns to Lower House membership after a second term, it would be premature to conclude that politicians accrue no financial benefits from political office. In particular, it might be of interest to investigate what serves as the control group in this study: studying the networks and the interplay between various political functions can provide insights into the political class as a whole, rather than only Lower House members (cf. Dal Bó et al., 2009; Fafchamps and Labonne, 2017; Dal Bó and Finan, 2018). The results, in demonstrating a one-off pattern of financial returns to Lower House membership, might also be inconsistent with evidence that implies a constant marginal return curve to politics, e.g. insider trading by politicians (Bourveau et al., 2021) or embezzlement of public funds (Baltrunaite, 2020). Juxtaposing the evidence of the present study with these studies raises questions about the broader determinants of the returns to politics. This study makes a step in that direction by exploiting heterogeneity at the district, election or candidate level.

Finally, from a historical perspective, the findings confirm widespread views about European politics in the nineteenth century as being dominated by a wealthy, oligarchical elite, subject to few constraints (Van Den Berg and Vis, 2013; De Rooy, 2014). It also corroborates several empirical studies that emphasize the important role of political institutions, most notably, suffrage extensions, in disciplining politicians (Aidt and Franck, 2019; Lacroix, 2023; Marcucci et al., 2023). In future research, it would be interesting to find settings where there can be found direct evidence for politicians pursuing their own financial interest, thereby corroborating evidence from more contemporary settings (Tahoun and Van Lent, 2019; Baltrunaite, 2020; Bourveau et al., 2021).

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Tables and Figures Main Text

Table 7.1: Conditional Covariate Balance - First Run

	Margin Within 0.2			Margin Within 0.05			
	Mean Treated	Mean Control	p-value	Mean Treated	Mean Control	p-value	RD Estimate
Panel A: Newspaper Recommendations							
Rec.: Liberal	0.079	0.039	0.076*	0.050	0.081	0.450	0.009 (0.070)
Rec. Socialist	0.016	-0.028	0.000***	0.023	-0.012	0.117	0.025 (0.041)
Rec.: Protestant	-0.009	0.085	0.000***	-0.003	0.120	0.000***	-0.086 (0.053)
Rec. Catholic	0.009	0.008	0.957	0.036	0.031	0.845	0.003 (0.038)
Panel B: Election Characteristics							
Number of Tries Until Election	-0.215	0.076	0.006***	-0.267	-0.185	0.642	-0.166 (0.220)
Election Year	3.714	1.947	0.120	4.879	3.704	0.550	2.105 (4.465)
Year of Birth Candidate	3.908	2.156	0.204	4.551	4.654	0.964	1.632 (4.212)
Log(Turnout)	0.100	0.077	0.082*	0.132	0.140	0.745	-0.038 (0.043)
Log (Electoral Threshold)	0.194	0.249	0.171	0.291	0.458	0.029**	-0.146 (0.182)
Log(Electorate Size)	0.092	0.173	0.035**	0.155	0.317	0.028**	-0.159 (0.164)
Panel C: District Characteristics							
District Population	0.206	0.340	0.012**	0.242	0.422	0.028**	-0.145 (0.133)
% Labor Force Industry District	0.001	-0.027	0.000***	-0.001	-0.023	0.027**	0.017 (0.015)
% Labor Force Agriculture District	0.005	-0.025	0.000***	-0.003	-0.023	0.141	0.015 (0.020)
% Labor Force Services District	-0.005	0.052	0.000***	0.004	0.046	0.053*	-0.033 (0.032)
% Paying Wealth Tax District	0.047	-0.314	0.000***	0.008	-0.333	0.053*	0.260 (0.303)
Income Tax Share District	0.093	-0.113	0.002***	0.071	-0.105	0.132	0.108 (0.207)
% Catholic District	-0.018	-0.065	0.000***	0.003	-0.046	0.020**	0.048 (0.032)
% Protestant District	0.012	0.063	0.000***	-0.003	0.049	0.007***	-0.051 (0.030)
Distance to the Hague - District	0.603	-7.986	0.012**	-5.849	-9.677	0.498	2.718 (9.259)
Panel D: Birthplace Characteristics							
% Labor Force Industry Birth Place	0.012	-0.008	0.005***	0.003	-0.009	0.342	0.005 (0.016)
% Labor Force Agriculture Birth Place	0.012	-0.005	0.054*	0.000	-0.008	0.556	-0.001 (0.023)
% Labor Force Services Birth Place	-0.025	0.012	0.010**	-0.004	0.017	0.405	-0.002 (0.035)
% Catholic Birth Place	-0.007	-0.020	0.472	0.001	0.012	0.724	-0.011 (0.048)
% Protestant Birth Place	0.005	0.018	0.441	-0.002	-0.008	0.847	0.003 (0.046)
Distance to The Hague - BP	0.970	-2.642	0.356	-3.390	1.287	0.512	-3.165 (8.488)

Note: 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 ¡ 0.1, **: p ¡ 0.05, ***: p ¡ 0.01.

Table 7.2: Estimates of the Financial Returns to Politics

Period	First		Second		Third	Fourth
No. of Tries Since Last Elected	First	Less Than 3	Less Than 3	All	All	All
	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient (ITT)	0.368	0.595	1.997**	1.751**	-0.215	-0.359
SE (BC)	(0.474)	(0.451)	(1.031)	(0.865)	(0.361)	(0.496)
Mean DV Treated	11.904	11.453	11.501	11.501	11.596	11.551
Mean DV Control	11.257	10.924	10.145	10.335	11.951	11.600
N (Treated)	179	290	250	259	202	173
N (Control)	276	527	177	204	120	122
Bandwidth	0.222	0.208	0.172	0.180	0.301	0.195

Note: Table showing coefficients estimates of various periods of political activity on politicians' end-of-life personal wealth. The estimates are derived using the methodology in (Cattaneo et al., 2019) under MSE-optimal bandwidth. Standard errors are clustered at the politician-level. Model (1) shows the estimate of returns for the first period using a subsample of candidates who compete for the first time. Model (2) shows the returns for the first period usinga subsample of candidates who have never been elected and until election i, have competed less than three times, etc. The estimates in both panels control for age at election, year of election, and newspaper recommendations, and are conditional on party and district fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table 7.3: ATT and ITT estimates for different t*

	t = 1	t = 2	t = 3	t = 4	t = 5	t = 6	t = 7
Panel A: t* = 4							
Coefficient (ATT)	0.260	1.751**	-0.176	-0.359			
SE (ATT)	(0.402)	(0.866)	(0.365)	(0.496)			
Coefficient (ITT)	0.492	1.751**	-0.215	-0.359			
SE (ITT)	(0.388)	(0.865)	(0.361)	(0.496)			
Mean DV Treated	11.206	11.501	11.596	11.551			
Mean DV Control	10.986	10.335	11.951	11.600			
N (Treated)	342	259	202	173			
N (Control)	681	204	120	122			
Bandwidth	0.209	0.180	0.301	0.195			
Panel B: t* = 7							
Coefficient (ATT)	0.049	1.766**	-0.212	-0.305	-0.163	-0.688	0.754
SE (ATT)	(0.440)	(0.873)	(0.388)	(0.508)	(0.549)	(1.232)	(0.729)
Coefficient (ITT)	0.492	1.751**	-0.215	-0.359	-0.275	-0.607	0.754
SE (ITT)	(0.388)	(0.865)	(0.361)	(0.496)	(0.531)	(1.230)	(0.729)
Mean DV Treated	11.206	11.501	11.596	11.551	12.074	11.907	11.630
Mean DV Control	10.986	10.335	11.951	11.600	11.208	12.988	10.828
N (Treated)	342	259	202	173	123	88	63
N (Control)	681	204	120	122	66	61	37
Bandwidth	0.209	0.180	0.301	0.195	0.232	0.198	0.373

Note: Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under different $t^* \in \{4, 7\}$. All the ATT coefficients are derived andrecursively computed from ITT coefficients, which are in turn estimated using the methodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party and district fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

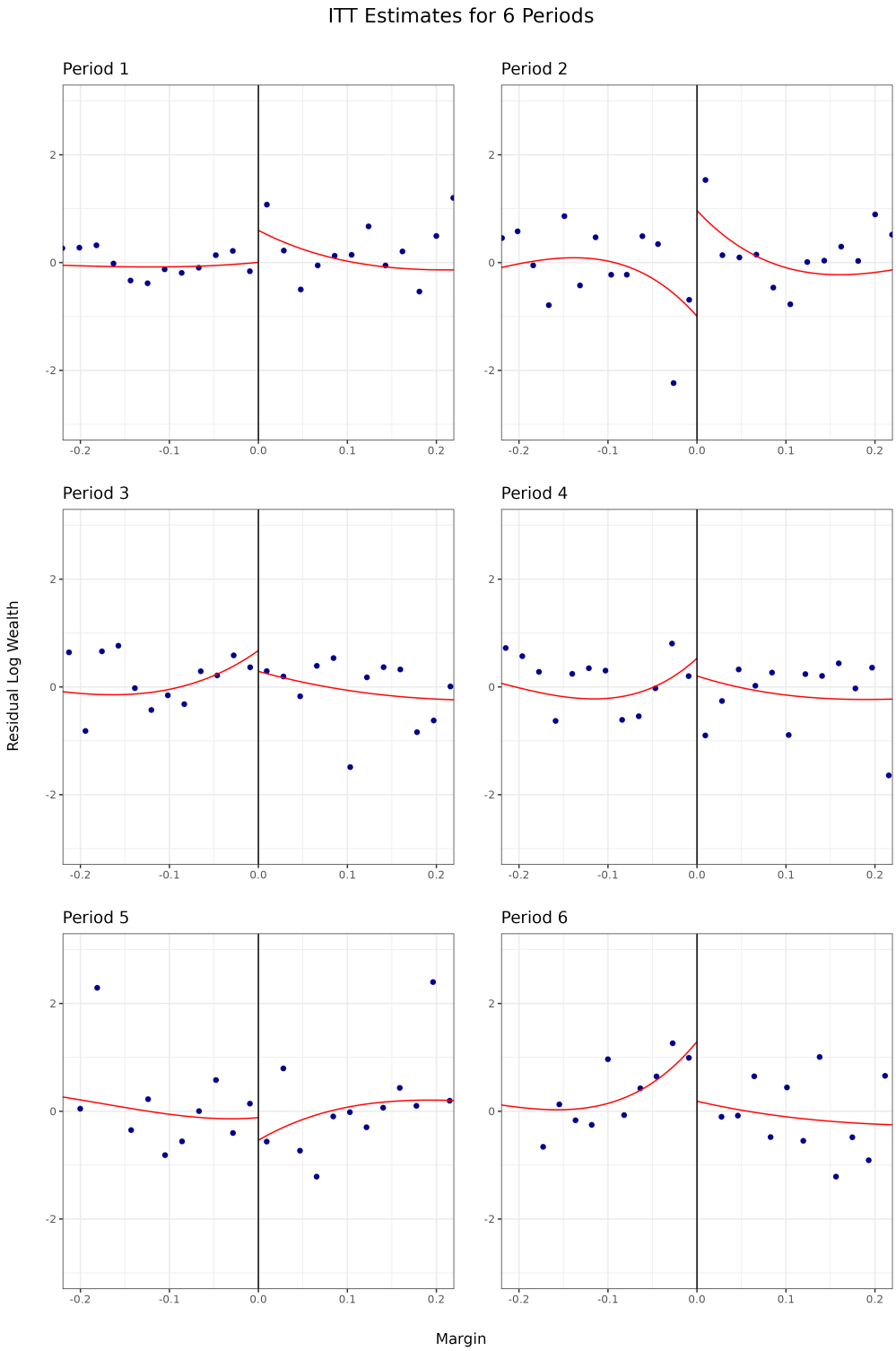

Figure 7.1: ITT Estimates of Being Elected for the τ'th time on End-of-Life Wealth

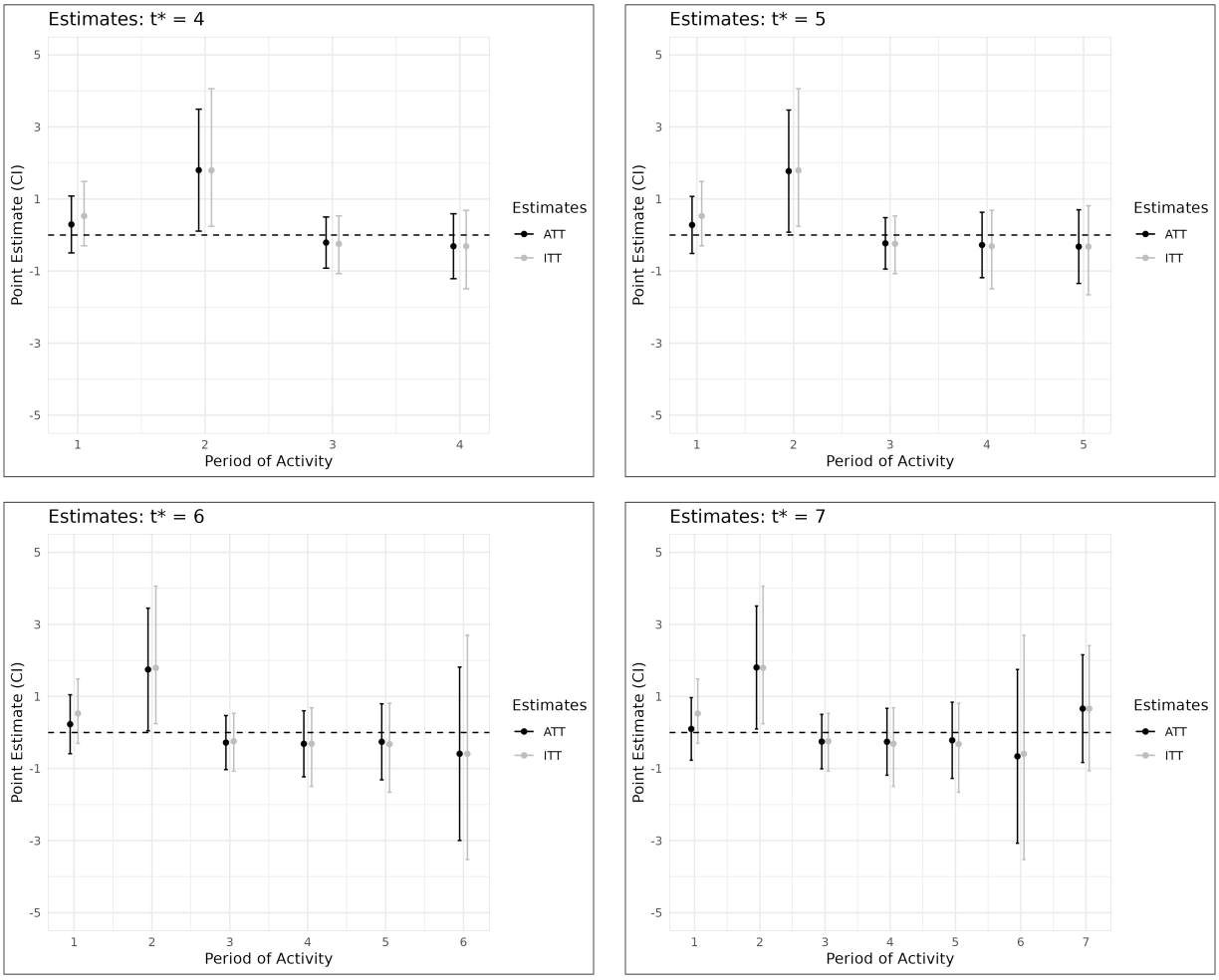

Figure 7.2: Dynamic Returns to Politics

Table 7.4: ATT and ITT estimates: Career Paths

	t = 1	t = 2	t = 3	t = 4
Panel A: DV: Wealth				
Coefficient (ATT)	0.215	1.748**	-0.164	-0.321
SE (ATT)	(0.396)	(0.867)	(0.348)	(0.506)
N (Treated)	342	259	202	173
N (Control)	681	204	120	122
Panel B: DV: Politics				
Coefficient (ATT)	-0.052	-0.007	-0.058	-0.173
SE (ATT)	(0.082)	(0.122)	(0.141)	(0.205)
N (Treated)	600	388	293	225
N (Control)	1267	293	224	157
Panel C: DV: National Politics				
Coefficient (ATT)	-0.106	0.079	-0.089	0.010
SE (ATT)	(0.069)	(0.104)	(0.103)	(0.150)
N (Treated)	600	388	293	225
N (Control)	1267	293	224	157
Panel D: DV: Provincial Politics				
Coefficient (ATT)	-0.094	-0.075	0.284**	-0.259
SE (ATT)	(0.092)	(0.120)	(0.140)	(0.202)
N (Treated)	600	388	293	225
N (Control)	1267	293	224	157
Panel E: DV: City Politics				
Coefficient (ATT)	-0.006	-0.057	-0.068	0.089
SE (ATT)	(0.071)	(0.104)	(0.093)	(0.122)
N (Treated)	600	388	293	225
N (Control)	1267	293	224	157
Panel F: DV: Business and Entreneurship				
Coefficient (ATT)	0.126*	0.038	0.048	0.138
SE (ATT)	(0.072)	(0.120)	(0.124)	(0.103)
N (Treated)	600	388	293	225
N (Control)	1267	293	224	157

Note: Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth and rough career paths: Politics, National Politics, Provincial Politics, City Politics, and Non-Politics. All the ATT coefficients are derived and recursively computed from ITT coefficients, which are in turn estimated using the methodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party and district fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table 7.5: Dynamic Results: Heterogeneity according to Party Incumbency

	Elected when Party Opposition				Elected when Party Incumbent			
	t=1	t=2	t=3	t=4	t=1	t=2	t=3	t=4
Panel A: Personal Wealth								
Coefficient (ATT)	0.034	1.644	-0.194	-0.667	0.724*	1.949**	0.454	-0.884
SE (ATT)	(0.659)	(1.660)	(0.580)	(0.656)	(0.389)	(0.872)	(0.470)	(0.682)
N (Treated)	168	135	112	105	174	124	90	68
N (Control)	432	106	62	74	249	98	58	48
Panel B: Political Career								
Coefficient (ATT)	0.061	-0.194	0.107	-0.160	-0.194	0.008	-0.338*	-0.191
SE (ATT)	(0.116)	(0.154)	(0.164)	(0.184)	(0.119)	(0.159)	(0.176)	(0.265)
N (Treated)	281	223	164	130	319	165	129	95
N (Control)	810	157	149	95	457	136	75	62
Panel C: National Politics								
Coefficient (ATT)	0.071	0.011	-0.034	-0.002	-0.362***	0.027	-0.182	-0.140
SE (ATT)	(0.098)	(0.124)	(0.120)	(0.163)	(0.115)	(0.160)	(0.143)	(0.262)
N (Treated)	281	223	164	130	319	165	129	95
N (Control)	810	157	149	95	457	136	75	62
Panel D: Provincial Politics								
Coefficient (ATT)	-0.075	-0.087	0.230	-0.141	-0.162	-0.127	0.105	-0.047
SE (ATT)	(0.127)	(0.189)	(0.152)	(0.181)	(0.110)	(0.140)	(0.148)	(0.180)
N (Treated)	281	223	164	130	319	165	129	95
N (Control)	810	157	149	95	457	136	75	62
Panel E: Municipal Politics								
Coefficient (ATT)	0.076	-0.228	0.014	0.118	-0.034	0.076	-0.085	-0.278
SE (ATT)	(0.110)	(0.147)	(0.115)	(0.176)	(0.094)	(0.122)	(0.109)	(0.177)
N (Treated)	281	223	164	130	319	165	129	95
N (Control)	810	157	149	95	457	136	75	62
Panel F: Non-Politics or Business								
Coefficient (ATT)	0.076	0.263	-0.122	0.396**	0.122	0.089	0.092	-0.010
SE (ATT)	(0.093)	(0.199)	(0.145)	(0.156)	(0.087)	(0.136)	(0.144)	(0.167)
N (Treated)	281	223	164	130	319	165	129	95
N (Control)	810	157	149	95	457	136	75	62

Note: Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth and rough career path outcomes under different $t^* = 4$ according to incumbency status of the candidate's party. All the ATT coefficients are derived and recursively computed from ITT coefficients, which are in turn estimated using themethodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party, district and decade fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Online Appendix for "Dynamic Returns to Political Tenure"

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Contents

A Extensive Historical Background

A.1 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) mentions 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).

A.2 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.

B Selection Bias

B.1 Truncated Wealth

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

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

$$W_i^* = \theta \cdot \mathbf{1}_{\{Politician_i\}} + \epsilon_i \tag{6}$$

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:

$$W_i = \begin{cases} W_i^* & \text{if } W_i^* > c \\ NA & \text{if } W_i^* \leq c \end{cases} \tag{7}$$

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:

$$\frac{\phi\left(\frac{c-\theta}{\sigma}\right)}{1-\Phi\left(\frac{c-\theta}{\sigma}\right)} < \frac{\phi\left(\frac{c}{\sigma}\right)}{1-\Phi\left(\frac{c}{\sigma}\right)} \tag{8}$$

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 8 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.2 Tax evasion

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

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

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

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

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

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

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

B.3 Differential Sampling

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

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

And the expected value of observed wealth looks like:

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

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

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

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

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

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

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

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

C Replication Package and Data Appendix

C.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.

In principle, 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.[19] 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

[19] 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,[20] 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,[21] a dummy socialist indicating a socialist candidate participated in the election in which candidate i also participated, the percentage socialist vote, a Herfndahl-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

[20] The two most numerous Protestant denominations.

[21] 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 data/analysis as final_dataset.csv.

Step 12: Add Past Margins: In this fnal step, I add the electoral margins for candidate i for 7 past elections, as far as available. These variables are only used in the dynamic analyses to estimate incumbency advantages, and ATT effects. I save this file in the folder `analysis` under the name final_dataset_with_history.csv.

C.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, 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.1). 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.

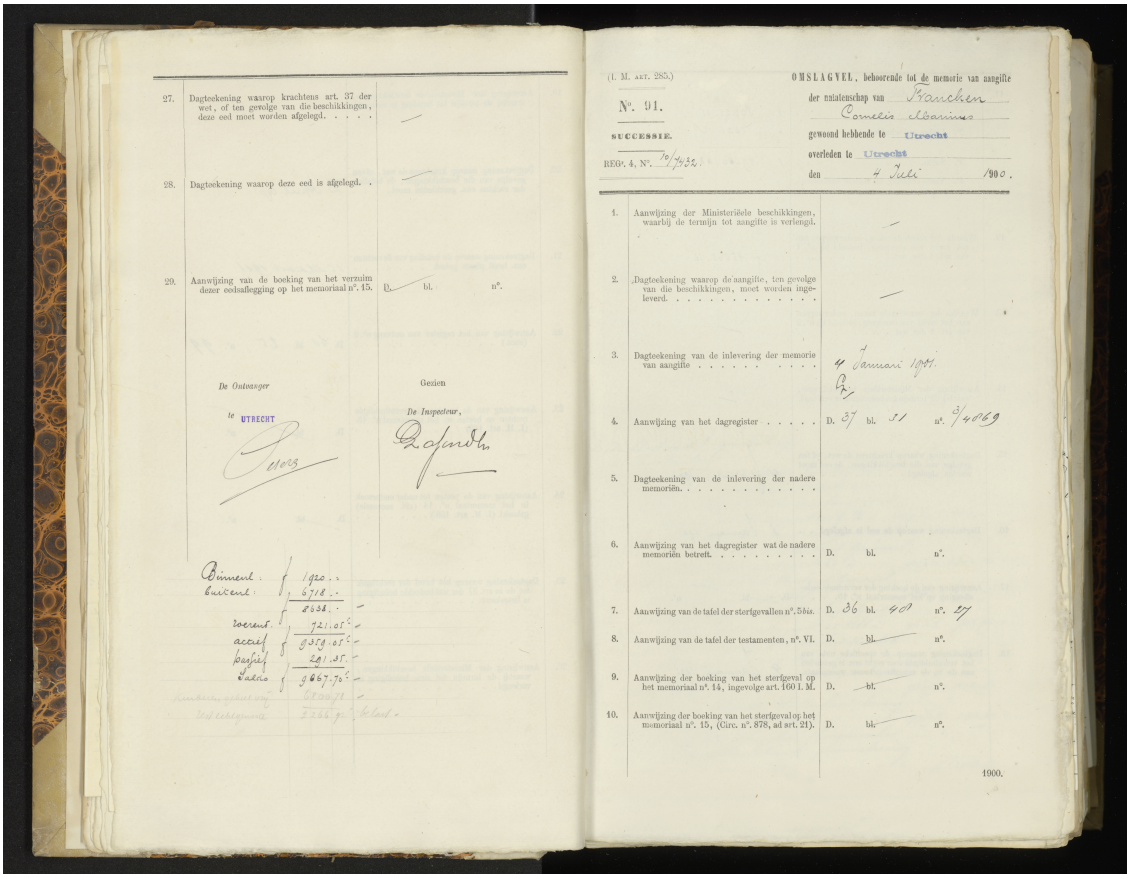

Figure C.1: Front page of a MVS (on the right)

The second page of a MVS is depicted below (figure C.2). 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.2, but visible on figure C.1 on the left).

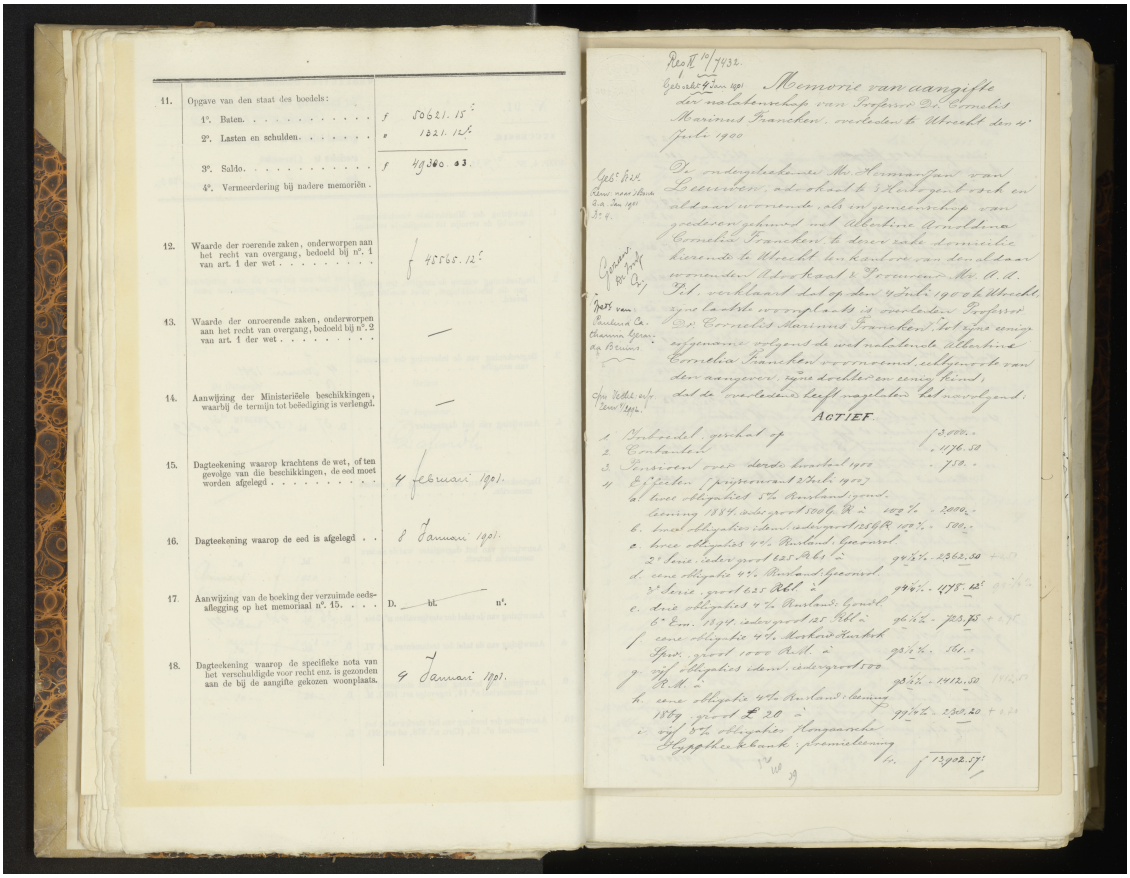

Figure C.2: Second and further pages of a MVS

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.

C.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).

D Robustness Checks & Supplementary Analyses

Table D.1: Conditional Covariate Balance - Second Run

	Margin Within 0.2			Margin Within 0.05			
	Mean Treated	Mean Control	p-value	Mean Treated	Mean Control	p-value	RD Estimate
Panel A: Newspaper Recommendations							
Rec.: Liberal	0.107	-0.054	0.000***	0.166	-0.024	0.009***	0.198* (0.114)
Rec. Socialist	0.000	0.006	0.704	-0.018	0.012	0.361	-0.025 (0.036)
Rec.: Protestant	0.051	0.038	0.685	0.076	0.083	0.901	0.035 (0.098)
Rec. Catholic	-0.008	-0.007	0.956	-0.016	-0.024	0.761	0.003 (0.032)
Panel B: Election Characteristics							
Number of Tries Until Election	-0.296	0.066	0.120	-0.304	0.004	0.459	-0.436 (0.414)
Election Year	4.265	1.136	0.111	5.836	5.405	0.897	0.058 (5.446)
Year of Birth Candidate	4.575	1.603	0.162	5.327	5.749	0.910	-1.417 (5.720)
Log(Turnout)	0.114	0.091	0.335	0.178	0.113	0.177	0.067 (0.075)
Log (Electoral Threshold)	0.256	0.180	0.253	0.315	0.409	0.487	-0.055 (0.194)
Log(Electorate Size)	0.140	0.089	0.411	0.135	0.296	0.227	-0.083 (0.154)
Panel C: District Characteristics							
District Population	0.056	0.137	0.445	-0.045	0.163	0.130	-0.199 (0.194)
% Labor Force Industry District	-0.003	-0.020	0.082*	-0.002	-0.004	0.936	-0.012 (0.030)
% Labor Force Agriculture District	0.002	-0.033	0.005***	0.003	-0.037	0.077*	0.039 (0.031)
% Labor Force Services District	0.001	0.053	0.011**	-0.001	0.041	0.265	-0.022 (0.057)
% Paying Wealth Tax District	-0.143	-0.473	0.068*	-0.227	-0.213	0.962	-0.232 (0.401)
Income Tax Share District	-0.057	-0.295	0.061*	-0.118	-0.060	0.769	-0.180 (0.241)
% Catholic District	-0.009	-0.017	0.705	-0.001	-0.001	0.992	0.017 (0.040)
% Protestant District	0.003	0.019	0.394	0.003	-0.006	0.769	0.001 (0.034)
Distance to the Hague - District	-3.654	-7.578	0.480	-9.817	-5.567	0.669	-0.141 (14.754)
Panel D: Birthplace Characteristics							
% Labor Force Industry Birth Place	0.002	-0.006	0.485	0.001	-0.007	0.773	-0.008 (0.026)
% Labor Force Agriculture Birth Place	0.012	-0.021	0.008***	0.032	-0.034	0.001***	0.041* (0.024)
% Labor Force Services Birth Place	-0.014	0.027	0.054*	-0.034	0.040	0.066*	-0.032 (0.042)
% Catholic Birth Place	-0.029	0.008	0.200	-0.021	0.012	0.562	-0.020 (0.068)
% Protestant Birth Place	0.025	-0.005	0.269	0.020	-0.016	0.499	0.025 (0.063)
Distance to The Hague - BP	0.145	-4.828	0.407	3.825	1.597	0.850	0.463 (15.161)

Note: 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 exactly 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 ¡ 0.1, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.2: Conditional Covariate Balance - Third Run

	Margin Within 0.2			Margin Within 0.05			
	Mean Treated	Mean Control	p-value	Mean Treated	Mean Control	p-value	RD Estimate
Panel A: Newspaper Recommendations							
Rec.: Liberal	0.072	0.027	0.322	0.112	0.069	0.616	0.052 (0.147)
Rec. Socialist	0.025	-0.016	0.033**	0.037	-0.009	0.196	0.037 (0.047)
Rec.: Protestant	-0.008	0.083	0.009***	0.024	-0.003	0.675	0.017 (0.093)
Rec. Catholic	0.010	0.011	0.938	0.041	0.059	0.679	-0.013 (0.078)
Panel B: Election Characteristics							
Number of Tries Until Election	-0.146	0.113	0.383	-0.299	-0.531	0.626	0.145 (0.727)
Election Year	2.788	2.165	0.762	3.646	2.593	0.742	-1.860 (5.647)
Year of Birth Candidate	2.896	2.158	0.739	4.031	2.723	0.719	-2.126 (5.936)
Log(Turnout)	0.104	0.101	0.937	0.197	0.139	0.175	0.057 (0.069)
Log (Electoral Threshold)	0.214	0.201	0.855	0.319	0.300	0.880	-0.116 (0.190)
Log(Electorate Size)	0.109	0.097	0.849	0.118	0.159	0.750	-0.162 (0.200)
Panel C: District Characteristics							
District Population	0.151	0.134	0.846	0.160	0.178	0.904	0.025 (0.215)
% Labor Force Industry District	-0.020	-0.012	0.442	-0.007	-0.015	0.717	0.008 (0.023)
% Labor Force Agriculture District	-0.026	-0.007	0.200	-0.014	-0.023	0.751	0.004 (0.037)
% Labor Force Services District	0.047	0.019	0.248	0.022	0.038	0.713	-0.017 (0.055)
% Paying Wealth Tax District	-0.281	-0.438	0.438	-0.046	-0.488	0.213	0.404 (0.472)
Income Tax Share District	-0.141	-0.321	0.167	-0.019	-0.307	0.191	0.258 (0.326)
% Catholic District	-0.029	-0.015	0.505	-0.049	0.039	0.046**	-0.087 (0.058)
% Protestant District	0.032	0.011	0.308	0.055	-0.023	0.053*	0.089* (0.052)
Distance to the Hague - District	-11.143	-5.225	0.354	-7.783	-11.464	0.720	9.026 (16.324)
Panel D: Birthplace Characteristics							
% Labor Force Industry Birth Place	-0.018	0.007	0.047**	-0.003	0.014	0.540	0.004 (0.034)
% Labor Force Agriculture Birth Place	-0.013	0.007	0.226	-0.034	-0.003	0.183	-0.001 (0.020)
% Labor Force Services Birth Place	0.031	-0.014	0.079*	0.037	-0.011	0.278	0.000 (0.053)
% Catholic Birth Place	-0.014	-0.001	0.708	-0.039	0.044	0.177	-0.053 (0.053)
% Protestant Birth Place	0.011	0.004	0.840	0.040	-0.034	0.209	0.054 (0.049)
Distance to The Hague - BP	-3.300	2.950	0.367	-16.744	6.062	0.071*	-20.938 (15.625)

Note: 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 exactly twice. 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 ¡ 0.1, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.3: Descriptive Statistics

	Mean	SD	Min	Max	N
Panel A: Party Affiliation					
Party: Catholic	0.12	0.32	0.00	1.00	6181
Party: Protestant	0.34	0.47	0.00	1.00	6181
Party: Liberal	0.46	0.50	0.00	1.00	6181
Party: Socialist	0.09	0.28	0.00	1.00	6181
Panel B: Newspaper Recommendations					
Rec.: Protestant	0.15	0.36	0.00	1.00	8496
Rec.: Liberal	0.18	0.38	0.00	1.00	8496
Rec.: Socialist	0.07	0.26	0.00	1.00	8496
Rec.: Catholic	0.06	0.24	0.00	1.00	8496
Panel C: Candidate-Election Characteristics					
Age at Election	49.34	10.36	21.00	117.00	6679
Year of Election	1882.08	20.63	1848.00	1918.00	8496
Number of Tries Until Election	3.09	3.76	0.00	25.00	8496
Election HHI	0.43	0.17	0.05	1.00	8372
Electoral Threshold	7.06	0.76	4.90	9.08	8372
Electorate Size	8.21	0.68	5.83	10.22	8496
Turnout In Candidates Election (% of Electorate)	0.68	0.17	0.16	0.98	8372
Turnout in Candidate Previous Election	0.67	0.17	0.16	0.98	6297
Panel D: District Characteristics					
Log Population District	11.34	1.14	0.00	13.38	8496
Share Protestant District	0.59	0.23	0.00	0.97	8442
Share Catholic District	0.34	0.26	0.00	1.00	8442
Labor Force Share Agriculture District	0.17	0.12	0.00	0.43	8264
Labor Force Share Industry District	0.42	0.09	0.27	0.68	8264
Labor Force Share Services District	0.41	0.18	0.08	0.72	8264
Wealth Tax Revenue	3590.91	3308.04	0.00	13 406.00	8496
District Paying Income Tax	6391.36	5722.32	0.00	26 840.00	8496
Distance to The Hague - District	87.37	58.63	0.00	216.12	8491
Panel E: Birthplace Characteristics					
Labor Force Share Agriculture Birthplace	0.08	0.11	0.00	0.44	3957
Labor Force Share Industry Birthplace	0.36	0.09	0.26	0.73	3957
Labor Force Share Services Birthplace	0.56	0.17	0.06	0.72	3957
Share Protestant Birthplace	0.58	0.26	0.00	1.00	4955
Share Catholic Birthplace	0.40	0.27	0.00	1.00	4955
Distance to The Hague - Birthplace	78.42	55.97	0.00	218.85	5455
Panel F: Dependent Variables					
Lifespan	22.48	13.18	−57.19	76.93	7177
Net Wealth (Deflated, Log)	10.40	3.47	0.00	15.09	4298
Inheritance (Deflated, Log)	9.39	3.33	0.00	15.71	1161
Career: Politics	0.53	0.50	0.00	1.00	5526
Career: Nat. Politics	0.34	0.47	0.00	1.00	5526
Career: Prov. Politics	0.22	0.42	0.00	1.00	5526
Career: Municipal Politics	0.12	0.32	0.00	1.00	5526
Career: Non-Politics	0.09	0.29	0.00	1.00	5526

Note: 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.

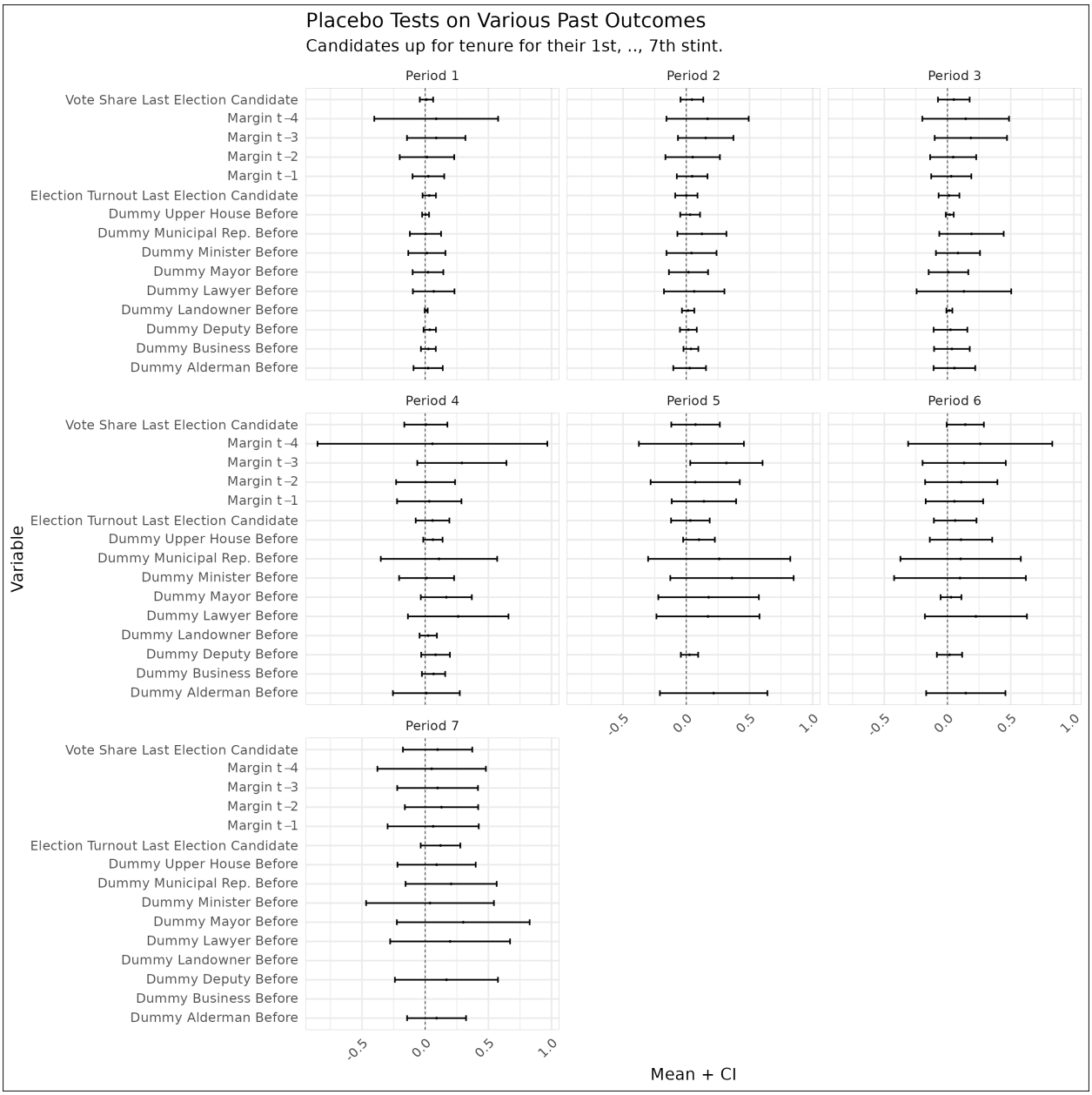

Figure D.1: Placebo Test for Past Values of Dependent Variables

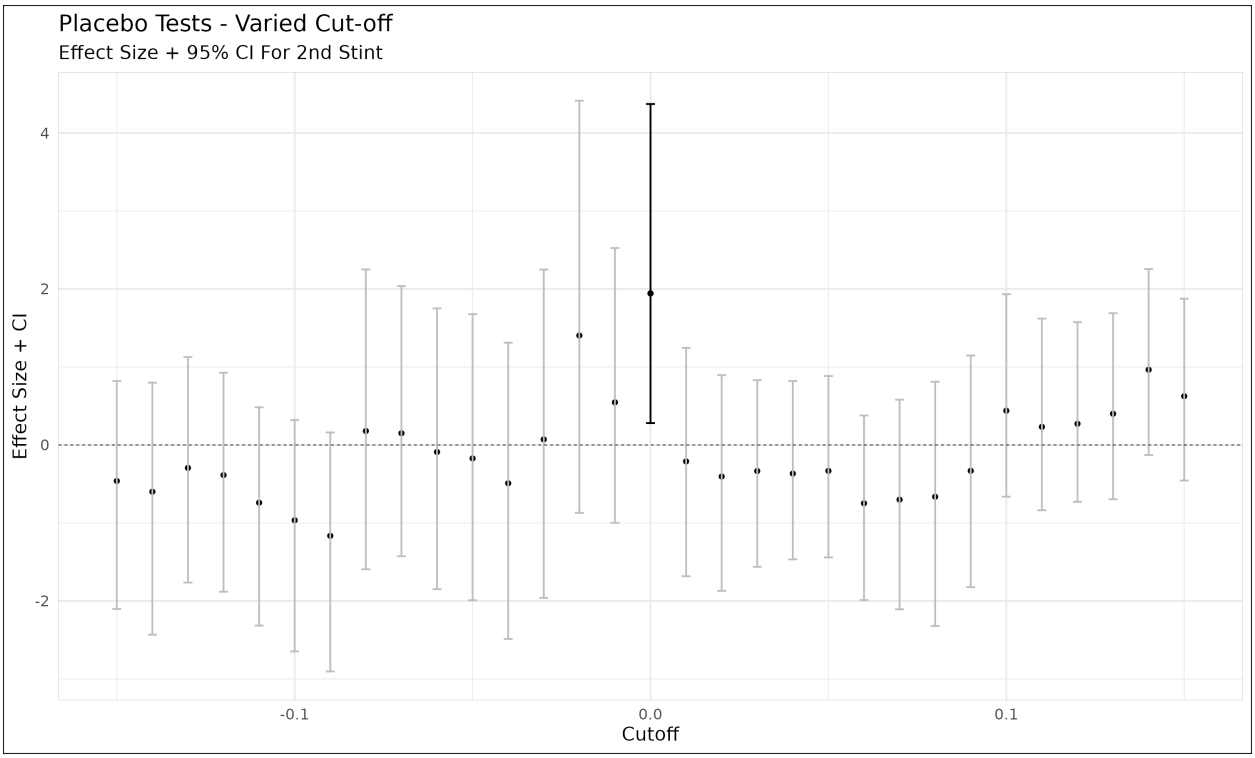

Figure D.2: Placebo Test Cut-Off

Table D.4: ATT and ITT estimates for different t*: Robustness to Bandwidth

	t = 1	t = 2	t = 3	t = 4	t = 5	t = 6	t = 7
Panel A: t* = 4							
Coefficient (ATT)	0.383	1.992**	-0.202	-0.512			
SE (ATT)	(0.453)	(0.912)	(0.477)	(0.593)			
Coefficient (ITT)	0.657**	1.996***	-0.257	-0.512**			
SE (ITT)	(0.438)	(0.910)	(0.473)	(0.593)			
Mean DV Treated	11.206	11.501	11.596	11.551			
Mean DV Control	10.986	10.335	11.951	11.600			
N (Treated)	342	259	202	173			
N (Control)	681	204	120	122			
Bandwidth	0.150	0.150	0.150	0.150			
Panel B: t* = 7							
Coefficient (ATT)	0.144	1.965**	-0.300	-0.505	-0.135	-1.304	0.613
SE (ATT)	(0.514)	(0.925)	(0.519)	(0.613)	(0.721)	(2.013)	(0.853)
Coefficient (ITT)	0.657**	1.996***	-0.257	-0.512**	-0.306	-1.238	0.613
SE (ITT)	(0.438)	(0.910)	(0.473)	(0.593)	(0.687)	(2.011)	(0.853)
Mean DV Treated	11.206	11.501	11.596	11.551	12.074	11.907	11.630
Mean DV Control	10.986	10.335	11.951	11.600	11.208	12.988	10.828
N (Treated)	342	259	202	173	123	88	63
N (Control)	681	204	120	122	66	61	37
Bandwidth	0.150	0.150	0.150	0.150	0.150	0.150	0.150

Note: Table showing coefficient estimates of the effect of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under different $t^* \in \{4, 7\}$. All the ATT coefficients are derived and recursively computed from ITT coefficients, which are in turn estimated using the methodology in (Cattaneo et al., 2019) using a custom bandwidth. Standard errors for the ATT estimates are derived using the delta method. The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party and district fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.5: ATT and ITT estimates for different t*: Robustness to Bandwidth (Small)

	t = 1	t = 2	t = 3	t = 4	t = 5	t = 6	t = 7
Panel A: t* = 4							
Coefficient (ATT)	0.820	2.698**	-0.176	-1.295*			
SE (ATT)	(0.588)	(1.069)	(0.569)	(0.672)			
Coefficient (ITT)	1.244**	2.746***	-0.315	-1.295**			
SE (ITT)	(0.572)	(1.067)	(0.564)	(0.672)			
Mean DV Treated	11.206	11.501	11.596	11.551			
Mean DV Control	10.986	10.335	11.951	11.600			
N (Treated)	342	259	202	173			
N (Control)	681	204	120	122			
Bandwidth	0.080	0.080	0.080	0.080			
Panel B: t* = 7							
Coefficient (ATT)	0.650	2.600**	-0.352	-1.403*	0.049	-1.962	-0.017
SE (ATT)	(0.711)	(1.107)	(0.703)	(0.723)	(1.272)	(4.189)	(0.804)
Coefficient (ITT)	1.244**	2.746***	-0.315	-1.295**	-0.161	-1.964	-0.017
SE (ITT)	(0.572)	(1.067)	(0.564)	(0.672)	(1.189)	(4.188)	(0.804)
Mean DV Treated	11.206	11.501	11.596	11.551	12.074	11.907	11.630
Mean DV Control	10.986	10.335	11.951	11.600	11.208	12.988	10.828
N (Treated)	342	259	202	173	123	88	63
N (Control)	681	204	120	122	66	61	37
Bandwidth	0.080	0.080	0.080	0.080	0.080	0.080	0.080

Note: Table showing coefficient estimates of the effect of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under different $t^* \in \{4, 7\}$. All the ATT coefficients are derived and recursively computed from ITT coefficients, which are in turn estimated using the methodology in (Cattaneo et al., 2019) using a custom bandwidth. Standard errors for the ATT estimates are derived using the delta method. The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party and district fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.6: ATT and ITT estimates for different t*: Robustness to Kernel

	t = 1	t = 2	t = 3	t = 4	t = 5	t = 6	t = 7
Panel A: Kernel: Epanechnikov							
Coefficient (ATT)	0.012	1.718**	-0.200	-0.199	-0.156	-0.610	0.738
SE (ATT)	(0.436)	(0.873)	(0.388)	(0.462)	(0.536)	(1.133)	(0.764)
Coefficient (ITT)	0.429	1.694**	-0.198	-0.255	-0.259	-0.531	0.738
SE (ITT)	(0.384)	(0.865)	(0.366)	(0.449)	(0.520)	(1.130)	(0.764)
Mean DV Treated	11.206	11.501	11.596	11.551	12.074	11.907	11.630
Mean DV Control	10.986	10.335	11.951	11.600	11.208	12.988	10.828
N (Treated)	342	259	202	173	123	88	63
N (Control)	681	204	120	122	66	61	37
Bandwidth	0.198	0.167	0.277	0.215	0.215	0.195	0.317
Panel B: Kernel: Uniform							
Coefficient (ATT)	-0.030	1.411*	-0.232	-0.234	-0.156	-0.587	0.604
SE (ATT)	(0.414)	(0.828)	(0.395)	(0.473)	(0.460)	(0.991)	(0.799)
Coefficient (ITT)	0.332	1.396*	-0.227	-0.278	-0.250	-0.523	0.604
SE (ITT)	(0.359)	(0.820)	(0.376)	(0.461)	(0.446)	(0.987)	(0.799)
Mean DV Treated	11.411	11.722	11.772	11.717	12.300	12.040	11.848
Mean DV Control	11.229	10.521	12.120	11.773	11.293	13.304	11.125
N (Treated)	342	259	202	173	123	88	63
N (Control)	681	204	120	122	66	61	37
Bandwidth	0.203	0.166	0.171	0.171	0.255	0.193	0.241

Note: Table showing coefficient estimates of the effect of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under different $t^* \in \{4, 7\}$. All the ATT coefficients are derived and recursively computed from ITT coefficients, which are in turn estimated using the methodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth and a non-standard kernel. Standard errors for the ATT estimates are derived using the delta method. The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party and district fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.7: ATT and ITT estimates for different t*: Robustness to p and q

	t = 1	t = 2	t = 3	t = 4	t = 5	t = 6	t = 7
Panel A: t* = 7							
Coefficient (ATT)	0.784	2.746**	-0.399	-1.759**	-0.029	-1.935	-0.118
SE (ATT)	(0.787)	(1.142)	(0.779)	(0.786)	(1.298)	(4.968)	(0.846)
Coefficient (ITT)	1.413**	2.919***	-0.393	-1.652**	-0.231	-1.948	-0.118
SE (ITT)	(0.621)	(1.091)	(0.603)	(0.726)	(1.182)	(4.967)	(0.846)
Mean DV Treated	11.206	11.501	11.596	11.551	12.074	11.907	11.630
Mean DV Control	10.986	10.335	11.951	11.600	11.208	12.988	10.828
N (Treated)	342	259	202	173	123	88	63
N (Control)	681	204	120	122	66	61	37
Bandwidth	0.150	0.150	0.150	0.150	0.150	0.150	0.150
Panel B: t* = 7							
Coefficient (ATT)	1.062	2.788**	-0.384	-1.692*	-0.060	-4.180	-0.887
SE (ATT)	(1.175)	(1.345)	(1.199)	(0.887)	(1.921)	(10.156)	(1.344)
Coefficient (ITT)	1.735**	3.160***	-0.108	-1.399	-0.464	-4.276	-0.887
SE (ITT)	(0.719)	(1.178)	(0.673)	(0.675)	(1.579)	(10.155)	(1.344)
Mean DV Treated	11.411	11.722	11.772	11.717	12.300	12.040	11.848
Mean DV Control	11.229	10.521	12.120	11.773	11.293	13.304	11.125
N (Treated)	342	259	202	173	123	88	63
N (Control)	681	204	120	122	66	61	37
Bandwidth	0.150	0.150	0.150	0.150	0.150	0.150	0.150
Panel C: t* = 7							
Coefficient (ATT)	0.747	2.737**	-0.266	-1.658**	0.129	-2.301	-0.044
SE (ATT)	(0.782)	(1.139)	(0.737)	(0.808)	(1.294)	(4.916)	(0.900)
Coefficient (ITT)	1.395**	2.919***	-0.226	-1.522**	-0.116	-2.306	-0.044
SE (ITT)	(0.614)	(1.088)	(0.552)	(0.750)	(1.180)	(4.915)	(0.900)
Mean DV Treated	11.411	11.722	11.772	11.717	12.300	12.040	11.848
Mean DV Control	11.229	10.521	12.120	11.773	11.293	13.304	11.125
N (Treated)	342	259	202	173	123	88	63
N (Control)	681	204	120	122	66	61	37
Bandwidth	0.150	0.150	0.150	0.150	0.150	0.150	0.150

Note: Table showing coefficient estimates of the effect of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under different $t^* \in \{4, 7\}$. All the ATT coefficients are derived and recursively computed from ITT coefficients, which are in turn estimated using the methodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method. The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party and district fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.8: ATT and ITT estimates for different t*: Robustness to Clustering

	t = 1	t = 2	t = 3	t = 4	t = 5	t = 6	t = 7
Panel A: t* = 4							
Coefficient (ATT)	0.268	1.797**	-0.167	-0.372			
SE (ATT)	(0.378)	(0.722)	(0.344)	(0.503)			
Coefficient (ITT)	0.506	1.798**	-0.207	-0.372			
SE (ITT)	(0.367)	(0.721)	(0.340)	(0.503)			
Mean DV Treated	11.206	11.501	11.596	11.551			
Mean DV Control	10.986	10.335	11.951	11.600			
N (Treated)	342	259	202	173			
N (Control)	681	204	120	122			
Bandwidth	0.200	0.174	0.293	0.189			
Panel B: t* = 7							
Coefficient (ATT)	0.058	1.813**	-0.202	-0.317	-0.161	-0.676	0.754
SE (ATT)	(0.420)	(0.731)	(0.372)	(0.516)	(0.548)	(1.348)	(0.718)
Coefficient (ITT)	0.506	1.798**	-0.207	-0.372	-0.272	-0.595	0.754
SE (ITT)	(0.367)	(0.721)	(0.340)	(0.503)	(0.527)	(1.346)	(0.718)
Mean DV Treated	11.206	11.501	11.596	11.551	12.074	11.907	11.630
Mean DV Control	10.986	10.335	11.951	11.600	11.208	12.988	10.828
N (Treated)	342	259	202	173	123	88	63
N (Control)	681	204	120	122	66	61	37
Bandwidth	0.200	0.174	0.293	0.189	0.229	0.202	0.374

Note: Table showing coefficient estimates of the effect of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under different $t^* \in \{4, 7\}$. All the ATT coefficients are derived andrecursively computed from ITT coefficients, which are in turn estimated using themethodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method and standard errors for the ITT estimates are clustered at the district-year level.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party and district fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.9: ATT and ITT estimates for different t*: Robustness to Incumbency Advantages

	t = 1	t = 2	t = 3	t = 4	t = 5	t = 6	t = 7
Panel A: t* = 4							
Coefficient (ATT)	0.423	1.708**	-0.214	-0.359			
SE (ATT)	(0.394)	(0.867)	(0.361)	(0.496)			
Coefficient (ITT)	0.492	1.751**	-0.215	-0.359			
SE (ITT)	(0.388)	(0.865)	(0.361)	(0.496)			
Mean DV Treated	11.206	11.501	11.596	11.551			
Mean DV Control	10.986	10.335	11.951	11.600			
N (Treated)	342	259	202	173			
N (Control)	681	204	120	122			
Bandwidth	0.209	0.180	0.301	0.195			
Panel B: t* = 7							
Coefficient (ATT)	0.254	1.762**	-0.277	-0.348	-0.184	-0.609	0.754
SE (ATT)	(0.431)	(0.875)	(0.393)	(0.523)	(0.538)	(1.230)	(0.729)
Coefficient (ITT)	0.492	1.751**	-0.215	-0.359	-0.275	-0.607	0.754
SE (ITT)	(0.388)	(0.865)	(0.361)	(0.496)	(0.531)	(1.230)	(0.729)
Mean DV Treated	11.206	11.501	11.596	11.551	12.074	11.907	11.630
Mean DV Control	10.986	10.335	11.951	11.600	11.208	12.988	10.828
N (Treated)	342	259	202	173	123	88	63
N (Control)	681	204	120	122	66	61	37
Bandwidth	0.209	0.180	0.301	0.195	0.232	0.198	0.373

Note: Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under different $t^* \in \{4, 7\}$. All the ATT coefficients are derived andrecursively computed from ITT coefficients, which are in turn estimated using the methodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party and district fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.10: ATT and ITT estimates for different transformations of DV

	t = 1	t = 2	t = 3	t = 4
Panel A: Personal Wealth/100				
Coefficient (ATT)	0.012	1.477**	-0.447	-0.159
SE (ATT)	(0.367)	(0.586)	(0.761)	(0.491)
N (Treated)	371	279	214	181
N (Control)	710	217	158	128
Panel B: Personal Wealth/1000				
Coefficient (ATT)	0.017	1.095**	-0.335	-0.198
SE (ATT)	(0.305)	(0.448)	(0.595)	(0.431)
N (Treated)	371	279	214	181
N (Control)	710	217	158	128
Panel C: Personal Wealth/10000				
Coefficient (ATT)	0.008	0.810**	-0.196	-0.217
SE (ATT)	(0.231)	(0.324)	(0.418)	(0.332)
N (Treated)	371	279	214	181
N (Control)	710	217	158	128
Panel D: Personal Wealth/100000				
Coefficient (ATT)	-0.001	0.561***	-0.121	-0.122
SE (ATT)	(0.138)	(0.196)	(0.231)	(0.195)
N (Treated)	371	279	214	181
N (Control)	710	217	158	128

Note: Table showing coefficient estimates of the effect of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under different $t^* \in \{4, 7\}$. All the ATT coefficients are derived andrecursively computed from ITT coefficients, which are in turn estimated using themethodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party and district fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.11: Poisson QMLE Analysis of Returns to Political Office

Period	1st	2nd	3rd	4th	5th	6th
	(1)	(2)	(3)	(4)	(5)	(6)
Elected	0.125	1.679***	−0.356	0.062	0.016	−0.325
	(0.251)	(0.394)	(0.226)	(0.207)	(0.421)	(0.555)
No. Tries Until Now	−0.177***	−0.020	−0.062	0.010	−0.094	0.040
	(0.066)	(0.058)	(0.055)	(0.044)	(0.064)	(0.038)
Margin	1.219	−1.286	2.189	−1.171	0.279	−0.247
	(0.960)	(0.872)	(1.843)	(0.858)	(1.352)	(0.632)
Margin x Elected	−3.469**	−4.297*	−1.948	2.121*	0.068	0.014
	(1.597)	(2.387)	(2.025)	(1.161)	(1.729)	(2.024)
Age at Election	0.035**	−0.013	−0.002	−0.029	−0.018	−0.128***
	(0.015)	(0.016)	(0.023)	(0.018)	(0.024)	(0.034)
Rec. Catholic	−0.968**	1.867*	−0.197	1.059***	1.293	−4.215***
	(0.418)	(1.050)	(0.389)	(0.377)	(0.865)	(1.595)
Rec. Liberal	−0.471	0.018	−0.737*	−0.084	0.675	0.450
	(0.295)	(0.283)	(0.406)	(0.360)	(0.524)	(0.529)
Rec. Socialist	−0.666	−2.651***	−0.243	−0.690*	−1.731*	0.428
	(0.610)	(0.524)	(0.558)	(0.414)	(0.886)	(0.601)
Year	−0.004	0.001	0.004	−0.012	−0.011	0.026
	(0.008)	(0.011)	(0.014)	(0.014)	(0.015)	(0.017)
N	948	456	348	281	181	140
ATT (Percentage)	0.13	4.36	−0.30	0.06	0.02	−0.28
District FE	✓	✓	✓	✓	✓	✓
Party FE	✓	✓	✓	✓	✓	✓

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

Note: Results show Poisson QMLE estimates of being elected on personal wealth for the 1st, ..., 6th term of political office. To mimic an RD approach, the estimates are weighted by 1/—Margin—. The dependent variable is deflated end-of-life wealth. Standard errors are clustered at the candidate level.

Table D.12: Dynamic Reuslts: District-Year FE

	t = 1	t = 2	t = 3	t = 4	t = 5	t = 6	t = 7
Panel A: t* = 4							
Coefficient (ATT)	0.485*	0.763*	-0.104	-0.323			
SE (ATT)	(0.257)	(0.458)	(0.236)	(0.246)			
Coefficient (ITT)	0.604**	0.768*	-0.139	-0.323			
SE (ITT)	(0.251)	(0.457)	(0.234)	(0.246)			
Mean DV Treated	11.216	11.549	11.596	11.551			
Mean DV Control	10.731	10.335	11.951	11.600			
N (Treated)	347	260	202	173			
N (Control)	950	210	120	122			
Bandwidth	0.239	0.166	0.246	0.183			
Panel B: t* = 7							
Coefficient (ATT)	0.516**	0.782*	-0.070	-0.300	-0.024	0.383**	0.012
SE (ATT)	(0.257)	(0.458)	(0.236)	(0.246)	(0.075)	(0.162)	(0.020)
Coefficient (ITT)	0.604**	0.768*	-0.139	-0.323	0.016	0.385**	0.012
SE (ITT)	(0.251)	(0.457)	(0.234)	(0.246)	(0.073)	(0.162)	(0.020)
Mean DV Treated	11.216	11.549	11.596	11.551	12.074	11.907	11.630
Mean DV Control	10.731	10.335	11.951	11.600	11.208	12.988	10.828
N (Treated)	347	260	202	173	123	88	63
N (Control)	950	210	120	122	66	61	37
Bandwidth	0.239	0.166	0.246	0.183	0.208	0.237	0.218

Note: Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under different $t^* \in \{4, 7\}$. All the ATT coefficients are derived andrecursively computed from ITT coefficients, which are in turn estimated using the methodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on district-year fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.13: ATT and ITT estimates for different t*: Only Party FE

	t = 1	t = 2	t = 3	t = 4	t = 5	t = 6	t = 7
Panel A: t* = 4							
Coefficient (ATT)	0.161	1.346	-0.239	-0.390			
SE (ATT)	(0.461)	(0.865)	(0.484)	(0.538)			
Coefficient (ITT)	0.356	1.341*	-0.281	-0.390			
SE (ITT)	(0.448)	(0.863)	(0.480)	(0.538)			
Mean DV Treated	11.206	11.501	11.596	11.551			
Mean DV Control	10.986	10.335	11.951	11.600			
N (Treated)	342	259	202	173			
N (Control)	681	204	120	122			
Bandwidth	0.214	0.204	0.262	0.221			
Panel B: t* = 7							
Coefficient (ATT)	0.067	1.336	-0.292	-0.443	0.235	-0.834	0.156
SE (ATT)	(0.508)	(0.875)	(0.506)	(0.554)	(0.679)	(1.366)	(0.901)
Coefficient (ITT)	0.356	1.341*	-0.281	-0.390	0.137	-0.818	0.156
SE (ITT)	(0.448)	(0.863)	(0.480)	(0.538)	(0.662)	(1.362)	(0.901)
Mean DV Treated	11.206	11.501	11.596	11.551	12.074	11.907	11.630
Mean DV Control	10.986	10.335	11.951	11.600	11.208	12.988	10.828
N (Treated)	342	259	202	173	123	88	63
N (Control)	681	204	120	122	66	61	37
Bandwidth	0.214	0.204	0.262	0.221	0.304	0.180	0.350

Note: Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under different $t^* \in \{4, 7\}$. All the ATT coefficients are derived andrecursively computed from ITT coefficients, which are in turn estimated using themethodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.14: ATT and ITT estimates for different t*: Robustness to Controls

	t = 1	t = 2	t = 3	t = 4	t = 5	t = 6	t = 7
Panel A: t* = 4							
Coefficient (ATT)	0.445	1.551*	-0.188	-0.270			
SE (ATT)	(0.402)	(0.858)	(0.374)	(0.455)			
Coefficient (ITT)	0.647	1.544*	-0.217	-0.270			
SE (ITT)	(0.388)	(0.857)	(0.371)	(0.455)			
Mean DV Treated	11.270	11.489	11.596	11.551			
Mean DV Control	10.986	10.335	11.951	11.600			
N (Treated)	330	250	196	168			
N (Control)	665	196	118	120			
Bandwidth	0.216	0.180	0.261	0.213			
Panel B: t* = 7							
Coefficient (ATT)	0.387	1.554*	-0.202	-0.266	0.010	-0.260	0.183
SE (ATT)	(0.434)	(0.865)	(0.397)	(0.467)	(0.521)	(1.297)	(0.602)
Coefficient (ITT)	0.647	1.544*	-0.217	-0.270	-0.028	-0.240	0.183
SE (ITT)	(0.388)	(0.857)	(0.371)	(0.455)	(0.501)	(1.295)	(0.602)
Mean DV Treated	11.270	11.489	11.596	11.551	12.074	11.907	11.630
Mean DV Control	10.986	10.335	11.951	11.600	11.050	12.988	10.466
N (Treated)	330	250	196	168	119	87	61
N (Control)	665	196	118	120	62	59	35
Bandwidth	0.216	0.180	0.261	0.213	0.269	0.192	0.354

Note: Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under different $t^* \in \{4, 7\}$. All the ATT coefficients are derived and recursively computed from ITT coefficients, which are in turn estimated using the methodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method. The estimates in both panels control for age at election, religious composition, industry composition, year of election, and newspaper recommendations. The estimates are conditional on party and ditrict fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.15: ATT and ITT estimates for different t*: Robustness for Winsorization

	t = 1	t = 2	t = 3	t = 4	t = 5	t = 6	t = 7
Panel A: t* = 4							
Coefficient (ATT)	0.260	1.751**	-0.176	-0.359			
SE (ATT)	(0.402)	(0.866)	(0.365)	(0.496)			
Coefficient (ITT)	0.492	1.751**	-0.215	-0.359			
SE (ITT)	(0.388)	(0.865)	(0.361)	(0.496)			
Mean DV Treated	11.206	11.501	11.596	11.551			
Mean DV Control	10.986	10.335	11.951	11.600			
N (Treated)	342	259	202	173			
N (Control)	681	204	120	122			
Bandwidth	0.209	0.180	0.301	0.195			
Panel B: t* = 7							
Coefficient (ATT)	0.049	1.766**	-0.212	-0.305	-0.163	-0.688	0.754
SE (ATT)	(0.440)	(0.873)	(0.388)	(0.508)	(0.549)	(1.232)	(0.729)
Coefficient (ITT)	0.492	1.751**	-0.215	-0.359	-0.275	-0.607	0.754
SE (ITT)	(0.388)	(0.865)	(0.361)	(0.496)	(0.531)	(1.230)	(0.729)
Mean DV Treated	11.206	11.501	11.596	11.551	12.074	11.907	11.630
Mean DV Control	10.986	10.335	11.951	11.600	11.208	12.988	10.828
N (Treated)	342	259	202	173	123	88	63
N (Control)	681	204	120	122	66	61	37
Bandwidth	0.209	0.180	0.301	0.195	0.232	0.198	0.373

Note: Table showing coefficient estimates of the effect of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under different $t^* \in \{4, 7\}$. All the ATT coefficients are derived andrecursively computed from ITT coefficients, which are in turn estimated using themethodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party and district fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.16: ATT and ITT estimates for t*=7: Robustness to Definitions

	t = 1	t = 2	t = 3	t = 4	t = 5	t = 6	t = 7
Panel A: Log NW0 (Start of Probate Inventory)							
Coefficient (ATT)	0.010	1.751**	-0.216	-0.284	-0.139	-0.822	0.667
SE (ATT)	(0.432)	(0.873)	(0.371)	(0.517)	(0.542)	(1.198)	(0.699)
Coefficient (ITT)	0.443	1.747**	-0.199	-0.321	-0.262	-0.751	0.667
SE (ITT)	(0.381)	(0.865)	(0.344)	(0.506)	(0.525)	(1.195)	(0.699)
Mean DV Treated	11.411	11.722	11.772	11.717	12.300	12.040	11.848
Mean DV Control	11.229	10.521	12.120	11.773	11.293	13.304	11.125
N (Treated)	342	259	202	173	123	88	63
N (Control)	681	204	120	122	66	61	37
Bandwidth	0.213	0.179	0.301	0.190	0.235	0.203	0.373
Panel B: Log NW11 (End of Probate Inventory							
Coefficient (ATT)	0.081	1.881**	-0.143	-0.236	-0.023	-0.821	0.669
SE (ATT)	(0.436)	(0.875)	(0.371)	(0.573)	(0.587)	(1.200)	(0.698)
Coefficient (ITT)	0.514	1.871**	-0.127	-0.261	-0.146	-0.749	0.669
SE (ITT)	(0.385)	(0.867)	(0.343)	(0.562)	(0.572)	(1.198)	(0.698)
Mean DV Treated	11.398	11.715	11.763	11.713	12.283	12.039	11.405
Mean DV Control	11.153	10.470	12.056	11.758	11.383	13.304	11.125
N (Treated)	343	260	202	173	123	88	63
N (Control)	670	204	120	122	66	61	37
Bandwidth	0.206	0.176	0.291	0.169	0.237	0.203	0.374
Panel C: Log NW0 Deflated Wealth (Negatives Included)							
Coefficient (ATT)	-0.173	2.284**	-0.646	-0.050	-0.080	-0.549	0.744
SE (ATT)	(0.539)	(0.912)	(1.103)	(0.662)	(0.801)	(1.337)	(0.853)
Coefficient (ITT)	0.305	2.193***	-0.638	-0.102	-0.177	-0.469	0.744
SE (ITT)	(0.481)	(0.894)	(1.091)	(0.647)	(0.787)	(1.334)	(0.853)
Mean DV Treated	10.659	10.432	10.100	11.551	11.320	11.907	11.630
Mean DV Control	10.866	10.040	10.336	10.379	11.208	12.988	9.281
N (Treated)	371	279	214	181	131	93	67
N (Control)	710	217	158	128	68	63	41
Bandwidth	0.216	0.153	0.221	0.198	0.278	0.184	0.324
Panel D: Ihs NW0 Deflated Wealth (Negatives Included)							
Coefficient (ATT)	-0.173	2.250**	-0.639	-0.064	-0.086	-0.552	0.743
SE (ATT)	(0.533)	(0.898)	(1.089)	(0.654)	(0.795)	(1.334)	(0.849)
Coefficient (ITT)	0.303	2.162***	-0.632	-0.117	-0.184	-0.472	0.743
SE (ITT)	(0.475)	(0.880)	(1.077)	(0.640)	(0.781)	(1.331)	(0.849)
Mean DV Treated	11.366	11.142	10.817	12.244	12.025	12.601	12.323
Mean DV Control	11.568	10.754	11.055	11.092	11.901	13.681	10.001
N (Treated)	371	279	214	181	131	93	67
N (Control)	710	217	158	128	68	63	41
Bandwidth	0.216	0.153	0.221	0.199	0.275	0.184	0.325

Note: Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under different $t^* \in \{4, 7\}$. All the ATT coefficients are derived andrecursively computed from ITT coefficients, which are in turn estimated using the methodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party and district fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.17: ATT and ITT estimates for different t*, Unique Sample

	t = 1	t = 2	t = 3	t = 4	t = 5	t = 6	t = 7
Panel A: t* = 4							
Coefficient (ATT)	-0.037	3.644*	-0.498	-0.207			
SE (ATT)	(0.527)	(2.090)	(0.598)	(0.644)			
Coefficient (ITT)	0.371	3.590*	-0.521	-0.207			
SE (ITT)	(0.475)	(2.088)	(0.594)	(0.644)			
Mean DV Treated	11.904	11.427	11.353	10.464			
Mean DV Control	11.257	9.027	11.915	12.277			
N (Treated)	179	198	145	126			
N (Control)	276	107	64	59			
Bandwidth	0.218	0.154	0.265	0.267			
Panel B: t* = 7							
Coefficient (ATT)	-0.647	3.801*	-0.622	-0.201	0.150	-1.985	0.416
SE (ATT)	(0.722)	(2.096)	(0.609)	(0.648)	(0.622)	(1.909)	(0.670)
Coefficient (ITT)	0.371	3.590*	-0.521	-0.207	-0.066	-1.940	0.416
SE (ITT)	(0.475)	(2.088)	(0.594)	(0.644)	(0.587)	(1.908)	(0.670)
Mean DV Treated	11.904	11.427	11.353	10.464	11.874	11.079	11.576
Mean DV Control	11.257	9.027	11.915	12.277	10.951	13.378	11.401
N (Treated)	179	198	145	126	99	65	52
N (Control)	276	107	64	59	44	32	25
Bandwidth	0.218	0.154	0.265	0.267	0.240	0.216	0.377

Note: Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under different $t^* \in \{4, 7\}$. All the ATT coefficients are derived andrecursively computed from ITT coefficients, which are in turn estimated using themethodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method. Standard errors for the ITT estimates are clustered at the election level.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party and district fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.18: Robustness Check Inheritance: ATT and ITT estimates for different t*

	t = 1	t = 2	t = 3	t = 4	t = 5	t = 6	t = 7
Panel A: t* = 4							
Coefficient (ATT)	0.379	3.701**	-1.151	-1.212			
SE (ATT)	(1.332)	(1.579)	(1.388)	(1.631)			
Coefficient (ITT)	0.953	3.639**	-1.281	-1.212			
SE (ITT)	(1.310)	(1.569)	(1.377)	(1.631)			
Mean DV Treated	1.547	1.413	2.106	2.647			
Mean DV Control	0.646	2.466	0.798	2.988			
N (Treated)	102	90	73	70			
N (Control)	171	46	28	41			
Bandwidth	0.186	0.197	0.147	0.188			
Panel B: t* = 7							
Coefficient (ATT)	0.319	3.665**	-1.193	-1.175	-0.369	-0.269	0.108**
SE (ATT)	(1.333)	(1.579)	(1.389)	(1.632)	(0.364)	(0.490)	(0.051)
Coefficient (ITT)	0.953	3.639**	-1.281	-1.212	-0.404	-0.257	0.108
SE (ITT)	(1.310)	(1.569)	(1.377)	(1.631)	(0.360)	(0.490)	(0.051)
Mean DV Treated	1.547	1.413	2.106	2.647	5.551	4.376	0.654
Mean DV Control	0.646	2.466	0.798	2.988	-0.827	8.120	1.263
N (Treated)	102	90	73	70	50	40	31
N (Control)	171	46	28	41	28	19	17
Bandwidth	0.186	0.197	0.147	0.188	0.257	0.126	0.175

Note: Table showing coefficient estimates of the effect of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under different $t^* \in \{4, 7\}$. All the ATT coefficients are derived andrecursively computed from ITT coefficients, which are in turn estimated using themethodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party and district fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.19: Robustness Check Inheritance: ATT and ITT estimates for different t*

	t = 1	t = 2	t = 3	t = 4	t = 5	t = 6	t = 7
Panel A: t* = 4							
Coefficient (ATT)	0.258	0.764	0.080	-1.258			
SE (ATT)	(0.675)	(1.126)	(1.155)	(1.520)			
Coefficient (ITT)	0.458	0.837	-0.056	-1.258			
SE (ITT)	(0.645)	(1.117)	(1.144)	(1.520)			
Mean DV Treated	11.826	11.196	11.498	11.057			
Mean DV Control	11.081	11.235	11.609	12.957			
N (Treated)	70	68	56	54			
N (Control)	137	38	21	26			
Bandwidth	0.206	0.171	0.195	0.274			
Panel B: t* = 7							
Coefficient (ATT)	0.151	0.892	0.047	-1.688	2.920*	-2.418	0.081
SE (ATT)	(0.711)	(1.145)	(1.173)	(1.535)	(1.691)	(1.834)	(0.626)
Coefficient (ITT)	0.458	0.837	-0.056	-1.258	2.656*	-2.409	0.081
SE (ITT)	(0.645)	(1.117)	(1.144)	(1.520)	(1.679)	(1.833)	(0.626)
Mean DV Treated	11.826	11.196	11.498	11.057	12.646	11.006	11.621
Mean DV Control	11.081	11.235	11.609	12.957	9.543	13.566	12.020
N (Treated)	70	68	56	54	39	32	26
N (Control)	137	38	21	26	19	15	15
Bandwidth	0.206	0.171	0.195	0.274	0.259	0.229	0.308

Note: Table showing coefficient estimates of the effect of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under different $t^* \in \{4, 7\}$. All the ATT coefficients are derived andrecursively computed from ITT coefficients, which are in turn estimated using themethodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party and district fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.20: IV Analysis of Returns to Political Office

Period	1st	2nd	3rd	4th	5th	6th
	(1)	(2)	(3)	(4)	(5)	(6)
Elected	−1.464	5.653*	10.404	−0.880	1.002	−1.378
	(1.775)	(3.330)	(280.080)	(2.255)	(1.461)	(3.443)
No. Tries Until Now	−0.071	0.022	0.044	−0.031	0.051	0.103
	(0.073)	(0.149)	(2.545)	(0.122)	(0.079)	(0.289)
Career in Politics Before Election	0.623**	−0.362	0.898	0.099	0.383	0.877
	(0.316)	(0.559)	(11.174)	(0.379)	(0.431)	(0.701)
Career in Law Before Election	0.403	1.497**	0.639	0.574	−0.037	−0.731
	(0.318)	(0.583)	(1.869)	(0.444)	(0.483)	(0.683)
Career in Business Before Election	0.981	−0.843	2.561	−1.830	−2.840	−4.741**
	(0.674)	(1.525)	(45.913)	(2.200)	(2.889)	(1.953)
Year	0.000	−0.015	−0.015	−0.029	−0.004	0.018
	(0.010)	(0.018)	(0.051)	(0.019)	(0.020)	(0.022)
N	892	448	317	278	181	144
Cragg-Donald F Stat.	6.29	5.63	0.00	6.70	15.56	1.85
District FE	✓	✓	✓	✓	✓	✓
Party FE	✓	✓	✓	✓	✓	✓

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

Note: Results show IV estimates of being elected on personal wealth for the 1st, ..., 6th term of political office. The endogenous variable is being elected and the instrument is being recommended by a newspaper. The dependent variable is log(1+Deflated Wealth). The standard errors are clustered at the candidate level.

Table D.21: Dynamic Results: Career Paths Granular

	t = 1	t = 2	t = 3	t = 4
Panel A: Wealth				
Coefficient (ATT)	0.215	1.748**	-0.164	-0.321
SE (ATT)	(0.396)	(0.867)	(0.348)	(0.506)
N (Treated)	342	259	202	173
N (Control)	681	204	120	122
Panel B: National: Minister				
Coefficient (ATT)	-0.078	0.119	-0.055	-0.130
SE (ATT)	(0.051)	(0.084)	(0.095)	(0.142)
N (Treated)	600	388	293	225
N (Control)	1267	293	224	157
Panel C: National: Upper House				
Coefficient (ATT)	-0.042	0.037	-0.157	0.113
SE (ATT)	(0.059)	(0.096)	(0.099)	(0.114)
N (Treated)	600	388	293	225
N (Control)	1267	293	224	157
Panel D: Provincial: Executive				
Coefficient (ATT)	0.011	-0.028	0.051	-0.049
SE (ATT)	(0.042)	(0.070)	(0.056)	(0.076)
N (Treated)	600	388	293	225
N (Control)	1267	293	224	157
Panel E: Provincial: Representative				
Coefficient (ATT)	-0.118	-0.030	0.294**	-0.223
SE (ATT)	(0.097)	(0.127)	(0.140)	(0.212)
N (Treated)	600	388	293	225
N (Control)	1267	293	224	157
Panel F: City: Mayor				
Coefficient (ATT)	-0.001	-0.005	0.012	-0.183**
SE (ATT)	(0.048)	(0.067)	(0.046)	(0.083)
N (Treated)	600	388	293	225
N (Control)	1267	293	224	157
Panel G: City: Alderman				
Coefficient (ATT)	-0.009	-0.010	-0.066	0.196
SE (ATT)	(0.064)	(0.087)	(0.084)	(0.130)
N (Treated)	600	388	293	225
N (Control)	1267	293	224	157
Panel H: Professional: Law				
Coefficient (ATT)	0.119	0.009	0.076	0.213**
SE (ATT)	(0.073)	(0.121)	(0.123)	(0.106)
N (Treated)	600	388	293	225
N (Control)	1267	293	224	157
Panel I: Professional: Entrepreneur				
Coefficient (ATT)	0.005	0.046	-0.007	-0.099
SE (ATT)	(0.034)	(0.035)	(0.047)	(0.074)
N (Treated)	600	388	293	225
N (Control)	1267	293	224	157

Note:
Table showing coefficient estimates of the {1, . . . , t^*}'th period of political activity on Personal Wealth and granular career paths: Upper House, Ministers, Provincial Executive, Provincial Representative, Mayor, Alderman, and two non-political career paths: a Judicial path and an Entrepreneural path. All the ATT coefficients are derived and recursively computed from ITT coefficients, which are in turn estimated using themethodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party and district fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.22: Dynamic Results: Heterogeneity by Party

	t = 1	t = 2	t = 3	t = 4
Panel A: Liberal				
Coefficient (ATT)	0.469	1.209	0.595	-0.518
SE (ATT)	(0.567)	(1.063)	(0.576)	(0.803)
N (Treated)	192	148	123	106
N (Control)	238	97	71	72
Panel B: Confessional				
Coefficient (ATT)	-0.312	1.070	0.197	0.514
SE (ATT)	(0.483)	(0.938)	(0.540)	(0.518)
N (Treated)	131	100	75	60
N (Control)	348	98	46	40
Panel C: Protestant				
Coefficient (ATT)	-0.172	1.732	0.064	0.523
SE (ATT)	(0.652)	(1.110)	(0.420)	(0.471)
N (Treated)	78	57	43	36
N (Control)	272	91	42	35

Note: Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth under $t^* = 4$ according to party. All the ATT coefficients are derived and recursively computed from ITT coefficients, which are in turn estimated using themethodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party, district fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.23: Dynamic Results: Heterogeneity according to Socialist Status

	Elected in Non-Soc. Distr.				Elected in Soc. Distr.			
	t=1	t=2	t=3	t=4	t=1	t=2	t=3	t=4
Panel A: Personal Wealth								
Coefficient (ATT)	0.299	1.905*	-0.133	-0.960	-0.224	0.261	3.607	-0.253
SE (ATT)	(0.457)	(1.030)	(0.399)	(0.786)	(1.021)	(0.893)	(3.360)	(0.912)
N (Treated)	266	217	170	130	63	39	32	40
N (Control)	552	168	100	97	97	31	18	22
Panel B: Political Career								
Coefficient (ATT)	-0.085	-0.033	-0.029	-0.384*	-0.006	0.591*	0.486	0.318
SE (ATT)	(0.092)	(0.145)	(0.157)	(0.209)	(0.240)	(0.340)	(0.613)	(0.257)
N (Treated)	446	330	245	178	112	53	46	43
N (Control)	913	238	196	122	262	47	26	33
Panel C: National Politics								
Coefficient (ATT)	-0.098	0.074	-0.101	-0.255	-0.288*	-0.025	0.078	0.050
SE (ATT)	(0.082)	(0.129)	(0.124)	(0.189)	(0.174)	(0.186)	(0.127)	(0.036)
N (Treated)	446	330	245	178	112	53	46	43
N (Control)	913	238	196	122	262	47	26	33
Panel D: Provincial Politics								
Coefficient (ATT)	-0.184*	-0.107	0.304**	-0.226	0.316	0.399	0.198	0.253
SE (ATT)	(0.109)	(0.134)	(0.128)	(0.192)	(0.239)	(0.332)	(0.639)	(0.191)
N (Treated)	446	330	245	178	112	53	46	43
N (Control)	913	238	196	122	262	47	26	33
Panel E: Municipal Politics								
Coefficient (ATT)	0.015	-0.100	-0.008	0.073	-0.173	0.518**	-0.039	0.033
SE (ATT)	(0.080)	(0.103)	(0.098)	(0.131)	(0.187)	(0.242)	(0.363)	(0.050)
N (Treated)	446	330	245	178	112	53	46	43
N (Control)	913	238	196	122	262	47	26	33
Panel F: Non-Politics or Business								
Coefficient (ATT)	0.135*	-0.009	0.022	0.248*	0.081	0.198	-0.089	0.023
SE (ATT)	(0.081)	(0.125)	(0.120)	(0.138)	(0.182)	(0.180)	(0.317)	(0.110)
N (Treated)	446	330	245	178	112	53	46	43
N (Control)	913	238	196	122	262	47	26	33

Note: Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth and rough career path outcomes under different $t^* = 4$ according to district socialist status. All the ATT coefficients are derived and recursively computed from ITT coefficients, which are in turn estimated using themethodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party, district and decade fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.24: Dynamic Results: Heterogeneity according to District Literacy

	Elected in Distr. with Low Literacy				Elected in Distr. with High Literacy			
	t=1	t=2	t=3	t=4	t=1	t=2	t=3	t=4
Panel A: Personal Wealth								
Coefficient (ATT)	0.326	2.685*	-1.304*	0.392	0.396	-0.022	0.154	-1.243*
SE (ATT)	(0.619)	(1.496)	(0.663)	(0.578)	(0.720)	(0.401)	(0.736)	(0.744)
N (Treated)	155	121	86	65	118	87	72	68
N (Control)	308	85	48	49	209	73	39	44
Panel B: Political Career								
Coefficient (ATT)	0.061	-0.270	0.050	-0.243	-0.124	0.230	-0.300	0.536**
SE (ATT)	(0.127)	(0.193)	(0.207)	(0.260)	(0.123)	(0.164)	(0.197)	(0.214)
N (Treated)	270	191	131	92	209	123	94	79
N (Control)	518	124	100	55	474	97	69	57
Panel C: National Politics								
Coefficient (ATT)	-0.013	0.085	0.230	-0.133	-0.118	0.030	-0.102	-0.025
SE (ATT)	(0.122)	(0.123)	(0.163)	(0.323)	(0.098)	(0.160)	(0.137)	(0.111)
N (Treated)	270	191	131	92	209	123	94	79
N (Control)	518	124	100	55	474	97	69	57
Panel D: Provincial Politics								
Coefficient (ATT)	-0.004	-0.255	0.094	-0.080	-0.010	0.249	0.134	0.280
SE (ATT)	(0.129)	(0.207)	(0.154)	(0.308)	(0.140)	(0.165)	(0.193)	(0.223)
N (Treated)	270	191	131	92	209	123	94	79
N (Control)	518	124	100	55	474	97	69	57
Panel E: Municipal Politics								
Coefficient (ATT)	0.153	-0.435*	-0.026	-0.002	-0.085	0.097	-0.308*	0.308*
SE (ATT)	(0.130)	(0.241)	(0.145)	(0.237)	(0.118)	(0.154)	(0.160)	(0.183)
N (Treated)	270	191	131	92	209	123	94	79
N (Control)	518	124	100	55	474	97	69	57
Panel F: Non-Politics or Business								
Coefficient (ATT)	0.185	-0.163	0.048	0.066	0.132	0.189	-0.065	0.410*
SE (ATT)	(0.117)	(0.185)	(0.144)	(0.254)	(0.118)	(0.149)	(0.187)	(0.212)
N (Treated)	270	191	131	92	209	123	94	79
N (Control)	518	124	100	55	474	97	69	57

Note: Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth and rough career path outcomes under different $t^* = 4$ according to literacy status of the district. All the ATT coefficients are derived and recursively computed from ITT coefficients, which are in turn estimated using themethodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party, district and decade fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.25: Dynamic Results: Heterogeneity according to Suffrage Extensions

	Before Suffrage Extension				After Suffrage Extension			
	t=1	t=2	t=3	t=4	t=1	t=2	t=3	t=4
Panel A: Personal Wealth								
Coefficient (ATT)	0.388	1.801*	-0.203	-1.117	0.173	-0.365	0.821	-0.171
SE (ATT)	(0.475)	(1.075)	(0.409)	(0.790)	(0.647)	(0.726)	(0.676)	(0.640)
N (Treated)	261	200	153	121	81	59	49	52
N (Control)	528	157	88	91	153	47	32	31
Panel B: Political Career								
Coefficient (ATT)	-0.057	-0.050	-0.012	-0.421**	0.070	-0.060	-0.306	0.456**
SE (ATT)	(0.099)	(0.125)	(0.154)	(0.212)	(0.220)	(0.246)	(0.270)	(0.190)
N (Treated)	439	298	227	174	161	90	66	51
N (Control)	893	226	182	108	374	67	42	49
Panel C: National Politics								
Coefficient (ATT)	-0.107	0.094	-0.057	-0.116	-0.289**	-0.020	0.218	-0.010
SE (ATT)	(0.084)	(0.113)	(0.111)	(0.178)	(0.144)	(0.130)	(0.145)	(0.080)
N (Treated)	439	298	227	174	161	90	66	51
N (Control)	893	226	182	108	374	67	42	49
Panel D: Provincial Politics								
Coefficient (ATT)	-0.120	-0.133	0.220*	-0.376**	0.051	0.004	-0.176	0.667***
SE (ATT)	(0.104)	(0.122)	(0.124)	(0.189)	(0.204)	(0.198)	(0.194)	(0.162)
N (Treated)	439	298	227	174	161	90	66	51
N (Control)	893	226	182	108	374	67	42	49
Panel E: Municipal Politics								
Coefficient (ATT)	0.046	-0.089	0.032	-0.012	-0.029	0.199	-0.407**	0.233
SE (ATT)	(0.088)	(0.104)	(0.092)	(0.141)	(0.166)	(0.222)	(0.192)	(0.177)
N (Treated)	439	298	227	174	161	90	66	51
N (Control)	893	226	182	108	374	67	42	49
Panel F: Non-Politics or Business								
Coefficient (ATT)	0.197**	0.030	0.128	0.271	0.161	-0.156	-0.147	0.093
SE (ATT)	(0.085)	(0.119)	(0.115)	(0.165)	(0.162)	(0.212)	(0.159)	(0.099)
N (Treated)	439	298	227	174	161	90	66	51
N (Control)	893	226	182	108	374	67	42	49

Note: Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth and rough career path outcomes under different $t^* = 4$ according to suffrage status (elected before or after suffrage extension). All the ATT coefficients are derived and recursively computed from ITT coefficients, which are in turn estimated using themethodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party, district and decade fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.26: Robustness: Consumption Patterns

	Short Lifespan Candidates				Long Lifespan Candidates			
	t=1	t=2	t=3	t=4	t=1	t=2	t=3	t=4
Panel A: Personal Wealth								
Coefficient (ATT)	-0.463	2.623*	0.604	-0.720	0.472	-0.169	-0.506	-0.052
SE (ATT)	(0.616)	(1.417)	(0.469)	(0.851)	(0.351)	(0.527)	(0.447)	(0.734)
N (Treated)	100	85	70	76	175	121	93	67
N (Control)	148	63	43	46	369	107	60	57
Panel B: Political Career								
Coefficient (ATT)	0.027	-0.061	-0.244	0.076	-0.118	0.378*	0.028	-0.269
SE (ATT)	(0.154)	(0.169)	(0.202)	(0.253)	(0.134)	(0.192)	(0.184)	(0.166)
N (Treated)	205	128	103	93	276	180	132	90
N (Control)	310	92	75	57	681	150	86	71
Panel C: National Politics								
Coefficient (ATT)	0.043	0.211	-0.181	0.115	-0.166	0.021	-0.047	-0.216
SE (ATT)	(0.123)	(0.142)	(0.150)	(0.159)	(0.110)	(0.137)	(0.165)	(0.199)
N (Treated)	205	128	103	93	276	180	132	90
N (Control)	310	92	75	57	681	150	86	71
Panel D: Provincial Politics								
Coefficient (ATT)	-0.160	-0.111	-0.140	0.007	-0.129	0.006	0.308*	-0.154
SE (ATT)	(0.185)	(0.142)	(0.137)	(0.238)	(0.134)	(0.131)	(0.166)	(0.141)
N (Treated)	205	128	103	93	276	180	132	90
N (Control)	310	92	75	57	681	150	86	71
Panel E: Municipal Politics								
Coefficient (ATT)	0.001	-0.122	0.033	0.125	-0.010	-0.059	-0.125	0.055
SE (ATT)	(0.123)	(0.137)	(0.087)	(0.214)	(0.096)	(0.133)	(0.117)	(0.144)
N (Treated)	205	128	103	93	276	180	132	90
N (Control)	310	92	75	57	681	150	86	71
Panel F: Non-Politics or Business								
Coefficient (ATT)	0.118	0.137	-0.036	0.265	0.241**	0.090	-0.027	0.002
SE (ATT)	(0.119)	(0.130)	(0.136)	(0.188)	(0.114)	(0.149)	(0.198)	(0.155)
N (Treated)	205	128	103	93	276	180	132	90
N (Control)	310	92	75	57	681	150	86	71

Note: Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth and rough career path outcomes under different $t^* = 4$ according to the subsequent lifespan of individuals. All the ATT coefficients are derived and recursively computed from ITT coefficients, which are in turn estimated using themethodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party, district and decade fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.27: ATT and ITT estimates for different t*: Wealth per Lifespan

	t = 1	t = 2	t = 3	t = 4	t = 5	t = 6	t = 7
Panel A: t* = 4							
Coefficient (ATT)	0.153	1.813**	-0.540	-0.532			
SE (ATT)	(0.356)	(0.752)	(0.406)	(0.486)			
Coefficient (ITT)	0.427	1.782**	-0.597	-0.532			
SE (ITT)	(0.343)	(0.751)	(0.403)	(0.486)			
Mean DV Treated	8.289	8.541	8.345	8.403			
Mean DV Control	7.929	7.591	9.265	8.964			
N (Treated)	340	259	202	173			
N (Control)	677	204	120	122			
Bandwidth	0.214	0.151	0.284	0.217			
Panel B: t* = 7							
Coefficient (ATT)	-0.089	1.839**	-0.582	-0.500	0.023	-0.930	0.850
SE (ATT)	(0.398)	(0.760)	(0.427)	(0.499)	(0.566)	(1.242)	(0.719)
Coefficient (ITT)	0.427	1.782**	-0.597	-0.532	-0.120	-0.838	0.850
SE (ITT)	(0.343)	(0.751)	(0.403)	(0.486)	(0.549)	(1.239)	(0.719)
Mean DV Treated	8.289	8.541	8.345	8.403	9.275	8.932	8.817
Mean DV Control	7.929	7.591	9.265	8.964	8.160	10.167	8.476
N (Treated)	340	259	202	173	123	88	63
N (Control)	677	204	120	122	66	61	37
Bandwidth	0.214	0.151	0.284	0.217	0.245	0.204	0.347

Note: Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth over years lived since treatment (lifespan), under different $t^* \in \{4, 7\}$. All the ATT coefficients are derived andrecursively computed from ITT coefficients, which are in turn estimated using themethodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on party and ditrict fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.

Table D.28: Correlation between Probability of Candidacy and Personal Wealth

	0 Wins	1 Win	2 Wins	3 Wins	4 Wins	5 Wins	6 Wins
Personal Wealth	−0.007	−0.002	0.027	0.051***	0.023	0.024	−0.016
	(0.008)	(0.011)	(0.017)	(0.018)	(0.026)	(0.025)	(0.055)
Num.Obs.	1270	554	391	352	219	178	126
R2 Within Adj.	0.023	−0.001	0.036	0.077	0.002	0.060	−0.016
District FE	✓	✓	✓	✓	✓	✓	✓
Party FE	✓	✓	✓	✓	✓	✓	✓
Electoral Controls	✓	✓	✓	✓	✓	✓	✓

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

Note: The table shows analyses of an indicator variable indicating candidate i is a candidate in the next election conditional on $0, ..., 6$ previous wins. The focus of the analysis is the conditional correlation between personal wealth and the indicator variable. All analysis control for electoral characteristics such as turnout, electorate size and the size of the electoral threshold, and all analyses include district and party fixed effects. Standard errors are clustered at the politician-level. *: p ¡ 0.1, **: p ¡ 0.5, ***: p ¡ 0.001.

Table D.29: Correlation between Probability of Candidacy and Recommendation, and Personal Wealth

	0 Wins	1 Win	2 Wins	3 Wins	4 Wins	5 Wins	6 Wins
Personal Wealth	0.011	0.007	0.027	0.046**	0.024	0.030	0.024
	(0.013)	(0.019)	(0.017)	(0.018)	(0.029)	(0.025)	(0.082)
Num.Obs.	1270	554	391	352	219	178	126
R2 Within Adj.	0.034	0.012	0.039	0.049	−0.006	0.115	0.013
District FE	✓	✓	✓	✓	✓	✓	✓
Party FE	✓	✓	✓	✓	✓	✓	✓
Electoral Controls	✓	✓	✓	✓	✓	✓	✓
Current Recommendation Conrols	✓	✓	✓	✓	✓	✓	✓

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

Note: The table shows analyses of an indicator variable indicating candidate i is a recommended by a newspaper and candidate in the next election conditional on $0, ..., 6$ previous wins. The focus of the analysis is the conditional correlation between personal wealth and the indicator variable. All analysis control for electoral characteristics such as turnout, electorate size and the size of the electoral threshold, and all analyses include district and party fixed effects. Standard errors are clustered at the politician-level. *: p ¡ 0.1, **: p ¡ 0.5, ***: p ¡ 0.001.

Table D.30: Correlation between Probability of Election and Personal Wealth

	0 Wins	1 Win	2 Wins	3 Wins	4 Wins	5 Wins	6 Wins
Personal Wealth	0.009	0.024***	−0.022	−0.015	0.002	−0.009	0.112**
	(0.009)	(0.008)	(0.013)	(0.018)	(0.023)	(0.024)	(0.045)
Num.Obs.	907	448	317	278	181	144	96
R2 Within Adj.	0.000	0.028	0.003	0.008	−0.021	0.115	0.092
District FE	✓	✓	✓	✓	✓	✓	✓
Party FE	✓	✓	✓	✓	✓	✓	✓
Electoral Controls	✓	✓	✓	✓	✓	✓	✓

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

Note: The table shows analyses of an indicator variable indicating candidate i wins election conditional on $0, ..., 6$ previous wins. The focus of the analysis is the conditional correlation between personal wealth and the indicator variable. All analysis control for electoral characteristics such as turnout, electorate size and the size of the electoral threshold, and all analyses include district and party fixed effects. Standard errors are clustered at the politician-level. *: p ¡ 0.1, **: p ¡ 0.5, ***: p ¡ 0.001.

Table D.31: Career Paths: District-Year FE

	t = 1	t = 2	t = 3	t = 4	t = 5
Panel A: DV: Wealth					
Coefficient (ATT)	0.486*	0.764*	-0.103	-0.325	0.016
SE (ATT)	(0.257)	(0.458)	(0.236)	(0.246)	(0.073)
N (Treated)	347	260	202	173	123
N (Control)	950	210	120	122	66
Bandwidth	0.239	0.166	0.246	0.183	0.208
Panel B: DV: Politics					
Coefficient (ATT)	-0.033	0.059	-0.032	0.073	-0.064
SE (ATT)	(0.051)	(0.055)	(0.067)	(0.091)	(0.081)
N (Treated)	604	389	293	225	153
N (Control)	1295	297	224	157	83
Bandwidth	0.251	0.225	0.259	0.217	0.372
Panel C: DV: National Politics					
Coefficient (ATT)	-0.096**	0.124**	-0.044	-0.011	-0.030
SE (ATT)	(0.045)	(0.059)	(0.045)	(0.058)	(0.052)
N (Treated)	604	389	293	225	153
N (Control)	1295	297	224	157	83
Bandwidth	0.211	0.207	0.303	0.192	0.272
Panel D: DV: Provincial Politics					
Coefficient (ATT)	-0.082	0.015	0.026	-0.005	-0.024
SE (ATT)	(0.054)	(0.055)	(0.044)	(0.057)	(0.073)
N (Treated)	604	389	293	225	153
N (Control)	1295	297	224	157	83
Bandwidth	0.204	0.213	0.278	0.227	0.345
Panel E: DV: City Politics					
Coefficient (ATT)	-0.002	-0.013	0.058	0.069	0.005
SE (ATT)	(0.039)	(0.048)	(0.045)	(0.066)	(0.062)
N (Treated)	604	389	293	225	153
N (Control)	1295	297	224	157	83
Bandwidth	0.199	0.267	0.182	0.186	0.280
Panel F: DV: Business and Entreneurship					
Coefficient (ATT)	0.083**	-0.072	-0.023	0.063	-0.060
SE (ATT)	(0.041)	(0.053)	(0.051)	(0.065)	(0.048)
N (Treated)	604	389	293	225	153
N (Control)	1295	297	224	157	83
Bandwidth	0.269	0.186	0.222	0.169	0.188

Note: Table showing coefficient estimates of the $\{1, \ldots, t^*\}$'th period of political activity on Personal Wealth and rough career paths: Politics, National Politics, Provincial Politics, City Politics, and Non-Politics. All the ATT coefficients are derived and recursively computed from ITT coefficients, which are in turn estimated using themethodology in (Cattaneo et al., 2019) using MSE-optimal bandwidth. Standard errors for the ATT estimates are derived using the delta method.The estimates in both panels control for age at election, year of election, and newspaper recommendations. The estimates are conditional on district-year fixed effects. *: p ¡ 0.10, **: p ¡ 0.05, ***: p ¡ 0.01.