Career Transitions and Health: Evidence from Norwegian Mayors*

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Abstract

Does the transition to senior leadership roles come at a cost to health? We examine this question using the case of full-time mayors in Norway—a role that bundles high status and income with longer hours, demanding tasks, and tighter scrutiny. Implementing an event-study design on linked administrative data, we compare newly elected mayors to a matched control group from the general population. We find that while this career transition yields large income gains, there is no evidence of adverse health effects. Instead, we observe a modest reduction in primary-care visits, particularly for psychological and musculoskeletal conditions. Overall, our findings challenge common concerns about the toll of political careers and suggest that upward career transitions into high-responsibility jobs need not come at the expense of health.

Keywords: Career transitions, health consequences, political selection, event study, administrative data.

JEL Classification: D72, I12, J81, M51

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1 Introduction

Career transitions into more senior roles are a central feature of working lives, typically bundling higher pay, greater authority, and improved status with more demanding tasks, longer hours, and tighter performance scrutiny. These changes can pull health in opposite directions: higher income and status may improve living conditions and reduce material hardship, while increased responsibility and time pressure may raise stress and strain. Existing evidence on the health consequences of career advancement is limited, mixed, and largely based on self-reported outcomes. We examine these questions using a context where the timing of promotion is sharp and largely orthogonal to underlying career trajectories: winning full-time political office, a move that brings a substantial increase in earnings and status together with high public visibility and sustained pressure.

Using detailed nationwide health-care utilization data from Norway, we study the immediate and intermediate health consequences of first-time promotion to mayor. We focus on first-time mayors because this transition represents a significant shift in public life, moving from relative anonymity to a position of considerable pressure and public scrutiny.[1] The mayoral position is a full-time role with primary political responsibility for the local public sector, which provides essential welfare services and employs a substantial portion of Norway's workforce. For many individuals, becoming mayor is thus a discrete upward career move from regular employment into a full-time, well-paid job with major non-wage benefits and substantial job demands. Serving as mayor also frequently acts as a stepping stone to a career in national politics (Cirone, Cox and Fiva, 2021), further highlighting its importance.

We employ an event study research design, with the treatment group comprising individuals who make this transition into mayoral office and the control group consisting of individuals from the general population matched by age (in five-year intervals), gen-

[1] In addition to these general job demands, harassment and threats against politicians are widespread and may further intensify the pressures of public service, especially for women in positions of power (see, e.g., Bjørgo et al., 2022; Daniele, Dipoppa and Pulejo, 2023; Håkansson, 2021; Herrick et al., 2021).

der, education level (high or low), and municipality of residence. In Norway, primary-care visits are closely tied to work capacity since physicians certify sickness absence. By tracking primary-care visits around political promotions, we assess how health shapes political representation and mayors' capacity to perform effectively in office. Under the identifying assumption of parallel trends, which we validate by showing that treatment and control individuals have statistically indistinguishable pre-treatment trends, differences in post-promotion trajectories between treated and control individuals can be interpreted as the causal effect of promotion to full-time mayoral office on income and health-care use.

In our main empirical analyses, we first confirm that individuals who assume full-time political roles in Norway experience a significant income boost, in line with findings from earlier studies (Andersen and Sørensen, 2022; Cirone, Cox and Fiva, 2021; Geys and Sørensen, 2024). Turning to health, measured by primary-care visits, our event-study analysis finds no evidence that career transitions to political office negatively affect individuals' health. On the contrary, we find that full-time mayoral office modestly reduces doctor visits, particularly those related to musculoskeletal and psychological conditions. These results challenge the common narrative that political life is inherently stressful and detrimental to well-being.[2]

One possible alternative explanation is that mayors delay or avoid seeking medical care while serving in office. To explore this possibility, we exploit the fact that some first-time mayors serve only a single four-year term while others are re-elected, due to a combination of shifts in electoral support between elections, parties replacing their top candidates, and mayors choosing not to run again. We use an event-study design comparing one-term mayors, who leave office after four years, to multiple-term mayors

[2]Anecdotal and journalistic accounts often portray political life as mentally taxing. For instance, *The Guardian* reports that "four in ten politicians experience low or very low mental wellbeing" due to the toxicity of political environments. See *The Guardian*, 3 May 2024, https://www.theguardian.com/world/article/2024/may/03/toxicity-of-politics-mental-health-brutal-business. Similarly, Nicola Sturgeon cited the "brutality" of modern political discourse and noted that "it takes its toll on you and on those around you" when announcing her resignation. See *The Guardian*, 15 February 2023, https://www.theguardian.com/politics/2023/feb/15/nicola-sturgeon-in-her-own-words-key-moments-resignation-speech.

who remain in office, and track both groups before, during, and after their first term. We find no clear evidence that mayors compensate for unmet health-care needs after stepping down.

To complement our focus on short- and medium-term outcomes, we also examine effects on longevity using historical parliamentary elections in Norway (1921–1981) and a regression discontinuity design. This broader perspective enables us to evaluate whether political office has delayed health consequences, and also to connect more closely to parts of the past literature. Consistent with our main results, we find no evidence that political office substantially affects lifespan.

This paper relates to two strands of the literature. First, we contribute to a literature on the health effects of workplace promotions and workplace stress that has produced mixed and inconclusive evidence, and has predominantly relied on survey data (see, e.g., Anderson and Marmot, 2012; Boyce and Oswald, 2012; Johnston and Lee, 2013).[3] Complementary evidence comes from studies that hold occupation fixed and instead exploit changes in working conditions over time. Belloni, Carrino and Meschi (2022) use within-occupation variation over time in job-quality indicators—driven largely by the 2009 economic crisis and broader macroeconomic conditions—and show that improvements in job quality reduce depressive symptoms, particularly among women.[4] We contribute by studying health effects around a clearly defined upward career transition using comprehensive administrative data. Whereas Belloni, Carrino and Meschi (2022) identify health effects from changes in working conditions within occupational categories, our setting captures a discrete transition across occupations—from regular employment into full-time political leadership. By relying on objective measures of health-care use, we avoid concerns related to self-reported outcomes and trace the dynamic causal effects of career transitions on health.

[3] Anderson and Marmot (2012) find that promotions among British civil servants decrease the probability of developing heart disease. Using data from the British Household Panel Survey, Boyce and Oswald (2012) find that workers moving from a nonsupervisory role to a managerial role experience a deterioration in their mental health. Johnston and Lee (2013) find corresponding negative mental health effects of promotions using survey data from Australia.

[4] Related work also examines heightened job demands at the top of organizational hierarchies. Using facial images of CEOs, Borgschulte et al. (forthcoming) document that sustained exposure to stressful work environments is associated with long-term health deterioration.

Second, we relate to the literature on the costs and benefits of winning political office. Existing studies document substantial economic returns to holding office across numerous countries.[5] However, existing research on health-related outcomes has primarily focused on longevity, producing ambiguous and contested findings.[6] We expand this literature by examining a critical yet understudied aspect: the short- and medium-term health consequences of political office, using comprehensive, population-wide healthcare utilization data. Investigating these immediate health impacts allows us to shed light on potential mechanisms shaping longer-term health trajectories among public officials.

2 Institutional Setting

Norway's unitary state has three levels of government – local, regional, and national – each governed by representative assemblies elected through proportional representation. As mentioned above, we focus on one full-time, well-paid political position: the local mayors. In this section, we outline the institutional context in which they operate and document the level of media scrutiny these politicians face. We also outline the primary health sector, an integral part of Norway's public healthcare system, which provides universal access to a wide range of highly subsidized health and care services.

[5]For example, Eggers and Hainmueller (2009) for the United Kingdom, Querubin and Snyder (2013) for the United States, Fisman, Schulz and Vig (2014) for India, Kotakorpi, Poutvaara and Terviö (2017) for Finland, Berg (2020) for Sweden, and Cirone, Cox and Fiva (2021) for Norway.

[6]Olenski, Abola and Jena (2015) document reduced longevity for elected officials relative to runners-up across 17 countries, and similarly, Link, Carpiano and Weden (2013) find a mortality disadvantage for U.S. presidents and vice presidents. Batinti, Costa-Font and Shandar (forthcoming), on the other hand, find increased longevity for European royals compared to other aristocrats – admittedly in a sample with modest generalizability. U.S.-based studies focusing on narrowly contested elections (Borgschulte and Vogler (2019); Barfort, Klemmensen and Larsen (2021)) also report substantial positive effects, indicating elected candidates live several years longer. These large positive findings have been criticized methodologically by Gelman (2022), who argues they likely reflect statistical artifacts arising from noisy data and researcher degrees of freedom. Albada (2025) similarly argues that the estimated longevity gains reported in Barfort, Klemmensen and Larsen (2021) are likely exaggerated, if not false positives.

2.1 The Role of Mayors in Local Politics

Norwegian local governments, employing 17 percent of the national labor force, provide essential welfare services (e.g., schooling, elderly care) and local public goods (e.g., waste management). During our sample period, there were about 430 local governments, averaging 10,000 residents but ranging from a few hundred to about 700,000 in the capital, Oslo.

Each local government is run by a local council which is elected in September every fourth year using a flexible-list system. In this system, parties almost always place their mayoral candidate at the top of their list. After the election, local councils form by the end of October. At the inaugural assembly, an executive board and a mayor, who chairs the executive board, are elected by majority vote. While local councilors are generally considered "leisure politicians", the mayor holds a full-time, well-paid position. For the individuals we study, appointment as mayor therefore represents a discrete step up the job ladder—from part-time or regular employment into a full-time leadership role in the local public sector.

When a single party secures a majority of council seats, its top-ranked candidate is almost guaranteed to assume the mayoral role. More often, however, no party wins a majority, and the choice of mayor is settled through negotiations following the election. Arntzen, Fiva and Sørensen (forthcoming) report that in the 2003–2019 period, the mayor is from the largest local party in 75% of cases, from the second-largest party in 18%, and from the third-largest or smaller parties in 7%.

Appendix Figure A.1 uses data from the Local Candidate Dataset (Fiva, Sørensen and Vøllo, 2024) to analyze the subsequent local political careers of the 434 mayors elected in 2003. Of these mayors, 73% participated in the 2007 election, with nearly all of them running as the first-ranked candidate on their respective lists (68%). Ultimately, 50% were re-elected as mayors. In subsequent elections (2011, 2015, 2019, and 2023), these percentages decline progressively. By the 2023 election, 24% of the original mayors continued to run, albeit typically no longer as top-ranked (mayoral) candidates.

2.2 Media Coverage of Mayors

Together with the media consultancy firm *Retriever*, we have collected data on individual politicians' appearance in newspapers (web and print), radio and television for the 2005–2020 period. Using the names and party affiliations of candidates, we have collected the daily appearances in each media outlet for all local-level mayoral candidates (i.e., first-ranked candidates).[7]

Appendix Figure A.2 presents the monthly media mentions for first-time appointed mayors in 2011 and 2015, analyzed over an eight-year period centered on the September 2011 election. The figures show a spike in media attention during the relevant election month, and it remains elevated throughout the subsequent four-year term. On average, mayors are mentioned in the media about ten times per month. The comparison groups consisting of not-yet elected mayors are on a considerably lower level, but pick up towards the end of the sample period as the next campaign period starts.

2.3 Primary Health Services in Norway

Norway offers publicly funded, universal healthcare, ensuring that all residents have access to essential medical services. Publicly contracted general practitioners (GPs) play a central role in the Norwegian health system, acting as the first point of contact for individuals with health concerns. These GPs are gatekeepers to specialized healthcare services and are responsible for managing patients' overall health. They certify sickness absences for work and address non-acute medical needs. Privately funded GP services represent only a very small part of the Norwegian healthcare system, particularly outside major cities, where publicly contracted GPs are almost entirely dominant. The GP system is organized under the municipalities, which represent the lowest level of government.

Patients pay a modest co-payment for GP visits, currently set at NOK 179 (about USD 16). Prescription medications are also subsidized, with co-payments capped at

[7] Cox et al. (2021) use these data to measure campaign efforts for candidates running for national-level office in 2017.

NOK 520 per three-month supply per prescription. Furthermore, Norway's health service exemption card (*"frikort for helsetjenester"*) system provides financial protection: once an individual's annual healthcare expenses reach NOK 3,165 (about USD 286), any additional GP visits or medications for the year are free of charge. These co-payments and the annual cap are set by the central government and apply uniformly across the country. Because GPs certify sickness absence and public primary care dominates, GP consultations provide a comprehensive and comparable measure of health and work capacity across mayors and their matched controls.

3 Administrative Data

We analyze linked, population-wide administrative data from multiple Norwegian registers to study the health consequences of promotion to mayor. Our datasets include labor market information from the Employer and Employee Register, primary healthcare records from the Norwegian Control and Payment of Health Reimbursements Database (KUHR), demographic data from the Population Register, income data from the Norwegian Tax Administration, and education records from the National Education Database. These datasets are linked by Statistics Norway using a unique personal identification number, providing comprehensive coverage of the Norwegian population from 2006 to 2019. Our study focuses on individuals aged 18 to 80.

The employment data include the International Standard Classification of Occupations (ISCO) codes and the start and end dates of all employment relationships. Using the ISCO code for mayors, we identify individuals in mayoral positions, though some misclassifications occur (e.g., multiple individuals listed as mayors within the same municipality).[8] As other members of the municipal council do not have this as their profession, they are rarely listed in the Employment Register – hence it is not possible

[8]To accurately identify first-time mayors, we employ a two-step process: (1) extract individuals with the mayor occupation code (ISCO-98: 1110103), along with their municipality, gender, and birth year; and (2) cross-reference this information with public records of municipal mayors, matching on gender and birth year. The first year an individual is recorded as elected mayor is designated as their event year. This approach identifies 563 (86.5 percent) out of 651 newly elected mayors, covering the 2007, 2011, and 2015 elections.

to follow other politicians at the municipal level. Since we only have data on individuals promoted to mayor and not on other mayoral candidates, strategies comparing winners and losers in close elections are not feasible.

To measure health outcomes, we rely on the KUHR database, which contains claims for reimbursements from general practitioners (GPs) under the National Insurance Scheme. This allows us to analyze the type and frequency of GP visits and the associated diagnostic codes, classified according to the second edition of the International Classification of Primary Care (ICPC-2). While the KUHR database does not include private, out-of-pocket health services, public health services dominate in Norway. Consequently, we can capture nearly all primary healthcare utilization, offering unique documentation of health events related to promotion to top political positions.

Additionally, our dataset allows us to identify both the marital and cohabiting partners of mayors, as well as those in the control group. Cohabitation status is registered when adults live within the same household, have opposite sex, and no family relation. This allows us to explore whether the partners of first-time mayors experience any health-related events associated with their partner's promotion to the top political position in their municipality.

3.1 Main variables

Our main outcome is the number of visits to the GP per year, which captures both general health problems and work-related sickness absences. In addition to counting the overall number of visits, we conduct subgroup analyses based on health problems classified under the International Classification of Primary Care (ICPC-2). The ICPC-2 comprises 17 chapters; however, several of these are infrequently used. We therefore limit the subgroup analysis to the ten most frequent categories in our analytical sample.[9]

For income, we use yearly personal income as registered by the Norwegian tax au-

[9]The subgroups include: General and unspecified (Chapter A), Digestive (Chapter D), Cardiovascular (Chapter K), Musculoskeletal (Chapter L), Neurological (Chapter N), Psychological (Chapter P), Respiratory (Chapter R), Skin (Chapter S), Endocrine/Metabolic and Nutritional (Chapter T). Health problems not falling within these categories were placed in a residual category labeled "others".

thorities, which includes earned wages, pensions, and other calculated incomes from business activities attributed to the individual and is expressed in units of G, the National Insurance Scheme's basic amount. Using this common scale allows us to quantify both the earnings returns to promotion to mayor and the earnings penalty upon exit from office in a comparable way over time. G is indexed annually to average wage growth in Norway and is used to calculate and update social insurance benefits and thresholds. In 2020, one G equals NOK 101,351, or roughly $10,000 USD.

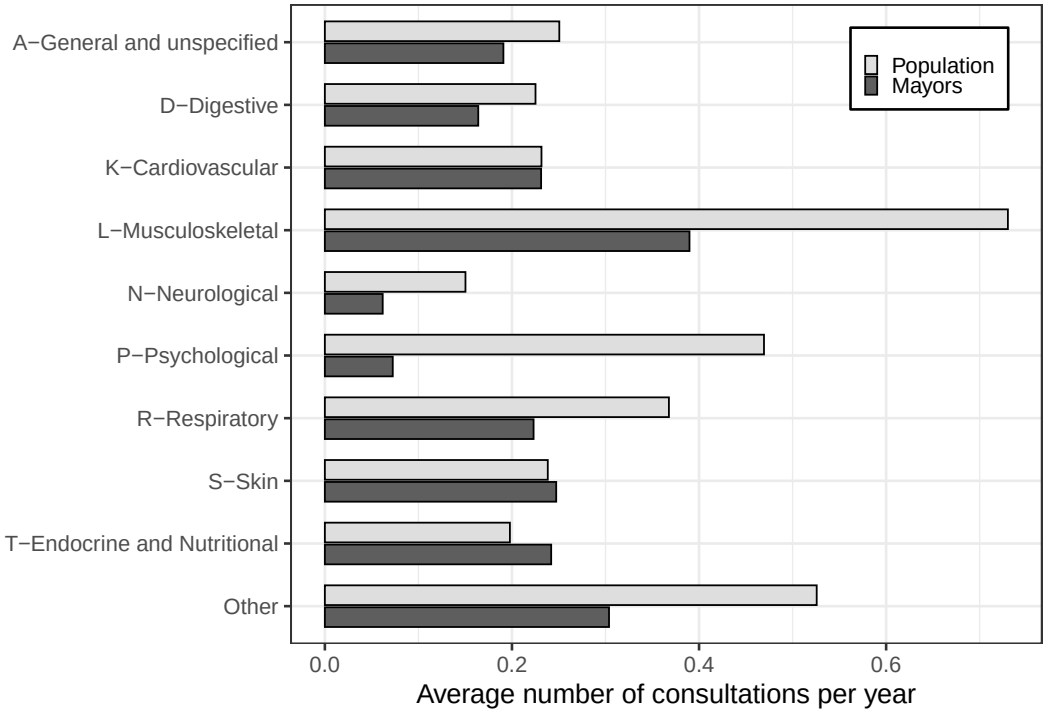
As selection into politics is non-random, we employ a matching approach to construct a control group for elected mayors. To do so, we use information on the municipality where the individuals resided in each year, as well as information from the educational database on whether or not the individual had completed higher education (bachelor's degree or higher).

3.2 Descriptive statistics

Table 1 presents descriptive statistics for the population, mayors and a matched control group. The matched control group was constructed to resemble the mayors along age, sex, education, and municipality at the time of election (see Section 4 for further details). As expected, the mayors in our sample differ from the general population across several dimensions. For instance, they are more likely to be male, tend to be older, and have higher levels of income and education. Additionally, mayors visit their GP less often for health issues compared to both the matched control group and the general population.

Figure 1 breaks down GP visits by health problem categories. Mayors seek primary care treatment less frequently than the general working-age population, with particularly notable disparities in musculoskeletal disorders and psychological health issues. These differences may partly reflect variations in demographic factors, such as age and gender, which strongly influence health care utilization (see Appendix Figure A.3). To account for these demographic differences, we apply matching techniques.

Figure 1: GP visits by type of diagnosis



Notes: The Figure shows the number of GP visits in 2017 by ICPC-2 chapter subdivided by active mayors in 2017 ($N = 372$) and the working age population (age 18-66; $N = 3,420,321$).

Table 1: Summary statistics for population and mayors

Variable	Population	Mayors	Matched group
Female (share)	0.49 (0.50)	0.30 (0.46)	0.30 (0.46)
Age	46.30 (16.33)	54.24 (9.42)	54.18 (9.64)
High education	0.32 (0.47)	0.54 (0.50)	0.55 (0.50)
Real income (Gs)	3.60 (4.04)	8.37 (3.64)	5.43 (4.10)
GP visits	2.35 (3.54)	1.55 (2.51)	2.16 (3.37)
Mental health visits	0.35 (1.67)	0.07 (0.40)	0.29 (1.49)
Observations	4,457,759	555	101,490

Note: This table shows means, with standard deviations in parentheses, of newly elected municipality mayors in election years 2007, 2011, and 2015, a matched control group, and the population aged 18–80, using 2017 as the base year. Individuals in the control group were matched with mayors based on their age, gender, education and municipality of residence (see Section 4 for details), and observations re-weighted according to the number of matches in the mayor group. The sample of mayors represented in the table excludes eight mayors who died between 2010 and 2018. G is the national insurance scheme's basic amount, which is used to index social insurance benefits and is adjusted annually to follow wage growth. In 2020, one G equals NOK 101,351, or roughly $10,000 USD. High education is a binary indicator for whether the individual had completed university-level education.

4 Research Design

The main challenge in studying the health and income effects of promotion to mayor is that selection into political office is non-random. We handle this by matching elected politicians to a comparable control group and estimate a dynamic differences-in-differences model.

To study the effect of being elected as mayor, we construct a control group C_i for politician i consisting of all (never-treated) individuals residing in the same municipality as i in the election year who have the same gender, age (in five-year bins), and education level, where education is classified as high if the individual has completed a bachelor's degree or higher, and low otherwise. Our empirical specification can be expressed as:

$$y_{i,t+r} = \alpha^t + \sum_{\substack{r=-5 \\ r\neq -1}}^{8} \tau_r^t \mathbf{I}_{t+r} + \sum_{\substack{r=-5 \\ r\neq -1}}^{8} \beta_r \mathbf{I}_{t+r} \times \text{Elected}_i + \theta^t \text{Elected}_i + \varepsilon_{i,t+r}, \quad (1)$$

where t is the election year and $\mathbf{I}_{t+r}$ is a binary indicator for time period, relative to

election year t. Elected individuals are assigned weight 1 and individuals in the control group weight $\frac{1}{\#C_i}$. This weighting assures that the total weight of control group C_i corresponds to the weight of politician i. This approach identifies the average treatment effect on the treated (ATT) under the assumption that, in the absence of treatment, the treated group would have followed the same trend as the control group.[10] Standard errors are clustered at the politician-control group level. This both solves problems of correlation within each politician-control group pair and heteroskedasticity resulting from different sizes of the control groups.

The parameters of interest are β_r. Because we omit the event time dummy in year $t-1$, the event time coefficients measure the impact of being promoted to full-time political office relative to the year just before the election. If our research design is valid, we expect β_r to be close to zero for all $r < 0$.

We also want to study the effect of exit from office. One reason is that it could be that mayors postpone health visits until after their term, resulting in excessively high visit frequencies upon exit from office. As exit from mayoral office is non-random, causality is less clear than in the case with entry into office.

There are several ways a candidate can exit from mayoral office (see Section 2.1). One is that the candidate's party doesn't win the subsequent election. The mayor is usually from the largest party, and if her party is no longer the largest, she will often be replaced. Moreover, the mayor is typically the top candidate on the electoral list. There are cases where the party may want to change their top candidate, hence leading to a new mayor from the same party. Finally, the candidate may chose not to stand for election in the next election, or at least not stand for election as her party's top candidate.

We compare mayors exiting office to mayors who do not exit, so the sample is individuals elected as mayor for the first time in year t. The exit group exits in year

[10]Under an assumption of homogeneous treatment effects, a GLS-type estimator taking the size of each control group into account and hence assigning more weight to pairs with larger control groups would achieve higher efficiency. Under heterogeneous treatment effects, which seems plausible, this would yield a skewed estimate of the ATT as mayors from large municipalities would be assigned too much weight.

$t+4$. Our empirical specification can be expressed as:

$$y_{i,t+r} = \eta^t + \sum_{\substack{r=-5 \\ r\neq -1}}^{8} \nu_r^t \mathbf{I}_{t+r} + \sum_{\substack{r=-5 \\ r\neq -1}}^{8} \gamma_r \mathbf{I}_{t+r} \times \text{Exit}_i + \xi^t \text{Exit}_i + \epsilon_{i,t+r}, \tag{2}$$

where all individuals are given weight 1. The variable Exit_i is a dummy for individual i exiting office after one period. Our main parameters of interest are γ_r. Again we constrain $\gamma_{-1} = 0$ so estimates are relative to the last year before being elected. This research design contrasts the trajectories of one-term mayors, both during and after their time in office, with those of multiple-term mayors, who form a natural comparison group.

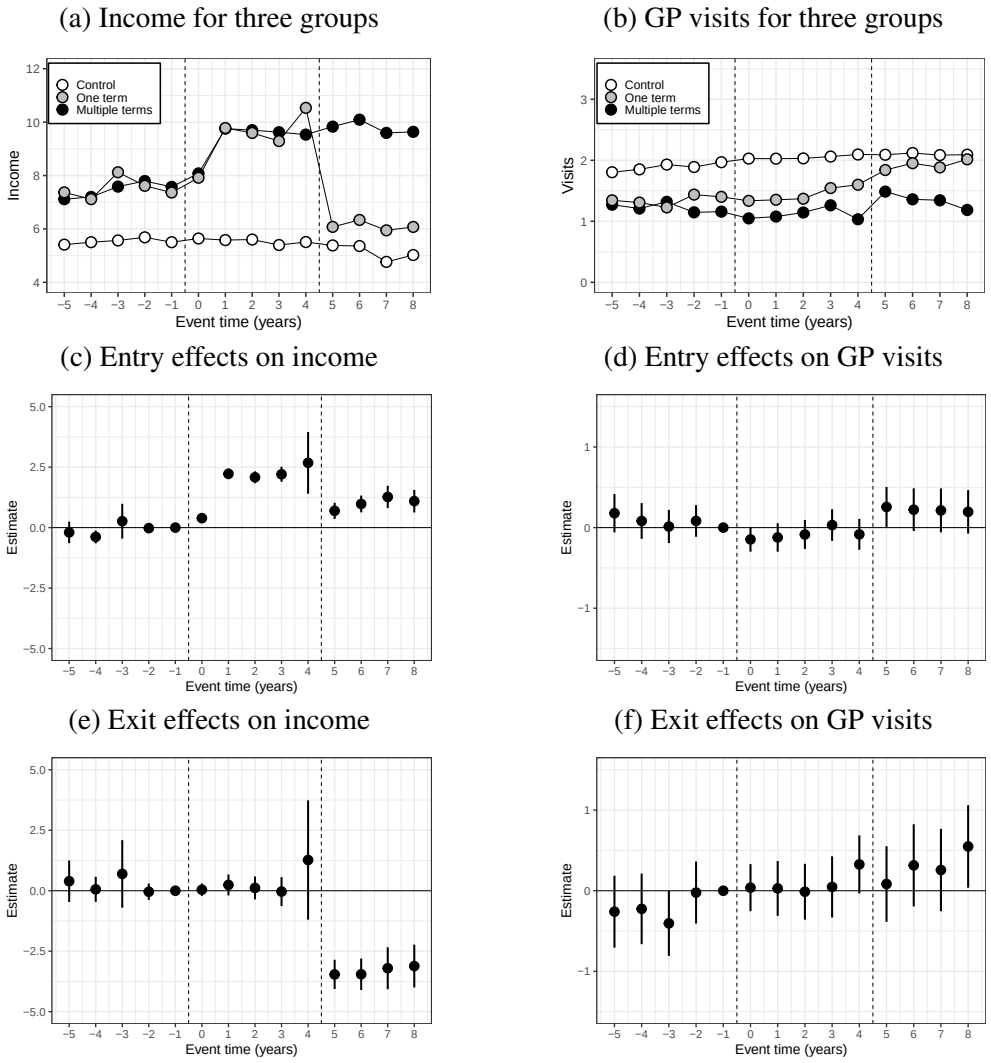
5 Results

We start by examining income effects. Winning full-time political office in Norway represents a sharp promotion shock that is largely orthogonal to underlying career trajectories, and it should therefore generate a clear change in earnings at the time of entry into office. Estimating our event-study model with income as the outcome allows us to characterize the size and timing of this earnings jump and to document how incomes evolve when mayors later leave office.

In Figure 2, we present the results from our event study design graphically. While the left-most panels (a, c, and e) use real average income as the outcome variable, the right-most panels (b, d, and f) uses the number of visits to general practitioners (GP) during an event year. We provide the full numerical results in Appendix Table A.1.

In the top panels (a and b), the circles display average outcomes over event time for three groups. The gray circles represent mayors who are elected as mayor at $t = 0$ and stay in office for one term (four years) only. The black circles represent mayors who are elected as mayor at $t = 0$ and stay in office for at least two terms. The empty circles display corresponding results for individuals matched on age (in five-year brackets), gender, education (high or low), and municipality of residence.

Figure 2: Event study analyses of mayoral entry and exit



Note: Panel (a) plots pensionable income measured in Gs over event time for individuals elected as mayor for the first time and staying for one term only (gray circles) and multiple terms (black circles) at event time $t = 0$ and the control group (empty circles). The control group consists of non-politicians that have the same age, gender, education, and municipality of residence as the individuals in the treatment group. Panel (b) plots the total number of health visits over event time for the same groups of individuals. Panels (c) and (d) contain estimates of the parameters of interest, β_r, in Equation (1) comparing all first time elected mayors to their control group. Panels (e) and (f) contain estimates of the parameters of interest, γ_r, in Equation (2), comparing individuals elected as mayors for the first time exiting after one period to individuals staying on for subsequent periods. All the event plots contain 95% confidence intervals with standard errors clustered at the politician-control group pair.

Local elections occur in September of year $t = 0$, with mayors officially taking office later in the fall. As a result, event year $t = 0$ reflects only a modest degree of exposure to the treatment—i.e., holding full-time political office—for elected mayors. The full effect of the promotion is therefore primarily captured in event years $t = 1$ through $t = 3$, with continued partial exposure into $t = 4$ for one-term mayors. In contrast, multiple-term mayors remain in office beyond their first term and thus receive full exposure during both $t = 4$ and the subsequent period from $t = 5$ to (the end of) $t = 8$. To aid interpretation, Figure 2 includes dashed vertical lines that delineate three periods: the pre-treatment phase ($t = -5$ to $t = -1$), the first-term exposure phase ($t = 0$ to $t = 4$), and the extended exposure or post-treatment phase ($t = 5$ to $t = 8$), depending on whether the mayor was re-elected.

The middle and bottom panels contain estimates of the parameters of interest, β_r, in Equation (1), and γ_r from Equation (2), respectively, together with 95% confidence intervals.

We notice that individuals elected as mayors tend to have a somewhat higher income than individuals in the control group before election. The trend over time, however, has a slight upward tendency and is essentially identical in the two groups as can be seen from panel (c). This supports the assumption of parallel trends, allowing us to causally interpret the estimates derived from the difference-in-differences analysis.

From Panel (b) of Figure 2, we observe that individuals elected as mayors also have fewer GP visits than individuals in the control group before election. It seems that the time trend is essentially flat in the different groups, again lending credibility to the assumption of parallel trends (see Panel (d)).

5.1 Income

Consistent with the findings of Cirone, Cox and Fiva (2021), panel (a) of Figure 2 shows that mayors get a substantial income increase when they enter office. Because the election is held in September of the event year, the full effect of promotion is first observed in event year $t = 1$. The effect persists for three more years for both one- and

multiple-term mayors. After one-term mayors leave office at the end of $t = 4$, their income falls substantially relative to multiple-term mayors.

In fact, the estimated income boost of first-time promotion to mayor, reported in panel (c), of about 2.4 Gs (about USD 24,000), is slightly smaller than the estimated income loss of exiting politics, reported in panel (e). Specifically, comparing one-term mayors to multiple-term mayors reveals an income loss in event years $t = 5$ to $t = 8$ of approximately -3.3 Gs (about USD 33,000). This empirical pattern suggests that there is a post-politics earnings penalty in Norway (Geys and Sørensen, 2024).

5.2 Health care use

In Panel (b) of Figure 2, we see that individuals promoted to mayor at the end of $t = 0$ are healthier than individuals in the control group before the election (as in Figure 1). We find no clear evidence, though, that being *appointed* mayor affects general health, measured by GP visits.

In the event-study capturing entry-effects, reported in Panel (d), we find point estimates close to zero in event years $t = 0$ to $t = 4$. Based on event year $t = 3$, the last full calendar year of the election term, we can rule out any effects on GP visits larger than 0.64 (0.22 SD).[11]

Panel (f) of Figure 2 displays the results from the event study capturing exit effects. The point estimates suggest a deterioration in health after mayors leave office, but the effects are not statistically distinguishable from zero. In Section 5.3, we complement this with a pooled analysis to increase precision and explore whether effects differ by type of diagnosis.

It has been suggested that women face harsher treatment than men in politics. To test this hypothesis, we consider heterogeneous effects by gender in Appendix Figure A.4. Women have more frequent visits to their GPs (both mayors and civilians). But again, we find no evidence that political promotions affect health status – as measured by doctor visits – for any gender. The same is true if we only focus on mental health

[11] This is found by finding the upper limit of the respective 95% confidence interval and dividing by the respective standard deviation for the whole sample.

visits as outcome variable (Appendix Figure A.5).

5.3 Pooled effects of entry and exit by health diagnosis

In Figure 3 we present the results from two-period difference-in-differences models where we split the baseline analyses by type of diagnosis. To preserve statistical precision, we pool observations into broader time periods. For entry into office, we compare the pre-election period ($t = -5$ to $t = -1$) with the post-election period ($t = 1$ to $t = 3$). For exit, we contrast mayors who leave office at $t = 5$ with those who remain. The pooled pre-period in this case is $t = 1$ to $t = 4$, and the post-period is $t = 5$ to $t = 8$.[12]

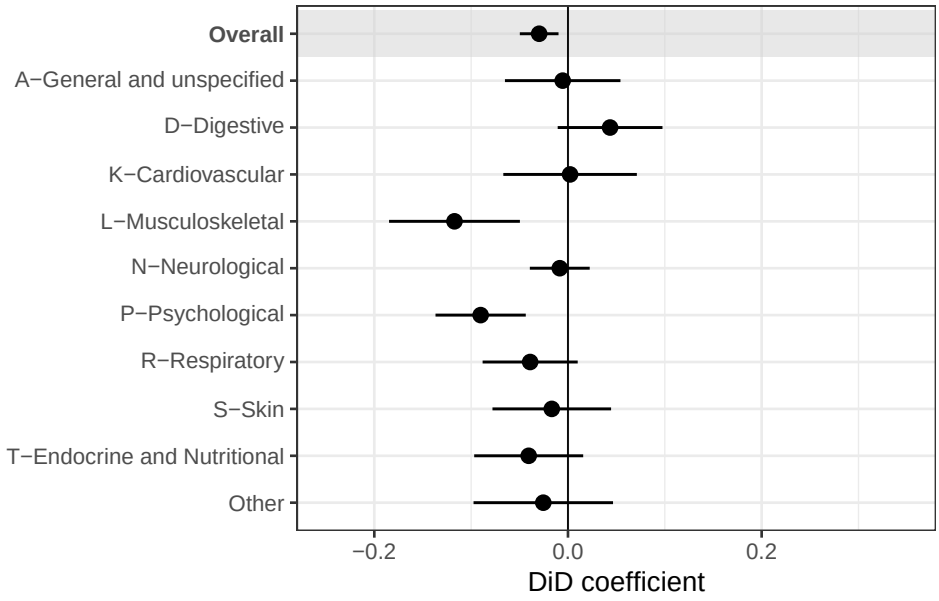
In the first row of panel (a) of Figure 3 we present the overall entry-effects. This point estimate essentially corresponds to taking averages across the yearly point estimates from Figure 2. Here we find a negative and statistically significant effect on GP visits of being elected as mayor. This suggests that winning political office may have a health-promoting effect, contrary to anecdotal evidence that often portrays political life as highly stressful and rife with harsh criticism. While many stories suggest that politicians experience deteriorating health due to the pressures of their role, our findings challenge this narrative. When disaggregating the analysis using the ICPC-2 classification system (second through eleventh rows of panel (a) in Figure 3), it becomes clear that this negative relationship is driven by a decrease in musculoskeletal and psychological issues. These are the two most prevalent types of health problems within this population, and the most common diagnoses underlying sickness absence. This pattern may indicate that individuals experience improvements in health while holding political office, or alternatively, it may reflect a higher threshold for sickness-related work absences among individuals serving in political office.

Panel (b) presents the corresponding results for exit from politics. We find no significant impacts on health, either overall or across different types of health problems. However, it should be noted that the exit analysis has relatively low statistical power,

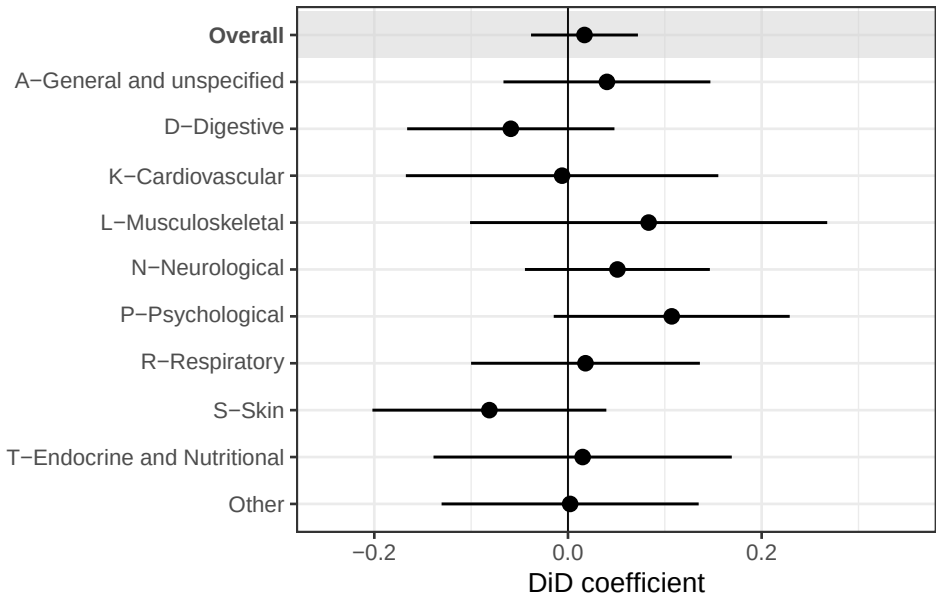
[12]Elections are held at the end of $t = 0$ and $t = 4$, hence we exclude these years from the analyses.

Figure 3: Effect by diagnosis of entry into and exit from office

(a) Entry effects on GP Visits



(b) Exit effects on GP visits



Notes: The figure displays estimates from difference-in-differences analyses of the effect of entering and exiting political office on the number of GP visits, disaggregated by ICPC-2 chapter. Error bars denote 95% confidence intervals. Standard errors were clustered on the level of treatment-control pairs (unique matching keys) for the entry model, and at the level of individual for the exit model. See Appendix Table A.2 for numerical results.

as it compares mayors who remain in office with those who leave.

5.4 Family members

The effect of being elected into a position as mayor could spill over to family members. Higher levels of work-related stress and demanding media attention could impact the share of the family burden carried by the partner. Moreover, there could also be direct impacts in terms of the family member attracting attention in traditional and social media.

As explained in Section 3, the Norwegian register data allow us to analyze the health outcomes of spouses of newly elected mayors. In Appendix Figure A.6 we show event plots analogous to Figure 2 for the spouses of newly elected mayors. We see that mayors are drawn from a pool where also the spouses have higher wages than those of spouses in the control group. Contrary to mayors, though, spouses do not see any effect on their income when the spouse is elected as mayor.

With respect to health, spouses of mayors appear very similar to those in the control group. Once again, we find no detectable health consequences associated with the spouse being elected as mayor. Thus, there is no evidence that stress or similar factors within the family lead to increased physician visits when someone is elected as mayor.[13]

6 Additional results on longevity

Much of the existing literature on the health consequences of political office focuses on a stark outcome—longevity. While some studies report surprisingly large positive effects (Borgschulte and Vogler, 2019; Barfort, Klemmensen and Larsen, 2021), these findings have been met with criticism (Albada, 2025; Gelman, 2022). To contribute to this debate, we examine the impact of winning office on longevity using historical Norwegian data. Our analysis applies the regression discontinuity (RD) design developed

[13] One might expect differences by gender, such as wives of male mayors being affected differently than husbands of female mayors. However, Appendix Figure A.7 shows no discernible effects.

by Fiva and Smith (2018), with full details provided in Appendix B.

We find no discernible effects on longevity, measured in days after the election and converted to years. Both close losers and close winners tend to survive, on average, about 28–29 years after the election. If anything, the RD estimate suggests that there is a small negative effect on lifespan. Indeed, we can reject large effects of either sign – in our preferred specification (Appendix Table B.1, Panel B, Column 6), the 95% confidence interval for serving one more term ranges from –2.2 to 1.1 years. Importantly, this excludes the large positive longevity effects reported in the RD studies using data from the United States (Borgschulte and Vogler, 2019; Barfort, Klemmensen and Larsen, 2021).

These findings draw on a dataset distinct from our main analysis, spanning a longer historical period and examining a cruder yet fundamentally important outcome—longevity. Nonetheless, our results are consistent with the primary conclusions presented earlier, suggesting that winning political office has limited, if any, impact on individual health outcomes.

7 Conclusion

Advancing into more senior roles is a central feature of working lives, yet its consequences for workers' health are not well understood. In this paper, we study a specific upward career transition: the first-time promotion to full-time political office. Because mayors hold key leadership roles in local government, the health consequences of such transitions matter both for labor-market outcomes and for political representation. Anecdotal accounts of politicians becoming sick from the pressures of office therefore raise concerns not only at the individual level but also for the recruitment and retention of political leaders.

In this paper, we address these issues using high-quality register data from Norway. This permits us to link all newly elected mayors to panel data on income and GP visits. Hence we can analyze the population of mayors, not only the ones whose stories appear in the media. Despite the potential stresses of the electoral process and the demands of

office, our difference-in-differences analyses show no substantial adverse health effects associated with assuming political office. If anything, we find that winning office is associated with a modest reduction in GP visits, particularly for musculoskeletal and psychological conditions.

Our historical analysis of longevity likewise finds no sizeable impact of winning office. This complementary evidence reinforces the broader conclusion that political office, at least in the Norwegian context, does not entail substantial personal health costs in either the short or the long run. We further show that mayors tend to be in better health prior to their election, suggesting that more resilient individuals are those who enter the political arena. This finding aligns with previous studies from Nordic countries, which document that candidates running for office are positively selected based on various measures of quality (Cox et al., 2021; Dahlgaard and Pedersen, 2025; Dal Bó et al., 2017; Jokela et al., 2025). Viewed through the lens of job promotions, our results are consistent with positive selection into demanding leadership positions.

Overall, our findings challenge the widespread notion that political office inevitably takes a toll on health. Instead, they highlight the ability of elected officials and the institutional environments in which they operate to absorb the pressures of leadership without compromising personal well-being. More generally, they illustrate how administrative data on health-care use, linked to sharply timed career transitions, can be used to study the health consequences of career advancement in other labour-market settings.

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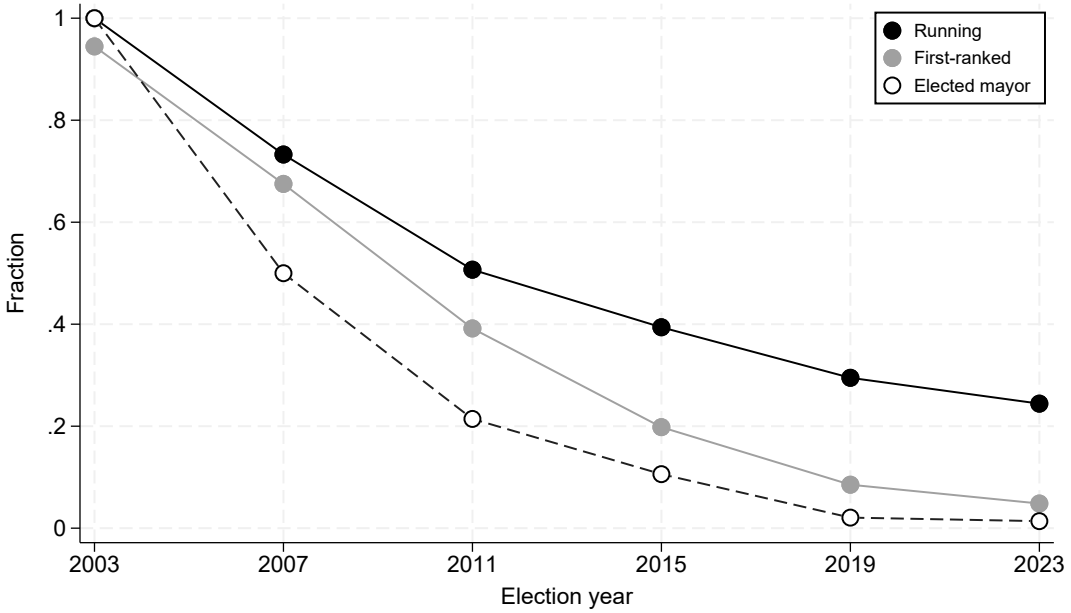
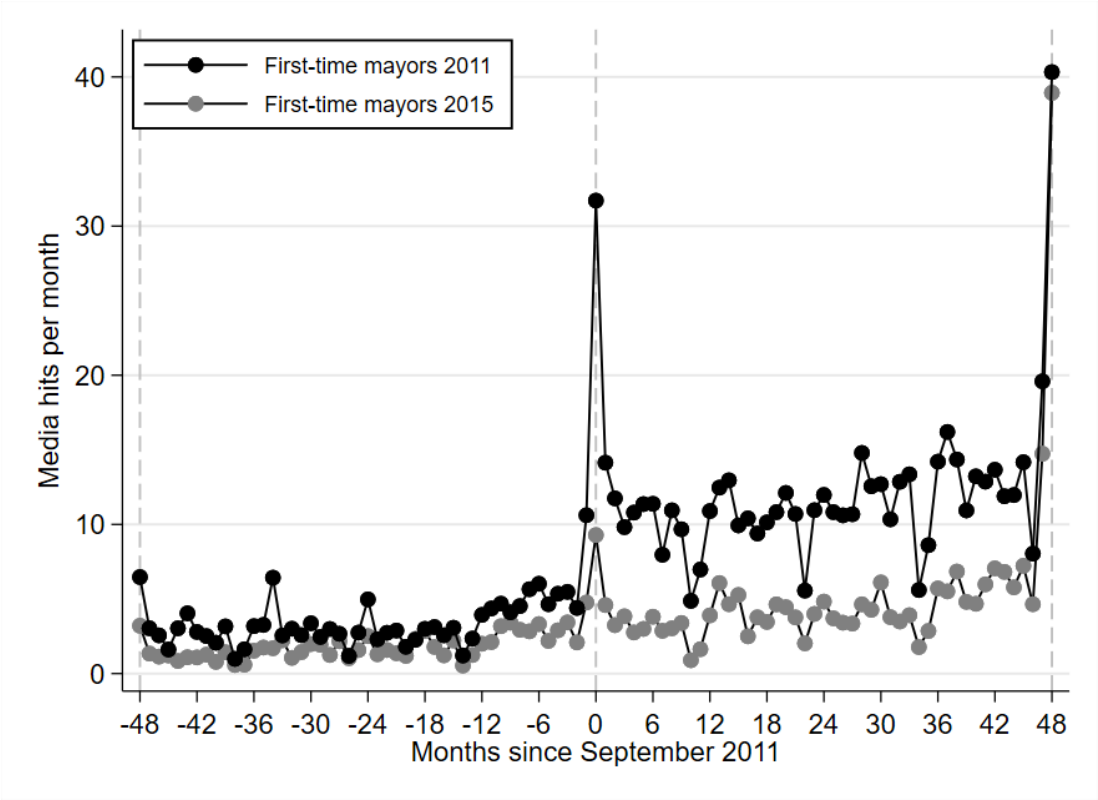
Appendix A: Supplementary figures and tables

Figure A.1: Local political careers of mayors elected in 2003



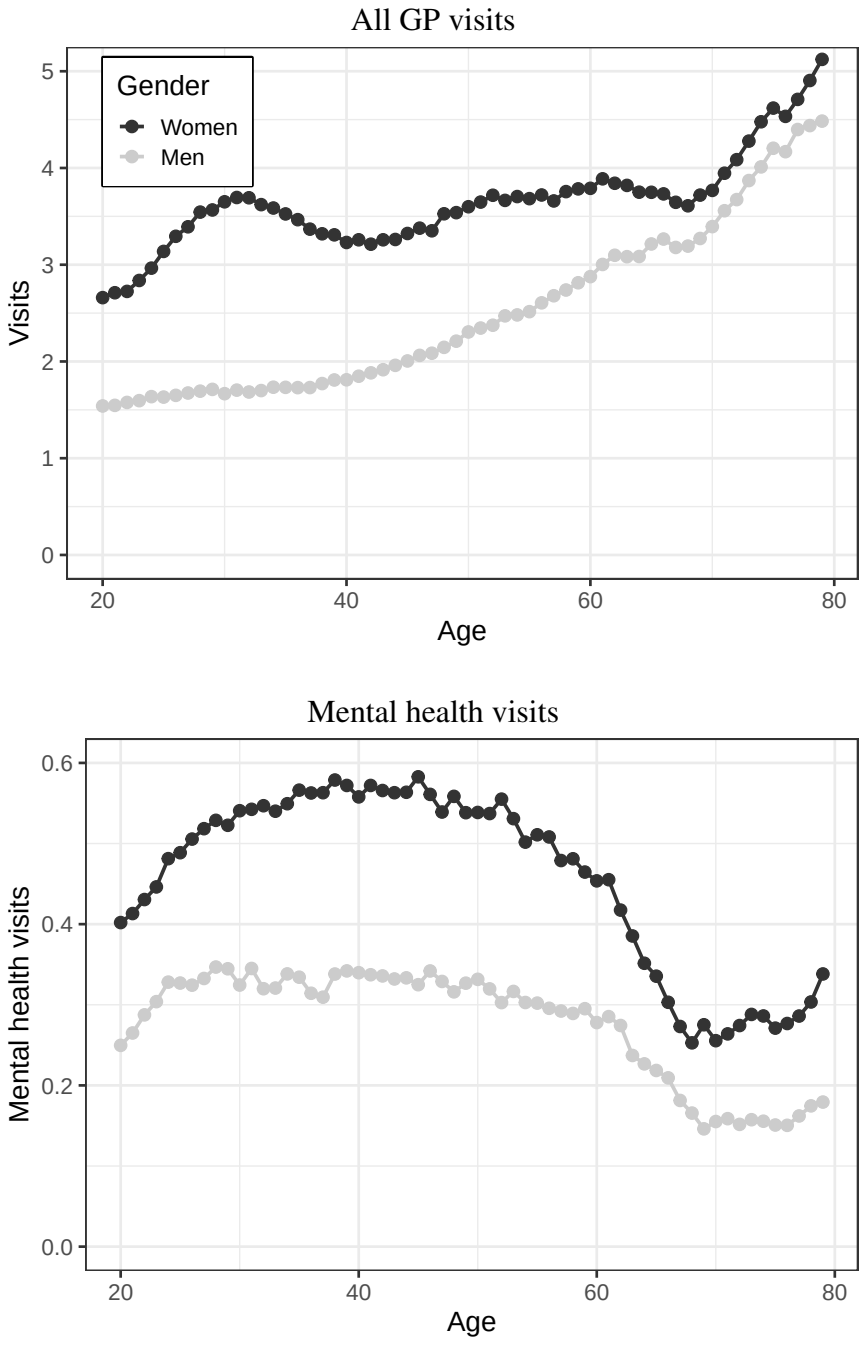
Note: This figure illustrates the subsequent local political careers of mayors elected in 2003 (N=434). Data from Fiva, Sørensen and Vøllo (2024).

Figure A.2: Monthly media hits over time, by first-time promotion to mayor



Note: This figure displays the average monthly media hits for first-time appointed mayors in 2011 and 2015, respectively (N=418). We drop 32 mayors that are not in the media data. The vertical dashed lines represent the election months (-48, 0, +48). The x-axis measures the number of months from the local election in September 2011.

Figure A.3: Average number of visits to general practitioner by age and gender (2018)

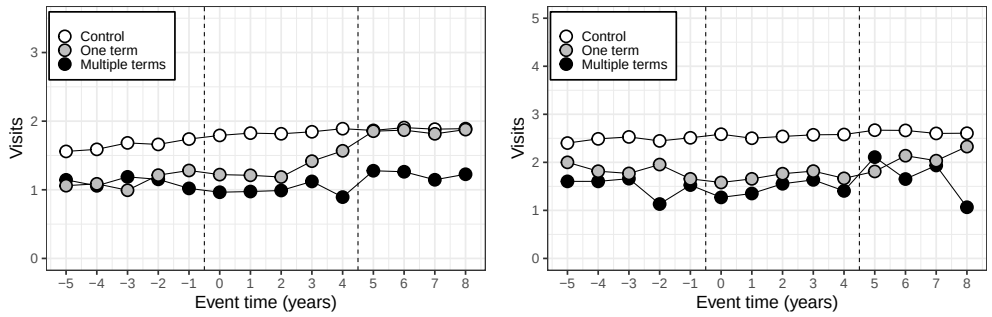


Note: The figure displays the average number of GP visits by age and gender, for any diagnosis (top panel) and mental health problems (bottom panel).

Figure A.4: Heterogeneous effects by gender: Total visits

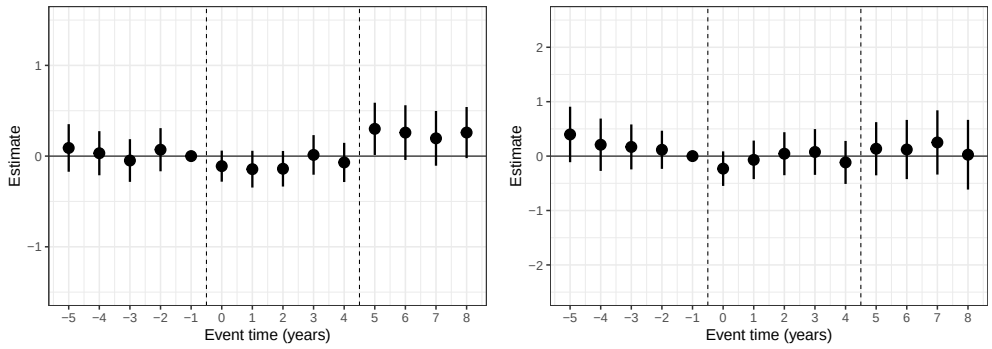
(a) Total visits – men

(b) Total vists – women



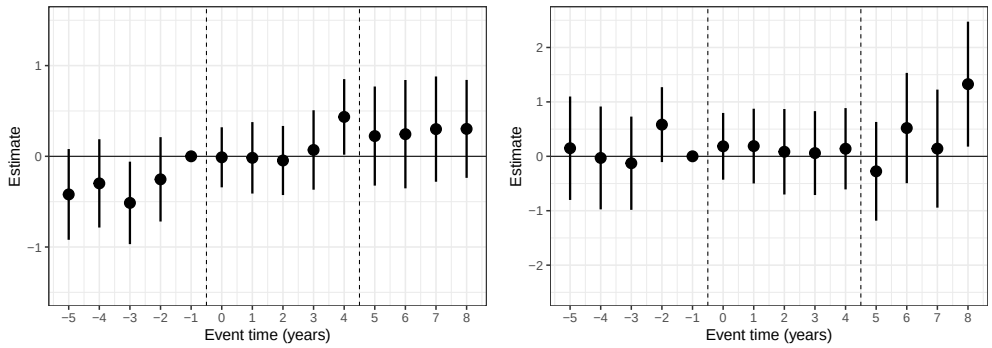
(c) Visits at entry – men

(d) Visits at entry – women



(e) Visits at exit – men

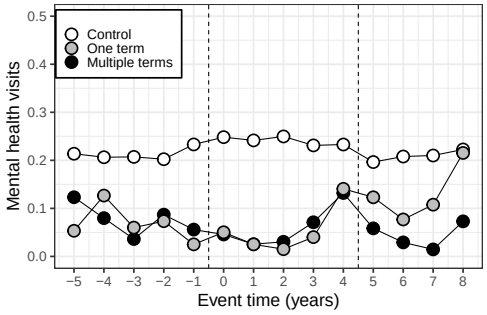
(f) Visits at exit – women



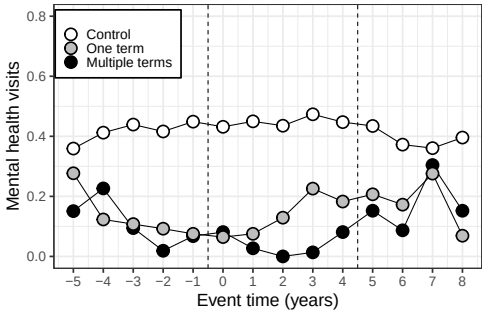
Note: Panel (a) plots the total number of health visits over event time for men and Panel (b) for women elected as mayor for the first time at event time $t = 0$ and the control group. The control group consists of non-politicians that have the same age, gender, education, and municipality of residence as the individuals in the treatment group. The middle and bottom panels contain estimates of the parameters of interest, β_τ, in Equation (1) together with 95% confidence intervals. Standard errors clustered at the individual level.

Figure A.5: Heterogeneous effects by gender: Mental health visits

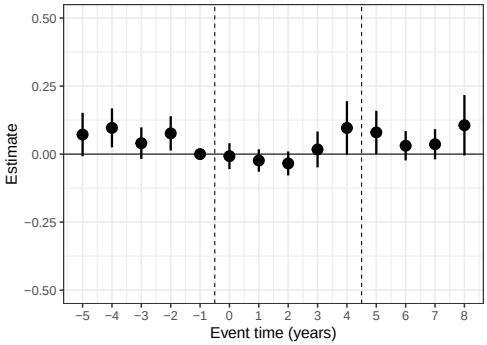
(a) Mental health visits – men



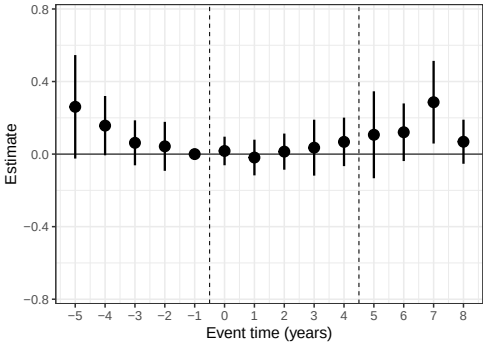
(b) Mental health visits – women



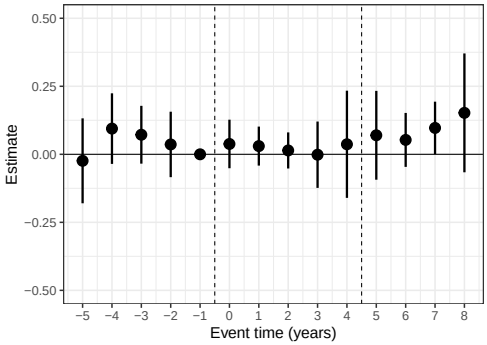
(c) Mental health visits at entry – men



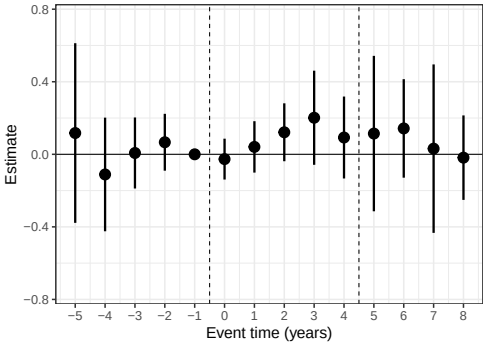
(d) Mental health visits at entry – women



(e) Mental health visits at exit – men

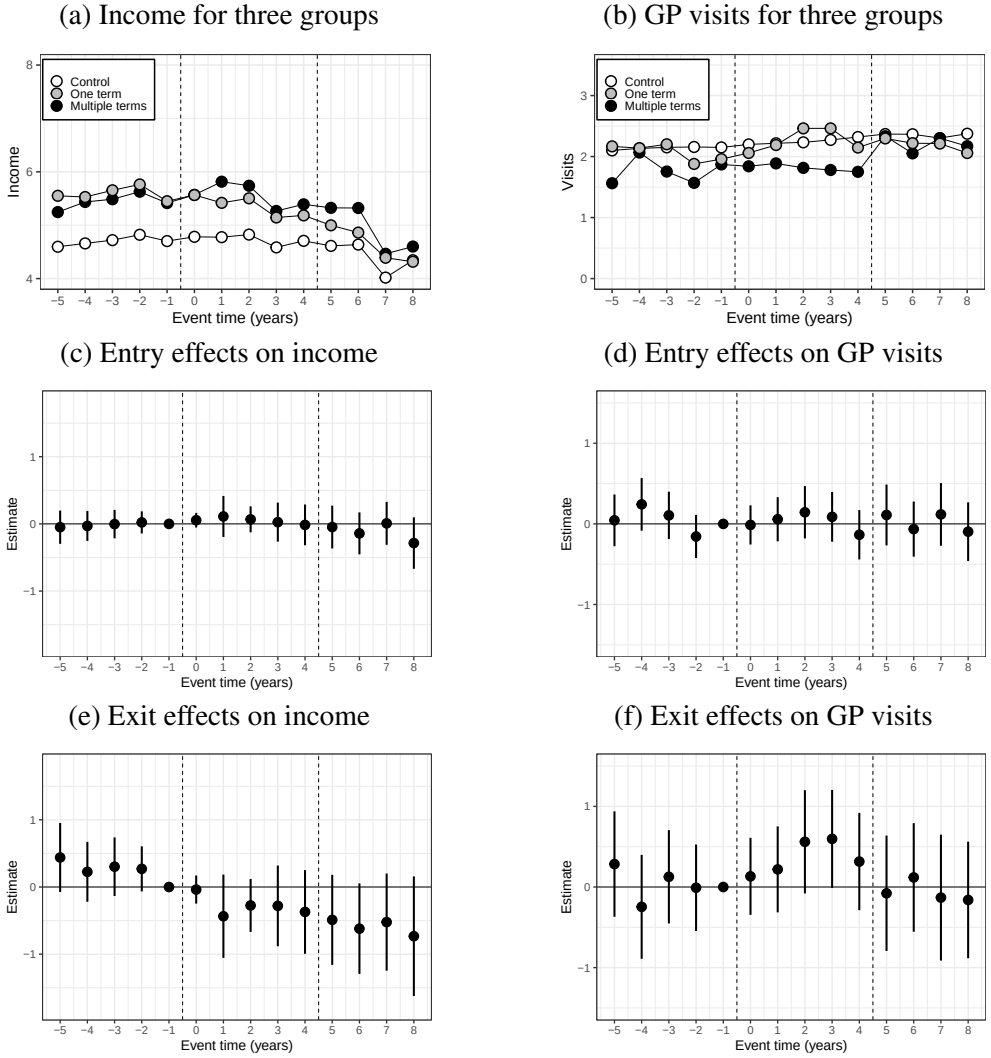


(f) Mental health visits at exit – women



Note: Panel (a) plots the total number of health visits over event time for men and Panel (b) for women elected as mayor for the first time at event time $t = 0$ and the control group. The control group consists of non-politicians that have the same age, gender, education, and municipality of residence as the individuals in the treatment group. The middle and bottom panels contain estimates of the parameters of interest, β_τ, in Equation (1) together with 95% confidence intervals. standard errors clustered at the individual level.

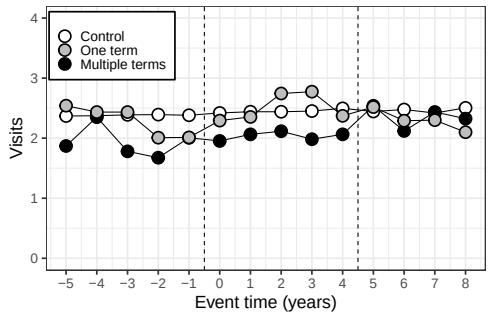
Figure A.6: Event Study Analyses of Mayoral Entry and Exit: Consequences for Their Partners



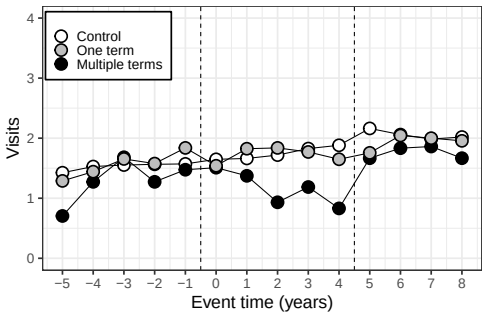
Note: Panel (a) plots partner's pensionable income measured in Gs over event time for individuals elected as mayor for the first time and staying for one term only (gray circles) and multiple terms (black circles) at event time $t = 0$ and the control group (empty circles). The control group consists of non-politicians that have the same age, gender, education, and municipality of residence as the individuals in the treatment group. Panel (b) plots the total number of health visits over event time for the same groups of individuals. Panels (c) and (d) contain estimates of the parameters of interest, β_r, in Equation (1) comparing all first time elected mayors to their control group. Panels (e) and (f) contain estimates of the parameters of interest, γ_r, in Equation (2), comparing individuals elected as mayors for the first time exiting after one period to individuals staying on for subsequent periods. All the event plots contain 95% confidence intervals with standard errors clustered at the politician-control group pair.

Figure A.7: Heterogeneous effects on partners by gender: Total visits

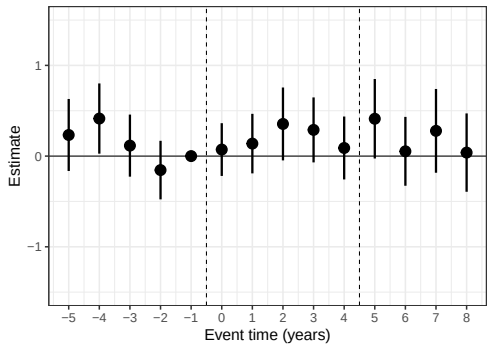
(a) Total visits – Partners of male mayors



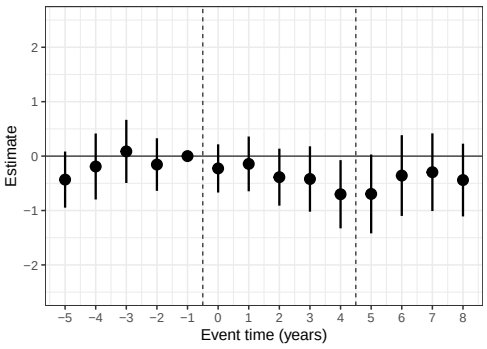
(b) Total vists – Partners of female mayors



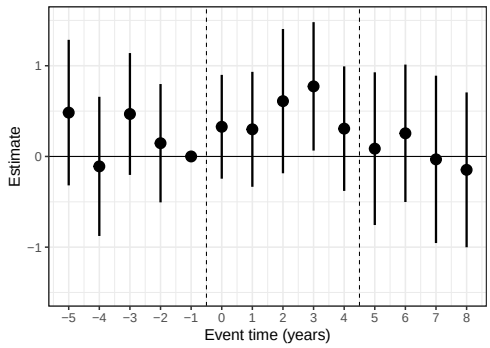
(c) Visits at entry – Partners of male mayors



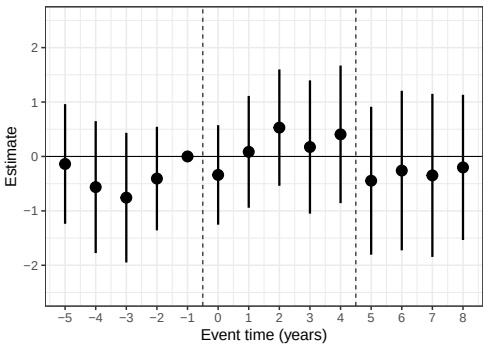
(d) Visits at entry – Partners of female mayors



(e) Visits at exit – Partners of male mayors



(f) Visits at exit – Partners of female mayors



Note: Panel (a) plots the total number of health visits over event time for men and Panel (b) for women elected as mayor for the first time at event time $t = 0$ and the control group. The control group consists of non-politicians that have the same age, gender, education, and municipality of residence as the individuals in the treatment group. The middle and bottom panels contain estimates of the parameters of interest, β_r, in Equation (1) together with 95% confidence intervals. standard errors clustered at the individual level.

Table A.1: Event Study Results: Income and GP Visit Effects of Mayoral Entry and Exit

Relative Time	**Left Panel: Income Effects**						**Right Panel: GP Visit Effects**					
	Entry Effects			**Exit Effects**			**Entry Effects**			**Exit Effects**		
	n_t	Coef.	SE	n_t	Coef.	SE	n_t	Coef.	SE	n_t	Estimate	Std. Error
-5	76,201	-0.198	0.227	406	0.393	0.437	76,201	0.178	0.121	406	-0.260	0.227
-4	76,199	-0.385***	0.135	406	0.057	0.264	76,199	0.082	0.113	406	-0.225	0.223
-3	76,199	0.265	0.366	406	0.695	0.710	76,199	0.013	0.105	406	-0.405**	0.205
-2	76,198	-0.021	0.094	406	-0.043	0.173	76,198	0.083	0.100	406	-0.023	0.196
-1	104,860	0.000	0.000	563	0.000	0.000	104,860	0.000	0.000	563	0.000	0.000
0	104,860	0.391***	0.073	563	0.042	0.131	104,860	-0.146*	0.078	563	0.038	0.149
1	104,860	2.224***	0.112	563	0.240	0.220	104,860	-0.122	0.090	563	0.029	0.173
2	104,860	2.081***	0.126	563	0.116	0.241	104,860	-0.085	0.092	563	-0.013	0.176
3	64,409	2.205***	0.158	371	-0.035	0.305	104,860	0.032	0.100	563	0.047	0.193
4	64,409	2.679***	0.650	371	1.270	1.255	104,860	-0.084	0.097	563	0.327*	0.183
5	64,409	0.693***	0.171	371	-3.460***	0.308	64,409	0.256**	0.126	371	0.083	0.239
6	64,409	0.978***	0.176	371	-3.454***	0.331	64,409	0.222*	0.135	371	0.314	0.260
7	28,662	1.268***	0.235	157	-3.204***	0.442	64,409	0.213	0.139	371	0.257	0.260
8	28,662	1.094***	0.238	157	-3.115***	0.451	64,409	0.196	0.138	371	0.549**	0.261
N	1,039,197			5,674			1,191,593			6,486		

Note: The table presents the estimated effects of becoming a mayor ("Entry effects") and stepping down as a mayor ("Exit effects"), derived from estimating Equations 1 and 2, respectively. Panel A displays the results for annual income, while Panel B presents the results for the annual number of general practitioner (GP) visits. Within each panel, the three columns under "Entry effects" report the number of observations in each relative time period (n_t), the estimates of β_r (Coef.), and their standard errors (SE), based on Equation 1. The three columns under "Exit effects" similarly report the number of observations in each relative time period (n_t), the estimates of γ_r (Coef.), and their standard errors (SE), based on Equation 2.

Table A.2: DiD Model Results for Different Health Problems

ICPC2 Chapter	Entry			Exit		
	Coef.	SE	p-value	Coef.	SE	p-value
Overall	-0.03***	0.01	0.0036	0.02	0.03	0.5452
A-General and unspecified	-0.01	0.03	0.8580	0.04	0.05	0.4610
D-Digestive	0.04	0.03	0.1156	-0.06	0.05	0.2802
K-Cardiovascular	0.00	0.04	0.9508	-0.01	0.08	0.9411
L-Musculoskeletal	-0.12***	0.03	0.0007	0.08	0.09	0.3765
N-Neurological	-0.01	0.02	0.5916	0.05	0.05	0.2952
P-Psychological	-0.09***	0.02	0.0002	0.11*	0.06	0.0855
R-Respiratory	-0.04	0.03	0.1194	0.02	0.06	0.7644
S-Skin	-0.02	0.03	0.5925	-0.08	0.06	0.1884
T-Endocrine and Nutritional	-0.04	0.03	0.1581	0.02	0.08	0.8471
Other	-0.03	0.04	0.4878	0.00	0.07	0.9737
Observations (N)	409,712†			3,104†		

Notes: For the "overall" models, the number of total observations were $N = 4,098,896$ *and* $31,048$*, respectively. The coefficients from a two-period difference-in-differences estimation, the reported coefficient is the interaction between a binary indicator for the treated group (mayors) and a binary indicator for time period (after entry or after exit). In the Entry model, the pre-periods were* $t = -5$ *to* $t = -1$ *and post-periods were* $t = 1$ *to* $t = 3$*. In the Exit model, the pre-periods were* $t = 1$ *to* $t = 4$ *and post-periods were* $t = 5$ *to* $t = 8$*. The control group in the entry model was a matched control group, while in the exit model is was mayors that stayed in office. Significance shown by* *** $p < 0.01$*,* ** $p < 0.05$*,* * $p < 0.1$

Appendix B: Political Office and Longevity

Existing studies examining the health consequences of holding public office typically use longevity as an outcome measure but yield conflicting and methodologically contested results. Two recent innovative studies based on regression discontinuity (RD) applied to data from the United States (Borgschulte and Vogler, 2019; Barfort, Klemmensen and Larsen, 2021) report substantial positive effects, suggesting that elected candidates live five or more years longer than their counterparts. While RD designs are among the most credible quasi-experimental methods, they are not without limitations. Specifically, the large positive findings from Borgschulte and Vogler (2019) and Barfort, Klemmensen and Larsen (2021) have been methodologically criticized by Albada (2025) and Gelman (2022), who argue these results may reflect statistical artifacts driven by noisy data and researcher degrees of freedom inherent to RD analyses.

To assess the generalizability of prior findings, we apply a similar RD design to a new context: Norwegian historical parliamentary elections. Our dataset, drawn from Fiva and Smith (2017), encompasses all parliamentary candidates following Norway's adoption of closed-list proportional representation (PR) in 1919.[14] Under this electoral system, voters cast ballots for political parties rather than individual candidates. Seats are then allocated to candidates based on the order determined by each party, meaning voters influence which parties gain representation but have no direct influence over individual candidate selection.

We adopt the RD design employed by Fiva and Smith (2018), which exploits district-level party vote counts to measure how close each candidate was to winning or losing a parliamentary seat, considering party vote shares, seats won, and candidate list positions. Candidate-specific birth and death dates, provided in the *Archive of Politicians* at *Norwegian Agency for Shared Services in Education and Research* (SIKT), allow us to analyze the mortality effects associated with holding office.[15] We

[14] Norway has fixed parliamentary term lengths: three years between 1921 and 1936, and subsequently four years from 1945 onward.

[15] For a small number of candidates, only the year of birth or death is available; in these cases, we use July 1st as the assumed date when calculating survival after the election.

keep elections up until 1981, because after this many candidates are still alive.[16]

Measuring electoral closeness in two-party, single-member district elections is straightforward, as a simple threshold (50% of the total vote) determines victory. However, in multi-member proportional representation systems, seat allocation depends on vote shares across *all* parties, lacking a fixed threshold for gaining an additional seat. To address this complexity, we implement the metric proposed by Folke (2014), defined as the minimum total vote change across all parties required to alter seat distribution for a given party. Figure B.1 illustrates the distribution of *Win Margin* for candidates that are either next in line to win a seat (marginal losers), or first in line to lose a seat (marginal winners). As previously reported by Fiva and Smith (2018), there is no evidence of candidate sorting around the electoral threshold, likely because precise prediction of seat thresholds is nearly impossible in proportional representation elections.

Our baseline empirical specification is a local linear regression of the form:

$$Y_i = \beta_0 + \beta_1 Seat_i + \beta_2 \textit{Win Margin}_i + \beta_3 \textit{Win Margin}_i * Seat_i + \xi_i, \qquad (3)$$

where $Seat_i$ is a dummy equal to one that is deterministically assigned to candidate i in election t if he or she has a positive win margin and therefore wins a regular seat in parliament.[17] Equation (1) allows the slope of the regression line to differ on either side of the cut-off by including interaction terms between *Win Margin* and $Seat$. ξ_i is an error term. We cluster standard errors at the candidate level.

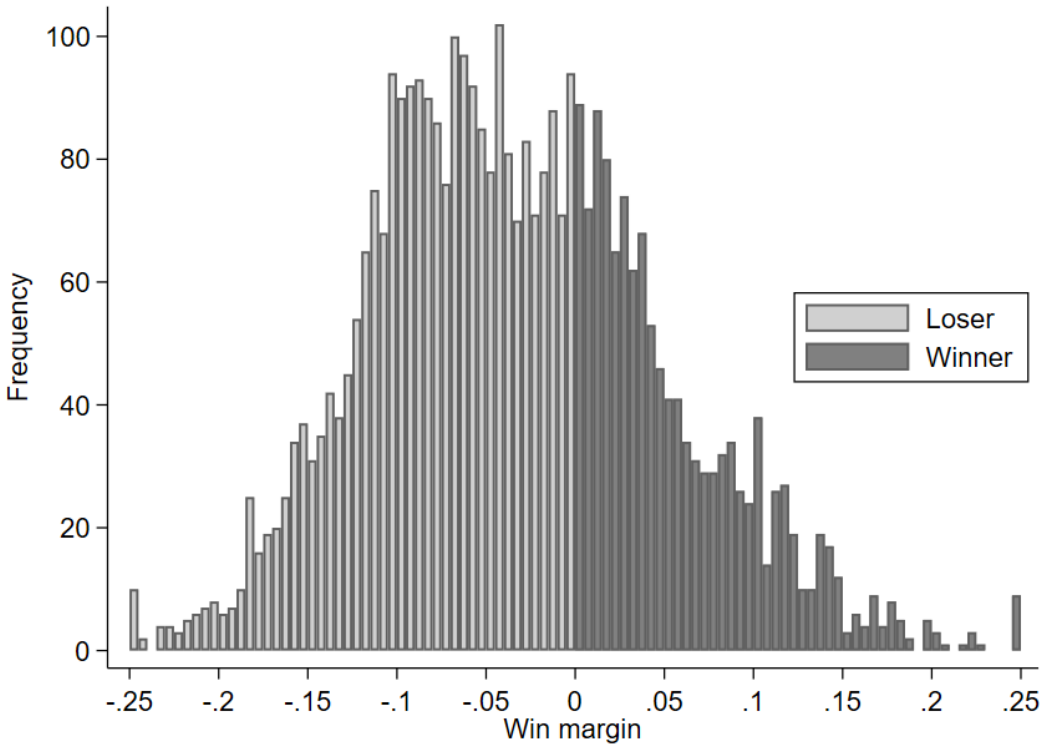
The dependent variable Y_i represents two distinct outcomes. First, we measure the total terms served by candidate i; this serves as our first-stage relationship. Second, our main outcome is candidate survival, measured in days after the election and converted to years.

Following Fiva and Smith (2018), we provide graphical evidence using a common

[16] Within a five percentage point window, we lack information about year of birth or year of death for 15.7% of candidates. Our main results are substantially unaltered if we extend our sample period to, e.g., 2001.

[17] In the 1930–1949 period, parties were allowed to form *listeforbund*, an arrangement where parties—each with their own list and candidates—could pool their votes for the purpose of seat allocation if doing so would result in more seats overall. As a result, it was possible for some candidates with a negative win margin to be awarded a seat. Our analysis ignores this specific institutional arrangement.

Figure B.1: Distance to seat threshold [Win Margin] for marginal candidates



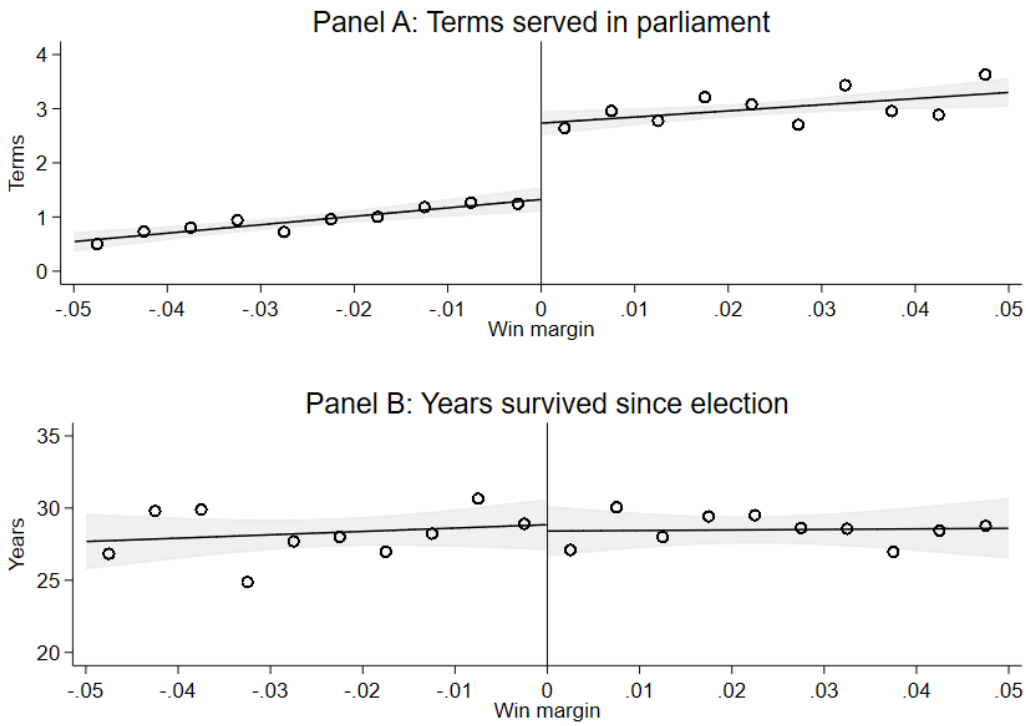
Note: Each bin represents a 0.5 percentage-point interval. Sample consists of candidates that are either next in line to win a seat (marginal losers), or first in line to lose a seat (marginal winners) in the 1921–1981 period. The unit of observation is candidate-year. The figure is truncated at -0.25 and $+0.25$.

bandwidth of five percentage points for *Win Margin*. Figure B.2 depicts local averages calculated within half-percentage-point intervals, with separate regression lines fitted on each side of the discontinuity. The vertical line at zero indicates the transition between marginal losers and marginal winners.

Panel A of Figure B.2 shows the results for *terms served*. We identify a clear and substantial difference in terms served between marginal winners and losers. Specifically, marginal losers serve, on average, slightly above one term, whereas marginal winners serve, on average, closer to three terms.

Panel B addresses our primary outcome: *candidate longevity*, measured in days after the election and converted to years. Candidates on either side of the electoral cutoff tend to survive close to 30 years, on average, following the election. Importantly, we observe no clear discontinuity in longevity at the electoral threshold, indicating no measurable average causal impact of parliamentary office on lifespan in our context.

Figure B.2: RD plots



Note: Sample restricted to candidates running in the 1921–1981 period who are less than 5 percentage points away from the seat threshold. Each bin represents an interval of half a percentage point. Separate linear regression lines are estimated to the left and right of the discontinuity using the underlying data, not the binned scatterpoints. Shaded areas represent 95% confidence intervals.

Table B.1 presents the regression results, using the mean-square-error optimal bandwidth for each model identified by the algorithm developed by Calonico et al. (2019). Column (1) reports the baseline estimates based on Equation (1), while Columns (2) through (5) sequentially introduce fixed effects for election year, party, district, and list rank, as in Fiva and Smith (2018). Column (6) includes additional controls for candidate gender and age.

In line with the graphical evidence, we find a substantial effect on terms served: marginally elected candidates serve about 1.4 additional terms (Panel A, Column 1). The effect is statistically significant and remains robust to the inclusion of the full set of fixed effects and controls (Columns 2–6).

In contrast, we find no statistically significant effects for our primary outcome of interest, post-election survival (Panel B). The point estimates are consistently negative, and the confidence intervals rule out substantial positive effects relative to the mean

survival of about 28.5 years.

Adding controls for candidate age (at the time of the election) and gender substantially improves explanatory power—the R^2 increases from 0.14 to 0.39 (Panel B, Column 6)—and reduces the standard error of the RD estimate.[18] We therefore consider this our preferred specification.

Based on this specification, the reduced-form 95% confidence interval ranges from –3.01 to 1.58 years. Dividing by the first-stage coefficient of 1.38 yields the confidence interval for the effect of serving one additional term: from –2.18 to 1.14 years.[19] Importantly, this excludes the large positive longevity effects reported in recent U.S.-based RD studies (e.g., Borgschulte and Vogler, 2019; Barfort, Klemmensen and Larsen, 2021).

[18]The point estimate itself remains largely unchanged, suggesting no systematic differences in these characteristics around the cutoff. This supports the validity of the RD design. See Fiva and Smith (2018) for additional balance and RD validation checks.

[19]This null finding is robust to varying the bandwidth around the seat threshold from 2%-points to 10%-points, as shown in Figure B.3.

Table B.1: RD estimates

Panel A: Terms served

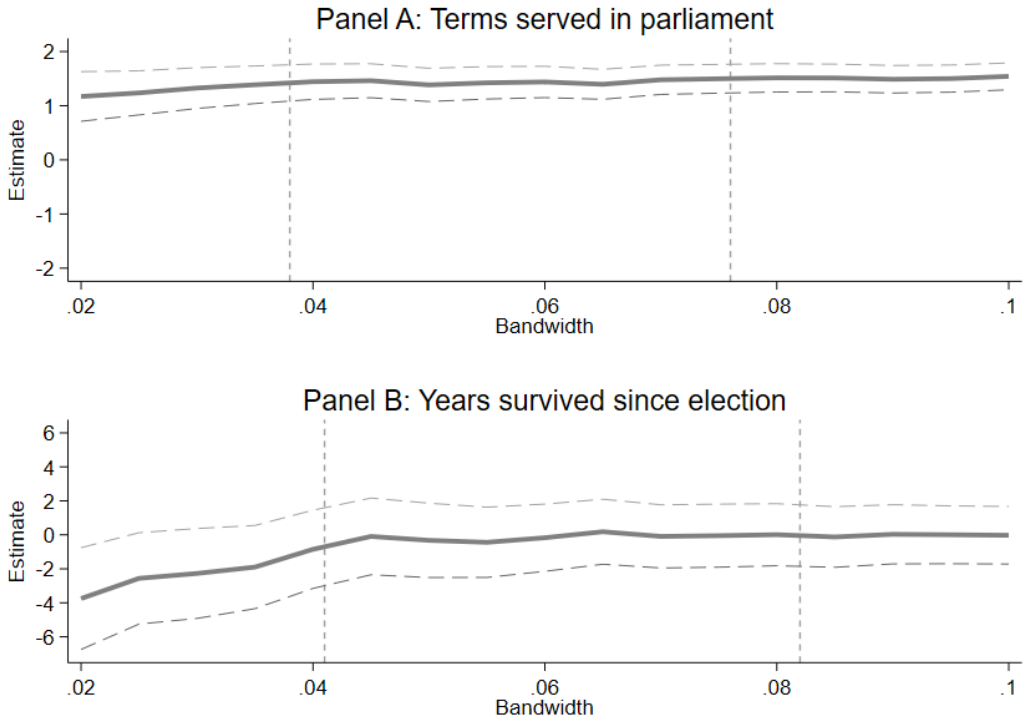
	(1)	(2)	(3)	(4)	(5)	(6)
RD estimate	1.366	1.337	1.396	1.393	1.379	1.378
	(0.177)	(0.174)	(0.172)	(0.169)	(0.168)	(0.170)
female						-0.145
						(0.179)
age						-0.025
						(0.007)
R^2	0.29	0.30	0.36	0.40	0.41	0.41
N	1184	1184	1184	1184	1184	1112
Mean Y	1.95	1.95	1.95	1.95	1.95	1.95
BW	0.038	0.038	0.038	0.038	0.038	0.038
Year FE	No	Yes	Yes	Yes	Yes	Yes
Party FE	No	No	Yes	Yes	Yes	Yes
District FE	No	No	No	Yes	Yes	Yes
Rank FE	No	No	No	No	Yes	Yes

Panel B: Years survived since election

	(1)	(2)	(3)	(4)	(5)	(6)
RD estimate	-0.772	-0.799	-0.525	-0.651	-0.610	-0.718
	(1.410)	(1.392)	(1.395)	(1.377)	(1.374)	(1.170)
female						2.761
						(1.338)
age						-0.818
						(0.047)
R^2	0.00	0.04	0.07	0.13	0.14	0.39
N	1109	1109	1109	1109	1109	1107
Mean Y	28.37	28.37	28.37	28.37	28.37	28.37
BW	0.041	0.041	0.041	0.041	0.041	0.041
Year FE	No	Yes	Yes	Yes	Yes	Yes
Party FE	No	No	Yes	Yes	Yes	Yes
District FE	No	No	No	Yes	Yes	Yes
Rank FE	No	No	No	No	Yes	Yes

Note: Each cell represent RD estimates based on equation (1) using the mean-squared-error optimal bandwidth calculated by the Calonico et al. (2019) algorithm. All specifications include separate linear control functions on each side of the discontinuity. Standard errors clustered at the candidate level are in parentheses.

Figure B.3: Robustness of RD results to alternative bandwidths



Note: Graphs display the RD estimates and 95% confidence intervals as a function of the bandwidth chosen for various outcome variables (given in the title of each panel). The left-most vertical lines in each panel mark the optimal bandwidth chosen by the Calonico et al. (2019) method, as obtained by the *rdrobust* module in Stata. These correspond to specification (5) in Table B.1. The right-most vertical lines mark twice the optimal bandwidth from the Calonico et al. (2019) method.