Child Penalties in Politics*

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Abstract

Women tend to experience a substantial decline in their labour income after their first child is born, while men do not. Do such 'child penalties' also exist in the political arena? Using comprehensive administrative data from Norway, we find that women are less likely than men to secure elected office after their first child is born. The effects manifest already from the nomination stage, where mothers receive less favourable rankings on party lists relative to comparable fathers. This paper broadens our understanding of a fundamental social issue in political representation and demonstrates how motherhood affects even positively selected women.

Keywords: gender gap, child penalties, political selection

JEL Classification: D63, D72, J13, J16

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1. Introduction

Despite considerable reductions in gender inequality over time, parenthood continues to shape the gender income gap. Even in Scandinavia, a part of the world considered to be among the most gender-equal, the event of having a child creates a long-run gender gap in earnings of 20–30%. In countries characterised by more gender-conservative views, such as Germany and Austria, the estimated 'child penalties' are around twice as great (Kleven *et al.*, 2019a). There are several reasons that may explain why men's and women's career paths diverge after parenthood, including career interruptions due to parental leave and greater continuing child-rearing responsibility as children grow older. Goldin (2014, 2021) argues that the gender gap would be considerably reduced if firms did not have an incentive to disproportionately reward individuals who are willing to work long and unpredictable hours. With a strongly convex earning structure, the rational response is for one parent to specialise in lucrative 'greedy work', and for the other – typically the mother – to prioritise the children.

The political arena shares many of the characteristics of the 'greedy jobs' highlighted by Goldin (2014, 2021), such as law, accounting, and finance. In politics, one is expected to work long and unpredictable hours; there is a well-defined career ladder; and the 'prizes', both in terms of income, but also prestige, are mostly found at the top of the political hierarchy (Bernhard *et al.*, 2021; Cirone *et al.*, 2021; Foos and Gilardi, 2020; Silbermann, 2015). Can the combination of career-based political selection and motherhood explain why women continue to be underrepresented in politics? This important question has not yet received much attention in the literature.

The importance of addressing the gender gap in political representation stems from three fundamental reasons. First, starting from the position that innate talent is equally distributed between men and women, it must be the case that superior political outcomes would be achieved if women had the same odds as men to make it to the top of the political hierarchy (Bertrand, 2018). Second, the extensive capabilities of the modern

state allow political leaders to exert wide-ranging positive—and negative—influence on social and economic progress. If having more women in political offices enables a better representation of the overall societal preferences, it is vital to address gender imbalances in political representation (Hessami and da Fonseca, 2020).[1] Third, women in public office may serve as role models that can improve perceptions of female leader effectiveness and weaken gender stereotypes in society (see, e.g., Beaman *et al.*, 2009; Gilardi, 2015; Ladam *et al.*, 2018).[2]

In this paper we use Norwegian administrative data and an event-study framework to examine the career trajectories of politicians who become parents. Norway is an interesting case to study for several reasons. First, even though international rankings put Norway at the very top when it comes to opportunities for women, a substantial gender wage gap still remains (Andresen and Nix, 2022; Bütikofer *et al.*, 2018). As we show below, there is also a considerable gender gap in political representation despite the fact that most political parties introduced gender quotas decades ago. Second, we have access to administrative data covering the entire Norwegian adult population (of around 4.2 million) that is merged with data on the universe of candidates running for political office. The detailed data allow us to study how having children affects the political careers of men and women in the short and long run, as well as their labour market outcomes.

We first present evidence showing that women who make it to the top of the political hierarchy in Norway have fewer children than men in the same positions. These descriptive findings resemble existing evidence from other settings (Schwarz and Coppock, 2022;

[1] The citizen-candidate framework suggests that politicians' identities matter for public policy (Besley and Coate, 1998; Osborne and Slivinski, 1996). In line with this prediction, many studies have documented that candidates' gender causally affects public policies (see, e.g., Chattopadhyay and Duflo, 2004; Bhalotra and Clots-Figueras, 2014; Baskaran and Hessami, 2023). The findings in this literature are not, however, unequivocal (see, e.g., Bagues and Campa, 2021; Ferreira and Gyourko, 2014). Hessami and da Fonseca (2020) provide a literature review.

[2] The evidence for this hypothesis is also somewhat mixed. For example, Bhalotra *et al.* (2017) reject that new female candidates are induced to enter politics in Indian constituencies in which a woman won the previous election. Instead, it seems that barriers to entry intensify in areas characterised by relatively high levels of gender prejudice. Related literature has focused on the importance of executives' gender in determining company policies and gender-specific labour outcomes. Murray *et al.* (2023) use data from the United States to investigate whether the gender composition of executives at the company, defined as top earners, affects the labour market outcomes of new mothers. They find no evidence that the earning losses of new mothers are related to the proportion of executives who are female.

Teele *et al.*, 2018). In our main analysis, we study how the arrival of children affects future outcomes among candidates who ran for office before having their first child. We document that the probability of winning a seat in subsequent elections decreases twice as much for women compared to men in the same situation. These effects remain strong and statistically significant for several election periods into the future. They also translate to pervasive differences in the probability to attain local political leadership positions.

Norwegian local elections are decided by a flexible-list system whereby both political parties and voters affect candidate selection. This institutional feature is helpful for understanding the mechanisms that drive our main results. In supplementary analyses, we document that child penalties already materialise in the nomination process. We find that mothers are less likely to be ranked favourably by parties relative to comparable fathers and this impedes the advancement of women's political careers in the long run. There is no clear evidence that voters discriminate against mothers (if anything the data suggest the opposite). We also present suggestive evidence that more family-friendly working environments might reduce women's underrepresentation in politics.

This paper contributes to our understanding of the formation of gender gaps in political outcomes. We engage with two overarching bodies of literature. First, we relate conceptually and methodologically to the growing literature on labour supply responses to parenthood.[3] Across a wide range of countries, such as Denmark (Kleven *et al.*, 2019b), Sweden (Angelov *et al.*, 2016), Norway (Andresen and Nix, 2022; Bütikofer *et al.*, 2018), Italy (Casarico and Lattanzio, 2023) and the United States (Chung *et al.*, 2017; Kuziemko *et al.*, 2018; Murray *et al.*, 2023), parenthood has been found to have great and persistent effects on the labour market outcomes of women, but not of men. To the best of our knowledge, our paper is the first to estimate child penalties in the political arena.

[3]We refer to the 'child penalty' literature, using the event-study methodology, which has become popular in recent years. However, research on the role of parenthood in explaining labour market outcomes for men and women dates back considerably further; some influential examples include Angrist and Evans (1998); Bertrand *et al.* (2010); Goldin (2014); Lundberg and Rose (2000); Lundborg *et al.* (2017); Paull (2008); Sigle-Rushton and Waldfogel (2007a,b) and Waldfogel (1998). The term 'child penalty' was used in this context at least as early as Waldfogel (1995). The career costs of children are also sometimes referred to as 'motherhood penalties' (see, e.g., Correll *et al.*, 2007).

Our paper demonstrates that child penalties exist in other parts of society besides traditional labour markets. A related study by Kim and Moser (2023) uses historical biographical data from the United States (1946-1964) to investigate the long-run effects of motherhood in science. More specifically, the authors examine how children affect the life-cycle publications of mothers relative to other scientists, and how differences in the timing of productivity influence promotions. The authors find a large, albeit transient, impact of children on scientific output. Taken together with the results of our study, this evidence suggests that children strongly affect the careers of mothers even in professions where women are positively selected prior to having children.

Second, we contribute to the vast literature on the underrepresentation of women in politics. Several studies have documented that when women run for office, they fare at least as well as their male counterparts (see, e.g., Anastasopoulos, 2016; Lawless, 2015; Wasserman, 2023).[4] This has prompted some scholars to investigate the institutions and mechanisms through which candidates are driven to seek elected office in the first place to understand the gender gap. One prominent view is that men and women have different ambitions; among individuals of equal merit, women are less often encouraged to seek elected office and perceive themselves as less qualified than men (Fox and Lawless, 2004, 2005).[5] Other scholars have focused on the extent to which there are gender differences in the *political persistence* of men and women. The results are mixed; while Bernhard and de Benedictis-Kessner (2021) find that male and female candidates are similarly persistent after losing elections, Brown *et al.* (2021) and Wasserman (2023) reach the

[4]Even if women 'win elections as often as men', a gender bias may still exist if female politicians have to be of a consistently higher quality than men in order to achieve gender parity (Anzia and Berry, 2011). In a survey of the literature, Krook (2018, p. 183) concludes that 'existing work is ambivalent as to the nature and effects of bias against female candidates'. While survey experiments, such as Teele *et al.* (2018), suggest that outright discrimination or double standards (unconscious biases) are no longer important drivers of the gender gap in political outcomes, Le Barbanchon and Sauvagnat (2021) find that female candidates obtain fewer votes in areas with less favourable attitudes towards women, and are less likely to run for election in these areas.

[5]Women also receive less attention from political recruiters (Fox and Lawless, 2010), and face increased barriers to entry in the form of tougher competition in primary elections (Lawless and Pearson, 2008), less media coverage (Kahn, 1994) and having to work harder to secure funding (Barber *et al.*, 2016; Jenkins, 2007). Thomsen and King (2020) point to the lack of women in pipeline professions as another important reason for the gender gap.

opposite conclusion (stronger attrition among women).[6] By studying political careers surrounding parenthood, we contribute to a better understanding of one of the most pressing social issues in political representation.

2. Empirical case: Norway

2.1 *Elections*

Norway is a unitary state with three levels of government. National legislative elections take place every fourth year. Sub-national elections, for local and regional councils, also occur every fourth year, but the timing is staggered by two years relative to the national election cycle. Elections are decided by list-based proportional representation (*PR*). In national elections, voters choose among competing lists, and candidates are elected in the order in which they appear on the ballot (*closed-list PR*). At the sub-national levels, voters can affect the election outcome by casting personal votes for specific candidates (*flexible-list PR*).

In this paper, we focus primarily on local government elections, which serve as the starting point for most political careers in Norway (Cirone *et al.*, 2021). All Norwegian citizens who are at least 18 years old in the election year, as well as foreign nationals who have lived in the country for at least three years on a continuous basis, can run for office. Local councillors have the political responsibility for important welfare services, such as child-care, elderly care and compulsory schooling. In total, local governments employ around 17% of the Norwegian workforce.

Elections are held on the second Monday of September, but preparations begin up to a

[6] All of these studies use regression discontinuity designs and data from the United States. In addition, Cipullo (2023) uses data from partisan primaries for the United States and run-off elections for mayors in Italy, and finds that women who barely qualify for the final round are less likely to be elected than barely qualified men. Gagliarducci and Paserman (2012) find that the probability of early termination of Italian municipal legislatures is higher when they are headed by female mayors. The effect is particularly pronounced when the female mayor heads an all-male council. In France, Lassébie (2020) finds that women are more likely than men to leave the political arena after serving for only one term, and the greater propensity of women to exit is correlated with local gender norms concerning the role of women in society.

year before with a closed and non-standardised nomination process.[7] The election system allows parties to give certain candidates a 'head start', which corresponds to 25% o,pof the total number of votes received by the party. This bonus makes it almost impossible for other candidates to compete with 'head start' candidates from the same party (Fiva and Røhr, 2018). In the voting process, voters are required to select a party list and have the option to assign personal votes to specific candidates by marking checkboxes on the party list. This, combined with candidates' bonus, yields the poll which forms the basis of the *within-party* distribution of seats.[8]

After the election, newly-elected local councils are formed by the end of October. An executive board, consisting of five to 19 members of the council, is elected at the initial assembly. The board serves for four years and is led by the mayor, who is also elected by the council.

In Norway, as in the other Scandinavian countries, being a local politician is a part-time position that is typically held concurrently with other jobs. The economic returns from holding an ordinary seat on a local council are small or non-existent (Berg, 2020; Cirone *et al.*, 2021; Kotakorpi *et al.*, 2017).[9] Being a mayor, however, is a full-time, well-paid job. Cirone *et al.* (2021) document that, on average, candidates gain around 25% in their income when they become mayors, compared to similar, non-promoted candidates.

[7]Lists are required by law to be submitted to the municipal government no later than 31 March in an election year. Election authorities then have until 1 June to formally approve the lists. At this point, it is generally not possible for a candidate to withdraw from either a list or from political appointment, if elected to office.

[8]The poll for candidate i running for party l is given by:

$$Poll_{il} = \begin{cases} PersonalVotes_i & \text{if } i \text{ has no head start} \\ PersonalVotes_i + 0.25 \cdot PartyVotes_l & \text{if } i \text{ has a head start at list } l \end{cases}$$

The initial ranking on the ballot comes into play solely when a tie occurs in the poll between two or more candidates. The distribution of seats *across parties* is determined by the number of *party votes* cast for the respective parties (Fiva and Røhr, 2018).

[9]Appendix Figure A.1 documents that almost half of municipal council members in 2011 received no additional remuneration besides standard meeting compensation. Most of the remainder received less than NOK 50,000 (USD 6,200). For comparison, the median income in 2011 was NOK 349,000 (USD 43,200). Executive board members also received modest fees.

2.2 *Political selection*

In the 1970s and 1980s, most Norwegian parties introduced gender quotas requiring that all ballots should feature at least 40% women (Fiva and Smith, 2017).[10] During these decades the fraction of women elected to parliament quadrupled from around 10% to about 40%. A 1992 legislative reform further mandated executive boards at the local level to comprise as a minimum 40% politicians of each gender.[11] The current status of these quotas is summarised in the second column of Table 1.

The third column of Table 1 shows that women account for around 41–45% of candidates across levels of government for which quotas are enforced.[12] Among municipal mayors, women account for less than 25%. While this is strong evidence of the effectiveness of gender quotas in terms of compliance, it is perhaps surprising that, decades after their implementation, these measures do not appear to have stimulated a further increase in the proportion of female representatives beyond that which is decreed by the quotas directly. Taken together with the findings from Bertrand *et al.* (2019), this suggests that one should be wary of placing too much faith in gender quotas as a solution to the persistent underrepresentation of women in the top layers of business and politics.

The remainder of Table 1 provides descriptive evidence of political selection for the three levels of government, separately for female and male politicians. For comparison, we include a row with information about the general adult population. There are three key takeaways from this table. First, even though women and men who run for entry-level political office (local and regional councils) have around the same number of children, there are substantial differences at the top of the hierarchy (members of parliament; cabinet ministers), where women have an average of 1.70 and 1.51 children, respectively,

[10] The scope of these quotas vary by political party. Currently, two of the major national parties (*AP* and *Rødt*) employ 50% gender quotas, five maintain 40% quotas (*SV*, *SP*, *V*, *MDG* and *KrF*) and two have no quotas at all (*H* and *FrP*).

[11] Geys and Sørensen (2019) evaluate this reform and find little evidence that female politicians – or women more generally – have become more empowered politically in the aftermath of the reform beyond their increased presence on the executive boards themselves.

[12] Appendix Figure A.2 shows some evidence that the gender gap in local politics might be closing for the younger cohorts.

compared to 1.82 and 1.73 for men.[13] Second, across all governmental levels, female politicians are better qualified than men, as measured by their level of education, in line with existing research (see, e.g., Anzia and Berry, 2011; Baltrunaite *et al.*, 2014; Besley *et al.*, 2017; Dal Bó *et al.*, 2017). Third, while men tend to have higher pre-election incomes than women at the sub-national levels of government (and in the population in general), this evens out at the national level.[14] Overall, the descriptive patterns in Table 1 suggest that female politicians are more positively selected than male politicians, and that these women – like female scientists (Kim and Moser, 2023) – have internalised the career costs of having children. Appendix Table A.1 substantiates these findings by reporting t-tests for the differential effects of gender on education and income. Almost all differences are statistically significant at the 1% level.

2.3 *Parental leave and child care*

Outside the realm of politics, the Norwegian welfare state actively promotes the combination of employment and family responsibilities for all parents. Firstly, a generous parental leave programme provides incentives for both fathers and mothers to take time off work during their child's first year. Salary-compensated parental leave has seen multiple extensions since the 1970s, beginning with 18 weeks of leave with full salary compensation in 1977 and reaching 49 weeks in 2021. In 1993, Norway became the first country in the world to reserve part of the parental leave period explicitly for fathers, resulting in a significant shift in fathers' leave-taking behaviour while mothers continued to take the longest periods of leave (Cools *et al.*, 2015).

Secondly, affordable high-quality child care facilitates dual-earner families as the children grow older. In 2000, close to 80% of 3-5 year-olds were enrolled in formal child care (Andresen and Havnes, 2019). However, there was still significant unmet demand,

[13] These numbers are similar to those that Teele *et al.* (2018) report from the United States. Among representatives in the 115th US Congress (2017–2019), women have an average of 1.5 children, compared to 1.9 for men.

[14] This equalisation partly reflects that some members of parliament were already serving in parliament at $t-1$. If we restrict the sample to candidates elected to parliament for the first time, the incomes at $t-1$ are 91.9 for women and 94.6 for men.

Table 1: Political selection by level of government and politicians' gender

			Children		Years of educ.		Income	
Level	Gender quota	Female	Women	Men	Women	Men	Women	Men
National level								
Cabinet (N = 528)	Party	42.2 %	1.51	1.73	16.3	15.9	113.4	112.8
Parliament (N = 845)	Party	40.6 %	1.70	1.82	15.4	14.7	103.5	104.5
Candidate (N = 18,316)	Party	42.3 %	1.77	1.83	14.2	13.8	50.4	57.4
Regional level								
Council (N = 3,373)	Party	44.8 %	1.88	1.88	14.4	14.1	64.7	75.5
Candidate (N = 33,423)	Party	43.6 %	1.97	2.00	14.1	13.6	45.3	53.7
Local level								
Mayor (N = 2,060)	-	24.6 %	2.40	2.38	14.8	13.9	80.7	86.3
Executive board (N = 15,141)	Law	41.3 %	2.22	2.33	14.4	13.8	59.6	74.0
Council (N = 51,799)	Party	37.8 %	2.07	2.20	14.2	13.5	52.0	65.7
Candidate (N = 292,590)	Party	41.4 %	2.07	2.05	13.6	13.1	40.0	52.9
Population (N = 4,218,917)	-	49.9 %	1.64	1.42	13.0	12.7	35.9	50.4

Notes: The Table reports statistics for all men and women across the political hierarchy in the 2003–2021 period. 'Gender quota' reports whether the gender composition at the indicated level is regulated by rules that are mandated by law (Law), or by bylaws that are voluntarily adopted by most major parties (Party). 'Female' reports the percentage of women at the indicated level. 'Children' reports the average number of children, for men and women, respectively. 'Years of educ.' reports average years of education, for men and women, respectively. 'Income' reports average yearly income in 1000s of constant (2015) USD, for men and women, respectively (only observed until 2019). All outcomes are observed in the year before candidacy. Individuals can appear in multiple periods and at different levels within periods (i.e. all local council members are also local candidates). The bottom row reports statistics for the population of Norwegians aged 18 and older in 2019.

particularly among younger children. A reform passed by parliament at the beginning of our sample period (in 2003) aimed to offer affordable child-care to all children. Over the next decade, municipal child care coverage rates increased sharply, with approximately 95% of 3-5 year-olds enrolled in formal child care in 2012.

3. Methodology

3.1 *Data*

To estimate child penalties for political outcomes, our starting point is the universe of candidates running for local office in Norway in the 2003–2019 period (about 60,000 candidates each year) (Fiva *et al.*, 2021).[15] Our dataset includes election outcomes for every candidate, along with comprehensive background information such as political party affiliation, municipality, list rank, and 'head start' status. We argue that it is useful to study the lowest level of political office for a couple of reasons. First, about 75% of national-level politicians started their political careers at the local level (Cirone *et al.*, 2021).[16] It is important to understand whether imbalances at the top of the political hierarchy could be driven by the arrival of children early in a political career. We know from the labour market literature that women's labour market outcomes lag behind men's for at least a decade after they become parents. Second, there are many local politicians, which enables us to make meaningful statistical inferences.[17] Each politician-year observation is then matched with the administrative registers of Statistics Norway, enabling us to pin-

[15] Fiva and Røhr (2018) originally collected these data for a study of the incumbency advantages in party-list systems, documenting that candidates who barely win a seat on the local council have around 9 percentage points (43%) greater probability of getting elected in the next election, compared to a candidate who just misses out on a seat on the same party list. Using the same data, Nowacki (2022) finds that the incumbency advantage is smaller for women than for men.

[16] The political careers of successful candidates typically span several decades. In 2021, the average age of candidates elected to parliament was 46. Our data shows that these candidates first appeared on local, regional or national election lists at an average age of 29 (for both genders). 41% of these candidates had their first child before running for office for the first time.

[17] For example, among the 10,114 candidates elected in 2003, 221 had a child before the 2007 election. At the national level, in contrast, there are 169 elected candidates. In the 2005–2009 election period, only five had a child.

point the month and year of birth of politicians' children and extract other observables such as education and labour market outcomes (Statistics Norway, 2020a,b, 2021a,b, 2022a,b,c,d,e,f,g). We also incorporate data about candidates running for national office (Fiva and Smith, 2017), including the most recent (September 2021) election.

We focus our analysis on candidates who were already politically active *before* becoming parents. This implies that we estimate political perseverance and advancement as a result of parenthood, rather than candidate emergence. Our main specification studies the evolution of a wide set of political outcomes as a function of event time (t). For each parent in the data, we denote the election period when the individual has his/her first child by $t = 0$, and index all periods relative to that period. An 'election period' is defined as the four-year interval spanning from October in an election year to September in the next election year.

Our baseline sample consists of all candidates running for local office in *any* election period prior to $t = 0$ (23,935 candidate-year observations).[18] For maximum statistical power, we base our main estimation results on an unbalanced sample that tapers in size before $t = -1$ and after $t = 1$. Results using fully-balanced samples (with fewer observations) are presented in the appendix. We also construct a separate timeline (k) to assess labour market outcomes that are observed in yearly intervals. Here, $k = 0$ denotes the year the individual has his/her first child. Summary statistics for the population of Norwegian first-time parents and our main sample of politicians are presented in Appendix Table A.2.

3.2 *Identification*

Our identification strategy is inspired by the event-study framework of Angelov *et al.* (2016) and Kleven *et al.* (2019b). These authors argue that, even though fertility choices are not exogenous, the event of having a first child generates sharp changes in labour mar-

[18] We omit from our main sample candidates whose initial run for office was in *Oslo*, *Bergen* and *Tromsø*, as these municipalities employ a parliamentary system where there is no executive board and the function of the mayor is more ceremonial.

ket outcomes that are orthogonal to unobserved determinants of those outcomes, which should evolve smoothly over time. As the focus of this study is on candidates who already ran for office before becoming parents, political involvment mechanically mean-reverts as event time progresses. This motivates our event study difference-in-differences (DiD) specification whereby outcomes for female politicians around parenthood are compared directly to male counterparts.[19] Our main empirical specification (regardless of being applied to timeline t or k) takes the following form:

$$\begin{aligned} Y_{ist} = \sum_{j \neq -1} \alpha_j \cdot \mathbf{I}[j = t] + \sum_{j \neq -1} \beta_j \cdot \mathbf{I}[j = t] \cdot \mathbf{I}[female_i] + \gamma \cdot \mathbf{I}[female_i] + \\ \sum_{l} \delta_l \cdot \mathbf{I}[l = age_{is}] + \sum_{y} \eta_y \cdot \mathbf{I}[y = s] + \sum_{p} \pi_p \cdot \mathbf{I}[p = party_i] + \epsilon_{ist} \end{aligned} \tag{1}$$

In Equation (1), Y_{ist} represent an outcome for individual i in year s at event time t. The parameters of interest, β_j, measures the differences in outcomes relative to $t-1$ between women and men who have a child in $t = 0$. In the following, we refer to this as the 'child penalty'. Our baseline empirical specification also includes age fixed effects ($\sum_l \delta_l \cdot \mathbf{I}[l = age_{is}]$), which control nonparametrically for underlying life-cycle trends; election year fixed effects ($\sum_y \eta_y \cdot \mathbf{I}[y = s]$), which control nonparametrically for secular time trends; and party fixed effects ($\sum_p \pi_p \cdot \mathbf{I}[p = party_i]$), which means that all inference is drawn from candidates belonging to the same political party (at $t = -1$). We allow for arbitrary correlation of the error terms ϵ_{ist} within individuals over time, by clustering standard errors at the individual level. Clustering at the municipality level gives almost identical results.

We estimate Equation (1) for two broad sets of outcomes: First, we consider **political outcomes**, and let Y_{ist} represent one of three outcome variables: (i) a dummy variable

[19] Labour market child penalties are typically estimated in separate regressions by gender. Given that this effect, for men, is generally identified as a 'non-event', our design change should not matter much. We validate the DiD by showing that its implementation to estimate labour market child penalties for the full population of Norwegian parents yields results that are largely consistent with the existing literature.

equal to one if candidate i runs for local office in election period t; (ii) a dummy variable equal to one if candidate i wins a local council seat in election period t; or (iii) a dummy variable equal to one if candidate i gets a leadership position in the local council (municipal executive board) in election period t. In the appendix, we also estimate effects for the probability of becoming mayor (a very rare outcome regardless of gender), as well as for individuals' political careers in higher political office. Second, we consider **labour market outcomes**, where Y_{isk} represents income in 1000s of constant (2015) USD in event year k.[20] The specification is estimated for the main sample of politicians but, for validation, also for the universe of Norwegian first-time parents in the sample period (2003-2019).

A common criticism of the child-penalty methodology is that the timing of childbirth should not be considered an exogenous event. This concern may be particularly pressing when relying on four-year election periods, as we do here.[21] In general, the likelihood of outcomes being affected by mechanisms other than parenthood increases the more time passes between each observation. We are not able to mitigate these concerns fully. But the fact that our dynamic responses mirror the labour market outcomes (which do use yearly intervals), combined with universally flat pre-trends, makes us less concerned. Even if our analysis were to be considered as a more descriptive exercise, we believe it is essential to map out how the political careers of men and women evolve around childbirth. Another issue is that children may discourage prospective candidates from pursuing political careers in the first place. If this is the case, our estimates capture only a part of the overall effects.

[20] Income is defined as the sum of pre-tax labour market income from wages, self-employment and work-related cash transfers, including unemployment benefits, sick leave benefits and parental leave benefits (*pensjonsgivende inntekt*). We exclude the party fixed effects from the analysis of labour market outcomes.

[21] Appendix Figure A.3 shows that there is no clear gender difference in the timing of first-time childbirth during each four-year election period cycle.

4. Results

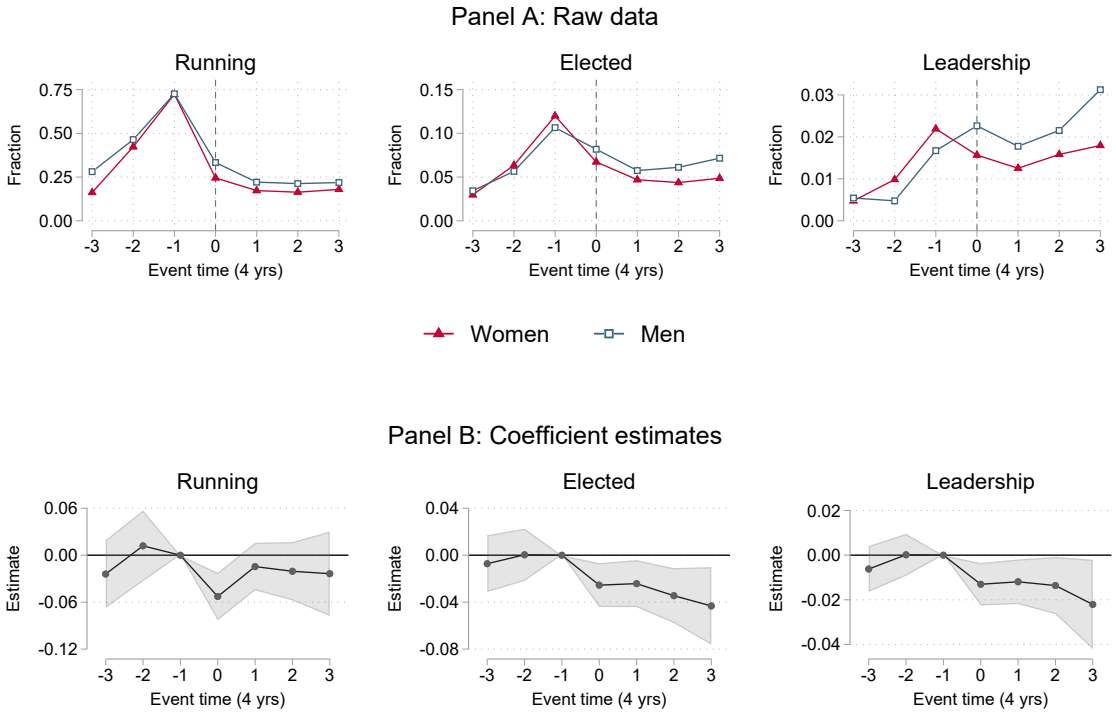
4.1 *Child penalties in politics*

Figure 1 contains the baseline results from our event-study DiD on local political outcomes. Consider first the top-left window, which plots the fractions of male (blue squares) and female (red circles) candidates who ran for office, i.e. were on a list, at each election period. We observe that around 73% of candidates of either gender in our main sample ran for office at $t = -1$. Slightly less than 50% ran in the preceding period.[22] At event time $t = 0$, the election period during which candidates become parents for the first time, the outlook changes substantially depending on the candidates' gender; for men, the probability of running again is 33%. For women, the probability falls by an additional 8 percentage points, revealing a gender gap of about 26% relative to men. This difference diminishes somewhat in the subsequent period, after which the two lines remain parallel until we last observe them at $t = 3$ (12 years after $t = 0$). The bottom left-hand plot contains estimates of β_j in Equation (1), i.e. the difference between the blue and red lines after controlling for age, year and party fixed effects ($t = -1$ serves as the reference category). The 95% confidence intervals overlap with zero from $t = 1$ onwards, but the effect at $t = 0$ is substantial.

In the middle set of plots, we estimate the probability of being *elected* to office, i.e. winning a seat on the local council. As before, the two outcomes are parallel prior to $t = 0$. Between periods $t = -1$ and $t = 0$, men drop by around 2.5 percentage points – or 23% – while women drop by 5.3 percentage points – or 44%. These differences are substantial. In particular, the chance of winning falls more than twice as much for women relative to men. The gap continues to grow over time; at $t = 3$, the 4 percentage points

[22] Recall that all persons ran for office at least once before $t = 0$. The participation rates of women and men mechanically deviate at $t = -3$ (12 years before $t = 0$) because the age of majority is 18 and women are, on average, about two years younger than men when they have their first child (Appendix Table A.2). When controlling for age fixed effects, the difference between women and men is no longer statistically significant (bottom panel of Figure 1; Appendix Table A.3).

Figure 1: The child penalty in local political outcomes



Note: Panel A plots the fraction of men (blue squares) and women (red circles) for whom the outcome variable is equal to one at each election period t. Panel B contains estimates of the parameters of interest, β_j, in Equation (1) along with 95% confidence intervals. The reference category is event period $t = -1$. Standard errors are clustered at the individual level. The sample consists of all politicians who ran for office prior to election period $t = 0$ (23,935 observations from 4,787 individuals). Numerical results in Appendix Table A.3.

DiD estimate implies a long-run child penalty of approximately 50% of men's chances of becoming elected.

The final outcome we consider in Figure 1 is the appointment to a local leadership position. Here, women have a more pronounced advantage prior to $t = 0$ than men, probably as a result of the gender quota that governs promotion to the executive board (see section 2.2). Starting with the election period when candidates become parents for the first time, we observe a clear break for women, while men continue to trend upwards. These effects are small in levels, but large in relative terms. In the very last period, fathers (at $t = 0$) are over 50% more likely to act as leaders than mothers (at $t = 0$).

Taken together, the results in Figure 1 suggest that even though the propensity to run balances out somewhat over time, this does not translate into equal representation in political assemblies.[23] We believe this result is in part institutional. Previous research has established that Norwegian political parties rely on seniority progression rules to structure political selection (Cirone *et al.*, 2021). These rules create career paths within parties, so that nominations are meritocratic for entry-level jobs but then become progressively more seniority-based as one moves up in the hierarchy. By the time the gender gap on 'running' shrinks, men have accrued more seniority, which translates into improved opportunities for advancement to higher positions on party lists. We return to this discussion in Section 5.

In Appendix Figure A.6, we estimate child penalties for the probability of advancing to higher levels of the political hierarchy. The first outcome we consider is becoming a municipal mayor. As the only full-time position among those considered so far, the mayor – who leads the executive board – holds the most senior position in local politics. Regarding this outcome, there are negligible short-term effects, since very few candidates

[23] We provide a series of robustness checks in the appendix. First, Appendix Figure A.4 reports estimates from models that include candidate fixed effects. Second, Appendix Figure A.5 contains results when conditioning on candidacy at election period $t = -1$ only (i.e. omitting from our main sample all candidates who ran prior to $t = -1$ but *not* in $t = -1$). The results are similar to our baseline estimates in both cases, but there is some discrepancy in whether the long-run 'running' effects are statistically different from zero. What they all agree on is that the gap at $t = 0$ shrinks substantially (or even disappears) before $t = 1$. Lastly, Appendix Table A.4 reports cohort-specific estimates. While there is some variation in magnitude and precision, the effects after $t = -1$ are unanimous in direction.

attain mayoral positions at a young age (with the median age at election being 50). In the long run, however, men are several times more likely than women to become mayors, but none of the effects are statistically significant. The middle and left sets of plots consider the probability of advancing to the regional and national levels. Again, the coefficient estimates suggest that parenthood has a differential effect on women's and men's future political careers, but statistical precision is low and the confidence intervals overlap with zero for both outcome measures.[24]

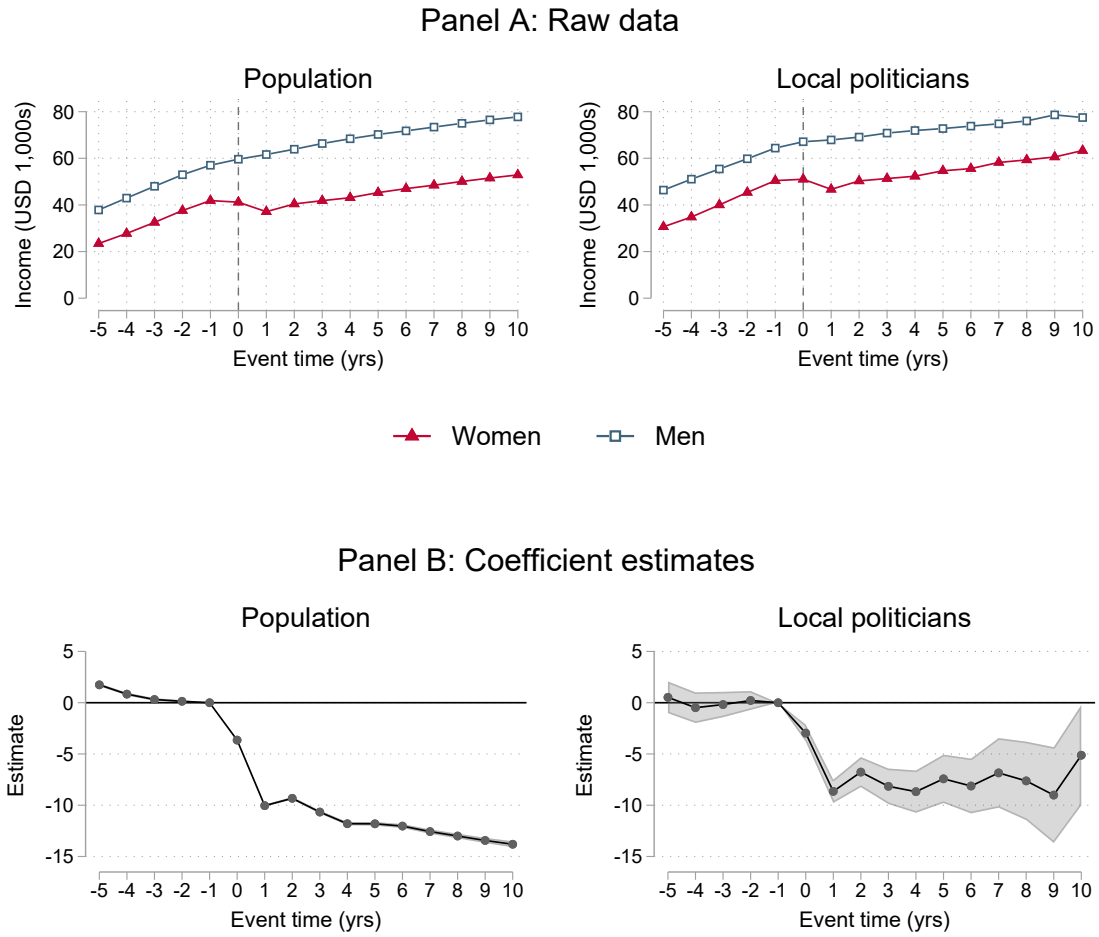
4.2 *Child penalties in the labour market*

Since the vast majority of Norwegian local politicians maintain a traditional career besides holding office, we can learn more about the political child penalty by studying these individuals' labour income trajectories. Moreover, by exploiting that our administrative data covers the entire population of Norwegian adults, these effects can be compared to the general population. Figure 2 contains two separate sets of plots, each constructed in a manner similar to Figure 1, except that labour outcomes are observed in yearly intervals. In the top left window, we show the average income for men (blue squares) and women (red circles) at each event period, using the *full population of Norwegian parents*. At any given point before $k = 0$, Norwegian women earn, on average, around USD 15,000 less than men. After parenthood, the gender gap in income sharply doubles, due entirely to a drop in female income, which does not occur for men. This enlarged difference prevails even ten years after the event. The DiD coefficients are reported in the lower panel as before ($k = -1$ serves as the reference category). Since effects are measured for the entire population, these estimates are extremely precise; at impact, the earnings penalty for women relative to men is approximately USD 10,000 per year, which amounts to around 22% relative to the counterfactual at $k = 1$. The gap continues to increase over time, almost reaching USD 15,000 at the end of the sample period. Our estimates are almost identical to child penalties previously estimated for Norwegian data by Andresen

[24]The low precision reflects that these are rare outcomes. In any given election, about ten times as many candidates run for local office than for regional or national office.

and Nix (2022) and Bütikofer *et al.* (2018).[25]

Figure 2: The child penalty in income



Note: Results from our labour market analysis using the full population of Norwegian parents between 2003 and 2019 (left; $10,960,385$ *observations from* $903,385$ *individuals) and our main politicians sample (right;* $52,882$ *observations from* $4,787$ *individuals). Panel A plots the mean income in 1000s of constant (2015) USD for men (blue squares) and women (red circles) at each event-year* k*. Panel B contains estimates of the parameters of interest,* β_j*, in Equation (1), together with 95% confidence intervals. Party fixed effects are omitted from the specification. The reference category is event period* $k = -1$*. Standard errors are clustered at the individual level.*

In the plots to the right, we repeat the analysis using our main sample of politicians. Local politicians earn, on average, more than the general population both before and

[25]There are also some statistically significant effects in the pre-treatment period. While we would ideally like to see pre-trends that are indistinguishable from zero, the observed differences are small relative to the treatment effects, and otherwise comparable to results in many other child penalty event designs.

after the birth of their first child (in line with Table 1). Effects here are similar to the population, even if the gender gap is slightly less prominent over time.

An important distinction between the samples in Figure 2 is that – unlike in the general population – only a fraction of politicians are *partners*. In Appendix Figure A.7, we estimate child penalties using the respective partners of politicians as the comparison group (as in Angelov *et al.* (2016)). Again, we find no evidence that politicians differ substantially from the general population in terms of the income penalty; the estimates are mostly identical regardless of whether the mother or father is politically active.[26]

4.3 *Design validity*

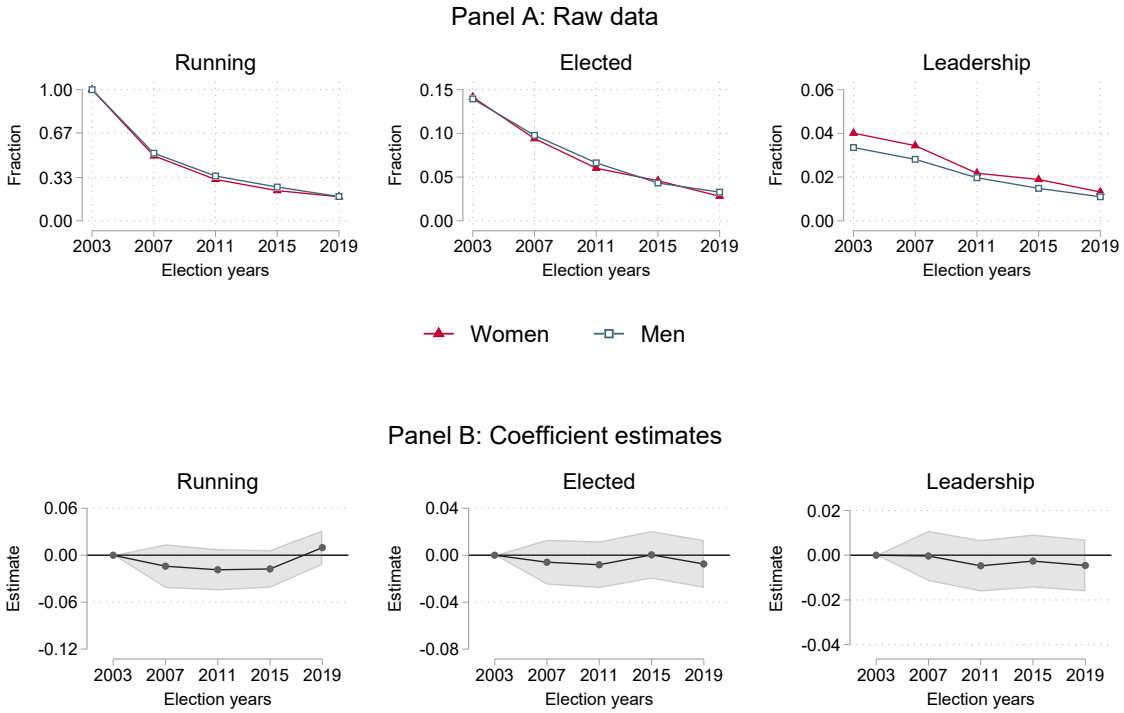
If the effects identified in the previous sections capture the impact of having children, then we should not expect to find similar results among politicians who have never had children. Following this logic, we conduct a couple of placebo checks to investigate the validity of our research design. First, Figure 3 compares political outcomes over time for childless candidates who ran for office in the 2003 election, and remained childless throughout our sample period (until 2021). The developments are remarkably similar across genders.

Second, we conduct an investigation that is commonly used in the literature, where 'child arrivals' are assigned randomly to candidates without children. This placebo sample is then fed to the baseline model as before.[27] The results are shown in Appendix Figure

[26] Appendix Figure A.7 shows that the *level* of earnings is lower for female partners (of male politicians) than for female politicians. We view this in connection with empirical findings from Sweden that, even among top earners, women are far more likely to match with high-income men than vice versa (Boschini *et al.*, 2020). The results can also be interpreted as evidence against the notion that child penalties materialise because women have a comparative advantage in childrearing. If this was the case, we would expect that female politicians (who earn relative more compared to their partners) would incur smaller penalties because they are less likely to specialise in childcare.

[27] Of course, many childless candidates in our data had just not become parents *yet* when we last observed them in 2021. To minimise the risk of contamination, we follow Kleven *et al.* (2019b) by including only candidates who are either (i) old enough for us to observe their complete fertility history, or (ii) whose background characteristics suggest they are most likely to *remain childless* in the future (as explained in the note to Appendix Figure A.8). We proceed by simulating our baseline analysis for the placebo sample 1,000 times. At the start of each iteration, 'placebo children' are assigned to individuals at a time that is drawn from a uniform distribution of in-sample years. We ensure that the number of individuals in each iteration ($n \approx 5,200$) is comparable to the number of individuals in the baseline analyses ($n = 4,787$).

Figure 3: Political outcomes for childless candidates running in the 2003 election



Note: Panel A plots the fraction of men (blue squares) and women (red circles) for whom the outcome variable is equal to one in each election year. Panel B plots the difference between genders, after controlling for age and party fixed effects (as in Equation (1)), along with 95% confidence intervals. Standard errors are clustered at the individual level. The sample consists of all candidates who ran for office in the 2003 election and (as of 2021) never had children ($28,262$ *observations from* $5,653$ *individuals).*

A.8, which plots the distributions of simulation estimates of the parameters of interest, β_j, in Equation (1), for each local political outcome after $t = -1$, respectively. We highlight the actual estimates from Figure 1 by denoting them with red lines. In every instance where we identified a gender gap earlier, these effects lie either well below or within the lower tails of the placebo distributions, supporting our identification strategy.

There is a tendency for the centre of mass to fall below zero in some of the placebo distributions. This suggests that women are less likely than men to be re-elected or appointed to executive positions *in general*. The magnitude of child penalties reported in Section 4.1 could therefore be somewhat over-estimated. Overall, however, such 'leaky pipeline' effects are small relative to our main estimates (e.g. for leadership at $t = 3$, the average placebo and actual effects are, respectively, -0.002 and -0.022).

5. Mechanisms

The empirical finding of child penalties in labour markets have proved incredibly robust across both countries and time, and several potential explanations for their occurence been considered. An obvious candidate is biology: only women can bear and give birth to children, and only women have the option to breastfeed. However, since the long-run child penalties for biological and adoptive families are remarkably similar, this factor does not seem to be important (Andresen and Nix, 2022; Kleven *et al.*, 2021). Instead, Kleven *et al.* (2019b) demonstrate that there are strong intergenerational links between mothers' labour supply and the labour supply history of maternal grandparents, suggesting that preferences or gender norms associated with parenthood play a role in mediating these effects. This interpretation aligns well with related studies that show how gender identity norms shape marriage and labour markets (see, e.g., Bertrand *et al.*, 2015; Bursztyn *et al.*, 2017; Folke and Rickne, 2020). In our political context, it is clear that a host of reasons, including mothers' unwillingness to take on more political work, outright discrimination, or, as Goldin (2021, p. 166) puts it, 'well-intentioned paternalism' from

political gatekeepers can play a role (see, e.g., Baltrunaite *et al.*, 2014; Besley *et al.*, 2017). Institutional features of the election system allow us to hone in on the specific stages during which child penalties materialise. To the extent that the political hierarchy mirrors that of other organizations where child penalties prevail, these analyses can shed light on where in the decision-making process such effects occur.

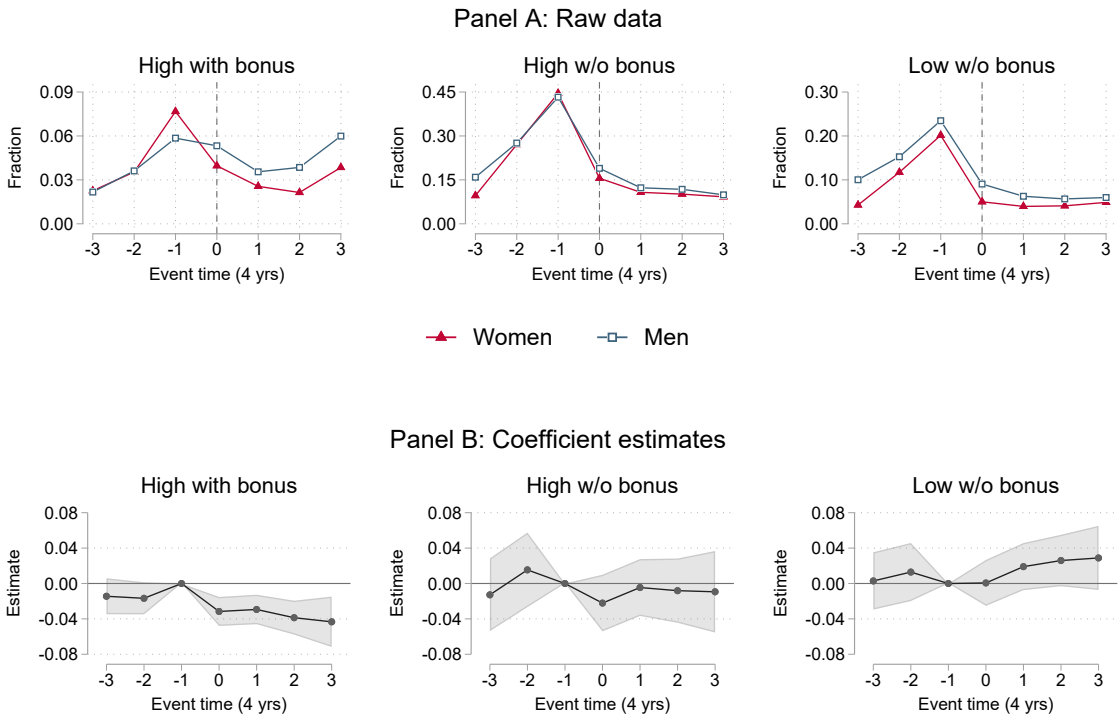
5.1 *Party lists*

Our event-study framework allows us to quantify the effect of parenthood on future political careers *unconditional on running*. We are in principle also interested in estimating child penalties *conditional on running*, but this requires addressing selection into future candidacy (Anagol and Fujiwara, 2016). We therefore rely on features of the election system to understand the drivers of our main results, rather than conditioning on running in our baseline analysis.

As explained in Section 2.1, political parties control the nature of competition within lists by giving certain candidates a 'head start' ('party bonus'). These are then named in bold type at the top of the lists. In our data, the median number of candidates with a 'head start' is two, while the median list length is 19. The ranking of candidates on party lists does not otherwise play any formal role, unless there is a tie, but is still important since voters probably view the rankings as an indication of candidate quality. The top-ranked candidate on a list is typically the party's mayoral candidate.

In Figure 4 we explore how the child penalty unfolds across local party lists using our event study design. We consider three dummy outcome variables (all measured unconditional on running), as indicated in the sub-panel headings. The results suggest that mothers are less likely to be ranked favourably by the party relative to comparable fathers; they are less likely to get the bonus, about equally likely to be ranked high without the bonus, but more likely to be low-ranked without the bonus (although this effect is not statistically distinguishable from zero). Because the fraction of candidates elected in each of these three groups varies dramatically (74%, 16%, and 3%), it is clear

Figure 4: The child penalty on party lists



Note: Panel A plots the fraction of men (blue squares) and women (red circles) for whom the outcome variable is equal to one at each election period t. Panel B contains estimates of the parameters of interest, β_j, in Equation (1) together with 95% confidence intervals. 'High with bonus' means receiving the discretionary 25% boost in personal votes from the party. 'High w/o bonus' refers to otherwise being listed among the top two thirds of party lists (but not receiving the party bonus). 'Low w/o bonus' refers to being in the bottom third on party lists. Standard errors are clustered at the individual level. The sample consists of all politicians who ran for local *office prior to election period $t = 0$ (23,935 observations from 4,787 individuals).*

that the child penalty on party list mediates our findings in Figure 1.

5.2 *Personal votes*

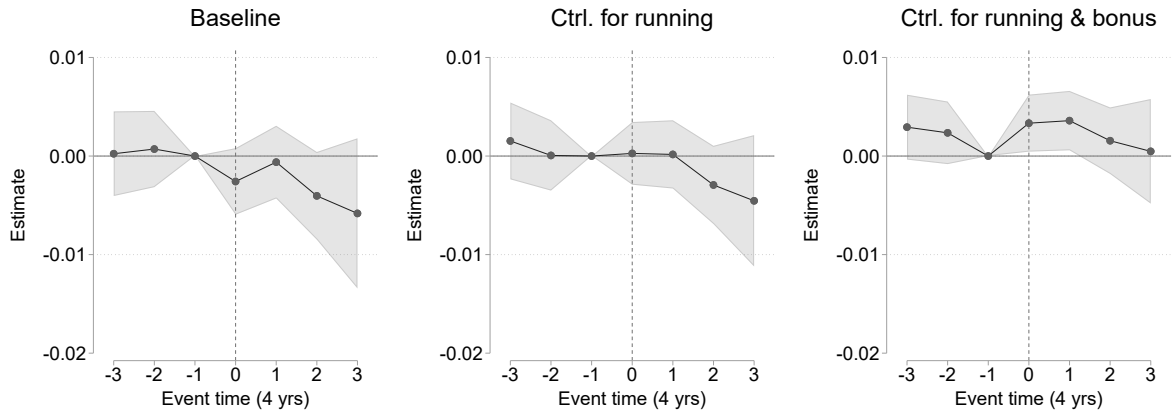
In Norway's flexible-list electoral system, voters affect the election outcome by casting preference votes. If child penalties arise partly due to voter discrimination, we would expect candidates who are mothers to get lower personal vote shares than candidates who are fathers. Figure 5 shows event study plots where candidates' personal vote share (v_{it}^s) is the dependent variable. This variable is defined as $v_{it}^s = v_{it}^n / v_t^N$, where v_{it}^n is the number of personal votes received by candidate i in year t and v_t^N is the total number of personal votes received by the party-district in year t. We code individuals who are not running as having a personal vote share of zero. Because women run less frequently than men at event time $t > -1$ (see Figure 1), this should yield a lower bound on the conditional effect on personal vote shares, i.e. an upper bound on voter discrimination.

In the left-hand plot, we estimate small negative effects which are statistically indistinguishable from zero. At most, the 95% confidence intervals extend no further than -0.013, corresponding to about 18% of a standard deviation in the dependent variable (vote share has a mean of 0.051 and a standard deviation of 0.072). In the middle and right-hand plots, we add controls for the (post-treatment) running and bonus statuses of candidates as a crude proxy for the child penalty on political lists. As expected, the point estimates increase slightly when controlling for candidacy. When we also add a control variable for receipt of the party bonus, there is even weak evidence of 'motherhood favouritism' among voters.[28]

These results diminish the significance of voters' role in the formation of child penalties. In fact, Appendix Figure A.9 shows that setting personal votes to zero (i.e. simu-

[28] Another piece of evidence with an alternative estimation strategy is presented in Appendix Table A.5. Here, we drop the dynamic format and instead compare vote shares of candidates who are parents with candidates of the same gender who are childless at the start of each election period. While conditioning on candidacy may be a naïve approach, the inclusion of incumbency, bonus and flexible rank controls implies that we are comparing vote shares of candidates who are equally experienced, and who have already been evaluated by party leaders to be of comparable merit. All specifications also include a range of flexible controls for background characteristics such as age, location and proxies for quality. For both genders, having children is associated with receiving *more* personal votes.

Figure 5: The child penalty on vote shares



Note: The figure shows estimates of the parameters of interest, β_j, in Equation (1) together with 95% confidence intervals, when the dependent variable is the share of personal votes received by each candidate (coded as 0 if the candidate is not running). In the middle and left-hand plots we add to the baseline model dummy variables to control for candidacy and party bonus status, respectively. The sample consists of all politicians who ran for local *office prior to election period $t = 0$ (23,935 observations from 4,787 individuals). Standard errors are clustered at the individual level.*

lating a closed-list political environment) leads to election outcomes that are analogous to those in Figure 1. This is consistent with evidence from Teele *et al.* (2018), who document that motherhood is viewed as a favourable candidate trait among American voters. A meta-analysis of 67 survey experiments by Schwarz and Coppock (2022) also finds evidence against demand-side explanations of the gender gap in politics, concluding that 'rather than discriminating against women who run for office, voters on average appear to reward women' (p. 665). Child penalties in politics materialise primarily during the nomination process. The fact that mothers are less likely to get the top positions on party ballots – which includes the party bonus – drives our main results (recall that the bonus is so large that it isolates candidates from lower-ranked competition). We know that political parties play a key role in candidate screening and selection in list-based proportional representation systems (see, e.g., Buisseret *et al.*, 2022; Cox *et al.*, 2021), but exactly what takes place during nomination meetings is hard to elucidate from our data. To further probe the root cause of these effects, we next turn our attention to the influence of gender norms.

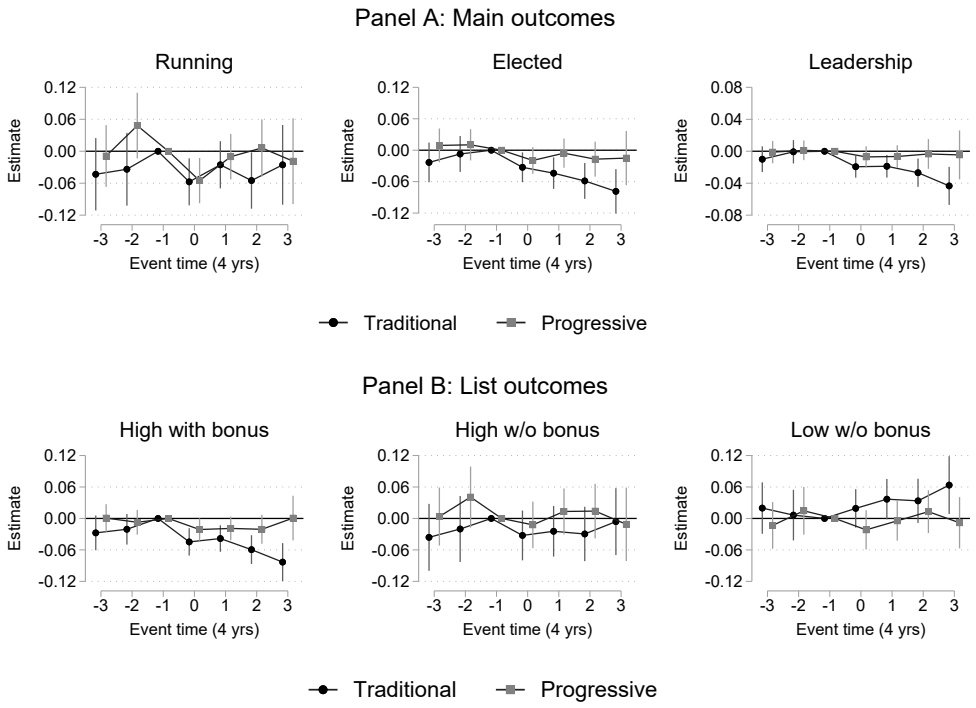
5.3 *Norms and preferences*

Several studies have documented a relationship between parents' attitudes to gender roles and the labour market participation of their sons or daughters (see, e.g., Fernández *et al.*, 2004; Farré and Vella, 2013). If child penalties in the political arena are rooted in gender norms, female candidates who were exposed to more stereotypical gender patterns during childhood would be more likely to fall behind after parenthood than others. Following Kleven *et al.* (2019b), we test this hypothesis by considering the division of labour within families during the time when our political candidates were growing up.

Our analysis takes advantage of the fact that our income data goes back to 1967. For each child born in our estimation sample, we compute the average income of their grandparents over the period when their parent (who later pursues a political career) was $0-18$ years old. To measure the division of labour among the grandparents, we construct an index (I) that relies on the average income of grandmothers ($\bar{W}_{gm}$) vis à vis grandfathers ($\bar{W}_{gm}$): $I = \bar{W}_{gm}/(\bar{W}_{gf} + \bar{W}_{gm})$. Appendix Figure A.10 shows that this index has a median just below 1/3, confirming that men were the primary breadwinners in these households. We split the sample of politicians into two groups, depending on whether they grew up in a family whose division of labour was more 'traditional' (I below median) or more 'progressive' (I above median). We then estimate child penalties separately for the two sub-samples.

The results from the split-sample regressions are reported in Figure 6. Panel A considers the main outcomes from Figure 1, while Panel B considers the outcomes from Figure 4. In all subplots, estimates for the 'progressive' group generally hover around zero. This is notably not the case for 'traditional' candidates, whose gender gap appears to accelerate over time in the elected and leadership outcomes. Panel B provides some evidence that the actions that bring about these differences occur during the nomination process, echoing our findings in Section 5. It may seem that child penalties in politics are driven by candidates who grew up in households with a below-median division of labour.

Figure 6: The political child penalty split by grandparents' division of labour



Note: The figure reports split-sample estimates of the parameters of interest, β_j, in Equation (1) together with 95% confidence intervals, separately for candidates growing up in households with a below-median division of labour (black circles; $10,819$ *observations from* $2,333$ *individuals) and an above-median division of labour (grey squares;* $10,813$ *observations from* $2,378$ *individuals). Panel A considers the main political outcomes reported in Figure 1, while Panel B considers the list outcomes reported in Figure 4. Standard errors are clustered at the individual level. A few individuals from the baseline sample who were not successfully matched with grandparents are omitted.*

However, because most of the 95% confidence intervals overlap, we refrain from drawing strong conclusions about heterogeneity across family types.

5.4 *Family-friendly environments*

What can be done to mitigate child penalties in politics? One discernible tool which is already used comprehensively in Scandinavian politics is gender quotas. This may be an attractive remedy in some settings, but the fact that seven out of the 'big nine' Norwegian political parties had such policies in place during our entire sample period implies that quotas are not a complete solution. Instead, we turn our attention to the logistics aspects of public office. Goldin (2021, pp. 216-217) argues that '... time is the enemy of women's quest for career and family. On-call, rush, emergency, evening, and weekend time is demanded simultaneously from the home and office.'

In our setting, office-holding parents have to balance an additional obligation on top of the standard work-life commitments faced by ordinary families. The fact that local council meetings are often held in the evenings has led to significant concern among some women; for example, a local council member in *Trondheim municipality* (the third-largest city in Norway) wrote in an op-ed piece in 2016: 'It is too hard to combine political work and family life. Council meetings should be moved from evening to daytime' (Løvik, 2016).

We collected original data for local council meetings held in municipalities at the end of our sample period.[29] These data cover 396 out of the 422 municipalities existing in 2018. Of these, 290 held their local council meetings in the evenings, or as a combination of daytime and evening meetings. 106 municipalities, however, consistently held all their meetings during standard business hours. We classify the latter category as 'family-friendly' municipalities.

To shed light on the effectiveness of prospective policies aimed at reducing the child

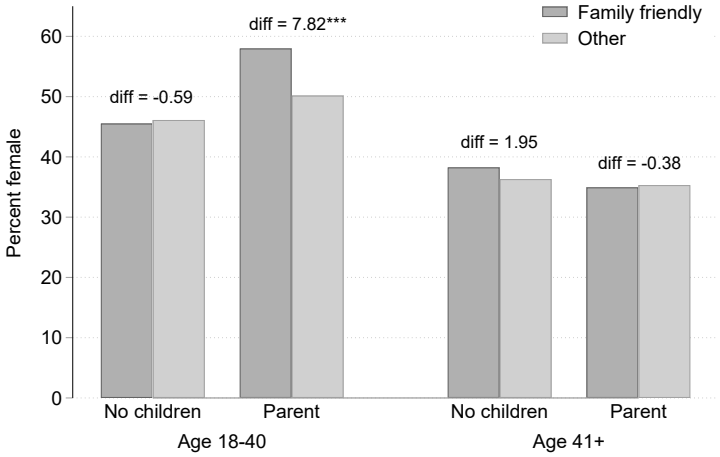
[29] Our data covers the year 2018. We relied primarily on meeting calendars available online, but also contacted municipalities directly when this was necessary. Ideally, we would like to have data for all sample years, but historical meeting records do not go very far back in time.

penalty, we investigate whether family-friendly municipalities have more women with children elected to office. Using data from the October 2015-September 2019 election period, Figure 7 plots the fraction of women in local councils by age groups ($18-40$ and 41+), parental status at the time of the election (yes or no), and family friendliness (daytime meetings or not). Interestingly, we find a substantial gap in women's representation between family-friendly and other municipalities for parents aged $18-40$, while there are no differences for the three other categories.[30] One interpretation of this result is that women with young children (typically found in the $18-40$ category) actively push for more family-friendly meeting schedules.[31] The alternative interpretation is that women with young children are more willing to pursue political careers in family-friendly settings. This is consistent with the findings of Bütikofer *et al.* (2018), who document that labour market child penalties are smaller in more flexible professions in Norway. While the evidence presented in this section is solely descriptive, it does suggest that more family-friendly working environments might reduce women's underrepresentation in politics. We leave the task of causally assessing such statements to future research.

[30] Appendix Figure A.11 shows corresponding results for political candidacies.

[31] Elected representatives in Norway have the right to a leave of absence from work to attend local council meetings (Section 12-13 of the Working Environment Act). While there are, presumably, a range of reasons why candidates might find evening schedules attractive, the institutional features required to minimise the economic costs of undertaking political work during daytime hours are largely in place.

Figure 7: Share of women among elected candidates, by parental status, age and municipality type



*Note: This figure shows the share of female councilors in the 2015-2019 election period by parental status, age and municipality type. Municipalities are classified as 'family friendly' if all local council meetings in 2018 were held during standard business hours. The labels above each pair of bars report the differences between bars, together with the degree of statistical significance. Standard errors are clustered at the municipality level. * denotes 10% statistical significance, ** 5% and *** 1%.*

6. Conclusion

The underrepresentation of women in political leadership remains an important global issue. According to the United Nations, women constitute 34% of elected members of local deliberative bodies and only 27% of national-level MPs (UN Women, 2023). In Norway, women account for around 40% of candidates across all levels of the political hierarchy. While this is high relative to many other countries, it is also disappointingly low considering that most major parties have complied with progressive gender quotas for several decades.

In this paper, we have used insights from the labour literature to investigate the impact of parenthood on political careers. In sum, we find that child penalties affect political participation in much the same way as in the labour market; the probability of winning a seat in the election period when politicians become parents for the first time falls twice as much for women than for men. Over the following periods, the gender gap widens to around 50% of men's chance of electoral success. Women are less likely to gain leadership positions than men after having children. This suggests that parenthood is a key factor in explaining the underrepresentation of women in politics.

Our results have several key implications. First, we learn that parenthood can be a crucial barrier to women's political careers, even if they are already inside the political sphere. These findings broaden our understanding of the 'leaky pipeline to power' in public office, and could open the door to a new research agenda on the formation of gender gaps in politics. Second, it appears that widely-used policies designed to propel women into public office, such as gender quotas, are not enough to prevent a higher outflow of women from party lists or to prevent those on the lists from falling behind. It is not a lack of female candidates that is the primary driver of our results – child penalties arise because women who become mothers are less likely to get the top-ranked spots on electoral lists. Instead, we suggest that child penalties can be mitigated by targeting the root cause of the problem (such as by using family-friendly meeting schedules). Third, the

fact that the overall ratio of female candidates never declined during our sample period implies that political parties counteract attrition by continuously recruiting more women than men. We have not discussed the importance of political experience in detail, but we know that seniority matters for the allocation of leadership positions and nominations for higher offices (see, e.g., Cirone *et al.*, 2021; McKelvey and Riezman, 1992; Kellermann and Shepsle, 2009). This implies that even if we achieve gender equality across electoral lists, it is unlikely to result in gender balance in political representation for as long as child penalties exist. Lastly, while generalising our results to other countries is not a straightforward exercise, the liberal gender laws and generous welfare programmes for child care and parental leave in Norway make it hard to believe that child penalties in politics only exist in our context.

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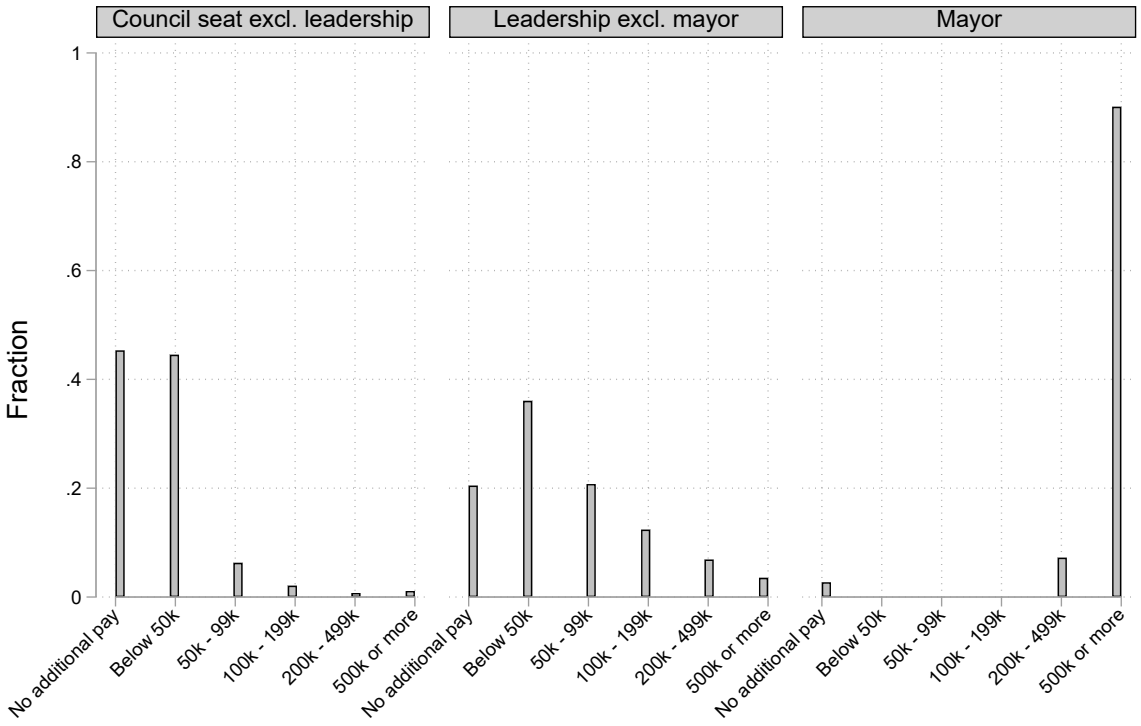
Appendix A: Supplementary figures and tables

Figure A.1: Remuneration for local political roles

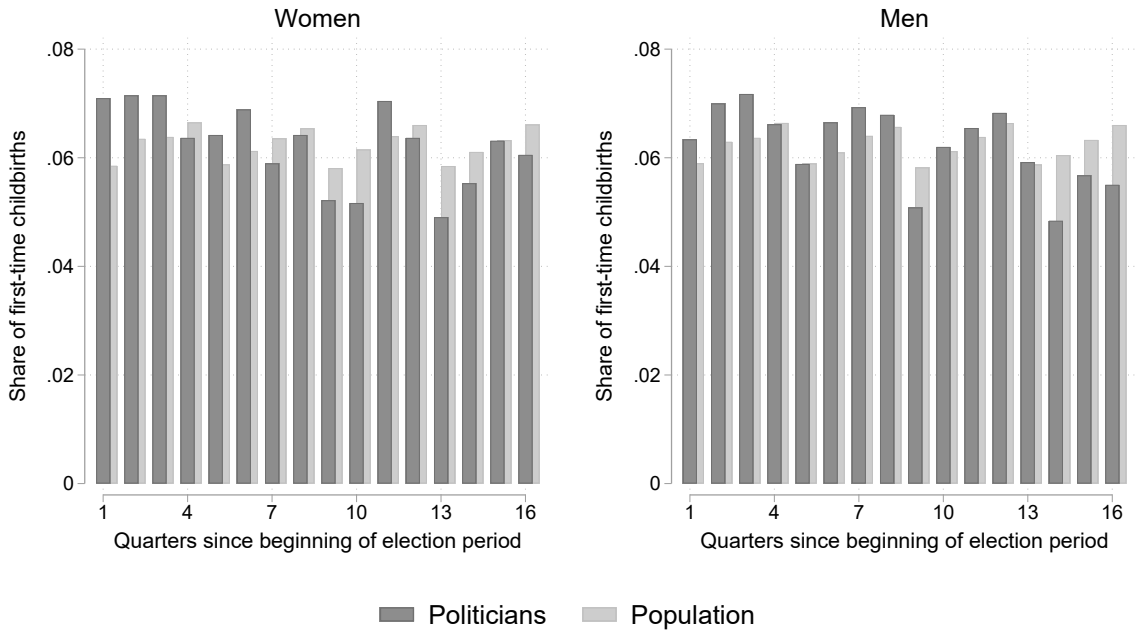


Note: The figure plots survey responses collected from local political office holders in 2011 (N=2,234). The survey question asked (translated from Norwegian): 'What remuneration do you receive as local politician from the municipal government for performing political duties (besides standard meeting compensation)?' Source: Lokalpolitiker- og rådmannsundersøkelsen 2010/2011 (Statistics Norway, 2011).

Figure A.2: Number of elected candidates by gender and age

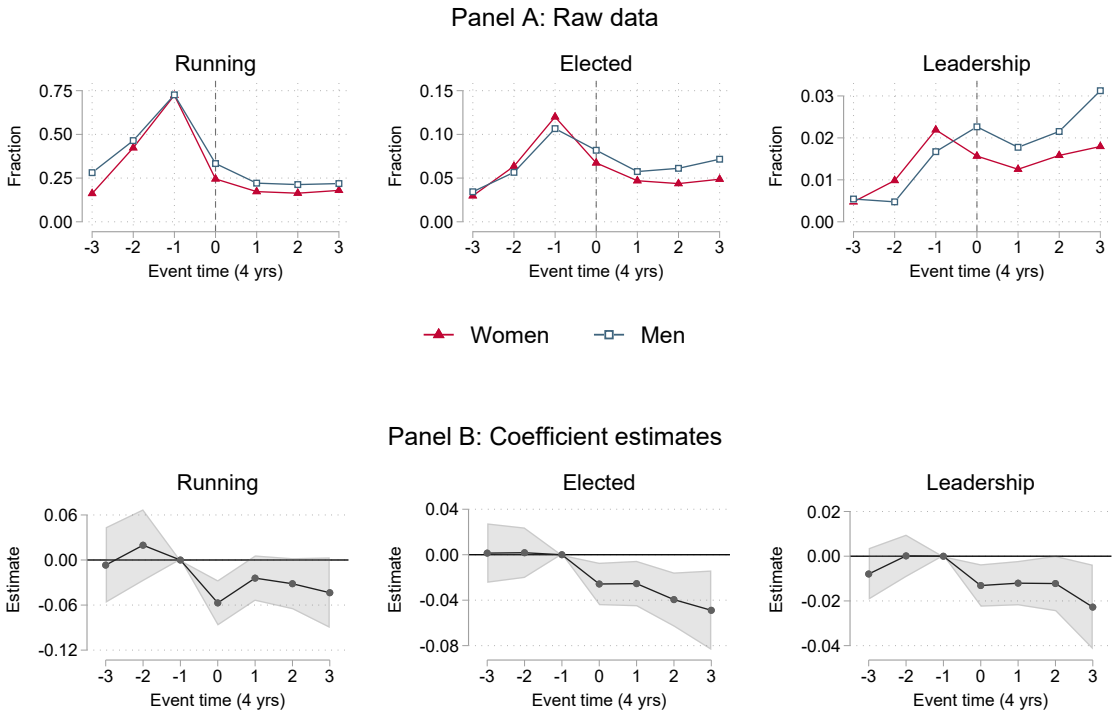
Note: The figure shows the number of elected individuals by gender and age, separately for each election year 2003–2019. In each election year, about 10,000 candidates are elected to the local council. The number is slightly lower in the 2019 election because of the municipal merger reform that reduced the number of municipalities from 428 to 356.

Figure A.3: Distribution of first-time childbirths, by gender



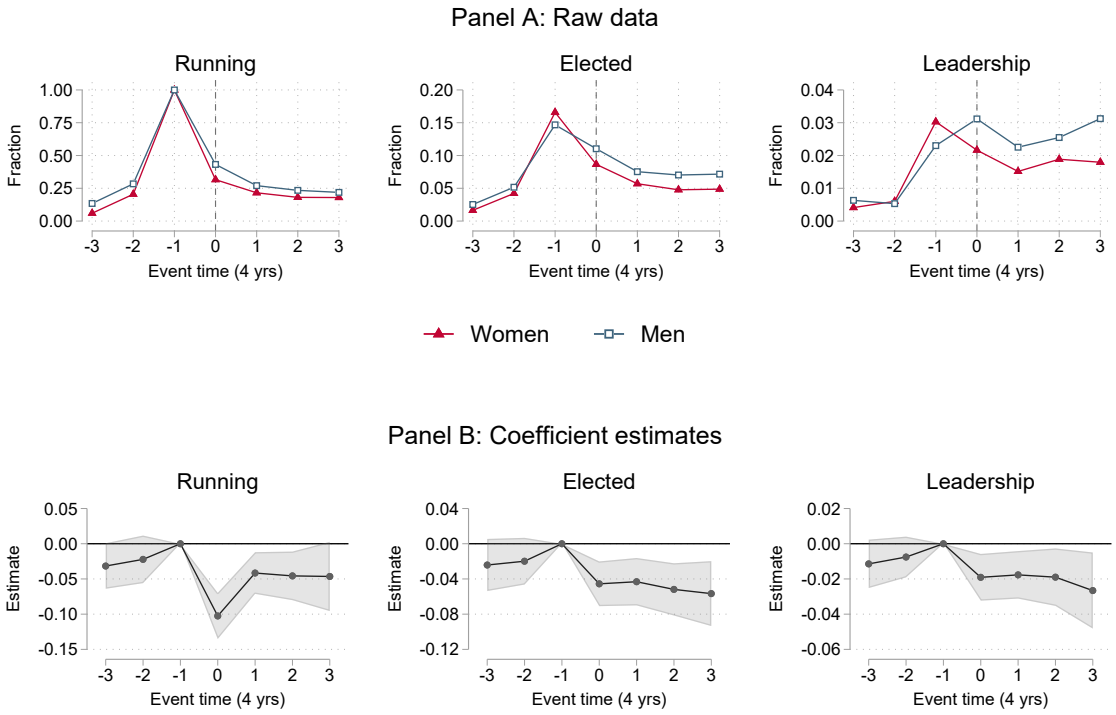
Note: The figure shows the distribution of first-time childbirths for male and female candidates in our main sample over the 16-quarter period starting with October-December in each election year to July-September four years later (observations are stacked across periods). The distributions for population first-time parents during the same periods are shown in the background. Each bar indicates the fraction of candidates (among each gender) who received their first child during that quarter.

Figure A.4: The child penalty in local political outcomes, candidate fixed effects



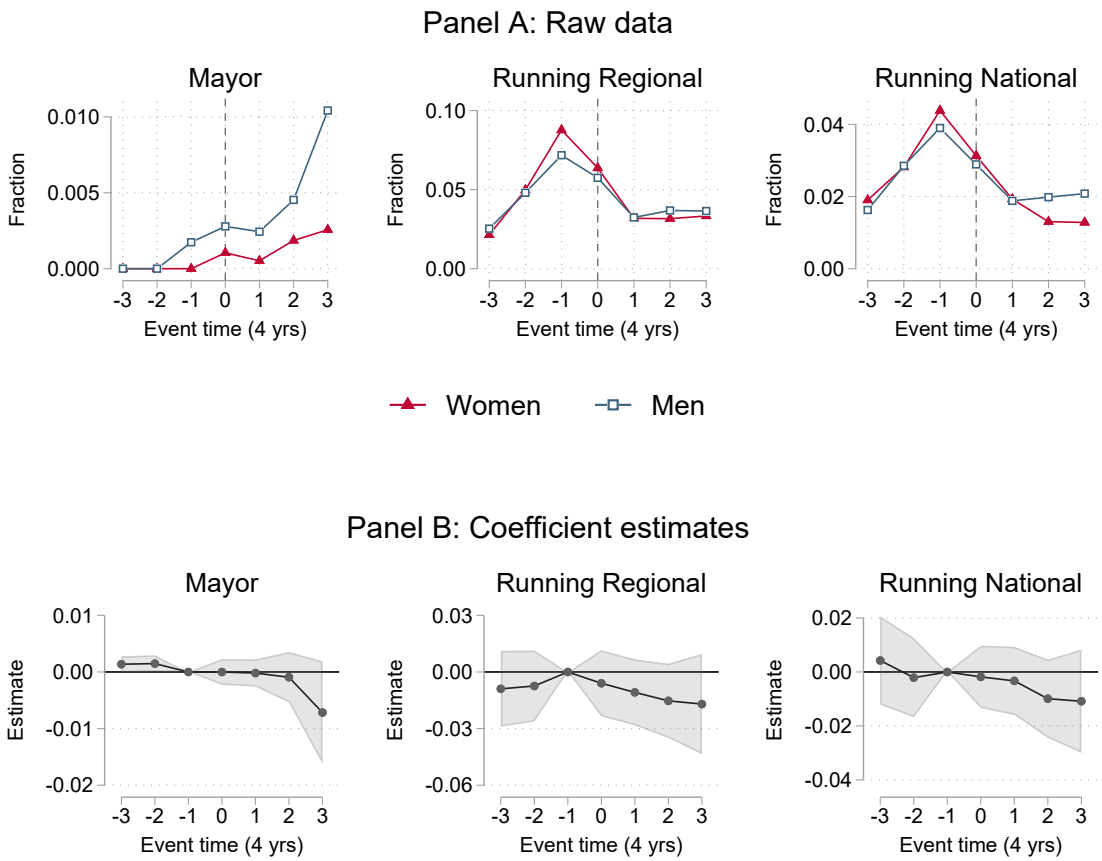
Note: Panel A plots the fraction of men (blue squares) and women (red circles) for whom the outcome variable is equal to one at each election period t. Panel B contains estimates of the parameters of interest, β_j, in Equation (1) together with 95% confidence intervals. These specifications also include candidate fixed effects. Standard errors are clustered at the individual level. The sample consists of all politicians who ran for office prior to election period $t = 0$ (23,935 observations from 4,787 individuals).

Figure A.5: The child penalty in local political outcomes, conditional on running at $t = -1$



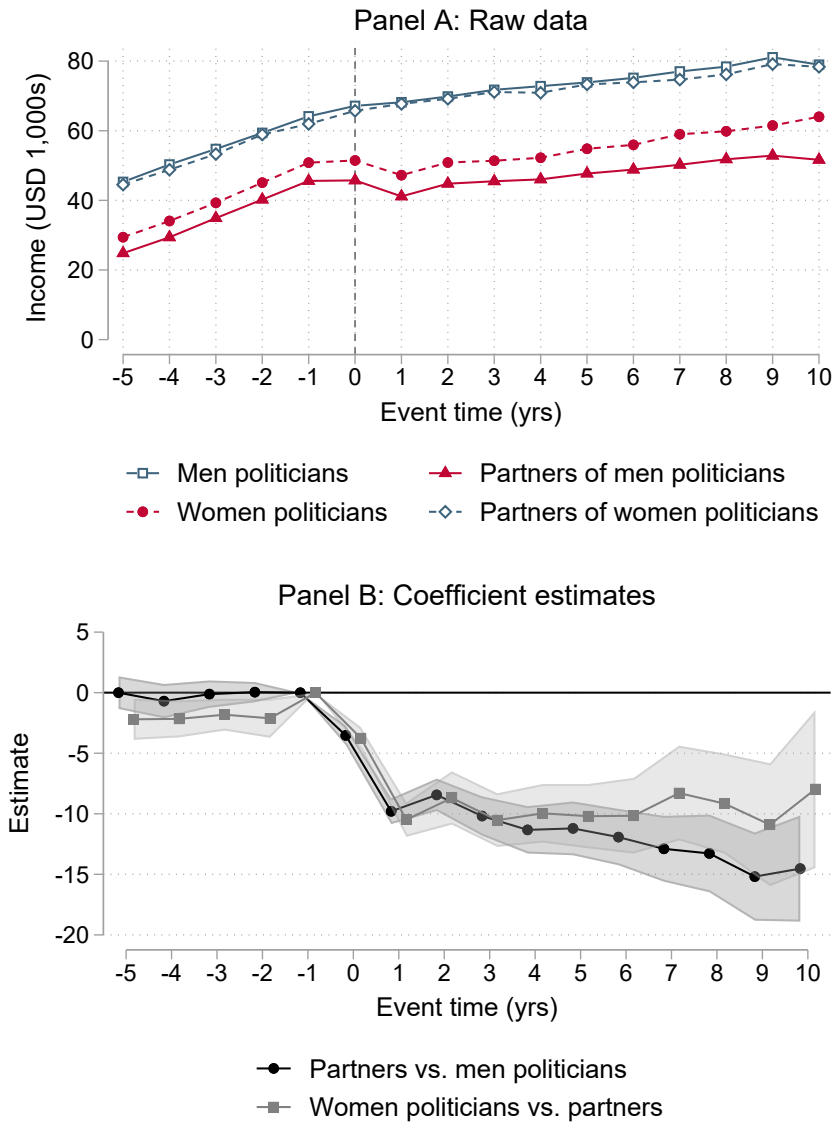
Note: Panel A plots the fraction of men (blue squares) and women (red circles) for whom the outcome variable is equal to one at each election period t. Panel B contains estimates of the parameters of interest, β_j, in Equation (1) together with 95% confidence intervals. Standard errors are clustered at the individual level. The sample consists of the subset of candidates in our main sample who ran for office at $t = -1$ (16,236 observations from 3,471 individuals).

Figure A.6: The child penalty in higher-level political outcomes



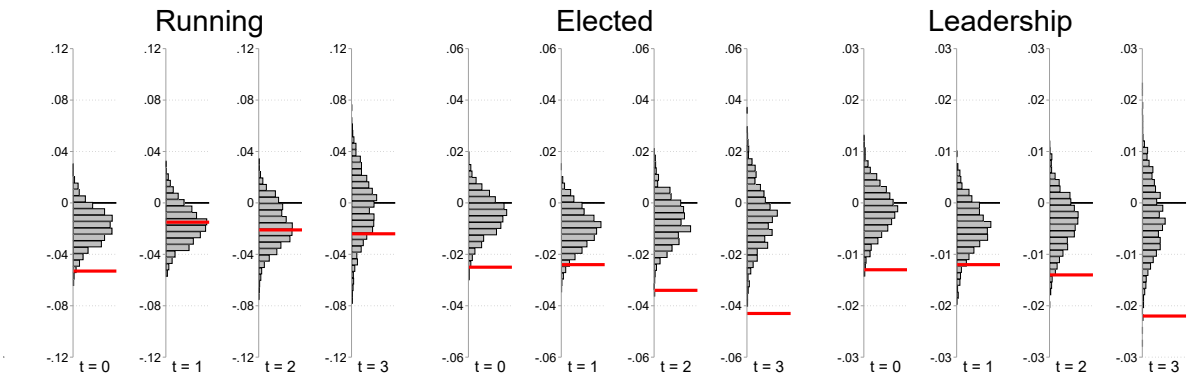
Note: Panel A plots the fraction of men (blue squares) and women (red circles) for whom the outcome variable is equal to one at each election period t. Panel B contains estimates of the parameters of interest, β_j, in Equation (1) together with 95% confidence intervals. Standard errors are clustered at the individual level. The sample consists of all politicians who ran for local *office prior to election period $t = 0$ (23,935 observations from 4,787 individuals).*

Figure A.7: The child penalty in income for politicians and their partners



Note: Results from our labour market analysis using all male politicians in our main sample versus their partners (solid lines), and all female politicians in our main sample versus their partners (dashed lines). Panel A plots the mean income in 1000s of constant (2015) USD for men (blue squares/diamonds) and women (red circles/triangles) at each event-year k. Panel B contains estimates of the parameters of interest, β_j, in Equation (1) together with 95% confidence intervals. The reference category is event period $k = -1$. The male politician-partner sample consists of $55,054$ observations from $2,458$ individuals while the female politician-partner sample consists of $34,428$ observations from $1,600$ individuals.

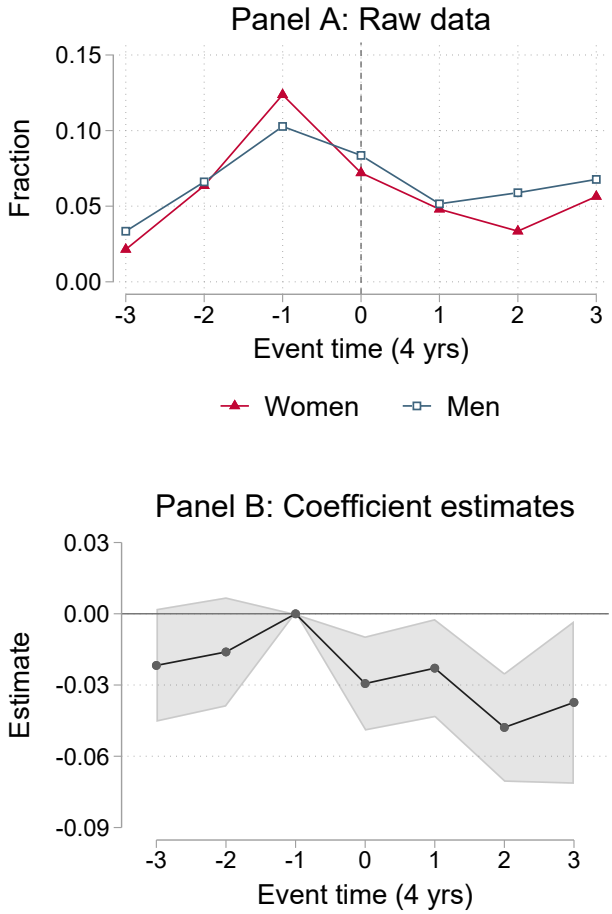
Figure A.8: Distributions of placebo estimates



Note: Results from our placebo analyses where childbirths are assigned randomly to politicians in the placebo sample using a uniform distribution of within-sample years. Each subplot shows the distribution of estimates of the parameters of interest, β_j, in Equation (1), at each election period from $t = 0$, after 1000 repeated iterations. The red lines show the actual estimates from Figure 1. Regressions are run on a pooled sample of, on average, 24,475 observations from 5,362 individuals (the number of observations vary slightly in each iteration depending on the random assignment of childbirths).

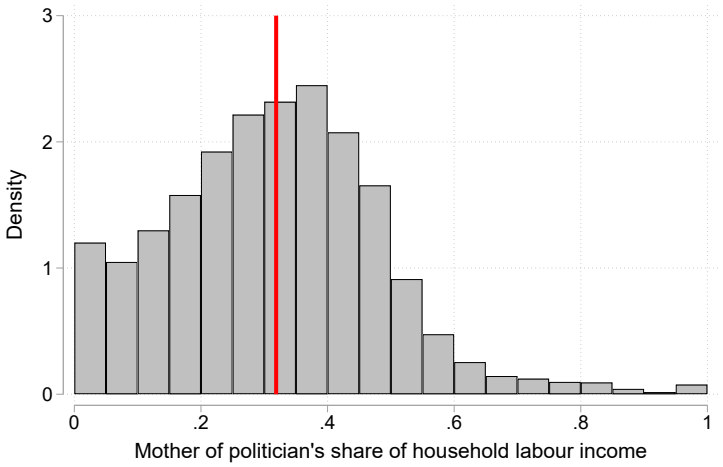
To construct the placebo sample, we only include candidates who are either (i) old enough for us to observe their complete fertility history, or (ii) whose background characteristics suggest they are most likely to remain childless *in the future. We follow Kleven* et al. *(2019b) and place all childless candidates who are 40 or older in 2021 in the placebo sample automatically. For younger cohorts, candidates are assigned to the placebo sample so that their zero-fertility distribution equals that of the older cohorts (where fertility is observed). Specifically, we use the universe of political candidates born between 1950 and 1981 to estimate $P_i = \boldsymbol{\lambda}'\mathbf{X_i}$, where P_i is a dummy variable indicating zero fertility for individual i in 2021, and X_i contains the following set of dummy variables: cohort-specific income quartile at age 25, maximum level of education obtained, municipality of birth, and the decades of birth of each individual's mother and father. We then use these estimates to predict the probability of zero lifetime fertility ($\hat{P}_i$) for childless politicians born after 1981, and keep those n_c candidates with the highest $\hat{P}_i$ such that $\frac{n_c}{N_c} = P_{1950-1981}$, where N_c is the total number of politicians in cohort c and $P_{1950-1981}$ is the average probability for zero lifetime fertility among politicians born between 1950 and 1981.*

Figure A.9: Closed-list hypothetical election outcomes



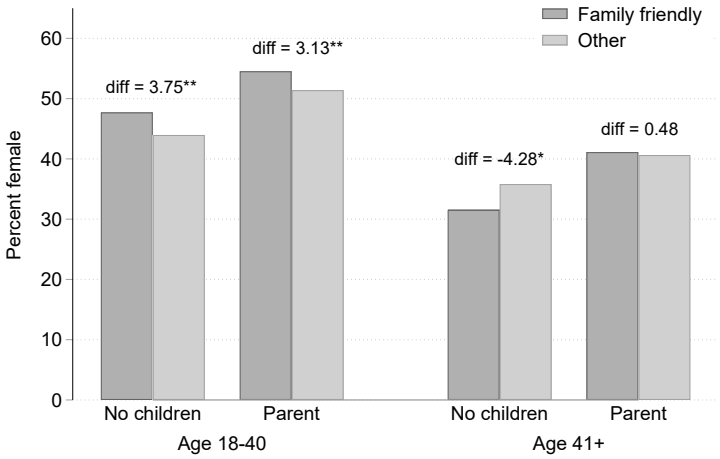
Panel A plots the fraction of men (blue squares) and women (red circles) who would have been elected in a closed-list counterfactual scenario (i.e. without taking personal votes into account) at each election period t. This outcome takes a value equal to one if the candidate's rank position on the ballot is higher than or equal to the number of seats awarded to the candidate's list. Panel B contains estimates of the parameters of interest, β_j, in Equation (1) together with 95% confidence intervals. Standard errors are clustered at the individual level. The sample consists of all politicians who ran for office prior to election period $t = 0$ (23,935 observations from 4,787 individuals).

Figure A.10: Distribution of politicians' parents' labour division index



Note: Histogram of the index I, which captures politicians' mothers' share of household income when each politician was 0-18 years old. Each bin is five percentage points wide. The red line denotes the median of the distribution.

Figure A.11: Share of women among running candidates, by parental status, age and municipality type



*Note: This figure shows the share of female candidates in the 2015 election by parental status, age and municipality type. Municipalities are classified as 'family friendly' if all local council meetings in 2018 were held during standard business hours. The labels above each pair of bars report the differences between bars, together with the degree of statistical significance. Standard errors are clustered at the municipality level. * denotes 10% statistical significance, ** 5% and *** 1%.*

Table A.1: Differences in political selection by level of government and gender

	Years of educ.					Income				
	Women		Men			Women		Men		
Level	Mean (1)	Diff. (2)	Mean (3)	Diff. (4)	DiD (5)	Mean (6)	Diff. (7)	Mean (8)	Diff. (9)	DiD (10)
National level										
Cabinet (N = 528)	16.3	3.3***	15.9	3.2***	0.1	113.4	77.6***	112.8	62.4***	15.2***
Parliament (N = 845)	15.4	2.4***	14.7	2.0***	0.4**	103.5	67.7***	104.5	54.2***	13.5***
Candidate (N = 18,316)	14.2	1.2***	13.8	1.1***	0.1***	50.4	14.6***	57.4	7.0***	7.5***
Regional level										
Council (N = 3,373)	14.4	1.4***	14.1	1.4***	-0.0	64.7	28.8***	75.5	25.2***	3.6***
Candidate (N = 33,423)	14.1	1.1***	13.6	0.9***	0.1***	45.3	9.5***	53.7	3.3***	6.1***
Local level										
Mayor (N = 2,060)	14.8	1.8***	13.9	1.2***	0.6***	80.7	44.8***	86.3	35.9***	8.9***
Executive board (N = 15,141)	14.4	1.4***	13.8	1.1***	0.4***	59.6	23.7***	74.0	23.6***	0.1
Council (N = 51,799)	14.2	1.2***	13.5	0.8***	0.4***	52.0	16.1***	65.7	15.3***	0.8***
Candidate (N = 292,590)	13.6	0.6***	13.1	0.4***	0.2***	40.0	4.2***	52.9	2.5***	1.6***
Population (N = 4,218,917)	13.0		12.7			35.9		50.4		

*Notes: Columns (1) and (3) show average years of education, for men and women, respectively, as in Table 1. Columns (6) and (8) show average yearly income in 1000's of constant (2015) USD, for men and women, respectively, as in Table 1. Columns (2) and (4) show differences in politicians' education relative to the population (reported in the bottom row), by gender. Corresponding differences for income are shown in columns (7) and (9). Columns (5) and (10) show differential effects between genders (DiD). Significance stars reflect conventional t-tests of equal effects. * denotes 10% statistical significance, ** 5% and *** 1%.*

Table A.2: Summary statistics by sample

Panel A: Politicians	Full sample		Women		Men	
	Mean	SD	Mean	SD	Mean	SD
Event-time $k = -1$						
Female (percent)	40.03	49.00				
Age	31.07	5.52	29.17	4.17	32.33	5.93
Income, constant (2015) USD 1000s	58.81	30.35	50.45	23.90	64.39	32.82
Years of education	14.59	2.78	15.33	2.52	14.10	2.84
Number of children (as of 2021)	1.91	0.73	1.89	0.68	1.93	0.76
N	4,787		1,916		2,871	

Panel B: Population	Full sample		Women		Men	
	Mean	SD	Mean	SD	Mean	SD
Event-time $k = -1$						
Female (percent)	49.75	50.00				
Age	28.98	5.50	27.78	4.99	30.16	5.72
Income, constant (2015) USD 1000s	49.11	32.50	41.57	25.28	56.57	36.84
Years of education	13.57	3.20	13.95	3.18	13.19	3.17
Number of children (as of 2021)	1.97	0.77	1.98	0.76	1.96	0.78
N	796,471		396,271		400,200	

Notes: Summary statistics for our main politicians sample (Panel A) and the population of Norwegian parents during our sample period (Panel B). Each parent is observed one year before the birth of their first child (event-time $k = -1$*).*

Table A.3: Baseline results, political outcomes

		Running		Elected		Leadership	
	n_t	(1)	(2)	(3)	(4)	(5)	(6)
Female $\times$ $t = -3$	1,947	-0.117***	-0.024	-0.018	-0.007	-0.006	-0.006
		(0.025)	(0.022)	(0.012)	(0.012)	(0.005)	(0.005)
Female $\times$ $t = -2$	3,629	-0.039	0.012	-0.006	0.000	-0.000	0.000
		(0.026)	(0.023)	(0.011)	(0.011)	(0.005)	(0.005)
Female $\times$ $t = -1$	4,787	ref.	ref.	ref.	ref.	ref.	ref.
Female $\times$ $t = 0$	4,787	-0.086***	-0.053***	-0.028***	-0.025***	-0.012**	-0.013***
		(0.016)	(0.015)	(0.009)	(0.010)	(0.005)	(0.005)
Female $\times$ $t = 1$	4,787	-0.046***	-0.015	-0.024**	-0.024**	-0.010**	-0.012**
		(0.016)	(0.015)	(0.010)	(0.010)	(0.005)	(0.005)
Female $\times$ $t = 2$	2,840	-0.047**	-0.021	-0.031***	-0.034***	-0.011*	-0.014**
		(0.019)	(0.019)	(0.012)	(0.012)	(0.006)	(0.007)
Female $\times$ $t = 3$	1,158	-0.037	-0.024	-0.036**	-0.043**	-0.019*	-0.022**
		(0.028)	(0.028)	(0.016)	(0.017)	(0.010)	(0.010)
Party FE		No	Yes	No	Yes	No	Yes
Age FE		No	Yes	No	Yes	No	Yes
Year FE		No	Yes	No	Yes	No	Yes
Observations		23,935	23,935	23,935	23,935	23,935	23,935
Clusters		4,787	4,787	4,787	4,787	4,787	4,787
R-squared		0.17	0.27	0.01	0.04	0.00	0.02

*Notes: Each column represents a separate regression of Equation (1), where the dependent variable is a dummy capturing the outcome indicated by the column headers. Level effects are not reported. The number of individuals (clusters) observed at each election period are reported in the second column. The reference category is event period $t = -1$. Standard errors are clustered at the individual level and reported in parentheses. * denotes 10% statistical significance, ** 5% and *** 1%.*

Table A.4: Baseline results by cohorts, political outcomes

	Running			Elected			Leadership		
	(1) 2007	(2) 2011	(3) 2015	(4) 2007	(5) 2011	(6) 2015	(7) 2007	(8) 2011	(9) 2015
Female × $t = -3$			-0.025			-0.003			-0.005
			(0.030)			(0.016)			(0.007)
Female × $t = -2$		-0.056	0.040		-0.007	0.001		-0.005	0.002
		(0.039)	(0.034)		(0.018)	(0.016)		(0.007)	(0.007)
Female × $t = -1$	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
Female × $t = 0$	-0.002	-0.118***	-0.062***	-0.024	-0.033**	-0.026*	-0.009	-0.025***	-0.006
	(0.031)	(0.027)	(0.023)	(0.024)	(0.015)	(0.014)	(0.012)	(0.007)	(0.008)
Female × $t = 1$	-0.047*	-0.040	-0.006	-0.033	-0.026	-0.028*	-0.014	-0.017**	-0.007
	(0.028)	(0.028)	(0.024)	(0.025)	(0.017)	(0.015)	(0.013)	(0.007)	(0.008)
Female × $t = 2$	-0.036	-0.059**		-0.061**	-0.032*		-0.010	-0.017**	
	(0.027)	(0.027)		(0.025)	(0.017)		(0.014)	(0.008)	
Female × $t = 3$	-0.030			-0.062**			-0.025*		
	(0.026)			(0.025)			(0.014)		
Party FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,790	8,410	9,735	5,790	8,410	9,735	5,790	8,410	9,735
Clusters	1,158	1,682	1,947	1,158	1,682	1,947	1,158	1,682	1,947
R-squared	0.37	0.25	0.23	0.04	0.04	0.05	0.02	0.02	0.02

*Notes: Each column represents a separate regression of Equation (1) on individual cohorts, where the dependent variable is a dummy capturing the outcome indicated by the column group headers. Level effects are not reported. The reference category is event period $t = -1$. Standard errors are clustered at the individual level and reported in parentheses. * denotes 10% statistical significance, ** 5% and *** 1%.*

Table A.5: Relationship between parenthood and vote shares

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
No children	ref.	ref.	ref.	ref.	ref.	ref.
Children	0.18**	0.17**	0.19***	0.60***	0.52***	0.42***
	(0.073)	(0.070)	(0.045)	(0.068)	(0.062)	(0.040)
Incumbent	No	Yes	Yes	No	Yes	Yes
Rank FE	No	No	Yes	No	No	Yes
Party bonus	No	No	Yes	No	No	Yes
Observations	94,431	94,431	94,431	129,233	129,233	129,233
Clusters	1,563	1,563	1,563	1,563	1,563	1,563
R-squared	0.39	0.46	0.77	0.37	0.48	0.77

*Notes: Each column represents a separate regression where the dependent variable is the share (in %) of personal votes received by each candidate-year. The sample consists of the universe of candidates who ran for office in the 2007-2019 Norwegian local elections. Incumbent is a dummy equal to one if the candidate also held a seat in the local council in the previous term (this implies that we lose observations from 2003). In addition to the reported effects, all specifications control nonparametrically for age, year-specific income quartile (calculated as an average over the four years up to and including the election year), education (nine levels), party affiliation, geography and time (municipality-year fixed effects). The reference category is candidates who are childless in the election year. Standard errors are clustered at the municipality-year level and reported in parentheses. * denotes 10% statistical significance, ** 5% and *** 1%.*