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Abstract

Women tend to experience substantial declines in their labor income after their first child is born, while men do not. Do such "child penalties" also exist in the political arena? Using extensive administrative data from Norway and an event-study methodology, we find that women drop out of local politics to a larger extent than men after their first child is born. Parenthood also seems to have a differential long-term effect on women and men's political careers, which may explain why women, especially women with children, are underrepresented at higher levels of the political hierarchy.

JEL-Codes: D630, D720, J130, J160.

Keywords: gender gap, child penalties, political selection.

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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 about 20–30 percent. In countries characterized by more gender conservative views, such as Germany and Austria, the estimated "child penalties" are about twice as large (Kleven et al., 2019a). There are several reasons that may explain why men 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 specialize in lucrative "greedy work", and for the other – typically the mother – to prioritize the children.

The political arena shares many of the characteristics of the "greedy jobs" highlighted by Goldin (2014, 2021), e.g., 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 (Cirone et al., 2021). 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.

There are two key reasons for caring about the gender gap in political representation. First, the citizen-candidate framework suggests that politicians' social ties and group 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, 2019).[1] Second, 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).

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 (World Economic Forum, 2021)[2], 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 population (of about 5.4 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 labor 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, such as the United States (Teele et al., 2018). In our main analysis, we study how the arrival of children affects future outcomes among candidates running for office in the election period before receiving their first child. We document that the probability of winning a seat in the subsequent election falls almost twice as much for women compared to men in the same situation. These effects remain strong and statistically significant several election periods into the future. Women are also less likely than men to obtain leadership positions after becoming

[1] 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 on the substantive effects of female representation on policies.

[2] In the 2021 global gender gap index, Norway ranks third among the 156 countries included. However, the share of women in senior roles, as defined by World Economic Forum (2021), has been declining in the 2000s and is currently at 34.5%.

parents. We find no evidence that these effects arise due to demand-side factors, either coming from voters or from within the party itself. Instead, our findings are consistent with the idea that women's preferences over family and career are shaped by the gender roles they are exposed during childhood.

This paper contributes to our understanding of the formation of gender gaps in political outcomes. We engage with two broad literatures. First, we relate conceptually and methodologically to the growing literature on labor 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, 2021) and the United States (Chung et al., 2017; Kuziemko et al., 2018), parenthood has been found to have a large and persistent effect on the labor market outcomes of women, but not men.[4] To the best of our knowledge, our paper is the first to estimate child penalties in the political arena. We also demonstrate that child penalties exist in other parts of society besides traditional labor markets.

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, 2021).[5] This has prompted some scholars to investigate the institutions and

[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 labor 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). See also Waldfogel (1997); Budig and England (2001); and Correll et al. (2007).

[4]Several potential explanations for why child penalties materialize have 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, because 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' labor supply and the labor supply history of maternal grandparents, suggesting that preferences or gender norms associated with parenthood are the most-probable drivers of child penalties. This interpretation aligns well with related studies showing how gender identity norms shapes marriage and labor markets (see, e.g., Bertrand et al., 2015; Bursztyn et al., 2017; Folke and Rickne, 2020).

[5]Even if women "win elections as often as men", a gender bias may still exist if women politicians have to be of a consistently higher quality than men in order to achieve gender parity. While some evidence suggests that this is the case (e.g., Anzia and Berry, 2011), and that gender preferences shape voter behavior (Sanbonmatsu, 2002), experimental evidence from Teele et al. (2018) suggest that outright

mechanisms through which candidates are driven to seek elected office in the first place for understanding 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).[6] 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 men and women candidates are similarly persistent after losing elections, Brown et al. (2021) and Wasserman (2021) reach the opposite conclusion (stronger attrition among women). All of these studies use regression discontinuity designs and data from the United States.[7] By studying political careers surrounding parenthood, we hope to contribute to a better understanding of why women remain underrepresented in politics.

2. Empirical case: Norway

2.1 *Norwegian elections*

Norway is a unitary state with three levels of government. National legislative elections take place every fourth year in September (e.g., 2001, 2005, ..., 2021). 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 (e.g., 2003, 2007, ...,

discrimination or double standards (unconscious biases) are not important drivers of the gender gap in political outcomes.

[6]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.

[7]Cipullo (2021) uses a difference-in-discontinuity design and data from two cases, partisan primaries for the United States House of representatives, and run-off elections for mayors in Italy. He finds that women who barely qualify for the final round are 7-9 percentage point less likely to be elected than barely qualified men, and these effects persist for subsequent elections. 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 higher propensity of women to exit is correlated with local gender norms concerning the place of women in society. Nowacki (2022) finds that in Norway, women experience an incumbency advantage in winning future elections that is up to 60 percent lower than men's.

2019). All Norwegian citizens who are at least age 18 in the election year are eligible to vote and can run for political office.[8] 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 local government elections, parties have the opportunity to give certain candidates a "head start". This bonus, which corresponds to 25% of the total number of votes received by the party, makes it almost impossible for other candidates to compete with the "head start" candidates (Fiva and Røhr, 2018). Overall, Norway should be considered a party-centered electoral environment where incumbents have a strong advantage over newcomers (Cirone et al., 2021).

Preparations for an election begin up to a year before the actual polling day with a closed and non-standardized nomination process. Typically, parties form district-specific committees who recommend lists based on announced candidacies among party members. Special selection criteria, such as gender quotas, are also taken into account. Lists are required by law to be handed in to the municipal government no later than the 31st of March in an election year. Election authorities then have until the 1st of June to formally approve the lists. At this point, it is generally not possible for a candidate to withdraw either from a list or from political appointment if elected into office. In special circumstances, however, exceptions may be granted.

Elections are held on the second Monday of September, although some municipalities accept ballots also on the Sunday before and an increasing share of voters are taking advantage of advance polling available from mid-August. After the election, newly-elected municipal councils on the local and regional levels form by the end of October. An executive board, consisting of members of the council, is elected at the initial assembly.[9]

[8]Foreign nationals who have lived in the country for three years continuously can vote in local, but not national elections.

[9]The executive board is usually elected through proportional representation based on candidate lists put forward by parties. The local government act requires parties' candidate lists to comprise at least

The board serves for four years and is led by the mayor, who is also elected by the council.[10]

In Norway, like in the other Scandinavian countries, being a local politician is a part-time position that typically is held concurrently with other jobs.[11] The economic returns from holding a regular seat in a local council is small or non-existent (Berg, 2020; Kotakorpi et al., 2017; Cirone et al., 2021).[12] Being a mayor, however, is a full-time well-paid job. Cirone et al. (2021) document that candidates, on average, get about a 25 percent increase in their income when they become mayors compared to similar non-promoted candidates.

2.2 *Women in politics*

In the 1970s and 1980s, most Norwegian parties introduced gender quotas requiring that all ballots should feature at least 40 percent women (Fiva and Smith, 2017).[13] During these decades the fraction of women elected to parliament quadrupled from about 10 percent to about 40 percent. A few years later, a 1992 legislative reform further required that local-level executive boards consist of at least 40 percent politicians of each gender (Geys and Sørensen, 2019). The current status of these quotas is summarized in Table 1. From the third column, we see that women continue to make up about 40 percent of candidates across all levels of government where quotas are enforced.[14] Among municipal mayors, which is the only political office not formally affected by gender-balancing

40% politicians of each gender (Geys and Sørensen, 2019). In the 2019-2023 election period, executive boards vary in size from 5 to 19, with a median size of 7.

[10] Three municipalities have during our sample period used a parliamentary system (Oslo, Bergen, and Tromsø). We exclude these municipalities from our empirical analysis below.

[11] Elected representatives have the right to a leave of absence from work to attend local council meetings (Working Environment Act § 12-13).

[12] 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 remaining half received less than NOK 50,000 (for comparison, the median income in 2011 was NOK 349,000). Executive board members also collected modest fees.

[13] 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*).

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

policies, women make up less than 25 percent. While this is strong evidence of the effectiveness of gender quotas in terms of compliance, it is perhaps surprising that even decades after their implementation, these measures do not appear to have stimulated a further increase in the share of women representatives beyond that which is decreed by the quotas directly.

Columns 4 and 5 of Table 1 show the politicians' average number of children at the different political offices, separately for male and female candidates. Across all levels, women have fewer children than men. The difference are especially salient at the top of the hierarchy (members of parliament; higher offices) where women have an average of 1.86 and 1.70 children, respectively, compared to 2.15 and 2.07 for men.[15] This pattern is consistent with child penalties that accumulate over political careers but could also be driven by other factors.

Table 1: Summary of gender outcomes across different political offices, 2003–2019

			Average number of children		
Level	Gender quota	Female	Women	Men	Diff (W-M)
Higher office (N = 660)	Party enforced	41 %	1.70	2.07	-0.38***
National parliament (N = 813)	Party enforced	39 %	1.86	2.15	-0.29***
Regional council (N = 3,380)	Party enforced	45 %	2.02	2.08	-0.06
Municipal Mayor (N = 2,062)	None	25 %	2.43	2.45	-0.01
Executive board (N = 15,146)	Law mandated (40 %)	41 %	2.32	2.42	-0.10***
Local council (N = 51,574)	Party enforced	38 %	2.21	2.33	-0.12***

*Notes: The table contains descriptive outcomes related to gender across different political offices during the 2003–2019 period. In the second column, "Party enforced" means that most parties have employed discretionary gender quotas at this level over the entire sample period. The last column performs a conventional (robust) t-test of differences in means between the number of children among men and women in the preceding columns. * denotes 10% statistical significance, ** 5% and *** 1%.*

[15] 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.

2.3 *The welfare state shapes family life*

Outside of the political arena, the Norwegian welfare state encourages the combination of employment and family duties for both women and men.[16] First, a generous parental leave scheme encourages both fathers and mothers to take time off work in a child's first year. Wage-compensated parental leave has been extended repeatedly since the 1970s, from the 18 weeks of leave with full wage compensation first granted in 1977, to 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.[17] This caused a drastic change in fathers' leave-taking behavior, but mothers continued to take the longest spells (Cools et al., 2015). Second, affordable high-quality child care facilitates dual-earner families as children grow older. Close to 80% of 3-5 year-olds were enrolled in formal child care in 2000 (Andresen and Havnes, 2019). However, there remained a substantial excess demand, especially among younger children. A reform passed in 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. About 95% of 3-5 year-olds were enrolled in formal child care in 2012.

3. Methodology

3.1 *Data*

To estimate child penalties on 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).[18] Our data contain election outcomes for every

[16] Another example, which illustrates Norway's progressiveness regarding gender equality, is the 2003 implementation of a board member gender quota affecting all publicly-listed Norwegian companies (see, e.g., Bertrand et al. (2018)).

[17] The quota has been expanded multiple times, from four weeks in 1993 to 15 in 2018.

[18] 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 in the local council have about a 9 percentage points (43 percent) higher probability to get 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)

candidate, as well as detailed background information such as political party, 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 percent of national-level politicians started their political career at the local level (Cirone et al., 2021). 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 labor market literature that women's labor market outcomes lag behind men for at least a decade after they become parents. Second, there are many local politicians, which makes it possible to conduct meaningful statistical inference.[19] Each politician-year observation is then matched with the administrative registers of Statistics Norway, enabling us to pinpoint the year of birth of politicians' children and extract other observables like education and labor market outcomes. We also incorporate data about candidates running for national office (Fiva and Smith, 2017), including the most recent (September 2021) election.

To capture the child penalty *on politicians*, we focus our analysis on candidates who were already politically active *before* becoming parents. This implies that we are estimating 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 election period $t = -1$ (15,779 candidate-year observations).[20] For maximum statistical power, we base our main estimation results on an unbalanced sample which tapers in size before $t = -1$

finds that the incumbency advantage is smaller for women than for men.

[19]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 only 169 elected candidates. In the 2005–2009 election period, only five had a child.

[20]We omit from our main sample candidates who ran for local office in *Oslo*, *Bergen* and *Tromsø* at $t = -1$ as these municipalities employ a parliamentary system, where there is no executive board and the function of the mayor is more ceremonial.

and after $t = 1$. Results using a fully-balanced sample (with fewer observations) are presented in the appendix. We also construct a separate time line to assess labor 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 baseline politicians sample are shown in Appendix Table A.1.

3.2 *Identification*

We adopt a variant of the event-study methodology suggested by Kleven et al. (2019b), which is centered on changes around the birth of the first child for mothers relative to fathers. Kleven et al. (2019b) argue that, even though fertility choices are not exogenous, the event of having a first child generates sharp changes in 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 participated in local politics before becoming parents, we observe a mechanical mean reversion in political outcomes as event time progresses. Instead of relying on smoothness assumptions, we implement an event study difference-in-differences (DiD) specification where political outcomes for women after parenthood are compared directly to those of their male counterparts. Given that the labor market child penalty for men is typically estimated to be a "non-event", this design change should not matter much.[21] Our main empirical specification (regardless of being applied to time line t or k) takes the following form:

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

In Equation (1), Y_{ist} represent an outcome for individual i in year s at event time

[21]We validate the DiD by showing that its implementation to estimate labor market child penalties on the full population of Norwegian parents yield results which are largely consistent with the existing literature.

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 ($\sum_l \gamma_l \cdot \mathbf{I}[l = age_{is}]$), which controls nonparametrically for underlying life-cycle trends[22], election year fixed effects ($\sum_y \delta_y \cdot \mathbf{I}[y = s]$), which controls 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 $t=-1$).[23] We allow for arbitrary correlation of the error terms ϵ_{ist} within individuals over time, by clustering standard errors at the individual level.[24]

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: 1) a dummy variable equal to one if candidate i wins a local council seat in election t; 2) a dummy variable equal to one if candidate i gets a leadership position in the local council (municipal executive board) following election t; or 3) a dummy variable equal to one if candidate i becomes appointed to municipal mayor following election t. In the appendix, we also consider effects on individuals' political careers at higher political offices. Second, we consider **labor market outcomes**, where Y_{isk} represents income in 1000s of constant (2015) Norwegian kroner in event year k.[25] The specification is run on the baseline politicians sample but, for validation, also on 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 child births should not be considered an exogenous event. This concern may be particularly pressing when relying on four-year election periods, as we do here. In general, the likelihood of outcomes being affected by mechanisms other than parenthood increases the more

[22]In the general Norwegian population, women are, on average, about two years younger than men when they have their first child (29.5 years versus 31.8 years in 2018).

[23]We exclude the party fixed effects from the analysis of labor market outcomes.

[24]Clustering at the municipality level gives almost identical results.

[25]Income is defined as the sum of pre-tax market income from wages, self-employment and work-related cash transfers, including unemployment benefits, sick leave benefits, and parental leave benefits ("*pensjonsgivende inntekt*").

time passes between each observation. We are not able to mitigate these concerns fully. But the fact that our dynamic responses mirror the labor market outcomes (which do use yearly intervals) combined with generally insignificant pre-trends makes us less concerned regarding the second issue. However, even if our analysis should 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.

4. Results

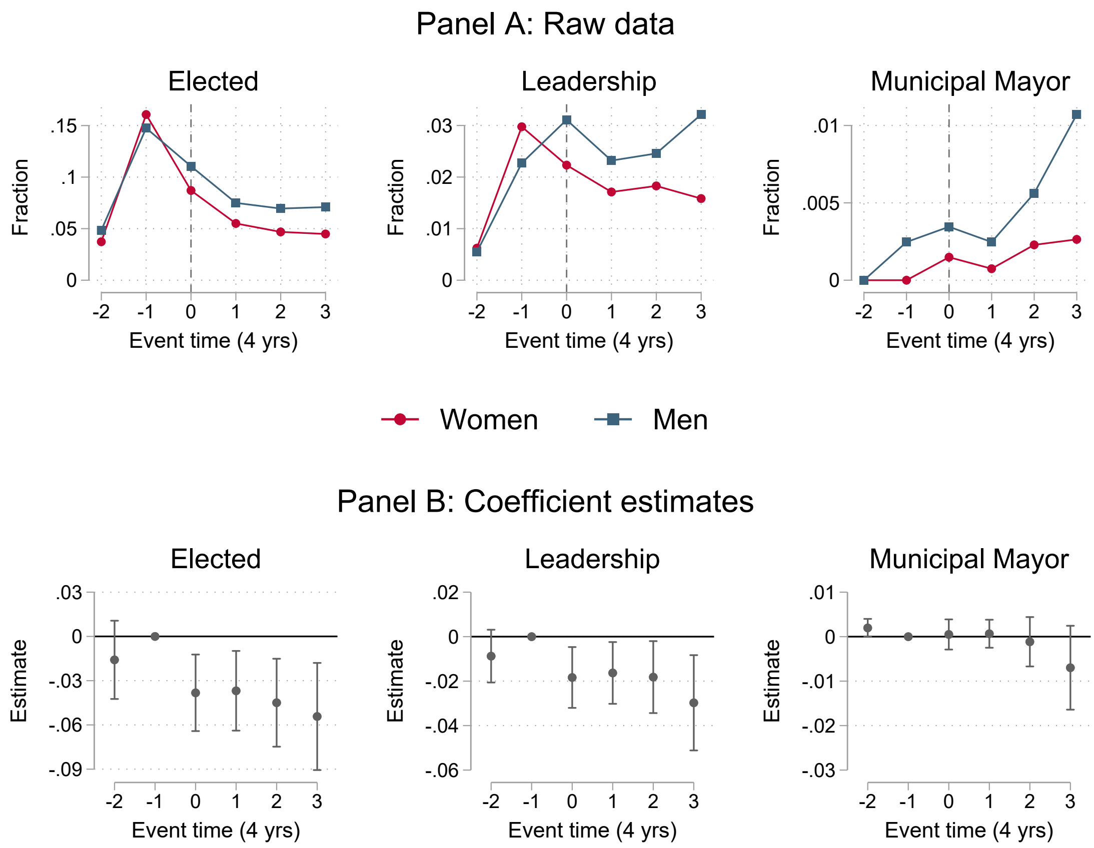
4.1 *Political outcomes*

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 for whom the outcome variable is equal to one at each event period. We observe that among candidates running for local office at $t = -1$, approximately 15 percent won a local council seat in that election period. The fraction of winners among women is slightly higher than that among men, but the difference is negligible. The same is true at election period $t - 2$, although candidates' young age at this point implies that the probability of winning is much smaller for both groups.[26] At event time $t = 0$, the election period during which candidates become parents for the first time, outlook changes drastically depending on the candidates' gender; for men, the probability of winning falls by about 3.5 percentage points to a little over eleven percent. For women, the drop is almost twice as large, implying a probability of winning at $t = 0$ of approximately only 8.5 percent. Both outcomes continue to fall in subsequent periods, and the two lines remain parallel until we last observe them at $t = 3$ (12 years after $t = 0$). The bottom left plot contains estimates of $\boldsymbol{\beta'}$ 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

[26] These estimates are obtained using the baseline politicians sample, which is unbalanced at $t - 2$. We provide results from using a fully-balanced panel in Appendix Figure A.3. With fewer observations, these estimates are less precise but generally mirror the results from the unbalanced sample.

as the reference category).[27] We see that the 95 % confidence intervals never overlap with zero after $t = -1$.[28]

Figure 1: The Child Penalty in Local Political Outcomes



Note: Results from our baseline specification conditional on candidates running at $t = -1$. *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 from the parameter-vector of interest,* $\boldsymbol{\beta}$, *in Equation (1) along with 95 % confidence intervals. Regressions are run on the (unbalanced) baseline politicians sample of* 15,779 *observations from* 3,370 *individuals.*

In the middle set of plots, we estimate the probability of being appointed to a local leadership position after the arrival of the first child. We define leadership candidates as

[27]By focusing on the *differences* between genders instead of the levels, the DiD estimates are not affected by the mechanical mean reversion in outcomes (which causes the "kinks") observed in the top row plots.

[28]A summary of regression results, including specifications without nonparametric controls, is provided in Appendix Table A.2.

any members of municipal executive boards, which is a subset of the elected individuals, and includes both mayor and deputy mayor positions. This time women have a more pronounced advantage at $t = -1$ than men, perhaps as a result of the gender quota that governs promotion to the executive board (see footnote 9). In every subsequent period, however, the reverse is true; starting with the election period when candidates become parents for the first time, women drop almost two percentage points relative to men. Because appointment to leadership status is rare for any candidate, the effects are small in levels but large in relative terms. After $t = 0$, the two outcomes move in parallel until the very last period, where fathers (at $t = 0$) are over twice as likely to act as leaders than mothers (at $t = 0$). Interestingly, female candidates seem to trend downward over time whereas male candidates do not.

The final outcome we consider on the local level is appointment to mayor (rightmost plots). As the only full-time position among those considered so far, the mayor is at the top of the local political hierarchy. For this outcome, there is some indication of a long-run child penalty. At the end of the sample, among candidates who ran for office at $t = -1$, and for whom we have enough observations, men have about a one percent chance of becoming a mayor. This is several times greater than the comparable probability for women. However, none of the effects are statistically significant, reflecting that mayoral appointment is a rare outcome in our sample. Because almost no candidates become mayors at a young age (the median age at election is 50), there is no substantial short-run child penalty for this outcome.

In Appendix Figure A.4, we estimate the probability of advancing to higher levels of the political hierarchy for candidates running for local office in period $t = -1$. 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, such that nominations are almost entirely meritocratic for entry-level jobs (for which no feeder offices exist) but then become progressively more seniority-based as one moves up in the hierarchy of offices. We focus our sample slightly by excluding the 418 individuals who already competed for a national or regional office before $t = 0$, and implement the same kind of specification used to analyse local-level outcomes above. There is no evidence in Appendix Figure A.4 that parenthood causes a gap in the probability running for office at the regional level. On the national level, we find some evidence that female politicians who become mothers early in their careers miss out on the opportunity to reach the top of the political ladder. However, this outcome is a rare occurrence in our sample, implying that results need to be interpreted with caution.

4.2 *Labor market outcomes*

Since the vast majority of Norwegian local politicians maintain a traditional career outside of holding office, we can learn more about the political child penalty by studying these individuals' labor income trajectories. Moreover, by exploiting that our administrative data cover 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 labor 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 NOK 100,000 less than men. After parenthood, however, the gender gap in income sharply doubles, owing entirely to a drop in female income, which does not occur for men. Even 10 years after the event, women appear not to have recovered from this gap. The DiD coefficients are reported in the lower panel as before ($k = -1$ serves as the reference category). Since effects are measured on the entire population, these estimates are extremely precise; at impact, the earnings penalty for women relative to men is approximately NOK 75,000 per year, which amounts to around 22 percent relative to the counterfactual at $k = 1$. The gap continues to increase over time, reaching NOK 120,000 at the end of the sample period. Despite applying a slightly different empirical specification, which relies on the assumption of parallel trends instead of smoothness, our estimates are almost identical to child penalties previously estimated on Norwegian data by Bütikofer et al. (2018) and Andresen and Nix (2022).[29]

In the plots to the right, we repeat the analysis using our main politicians sample (i.e., the same sample used to construct Figure 1). Local politicians earn, on average, considerably more than the general population both before and after the birth of their first child.[30] Other than that, the patterns observed here are remarkably similar; women's income drops by approximately NOK 75,000 after the birth of their first child while men's income continues to develop smoothly upward. We do observe a tendency for the gender income gap to shrink after some years. While this could indicate a different dynamic response among politicians, the increasing confidence intervals also suggest that these estimated effects are not necessarily different from those in the general population.[31]

An important distinction between each sample in Figure 2 is that – unlike in the general population – only a fraction of politicians are *partners*. In Appendix Figure A.5 we estimate child penalties using the respective partners of politicians as the comparison group. Again, we find no evidence that politicians differ from the general population in terms of the income penalty; the DiD estimates are universally equivalent regardless of whether it is the mother or father who is politically active.[32]

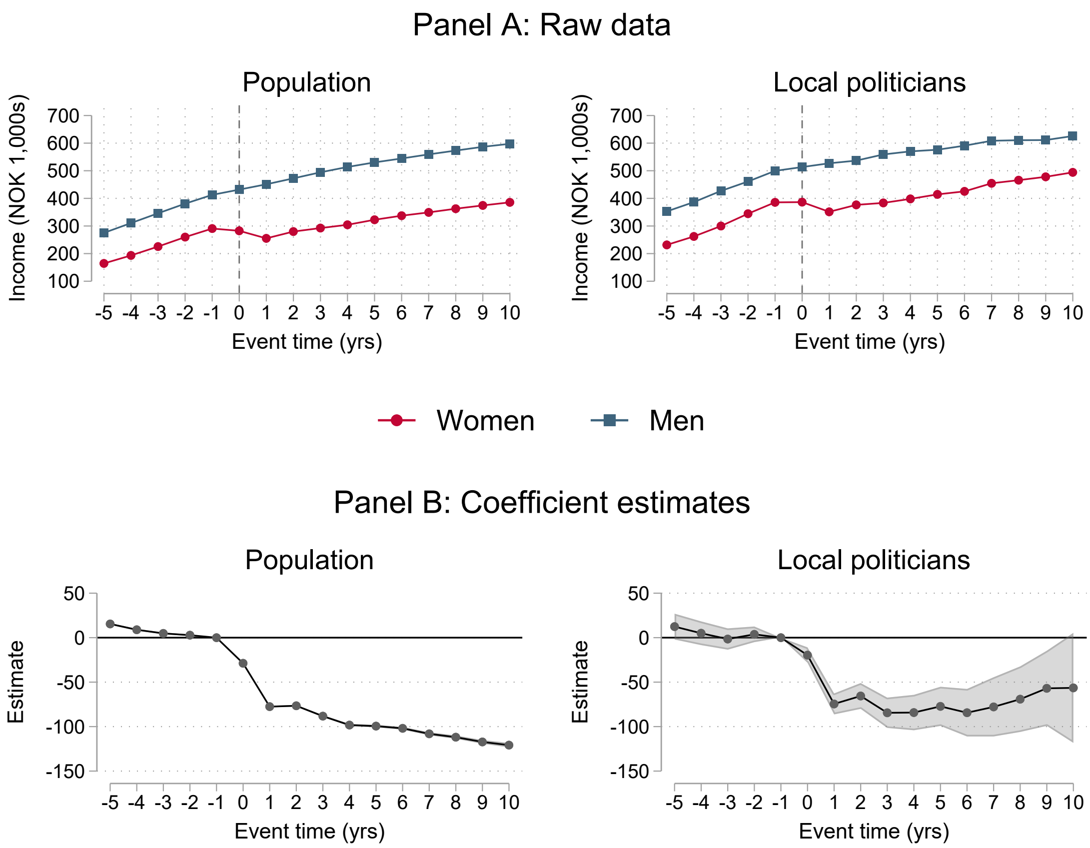
[29] There are some statistically significant effects also in the pre-treatment period. While we would ideally have pre-trends that are indistinguishable from zero, the observed differences are small relative to the treatment effects, and otherwise comparable to results in many other child penalty event designs. Andresen and Nix (2022) argue that significant pre-trends do not necessarily violate the parallel trends assumption, as long as they would have also occurred even in the absence of parenthood.

[30] This suggests that local politicians are positively selected, as in the other Scandinavian countries (Dal Bó et al., 2017; Dahlgaard and Pedersen, 2020), but may be also be partly driven by the fact that politicians in our sample are slightly older than the general population when they have their first child (see Appendix Table A.1).

[31] Numerical regression results are provided in Appendix Table A.3. We observe that long-run estimates are somewhat sensitive to the inclusion of nonparametric controls.

[32] Appendix Figure A.5 shows that the *level* of earnings is lower for women partners (of men politicians) than for women 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). These results can also be interpreted as evidence against the idea that child penalties materialize because women have a comparative advantage in childrearing. If this were the case, we would expect that women politicians (who earn more relative to their partners) are less likely to specialize in child care. This does not appear to be the case.

Figure 2: The Child Penalty in Income



Note: Results from our labor market specification run on the full population of Norwegian parents between 2003 and 2019 (left) and our baseline politicians sample (right). Panel A plots the mean income in 1000s of constant (2015) Norwegian kroner for men (blue squares) and women (red circles) at each event year k. Panel B contains estimates from the parameter-vector of interest, $\boldsymbol{\beta}$*, in Equation (1) along with 95 % confidence intervals. Event period* $k = -1$ *serves as the reference category. The population regression is run on a sample of* $10,201,115$ *observations from* $891,989$ *individuals while the politicians regression is run on the baseline politicians sample consisting of* $35,905$ *observations from* $3,370$ *individuals. As the event window for parents with first child births occurring later in the sample is truncated, precision falls toward the end in both samples.*

4.3 *Design validity*

If our coefficient estimates capture the effect of having children, then only candidates who become parents should incur these effects. To investigate the validity of our research design, we conduct a placebo check which is commonly used in the literature. The procedure involves running Monte Carlo simulations where "placebo child arrivals" are assigned randomly to candidates who never have children, before estimating the baseline model as before.

Our starting point in constructing a placebo sample is the subset of political candidates who are childless at the end of our sample period. This group includes those who never have children, but also, due to the truncation of years after 2019, those who have just not become parents yet when the sample ends in 2019. We follow Kleven et al. (2019b) in taking 40 as the critical age when individuals are assumed to have passed the opportunity for having their first child. This implies that every childless candidate born before 1980 gets allocated to the placebo sample automatically. For later cohorts, we include only those childless individuals whose background characteristics suggest they are least likely to become parents in the future.[33] We proceed by simulating our baseline analysis on the placebo sample 1000 times. At the start of each iteration, we assign to every individual a "placebo child" in a year which is drawn from a uniform distribution of within-sample years (the year of birth gets redistributed after each iteration).[34]

[33]The predictions for zero lifetime fertility are obtained from estimating a linear probability model on the more senior cohorts of politicians. Specifically, we use the universe of political candidates born between 1950 and 1979 to estimate $P_i = \boldsymbol{\beta'}\mathbf{X_i}$, where P_i is a dummy variable indicating zero fertility for individual i in 2019, 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 1979, and keep those n_c candidates with the highest $\hat{P}_i$ such that $\frac{n_c}{N_c} = P_{1950-1979}$, where N_c is the total number of politicians in cohort c and $P_{1950-1979}$ is the average probability for zero lifetime fertility among politicians born between 1950 and 1979. This entire algorithm is adapted from Kleven et al. (2019b).

[34]Ideally, we would assign child births by drawing from the empirical distribution of parents' age at the birth of their first child observed in the main sample. This ensures that placebo candidates have the same age as the people they are compared with. However, politicians are, on average, much older than the typical person of childbearing age (see Appendix Figure A.2). Using this method thus leads to assignment of births that are out-of-sample for most candidates, resulting in an estimation pool consisting of fewer than 200 individuals (versus more than 3,000 in our baseline analysis and when children are

The results are shown in Appendix Figure A.6, which plots the distributions of simulation estimates of the parameter vector of interest, $\boldsymbol{\beta}$, in Equation (1), for each local political outcome after $t = -1$ respectively. We highlight the actual estimates from Figure 1 by red lines. In every instance where we identified a gender gap earlier, these effects lie either well below or in the lower tails of the placebo distributions, supporting our identification of child penalties in the main sample. There is a tendency for the center of mass to fall below zero in some of the distributions. This suggests that women are less likely than men to be re-elected or appointed to executive positions *in general*. Overall, however, such "leaking pipeline" effects are small.

5. Mechanisms

While the empirical finding of child penalties in labor market outcomes has proven incredibly robust across both countries and time, the underlying causes and mechanisms behind these results are less understood. The general consensus in the labor literature points to either gender norms or a change in preferences surrounding parenthood which, together or separately, result in a downward shift in the labor supply of women after the arrival of their first child.[35] Similarly, we hypothesize that child penalties in politics are driven by an increase in the opportunity cost of political participation around the time women become mothers. The effect is accentuated because of the "spare-time nature" of local office holding in Norwegian politics. Appendix Figure A.7, which repeats our baseline analysis using a dummy for "running for local office" as the outcome variable, confirms that women who drop out of office are likely to leave the political arena altogether. More broadly, these findings could also reflect the impact of either voter or party demand. We explore each of these mechanisms in turn below.

uniformly assigned). Such a small sample leads to variations between iterations that are too large for the analysis to serve any meaningful purpose.

[35]Physiological consequences from giving birth (Kleven et al., 2021) or a comparative advantage in child rearing (Andresen and Nix, 2022) for women have largely been ruled out.

5.1 *Voters or parties do not discriminate against mothers*

In Norway, both voters – who cast preference votes – and party elites – who nominate candidates for the "party bonus" – affect local election outcomes. We use this institutional feature to form two hypotheses. The first is that *if* child penalties materialize due to voter discrimination, in which case, conditional on qualifications and ability, female candidates who are mothers should receive fewer personal votes than female candidates who are childless. The second hypothesis is that *if* party leaders disfavor mothers over other female politicians, making mothers less likely to receive the party bonus, conditional on the same set of characteristics.

To test these hypotheses, we exploit the fact that our data contain the number of personal votes received by each candidate, as well as their party bonus status. Using the universe of women who ran for office in the 2007-2019 Norwegian local elections (constructing the incumbent variable implies we lose observations from 2003), we fit a linear regression model where the dependent variable is either the personal votes share[36] for each candidate-year observation or an indicator for whether or not the person was given a party bonus. We estimate the effect of parenthood using three dummy variables capturing the school age of the mother's youngest child (preschool, elementary-middle school, adult) in the election year. We include incumbency status, and control non-parametrically for an increasing range of observables like year-specific income quartile (average constant (2015) income over the last four years) and eight education-level dummies. In the first model, we also control non-parametrically for rank position on the party ballot and receiving the party bonus.[37] While we should be careful about giving our results a causal interpretation, the inclusion of these variables, along with incumbency status, imply that we are comparing women who are equally experienced, and who have already been evaluated by party leaders to be of comparable merit.

[36]We define the personal votes share 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.

[37]Controlling for list rank in the second model makes little sense as these outcomes are jointly determined.

Table 2: Relationship Between Political Outcomes and Motherhood

	Personal votes share (%)				Received party bonus (%)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No children	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
Youngest child 0-6	0.71***	0.65***	0.53***	0.56***	-1.69***	-1.30***	-0.71	-0.64
	(0.262)	(0.160)	(0.154)	(0.154)	(0.482)	(0.444)	(0.439)	(0.437)
Youngest child 7-16	0.92***	0.78***	0.61***	0.72***	-0.72	-0.97**	-0.41	-0.20
	(0.268)	(0.158)	(0.152)	(0.153)	(0.516)	(0.457)	(0.456)	(0.449)
Youngest child 17+	0.30	0.50***	0.31**	0.51***	-0.30	-0.85*	-0.34	0.03
	(0.258)	(0.156)	(0.149)	(0.151)	(0.522)	(0.451)	(0.446)	(0.444)
Incumbent		1.29***	2.00***	1.75***		39.12***	39.59***	38.39***
		(0.195)	(0.183)	(0.179)		(0.617)	(0.620)	(0.625)
Party bonus		-1.50***	-0.71**	-0.73**				
		(0.309)	(0.297)	(0.296)				
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Rank FE	No	Yes	Yes	Yes	No	No	No	No
Party FE	No	No	Yes	Yes	No	No	Yes	Yes
Municipality FE	No	No	Yes	Yes	No	No	Yes	Yes
Earnings Qt./Year FE	No	No	No	Yes	No	No	No	Yes
Education FE	No	No	No	Yes	No	No	No	Yes
Observations	91,255	91,255	91,209	90,963	95,417	95,417	95,365	95,104
Clusters	701	701	701	701	701	701	701	701
R-squared	0.02	0.53	0.57	0.73	0.01	0.14	0.16	0.26

*Notes: Each column is a separate regression of personal votes share (columns 1-4) or party bonus (columns 5-8) on a set of dummy variables and nonparametric controls, as indicated. Both dependent variables are in percent (0-100). Women who are childless at the time of the election serve as the reference category. The sample is every female candidate running in a Norwegian local election between 2007 and 2019. Standard errors are clustered at the municipality level and reported in parentheses. * denotes 10% statistical significance, ** 5% and *** 1%.*

The results are shown in Table 2. Columns (1)-(4) contain estimates for the first model where personal votes share serves as the dependent variable. In all four specifications, we find that mothers receive a 0.5 to 0.9 percentage points *higher* share of personal votes relative to the no-child reference group. It does not seem to matter whether the mother has children in the (often more time-consuming) preschool age or whether her children are older at the time of the election.[38] Columns (5)-(8) contain results from the second specification where we investigate whether parenthood makes women less likely to receive the party bonus. Given the patterns observed in columns (1)-(4) of Table 2, this does not seem like a vote-maximizing strategy. Indeed, there is no clear evidence that party leaders discriminate against mothers – or mothers with young children in particular.[39]

Our analysis reveals that demand channels are not the likely mechanisms through which child penalties arise in politics. While causality is not explicit, if anything, our findings suggest that mothers are *preferred* over non-mothers as politicians. This is consistent with experimental evidence from Teele et al. (2018), who document a similar "motherhood favoritism" among American voters.[40] Next, we proceed by investigating whether our main results can be attributed to factors influencing the mothers' own supply of political candidacies.

5.2 *Gender norms appear to be important*

Several studies have documented a relationship between parents' attitudes to gender roles and the labor force participation of their sons or daughters.[41] Connecting this discussion

[38]Party bonus is estimated to have a negative effect on the share of personal votes received. This likely occurs because candidates who are isolated from competition spend less time campaigning (Fiva et al., 2022). Importantly, our measure of personal votes does not include the additional 25 % advantage votes brought on by the bonus.

[39]The parameter estimates suggests that women with young children are somewhat less likely to get the party bonus. However, the effects are many orders of magnitude smaller than the effect of being an incumbent, and they are not statistically significant at conventional levels in the most demanding specifications (columns (7) and (8)).

[40]Without a credible proxy for unobserved ability, the results in columns (5)-(8) of Table 2 are more descriptive. An alternative explanation, which is entirely plausible given these results, is that women who have children are more likely to receive the party bonus precisely because it is only the best mothers who overcome the bias of their party colleagues or voters. Controlling for incumbency status and other observables mitigate this concern somewhat, but not completely.

[41]See, e.g, Fernández et al. (2004); Farré and Vella (2013).

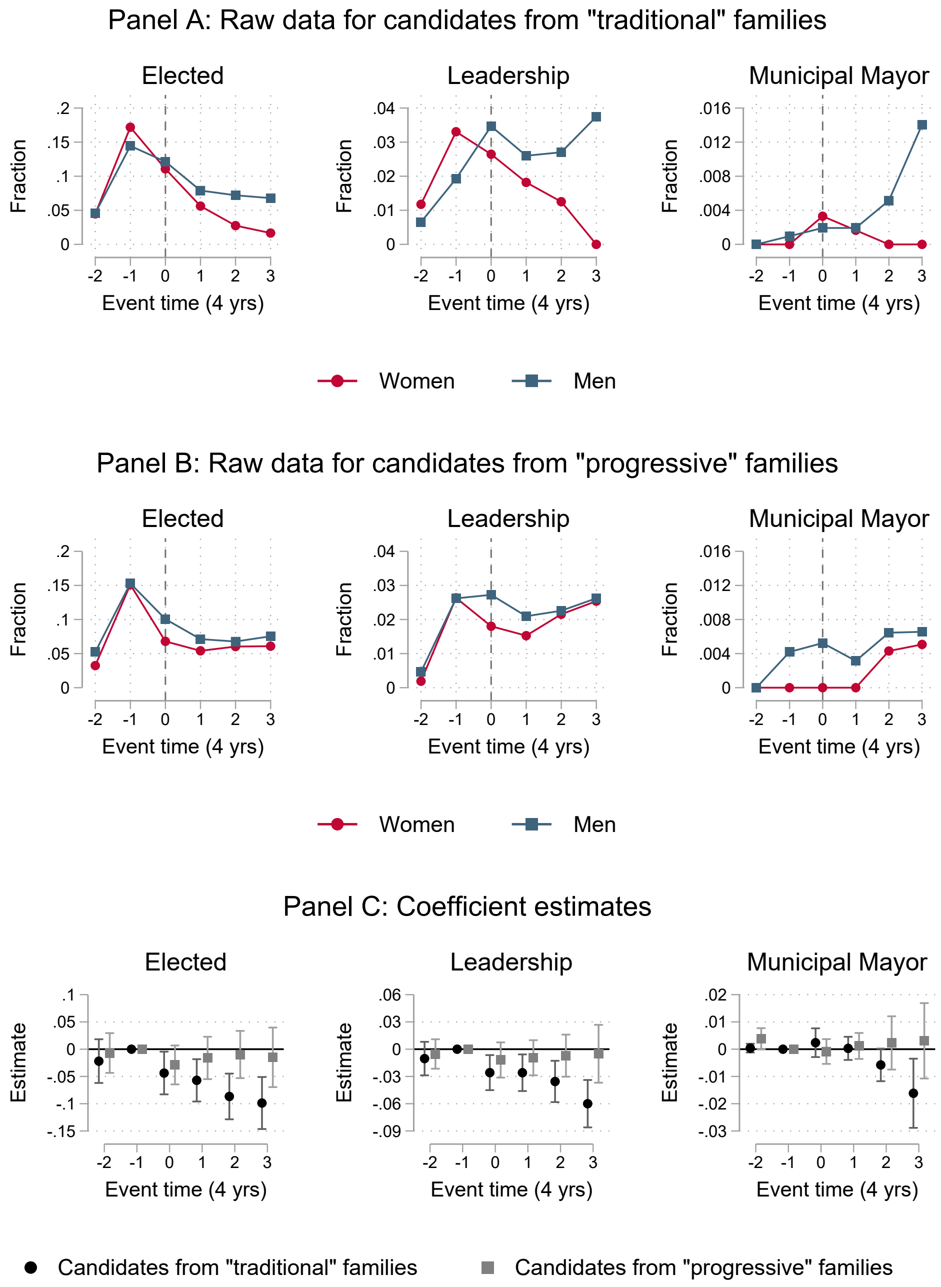
to the formation of child penalties in labor markets, Kleven et al. (2019b) find evidence of intergenerational links between mothers' earnings penalty and the labor supply of the children's maternal grandparents, suggesting that their effects are mainly driven by the influence of family-career preferences inherited from earlier generations. Similarly, if gender norms are important drivers of child penalties in the political arena, then candidates who were exposed to a more stereotypical gender pattern during childhood should be more likely to withdraw from politics than others. We test this hypothesis by considering the division of labor within families during the time when our political candidates grew up.

Our analysis takes advantage of the fact that our income data go 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 labor 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.8 shows that this index has a trimodal distribution with 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 labor 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 3.[42] Here, the top two panels show outcomes for male (blue squares) and female (red circles) candidates, separately for those who grew up in a household with an above-median (Panel A) and below-median (Panel B) division of labor. We observe that there are striking dynamic effects; after $t = 0$, female candidates from "progressive" families seem to bounce back to the trajectory of their male colleagues. This is not the case among female candidates from "traditional" families, whose trend continues to fall over time. At $t = 3$, only around

[42] Appendix Table A.4 contains the numerical results.

Figure 3: The Child Penalty in Local Political Outcomes, Split by Grandparents' Division of Labor



Note: Results from our baseline specification conditional on candidates running at $t = -1$. The top two panels plot the fraction of men (blue squares) and women (red circles) for whom the outcome variable is equal to one at each election period t, separately for candidates growing up in households with a below-median division of labor (Panel A) and an above-median division of labor (Panel B). Panel C contains estimates from the parameter-vector of interest, $\boldsymbol{\beta}$, in Equation (1) along with 95 % confidence intervals for both subsamples. Regressions in the first group are run on an (unbalanced) subsample of $7,748$ observations from $1,643$ individuals while regressions in the second group are run on an (unbalanced) subsample of $7,784$ observations from $1,675$ individuals. A few observations from the baseline analysis who were not successfully matched with grandparents are omitted.

two percent of women in this group are elected into office and, in particular, not a single candidate is appointed to the executive board (or becomes mayor). As in Figure 1, Panel C contains the coefficient estimates of our parameter-vector of interest, this time split to capture the differences between men and women within each sub-sample. In all three specifications, the estimated child penalties are larger among the "traditional" candidates than the "progressive" candidates. Moreover, the confidence intervals for the first group typically do not overlap with zero, whereas this is always the case for the opposite group.

Our results are consistent with those of Kleven et al. (2019b), suggesting that intergenerational linkages of child penalties also exist in the political arena. Indeed, the effects in Figure 1 seem to be almost entirely driven by candidates who grew up in households with a below-median division of labor. This is in line with what we would expect to find if gender norms were important. What remains ambiguous, however, is the normative implication of this result. As discussed by Andresen and Nix (2022), it is impossible to distinguish between a gender norms effect which is "enforced" through generations and a natural intergenerational correlation in preferences which is due to a biological similarity between parent and child.

5.3 *Child penalties can be reduced*

What can be done to mitigate child penalties in politics? As we have discussed, political work shares many of the features of the greedy jobs highlighted by Goldin (2014, 2021). In particular, Norwegian office-holding parents have to balance an additional obligation on top of the standard work-life commitments faced by regular families. The fact that local council meetings are often held in the evenings has caused significant concern among some women, for example, a local council member in *Trondheim municipality* (third-largest city in Norway) wrote 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 day time".[43]

We collected original data for local council meetings held in municipalities at the end

[43]https://www.midtnorskdebatt.no/meninger/ordetfritt/2016/05/01/Det-er-for-vanskelig-å-kombinere-politisk-arbeid-med-familieliv-12677920.ece

of our sample period.[44] These data cover 392 out of the 422 municipalities existing in 2018 (i.e., over 90 percent).[45] Of these, 280 held their local council meetings in the evenings, or in a combination of daytime and evenings. 102 municipalities, however, consistently held all their meetings during standard business hours. We classify the latter category as "family-friendly" municipalities.

To shed some light on the effectiveness of prospective policies aimed at reducing the child penalty, we conduct two types of analyses. First, we investigate whether family-friendly municipalities have more women with children elected to office. Figure 4 plots the fraction of women in local councils by age groups ($18-40$ and $41+$), parent 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 clear differences for the three other categories. One interpretation of this result is that women with small children (typically found in the $18-40$ category) are more willing to pursue a political career in family-friendly settings. Alternatively, it could be that women with small children push for family-friendly meeting schedules.[46]

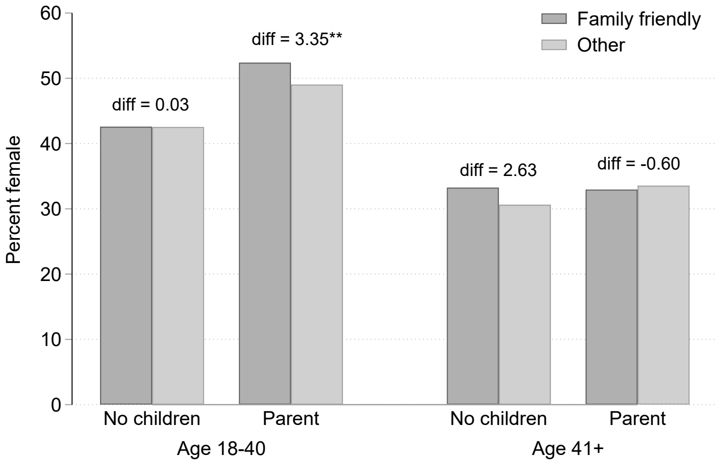
Second, we study whether political child penalties are lower in family-friendly municipalities. Similar to the procedure used in section 5.2, our main sample is split in two based on the type of municipality each candidate ran in at $t-1$. We then estimate child penalties separately for the two sub-samples, this time considering only the elected outcome (i.e., appointment to the local council). The results are shown in Figure 5. While overlapping confidence intervals make it hard to draw firm conclusions, there does appear to be a relationship between our baseline effects and the meeting schedules of the

[44] Our data cover 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.

[45] Over the 2017–2020 period, a municipality amalgamation reform reduced the number of municipalities from 428 to 356. We omit from our analysis council members serving after the reform, i.e. those elected in 2019.

[46] Appendix Figure A.9 shows a clear depression in the age distribution of women politicians between the ages of mid 20s and late 40s which is less prevalent among women in family-friendly municipalities. For men, the differences between groups are hardly noticeable.

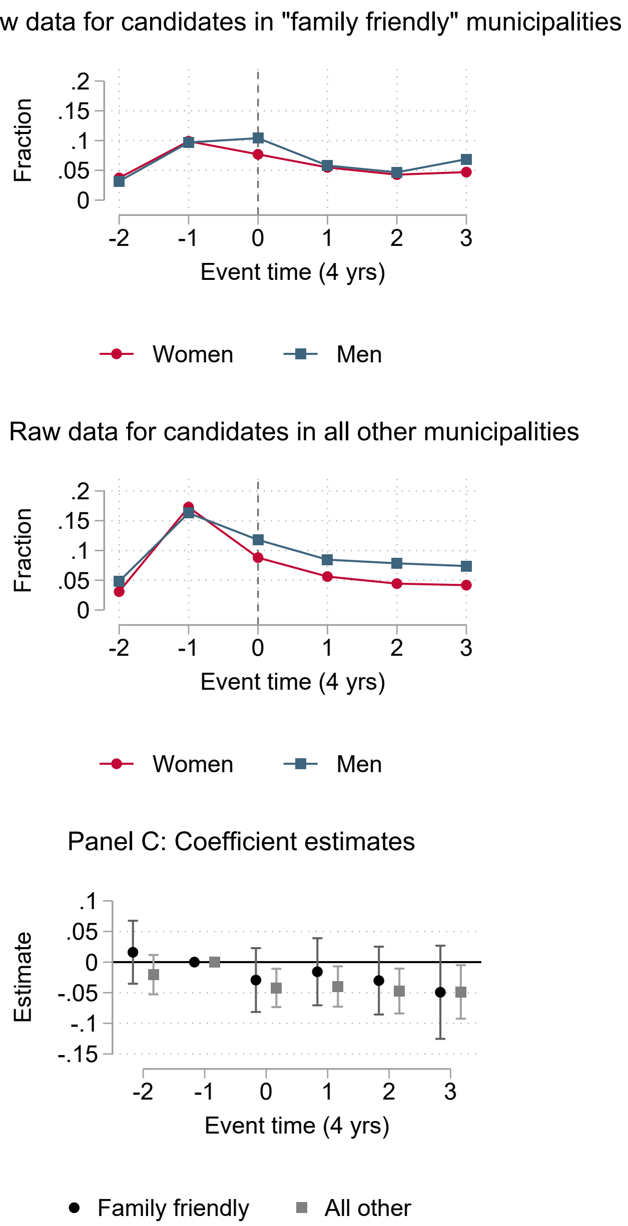
Figure 4: Share of Women Among Elected Candidates, by Parent Status, Age and Municipality Type



*Note: This figure shows the share of female councilors by parent status, age and municipality type. The sample is restricted to candidates elected in the 2003, 2007, 2011, and 2015 elections. 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, along with the level of statistical significance when standard errors are clustered at the municipality level. * denotes 10% statistical significance, ** 5% and *** 1%.*

municipal councils. Taken together, our findings suggest that more family-friendly work environments might reduce women's underrepresentation in politics.

Figure 5: The Child Penalty in Local Political Outcomes, Split by Municipality Type



Note: Results from our baseline specification conditional on candidates running at $t = -1$*. The top two panels plot the fraction of men (blue squares) and women (red circles) for whom the outcome variable is equal to one at each election period* k*, separately for candidates who ran for office (at* $t-1$*) in a municipality where local council meetings are held exclusively during standard business hours (Panel A), and all other municipalities (Panel B). Panel C contains estimates from the parameter-vector of interest,* $\boldsymbol{\beta}$*, in Equation (1) along with 95 % confidence intervals for both subsamples. Regressions in the first group are run on an (unbalanced) subsample of* $3,202$ *observations from* 679 *individuals while regressions in the second group are run on an (unbalanced) subsample of* $11,360$ *observations from* $2,360$ *individuals. Some observations from the baseline analysis were omitted due to missing meetings data.*

6. Conclusion

The underrepresentation of women in political leadership remains an important global issue. According to the United Nations, women constitute 36 percent of elected members in local deliberative bodies and only 25 percent of national-level MPs (UN Women, 2020). In Norway, women make up around 40 percent 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 had gender quotas in place since the 1970's.

In this paper, we have used insights from the labor literature to investigate whether becoming a parent can explain why women remain underrepresented in politics. In sum, we find that child penalties affect political participation in much the same way as in the labor market; among politicians who ran for office one election period before having their first child, the probability of winning a seat in the first election after having a child falls twice as much for women relative to men. At each of the following periods, the size of the gender gap is about one third of men's chance of electoral success. In comparison, we know that the overall gender gap in local councils is about 39 percent. This suggests that parenthood is a key factor explaining the underrepresentation of women in Norwegian politics.

We also find that women are less likely to obtain leadership positions than men after having children. We find no evidence that these effects arise due to demand-side preferences, i.e., either coming from voters or from within the party itself. Instead, there is evidence that mothers who retire from the political arena do so either due to preferences or gender norms which are inherited across generations, supporting the hypothesis that political child penalties are primarily a supply-driven phenomenon.

Our results have several key implications. First, we learn that parenthood can be a crucial barrier to the political careers of women, even if they are already inside the political sphere. These findings broaden our understanding of the "leaking 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, our results reflect the limits of gender quotas; despite near-perfect compliance with policies designed to propel women into political office, these measures are not able to prevent a higher *outflow* of women. Third, the fact that the overall share of women candidates never declined during our sample period implies that political parties counteract female attrition by continually recruiting more women than men. While it goes beyond the scope of this paper to discuss the normative value of candidates' political experience, we know that seniority matters for allocation of leadership positions and nominations for higher offices (e.g. McKelvey and Riezman, 1992; Kellermann and Shepsle, 2009; Cirone et al., 2021). This implies that even if we achieve gender equality across electoral lists, this is unlikely to result in gender balance in political representation so long as child penalties exist. Lastly, while generalizing our results to other countries is not straightforward, the liberal gender laws and generous welfare programs for child care and parental leave in Scandinavia suggests that political child penalties elsewhere are likely to be even larger.

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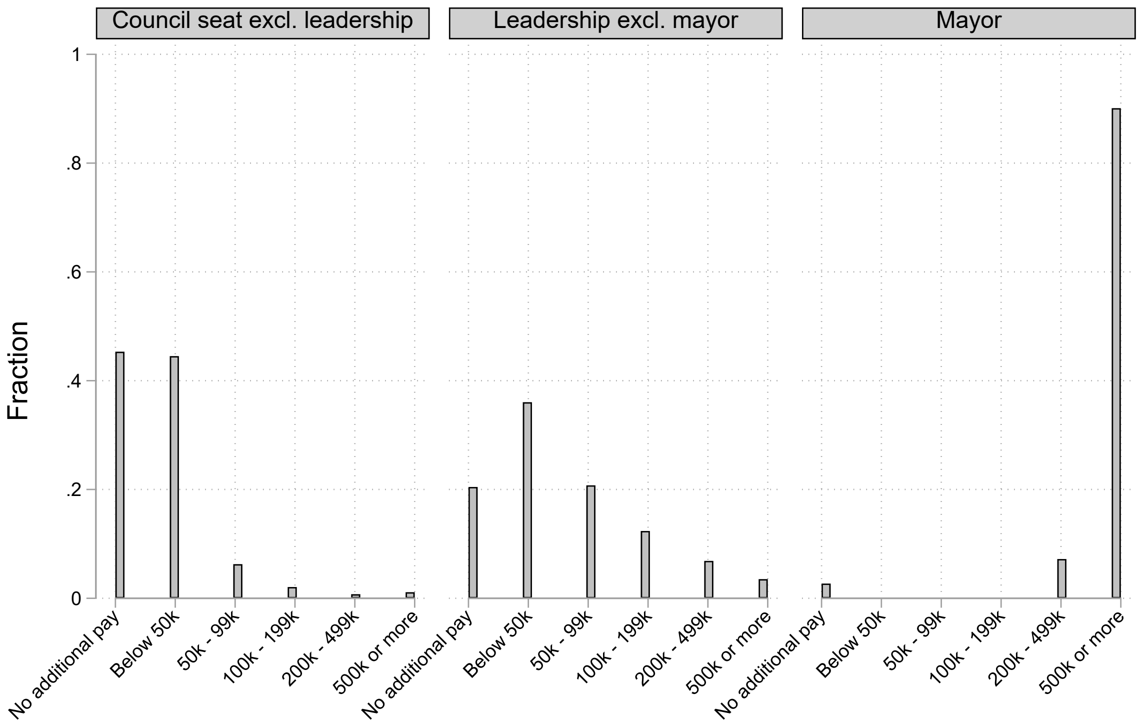
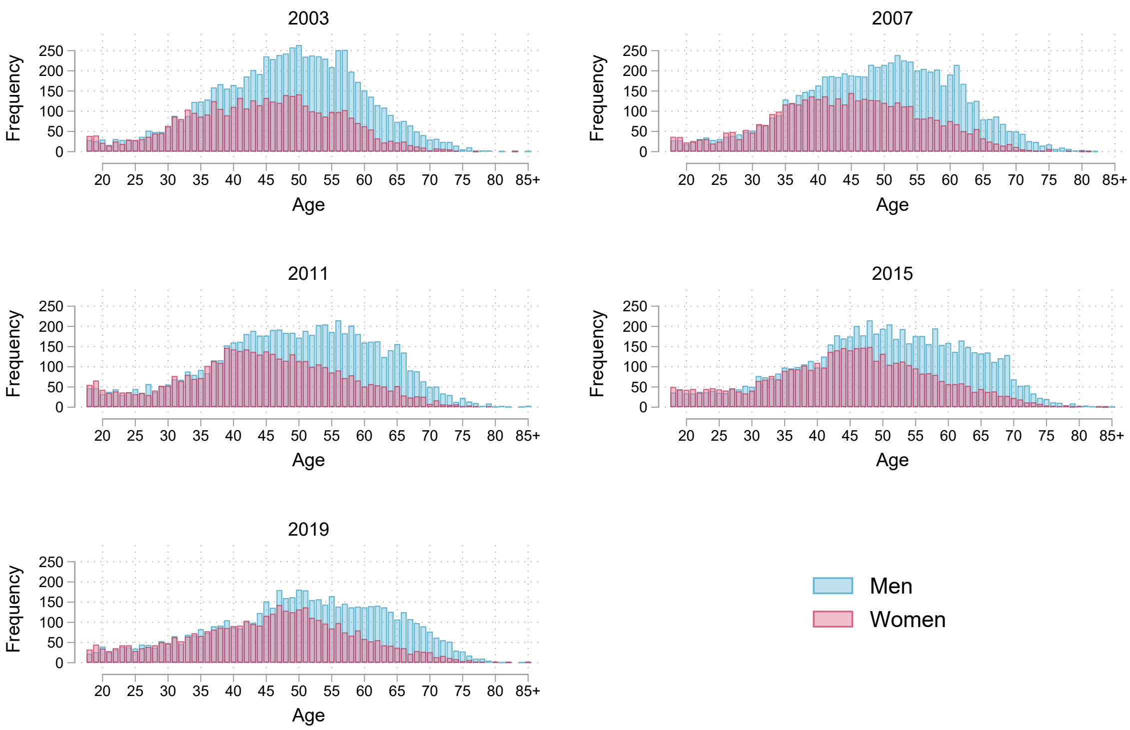
Appendix A: Supplementary figures and tables

Figure A.1: Additional Compensation for Local Political Activities



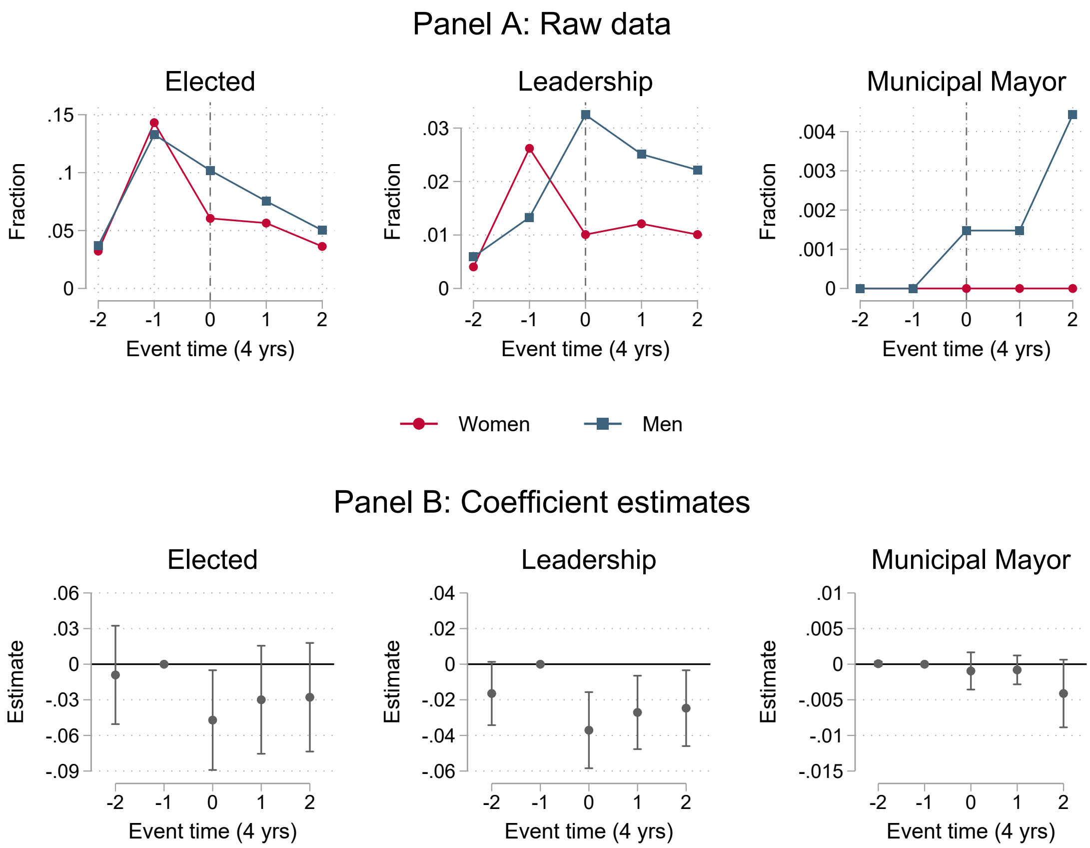
Note: The figure plots survey responses collected from local political office holders in 2011 (N=2,234). The survey question asked (translated from Norwegian): "Remuneration from the municipal government for performing political duties (besides standard meeting compensation)"

Figure A.2: Histogram Showing the Number of Elected Individuals by Gender and Age



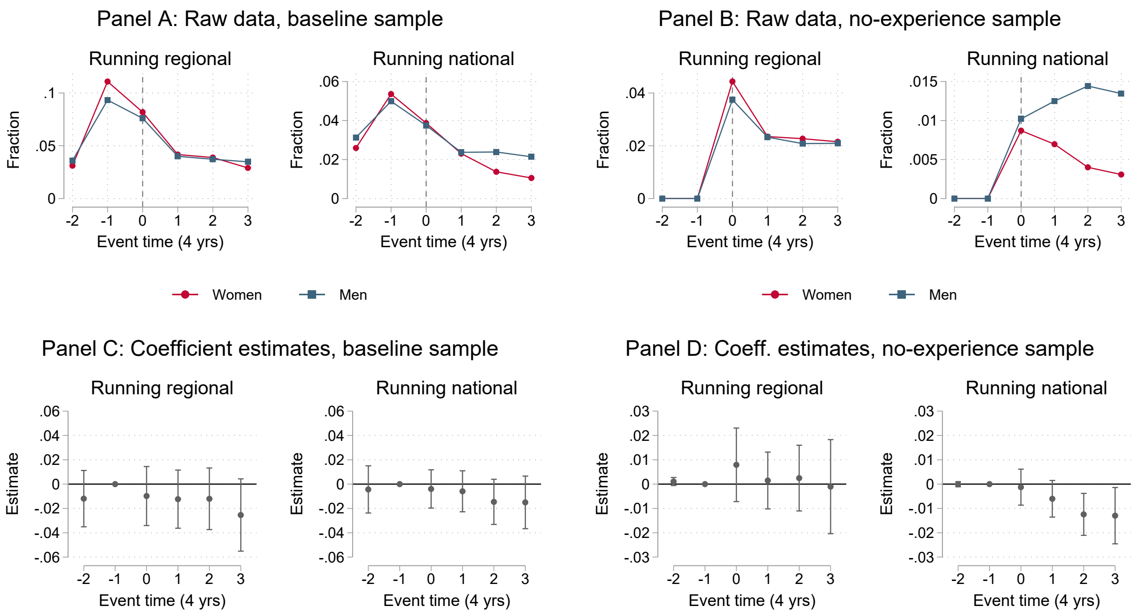
Note: This figure shows the number of elected individuals by gender and age, separately for each election year 2003–2019. In each election year, about 10,000 candidates are elected to the local council. The number is slightly lower in the 2019 election because of the municipal merger reform that reduced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

Figure A.3: The Child Penalty in Local Political Outcomes, Fully-balanced Sample



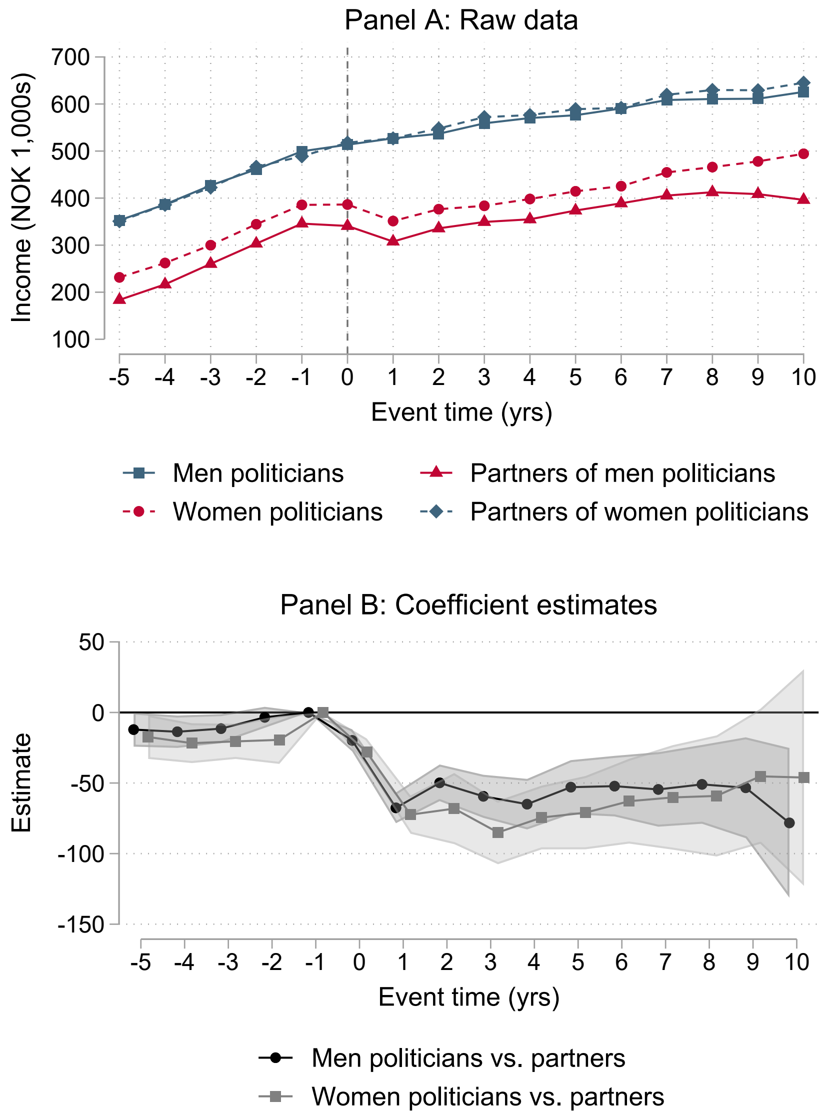
Note: Results from our baseline specification conditional on candidates running at $t = -1$*. Panel A plots the fraction of men (blue squares) and women (red circles) for whom the outcome variable is equal to one at each election period* k*. Panel B contains estimates from the parameter-vector of interest,* $\boldsymbol{\beta}$*, in Equation (1) along with 95 % confidence intervals. Regressions are run on a fully-balanced sample of* 5,865 *observations from* 1,173 *individuals.*

Figure A.4: The Child Penalty on Higher-level Political Outcomes



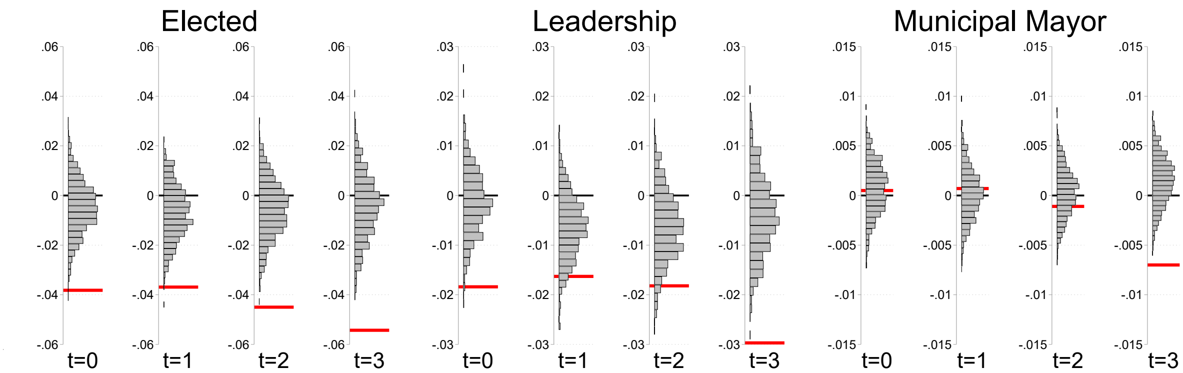
Note: Results from our baseline specification conditional on candidates running for local office at $t = -1$ *(Panels A and C), and conditional on candidates running for local office at* $t = -1$*, but not for regional or national office at* $t = [-1, -2]$ *(Panel B and D). Panels A and B plots the fraction of political candidates for whom the outcome variable is equal to one at each period. Panels C and D contains estimates from the parameter-vector of interest,* $\boldsymbol{\beta}$*, in Equation (1) along with 95 % confidence intervals. Regressions in Panel C are run on the (unbalanced) baseline politicians sample of* $15,779$ *observations from* $3,370$ *individuals, while regressions in Panel D are run on the (unbalanced) modified baseline politicians sample of* $13,641$ *observations from* $2,911$ *individuals.*

Figure A.5: The Child Penalty in Income for Politicians and Their Partners



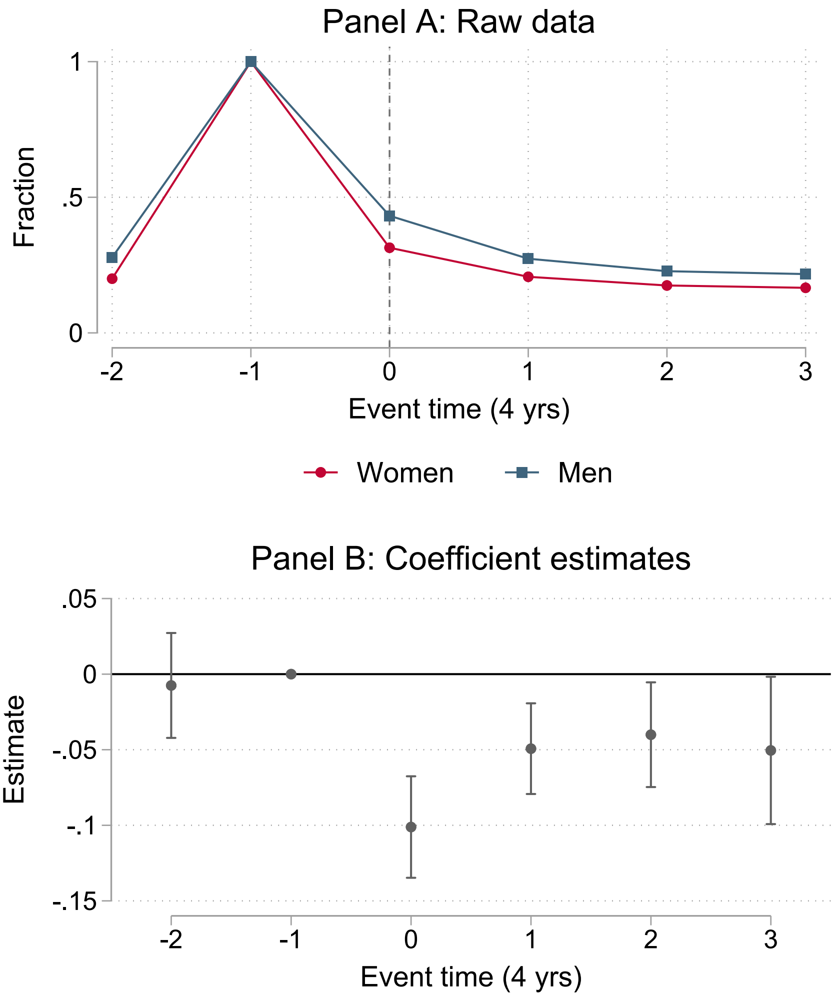
Note: Results from our labor market specification run on separate samples of baseline men politicians and their partners (solid lines), and baseline women politicians and their partners (dashed lines). Panel A plots the mean income in 1000s of constant (2015) Norwegian kroner for men (blue squares/diamonds) and women (red circles/triangles) at each event year k. Panel B contains estimates from the parameter-vector of interest, $\boldsymbol{\beta}$*, in Equation (1) along with 95 % confidence intervals. Event period* $k = -1$ *serves as the reference category. The men politician-partner sample consists of* $40,572$ *observations from* $2,026$ *individuals while the women politician-partner sample consists of* $25,440$ *observations from* $1,344$ *individuals.*

Figure A.6: Simulation Results of Placebo Effects



Note: Results from our placebo simulations where child births are assigned to the placebo sample using a uniform distribution of within-sample years. Each subplot shows the distribution of estimates from the parameter-vector of interest, $\boldsymbol{\beta}$, in Equation (1), at each election period after $t = -1$, after 1000 repeated iterations. The red lines show the actual estimates from Figure 1. Regressions are run on a pooled sample of, on average, $13,628$ observations from $3,383$ individuals (the number of observations vary slightly in each iteration depending on the random assignment of child births).

Figure A.7: The Child Penalty on the Probability to Run for Local office

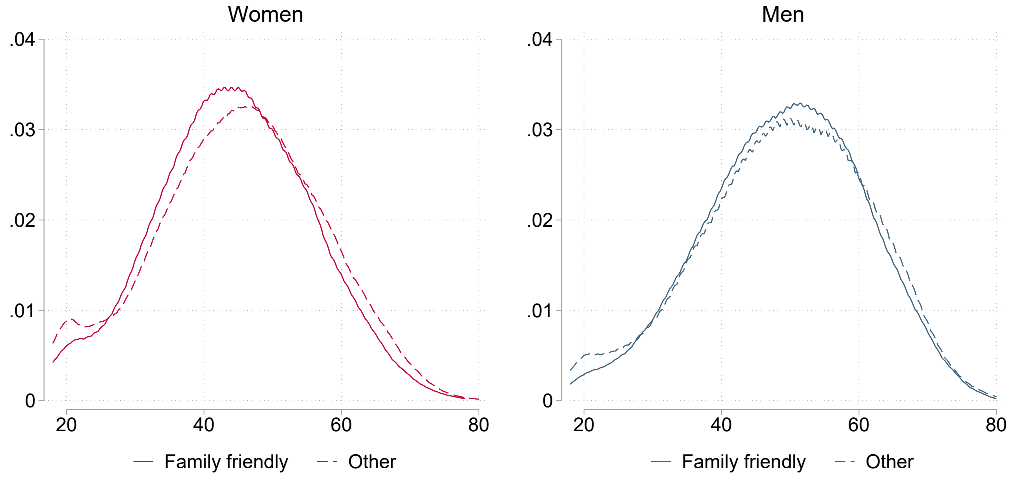


Note: Results from our baseline specification conditional on candidates running at $t = -1$. Panel A plots the fraction of men (blue squares) and women (red circles) who ran for local office at each election period t. Panel B contains estimates from the parameter-vector of interest, $\boldsymbol{\beta}$, in Equation (1) along with 95 % confidence intervals. Regressions are run on the (unbalanced) baseline politicians sample of $15,779$ observations from $3,370$ individuals.

Figure A.8: Distribution of Grandparents' Labor Division Index

Note: Histogram of the index I, which captures the grandmother's share of household income when the politician was 0-18 years old. The red line denotes the median of the distribution.

Figure A.9: Kernel Density Plots of Elected Candidates' Ages



Note: The left panel (right panel) shows kernel density plots for the age of female (male) council members separately by municipality type. The sample is restricted to candidates elected in the 2003, 2007, 2011, and 2015 elections. Municipalities are classified as "family friendly" if all local council meetings in 2018 were held during standard business hours.

Table A.1: Summary Statistics for Background Variables among Norwegian Parents

Panel A: Population	Full Sample		Men		Women	
	Mean	SD	Mean	SD	Mean	SD
Event-time $k = -1$:						
Age	28.74	5.61	30.05	5.76	27.46	5.16
Income, constant(2015) NOK 1,000s	350.72	278.61	412.71	314.83	290.74	222.52
Finished high school	0.72	0.45	0.71	0.45	0.72	0.45
Higher education	0.42	0.49	0.35	0.48	0.48	0.50
Number of children (as of 2019)	1.85	0.79	1.84	0.79	1.85	0.78
N	840,155		413,124		427,031	

Panel B: Politicians	Full Sample		Men		Women	
	Mean	SD	Mean	SD	Mean	SD
Event-time $k = -1$:						
Age	30.67	5.65	32.00	6.06	28.67	4.25
Income, constant(2015) NOK 1,000s	454.09	242.56	499.52	263.01	385.62	188.37
Finished high school	0.90	0.30	0.88	0.32	0.93	0.25
Higher education	0.56	0.50	0.46	0.50	0.73	0.45
Number of children (as of 2019)	1.76	0.74	1.78	0.77	1.73	0.69
N	3,370		2,026		1,344	

Notes: Summary statistics from a cross-sectional sample of the full population of Norwegian parents (Panel A) and our baseline politicians sample (Panel B). Each parent enters only once, and is observed one year before the birth of their first child (event-time $k = -1$).

Table A.2: Baseline Results, Political outcomes

		Elected		Leadership		Municipal Mayor	
		(1)	(2)	(3)	(4)	(5)	(6)
Female × $t = -2$	$[n = 2,244]$	-0.024*	-0.016	-0.006	-0.009	0.002**	0.002*
		(0.013)	(0.014)	(0.006)	(0.006)	(0.001)	(0.001)
Female × $t = -1$	$[n = 3,370]$	ref.	ref.	ref.	ref.	ref.	ref.
Female × $t = 0$	$[n = 3,370]$	-0.037***	-0.038***	-0.016**	-0.018***	0.001	0.000
		(0.013)	(0.013)	(0.007)	(0.007)	(0.002)	(0.002)
Female × $t = 1$	$[n = 3,370]$	-0.033**	-0.037***	-0.013*	-0.016**	0.001	0.001
		(0.013)	(0.014)	(0.007)	(0.007)	(0.002)	(0.002)
Female × $t = 2$	$[n = 2,299]$	-0.036**	-0.045***	-0.013*	-0.018**	-0.001	-0.001
		(0.015)	(0.015)	(0.008)	(0.008)	(0.003)	(0.003)
Female × $t = 3$	$[n = 1,126]$	-0.039**	-0.054***	-0.023**	-0.030***	-0.006	-0.007
		(0.018)	(0.019)	(0.011)	(0.011)	(0.005)	(0.005)
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		15,779	15,779	15,779	15,779	15,779	15,779
Clusters		3,370	3,370	3,370	3,370	3,370	3,370
R-squared		0.02	0.04	0.00	0.01	0.00	0.01

*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. Main effects are not reported. Standard errors are clustered at the individual level and reported in parentheses. The number of individuals (clusters) observed at each event time period are reported in brackets. * denotes 10% statistical significance, ** 5% and *** 1%.*

Table A.3: Baseline Results, Income

		Population		Local Politicians	
		(1)	(2)	(3)	(4)
Female × $k=-5$	$[n_{(1),(2)} = 625,784,\ n_{(3),(4)} = 3,370]$	11.31***	15.47***	-7.39	12.45*
		(0.56)	(0.54)	(7.48)	(7.42)
Female × $k=-4$	$[n_{(1),(2)} = 679,956,\ n_{(3),(4)} = 3,370]$	4.85***	8.91***	-10.62	4.96
		(0.54)	(0.52)	(6.86)	(6.82)
Female × $k=-3$	$[n_{(1),(2)} = 734,480,\ n_{(3),(4)} = 3,370]$	1.58***	4.82***	-13.18**	-1.53
		(0.53)	(0.52)	(6.16)	(6.12)
Female × $k=-2$	$[n_{(1),(2)} = 787,128,\ n_{(3),(4)} = 3,370]$	0.95**	2.82***	-2.82	3.76
		(0.41)	(0.41)	(4.51)	(4.49)
Female × $k=-1$	$[n_{(1),(2)} = 840,155,\ n_{(3),(4)} = 3,370]$	ref.	ref.	ref.	ref.
Female × $k=0$	$[n_{(1),(2)} = 848,430,\ n_{(3),(4)} = 3,370]$	-27.31***	-28.78***	-13.16***	-19.61***
		(0.35)	(0.34)	(4.48)	(4.44)
Female × $k=1$	$[n_{(1),(2)} = 802,211,\ n_{(3),(4)} = 3,144]$	-73.86***	-77.51***	-61.90***	-74.52***
		(0.45)	(0.44)	(6.05)	(6.01)
Female × $k=2$	$[n_{(1),(2)} = 753,763,\ n_{(3),(4)} = 2,888]$	-70.66***	-76.56***	-46.39***	-65.55***
		(0.52)	(0.51)	(7.44)	(7.43)
Female × $k=3$	$[n_{(1),(2)} = 702,243,\ n_{(3),(4)} = 2,587]$	-79.98***	-88.22***	-61.51***	-84.46***
		(0.59)	(0.58)	(8.69)	(8.71)
Female × $k=4$	$[n_{(1),(2)} = 650,058,\ n_{(3),(4)} = 2,299]$	-87.43***	-98.24***	-58.16***	-84.20***
		(0.68)	(0.67)	(10.09)	(10.17)
Female × $k=5$	$[n_{(1),(2)} = 650,058,\ n_{(3),(4)} = 2,052]$	-85.77***	-99.44***	-47.86***	-77.23***
		(0.75)	(0.74)	(11.06)	(11.24)
Female × $k=6$	$[n_{(1),(2)} = 597,960,\ n_{(3),(4)} = 1,740]$	-85.18***	-101.91***	-51.46***	-84.37***
		(0.80)	(0.79)	(13.31)	(13.64)
Female × $k=7$	$[n_{(1),(2)} = 545,701,\ n_{(3),(4)} = 1,470]$	-88.05***	-108.09***	-40.06**	-77.81***
		(0.90)	(0.89)	(16.53)	(16.99)
Female × $k=8$	$[n_{(1),(2)} = 491,922,\ n_{(3),(4)} = 1,126]$	-88.45***	-111.88***	-30.85*	-69.14***
		(0.99)	(0.98)	(18.44)	(18.81)
Female × $k=9$	$[n_{(1),(2)} = 437,483,\ n_{(3),(4)} = 884]$	-90.19***	-117.21***	-19.21	-56.93***
		(1.09)	(1.08)	(21.24)	(21.55)
Female × $k=10$	$[n_{(1),(2)} = 380,640,\ n_{(3),(4)} = 575]$	-90.05***	-120.79***	-17.64	-56.45*
		(1.26)	(1.26)	(31.21)	(31.57)
Age FE		No	Yes	No	Yes
Year FE		No	Yes	No	Yes
Observations		10,201,115	10,201,115	35,905	35,905
Clusters		891,989	891,989	3,370	3,370
R-squared		0.13	0.22	0.15	0.22

*Notes: Each column represents a separate regression of Equation (1), where the dependent variable is income in 1,000s of constant (2015) Norwegian kroner. Regressions in columns (1)-(2) are run on the entire population of Norwegian first-time parents between 2003-2019, while columns (3)-(4) are run on the baseline politicians sample. Main effects are not reported. Standard errors are clustered at the individual level and reported in parentheses. The number of individuals (clusters) observed at each event time period are reported in brackets. * denotes 10% statistical significance, ** 5% and *** 1%.*

Table A.4: Split-sample regressions, Political outcomes

		Elected		Leadership		Municipal Mayor	
		(1)	(2)	(3)	(4)	(5)	(6)
		< p50	≥ p50	< p50	≥ p50	< p50	≥ p50
Female × $t = -2$	$[n_{<p50} = 1,036,\ n_{\geq p50} = 1,173]$	-0.022	-0.007	-0.010	-0.005	0.000	0.004*
		(0.020)	(0.019)	(0.009)	(0.008)	(0.001)	(0.002)
Female × $t = -1$	$[n_{<p50} = 1,643,\ n_{\geq p50} = 1,675]$	ref.	ref.	ref.	ref.	ref.	ref.
Female × $t = 0$	$[n_{<p50} = 1,643,\ n_{\geq p50} = 1,675]$	-0.044**	-0.029	-0.026***	-0.012	0.002	-0.001
		(0.020)	(0.018)	(0.010)	(0.010)	(0.003)	(0.002)
Female × $t = 1$	$[n_{<p50} = 1,643,\ n_{\geq p50} = 1,675]$	-0.057***	-0.016	-0.026**	-0.009	0.000	0.001
		(0.020)	(0.020)	(0.010)	(0.010)	(0.002)	(0.002)
Female × $t = 2$	$[n_{<p50} = 1,176,\ n_{\geq p50} = 1,084]$	-0.087***	-0.010	-0.036***	-0.007	-0.006*	0.002
		(0.021)	(0.022)	(0.012)	(0.012)	(0.003)	(0.005)
Female × $t = 3$	$[n_{<p50} = 607,\ n_{\geq p50} = 502]$	-0.099***	-0.015	-0.060***	-0.005	-0.016**	0.003
		(0.024)	(0.028)	(0.013)	(0.016)	(0.006)	(0.007)
Party FE		Yes	Yes	Yes	Yes	Yes	Yes
Age FE		Yes	Yes	Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes	Yes	Yes
Observations		7,748	7,784	7,748	7,784	7,748	7,784
Clusters		1,643	1,675	1,643	1,675	1,643	1,675
R-squared		0.05	0.04	0.02	0.02	0.01	0.02

*Notes: Each column represents a separate split-sample regression of Equation (1), where the dependent variable is a dummy capturing the outcome indicated by the column header. The regressions in columns (1), (3) and (5) are run on the subset of candidates who grew up in families with a below-median division of labor, and regressions in columns (2) (4) and (6) are run on the subset of candidates who grew up in families with an above-median division of labor. A few persons from the baseline analysis who were not successfully matched with grandparents are omitted. Main effects are not reported. Standard errors are clustered at the individual level and reported in parentheses. The number of individuals (clusters) observed at each event time period are reported in brackets. * denotes 10% statistical significance, ** 5% and *** 1%.*

Table A.5: Split-sample regressions, Meeting times

		Elected	
		(1) Daytime	(2) Other times
Female × $t = -2$	$[n_{day} = 441,\, n_{other} = 1,594]$	0.016	-0.020
		(0.026)	(0.016)
Female × $t = -1$	$[n_{day} = 686,\, n_{other} = 2,385]$	ref.	ref.
Female × $t = 0$	$[n_{day} = 686,\, n_{other} = 2,385]$	-0.029	-0.042***
		(0.027)	(0.016)
Female × $t = 1$	$[n_{day} = 686,\, n_{other} = 2,385]$	-0.016	-0.040**
		(0.028)	(0.017)
Female × $t = 2$	$[n_{day} = 487,\, n_{other} = 1,616]$	-0.030	-0.047**
		(0.028)	(0.019)
Female × $t = 3$	$[n_{day} = 245,\, n_{other} = 791]$	-0.049	-0.049**
		(0.039)	(0.022)
Party FE		Yes	Yes
Age FE		Yes	Yes
Year FE		Yes	Yes
Observations		3,231	11,156
Clusters		686	2,385
R-squared		0.05	0.05

*Notes: Each column represents a separate split-sample regression of Equation (1), where the dependent variable is a dummy capturing the outcome indicated by the column header. The regression in column (1) is run on the subset of candidates who ran for office (at $t-1$) in a municipality where local council meetings are held during standard business hours, and the regression in column (2) is run on the subset of candidates who ran for office (at $t-1$) in all other municipalities. Some observations from the baseline analysis were omitted due to missing meetings data. Main effects are not reported. Standard errors are clustered at the individual level and reported in parentheses. The number of individuals (clusters) observed at each event time period are reported in brackets. * denotes 10% statistical significance, ** 5% and *** 1%.*