

All the Single Ladies: Job Promotions and the Durability of Marriage

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Abstract

We investigate how promotions to top jobs affect the probability of divorce. We compare the relationship trajectories of winning and losing candidates for mayor and parliamentarian, and find that a promotion to one of these jobs doubles the baseline probability of divorce for women, but not for men. We also find a widening gender gap in divorce rates for men and women after being promoted to CEO. An analysis of possible mechanisms shows that divorces are concentrated in more gender-traditional couples, while women in more gender-equal couples are unaffected.

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1. Introduction

Around the world, women are severely underrepresented at the top of organizational hierarchies. In 2017, men accounted for 94% of CEOs in Forbes 500 firms and more than 77% of the world's parliamentarians (www.fortune.org, www.ipu.org). This inequality translates into gender gaps in income, status, voice, and democratic representation (e.g. Albrecht, Bjorklund, and Vroman 2003; Arulampalam, Booth, and Bryan 2007). It also feeds negative stereotypes about women's leadership abilities and depresses the career ambitions of young women (Beaman et al. 2009, 2012).

This paper contributes to understanding women's absence from top jobs by investigating the effect of job promotions on divorce. We add to previous work about the friction between marital stability and women's career success. Research has shown that increases in women's – but not men's – earnings are correlated with divorce (Becker, Landes, and Michael 1977; Weiss and Willis 1997). Higher levels of divorce and marital problems have also been found in couples where the wife earns more than the husband (Bertrand, Pan, and Kamenica 2015). Speed dating experiments have shown that men shy away from women they perceive to be smarter or more ambitious than themselves (Fisman et al. 2006). Evidence that the career–relationship trade-off is noticeable for women further down on the career ladder comes from recent field experiments on MBA students. When told that their self-reported ambitions would be shared with fellow students, single women drastically understated their ambition levels compared to women already in relationships (Burtzyn, Fujiwara, and Pallais 2017).

We contribute the first causal evidence on how promotions impact marriage durability across genders. We document that being promoted to a top job leads to a large increase in the probability of divorce among women, but not among men. This causal claim is based on 30 years of detailed Swedish register data and a differences-in-differences (DID) design that follows job candidates before and after promotions. Most of the analysis focuses on the jobs of mayor and parliamentarian, two jobs at the pinnacle of power in the public sector.¹ We can identify and follow both winning and losing candidates for these positions over time, before and after their promotion. This lets us document the absence of pre-trends in divorce and other observables between promoted and non-promoted candidates (see e.g. Greene and Quester 1982; Johnson and Skinner 1986).

The results show that a promotion doubles the divorce rate among promoted women compared to those who tried, but failed, to get the promotion. After three years on the new job, 7 percentage points fewer of the female mayors and parliamentarians remained married to their spouse compared to women

¹ These jobs are in the top 5% of the annual Swedish earnings distribution (authors' calculations, see Web Appendix Figure W1). They are paid slightly less than representatives in the U.S. Congress, and nearly the same as California state legislators. Our sample of Swedish CEOs would have their closest equivalent in CEOs of mid-size American firms.

who ran for office but lost. This result is corroborated in a sub-sample of closely contested elections, a situation in which the promotion is quasi-randomly assigned between job rivals.

Extending the analysis to the private sector, we study all CEO promotions in Swedish private firms over a 12-year period. Comparing men and women promoted to the CEO level, we document a strikingly similar pattern of a widening gender gap in divorce rates after promotion.

To analyze the potential mechanism(s) behind the divorce effect for women, we split the sample of political candidates according to characteristics of the household and the politician. The main finding is that post-promotion divorces are concentrated in gender-traditional couples – those with a larger spousal age gap and a more gender-skewed division of parental leave. Women in more gender-equal couples do not divorce more often after a promotion. This suggestive evidence highlights the problematic link between couple formation and the glass ceiling for women in the labor market (see also the discussion in e.g. Bertrand 2018. Men with high earnings ability are often in relationships that focus on *his* career, while women with high earnings ability tend to be in dual-earner relationships in which she is the primary caregiver (e.g. Ely, Stone, and Ammerman 2014; Kleven, Landais and Sogaard 2018). For women in traditional marriages, reaching a top job is likely to cause more adjustment costs – such as stress and strain from task renegotiation – than for men in these traditional unions.

Other explanations for this rise in the probability of divorce for promoted women find less support. There is no evidence of a “temptation effect” in which women’s (but not men’s) promotions increase the chances of finding a new partner. We do not find that women who worked in a more female-dominated previous workplace before entering public office are more likely to divorce (McKinnish 2004).² Divorced and promoted women also remarry at a slower rate than divorced women without promotions. Another explanation that receives little support is that promotions give women (but not men) the necessary economic independence to get a divorce. The vast majority of women in our sample have high earnings *before* their promotion; there is only a small positive correlation between the size of the earnings increase from the promotion and the probability of divorce. Finally, we find some evidence of sensitivity to social norms on the earnings distribution within the couple.

Our paper contributes to the economics, political economics, political science, and sociology literatures. In economics, we supplement the growing discussion of the causes and consequences of career inequality by gender (e.g. Lazear and Rosen 1990; Bertrand, Goldin, and Katz 2010; Bjerk 2008; Booth, Francesconi, and Frank 2003; Smith, Smith, and Verner 2013). Our analysis of promotions complements previous work on the link between labor market performance and marriage durability: we improve on the measurement of labor market performance by using actual promotions instead of earnings, and

² A female dominated workplace is defined as an organization, at the plant-level, with an above median share of women and, separately, an above-median number of women.

provide causal estimates.³ Notably, by studying promotions to top jobs, our analysis offers a different margin of variation than previous work focusing on labor market entry, which positions us to better understand gender gaps at the top of the income distribution.

For the field of political economics, we offer the first (to our knowledge) analysis of the non-monetary costs of holding political office.⁴ Evidence of such non-monetary components of the cost–benefit analysis can help improve our understanding of politicians’ career choices (e.g. Diermeier, Keane, and Merlo 2005; Matozzi and Merlo 2008). Our findings suggest that marital stability may enter differentially into women’s and men’s cost–benefit analyses when deciding whether to run for political office. Notably, a negative demonstration effect on junior women is fully compatible with a situation where divorce makes promoted women better off (emotionally, financially, or both). As long as a subset of men and women who contemplate a career also value a continued relationship, a perceived trade-off for women but not for men will create a gender-skewed candidate pool.

We make a similar contribution to the field of political science. This literature has shown descriptively that women politicians are more likely to be divorced or single than their male colleagues (e.g. Carroll and Sanbonmatsu 2013). Our paper provides a causal link between political promotion and marriage duration, and suggests that the circumstances of couple formation can be an important factor that shapes gender representation in politics. Finally, our causal analysis of job promotions contributes to sociology research about the drivers of marriage dissolution in general, and the role of labor market events in particular (Amato and Previti 2003; Oppenheimer 1997; Heckert, Nowak, and Snyder 1998; Jalovaara 2003; Liu and Vikat 2004; Rogers 2004).

The paper is organized as follows. We start by describing the data and sample selection for politicians and CEOs. We then describe the pre-promotion characteristics of individuals and households, using both register data and a survey conducted by the authors for this study. This is followed by the DID estimation for politicians and a descriptive event study for CEO promotions. The final sections investigate possible mechanisms by splitting the sample based on theoretically relevant background variables of political job candidates and their households.

2. Data and sample selection

We use Swedish register data for the country’s entire working-age population, which contains yearly observations from 1979 to 2012. Each person has a mandatory ID code, which is recorded in interactions

³ In addition to the work mentioned above, see Kesselring and Bremmer (2010) and Newman and Olivetti (2017), as well as work on how the negative economic shock of unemployment triggers the risk of divorce when the husband, but not the wife, becomes unemployed (Rege, Telle, and Votruba 2007; Charles and Stephens 2004; Eliasson 2012; Doiron and Mendolia 2012).

⁴ A number of papers study *monetary* costs, either empirically (e.g. Eggers and Hainmueller 2009; Lundqvist 2015; Fisman, Schulz, and Vig 2014) or theoretically (e.g. Diermeier, Keane, and Merlo 2005; Matozzi and Merlo 2008).

with numerous public authorities and thus links individuals to various administrative registers. These registers can be pooled into high-quality datasets with little misreporting and few missing observations.

We use the Marriage Register to link spouses to each other and to determine whether they divorced. Sweden has no-fault divorce, and couples are not required to undergo mediation or a period of living separately. In most cases, divorce proceedings can be processed within one month. The divorce law mandates a 6-month cooling-off period between filing for and finalizing a divorce if at least one spouse demands it or if the couple has children under 16. About 40% of the couples in our data have children under 18, meaning that some non-negligible proportion of the divorces in a specific year was initiated in the previous year. The couple's assets are divided equally after a divorce, but apart from child custody payments there is no alimony (maintenance/spousal support) to retroactively compensate spouses for labor market decisions within the household.

Cohabitation is not perfectly measured in Swedish registers. Joint family ID codes are assigned if a couple has a child together or lives together in a single-family home, i.e. not in an apartment. Couples without children or who live in apartments are thus excluded. Our study does not extend to cohabitation for this reason, and because a large share of individuals in our sample – 61% of women and 70% of men – were married prior to their promotion.

Our socioeconomic variables are taken from the longitudinal integration database for health insurance and labor market studies (LISA, according to its Swedish acronym). This database includes data from tax records on wage income, income from business ownership, and parental leave (variable definitions are discussed further in Section 5). Between 1979 and 1989 we extract this information from the Income and Taxation Register. Additional background variables taken from LISA are sex, birth year, birth region, education length, industry code, and occupation code.

We identify CEOs from the occupations listed in the register data (see Andersson and Andersson 2009). Politicians are measured in a separate dataset that is linked to LISA. All Swedish political parties must report a list of their candidates (including their personal ID codes) to the electoral authority in every election. This creates a pooled dataset of ID codes for the universe of nominated politicians at all levels of politics, which includes their political party, their list rank on the rank-ordered electoral ballot, and whether or not they were elected.

2.1. Sample selection: contenders for political promotion

The jobs of parliamentarian and mayor both offer salaries in the top 5% of the Swedish earnings distribution (Web Appendix Figure W1). Being promoted to one of these two jobs results in a similar average increase in earnings of about 20% (see Lundqvist 2015 for an analysis of parliamentarians; for mayors see Figure 7). Like other top jobs, they also offer a more generous pension plan than middle or lower-level jobs. Work hours are long, and parliamentarians also frequently commute to Stockholm from their home district (we return briefly to this variation in the robustness checks in Section 4.2).

A clear advantage of studying promotions to political jobs is that these positions are highly comparable across time and space. In Sweden, the media does not focus on politicians' family situations as they do in some other countries. Politicians' spouses are not involved in political campaigns, and are a non-topic in the media. There are thus few incentives to stay with one's spouse for the sake of public appearance. The election campaign season is also quite short, with just one month of intensive campaigning.

The main advantage of analyzing political jobs is that we can identify candidates who either won the promotion or sought it but lost. Consider parliamentarians first. In Sweden's list-based proportional representation system, a party's seats in parliament are counted from the top of the ballot paper. If three seats are won, the top three people on the ballot are allocated to these seats, and cannot be removed from office by the party after that fact. Candidate nominations are organized in a geographical quota system with 29 electoral districts. Politicians in each district climb their way up through the ballot ranks over time, which is similar to climbing the career ladder of a private firm. For each ballot in the 29 electoral districts, we define the contenders as the last elected person (i.e. the lowest-ranked candidate who got elected) and the first unelected person (the highest-ranked person who did not get elected). We exclude the extremely small proportion of electoral ballots (1%) from which preference votes determined who was elected (further sample restrictions are described below).⁵

Another important characteristic of Sweden's parliamentary system is relevant for studying promotions to *mayor*. Each of the country's 290 municipalities has a mayor, who is appointed by the ruling coalition (in the same way that the prime minister is appointed at the national level). A ruling coalition typically forms within either the right or left bloc of political parties (Alesina, Roubini, and Cohen 1997), giving us two rivals for the position of mayor in each local election: the politician at the top of the ballot of the largest party in the left bloc and his or her counterpart from the right bloc. The person whose bloc wins becomes mayor, and the person whose bloc loses usually becomes the opposition leader – a position with substantially less influence, work hours, and responsibilities. On average over our sample period, the average annual earnings of mayors were about 25% higher than the average for opposition leaders.⁶

The mayor is nearly always chosen via the bloc-level electoral contest between the top-ranked candidates in the largest parties in the two political blocs. But there are three deviations from this rule in the data. First, in 21% of the elections, political parties outside the traditional blocs won enough votes so that neither of the two blocs received a seat majority. Second, in 10% of the elections in which a bloc

⁵ Since 1998, Swedish voters have been able to cast one voluntary preference vote for any candidate on the ballot of the party they vote for. There are three reasons that this system does not affect which of the marginal candidates is elected, and is therefore unimportant for our analysis. First, only one-third of voters utilize their voluntary vote. Second, the vast majority of those who do use their voluntary vote select the top candidates on the ballot rather than marginal candidates. Third, the threshold of votes needed to win a seat is prohibitively high (see Folke, Person, and Rickne 2016 for a detailed description).

⁶ In-depth descriptions of the positions of mayor and opposition leader can be found in Nilsson 2001; Jonsson 2003; and Montin 2007.

did win a seat majority, the largest party did not appoint the mayor. In our sensitivity tests we show that our results are robust to excluding these elections. The third deviation is that the top-ranked person on the party's ballot fails to become mayor in about 10% of the elections.⁷ This measurement error biases our results toward a null effect from promotion, since we count some people as promoted even though they were not.

Parties' electoral fortunes can shift over time to allow politicians to first win, then lose, and then get re-elected. To correct the promotion variable for this possibility, we only include persons who have never held either of these two political jobs in the past. We allow losers to appear in the dataset more than once, but cluster the standard errors at the individual level. A robustness check shows that the main result is not sensitive to excluding those who run again after having lost in an earlier election (see Section 4.2). Nevertheless, since the split-sample analysis in the mechanism section quickly suffers from small-sample issues, we keep the repeating losers throughout to avoid going back and forth between different estimation samples.

We pool the rival candidates for the positions of mayor and parliamentarian in all six elections between 1991 and 2010.⁸ We then go back in time four years before the election in which a particular person was a candidate ($t = -4$). In this year, we drop politicians who were not married, leaving 70% of the men and 61% of the women (robustness checks show that the results are not driven by the choice of starting year).⁹ We also exclude people who reached the age of 65 before the end of the election period (10% of the sample), a potential confounder for divorce if promoted politicians stay longer in the workforce (although the sample restriction does not affect the estimated results). The final sample includes 641 women and 1,246 men. As illustrated in Figure 1, we follow the relationship statuses of this sample over time, starting four years prior to the election in $t = -4$, up to the election in $t = 0$, and eight years afterwards to $t = 8$. We conduct the analysis on a pooled sample of promotion winners and losers from *different* electoral ballots or municipal elections (following e.g. Eggers and Hainmueller 2009, and Kotakorpi, Poutvaara, and Terviö 2017), because two married women rarely appear as (close) contestants on the same ballot or municipal election.

⁷ This can be verified in our data, which includes the ID code of all mayoral appointments made after the 2006 and 2010 elections.

⁸ We exclude the 1994 parliamentary election because of erroneous recording of the list rank variable for this election.

⁹ Three same-sex partnerships among women politicians were excluded from the sample to facilitate our later analysis of heterogeneity between gender-equal and gender-traditional couples. There were no registered same-sex partnerships among the men in the estimation sample.

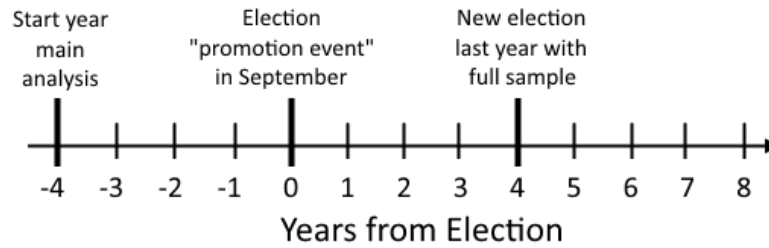


Figure 1. Timing of events.

2.2 Sample selection: CEOs

For CEOs, we can only observe people who are promoted, and not those who applied but did not get the job. The occupation code that identifies all CEOs is available from 2002 to 2012. We limit the sample to firms with more than 100 employees to capture top positions in the country’s economic structure. To ensure that we capture actual promotions rather than lateral moves, we also limit the sample to internal promotions to CEO. We go back four years before the promotion and select the married people (68% of the men and 65% of the women). We also exclude people who turned 65 within four years of their promotion ($t = 3$). The final sample includes 105 women and 715 men.

3. Descriptive statistics

We present two types of descriptive statistics for the sample of politicians, and place details of the CEO sample in the Appendix to save space. First, we compare the means of socioeconomic variables at both the individual and couple levels for men and women who were either promoted or not. These variables are measured in the year(s) before the promotion (i.e. pre-treatment), and the sample is split by gender and by (subsequent) promotion. Selected demographics are also compared between the estimation sample and the general Swedish population. A second set of descriptive statistics compares the types of promotions that men and women receive by plotting the distribution of increases in earnings and reporting data from a survey on work hours and influence conducted by the authors for this study. The aim is to show that our analysis of divorce is not confounded by men and women obtaining promotions to jobs with different characteristics.

We start by comparing the socioeconomic characteristics of job candidates prior to the election that assigns the promotion. Relevant variables are derived from previous research on labor market and marriage durability and from sociological research on the drivers of divorce (see, e.g. Becker, Landes, and Michael 1977; Weiss and Willis 1997; Amato and Previti 2003). Some of these variables cannot be affected by the election/campaign work and are therefore measured in the election year ($t = 0$). These individual-level variables include age, age at marriage, and a dummy variable for having completed

tertiary education. At the household level, we measure marriage length, a dummy for whether the marriage is not the politician's first, and a dummy for whether both spouses were born in Sweden. Family structure is measured using two dummies that measure whether the household has children: one indicates at least one child aged 0–17 and the other denotes at least one child aged 0–6.

Earnings are measured as the sum of deflated annual earnings from jobs and business ownership. For each person, we take the average of these sums over the three years prior to the election ($t = -3$, $t = -2$, and $t = -1$). Using this average rather than a single year provides a more stable measure of earnings, which balances out year-to-year variability from temporary labor market absences or from events such as temporary unemployment, sickness, or parental leave. We report the means of the politician's earnings, the spouse's earnings, and a dummy for whether the politician's earnings are higher than the spouse's earnings.

We compute the division of parental leave based on insurance payments, which is available for the full period, and cross-check this information against data for days of leave, available from 1993.¹⁰ Payments are summed for the three first years of each child's life; if the couple has more than one child, they are averaged across all children. We only consider joint children with the current (pre-promotion) spouse.

Table 1 compares the means of the pre-promotion traits within genders and between persons who were (subsequently) promoted and those who were not. A simple regression is used to detect statistically significant differences at the 5% level.¹¹

Although the causal claim of the DID design hinges on parallel pre-trends in the outcome variable, we can gain useful insights from comparing the levels of observable traits between the treatment and control groups. The comparison shows that only a handful of traits are unbalanced, and that the differences are small in absolute terms. One exception is politicians' earnings, for which we find a pre-promotion difference for both men and women. We return to this variable in the DID analysis below, showing that although there is a difference in means, there are no differential pre-trends in earnings.

Differences in traits *between* men and women cannot be omitted variables in the main analysis, which assesses men and women separately. But such differences are relevant for understanding differences in the size of the treatment effect for women and men. The men and women in our sample have a similar average age (roughly 50), and a similar length of marriage (20 years). Close to 10% of both men and women have children under 6 years old in the household. There are also traits that differ. Men appear to be in relationships that focus on their careers: they are older than their spouse, earn more, and do less care work. The women appear to be in relationships with a more equal distribution of earnings, but they are generally younger and have used more than 80% of the couple's total parental leave. This means

¹⁰ Section W1 of the Web Appendix shows that these measurements are highly correlated.

¹¹ We run a regression instead of a t-test so that we can cluster the standard errors at the individual politician level.

that even among the women in our sample, who are candidates for top jobs, many are in relationships that specialize in the husband's career (we return to these differences in Section 6).

Table 1. Comparison of pre-promotion traits.

Subsequently promoted	Women		Men	
	Yes	No	Yes	No
<i>Couple characteristics</i>				
Marriage length (years)	20.76	21.01	20.91	20.78
Second marriage (%)	0.03	0.04	0.01	0.01
Same birth region (%)	0.89	0.92	0.94	0.93
Has children (0–17)	0.37	0.36	0.48	0.42
Has children (0–6)	0.10	0.09	0.11	0.10
Politician's share of earnings	0.57	0.54	0.68	0.66
Politician out-earns spouse (share)	0.66	0.53	0.90	0.87
Politician's share of parental leave	0.83	0.81	0.14	0.15
Age difference (politician-spouse)	-3.70	-4.01	1.70	1.85
<i>Individual characteristics</i>				
Politician's age	48.60	48.94	49.81	50.89
Politician's age at marriage (1)	28.10	28.14	29.02	30.17
Politician's earnings (2)	335.4	290.6	370.4	344.3
Politician's tertiary education (share)	0.65	0.67	0.52	0.53
Spouse's age	52.60	53.15	48.24	49.08
Spouse's earnings	282.8	289.8	174.6	179.5
Spouse's tertiary education (share)	0.42	0.46	0.54	0.52
Observations	235	406	492	749

Notes: Bold letters represent differences between promoted and non-promoted individuals of the same sex at the 5% level or lower, using ordinary least squares (OLS) regressions with standard errors clustered at the individual level.

(1) Dummy variable that indicates whether the spouses are from different birth regions: Sweden, other Nordic countries, EU27 excluding Nordic countries, Asia, Europe excluding Nordic countries and EU27, South America, Africa, North America, Soviet Union and Oceania.

(2) We only know the exact year of marriage for persons who married after 1979. For those married before 1979 we define the year of marriage as the year the couple's first child was born. For couples already married in 1979 and with no children born before that year, we count 1979 as the year of marriage. This approximation was previously used by e.g. Ginther and Sundström (2010).

(3) All measures of earnings are in units of 1,000s SEK (1 SEK \approx 0.12 USD).

We also compare the women and men in the estimation sample to the general Swedish population in terms of civil status (percent married/divorced) and family structure (percent with any children, percent with at least one non-adult child, percent with at least one small child). The population data are weighted to correspond to the composition of birth cohorts and year of observation for the estimation

sample (the detailed analysis can be found in Web Appendix Section W2 and Table W1).¹² The key take-away from the description is that men with high-powered careers deviate from the population by having more stable relationships, while women with similar careers do not. Women in our estimation sample instead have highly similar family structures to the general population, echoing previous descriptions of Swedish professional women as equally likely as those with less education to have children or to ever marry (e.g. Bertrand et al. 2016; Boschini et al. 2011).

Next, we compare the promotions that men and women receive. Restricting the data to promoted people, we compute pre-promotion earnings as the average earnings in the three years prior to the promotion and post-promotion earnings as the average of the first three years on the job. Figure 2 plots the differences between these two measurements for women and men separately. The two distributions are highly similar, showing that women and men receive largely similar distributions of pay raises from their job promotions.

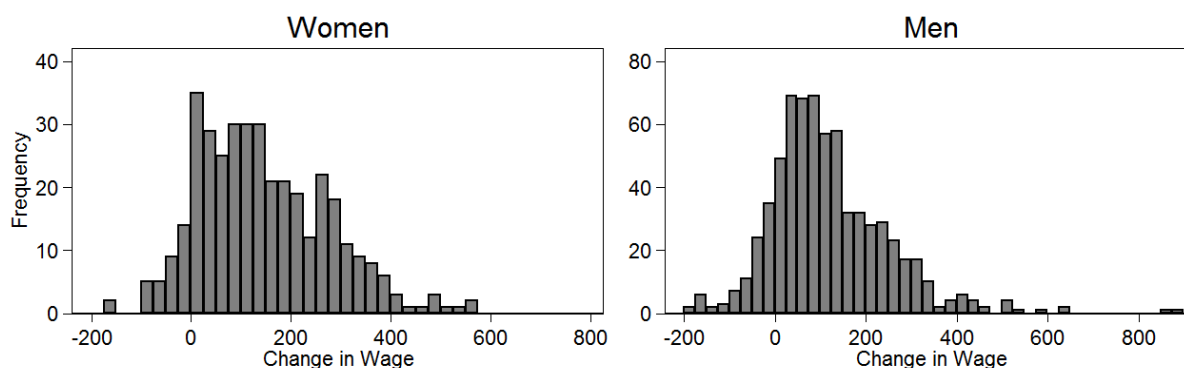


Figure 2. Distribution of changes in earnings before and after promotion (1,000 SEK).

Notes: The figures show the distribution of changes in earnings for promoted men and women. Pre-promotion earnings are measured as the average of annual earnings during the three years prior to the promotion, and post-promotion earnings are averaged across the three years after the promotion.

Another aspect of promotions is the workload of the new job. For municipal politicians, we surveyed mayors to assess their workloads (with a response rate of 70%). The distribution of self-reported weekly work hours is highly similar between female and male mayors (Figure 3). Using data from the 2012 survey of Swedish local politicians (KOLFU survey (Karlsson and Gilljam 2014)), we can also verify that the distributions of male and female mayors' self-perceived political influence are very similar (Figure 4). The description shows that men and women have very similar distributions of pay raises from their promotions, and that male and female mayors have highly similar distributions of self-reported workloads and influence. Differences between genders in these traits of the promotion itself are thus unlikely to confound our analyses.

¹² We also weigh the population sample to match politicians in terms of their municipality of residence and education level, and we do a separate comparison between political candidates and people who hold upper-level jobs (as indicated by the occupation code).

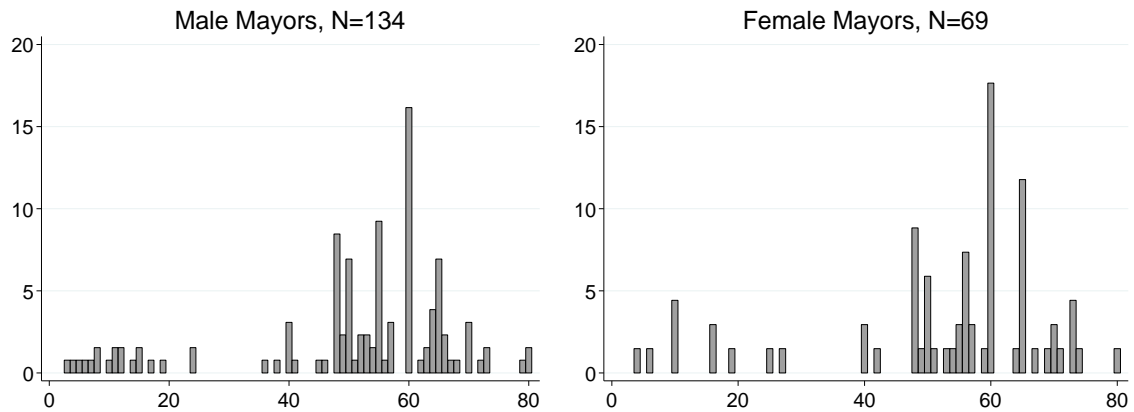


Figure 3. Self-reported work hours in a typical week by male and female mayors.

Notes: Data from the authors' survey of mayors in 2013.

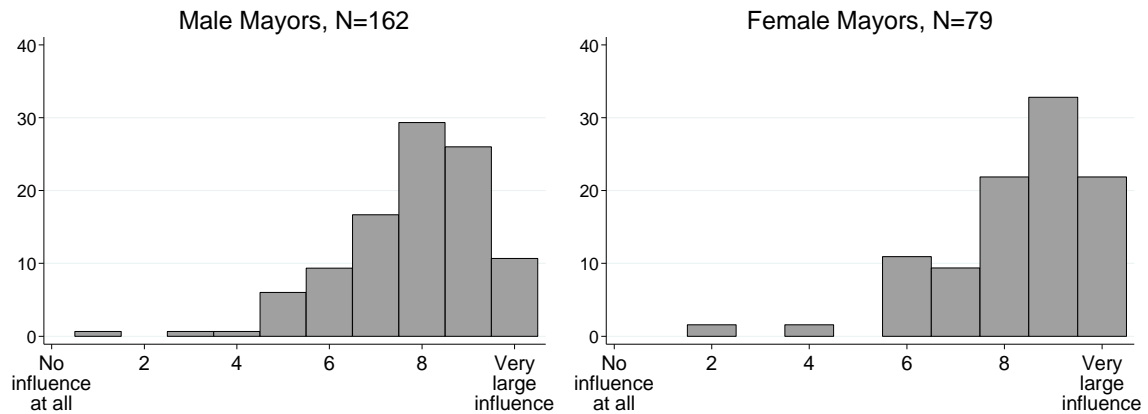


Figure 4. Comparison of male and female mayors' self-perceived political influence

Notes: Data from the 2012 KOLFU survey (Karlsson and Gilljam 2014) of all municipal politicians (response rate among mayors = 83%, 241/290). The survey question asked the politician to rate his or her own influence over “policy outcomes at the municipal level.”

4. Promotion and divorce among mayors and parliamentarians

The main results are shown in Figure 5, with descriptive evidence in the top panel and regression evidence below. The plots show time trends in marriage durability for men and women separately. The lines represent the share of persons that remained married to their spouse among persons who are promoted (black lines) or not promoted (gray lines). Both lines start at the value "1" four years before the election, reflecting our sample selection of married individuals. Over time, all lines slope downward as some marriages end in divorce in each year.

Inspecting the rate of marriage durability in the years leading up to the promotion event in $t = 0$, we see no differences between men and women who are subsequently promoted (or not). This validates the key identifying assumption of our design – the lack of differential trends in divorce rates between

the treatment and control groups prior to the promotion. In the years after the promotion, the trend lines for women start to diverge. Promoted women get divorced at twice the rate of non-promoted women, resulting in a 7-percentage-point divorce gap three years after the election. Looking at the long-term development we can also see that the gap continues to grow, rather than converge, over time. For men, there is no difference in divorce rates between promoted and non-promoted in either the short or long run.

In the bottom half of Figure 5 we report estimations of the difference in marriage durability between promoted and non-promoted men and women over time. The size of the difference between the treatment and control groups in the probability of remaining married in each year is benchmarked against this difference in the election year ($t = 0$). It is estimated in the following DID model:

$$Y_{i,e,t} = \beta_t P_{i,e} * T_t + T_t + \delta_{i,e} + S_{i,e} * T_t + \tau_e * T_t + \varepsilon_{i,e,t}, \quad (1)$$

where the dependent variable is a binary indicator for remaining married. The vector (T_t) is a set of dummy variables for each year before or after an election, starting four years before the election ($t = -4$) and ending eight years afterwards ($t = 8$). The variable $P_{i,e}$ takes a value of 1 for those who are promoted and 0 for those who are not, which corresponds to the black or gray lines in the top panel of the figure (and thus is constant for the time window around each election). By excluding the time dummy for the year of the election ($t = 0$), we let this year become the reference category. The estimates on the interactions between each time dummy and the promotion dummy (β_t) thus capture the gap in remaining married between promoted and non-promoted people, relative to the size of that gap in $t = 0$. By examining the estimates for the pre-promotion years, $t = -4$, $t = -3$, $t = -2$, and $t = -1$, we can verify that there is no pre-existing difference in the trend in divorce rates between *subsequently* promoted or non-promoted people *before* the promotion. Correspondingly, if the promotion causes a divergence in marriage durability between the promoted and non-promoted, this should show up as positive or negative estimates for β_t in the years after the election ($t = 1$ to $t = 8$).

The regression specification also includes independent terms for the time dummies before or after the election (T_t), and interactions between these time dummies and i) fixed effects for each election τ_e and ii) a binary indicator for belonging to the parliamentary sample, $S_{i,e}$. We use these interaction effects to control for the fact that the probability of being promoted, or temporal trends in the probability of being promoted, could differ between elections and between the parliamentary and mayoral samples. Finally, we add fixed effects for each combination of election and individual, $\delta_{i,e}$ (recall that losing candidates can appear multiple times). The fixed effects structure makes it redundant to control for the independent terms of $S_{i,e}$, τ_e and $P_{i,e}$. Standard errors are clustered at the individual level.

Figure 5 plots the estimates of β_t together with 95% confidence intervals. The gray points and lines show the estimated promotion effect for men, and the black dots and lines show the estimates for women. These estimates show the exact same patterns as in the descriptive evidence in the top half of

the figure. For both men and women, the pre-promotions estimates are close to zero and lack statistical significance. After promotions, there is no indication of an effect for men, but a sizeable negative effect for women. Three years after the election, promoted women are 7 percentage points less likely to remain married than their non-promoted counterparts.

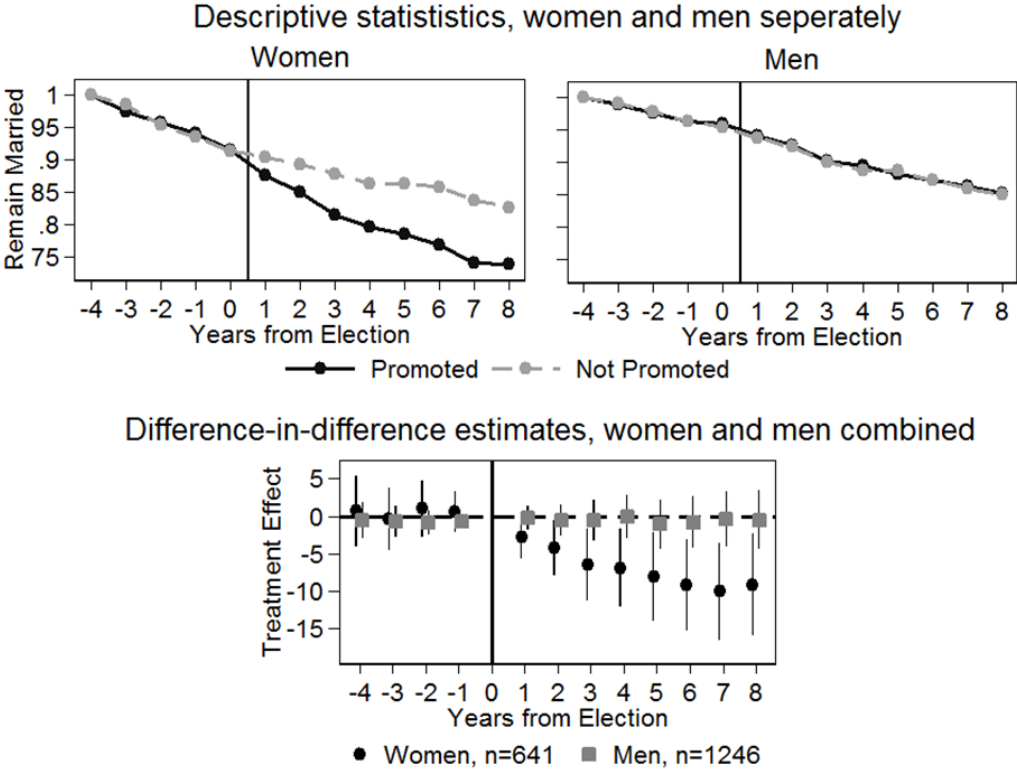


Figure 5. The effect of political promotion on remaining married.

Notes: The top panel plots the share of job candidates who remain married to the person they were married to in ($t = -4$) in each year. The x-axis shows the year, from four years before the election to eight years after the election. In September of election year ($t = 0$), the persons represented by the black line are promoted to either mayor or parliamentarian. The lower panel contains the estimates from the DID model (equation 1), which is run separately for men (gray markers) and women (black markers). The markers show the percentage point difference in the probability of remaining married between promoted and control groups in each year compared to the baseline event year ($t = 0$). Horizontal lines are 95% confidence intervals.

The following sections report the results of our robustness checks. We maintain that we have identified a causal effect of promotion on divorce, which is different for men and women in our sample. This does not mean, however, that sex *per se* is the cause of this difference. In other words, although men and women are differentially affected by promotions, we do not expect this gender difference to be a consequence of (biological) sex. It might instead be the result of family or individual-level characteristics that differ between the selected sample of male and female candidates who have become candidates for top jobs (see Table 1). In the mechanism section we seek more insights into these characteristics using a purely descriptive approach.

4.1 Robustness test in close elections

Despite the parallel time trends in marriage durability before promotion and the balance of most pre-promotion traits, a concern about our main analysis could be that women – but not men – decide to pursue a promotion when their marriage is on the rocks. We address this concern by narrowing the sample of elections to very close ones, in which it is highly uncertain which candidate will win. This means that the promotion is near-randomly assigned, and that the promoted and non-promoted persons should have similar expectations about winning. Selection into candidacy based on observed and unobserved characteristics should thus be even less of a problem than in the main analysis.

We construct two binary variables that indicate if an election is close, one for parliamentary elections and one for municipal elections. For municipalities, the indicator captures how similar the two political blocs are in size. A close election is defined as one in which the winning bloc's win margin, i.e. its share of the total vote, is below 5%. Calculating the margin of victory for parliamentary elections is a bit more complex. Parliamentary seats are allocated in two rounds, at the district and national levels, and the seat allocation is proportional to the national vote share. Another complication is that the win margin measured in vote share constitutes a closer win margin in a large party than in a small party. We follow Freier and Odendahl (2015) and adopt a pure simulation approach to calculate the margin and, in turn, set a delimitation value for our binary indicator. A close election is defined as a party losing its last (marginal) seat in at least 30% of the simulations. Appendix Section W3 contains a detailed description of how we calculate the municipal and parliamentary indicators of close elections. Appendix Table W2 replicates Table 1, the descriptive statistics for pre-promotion traits, in the close election sample. As expected, differences in traits between winning and losing candidates are (even) smaller than in the full sample.

Figure 6 replicates the main analysis (Figure 5) for the sample of close elections. The results are very similar to the main analysis, with one exception: the treatment effect is postponed by one year. This is logical, given that the average promotion in this sample becomes evident on election night rather than when the ballot rank order is set by the party 10–12 months before. Although the treatment effect is postponed by one year, the size of the estimated effect is still the same at the end of the election period ($t = 3$). The descriptive evidence suggests that the promotion doubles a woman's probability of getting divorced in the three years after the election. But compared to the main analysis, the effect is less precisely estimated due to the smaller sample size. The estimate at $t = 3$ is barely below the 10% significance level.

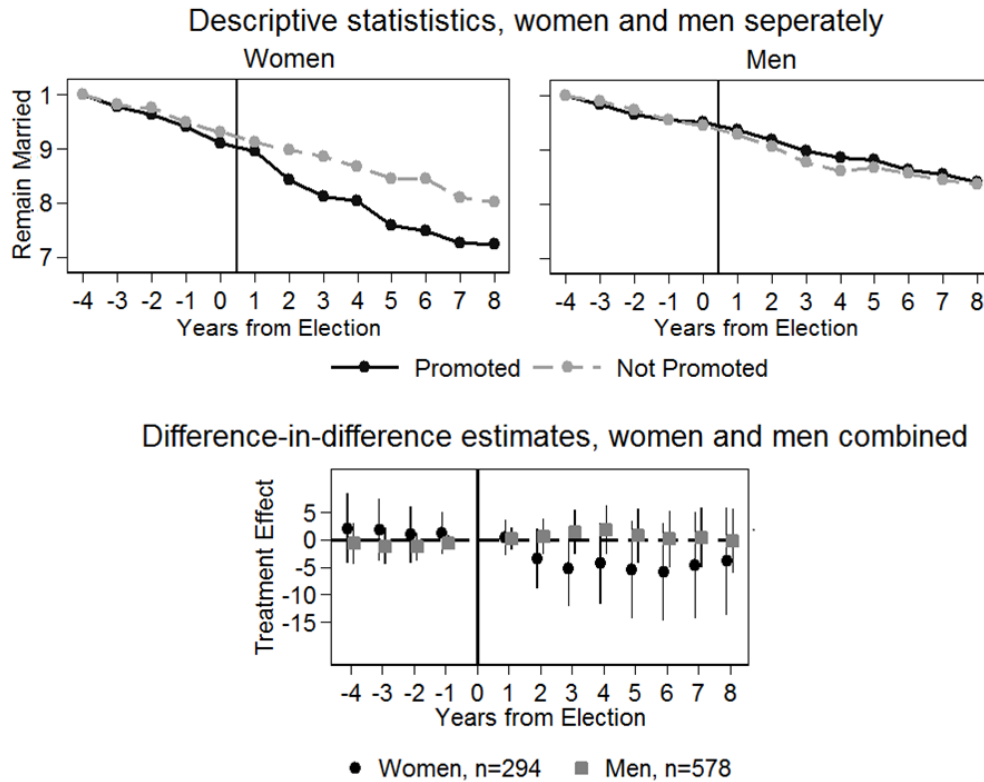


Figure 6. The effect of political promotion on remaining married in a sub-sample of close elections.

Notes: The structure of the plot is described in the note under Figure 5. For municipalities, a close election is defined as having a win margin less than 5%. For parliamentary elections, a pair of list ranks is defined as a close election if the party loses the marginal seat in at least 30% of the simulations described in Web Appendix section W3.2.

Appendix Tables W3 and W4 show the results for four alternative cut-offs for close elections. With a more generous cut-off than the baseline, the estimates become more precise. With more restrictive cut-offs, the sample size and precision are reduced to the point that the estimated coefficients lose statistical significance at conventional levels. The size of the treatment effect is also reduced, but with the large standard errors, we cannot distinguish random noise from a truly smaller treatment effect. We address this problem maintaining the cut-off for close elections but changing the year that we use to select the sample of married politicians. Instead of using politicians who are married in $t-4$, we use those who are married in $t=0$. This increases the precision of the estimate, since about 10% of the women in the baseline sample divorced before between $t=-4$ and $t=0$, making them immune to the treatment. By selecting politicians who were married in the election year ($t=0$), we replace these divorcees with women who got married in the four-year period leading up to the election. This means that the treatment effect should increase slightly, since 100% of the sample instead of 90% can be affected by the treatment. Changing the sample gives more stable estimate sizes across the different definitions of close elections. The estimated effect three years after the election only loses significance at the 5% level for the most restrictive definition of close elections (see Appendix Tables W5 and W6).

4.2 Robustness tests for sample selection and pre-trends in earnings

We run seven additional sensitivity checks: i) including control variables for observables in the baseline estimation; ii) restricting the sample to those who were candidates for top offices for the first time; iii) using a different starting year in the sample selection; iv) excluding candidates with spouses of retirement age; v) estimating the effect separately for mayors and parliamentarians; vi) excluding data with uncertainty in the electoral allocation of promotions; and vii) testing for differential, pre-promotion time trends in the earnings of promoted and non-promoted candidates and their spouses.

First, we re-estimate equation (1) but include control variables for all the predetermined characteristics and traits that were examined in Table 1 (except the division of parental leave, which is missing for the 45% of the sample with children born before 1980). All controls are included as an interaction with the time dummy variables, and the results replicate the baseline findings (see Web Appendix Figure W5). The second test is to re-estimate equation (1) excluding candidates who previously ran for either of the offices but lost. Losing about one-fourth of the sample produces slightly larger standard errors, but the size of the point estimates is unaffected (see Web Appendix Figure W6). The third robustness check (see Figure W7) shows that the results are unaffected across six alternative starting years for the sample selection of married politicians, ranging from $t = -6$ to $t = 0$, and excluding the starting year used in the main analysis ($t = -4$). The fourth test demonstrates the robustness of the results to excluding candidates with spouses who have retired, i.e. reached the age of 65, or will retire while the candidate is in office (Figure W8).

Next, we split the main sample by the two jobs, mayor or parliamentarian (Figure W9). The estimated treatment effect is clearly larger for parliamentarians. After three years on the job, the treatment effect is 11 percentage points for women parliamentarians and 5 percentage points for women mayors. Further probing of this difference shows that it is not due to commuting distance. The size of the divorce effect is very similar in districts that are close to, or far from, the parliament in Stockholm (results available from the authors).

The sixth test (see Figure W10) shows that our results are not sensitive to excluding data from elections in which the mayoral party is decided via coalitional bargaining, namely when i) neither of the traditional left or right blocs obtains a seat majority, or ii) the largest party in the winning bloc did not appoint the mayor.

A seventh and final robustness check examines the development of earnings, which are separately estimated for politicians and their spouses. The purpose is to test for differential trends in labor market behavior that can indicate sorting of people with more or less stable marriages into promotion and, subsequently, divorce. Women with less stable marriages might compete more fiercely to get elected and perhaps simultaneously strive harder for a promotion in their job outside of politics. This sorting would result in a positive, or possibly even a negative, pre-trend in the earnings difference between

promoted and non-promoted women. It could also show up as a pre-treatment trend in the differences in earnings between the politicians' spouses. If the politician increases his or her labor supply, the spouse may also increase their career input if joint leisure time is reduced, or reduce their labor input to take on more household responsibilities, or increase it to match the politician's increased work hours.

We run regression (1) with the outcome variable of the earnings of the politician and his or her spouse separately (in real Swedish Kronor, measured in 1,000s). The estimates are presented in Figure 7. Negative estimates in the years prior to the promotion may be explained by the fact that elections occur in September of the election year ($t = 0$), so the earnings increase for the election winner took effect for only four months of that year. In the three years leading up to the election, the estimates in the figure show no clear differences in trends in the earnings of the promoted women (or men) relative to women (or men) in the control group. This indicates that women (or men) who were promoted did not work more, or less, intensively in their previous job.

There are no clear differential pre-trends between the spousal earnings of either men or women. The post-promotion estimates also appear void of spousal adjustments which, to some extent, is a research finding in itself. This finding helps us rule out (gender differences in) spousal responses after a promotion, or lack thereof, as a major mechanism behind our baseline finding that promotions raise the divorce rate among women but not men. It also shows that families do not respond to promotions with large shifts in labor market specialization (which relates to previous research by e.g. Devereux 2004).

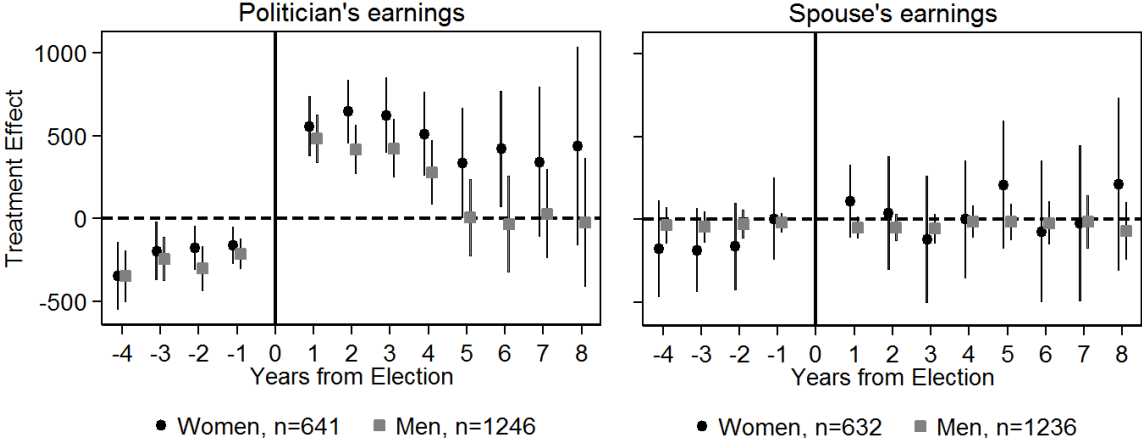


Figure 7. Pre-trends in the annual earnings of politicians and their spouses.

Notes: The figure shows estimates for running the promotion regression (equation 1) on the outcomes of the politician's earnings (left) and the spouse's earnings (right). The unit on the y-axis is the relative earnings difference between promoted and non-promoted individuals in each year and relative to the baseline year ($t = 0$). The unit of measurement is 100 SEK. The sample is restricted to politicians who were married as of 4 years prior to the election. The gray dots represent the difference relative to election year between promoted men and non-promoted men, and the black dots provide the corresponding estimates for women. Vertical lines indicate 95% confidence intervals.

5. Promotion and divorce among CEOs

The position of CEO is clearly the most prestigious in any firm, and is typically the pinnacle of a career within that organization. Being the CEO of a firm with more than 100 employees is a top job in the private sector. Their average annual earnings are well above the 99th percentile of the distribution of earnings within the working-age population of employed persons (authors' own calculations, see Web Appendix Figure W1).

As described above, our sample of CEOs consists of men and women who all received the promotion, as we have no data on unsuccessful applicants. The pre-promotion descriptive statistics for this sample show strong similarities to political job candidates in terms of the average divisions of paid labor, spousal age gap, and division of parental leave (the full set of descriptive statistics can be found in Table W2 in the Web Appendix).

The left part of Figure 8 plots the proportions of men and women who remain married to their spouse in each year. A shorter post-promotion window, four years, is chosen to accommodate the shorter sample period compared to the political jobs. The right-hand figure shows the estimated gender difference in the probability of remaining married. Female CEOs who were married at the time of their promotion are more than twice as likely to have gotten divorced three years after their promotion compared to their male counterparts. The regression estimates show that the gender difference is statistically significant at the 5% level. Prior to the promotion, the sample shows no clear gender difference in rates of divorce. Although the descriptive event study of CEO promotions does not permit causal inference, the similarity to the political promotions analysis suggests that the baseline finding extends to the private sector.

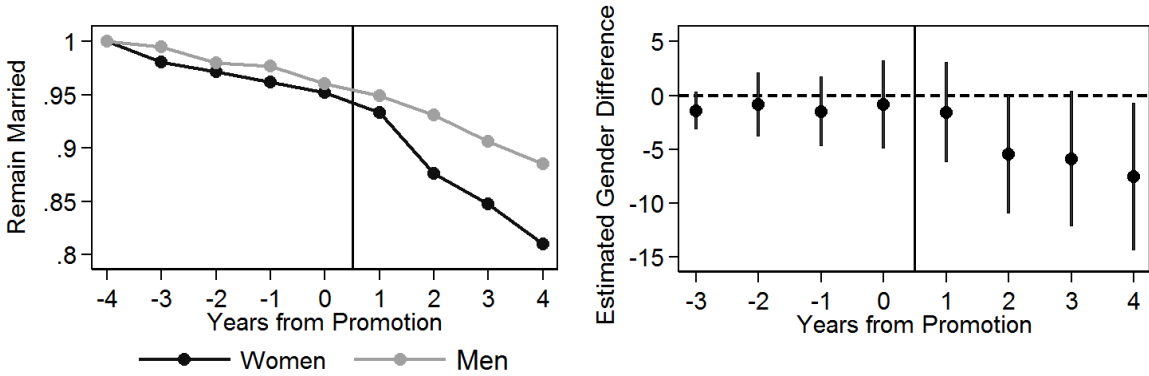


Figure 8. Event study of CEO promotion and remaining married.

Notes: The left-hand graph shows the proportions of men (gray line) and women (black line) that remain married to their partner in each year, starting four years before the promotion ($t = -4$) and ending four years afterwards ($t = 4$). The right-hand side of the graph shows the estimated relative probability that the promoted women will remain married in each year compared to the promoted men. The sample ($n = 105$ women and 715 men) includes all individuals who were internally promoted to CEO in a firm with at least 100 employees between 2003 and 2008. The sample is also restricted to individuals who were married as of four years before the promotion.

Extending the analysis to more sectors is difficult because promotions are not readily measurable in register data. An attempt to further generalize the findings can be found in Section 5 in the Web Appendix. We select five cohorts of university graduates in 1989–1993 from four university programs: medical doctors, priests, police, and pharmacists. We then go forward in time 20 years and measure each person’s career success relative to their graduating cohort. High career success is measured as an income above the cohort median, and low success as below this amount. Comparing the divorce rate for men and women with high and low career success reveals patterns that support the main finding of the paper. In three out of four professions (pharmacists are the exception), successful women have higher divorce rates than unsuccessful women. For men, the relationship is the opposite: a high level of career success is associated with lower divorce rates.

6. Mechanisms

Why does a promotion lead to divorce for women but not for men? In this section we re-estimate the main analysis in sub-samples of data to detect common features of women who divorce after they are promoted. We begin by testing – and rejecting – a “temptation effect,” in which women’s divorces are motivated by exposure to potential new partners. We then describe how divorces are more likely in traditional marriages and less common in more gender equal ones. Lastly, we describe a lack of evidence of an “economic independence effect” in which women’s divorces are driven by greater economic independence from their spouses.

The “Temptation Effect” A promotion can change a person’s work environment and introduce them to new potential partners. If women’s divorces are motivated by exposure to new potential partners, they should be concentrated among women who were the least exposed to opposite-sex coworkers prior to their promotion (following McKinnish 2004). We measure the proportion and number of opposite-sex coworkers in the politician’s previous workplace by leveraging the plant ID code in the register data.¹³ We exclude the politicians themselves from this calculation and, to reduce noise, we drop workplaces with fewer than 10 employees (less than 10% of the data). The sample is then split by the median of the share, or absolute number, of colleagues of the opposite sex. The two resulting groups have either “Low” or “High” expected temptation effects. Estimating equation (1) in the two samples does not support the temptation effect (see Web Appendix Figure W12). If anything, the promotion effect on divorce is larger for women who enter politics from relatively male-heavy environments.

A more direct test of the temptation effect is to study remarriage rates among promoted and divorced women. Figure 9 provides this description in two steps. We first use the pooled sample of winning

¹³ A workplace is defined by its Cfar code in LISA. This code ensures that we capture colleagues who were physically working together since it codifies the plant rather than the firm. Note here that the level of opposite-sex coworkers *after* the promotion is highly similar across all promoted politicians, so we can rely solely on the pre-promotion share for variation.

and losing candidates to compute the proportion of people that divorced within three years after the promotion event (black bars). This replicates the main finding that promoted women are more likely to divorce than any other group. We then fast forward eight years after the election and compute the proportion that remarried (gray bars). Comparing the black and gray bars for each sex and promotion status gives us the ratio of remarriage to divorce. This shows that 22% of the promoted and divorced women remarried, compared to 27% of the promoted and divorced men, and more than 30% for divorcees of either sex who were not promoted. This result – that promoted and divorced women are less likely than others to remarry – offers additional evidence against the temptation effect as an explanation for the higher divorce rate of promoted women.

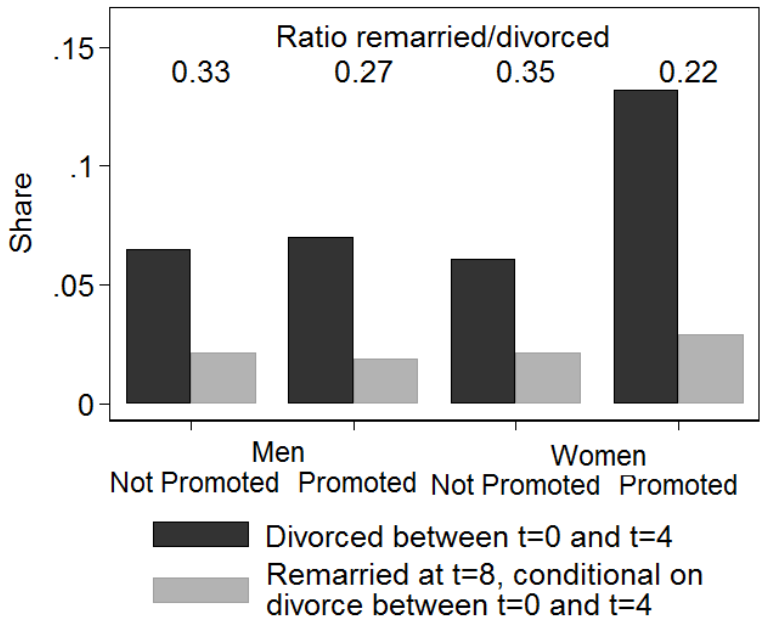


Figure 9. Promotion, divorce and remarriage.

Notes: The black bars show the proportion of men and women that divorced three years after the promotion, by promotion status. The gray bars show the proportion that remarried at eight years after the promotion. The numbers above each set of bars show the ratio of remarriage to divorce for each combination of sex and promotion status.

Traditional and gender equal marriages Economic theory suggests that marriages may be destabilized if job promotions moves the division of paid (or unpaid) labor away from the spouses’ expectations of those divisions (e.g. Becker, Landes, and Michael 1977; Weiss and Willis 1997). These expectations are formed early in the relationship, at the time of the “match.” If a husband is promoted in a gender-traditional couple that has specialized around his career since its early stages, this conforms with expectations. If, however, the wife is promoted in a gender-traditional couple, this deviates from the

expectation. In the context of our study, we expect women's divorces to be concentrated in traditional marriages rather than more gender-equal marriages.¹⁴

Economic theory is agnostic on which spouse would initiate the divorce when a promotion causes a change in the expected division of paid and/or unpaid labor. As the labor market outcomes of one spouse deviate more from the expected path, the cost–benefit calculus of remaining married shifts for both spouses (e.g. Becker, Landes, and Michael 1977; Weiss and Willis 1997). It is useful to think of this transition in terms of adjustment costs. When couples face greater adjustments to the division of their economic and social roles, stress and friction can affect both people and reduce the utility from a continued relationship.

We use two variables to capture gender-traditional marriages. The first is the spousal age gap, an indicator of gender-based specialization within the couple (Becker 1981; Eagly 1987).¹⁵ The second is the division of parental leave, described in Section 3. Like the spousal age gap, the split of parental leave measures gender-based specialization in the early phases of a relationship. This variable captures large time investments, since the Swedish leave policy covers 480 days of paid leave per child, and the vast majority of couples utilize the policy for at least 12 months (Statistics Sweden 2016).¹⁶ For the people in our sample, parental leave and couple formation happened decades ago since their average age is 50 and the average marriage length is 20 years (recall Table 1).

Figure 10 shows the distributions of the spousal age gap and the parental leave split. Although they are candidates for jobs that fall in the top 5% of the earnings distribution, the women's relationships show clear signs of prioritizing the husband's career. Four out of five women in the sample are married to an older man, and utilized more than 80% of the family's total parental leave themselves. Among the male politicians we see a similar pattern of gender-based specialization. Two-thirds are married to a younger woman, and fewer than one in five utilized more than 20% of the couple's total parental leave.¹⁷

¹⁴ A similar prediction can be drawn from sociology. A promotion may be seen as a critical transition point in a person's career. Such events can lower marital satisfaction by creating stress and conflict around the (expected) roles of the husband and wife (e.g. Coverman 1989). In our context, such conflicts could be expected to be the greatest for couples that started out with a more gender-traditional division of roles, but the wife is later promoted to a top job.

¹⁵ Using the terminology of social exchange theory, a couple in which the husband is older than the wife is organized to benefit from social exchanges that transfer economic resources from the husband to the wife and resources such as sex and children from the wife to the husband (following Thibaut and Kelley 1959).

¹⁶ The first 390 days have a wage-replacement rate of 80%, and the remaining 180 days a fixed payment rate of 20 euro. People whose earnings do not qualify them for the wage-replacement part of the benefits receive the fixed payment throughout.

¹⁷ These variables are not simply picking up the age of the politician. For women, the correlation between age and the spousal age gap is 0.08, and the correlation between age and the politician's share of parental leave is -0.04. For men, the corresponding correlations are 0.15 and 0.02.

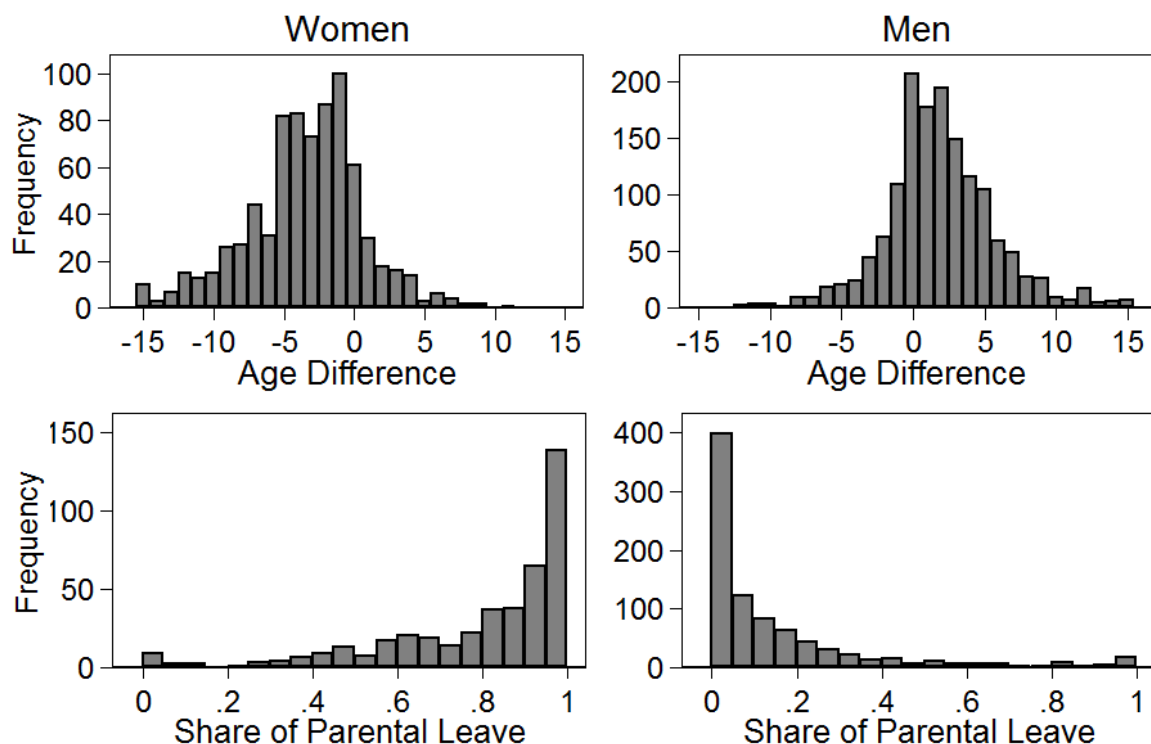


Figure 10. Distributions of the spousal age gap and the politician's share of parental leave.

Notes: The top graph shows the distribution of the spousal age gap, measured as the politician's age minus the spouse's age. The bottom graph shows the politicians' share of the total days of parental leave utilized by the couple and pooled for all children (for further details, see Section 3).

We divide the sample into three groups based on the spousal age gap: (1) the politician is younger by four years or more, (2) the age gap is relatively small (three years or less),¹⁸ and (3) the politician is older by four or more years. For the split of parental leave, we are forced to take a liberal view about what constitutes a more or less traditional marriage. A gender-traditional marriage is defined as a couple in which she took more than 90% of the leave, and remaining marriages are considered gender-equal.

The results in Figures 11 and 12 show that divorce among promoted women is concentrated in couples that were more specialized around the husband's career in the early stages of the relationship. Divorce is more likely to occur when the promoted woman (1) is younger by her spouse by a larger margin and (2) took a relatively larger share of the parental leave. Strikingly, we find no divorce effect in the sub-sample of women in more gender-equal couples. When the couple is closer in age and when the husband took relatively more parental leave, there is no increase in the divorce rate after the woman's promotion. Comparing Figures 11 and 12, the precision is smaller when we split the sample based on the parental leave division, but the pattern is similar to that for the spousal age gap: there is a larger

¹⁸ The empirical findings remain the same if we change the cut-off points by one year in either direction.

divorce effect in gender-traditional couples. Recall also that the sample size is smaller for this variable due to the unavailability of parental leave data for children born before 1980.

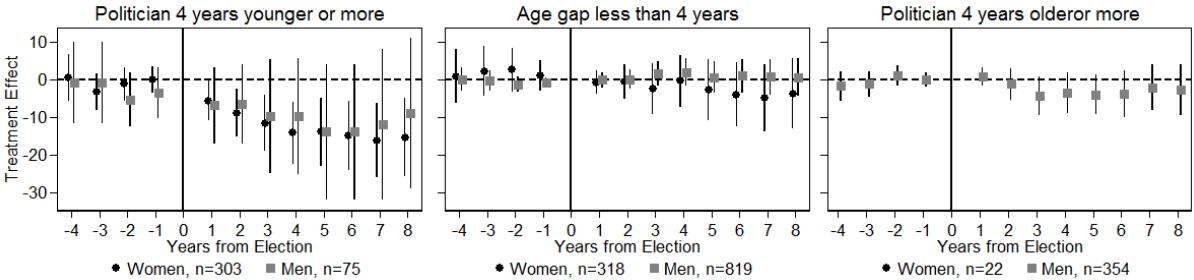


Figure 11. The effect of political promotion on remaining married in sub-samples based on the spousal age gap.

Notes: The spousal age gap is measured as the politician’s age minus the spouse’s age. The figure shows DID estimates from equation (1) for three sub-samples based on this variable. Results for women politicians who are at least four years older than their husband have been excluded due to the small sample size.

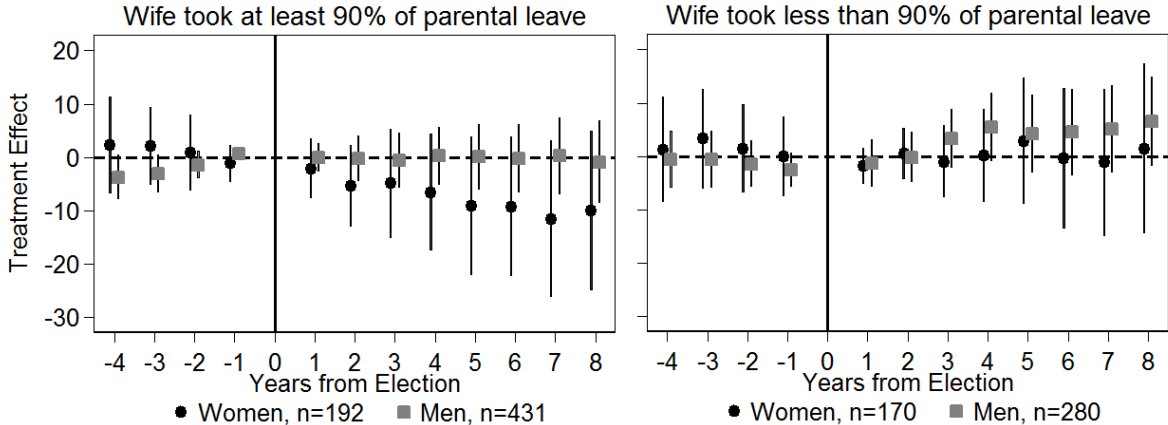


Figure 12. The effect of political promotion on remaining married in sub-samples based on the wife's share of total parental leave.

Notes: The figure shows DID estimates from equation (1) for two sub-samples based on the wife’s share of the total days of parental leave utilized by the couple. This variable is described in detail in Section 3.

The results in this section point to the importance of a mismatch between expectations about spousal behavior – in the early phases of the relationship – and actual labor market outcomes as an explanation of women’s divorces after promotion. Women in gender-traditional couples have a higher rate of divorce after being promoted to a top job, while women in more gender-equal couples do not. It appears that couples face adjustment costs when the spouse whose career was initially subordinate is promoted. Interestingly, the leftmost graph in Figure 11 suggests that *men* who are married to an older woman also face an increased probability of divorce after promotion (although these relationships remain unusual and standard errors are large). This indicates that deviations from expected economic roles, regardless of gender, can lead to divorce. Our baseline finding of women’s increased divorce rate after

promotion could therefore stem from the fact that women more often find themselves in relationships that initially focus on the career of the other person, while men do not.

A related interpretation of the findings in this section is that promoted women divorce men who are less supportive of their careers. Some additional descriptive statistics support this interpretation, while others do not. In support, promoted and divorced women are slightly more likely to be re-elected to office than promoted and divorced men. However, we find no correlation between divorce and characteristics of the husband that could proxy for a lower marriage utility for the wife.¹⁹

Economic independence and other explanations A promotion may give women economic independence from their spouse and enable them to get a divorce (e.g. Oppenheimer 1997). Potential support for this mechanism comes from research showing that marriages are more sensitive to women's than to men's economic outcomes (Heckert, Nowak, and Snyder 1998; Jalovaara 2003; Liu and Vikat 2004; Kesselring and Bremmer 2010; but c.f. Rogers 2004).

We sub-divide the sample by whether the increase in earnings from the promotion was larger or smaller than the median among promoted politicians of the same sex. We also do two sample splits to examine the relative earnings increase: being above the median for the change in earnings in proportion to the politician's own (pre-promotion) earnings, and being above the median for the change in the politician's own earnings as a proportion of total household earnings before the promotion.

The results, presented in Figure W13, suggest that economic independence is *not* a key mechanism for women's increased divorce risk. The estimates for the two groups of earnings increases, above and below the median, are similar in size and not statistically different from each other, for either absolute or relative earnings. The lack of evidence of an economic independence effect is not surprising given the level of pre-promotion earnings of the women in our sample. Most of them already had high earnings before the promotion, and many also out-earn their spouse (recall Table 1).

Some additional heterogeneity analysis can be found in the Web Appendix (Section W6). In sum, we find some evidence that a promotion that makes the wife the dominant earner in the household (>60%) is more strongly related to divorce than a promotion that drives her earnings above 50%. We do not find that divorces are concentrated in dual-earner couples, which would perhaps be expected from a mechanism whereby a promotion crowds out joint leisure time. Looking specifically at the portion of households with children under 18, we fail to detect any strong concentration of divorces to these households (the fraction of households with even younger children is too small for a meaningful analysis). Finally, splitting the sample by the median age at marriage rejects the role of limited information in

¹⁹ We tested the husbands' level of earnings prior to the promotion, the husband's education level, and the husband's cognitive and non-cognitive skills measured in Sweden's military draft (results available from the authors). Unfortunately, there are too few remarriages among promoted and divorced women and their spouses for us to examine "updated" spousal choices.

spousal choice as a potential mechanism. If anything, the divorce effect is larger for women who marry at an older age and where information is expected to be better.

7. Conclusions

We study the consequences for men's and women's relationships of being promoted to top jobs. The main result is that promotions destabilize women's marriages but not men's. This finding can help explain the persistently small proportion of women in top jobs. In our data, married women and men who obtained top jobs had both been married for 20 years, on average – quite a significant time investment. At the time of their promotion, women were substantially more likely to lose their spousal support system than men. Giving up on the relationship may very well be the woman's choice, and may be a positive outcome for her. But the results still highlight a large gender inequality in access to the first-best option for most: a functioning relationship *and* a successful career.²⁰ It is reasonable to expect that the candidate pool for top jobs would be skewed by a condition for women, but not for men, to put their relationship at risk.

Our descriptive analysis of the common features of women who divorce indicated a link between couple formation and the destabilizing role of promotions to top jobs. Couples that formed in a more gender-egalitarian manner did not experience an increased divorce rate after promotion. The divorces were instead concentrated among women in couples that focused on the husband's career in the early stages of the relationship. This result indicates a link between the marriage market and the labor market. As long as the marriage market produces mainly couples that specialize in the man's career, this pattern of couple formation may hinder gender equality in top jobs. Prioritization of the husband's career remains common around the world, even in progressive countries like Sweden (Boschini et al. 2011) and even for women at the top of the ability distribution. As long as there is little specialization in the opposite direction – households in which the wife is the dominant earner and the husband takes primary responsibility for childcare – the average woman will face greater stress in her family life when trying to obtain a very demanding top job.

These findings lead us to tentatively conclude that gender equality in top jobs should be further examined from the viewpoint of couple formation. More similar distributions of the economic roles in relationships could be a potential remedy for the persistent gender divide in career performance and earnings (e.g. Goldin 2014; Esping-Andersen 2016). Future research could explore the conditions that allow women at the top of the ability distribution to expand their choice set of partners to “marry down,” and for men to do the opposite.

²⁰ In Sweden, more than 98% of women and men in the 2010 World Values Survey reported that "Family" is "important" or "very important" in their lives.

Arguably, the Swedish welfare state and gender-egalitarian norms should provide an ideal environment for equal career opportunities. Household labor is more equally divided in Sweden than in most other countries, and affordable universal child and elder care relieves career-oriented families of substantial demands on their time. If the family is a source of tension for career women in this context, it might present even greater challenges in places with less generous policy conditions or social norms. However, a less permissive context could prohibit professional women from getting married in the first place (e.g. Bertrand et al. 2016). More research is needed to explore the joint developments of marriage and labor market across contexts and over time.

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Appendix for online publication

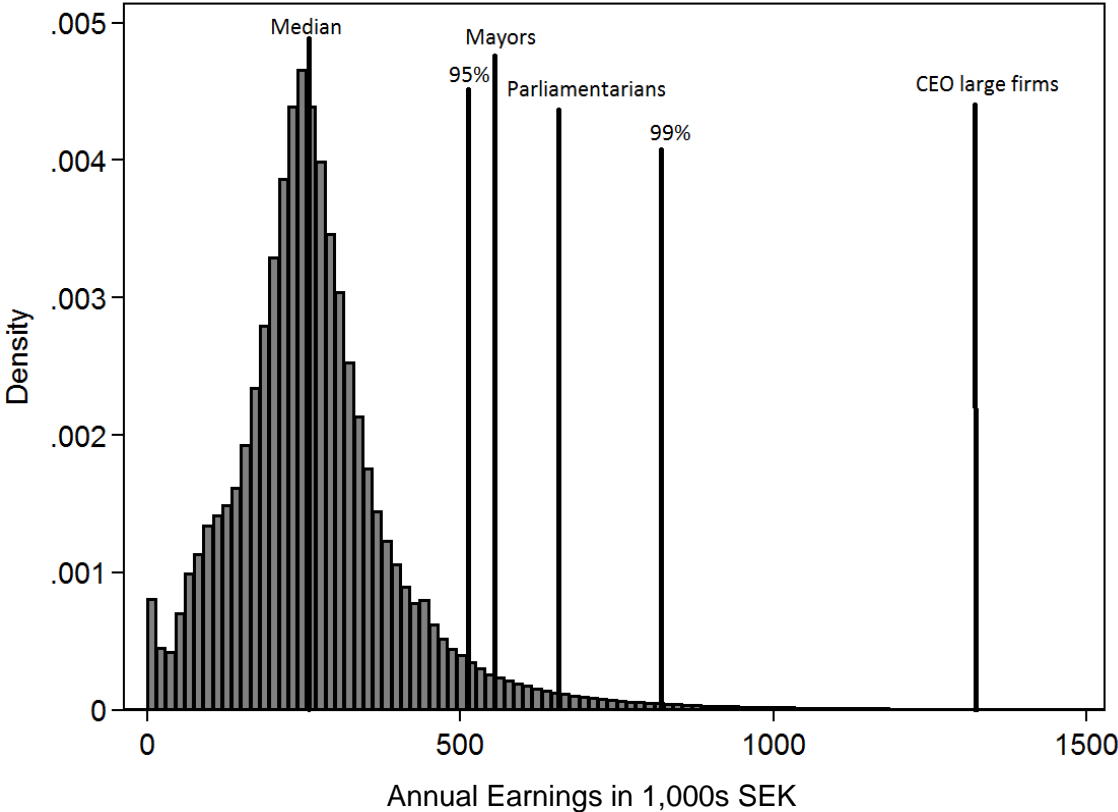


Figure W1. Placement of jobs in the distribution of annual earnings, 2011.

Note: Data for the full Swedish working-age population (20–65) that was employed in 2011.

Section W1. Measuring the division of parental leave

Ideally, we would like to measure the division of parental leave in terms of sharing time away from work. But because our dataset only includes this variable from 1993, we approximate the division of leave using the parents' share of total payments from the parental leave insurance program. Figure W2 shows the correlation between these two variables for the time period for which we have access to both (after 1993). The figure shows a clear positive correlation (0.815), with most observations clustered close to the 45-degree line.

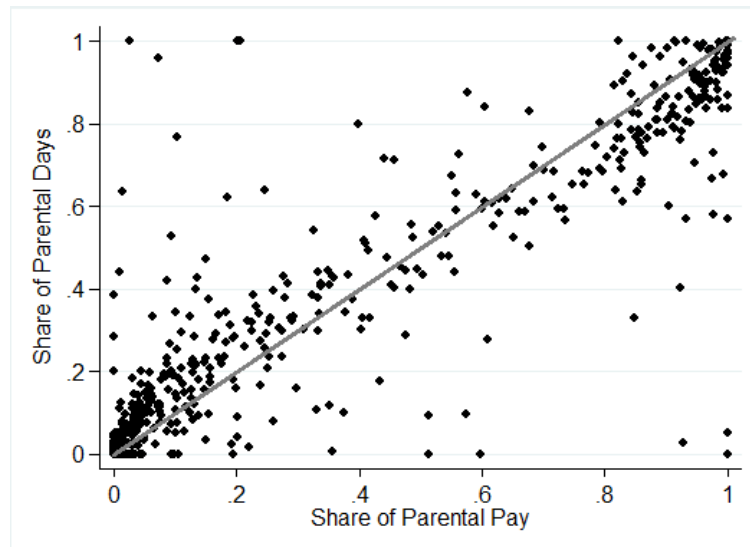


Figure W2. The politician's share of total payments to the household from the parental leave program (x-axis) and the politician's share of the household's total days of parental leave (y-axis).

Notes: N = 677. The number of days is the "net days," a variable for which half days have been merged into full days by the Swedish Insurance Agency.

Section W2. Comparison of estimation sample to the general population

In this section we compare the family structures (percent married/divorced, and percent with children) between our main estimation sample of political candidates and Sweden's general population separately for women and men. To make the comparison meaningful, we weigh the population data to match the age structure and year of observation for the politicians. This means that we compare the family structures of women politicians to women in the general population, observed in the same year and in the same birth cohort. Because our politicians are middle aged (50 years on average), we want to compare their family situations to other women of the same age, rather than women of all ages. Otherwise the comparison would reflect changes in child and marriage patterns over cohorts and over the life cycle, rather than capturing how our politician sample differs from the general population.

To construct the population average we use the following approach. For each combination of year and age, we measure the average outcome of each family structure variable in Swedish register data. For example, we record the proportion of married women among all 55-year-old women in 1992 etc. For each person in our estimation sample we then use their year of birth and year of observation in the dataset (for example the 1994 election year) to assign the population average for this combination of characteristics in a new variable. A 55-year-old female candidate observed in year 1994 will be assigned the average marriage rate of all 55-year-old women in 1994, and so on. To calculate the weighted population average, we take the average of the assigned values in the estimation sample. This automatically weights the population data to match the age and year-of-observation structure of the

estimation sample. The resulting average is the proportion of married women in the general population – but with the same age and year-of-observation structure as the women in the estimation sample.

Just as we can match the estimation sample to the population data in terms of age and year of observation, we can weigh the population data on other factors as well. In separate calculation, we add weights for education level (both male and female politicians have a higher education level than the general population) and municipality of residence (women are more likely to run in districts with a shorter commute to the Parliament in Stockholm). This gives us three different sets of weights that we use to calculate the population averages: (1) age & year of observation, (2) age, year of observation & municipality of residence and (3) age, year-of-observation & level of education. Table W1 shows these three sets of population averages in columns (2)–(4).

Table W1: Comparison of family structures between politicians and the general population.

<i>Time period</i>	<i>1991–2010</i>				<i>2002–2010</i>	
	Political candidates	Population (All occupation codes)			Political candidates	Population (Executives only)
Weighted by	None	Cohort, year	Cohort, year, municip.	Cohort, year, educ. level	None	Cohort-year
	(1)	(2)	(3)	(4)	(5)	(6)
Women						
Share divorced	0.14	0.15	0.14	0.15	0.13	0.15
Share married	0.63	0.56	0.58	0.57	0.62	0.58
Child	0.85	0.83	0.85	0.81	0.83	0.82
Child <17	0.39	0.40	0.40	0.41	0.42	0.44
Child <7	0.10	0.12	0.12	0.13	0.12	0.16
Men						
Share divorced	0.07	0.13	0.12	0.12	0.10	0.10
Share married	0.72	0.56	0.57	0.59	0.67	0.66
Child	0.85	0.77	0.78	0.78	0.82	0.85
Child <17	0.43	0.39	0.40	0.41	0.43	0.46
Child <7	0.13	0.13	0.12	0.14	0.14	0.17

Notes: The table compares the shares of divorced and married women and men political candidates (Column 1) to the full Swedish population weighted to match the politicians' i) distribution of birth cohorts and years of political candidacy (Column 2), ii) birth cohort, year and municipality of residence (Column 3), and iii) birth cohort, year and education level (Column 4). Column 5 shows the proportion of divorced and married men and women political candidates in years 2002–2010. Column (6) shows those statistics for the full population of people with ISCO occupation codes with a first digit of 1, i.e. executive jobs.

In the two rightmost columns of the Table (5 and 6), we compare the estimation sample of political candidates to people with senior positions in other sectors. We restrict the population data to people with executive occupations (having an ISCO occupation code with a first digit of 1). Since occupation codes are only available starting in 2002, we show the averages of the family structure variables for politicians in this time period in column (5) and for executives – matched on age and year of observation – in column (6).

Section W3. Defining close elections in proportional representation systems

There are complexities to measuring close elections in a proportional representation system. One challenge is that the seat share of a single party, or bloc of parties, is not a deterministic function of the vote share; it is jointly determined by the allocation of votes among parties. To measure the closeness of elections, we therefore rely on two different methods: one for municipal elections and another for parliamentary elections.

W3.1 Municipal elections

To measure electoral closeness at the municipal level, we use a simulation-based approach that builds on Fiva et al. (2018). The approach and code developed in this paper has also been applied in Folke et al. (2017). This approach constructs a forcing variable, which is continuous (rather than discrete, as is the seat share), and which does not sort or give a low density of observations close to the threshold of winning more than 50% of the seats. This simulated forcing variable takes two important features of the electoral system into account. The first is that a municipality may contain multiple electoral districts of different sizes. The second feature is that shifting a vote to (or from) one bloc to the remaining parties has a different impact on the seat share of the bloc winning (or losing) the vote, depending on which party within the winning and losing bloc won or lost it, respectively.

Our simulation departs from data on electoral outcomes. We want to measure how close the election is by capturing which shift of votes to or from a political bloc would have caused (1) a winning bloc to lose its seat majority or (2) a losing bloc to gain a majority of seats. For each election, we will thus have two forcing variable values, one for each bloc. When we measure closeness for a certain bloc, the other bloc always includes local parties.¹ The two closeness variables are measured in percentage terms, answering the question "which percentage of votes was needed, in a specific election, to give (or take) the seat majority from each of the two political blocs?"

For a bloc that won a seat majority, we start from the electoral result in the relevant election and move successively in the negative direction, incrementally *removing* 0.01 percentage points of the bloc's

¹ In Sweden, local parties – defined as not having representation in Parliament – hold, on average, 2% of the municipal assembly seats.

votes, starting from 0.01, 0.02, etc. For a losing bloc we do the opposite, adding small increments of votes. The goal is to find out, for each bloc at a particular time, what percentage of votes would have needed to move in order to shift the seat majority to the other bloc.

How does our simulated shift in votes affect the distribution of seats? The impact will of course differ between countries depending on the electoral system. In Sweden, seats are distributed based on the highest averages method, using a modified St. Lagu  formula. After shifting a small proportion of votes either to or from a bloc, we use this formula to compute the new seat distribution. For each shift of votes, we randomly simulate 1,000 alternatives for how that specific percentage of votes, for example 0.02%, shifted in terms of receiving and losing (1) parties and (2) districts. Each time, we also compute the new allocation of seats. In this simulation, we assume that large parties have a greater variance in their vote shares than small parties, but that the variance is not 100% proportional. The simulations also abstract from the fact that votes can shift between parties within a bloc.² Having computed the new seat allocation for each of the 1,000 shifts of the vote distribution, we tally the number of times the bloc either lost (for winning blocs) or won (for losing blocs) the seat majority under the new distribution. Out of all the simulations for each shift in the vote share, we then set the value of the forcing variable to the size of the smallest vote shift that caused a shift in the bloc's majority status in at least 50% of the 1,000 vote shifts.

Figure W3 illustrates the process of creating the forcing variable in the example of the municipality of Upplands V sby in the 2006 election. In this municipality, the center-right bloc won the governing majority, receiving 52.7% of the votes and 54.9% of the seats. The left bloc won 42.9% of the votes and 43.1% of the seats. Suppose that we want the value of the forcing variable for the left bloc, i.e., the minimum proportion of votes the bloc would need to win in order to gain a majority of seats. The *x*-axis in the figure shows the proportion of votes shifted, and the *y*-axis shows the proportion of times, out of our 1,000 simulated vote shifts, that caused the left bloc to win at least 50% of the seats. The upward slope of the line indicates that the larger the proportion of votes that shifted to the bloc, the greater the probability of a 50% seat shift. As illustrated by the vertical line, the left bloc gains a seat majority in about half of the simulations when we give it an additional 5.0 percentage points of the votes. This assigns the value of the forcing variable to 5.0% for the left bloc in this election.

² In the first step, we take a random number between 0 and 1 for each party in the giving and receiving blocs. We then multiply this random proportion by the party's vote share plus a constant of 0.1. For a party with a random shock of 0.4 and a 20% vote share, we thus calculate $0.4 \cdot (0.2 + 0.1)$. We call this variable q . Within each bloc, we then normalize the parties' q values so that they sum to 1, calculating $q_w = \frac{q_p}{\sum_1^p q_p}$, where q_p are the initially computed party shocks and q_w are the normalized shocks. The next step is to subtract fractions of the vote shift, for example 0.01 percentage points of the total votes, from one bloc and reward it in fractions to the other parties in a way that corresponds to the randomly drawn shocks. Finally, a new vote allocation is used to calculate the seat allocation, using the Swedish election formula.

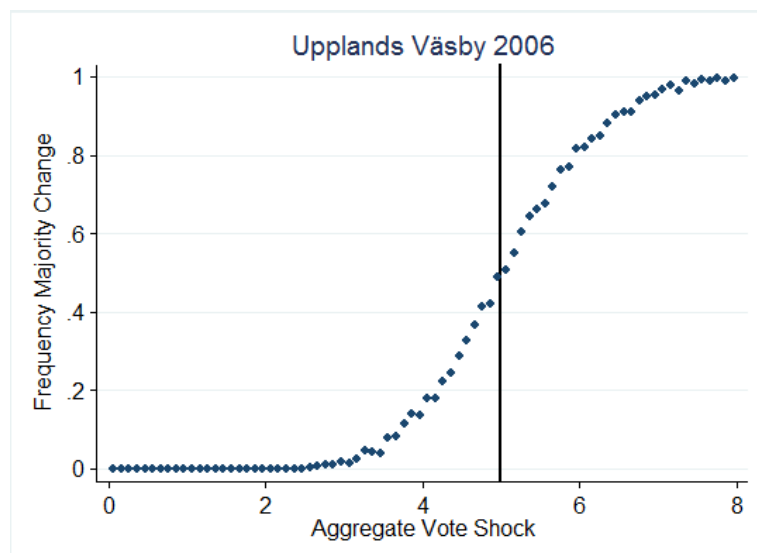


Figure W3. Illustration of simulation strategy.

Note: The figure shows the proportion of seat majority shifts to the left bloc (y-axis) based on 1,000 simulations of shifting a given proportion of votes from the center-right bloc to the left bloc (x-axis). The data used for the illustration comes from the Upplands Väsby municipality in the 2006 election.

A general concern with regression discontinuity designs is that the density of the forcing variable is not smooth across the threshold. A higher density of observations on either side of the seat threshold indicates that the treatment is not random, or that the forcing variable is wrongly specified in some way. In Figure W4, we show that this is not the case for our analysis. For both forcing variables, the frequency of observations is smooth as we cross the seat majority threshold.

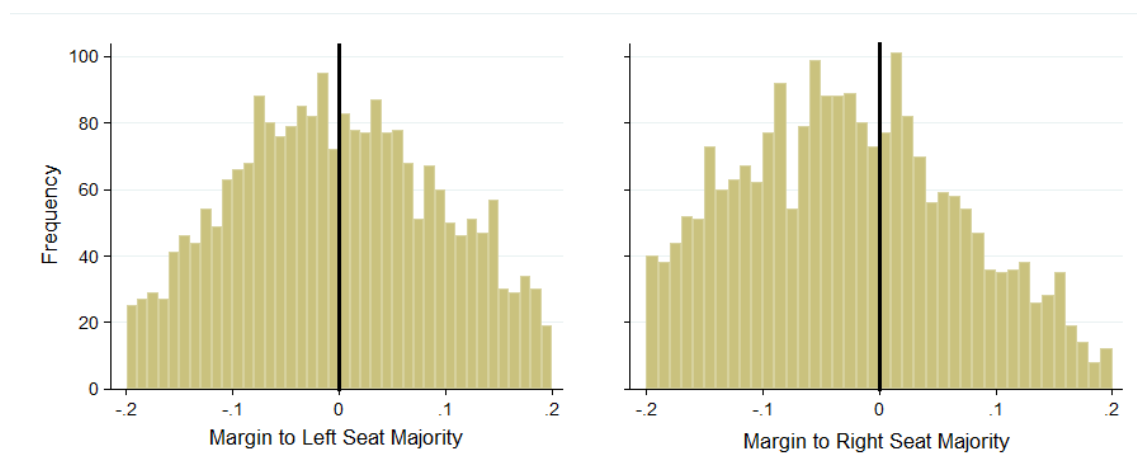


Figure W4. Frequency of observations, as a function of the margin to a seat majority for the left bloc (left-hand graph) and the right bloc (right-hand graph).

Note: Each bar corresponds to 0.01 units of the margin to the seat majority.

W3.2 Parliamentary elections

To define close elections for parliamentary seats, we follow the simulation approach suggested by Freier and Odendahl (2015) and use simulations to define close elections. There are two reasons for not using the same approach as at the municipal level. First, the seats are allocated in two rounds at two different levels, which makes it very technically complicated to implement our municipal-level approach. Second, using the vote share to define close elections would also mean that we would have to adjust the interval to define close elections for the smallest parties.

This approach is similar to the method we use at the municipal level. We start with the actual seat and vote allocation. We then simulate a large number (10,000) of likely vote changes, which allows for vote changes at both the national and local levels.³ For each new vote allocation, we calculate the seat allocation. The closeness of the election is measured by the frequency of seat changes. If a party loses a seat in at least 30% of the simulations, we define that party as being close to losing a seat, and if it loses a seat in 40% of the simulations we define it as being very close to losing a seat. See Freier and Odendahl (2015) for a more detailed description of this approach.

³ In the first step, we start with a party's actual vote share at the national level. We then add a vote shock at the national level. This shock is normally distributed with a mean of zero and a standard deviation that is defined as the vote share of the party times 0.2 plus a constant of 0.02. For a party with a 10% vote share, the standard deviation of the vote shock will thus be 4 percentage points.

In the next step, we allocate this vote shock to the districts by multiplying the districts' share of the parties' vote share multiplied by a random number that has a uniform distribution between 0 and 1. This gives us the variable q . For each party, we then normalize the q values across districts so they sum up to 1. These normalized q values decide how large a share of the national vote shock goes to a district.

We then add a shock at the *district* level. Again, this shock is also normally distributed with a mean of zero and a standard deviation that is defined as the vote share of the party times 0.2 plus a constant of 0.02. Within each district, we normalize the shocks across parties so that the total vote change in the district is zero.

We then add the national and district-level vote shocks to the initial votes. Finally, we distribute the seats according to the new vote distribution using the Swedish election formula, and calculate how often the party gains or loses a seat.

Table W2: Comparison of pre-promotion traits in the sub-sample of close elections.

Subsequently promoted	Women		Men	
	Yes	No	Yes	No
<i>Couple characteristics</i>				
Politician's share of earnings (%)	0.59	0.55	0.69	0.66
Politician's share of parental leave (%)	0.82	0.81	0.14	0.15
Age difference (politician-spouse)	-4.19	-4.13	1.62	2.05
Politician out-earns spouse (%)	0.67	0.58	0.91	0.89
Marriage length (years)	20.63	20.32	20.34	20.47
Second marriage (%)	0.02	0.03	0.02	0.02
Same birth region (%)	0.90	0.91	0.94	0.94
Has children (0–17)	0.37	0.41	0.48	0.41
Has children (0–6)	0.09	0.13	0.12	0.10
<i>Individual characteristics</i>				
Politician's age	48.40	48.44	49.64	50.48
Politician's age at marriage (1)	28.29	28.12	29.54	30.13
Politician's earnings (2)	355.3	310.2	375.3	367.3
Politician's tertiary education (%)	0.66	0.72	0.54	0.54
Spouse's age	53.13	52.56	48.27	48.55
Spouse's earnings	295.1	283.1	174.8	188.6
Spouse's tertiary education (%)	0.45	0.46	0.54	0.57
Observations	122	151	255	298

Notes: Bold letters represent differences between promoted and non-promoted individuals of the same sex at the 5% level or lower, using ordinary least squares (OLS) regressions with standard errors clustered at the individual level. The definition of close elections is described in Section W2. Measurement details for the variables in the table can be found in Section 3.

Table W3. Differences-in-differences estimate of the effect of political promotion on remaining married, including alternative definitions of close elections, for women married as of four years prior to the election.

	Full sample	Close elections				
Def close mun.		7%	6%	5%	4%	3%
Def close parl.		.2 prob	.25 prob	.3 prob	.35 prob	.4 prob
	(1)	(2)	(3)	(4)	(5)	(6)
Treat*t-4	0.65 (2.39)	-0.48 (2.48)	0.71 (3.13)	2.00 (3.27)	4.38 (3.76)	6.17 (3.79)
Treat*t-3	-0.32 (2.12)	-1.34 (2.14)	0.87 (2.77)	1.71 (2.84)	3.80 (3.19)	3.86 (3.20)
Treat*t-2	0.97 (1.89)	-0.46 (1.86)	0.38 (2.38)	0.86 (2.57)	2.79 (2.85)	2.83 (2.68)
Treat*t-1	0.63 (1.39)	0.60 (1.44)	0.76 (1.81)	1.15 (1.91)	1.71 (2.08)	1.81 (1.87)
Election year = reference category						
Treat*t+1	-3.36** (1.53)	-2.79* (1.50)	-2.64 (2.02)	-0.44 (1.83)	-0.35 (2.24)	-0.09 (2.84)
Treat*t+2	-4.78** (1.94)	-4.20** (1.96)	-6.24** (2.78)	-4.31 (2.86)	-3.23 (3.24)	-3.07 (3.69)
Treat*t+3	-7.04*** (2.47)	-6.09** (2.51)	-7.68** (3.36)	-6.08* (3.53)	-5.26 (4.11)	-3.22 (4.58)
Treat*t+4	-7.46*** (2.69)	-6.57** (2.76)	-7.59** (3.61)	-5.07 (3.79)	-3.91 (4.41)	-1.11 (5.00)
Treat*t+5	-8.41*** (3.01)	-7.71** (3.07)	-8.02* (4.13)	-5.89 (4.49)	-5.33 (5.20)	-1.40 (5.71)
Treat*t+6	-9.52*** (3.11)	-9.08*** (3.15)	-8.54** (4.15)	-6.25 (4.51)	-6.57 (5.30)	-2.77 (5.81)
Treat*t+7	-10.39*** (3.33)	-10.33*** (3.40)	-7.41 (4.61)	-5.11 (4.92)	-6.04 (5.76)	-1.73 (6.49)
Treat*t+8	-9.53*** (3.48)	-9.25** (3.58)	-6.33 (4.84)	-4.40 (4.99)	-5.19 (5.83)	-0.40 (6.59)
Observations	7,780	6,839	4,205	3,571	2,929	2,339

Notes: Robust standard errors clustered at the level of the candidate in parentheses*** p<0.01, ** p<0.05, * p<0.1 Details about the estimation and content of the table are available in the notes below Figure 5 in the main text.

Table W4. Differences-in-differences estimate of the effect of political promotion on remaining married, including alternative definitions of close elections, for men married as of four years prior to the election.

Def close mun. Def close parl.	Full sample	Close elections				
	(1)	7% .2 prob (2)	6% .25 prob (3)	5% .3 prob (4)	4% .35 prob (5)	3% .4 prob (6)
Treat*t-4	-0.63 (1.21)	-0.47 (1.26)	-0.49 (1.73)	-0.81 (1.92)	-0.04 (2.16)	0.59 (2.50)
Treat*t-3	-0.72 (1.07)	-0.44 (1.11)	-1.23 (1.48)	-1.25 (1.68)	-1.34 (1.95)	0.04 (2.31)
Treat*t-2	-0.91 (0.82)	-0.43 (0.84)	-1.08 (1.06)	-1.34 (1.24)	-1.31 (1.45)	-0.75 (1.70)
Treat*t-1	-0.67 (0.50)	-0.17 (0.45)	-0.48 (0.59)	-0.56 (0.66)	-0.72 (0.83)	-0.82 (0.99)
Election year = reference category						
Treat*t+1	-0.26 (0.80)	-0.31 (0.79)	-0.68 (1.12)	0.20 (1.00)	1.07 (1.05)	2.45** (1.14)
Treat*t+2	-0.55 (1.09)	-0.36 (1.10)	-0.37 (1.55)	0.65 (1.59)	1.29 (1.77)	2.01 (2.03)
Treat*t+3	-0.60 (1.39)	-0.69 (1.43)	0.15 (2.01)	1.46 (2.04)	1.60 (2.24)	3.93 (2.51)
Treat*t+4	-0.06 (1.50)	-0.54 (1.54)	0.45 (2.16)	1.85 (2.25)	1.65 (2.46)	4.33 (2.78)
Treat*t+5	-1.13 (1.66)	-1.84 (1.71)	-0.73 (2.38)	0.71 (2.53)	0.62 (2.79)	3.31 (3.27)
Treat*t+6	-0.84 (1.77)	-1.65 (1.81)	-0.72 (2.50)	0.12 (2.63)	0.00 (2.90)	2.05 (3.59)
Treat*t+7	-0.41 (1.89)	-1.40 (1.93)	-0.58 (2.65)	0.42 (2.76)	0.39 (2.95)	2.67 (3.64)
Treat*t+8	-0.52 (1.99)	-1.59 (2.03)	-1.25 (2.79)	-0.22 (2.94)	0.20 (3.16)	1.82 (3.80)
Observations	15,481	14,315	8,286	7,224	6,127	4,679

Notes: Robust standard errors clustered at the level of the candidate in parentheses*** p<0.01, ** p<0.05, * p<0.1 Details about the estimation and content of the table are available in the notes below Figure 5 in the main text.

Table W5. Differences-in-differences estimate of the effect of political promotion on remaining married, including alternative definitions of close elections, for women married as of four years prior to the election.

	Full sample	Close elections				
Def close mun.		7%	6%	5%	4%	3%
Def close parl.		.2 prob	.25 prob	.3 prob	.35 prob	.4 prob
	(1)	(2)	(3)	(4)	(5)	(6)
Election year = reference category						
Treat*t+1	-3.63** (1.65)	-3.27** (1.62)	-3.35 (2.14)	-1.28 (1.96)	-1.42 (2.42)	-1.31 (2.96)
Treat*t+2	-5.36** (2.10)	-5.22** (2.12)	-7.78*** (2.96)	-6.07** (3.05)	-5.47 (3.50)	-5.54 (3.90)
Treat*t+3	-8.16*** (2.54)	-7.61*** (2.56)	-9.94*** (3.42)	-8.68** (3.57)	-8.65** (4.18)	-6.77 (4.52)
Treat*t+4	-8.01*** (2.77)	-7.31** (2.83)	-9.00** (3.70)	-7.20* (3.84)	-6.82 (4.47)	-4.11 (4.95)
Treat*t+5	-9.41*** (3.04)	-8.51*** (3.13)	-9.54** (4.25)	-8.05* (4.55)	-8.43 (5.27)	-4.38 (5.70)
Treat*t+6	-10.51*** (3.16)	-9.85*** (3.22)	-10.02** (4.28)	-8.37* (4.59)	-9.64* (5.39)	-5.82 (5.83)
Treat*t+7	-11.36*** (3.45)	-11.80*** (3.52)	-8.95* (4.64)	-7.38 (4.99)	-9.36 (5.82)	-5.21 (6.47)
Treat*t+8	-10.43*** (3.58)	-10.65*** (3.69)	-7.83 (4.81)	-6.66 (5.05)	-8.51 (5.89)	-4.00 (6.54)
Observations	5,216	4,537	2,826	2,423	1,994	1,609

Notes: Robust standard errors clustered at the level of the candidate in parentheses*** p<0.01, ** p<0.05, * p<0.1 Details about the estimation and content of the table are available in the notes below Figure 5 in the main text.

Table W6. Differences-in-differences estimate of the effect of political promotion on remaining married, including alternative definitions of close elections, for men married in the election year.

	Full sample	Close elections				
Def close mun.		7%	6%	5%	4%	3%
Def close parl.		.2 prob	.25 prob	.3 prob	.35 prob	.4 prob
	(1)	(2)	(3)	(4)	(5)	(6)
Election year = reference category						
Treat*t+1	-0.31 (0.81)	-0.33 (0.82)	-0.66 (1.10)	0.19 (0.98)	1.04 (1.04)	2.36** (1.12)
Treat*t+2	-0.67 (1.10)	-0.48 (1.12)	-0.71 (1.54)	0.25 (1.59)	0.83 (1.79)	1.31 (2.09)
Treat*t+3	-0.91 (1.40)	-1.01 (1.45)	-0.17 (1.98)	1.06 (2.01)	1.19 (2.24)	3.21 (2.55)
Treat*t+4	-0.62 (1.52)	-1.06 (1.56)	-0.21 (2.15)	1.08 (2.25)	0.87 (2.49)	3.06 (2.85)
Treat*t+5	-1.44 (1.65)	-2.17 (1.71)	-1.18 (2.32)	0.33 (2.49)	0.35 (2.77)	2.83 (3.25)
Treat*t+6	-1.04 (1.77)	-2.00 (1.80)	-1.16 (2.45)	-0.24 (2.59)	-0.23 (2.88)	1.62 (3.57)
Treat*t+7	-1.09 (1.91)	-2.22 (1.95)	-1.80 (2.66)	-0.79 (2.79)	-0.41 (2.99)	1.59 (3.67)
Treat*t+8	-1.41 (2.02)	-2.62 (2.06)	-2.75 (2.82)	-1.79 (2.99)	-1.06 (3.23)	0.16 (3.88)
Observations	10,918	10,087	5,856	5,097	4,271	3,237

Notes: Robust standard errors clustered at the level of the candidate in parentheses*** p<0.01, ** p<0.05, * p<0.1 Details about the estimation and content of the table are available in the notes below Figure 5 in the main text.

Section W4. Additional Sensitivity Tests

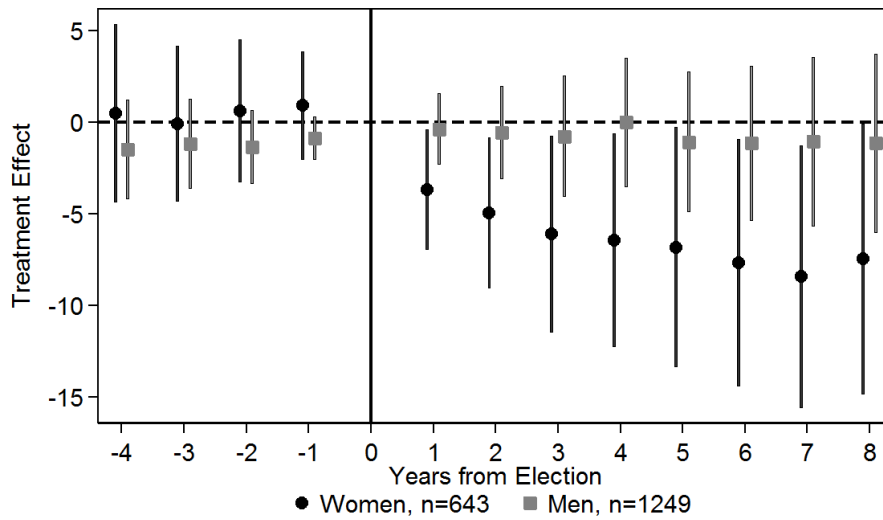


Figure W5. The effect of political promotion on remaining married, including control variables.

Notes: The figure shows results from estimating the main result (the bottom graph in Figure 5) after including as control variables all the variables listed in Table 1, Section 3, with the exception of the division of parental leave, which is excluded due to missing data. Details about the estimation and content of the figure are available in the notes below Figure 5 in the main text.

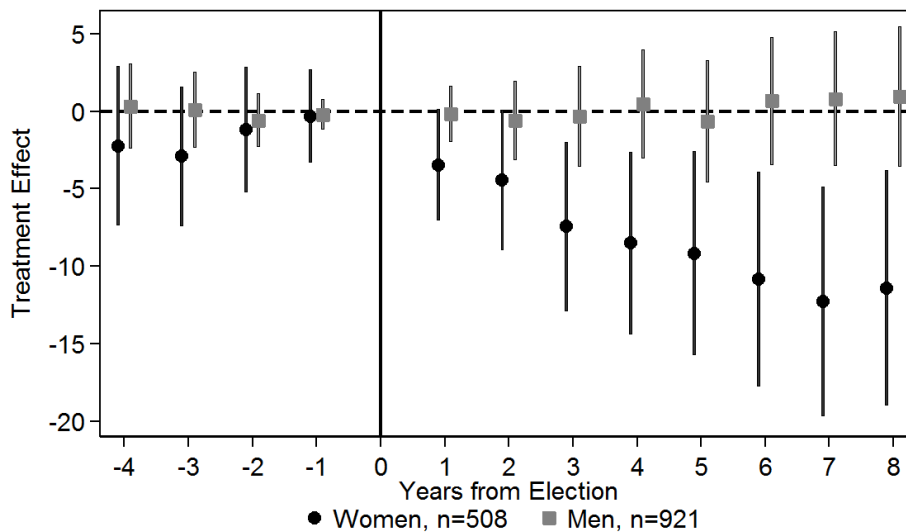


Figure W6. The effect of political promotion on remaining married, excluding repeat losers from the sample.

Notes: The figure shows results from estimating the main result (the bottom graph in Figure 5) after excluding observations for politicians who previously appeared in the sample as a losing job candidate. Details about the estimation and content of the figure are available in the notes below Figure 5 in the main text.

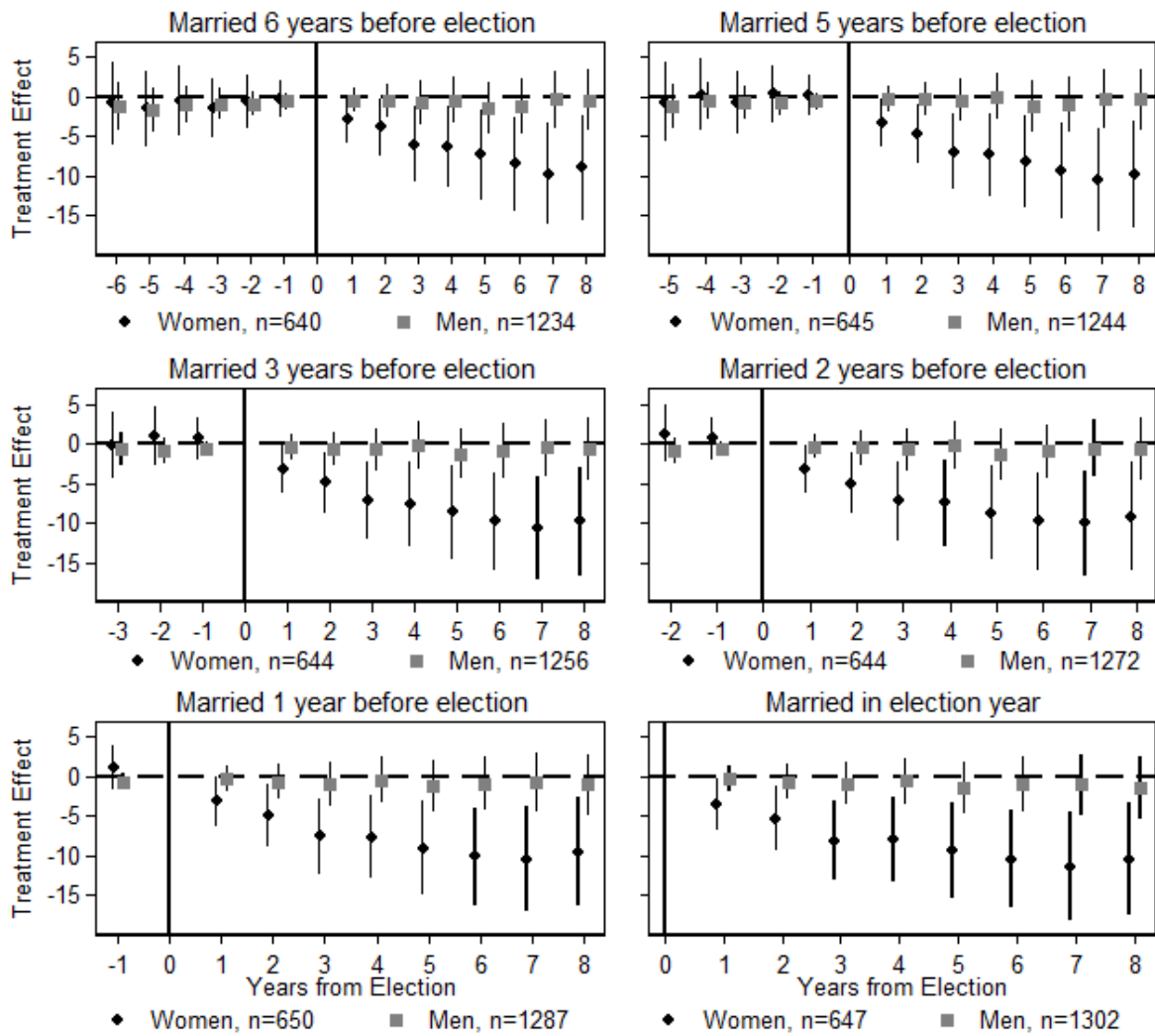


Figure W7. The effect of political promotion on remaining married, different start years.

Notes: Details about the estimation and content of the figure are available in the notes below Figure 5 in the main text. The plots in the figure vary the starting year for the sample selection procedure described in Section 2.1, from six years prior to the election that assigns the promotion, and to the year of the election. The starting year used in the main analysis, $t=-4$, is excluded.

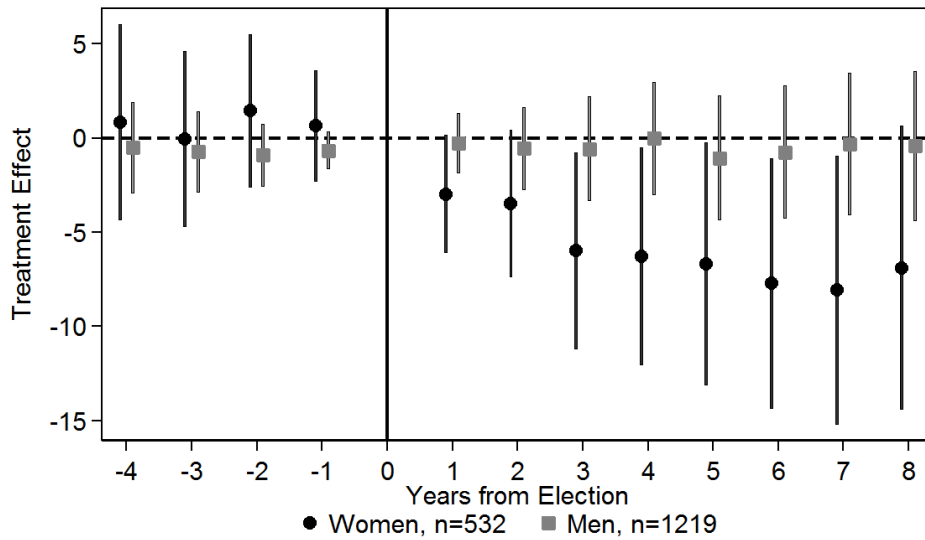


Figure W8. The effect of political promotion on remaining married, excluding candidates with retired (>65 years old) or retiring spouses.

Notes: The figure shows results from estimating the main result (the bottom graph in Figure 5) after excluding politicians whose spouse (or ex-spouse) reached the age of 65 during the election period after the promotion, i.e. between $t=1$ and $t=4$. Details about the estimation and content of the figure are available in the notes below Figure 5 in the main text.

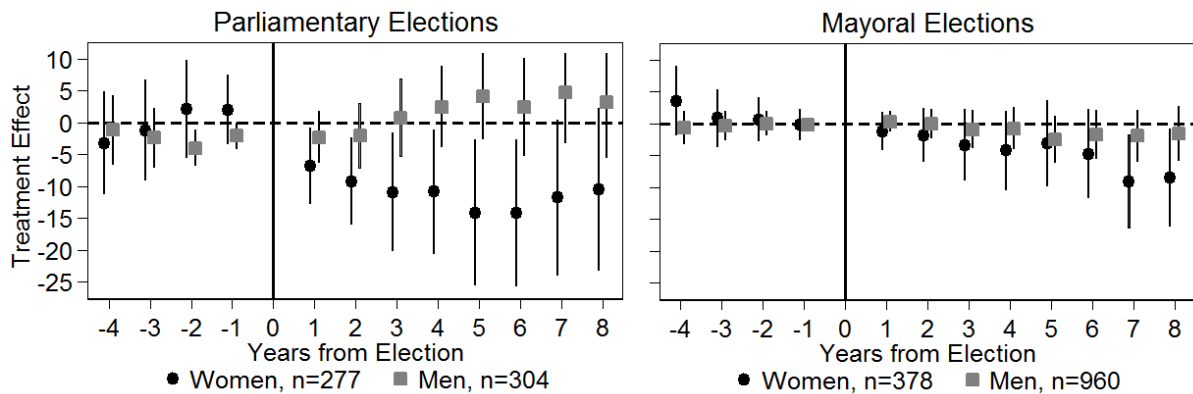


Figure W9. The effect of political promotion on remaining married, estimated separately in parliamentary (left-hand figure) and municipal sample (right-hand figure).

Notes: The figure shows results from estimating the main result (the bottom graph in Figure 5) separately for parliamentarians and mayors. Details about the estimation and content of the figure are available in the notes below Figure 5 in the main text.

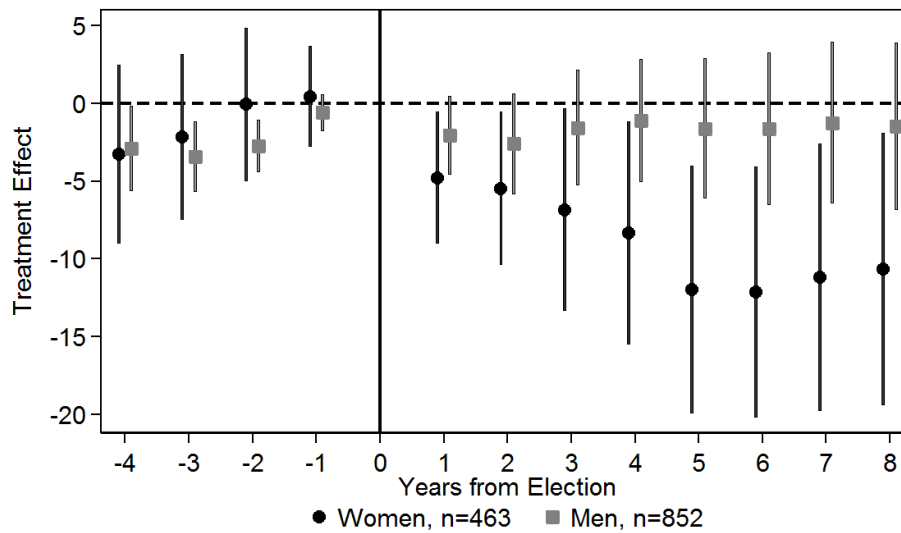


Figure W10. The effect of political promotion on remaining married, alternative definition of municipal treatment that eliminates coalition bargaining as a factor determining the party of the mayor.

Notes: The figure shows results from estimating the main result (the bottom graph in Figure 5) after excluding excludes municipal elections in which the treatment could have been determined by coalitional bargaining. Details about the estimation and content of the figure are available in the notes below Figure 5 in the main text.

Section W5. Career success and divorce in four occupations

To examine the external validity of our finding that women with successful careers are more likely to divorce than men with successful careers, we turn to four additional occupations in which, arguably, persons with the same education type have comparable career paths that occur within the same types of organizations: medical doctors, police, priests, and pharmacists.⁴ Although we cannot measure the exact timing of promotion events for these occupations, we can use income to determine who has had a successful career. As a comparison, we also report the proportions of divorced men and women in the occupations analyzed above: CEOs, mayors, and parliamentarians.

From our dataset that covers the full Swedish working-age population, we select all individuals who graduated from the relevant education programs between 1989 and 1993. In order to only compare the career trajectories of persons who remained in the occupation, we drop those who earned a degree in another field at any point until 2012. We also remove individuals who retired before 2012.

Divorce rates and career outcomes are measured within occupation-gender groups 20–23 years after graduation. Within each occupation, we compute the median of annual earnings in 2010–2012. We then benchmark each individual's average annual earnings to those of his or her peers. We denote persons who have reached a level of annual earnings above the median as having had a "high" career performance, and those below the median as having had a "low" performance. We compute the proportion of divorced persons by gender and occupation, divided by their career performance (high or low) and report these proportions in Figure W11.

⁴ We use industry codes for the post-graduation period to check if this is the case. The data show that for medical doctors, 92% of the year-individual observations have industry codes within medical care (2-digit SNI92 of 85). For the Police Academy, 93% of the observations are found in the police force industry code (4-digit SNI92 of 7425). Priests are found in religious organizations, as 75% of the observations occur in the 4-digit code of 9131. Finally, for pharmacists, 57% of the data is found in the 3-digit code 244 (medical companies) or the 5-digit code of 52310 (pharmacies).

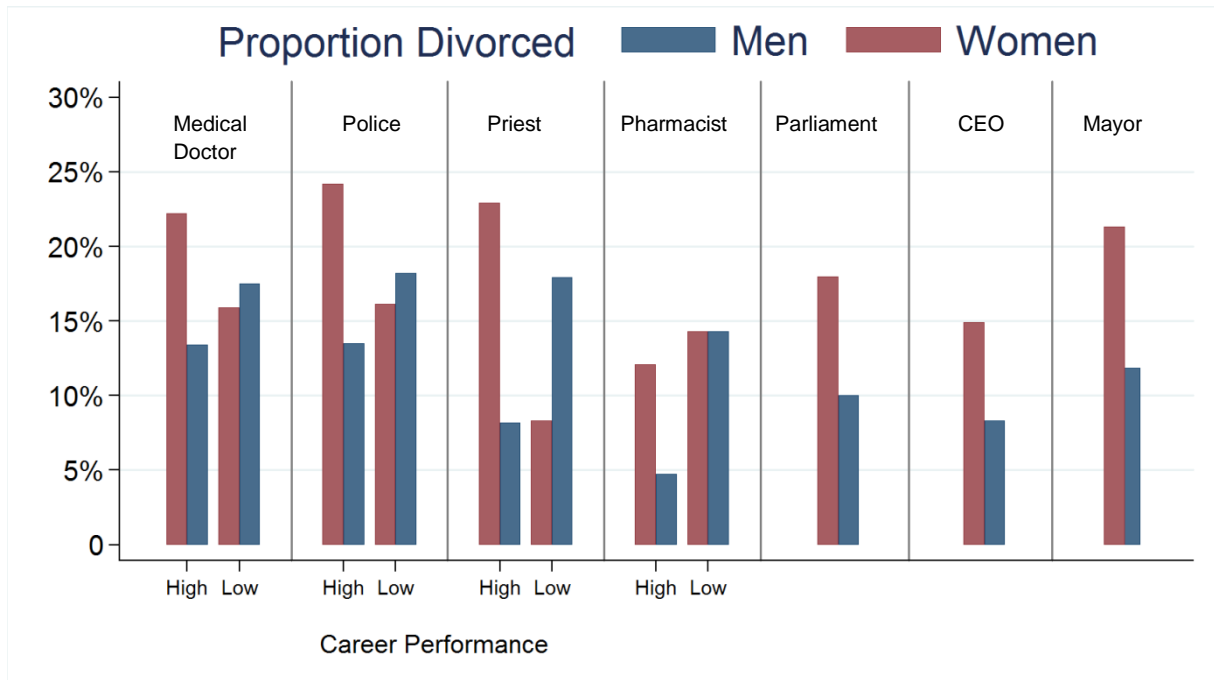


Figure W11. Proportion of divorced individuals by gender, occupation, and career performance.

Notes: For the first four occupations, career performance and divorce are measured in the 20–23rd year of the individual’s career. Parliamentarians and mayors form a pooled sample of the 1991–2010 election periods. CEOs in firms with more than 100 employees represent a pooled sample for 2002–2012.

The descriptive statistics indicate that our baseline findings reach beyond the political sector. Among all the professions, women are more likely to be divorced than men. But there is a striking gender difference between persons who have reached high and low levels of earnings. For men, the divorce rate is consistently higher among those with below-median earnings across all four occupations. For women, divorce is instead more common among high earners than low earners within the same profession. The only profession that does not show this pattern for women is pharmacists.

Section W6. Additional mechanism analysis

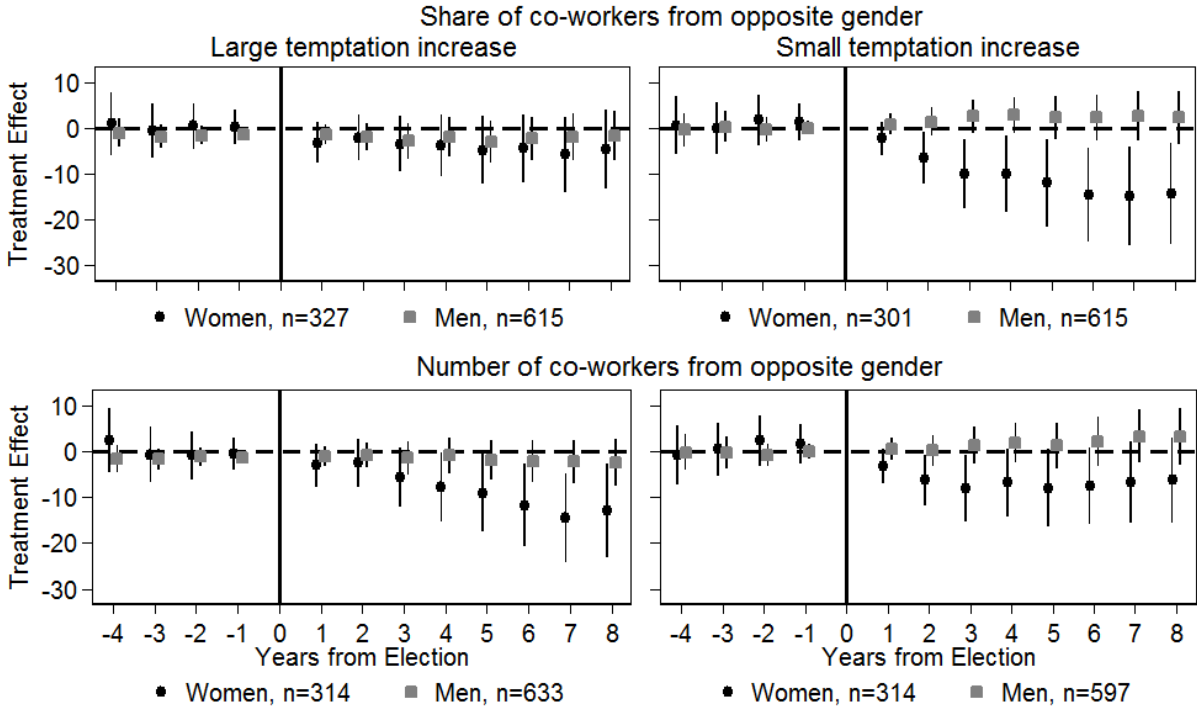


Figure W12. The effect of political promotion on remaining married in sub-samples based on the pre-promotion share of co-workers of the opposite sex.

Notes: Details about the estimation and content of the figure are available in the notes below Figure 5 in the main text. The sample is sub-divided in two ways. For each political job candidate, we depart from the plant-level workplace ID code (cfar in LISA). We then compute two variables for the total number of other people who have the same code in that year: the share of employees of the opposite sex from the politician, and the number of employees of the opposite sex. The sample is then split by the median of each of these variables. Only workplaces with more than 10 employees are included, and the sample size is also reduced when non-employed people are automatically dropped from the sample.

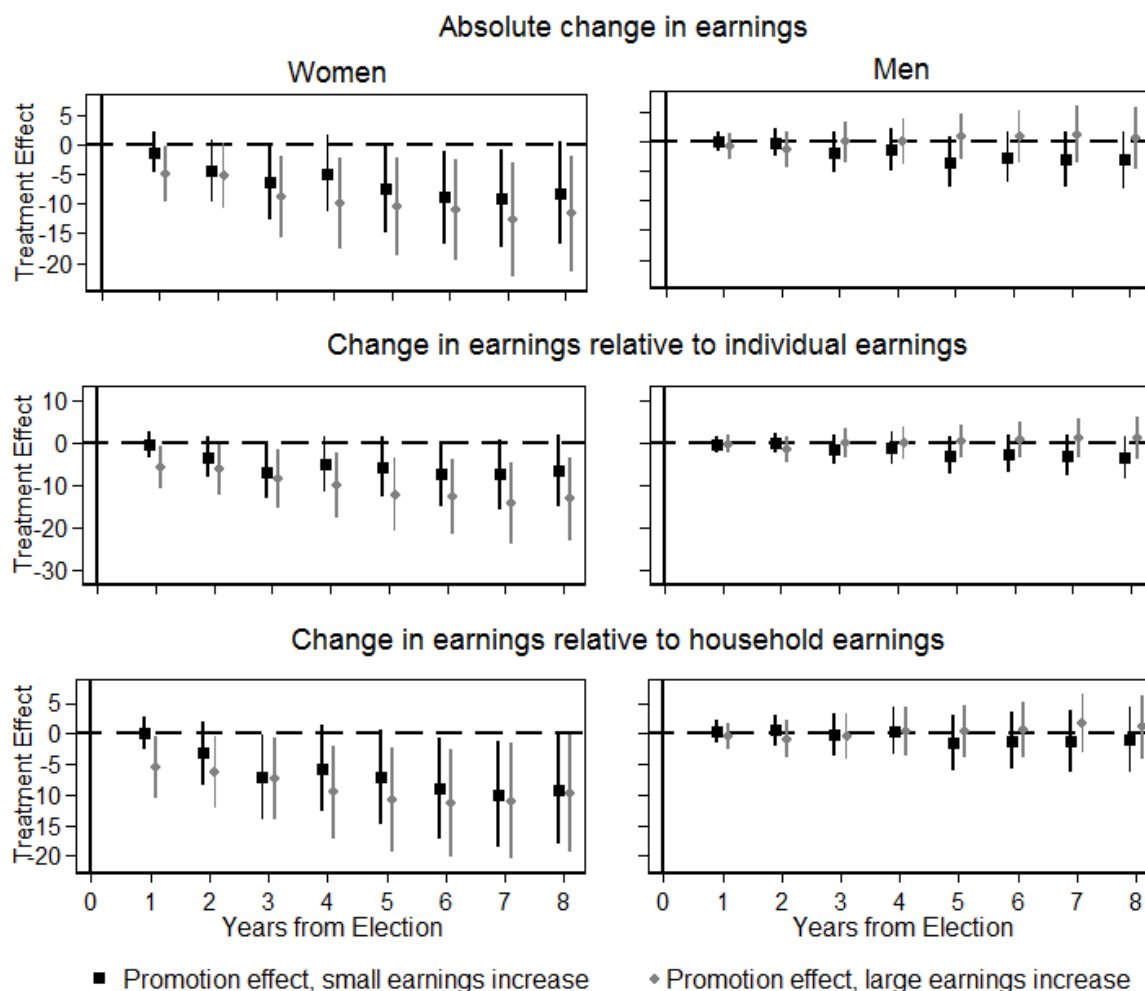


Figure W13. The effect of political promotion on remaining married differentiated by the size of the promoted politicians' increase in earnings.

Notes: Details about the estimation and content of the figure are available in the notes below Figure 5 in the main text. The DID estimates come from an expanded version of equation (1) in which the promotion treatment variable is subdivided into two different dummy variables before being included in the specification. This division is done in three ways, creating three sets of two mutually exclusive treatments. The first set of treatments is to be promoted and to have an earnings increase above the median of promoted politicians of the same sex. The second set is to be promoted and to have an earnings increase below the median. Earnings increases are measured as (1) the difference in the sum of earnings before and after the promotion, (2) the difference in relative earnings before and after the promotion, and (3) the difference in earnings before and after the promotion as a share of total household earnings. The estimated treatment effects of having earnings increases above the median are shown by gray markers, and the estimated treatment effects from below-median increases are shown with black markers. Since earnings relative to the household's total earnings can only be measured for married couples, and we measure this ratio in the years just prior to the promotion, we use the sample of politicians married in the election year. For comparability, the other two sample splits are also done on this same sample.

Norms on relative earnings A promotion may move the division of household earnings in a more or less specialized direction, which could affect the utility of the marriage. Recent work on identity economics has argued that individuals receive utility by complying with norms on this division, and lose utility if they break those norms (Akerlof and Kranton 2000; Bertrand, Pan, and Kamenica 2015). If

norms prescribe that “the husband should earn more than the wife,” promotions that cause couples to break this norm could be particularly harmful to marriage durability.

To investigate whether promoted men and women who break this norm are driving the main result, we subdivide the treatment variable into two separate dummy variables. Both of these treatment dummies are then put into equation (1) at the same time.⁵ One is a dummy for being promoted and surpassing the spouse in earnings. This dummy takes a value of 1 for promoted persons whose earnings were lower than their spouse’s in the year before the promotion and higher in the year after. The other dummy denotes being promoted and not surpassing the spouse in earnings between these two time points, i.e. remaining in the position of earning either less or more than the spouse. We then expand the DID regression (equation 1) to simultaneously estimate the effects of both treatments. In the sample, 16% of the promoted women and 7% of the promoted men start to earn more than their spouse after they are promoted.

The results in Figure W14 show that the promotion effect is not due to female politicians breaking the norm that “the husband should earn more.” The estimated effect of a woman being promoted and surpassing her husband in earnings is basically the same as for being promoted without starting to earn more. This means that the divorce effect is equally strong regardless of whether the promoted woman surpasses the spouse in earnings or not.

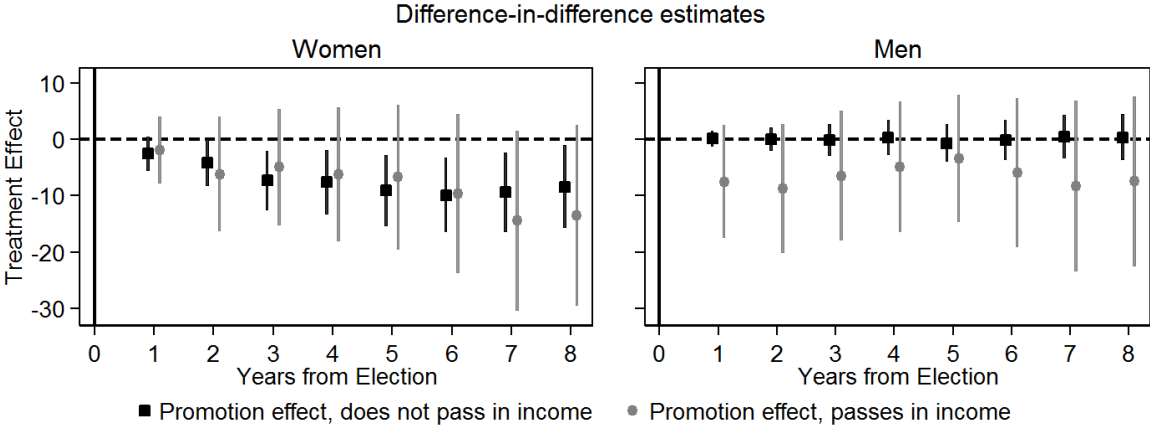


Figure W14. The effect of political promotion on remaining married, differentiated by whether the politician surpasses their spouse in earnings or not.

Notes: Details about the estimation and content of the figure are available in the notes below Figure 5 in the main text. The DID estimates come from an expanded version of equation (1) in which the promotion treatment variable is subdivided into two different dummy variables before being included in the specification. One is a dummy that denotes being promoted and surpassing the spouse in earnings (gray markers) and the other is a dummy for being promoted and not surpassing the spouse in earnings (black markers). Since earnings are

⁵ We cannot perform the same type of split-sample analysis as when we look at pre-promotion characteristics, because we lack information about who in the control group would have earned more than their spouse if they had been promoted.

measured in the years prior to the election, we restrict the sample to those that were married as of the election year.

In addition to identifying the specific point in the earnings division when the wife starts to earn more than her husband, a promotion can also make the wife the household's dominant earner. This cut-off might be more relevant in our case, since many women in our sample already out-earned their spouses before the promotion (Table 1). But it is also more demanding to test because there are more possible transitions. We divide the sample into three categories of earnings divisions prior to the promotion: traditional, dual earner, and reverse traditional (following, e.g., Becker, Landes, and Michael 1977; Becker 1981; Lundberg and Pollak 2007). In traditional households, the husband is the dominant earner with more than 60% of total household earnings (e.g. Fortin 2005; Bertrand, Pan, and Kamenica 2015); in reverse traditional households, the wife is the dominant earner; and in dual-earner households, neither spouse earns more than 60%. Descriptive statistics for this categorization show that about one-fourth of the promoted women and more than two-thirds of the men were the dominant earner in their household before the promotion. The women were over-represented in dual-earner households, 50% compared to 20% of the men. The distributions of the politicians' earnings shares are shown in Figure W15.

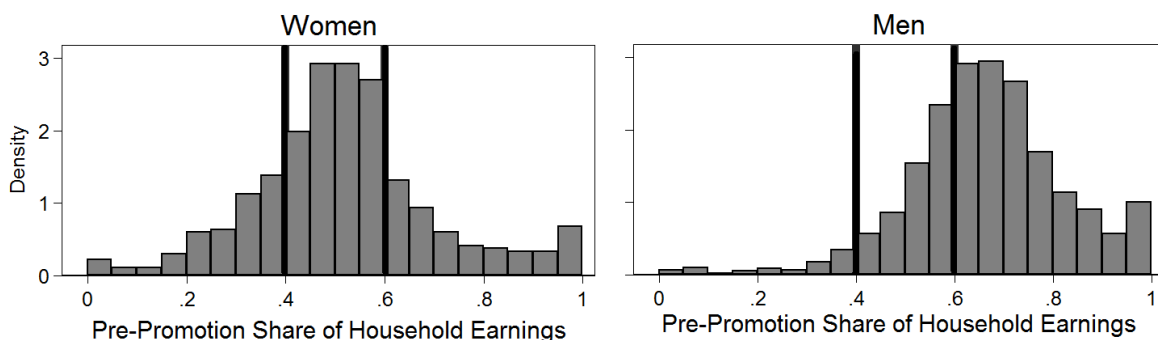


Figure W15. Distribution of the politicians' share of household earnings before promotion.

Notes: Earnings are measured as the sum of deflated annual earnings from jobs and business ownership in an average of the three years prior to the election. The black lines represent divisions into three household types. If the woman makes less than 40% of total earnings, the household is defined as "traditional"; if neither spouse earns more or less than 40–60% of earnings, the household is defined as "dual earner"; and if the wife makes more than 60% of total earnings, the household is defined as "reverse traditional."

For each promoted politician, we construct a matrix for shifts between the three household categories, comparing the status before and after promotion. We then compute the proportion of divorces in the first three years after the promotion for couples in each type of transition (or non-transition). These probabilities are recorded in Table W7. In accordance with the norm story, women who start out in dual-earner households are more likely to divorce when they move into reverse-traditional territory (15%) than if they remain in a dual-earner household (6%). Furthermore, men exhibit the opposite pattern: a transition from a dual-earner household to a traditional household is associated with a lower divorce rate

(3%) than remaining in the dual-earner category (6%). Arguably, shifting household earnings to make the wife the dominant earner is correlated with divorce, while shifting earnings *in accordance with* the norm of the male as the dominant earner has a positive effect on marriage durability. But given the small sample size and rudimentary empirical analysis, the reliability of these findings should not be overstated.

Table W7. Divorce rates and transitions between household types.

		Promoted female politicians			Promoted male politicians			
		Pre-promotion type			Pre-promotion type			
		T	DE	RT	T	DE	RT	
Post-promotion type	T	-	-	-	T	7% N = 335	3% N = 72	-
	DE	5% N = 20	6% N = 53	-	DE	6% N = 17	6% N = 47	-
	RT	-	15% N = 54	10% N = 50	RT	-	-	-

Notes: The table shows the proportion of marriages that ended in divorce within three years after the promotion, subdivided by transitions of household types before and after the promotion. Households are divided into three categories based on the division of earnings, which are computed as the average division in the three years prior to the promotion (pre-promotion) and the three years after (post-promotion). "T" stands for a traditional division of household earnings, in which the husband makes more than 60%. "DE" stands for a dual-earner division in which neither spouse makes more than 60% or less than 40%, and "RT" stands for reverse-traditional households in which the wife earns more than 60% of the household earnings. Cells are left blank (-) if the sample size is smaller than 10 couples. Since earnings are measured in the years prior to the election, we restrict the sample to those that were married as of the election year.

Dual-earner couples and children in the household The previous section showed that female politicians were more likely to be in dual-earner relationships than their male counterparts. This could result in more divorces among women if the spouses in dual-earner relationships lose more utility from a promotion. Compared to specialized relationships, dual-earner relationships derive more utility from consumption and leisure complementarities, that is, spending leisure time together and consuming things that they both like (Lam 1988; Stevenson and Wolfers 2007). When the promotion takes time away from these joint activities, it could reduce the gains from marriage more in dual-earner families than in specialized households that derive utility from specialization rather than complementarity.⁶

We again categorize households into traditional, dual earner, and reverse traditional to investigate if divorces are concentrated among dual-earner couples. The sample is split into the three household types before re-running the baseline analysis (see Figure W16). Figure W16 shows that women's divorces are not concentrated in the dual-earner category but seem to be about as common (at least in

⁶ Under specialization, spouses gain from production complementarities as each of them becomes an expert in his or her own domain (Becker 1973, 1974, 1981; Parsons 1949). With regard to the two types of relationships, an increase in household income from the promotion would be expected to raise utility more in dual-earner families, as it expands the scope for consumption complementarities (Lam 1988; Lundberg 2012).

the short term) in reverse-traditional families. For men, being in a dual-earner family is, if anything, *less* likely to result in divorce upon promotion. In sum, we find little support that the baseline finding stems from a particular sensitivity of dual-earner families to the promotion of one spouse.

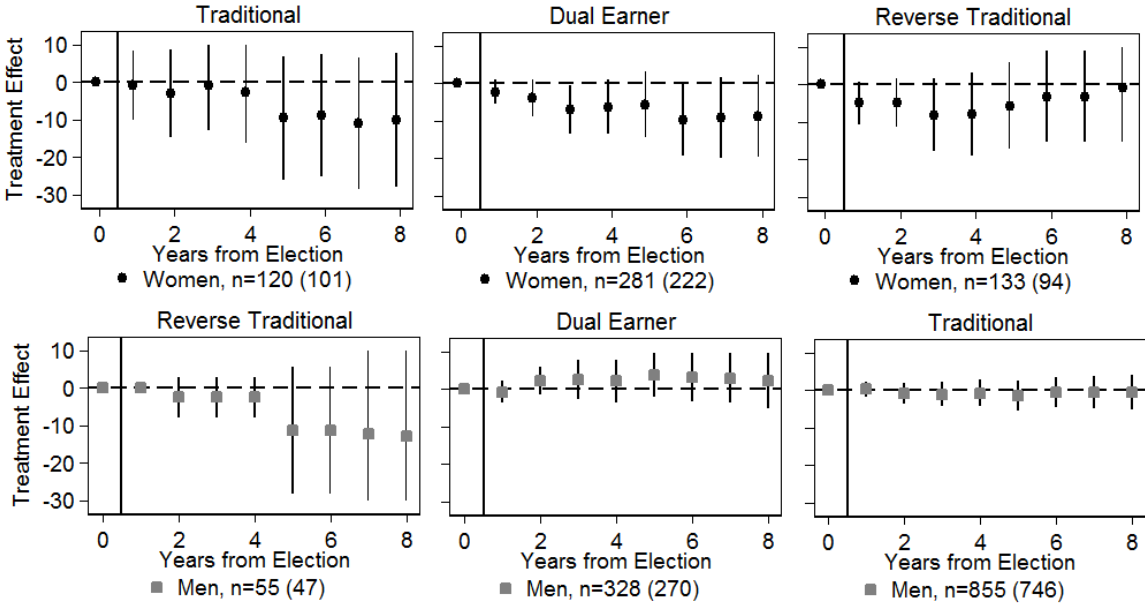


Figure W16. The effect of political promotion on remaining married based on sub-samples for three categories of earnings divisions in the household prior to the promotion.

Notes: The figure shows DID estimates from equation (1), and corresponding to the lower half of Figure (5), for categories of the average division of earnings between the spouses in the three years prior to the promotion. Estimates for women are shown in the top row of graphs, and estimates for men in the bottom row. Couples are divided into types based on the share of the wife’s contribution to total household earnings. Dual-earner couples are those in which the wife earns 40–60% of the total household income. Traditional households are those in which the wife earns less than 40%, and reverse traditional are those in which she earns more than 60%. Since earnings are measured in the years prior to the election, we restrict the sample to couples that were married as of the election year.

We also consider the presence of children in the household. Most people in our dataset have children over 18, and only 10% have children under 6 (see Table 1). This means that the intense parenting responsibilities associated with having younger children can, at most, be a problem for only a subset of the politicians in our estimation sample. Only a small number of people in the data have children under 6, disallowing a split-sample analysis based on this variable. In Figure W17 we split the sample according to whether the politician has at least one child under 18. This analysis does not reveal more (or fewer) divorces for politicians with children. One interpretation could be that older children are less relevant as a measure of total household and care work in a family. Another interpretation is that forces related to the presence of children are pulling on marriage durability in opposite directions. Children imply a larger workload, accentuating the impact of the promotion on total work, but families with children are also more likely to strive to avoid divorce.

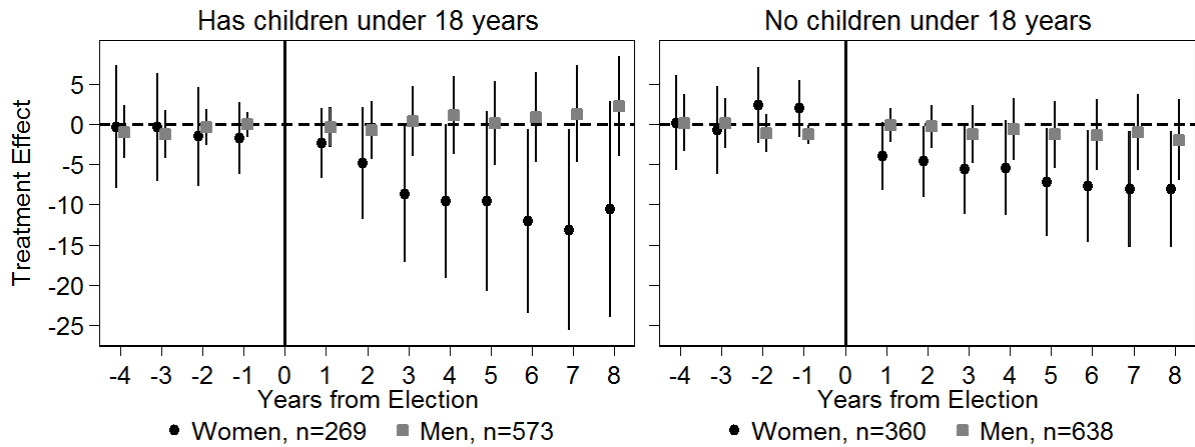


Figure W17. The effect of political promotion on remaining married in sub-samples based on having children under 18.

Notes: Details about the estimation and content of the figure are available in the notes below Figure 5 in the main text. The sample is subdivided according to whether a couple has a child under 18 as of the election year.

Age at marriage Age at marriage is sometimes used to approximate the amount of information that spouses have about each other when they get married. At a younger age, observable traits are less informative for predicting a person's future earnings trajectory. We split the sample according to the median age at marriage for our sample of women job candidates (29). Figure W18 shows that if anything, in our data promotions are associated with more divorces when a couple married at an older age. Restricting the sample to people who are in their first marriage does not alter this picture (not reported). Given the small substantive size of the estimates, their lack of significance and counter-theoretical direction, we do not further pursue this explanation of the main results.

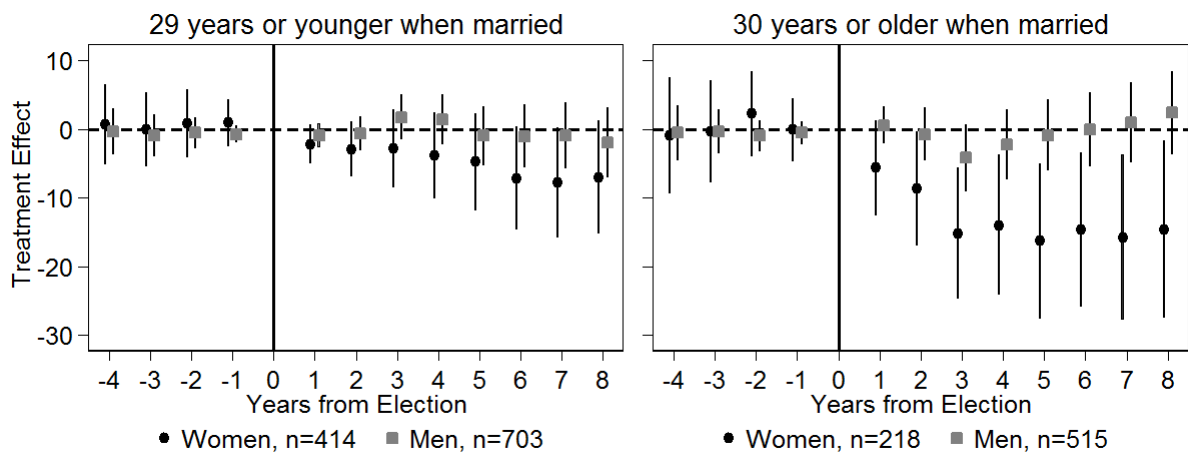


Figure W18. The effect of political promotion on remaining married in sub-samples based on the politician's age at marriage.

Notes: Details about the estimation and content of the figure are available in the notes below Figure 5 in the main text. The sample is subdivided according to whether the politician was above or below age 29 in the year he or she married the current spouse (the sample median for women and men combined).

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