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

Olle Folke[+] and Johanna Rickne[ψ]

Abstract

We investigate how promotions to top jobs affect the probability of divorce. In the first half of the paper we compare the relationship trajectories of winning and losing candidates for mayor and parliamentarian. Promotions double the baseline probability of divorce for women, but not for men. The second half of the paper uncovers a possible explanation for this finding by comparing the characteristics of couples that divorce and those that do not. Divorces are concentrated in couples that adhered to traditional gender roles in the early phase of the relationship, while women in more gender-equal couples are unaffected.

[+] Uppsala University, Department of Government and UCLS; olle.folke@statsvet.uu.se
[ψ] SOFI, Stockholm University and IFN; johanna.rickne@sofi.su.se

1. Introduction

Around the world, women are severely underrepresented in 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 of women's leadership potential 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. It has been documented that an unexpected increase in the wife's – but not the husband's – earnings is correlated with divorce (Becker et al. 1977; Weiss and Willis 1997). Divorce and self-reported marital problems are also more common in couples where the wife earns more money than her husband (Bertrand, Pan and Kamenicka 2015). Experiments have shown that men are reluctant to date successful women and that women are aware of this sentiment (Fisman et al. 2009, Burtzyn et al., forthcoming). An interlocking piece of evidence is that high-ability women are much less likely than high-ability men to enter into marriages that specialize around their own career. These women are more commonly found in marriages where she is the primary caregiver and there is either a dual career focus or a focus on the career of the husband (e.g. Ely, Stone, and Ammerman 2014; Kleven, Landais and Sogaard 2018).

We contribute the first causal evidence on how promotions impact marriage durability across genders and – in the second half of the paper – across more or less gender-equal couple formations. We document that a promotion 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 concerns two jobs at the pinnacle of power in the public sector – local mayors and national parliamentarians.[1] We can identify and follow both winning and losing candidates for these positions over time, before and after the promotion. This lets us document the absence of pre-trends in divorce and in other observables between the promoted and non-promoted candidates (see e.g. Greene and Quester 1982; Johnson and Skinner 1986).

---

[1] These are in the top 5 percent of the earnings distribution (authors' calculations, see Web Appendix Figure W1). From the perspective of the U.S. labor market, the annual earnings of the two political jobs are slightly below those in the U.S. Congress and nearly the same as those of Californian state legislators. Our sample of Swedish CEOs would have their closest equivalent in CEOs of mid-size American firms.

The results show that after the promotion, women divorce at twice the rate of women who did not 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 who ran for office but lost. This result is corroborated in a sub-sample of close elections in which promotions are quasi-randomly assigned between job candidates.

Our data also allows a descriptive analysis of all CEO promotions in 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.

The second part of the paper uses data from the political sector to uncover reason(s) why women who are promoted are more likely to divorce than those who are not. This analysis is descriptive, but uncovers some patterns of interest. We find that divorces are concentrated in couples that matched in a gender traditional way – putting greater priority on the husband's career in the early stages of the relationship. By contrast, women in more gender-equal couples do not divorce more often after a promotion. In this analysis we measure gender equality in couple formation as the spousal age gap and the woman's share of total parental leave. We argue that these variables capture events in the early phases of the relationships of our job candidates, who are 50 years old, on average, and who have been married for an average of 20 years. One interpretation of the results is that the wife's promotion could be more unexpected in a couple that prioritizes the husband's career (e.g. Becker, Landes, and Michael 1977). Another interpretation is that the wife's promotion causes more stress from task renegotiations in these unequal relationships (e.g. Coverman 1989). A third is that women leave relationships that offer the least flexibility and support for her career development.

We also explore if divorce is correlated with the wife's earnings as a share of household earnings. We find that divorce is more likely when the promotion makes the wife the dominant earner in the household (>60% of household earnings), but not when she starts earning more than her husband (>50% of household earnings). Hence, the results are weak but could indicate some sensitivity to social norms on the earnings distribution within the couple (e.g. Bertrand, Pan, and Kamenica 2015).

Other explanations receive no support. For example, women's divorces do not seem motivated by economic independence. The likelihood of divorce is only slightly higher for women whose increase in earnings from the promotion was above median, either in absolute or relative terms. Another explanation with little support is that dual-earner relationships, which are more common among the women in the data, or couples with children under 18, are particularly vulnerable. We also explore a potential "temptation effect" – that women divorce because the promotion exposes them to new potential partners (McKinnish 2004). This investigation reveals no correlation between the proportion

of men in the promoted women's pre-promotion workplaces and the probability to divorce. Divorced and promoted women also remarry at a slower rate than other groups. Finally, no correlation is found between divorce and the woman's age at marriage.

This 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 provide causal estimates.[2] 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.[3] 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 of political candidacy.

A similar contribution is made 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 findings not only provide a causal link between political promotion and marriage duration, but also suggest that 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 economic outcomes 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 the event study for CEO promotions. The second

---

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

[3] 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; Mattozzi and Merlo 2008).

half of the paper studies 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 for a period of 33 years, 1979–2012. Each person has a mandatory ID code, which is recorded in interactions 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 specifies 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.

Co-habitation 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 private home. 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 (IoT). 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 parties must report a list of their candidates and their personal ID codes to the electoral authority in every election. This creates a pooled dataset of the ID codes for the universe of nominated politicians for all levels of

politics, which also 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 percent of the Swedish earnings distribution (Web Appendix Figure W1). Being promoted to these two jobs give similar average increase in earnings at about 20 percent (see Lundqvist 2015 for an analysis of parliamentarians, for mayors see Figure 7). Another important observation about the top jobs in Swedish politics is that families or spouses are not part of any political campaigning. Spouses are also a non-topic in media reporting about politicians.

The two political jobs offer the advantage of having two observable top contenders for each promotion: one who lost the promotion and one and who won it. Consider parliamentarians first. Sweden uses the list-based proportional representation (PR) electoral system in which political parties control the rank order of the electoral ballot(s). This rank order represents the hierarchy of power within the party. Over time, a person climbs their way up through the ranks, which is similar to climbing the career ladder of a private firm. The seats that the party wins in an election are allocated starting at the top of this list. For each ballot in the 29 electoral districts we define the pair of 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 <u>not</u> get elected).[4] We only omit the extremely small proportion of electoral ballots (1%) from which preference votes determined who was elected.

For promotions to *mayor*, another important characteristic of Sweden's parliamentary democratic political system comes into play. 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). The ruling coalition is determined by the election result, and political coalitions are usually formed by parties that belong to the same political bloc (i.e. left or right) (Alesina, Roubini, and Cohen 1997). Thus, there are two rivals for the position of mayor: the top politician from 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

---

[4] 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, therefore, is unimportant for our analysis. First, only one-third of the voters utilize their voluntary vote. Second, the vast majority of those who do vote for 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).

with substantially less influence, work hours, and responsibilities. On average over our sample period, a promotion from vice mayor to mayor entailed an immediate 25% increase in annual earnings.[5]

Just as the process of climbing up through the ranks of an electoral ballot over time is similar to upward career moves in a private company, the selection of the top name, the party leader, is comparable to the selection of a manager. The person who is appointed mayor is the first-ranked person on the electoral ballot of the largest political party in the governing coalition.[6] We delete the small number of cases (2% of the sample) in which a political party offers several ballots with different top-ranked candidates in a municipality.

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 4.3). Nevertheless, since the split-sample analysis in the second half of the paper 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. 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 further restrict the sample to married people. This leaves 70% of the men and 61% of the women (robustness checks show that the results are not driven by the choice of starting year). We also exclude politicians who reach the age of 65 before the end of the election period (10% of the sample). The retirement age of 65 is more strictly applied outside of politics, i.e. for those in the control group. Keeping this group in the estimation would, thus, mean that the treatment effect of promotion on divorce would also capture the effect of staying in the labor force rather than retiring. 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*.

---

[5] In-depth descriptions of the positions of mayor and vice mayor can be found in Nilsson 2001; Jonsson 2003; and Montin 2007.

[6] Using data from 1991–2010, we know which party appointed the mayor and can verify that when either bloc obtained more than 50% of the seats, the largest party in that bloc had a 90% probability of appointing the mayor. For 2006 and 2010 we know the identity of the mayor in each district and can verify that this person was the top-ranked individual on the electoral ballot of the largest governing party in nine cases out of ten (see also Folke et al. 2016).
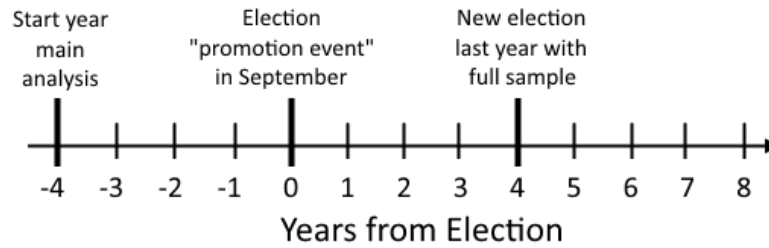
Figure 1. Timing of events.

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 make sure that we 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 CEOs. 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. 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. 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 self-collected survey on work hours and influence. The aim is to show that the analysis of divorce is not confounded by men and women receiving promotions with different characteristics.

We start by comparing socioeconomic characteristics prior to the promotion event. 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, earnings, 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 by two dummies for whether the household has children, one including at least one child aged 0–17 and the other for having 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.[7] 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 (subsequently) not. A simple regression is used to detect statistically significant differences at the 5% level.[8]

Although the DID design hinges on parallel pre-trends in the outcome variables, it naturally strengthens our identification strategy if our treatment and control groups are also comparable on observable traits. Comparing the descriptive statistics for the treatment and control groups shows that only a handful of traits are unbalanced, and the differences are small in absolute terms. One exception to the balance in traits is the politician's earnings, where we find a pre-promotion difference for both men and women. We return to this variable in the DID analysis below, which shows that although there is a difference in means, there is no difference in pre-trends.

---

[7] Section W1 of the Web Appendix shows that measuring the division of leave based on payments is highly correlated with measuring it in days.

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

Table 1. Comparison of pre-promotion traits.

Subsequently promoted	Women		Men	
	Yes	No	Yes	No
Couple characteristics				
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
Individual characteristics				
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) The variable takes a value of 1 if the spouses are from different birth regions, and 0 otherwise. The categorization of birth regions consists of: Sweden, Other Nordic countries, EU27 excluding Nordic countries, Asia (including the Middle East), 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 ≈ 0.12 USD).

Differences in traits *between* men and women cannot be omitted variables in the main analysis, which studies men and women separately. But such differences are, however, relevant for understanding differences in the size of the treatment effect for women and men. Comparing men and women to each other, we can see that the average age is similar at roughly 50, and the average length of marriage is similar at 20 years. Close to 10% of both men and women have children below 6 years old in the household. There are also traits that differ. In short, the 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 where she is younger and has used more than 80% of the couple's total parental leave. This means that even among the women in our sample, who are candidates for top jobs, there are signs that relationships specialize in the husband's career (we return to these differences in Section 6).

Next, we compare the promotions that men and women receive. Restricting the data to the 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.
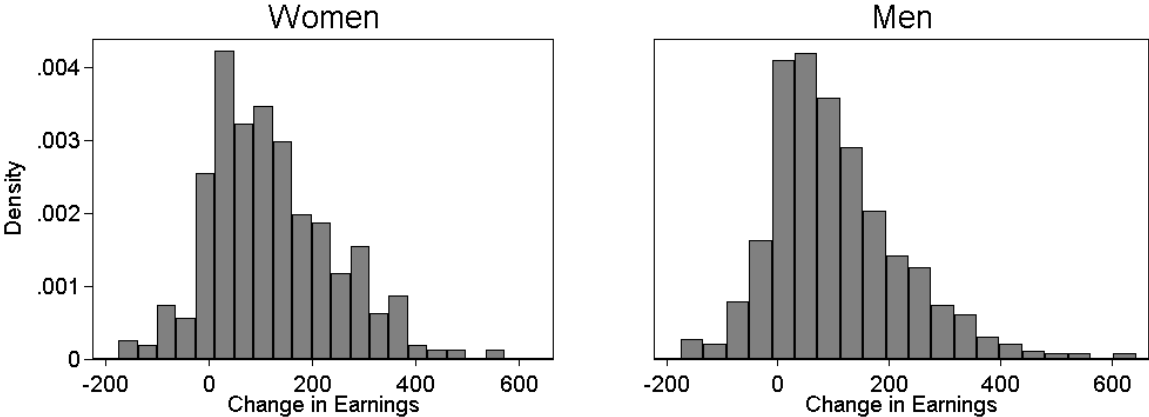

Figure 2. Distribution of changes in earnings before and after promotion (1,000 SEK).

Notes: The figures show the distributions 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 the 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, Karlsson and Gilljam 2014), we can also verify that the distributions of male and female mayors' self-perceived political influence are highly 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.
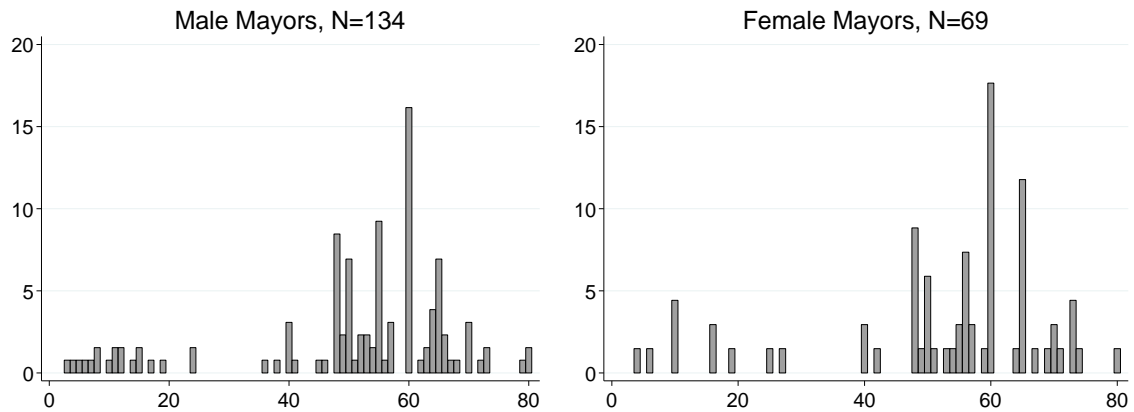
Figure 3. Self-reported work hours in a typical week by male and female mayors.

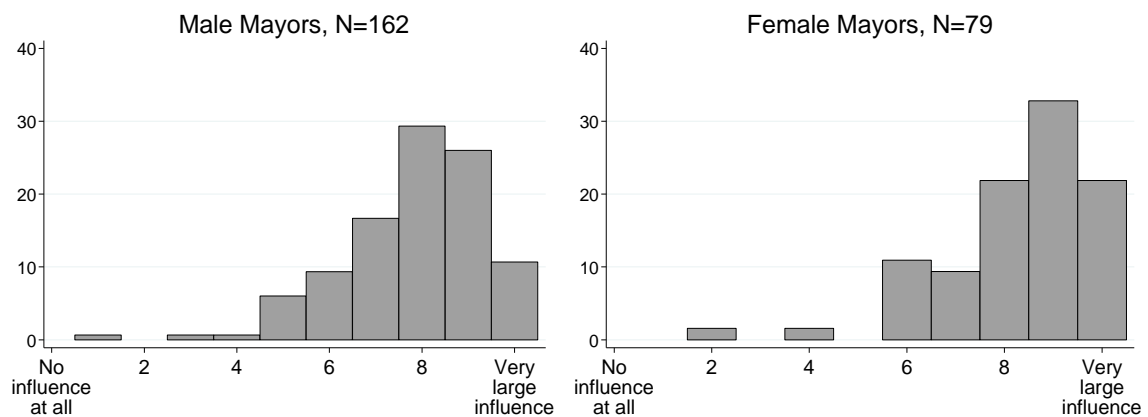Notes: Data from the authors' survey of mayors in 2013).

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 the 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 an 8-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. 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 group 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

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

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 trend in divorce 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 the 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 promotion probability, or temporal trends in the promotion probability, 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_t 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. The plot on the bottom left shows the results for the main sample of people married at $t = -4$. In the plot on the bottom right we provide an alternative estimation that selects the sample of political candidates in the election year ($t = 0$) rather than four years prior. This alternative sample gives us more observations of job candidates who were married right before their promotion, providing much-needed statistical power for the split-sample analysis in the second half of the paper.
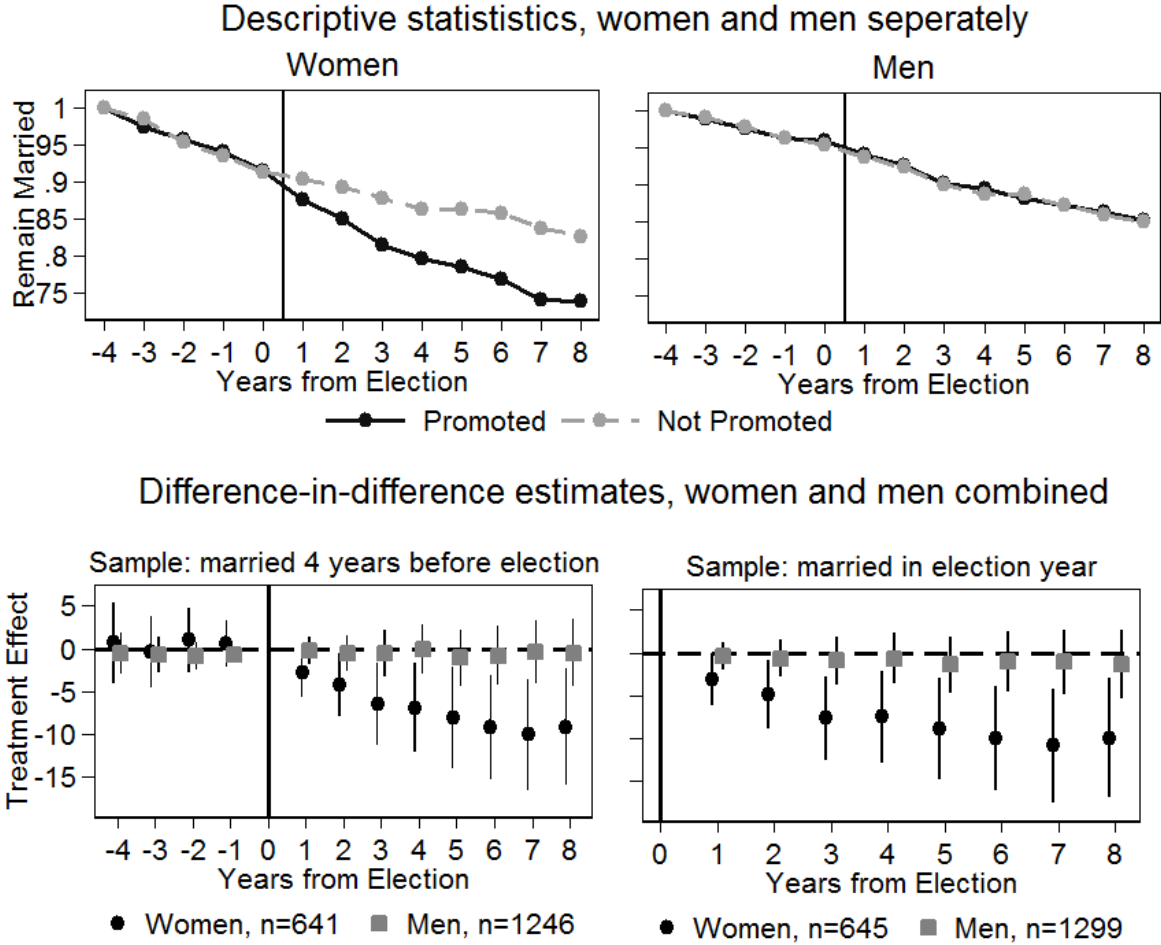

Figure 5. The effect of political promotion on remaining married.

Notes: The top panel plots the share of job candidates that 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 difference in the probability of remaining married between promoted and control group in each year compared to the baseline event year ($t = 0$). Horizontal lines are 95% confidence intervals. In the lower panel, the left-hand side graph uses the sample of politicians married 4 years prior to the election ($t = -4$) and the right-hand side shows estimates from the sample of politicians married in election year.

The differences-indifferences estimates at the bottom of Figure 5 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 8 percentage points less likely to remain married than their non-promoted counterparts.

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 consequence 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 W2 contains a detailed description of how we calculate the municipal and parliamentary indicators of close elections.

Appendix Table W1 replicates Table 1, the descriptive statistics for pre-promotion traits, in the close election sample. As expected, differences in traits across winning and losing candidates are (even) smaller than in the full sample.
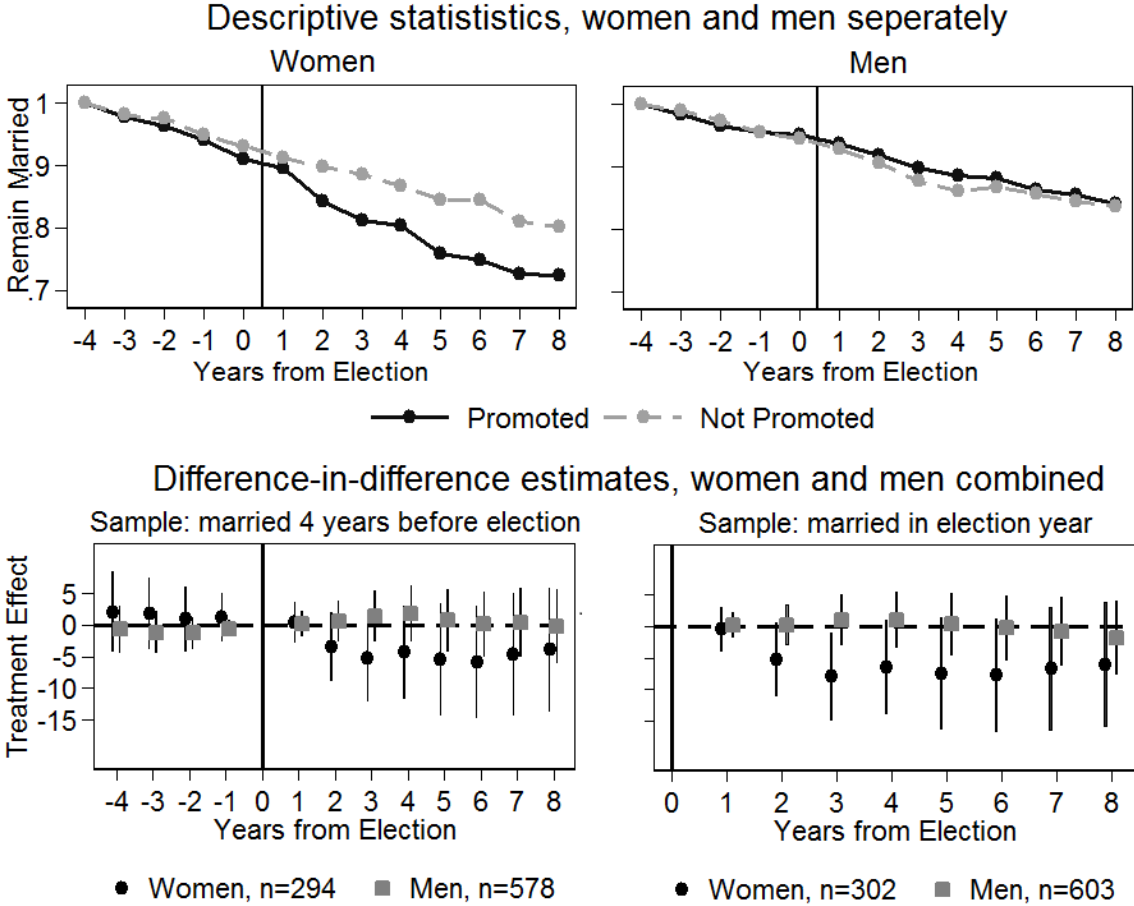

Figure 6. The effect of political promotion on remaining married in a sub-sample of close elections.

Notes: The structure of the plot is described in the note under Figure 5. For municipalities, a close election is defined as one with 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 simulations.

Figure 6 replicates the main analysis (Figure 5) for the sample of close elections. The results corroborate the findings of the main analysis, as the estimated effects are more or less identical, but with one exception: the treatment effect is postponed by one year. This is logical, given that fact that the average promotion in this sample becomes evident at the 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 during the three years following the election. But compared to the main analysis, the effect is less precisely estimated due to the smaller sample size. When we use the sample of politicians

married in the election year as the sample, the estimates are statistically significant at the 5% or 10% level. In the sample of politicians married four years before the election, the estimate at $t = 3$ is barely below the 10% significance level.

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

We run four additional robustness checks: i) including control variables for observables in the baseline estimation; ii) testing if the results are robust to restricting the sample to those who become candidates for the two jobs for the first time; iii) testing the sensitivity of the starting year of the sample (i.e., use of either $t = -4$ or $t = -1$ in Figure 5); and iv) 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. The results are available in Figure W5 in the Web Appendix.

Second, we re-estimate equation (1) excluding those who have previously been candidates for either one of the offices but lost (results available in Appendix Figure W6). The sizes of the point estimates are not affected by this change in the estimation sample, providing further evidence of the robustness of our baseline estimates. Naturally, larger standard errors follow from the loss of about one-fourth of the sample.

A third sensitivity test varies the temporal starting point of the sample selection of married couples, reporting more starting years than the two years used in the main results. In Figure W7 we use six different starting years, ranging from $t = -7$ to $t = -2$. As a reference point we also include the replicated results for the starting point of $t = -4$ in the main analysis. The results show that the size of the estimated promotion effect is not sensitive to the starting year. The effect is less precisely estimated for earlier starting years since the effective sample size shrinks as a larger proportion of the couples have had time to divorce before the election.

A fourth and final robustness check examines the development of earnings, which are separately estimated for the 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. If women with less stable marriages compete more fiercely to get elected, this could be indicated by a falling trend in earnings in the previous, pre-promotion job (which in the vast majority of cases is outside politics). Similarly, such efforts could be mirrored by a falling trend in spousal earnings if the spouse either takes on more household responsibilities, or an increase if they increase their own career investments to match their politician spouses.

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 do not show any clear difference 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 findings 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 (relate to previous research by e.g. Devereux 2004).
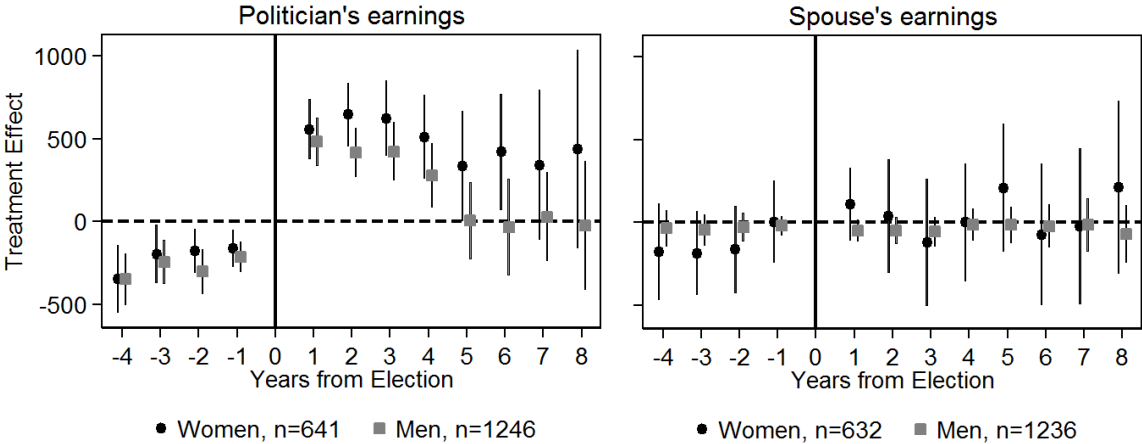

Figure 7. Pre-trends in the annual earnings of politicians and their spouses.

Notes: The figure shows estimates from running the promotion regression (equation 1) for the outcomes of i) 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 married 4 years prior to the election. The gray dots represent the difference relative to the election year between promoted men and non-promoted men, and the black dots give 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. Pre-promotion descriptive statistics for this sample show strong similarities to the 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 that 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, there is 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.
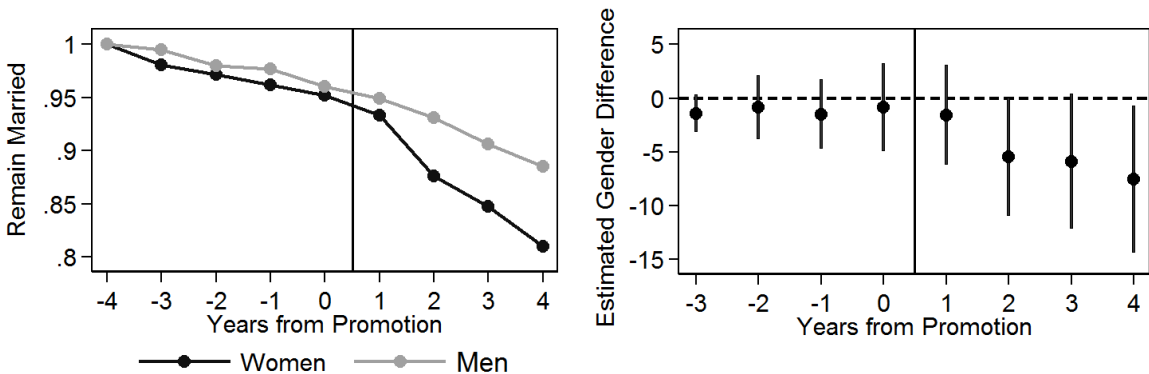
Figure 8. Event study of CEO promotion and remaining married.

Notes: The left-hand graph shows the proportions of men (gray line) and women (black line) who 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) is 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 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 3 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, with pharmacists being the exception, successful women have higher divorce rates than unsuccessful women. For men, the relationship is the opposite: high career success is associated with lower divorce rates.

6. Mechanisms

Why does a promotion lead to divorce among women but not among men? We attempt to answer this question by investigating whether women's divorces are concentrated in some sub-samples of the data. By splitting the sample of political candidates along theoretically relevant dimensions, we can detect some common features of women who divorce after promotion and those who do not. The empirical analysis for these tests follows the main analysis (Figure 6) and uses the sample of political job candidates who were married in the election year ($t = 0$). Because of the relatively small sample size, we split the sample based on one variable at a time. This section is structured into sub-sections for these features, motivated by previous theoretical and empirical research. We begin by describing the clearest and most striking empirical findings and then summarize the evidence on answers with less empirical viability.

6.1 Traditional couple formation, progressive at work: A recipe for divorce

When couples meet, they form a union that is more or less gender equal. We capture this gender equality in the early stages of the relationship using two variables. The first is the spousal age gap, a key indicator of gender-based specialization within the couple (Becker 1981; Eagly 1987).[9] The second variable is the division of parental leave, described in detail in Section 3. Like the spousal age gap, the split of parental leave between the husband and wife captures gender-based specialization in the early phases of the relationship. For the politicians in our sample, these events took place decades ago since

---

[9] Using the terminology of social exchange theory, a couple where 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).

their average age is 50 and they have been married an average of 20 years at the time of their candidacy to office (recall Table 1). It should also be noted that the division of parental leave captures large time investments, since Swedish parental insurance covers 480 days of paid leave per child, and the vast majority of couples utilize at least 12 months of leave between them (Statistics Sweden 2016).[10]

Economic theory speaks to the possible impacts on marriage stability from the wife's promotion in a union that initially prioritized the husband's career. Labor market events that change the performance of spouses in relation to what was expected at the time of couple formation may shift the balance of utility away from the marriage to being single (e.g. Becker, Landes, and Michael 1977; Weiss and Willis 1997). In a couple that is specialized in the husband's career, the wife's promotion to a top job is more likely to challenge initial expectations, while the husband's promotion would confirm them. This gives the expectation that divorces for women should be concentrated in couples with more gender-based specialization in the husband's career.[11]

Note here that we remain agnostic about which spouse initiates the divorce. In economic theory, a promotion of the wife in a male-specialized household shifts the cost–benefit analysis of being married vs. single for both spouses. Either spouse could initiate a divorce to realize their relatively improved outside options. An alternative view is that promoted women initiate divorce when they find themselves in male-specialized marriages that offer the least flexibility and career support (in the spirit of Newman and Olivetti 2017). This alternative interpretation is further discussed below.

Figure 9 shows the distributions of our two variables that capture the degree of specialization in the husband's (or wife's) career: the spousal age gap and the split of parental leave. It shows that despite being candidates for jobs in the top 5% of the earnings distribution, the women's relationships show signs of prioritizing the husband's career. Four out of five women in the sample are married to an older man, and used 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 have taken more than 20% of the parental leave.[12]

---

[10] 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.

[11] A similar prediction can be drawn from sociology. A promotion may be seen as a critical transition points 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 couples that started out with a more gender traditional division of roles, but when the wife is later promoted to a top job.

[12] 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.
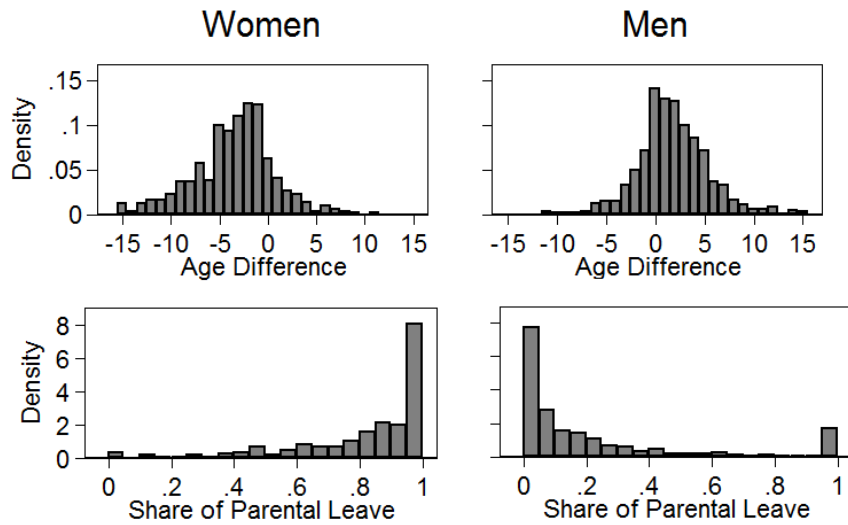
Figure 9. Distributions of the spousal age gap and the politician's share of parental leave.

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),[13] and (3) the politician is older by four or more years (Figure 10). For the split of parental leave, we are forced to take a liberal view on gender equality. A more gender-equal couple is defined as one in which the wife took less than 90%, and a male-specialized couple is one in which she took more than 90% (Figure 11).

The results in Figures 10 and 11 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 promotion. Comparing Figures 10 and 11, the precision is smaller in the results for the parental leave division, but the sign and size of the evidence are similar to those for the spousal age gap. Recall also that the sample size is smaller for this variable due to the unavailability of parental leave data for children born before 1980.

---

[13] The empirical findings remain the same if we change the cut-off points by one year in either direction.
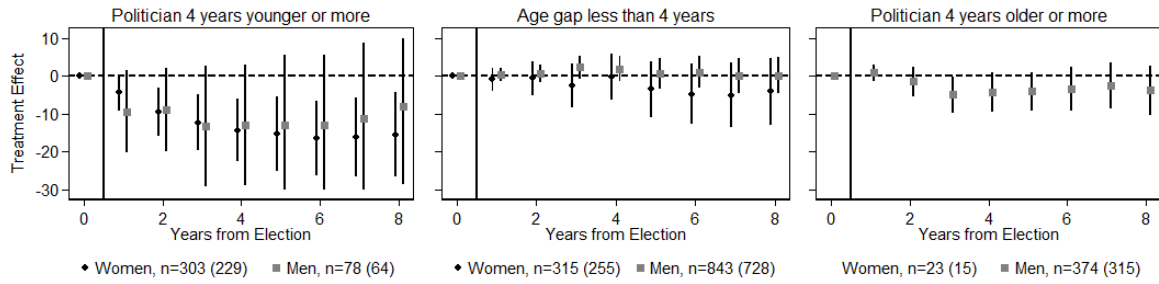
Figure 10. The effect of political promotion on remaining married in sub-samples based on the spousal age-gap.

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), corresponding to the lower half of Figure (5), for three sub-samples based on this variable. Results for women politicians who are at least 4 years older than their husband have been excluded due to the small sample size.
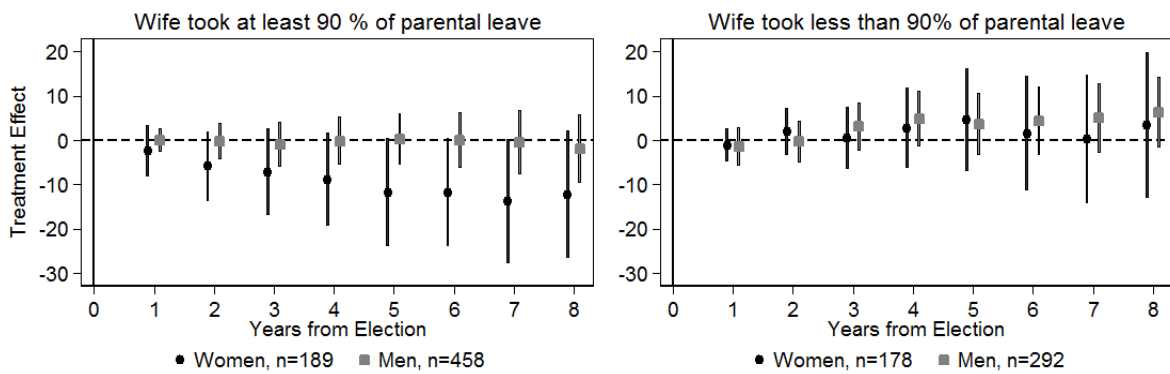

Figure 11. The effect of political promotion on remaining married in sub-samples based on the wife's share of total parental leave.

Notes: The figure shows DID estimates from equation (1), corresponding to the lower half of Figure (5), 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.

Gender traditional behaviors of couple formation underpin the results in this section. Divorces are concentrated in marriages that are focused on the husband's career, while there is no divorce effect for women in more gender equal couples. This pattern is consistent with several interpretations, for example that divorce happens when the promotion of a spouse deviates more from his or her expected labor market trajectory. Supporting this gender-neutral interpretation is the point estimates for men in the leftmost graph in Figure 11. They indicate that promoted *men* who are more than four years younger than their spouses also see an increased divorce rate upon promotion.

Another interpretation of the findings is that promoted women divorce the men who are the least supportive of their careers. Some additional descriptive statics support this interpretation, and some do not. In support, the promoted and divorced women are slightly more likely to be re-elected

23

for office than the promoted and divorced men. On the other hand, we find no correlations between divorce and characteristics of the husband that could proxy for a lower marriage utility for the wife.[14] In a later section (6.5), we also show that divorces are only slightly more common among women who make larger economic gains from the promotion.[15] In sum, we maintain that the evidence on more or less gender-equal couple formations is valuable, but that we cannot establish a conclusive interpretation of this pattern.

6.2 Norms regarding the division of household 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 drive the main result, we subdivide the treatment variable into two separate dummy variables. One is a dummy for being promoted and passing the spouse in earnings. This dummy takes the value one for promoted persons whose earnings were lower than their spouse's in the year before the promotion and higher in the year thereafter. The other dummy captures the event of being promoted and not passing the spouse in earnings between these two time points. 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 pass their spouse in earnings when they are promoted. 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 passed their spouse in earnings if they had been promoted.

---

[14] 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).
[15] There are too few remarriages among the promoted and divorced women and their spouses for us to examine "updated" spousal choices. We also lack data on which spouse initiated the divorce. Note also that there are more, but less likely, alternative interpretations of the results. One is that a larger spousal age gap indicates a worse match quality, i.e. that the spouses were not able to be very picky in their choice of partner, making the union more sensitive to changes in outside conditions. This interpretation contradicted by the lack of a divorce effect when the husband is substantially older than his wife (Figure 10). Another interpretation could stem from an assumption that the average man is less productive in household work than the average woman (along the lines of Mincer 1962). A promotion that shifts relatively more of that work from the wife to the husband would therefore crowd out more leisure time. But given modern household technologies and the opportunities to outsource various household services, it seems unlikely that productivities differ or that shifting tasks would incur much efficiency loss.

The results in Web Appendix Figure W9 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 passing the husband in earnings is basically the same as for being promoted without passing the husband in earnings. This means that the divorce effect is equally strong regardless of whether the promoted woman passes the spouse in earnings or not.

Besides the specific point in the earnings division when the wife starts to earns more, a promotion can also trigger the wife to become the household's dominant earner. This cut-off might be more relevant in our case since many women in our sample already out earn 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 the 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 instead over-represented in dual-earner households, 50% compared to 20% of the men (the distributions of the politician's earnings share can be found in Appendix Figure W10).

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 getting promoted for the couples in each type of transition (or non-transition). These probabilities are recorded in Appendix Table W3. 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 is positive for marriage durability. But given the small sample size and rudimentary empirical analysis, the reliability of these findings should not be overstated.

6.3 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.[16]

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 12). Figure 12 shows that women's divorces are not concentrated in the dual-earner category but seem to be about as common (at least in 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.

We also consider the presence of children in the household. Most people in our dataset have children over 18, and only 10% have children below 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 the age of six, disallowing a split-sample analysis based on this variable. In Web Appendix Figure W11 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.

---

[16] 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 the consumption complementarities (Lam 1988; Lundberg 2012).
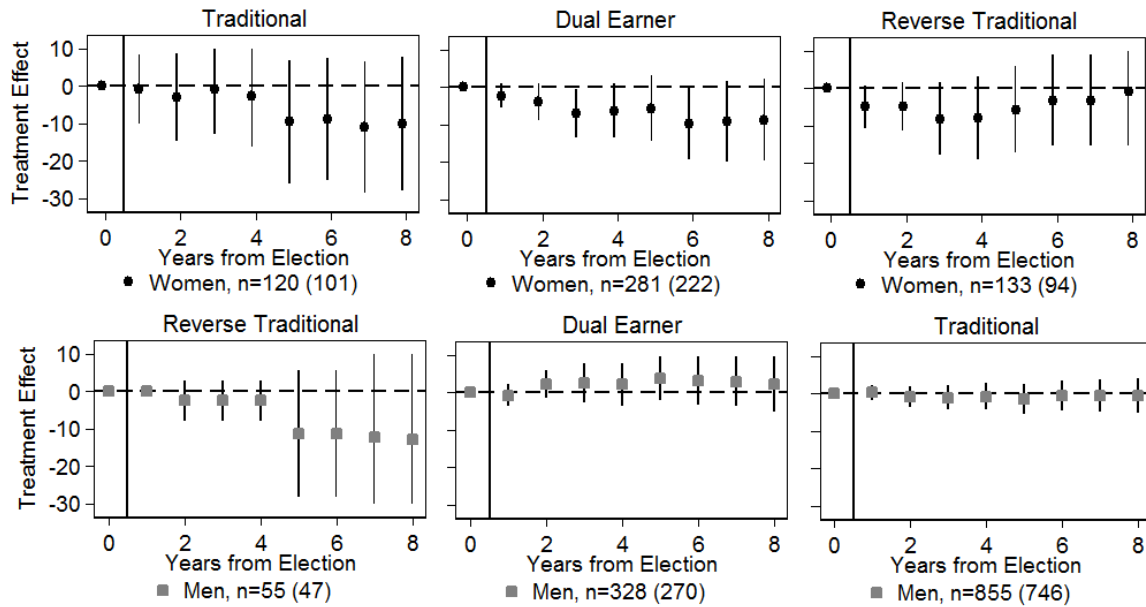
Figure 12. The effect of political promotion on remaining married based on sub-samples for three categories of earnings divisions in the household prior to the promotion.

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%.

6.4 The "temptation effect"

When a person is promoted, he or she may shift work environments and encounter different new potential partners. If this explains the baseline findings, divorce among women politicians should be concentrated among women who move into their political jobs from previously female-dominated job environments (following McKinnish 2004).

We measure the change in exposure to opposite sex co-workers as both the proportion and absolute number of co-workers of the opposite sex at the workplace level in the year before the promotion.[17] To reduce noise, we drop workplaces with fewer than 10 employees, which account for less than 10% of the data. We also exclude the politician from the calculation of the sex composition. The sample is then split by the median of the share, or absolute number, of colleagues of the opposite sex. The two resulting groups give us the samples of "Low" and "High" expected temptation effects.

---

[17] This is done using the workplace code Cfar in LISA to ensure that we capture colleagues who were physically close to the politician. Note here that the level of opposite-sex co-workers after the promotion is highly similar across all promoted politicians so that we can rely solely on the pre-promotion share for variation.

The results, shown in Web Appendix Figure W12, show that the temptation effect does not explain the higher share of women's divorces. The promotion effect is larger for women who enter politics from relatively male-heavy environments, and not the other way around (if anything, there could exist a small temptation effect for men, which is beyond the scope of this study).

A more direct test of the temptation effect is to examine the probability that divorced women remarry after their promotion. Figure 13 provides this description in two steps. We first use the pooled sample of winning 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 of the divorcees that has remarried (gray bars). Comparing the black and gray bar 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 the divorcees of either sex that did not win a promotion. This description suggests that gender differences in the improved outside options in the marriage market, i.e. the temptation effect, is not a likely mechanism behind our baseline findings. Promoted and divorced women were less likely to remarry than any other group.
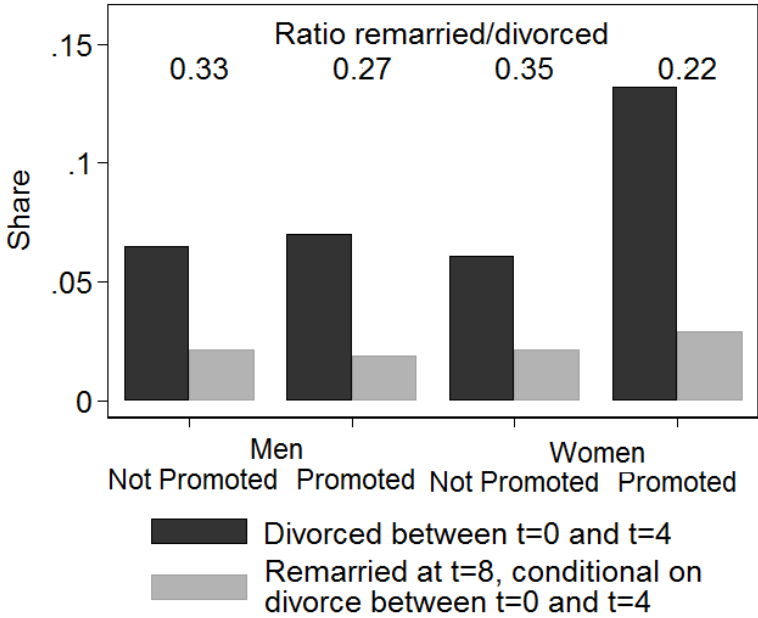

Figure 13. Promotion, divorce and remarriage.

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 of divorcees that remarried 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.

6.5 Women's economic independence

If women are in marriages in which they are economically dependent on their spouse, events that given them economic independence could trigger their latent desire to divorce (e.g. Oppenheimer 1997). Numerous papers have related the probability of divorce to various measures of changes in the absolute and relative earnings of men and women. Many such studies have found that marriage durability is more sensitive to women's economic outcomes than to men's (Heckert, Nowak, and Snyder 1998; Jalovaara 2003; Liu and Vikat 2004; Kesselring and Bremmer 2010; c.f. Rogers 2004).

Yet economic independence is less likely to trigger divorce in our study of promotions to top jobs. As shown in Table 1, the women in our dataset were already making enough money to be economically independent before their promotions. More than half of the promoted women also had higher earnings than their husbands.

To examine the role of economic independence, we use the same approach as when we examined the norm that "the husband should earn more" (see Section 6.2). In this case we divide the treatment dummy into two parts, depending on whether the increase in earnings from the promotion was larger or smaller than the median among promoted politicians of the same sex, to capture the increase in absolute earnings. We make two additional versions of this division to capture the relative earnings increase. The first measures the change in earnings in proportion to the politician's own (pre-promotion) earnings. The second measures 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 due to being promoted. 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. This is true for the increases in both absolute and relative earnings.

6.6 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 W14 in the Web Appendix 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.

7. Conclusions

We study the consequences for men's and women's relationships of being promoted to top jobs. The main result is that such promotions destabilize women's marriages but not men's. This finding can contribute to understanding why few women are 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 promotion, women were substantially more likely to give up the potential support system of a loving spouse than men. Giving up 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 loving relationship *and* a successful career.[18] It is also reasonable to expect that the candidate pool for top jobs would be skewed by a condition for women, but not for men, to willingly leave their relationship behind.

Our descriptive analysis of common features of women who divorce indicated a link between couple formation and the destabilizing role of the top promotion. 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 their 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 around 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 in 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 highly 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 should explore the (material or immaterial) 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.

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

---

[18] Even in secularized countries like Sweden, more than 98% of women and men in the 2010 World Value Survey reported that "Family" is "important" or "very important" in their lives (WVS 2010).

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. forthcoming). More research is needed to explore the joint developments of marriage and labor market across contexts and over time.

References

Akerlof, George A., and Rachel E. Kranton. 2000. Economics and Identity. *Quarterly Journal of Economics* 115: 715–53.

Albrecht, James, Anders Bjorklund, and Susan Vroman. 2003. Is There a Glass Ceiling in Sweden? *Journal of Labor Economics* 21(1): 145–77.

Alesina, Alberto, Nouriel Roubini, and Gerald Cohen. 1997. *Political Cycles and the Macroeconomy*. Cambridge, MA: MIT Press.

Amato, Paul R., and Denise Previti. 2003. People's Reasons for Divorcing: Gender, Social Class, the Life Course, and Adjustment. *Journal of Family Issues* 24(5): 602–26.

Andersson, Fredrik W., and Jan Andersson. 2009. Företagsledarna i Sverige – En algoritm för att peka ut företagens operativa ledare i näringslivet (Firm Managers in Sweden – An Algoritm to Determine Firms' Operational Leaders in Private Firms). Statistics Sweden Background Paper.

Arulampalam, Wiji, Alison L. Booth, and Mark L. Bryan. 2007. Is There a Glass Ceiling over Europe? Exploring the Gender Pay Gap across the Wage Distribution. *Industrial and Labor Relations Review* 60(2): 163–86.

Beaman, Lori, Chattopadhyay, Raghabendra, Duflo, Esther, Pande, Rohini och Petra Topalova. 2009. Powerful Women: Does Exposure Reduce Bias? *Quarterly Journal of Economics* 124(4): 1497–1540.

Beaman, Lori, Esther Duflo, Rohini Pande, and Petia Topalova. 2012. Female Leadership Raises Aspirations and Educational Attainment for Girls: A Policy Experiment in India. *Science* 335(6068): 582–86.

Becker, Gary S. 1973. A Theory of Marriage: Part I. *Journal of Political Economy* 81(4): 813–46.

Becker, Gary S. 1974. A Theory of Marriage: Part II. *Journal of Political Economy* 82(2): S11–S26.

Becker, Gary S. 1981. *A Treatise on the Family*. Cambridge, MA: Harvard University Press.

Becker Gary S., Elisabeth M. Landes, and Robert T. Michael. 1977. An Economic Analysis of Marital Instability. *Journal of Political Economy* 85: 1141–87.

Bertrand, Marianne. 2013. Career, Family, and the Well-Being of College-Educated Women. *American Economic Review: Papers & Proceedings* 103(3): 244–50.

Bertrand, Marianne, Patricia Cortés, Claudia Olivetti, and Jessica Pan. 2016. Social Norms, Labor Market Opportunities, and the Marriage Gap for Skilled Women. Working Paper 22015. Cambridge, MA: National Bureau of Economic Research.

Bertrand, Marianne, Claudia Goldin, and Lawrence Katz. 2010. Dynamics of the Gender Gap for Young Professionals in the Financial and Corporate Sectors. *American Economic Journal: Applied Economics* 2: 228–55.

Bertrand, Marianne, Jessica Pan, and Emir Kamenica. 2015. Gender Identity and Relative Income within Households. *Quarterly Journal of Economics* 130(2): 571–614.

Bjerk, David. 2008. Glass Ceilings or Sticky Floors? Statistical Discrimination in a Dynamic Model of Hiring and Promotion. *The Economic Journal* 118(530): 961–82.

Booth, Alison L., Marco Francesconi, and Jeff Frank. 2003. A Sticky Floors Model of Promotion, Pay, and Gender. *European Economic Review* 47(2): 295–322.

Boschini, Anne, Christina Håkanson, Åsa Rosén, and Anna Sjögren. 2011. Trading Off or Having it All? Completed Fertility and Mid-career Earnings of Swedish Men and Women. Working paper 2011:15. Uppsala: Institute for Evaluation of Labour Market and Education Policy.

Bursztyn, Leonardo, Fujuwara, Thomas, and Amanda Pallais. forthcoming. 'Acting Wife': Marriage Market Incentives and Labor Market Investments. *American Economic Review*.

Carroll, Susan J., and Kira Sanbonmatsu. 2013. *More Women Can Run: Gender and Pathways to the State Legislature.* Oxford: Oxford University Press.

Charles, Kerwin Kofi, and Melvin Stephens, Jr. 2004. Job Displacement, Disability, and Divorce. *Journal of Labor Economics* 22(2): 489–522

Coverman, Shelley. 1989. Role Overload, Role Conflict, and Stress: Addressing Consequences of Multiple Role Demands. *Social Forces* 67(4): 965–82.

Devereux, Paul J. 2004. Changes in Relative Wages and Family Labor Supply. *Journal of Human Resources* 39: 696–722.

Diermeier, Daniel, Michael Keane, and Antonio Merlo. 2005. A Political Economy Model of Congressional Careers. *American Economic Review* 95: 347–73.

Doiron, Denise, and Silvia Mendolia. 2012. The Impact of Job Loss on Family Dissolution. *Journal of Population Economics* 25: 367–98.

Eagly, Alice H. 1987. *Sex Differences in Social Behavior: A Social Role Interpretation*. Hillsdale, NJ: Lawrence Erlbaum Associates.

Eggers, Andy, and Jens Hainmueller. 2009. MPs for Sale? Returns to Office in Postwar British Politics. *American Political Science Review* 103: 513–33.

Eliasson, Marcus. 2012. Lost Jobs, Broken Marriages. *Journal of Population Economics* 25(4): 1365–97.

Ely, Robin J., Pamela Stone, and Colleen Ammerman. 2014. Rethink What You "Know" About High-Achieving Women. *Harvard Business Review* 92(12): 101–9.

Esping-Andersen, Gösta. 2016. *Families in the 21st Century*. Stockholm: SNS Förlag.

Fisman, Raymond, Florian Schulz, and Vikrant Vig. 2014. The Private Returns to Public Office. *Journal of Political Economy* 122(4): 806–59.

Fisman, Raymond J. Sethi, Sheena, Iyengar; Kamenica, Emir, and Itamar Simonson. 2006. Gender Differences in Mate Selection: Evidence from a Speed Dating Experiment. *The Quarterly Journal of Economics* 121(2): 693–97.

Folke, Olle, Torsten Persson, and Johanna Rickne. 2016. The Primary Effect: Preference Votes and Political Promotions. *American Political Science Review* 110(3): 559–78.

Fortin, Nicole. 2005. Gender Role Attitudes and Women's Labour Market Outcomes across OECD Countries. *Oxford Review of Economic Policy* 21(3): 416–38.

Freier, Ronny, and Christian Odendahl. 2015. Do Parties Matter? Estimating the Effect of Political Power in Multi-party Systems. *European Economic Review* 80: 310–28.

Ginther, Donna, and Marianne Sundström. 2010. Does Marriage Lead to Specialization? An Evaluation of Swedish Trends in Adult Earnings Before and After Marriage. Working paper 1/2010. Stockholm: Swedish Institute for Social Research.

Goldin, Claudia. 2014. A Grand Gender Convergence: Its Last Chapter. *The American Economic Review* 104(4): 1091–1119.

Greene, William, and Aline Quester. 1982. Divorce Risk and Wives' Labor Supply Behavior. *Social Science Quarterly* 63(1): 16–27.

Heckert, D. Alex, Thomas C. Nowak, and Kay A. Snyder. 1998. The Impact of Husbands' and Wives' Relative Earnings on Marital Disruption. *Journal of Marriage and the Family* 60(3): 690–703.

Jalovaara, Marika. 2003. The Joint Effects of Marriage Partners' Socioeconomic Positions on the Risk of Divorce. *Demography* 40: 67–81.

Johnson, William H., and Jonathan Skinner. 1986. Labor Supply and Marital Separation. *American Economic Review* 76(3): 455–69.

Jonsson, Leif. 2003. *Kommunstyrelseordförande: Kommunledare med politisk förankring* (Municipal Council Board Chair: Municipal Leader with a Political Mandate). Västra Frölunda: Nya Doxa.

Karlsson, David, and Mikael Gilljam (eds). 2014. *Svenska politiker: Om de folkvalda i riksdag, landsting och kommuner* (Swedish Politicians: Elected Representatives in Parliament, Counties, and Municipalities). Stockholm: Santérus Förlag.

Kesselring, Randall, and Dale Bremmer. 2010. Female Income and the Divorce Decision: Evidence from Micro Data. *Applied Economics* 38(14): 1605–16.

Kleven, Henrik, J., Camille Landais, and Jacob E. Sogaard. 2018. Children and Gender Inequality: Evidence from Denmark. NBER Working Paper 24219.

Lam, David. 1988. Marriage Markets and Assortative Mating with Household Public Goods: Theoretical Results and Empirical Implications. *Journal of Human Resources* 23(4): 462–87.

Lazear, Edward P., and Sherwin Rosen. 1990. Male–Female Wage Differentials in Job Ladders. *Journal of Labor Economics* 8(1): 106–23.

Liu, Guiping, and Andres Vikat. 2004. Does Divorce Risk Depend on Spouses' Relative Income? A Register-based Study of First Marriages in Sweden in 1981–1998. Working paper 2004-010. Rostock: Max Planck Institute for Demographic Research.

Lundberg, Shelly. 2012. Personality and Marital Surplus. *IZA Journal of Labor Economics* 1(3): 1–21.

Lundberg, Shelly, and Robert A. Pollak. 2007. The American Family and Family Economics. *The Journal of Economic Perspectives* 21(2): 3–26.

Lundqvist, Heléne. 2015. Politicians' Payments in a Proportional Party System. Mimeo, Stockholm University.

Mattozzi, Andrea, and Antonio Merlo. 2008. Political Careers or Career Politicians? *Journal of Public Economics* 92(3-4): 597–608.

McKinnish, Terra G. 2004. Occupation, Sex-Integration, and Divorce. American Economic Review 94(2): 322–25.

Mincer, Jacob. 1962. Labor Force Participation of Married Women: A Study of Labor Supply. In H. Gregg Lewis (ed.), *Aspects of Labor Economics*, 63–97. Cambridge, MA: National Bureau of Economic Research.

Montin, Stig. 2007. *Moderna Kommuner* (Modern Municipalities). Malmö: Liber.

Newman, Andrew F., and Claudia Olivetti. 2017. Career Women and the Durability of Marriage. Mimeo: http://people.bu.edu/afnewman/papers/irony.pdf.

Nilsson, Thorbjörn. 2001. *Den lokalpolitiska karriären: En socialpsykologisk studie av 20 kommunalråd* (The Local Political Career: A Social Psychology Study of 20 Full-time Local Politicians). PhD. Thesis, Växjö University.

Oppenheimer, Valerie Kincade. 1997. Women's Employment and the Gain to Marriage: The Specialization and Trading Model. *Annual Review of Sociology* 23: 431 –53.

Parsons, Talcott. 1949. The Social Structure of the Family. In Ruth N. Anshen (ed.), *The Family: Its Function and Destiny*, 173–201. New York: Harper.

Rege, Mari, Kjetil Telle, and Mark Votruba. 2007. Plant Closure and Marital Dissolution. Discussion Paper No. 514. Statistics Norway.

Rogers, Stacy J. 2004. Dollars, Dependency, and Divorce: Four Perspectives on the Role of Wives' Income. *Journal of Marriage and Family* 66: 59–74.

Smith, Nina, Valdemar Smith, and Mette Verner. 2013. Why Are So Few Females Promoted into CEO and Vice President Positions? Danish Empirical Evidence, 1997-2007. *International Labor Relations Review* 66(2): 380–408.

Statistics Sweden. 2016. Women and Men in Sweden 2016: Facts and Figures.

Stevenson, Betsey, and Justin Wolfers. 2007. Marriage and Divorce: Changes and their Driving Forces. *Journal of Economic Perspectives* 21(2): 27–52.

Thibaut, John. W. and Harold Kelley. 1959. *The Social Psychology of Groups*. New York: Wiley.

Weiss, Yoram, and Robert J. Willis. 1997. Match Quality, New Information, and Marital Dissolution. *Journal of Labor Economics* 15(1): S293–329.
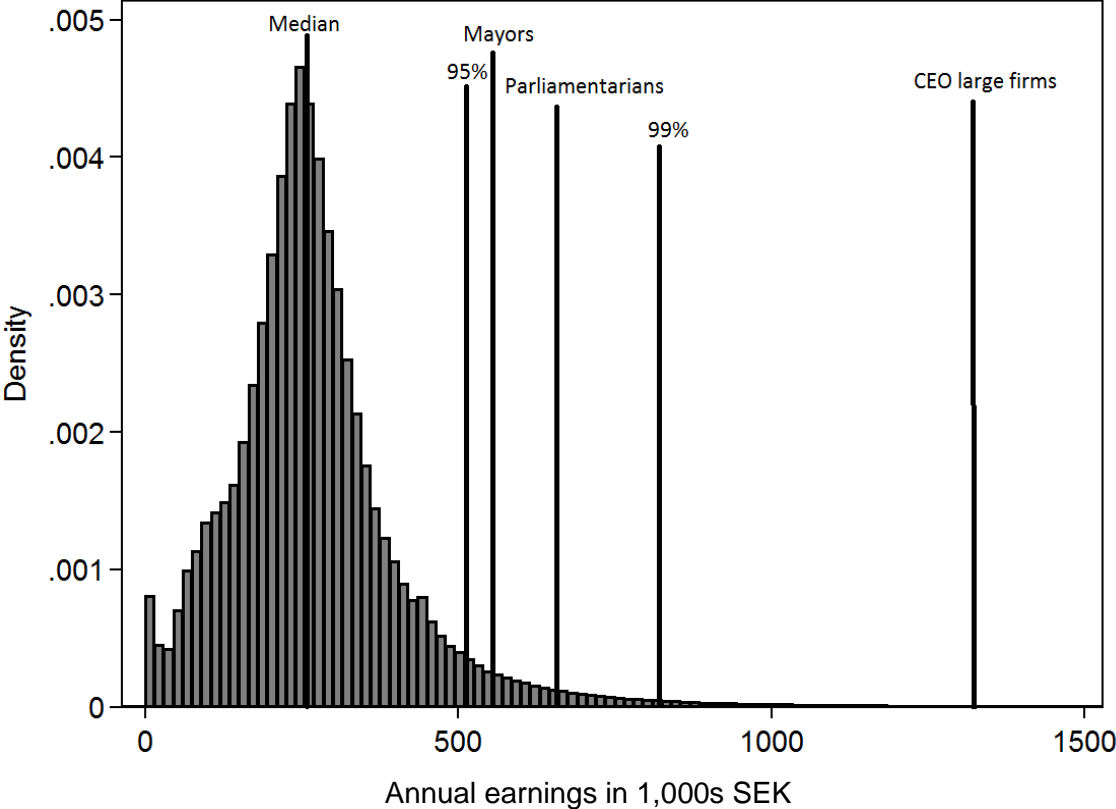
Figure W1. Placement of jobs in the distribution of annual earnings, 2011

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 the 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. Defining close elections in PR systems

There are complexities to measuring close elections in a proportional representation (PR) 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.

W2.1 Municipal elections

To measure electoral closeness at the municipal level, we use a simulation-based approach that builds on Fiva et al. (2016). The approach and code developed in this paper has also been applied in Folke et al. (2016). 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.[19] 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 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 the time, how large a 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. Lagué 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.[20] Having computed the new seat allocation for each of the 1,000 shifts of the vote distribution, we tally the number of times that 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 Väsby in the 2006 election. In this municipality, the center-right bloc won the

---

[19] In Sweden, local parties – defined as not having representation in parliament – hold, on average, 2% of the municipal assembly seats.

[20] 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*(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 $q_w = \frac{q_p}{\Sigma_1^p q_p}$ 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.

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 that 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.
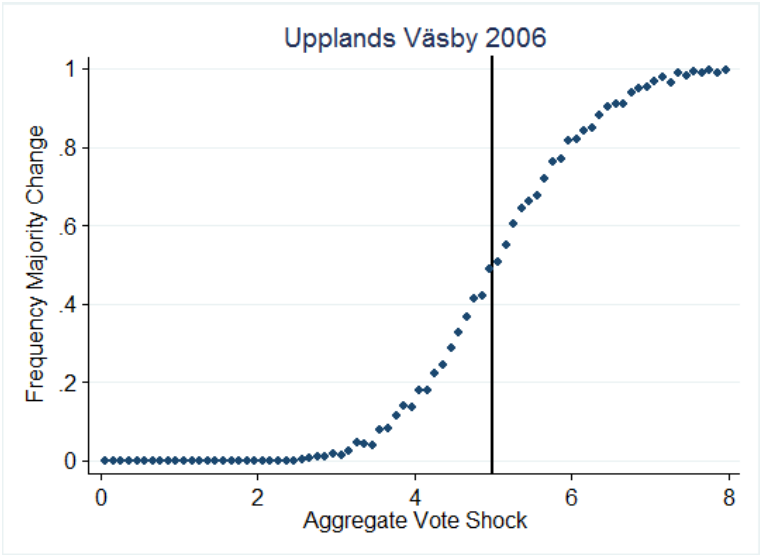

Figure W3. Illustration of simulation strategy.

Note: The figure shows the proportion of seat majority shifts to the left bloc (y-axis) 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.
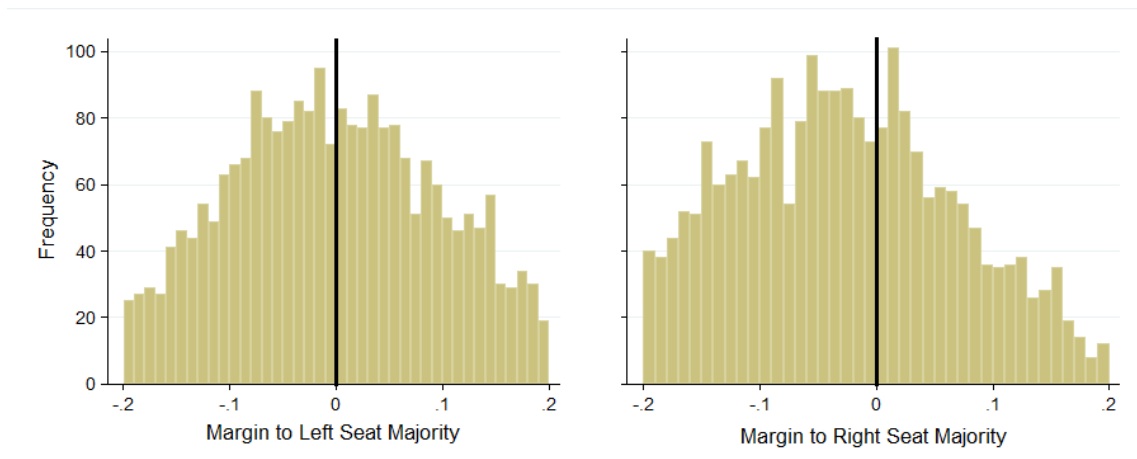
Figure W4. Frequency of observations, as a function of the margin to a seat majority for the left bloc (left-hand graph) and the right bloc (right-hand graph)

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

W2.2 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.[21] 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

---

[21] 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 that 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 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 vote shock and the district-level vote shock 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.

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.

Table W1. Comparison of pre-promotion traits in the sub-sample of close elections.

	Women		Men	
Subsequently promoted	Yes	No	Yes	No
Couple characteristics				
*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
Individual characteristics				
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.
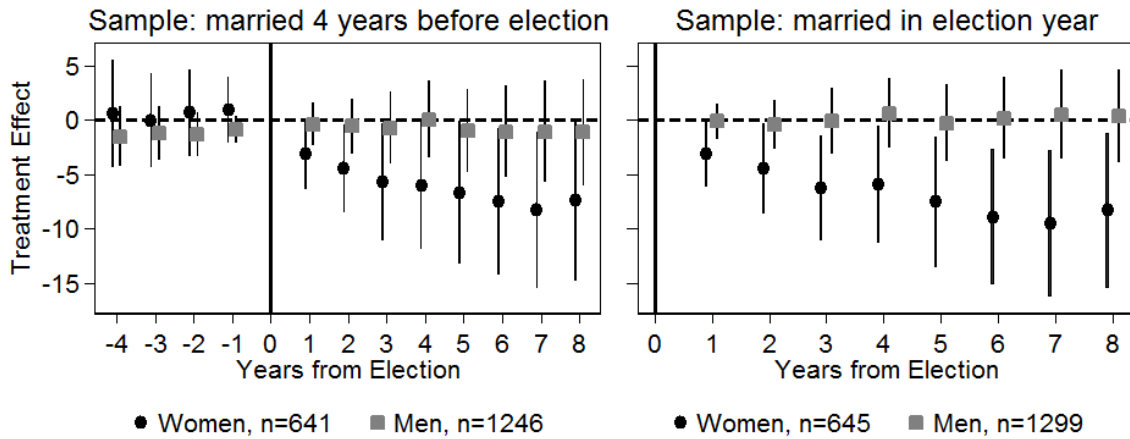
Figure W5. The effect of political promotion on remaining married, including control variables.

Notes: Details about the estimation and the content of the figure are available in the notes below Figure 5 in the main text. The control variables include all controls listed in Table 1, Section 3, with the exception of the division of parental leave, which is excluded due to missing data.
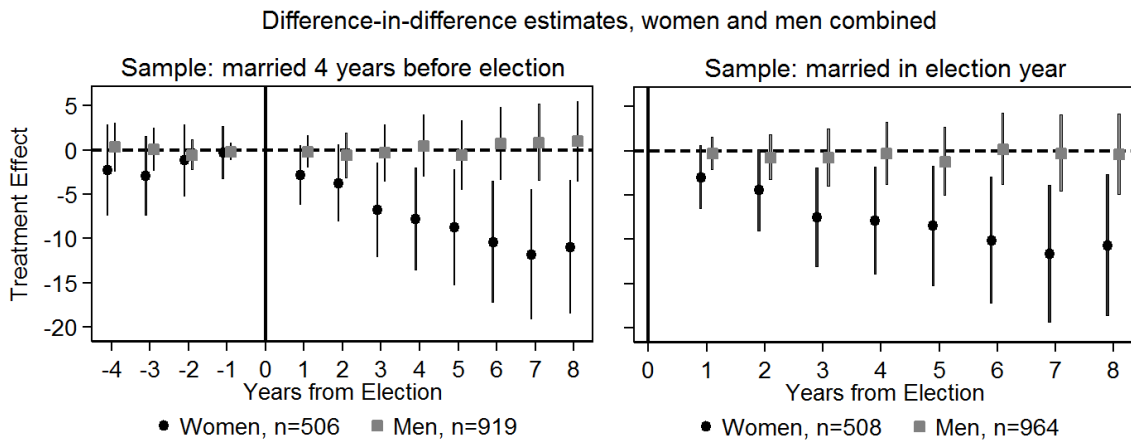

Figure W6. The effect of political promotion on remaining married, excluding repeating losers from the sample.

Notes: Details about the estimation and the content of the figure are available in the notes below Figure 5 in the main text. The figure uses the main sample but excludes politicians who previously appeared in the sample as a losing job candidate.
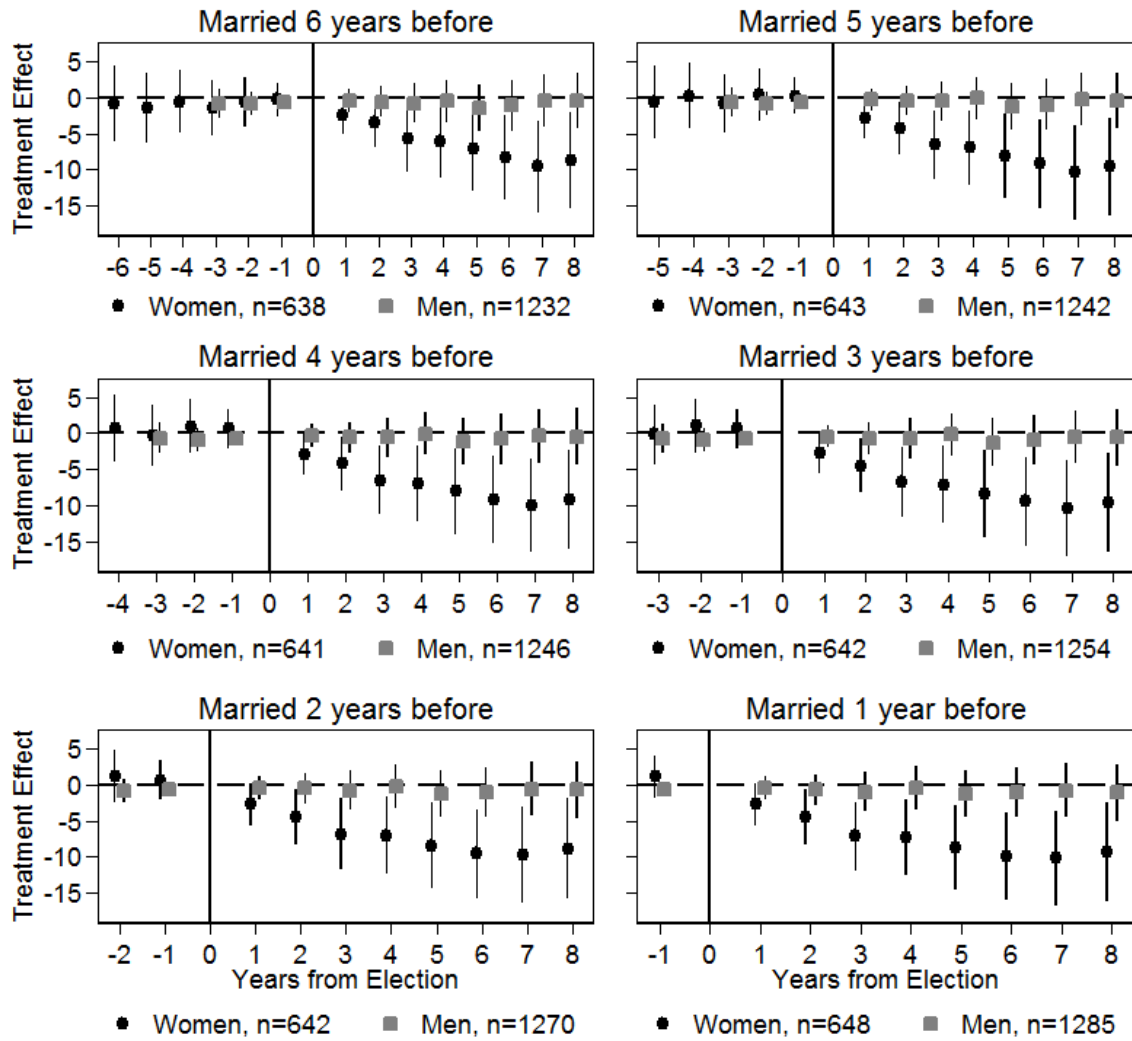
Figure W7. The effect of political promotion on remaining married, different start years.

Notes: Details about the estimation and the 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 year by year up to one year before the election.

Table W2. Comparison of pre-promotion traits among CEO candidates.

	Women	Men
Subsequently promoted	Yes	Yes
Couple characteristics		
*Politician's share of earnings (%)	0.58	0.78
*Politician's share of parental leave (%)	0.78	0.09
*Age difference (politician-spouse)	-3.05	1.78
Politician out-earns spouse (%)	0.62	0.95
Marriage length (years)	13.37	13.5
Second marriage (%)	0.11	0.07
Same birth region (%)	0.89	0.90
Has children (0–17)	0.74	0.71
Has children (0–6)	0.31	0.33
Individual characteristics		
Politician's age	43.76	45.5
Politician's age at marriage (1)	30.99	31.98
Politician's earnings (2)	624.2	796.1
Politician's tertiary education (%)	0.82	0.73
Spouse's age	46.82	43.75
Spouse's earnings	527.5	209.4
Spouse's tertiary education (%)	0.67	0.64
Observations	228	1,208

Notes: For variable definitions and details, see notes to Table W1. The sample selection is described in Section 2.2.

Section W3. 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.[22] 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 the year 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 W8.

---

[22] 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).
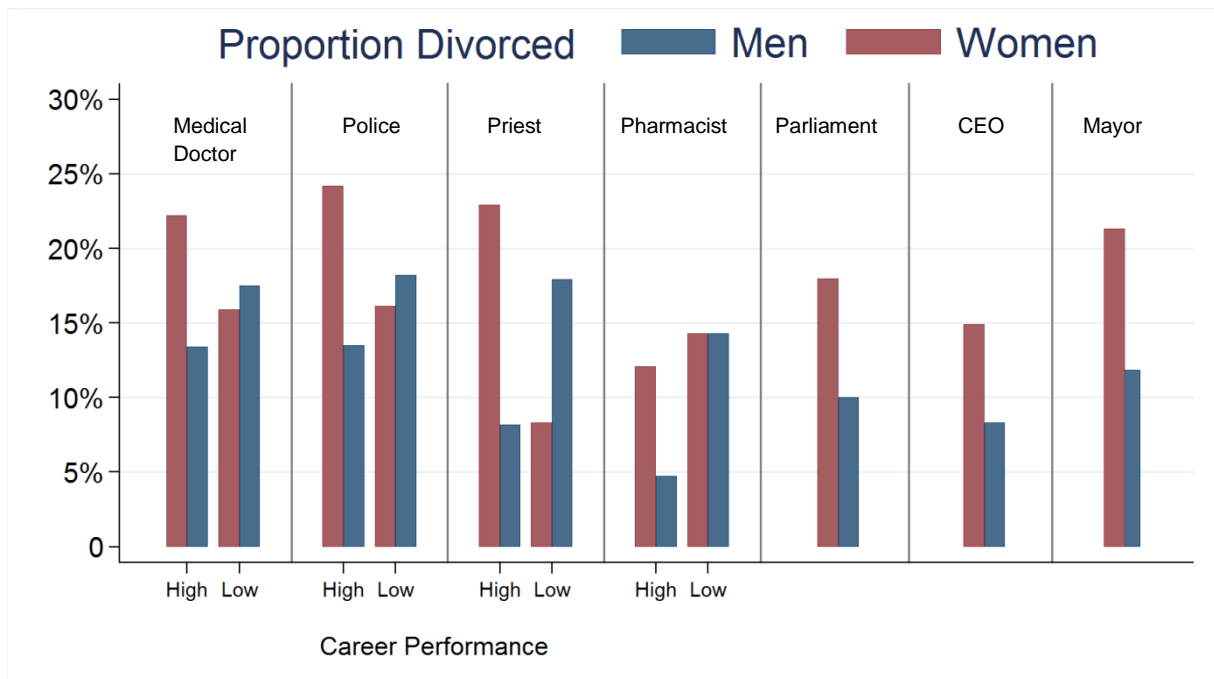
Figure W8. Proportion of divorced individuals by gender, occupation, and career performance

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. 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.
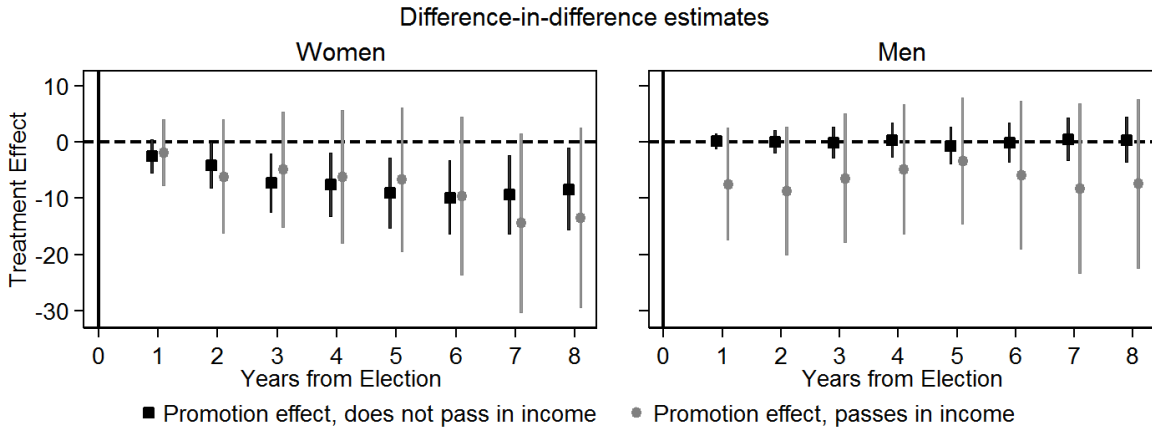
Figure W9. The effect of political promotion on remaining married, differentiated by whether the politician passes the spouse in earnings or not.

Notes: Details about the estimation and the 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 part is a dummy for being promoted and passing the spouse in earnings (gray markers) and the other is a dummy for being promoted and not passing the spouse in earnings (black markers).
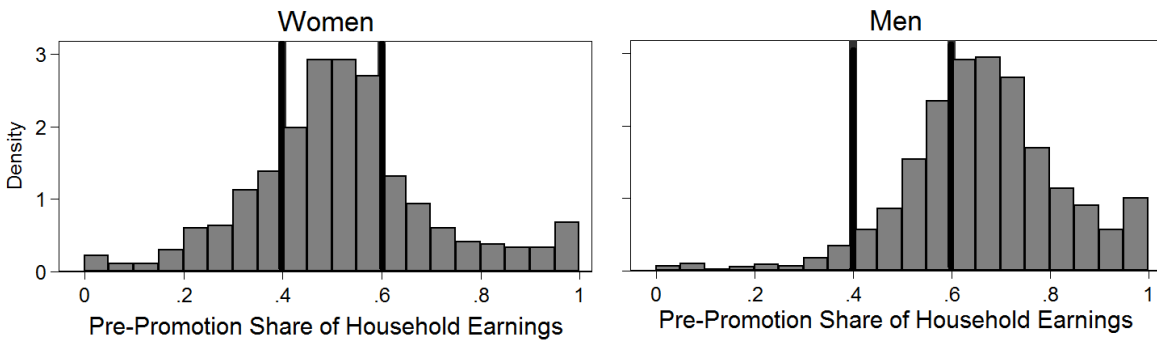

Figure W10. Distribution of the politician's share of household earnings before promotion.

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".

Table W3. 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 have ended in divorce 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 makes more than 60% of the earnings. Cells are left blank (-) if the sample size is smaller than 10 couples.
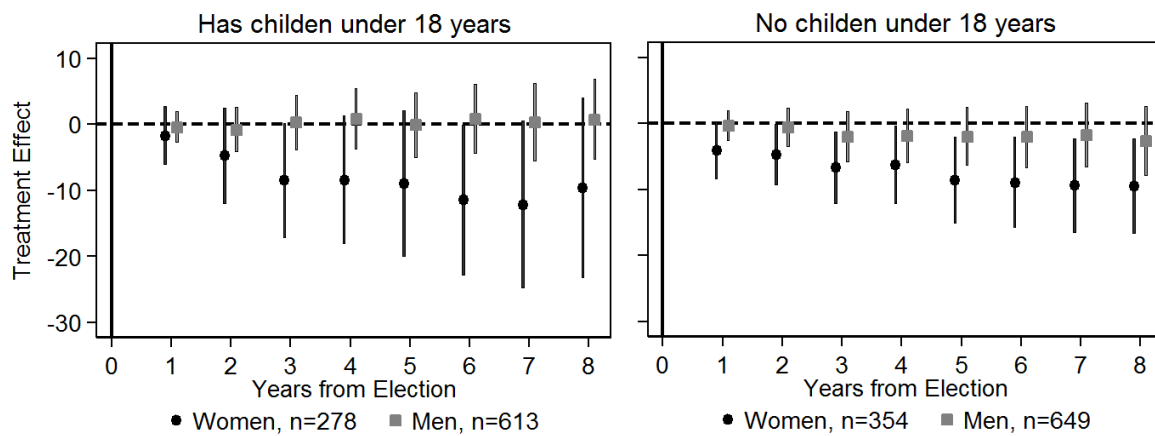

Figure W11. The effect of political promotion on remaining married in sub-samples based on having children under 18.

Notes: Details about the estimation and the content of the figure are available in the notes below Figure 5 in the main text. The sample is subdivided by whether a couple has a child under 18 in the election year.
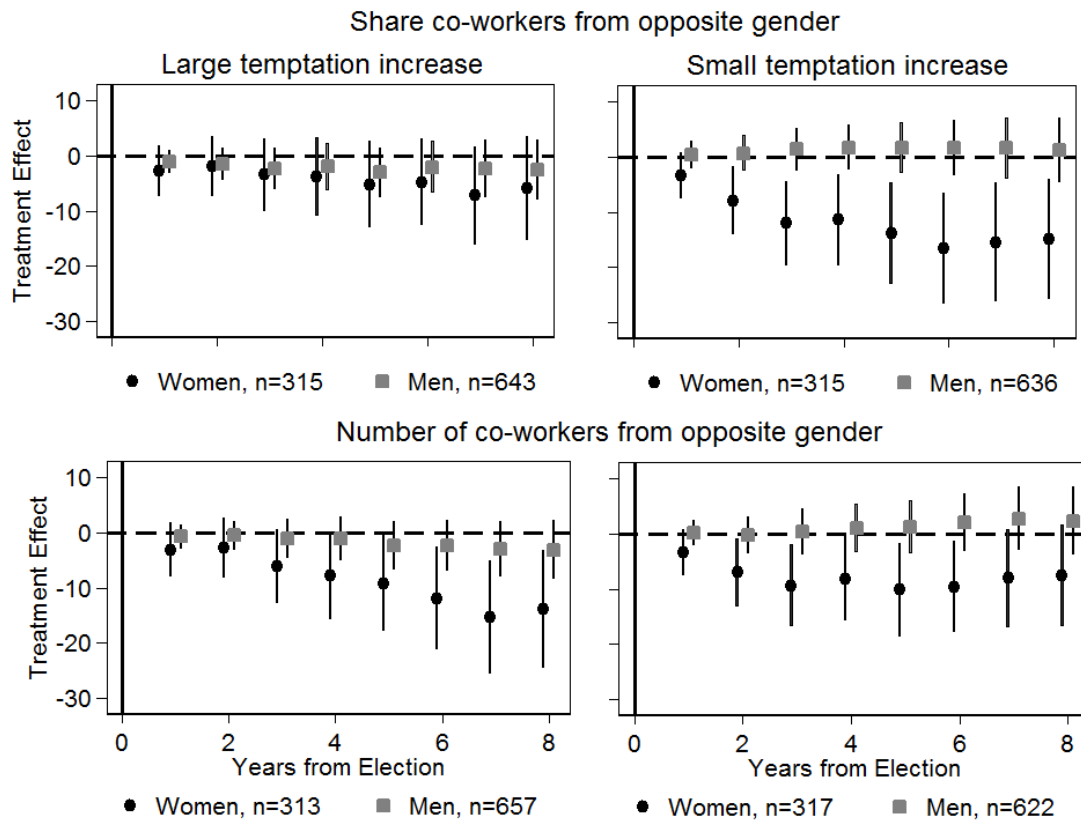
Figure W12. The effect of political promotion on remaining married in sub-samples based on the pre-promotion share of co-workers of the opposite sex.

Notes: Details about the estimation and the 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 as 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 the 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.
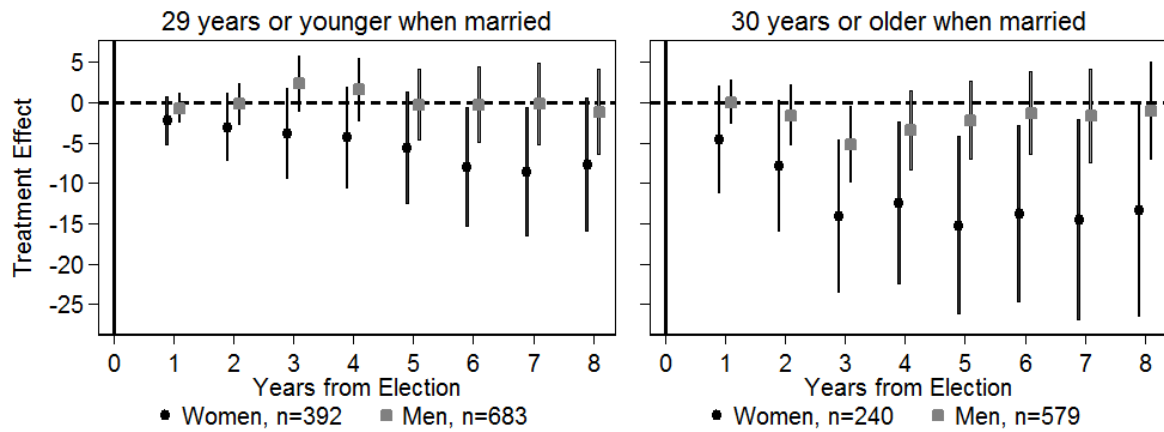
Figure W14. The effect of political promotion on remaining married in sub-samples based on the politician's age at marriage.

Notes: Details about the estimation and the content of the figure are available in the notes below Figure 5 in the main text. The sample is subdivided by 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).

Appendix References

Freier, Ronny, and Christian Odendahl. 2015. Do Parties Matter? Estimating the Effect of Political Power in Multi-Party Systems. *European Economic Review* 80: 310–28.

Fiva, Jon, Olle Folke and Rune Sørensen. 2016. The Power of Parties: Evidence From Close Municipal Elections in Norway. *Scandinavian Journal of Economics*, forthcoming

Folke, Olle, Torsten Persson, and Johanna Rickne. 2016. Dynastic Political Rents. *Economic Journal*, forthcoming.