

## All the Single Ladies: Job Promotions and the Durability of Marriage<sup>†</sup>

By OLLE FOLKE AND JOHANNA RICKNE\*

*We study how promotions to top jobs affect the probability of divorce. We compare the relationship trajectories of winning and losing candidates for mayor and parliamentarian and find that a promotion to one of these jobs doubles the baseline probability of divorce for women, but not for men. We also find a widening gender gap in divorce rates for men and women after being promoted to CEO. An analysis of possible mechanisms shows that divorces are concentrated in more gender-traditional couples, while women in more gender-equal couples are unaffected. (JEL J12, J16, M12, M51)*

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

This paper contributes to understanding women's absence from top jobs by investigating the effect of job promotions on divorce. We add to previous work about the friction between marital stability and women's career success. Research has shown that increases in women's—but not men's—earnings are correlated with divorce (Becker, Landes, and Michael 1977; Weiss and Willis 1997). Higher levels of divorce and marital problems have also been found in couples where the

\*Folke: Uppsala University, Department of Government, Box 514, 751 20 Uppsala, Sweden (email: [olle.folke@statsvet.uu.se](mailto:olle.folke@statsvet.uu.se)); Rickne: Stockholm University, Institute for Social Research (SOFI), 106 91 Stockholm, Sweden (email: [johanna.rickne@sofi.su.se](mailto:johanna.rickne@sofi.su.se)). Ilyana Kuziemko was coeditor for this article. Financial support from the Forte (2016-00363) and the Riksbankens Jubileumsfond (P16-0786:1) is gratefully acknowledged. The authors thank Pamela Campa, Hilary Hoynes, Petra Persson, Cecilia Josefsson, Pär Zetterberg, Sara Cools, Lena Edlund, Hilda Ralsmark, Mari Rege, and seminar participants at Berkeley OEW Seminar, Berkeley RWP, Columbia University Political Economy Seminar, the HEC political economy conference, Harvard University political economy workshop, Yale University, Harvard University gender and politics workshop, International Political Science Association, NICEP Inaugural Conference, Oxford University, Rice University, the Swedish National Conference for Economists, Stanford University, Stockholm University IIES, Stockholm University SOFI, UCL annual workshop, Texas A&M, Uppsala University Political Science Department, the Uppsala Conference on Gender Economics of Labor Markets, and the Linköping Institute for Advanced Sociology for helpful comments. We also thank Jonas Ahlerup, Johan Arntyr, Sirius Dehdari, Roza Khoban, and Elin Molin for excellent research assistance and Kelley Friel for excellent editorial assistance.

<sup>†</sup>Go to <https://doi.org/10.1257/app.20180435> to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

wife earns more than the husband (Bertrand, Kamenica, and Pan 2015). Speed dating experiments have shown that men shy away from women they perceive to be smarter or more ambitious than themselves (Fisman et al. 2006). Evidence that the career-relationship trade-off is noticeable for women further down on the career ladder comes from recent field experiments on MBA students. When told that their self-reported ambitions would be shared with fellow students, single women drastically understated their ambition levels compared to women already in relationships (Bursztyn, Fujiwara, and Pallais 2017).

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

The results show that a promotion doubles the divorce rate among promoted women compared to those who tried, but failed, to get the promotion. After three years on the new job, 7 percentage points fewer of the female mayors and parliamentarians remained married to their spouse compared to women who ran for office but lost. This result is corroborated in a subsample of closely contested elections, a situation in which the promotion is quasi-randomly assigned between job rivals.

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

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

<sup>1</sup> These jobs are in the top 5 percent of the annual Swedish earnings distribution (authors' calculations; see online Appendix Figure W1). They are paid slightly less than representatives in the US Congress and nearly the same as California state legislators. Our sample of Swedish CEOs would have their closest equivalent in CEOs of midsize American firms.

For women in traditional marriages, reaching a top job is likely to cause more adjustment costs—such as stress and strain from task renegotiation—than for men in these traditional unions.

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

Our paper contributes to the economics, political economics, political science, and sociology literatures. In economics, we supplement the growing discussion of the causes and consequences of career inequality by gender (e.g., Lazear and Rosen 1990; Bertrand, Goldin, and Katz 2010; Bjerk 2008; Booth, Francesconi, and Frank 2003; and 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.<sup>3</sup> 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.<sup>4</sup> 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 and Mattozzi and Merlo 2008). Our findings suggest that marital stability may enter differentially into women’s and men’s cost-benefit analyses when deciding whether to run for political office. Notably, a negative demonstration effect on junior women is fully compatible with a situation where divorce makes promoted women better off (emotionally, financially, or both). As long as a subset of men and women who contemplate a career also value a continued relationship, a perceived trade-off for women but not for men will create a gender-skewed candidate pool.

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

<sup>3</sup> In addition to the work mentioned earlier, see Kesseling and Bremmer (2006) and Newman and Olivetti (2018), as well as work on how the negative economic shock of unemployment triggers the risk of divorce when the husband, but not the wife, becomes unemployed (Rege, Telle, and Votruba 2007; Charles and Stephens 2004; Eliasson 2012; Doiron and Mendolia 2012).

<sup>4</sup> A number of papers study *monetary* costs, either empirically (e.g., Berg 2018; Eggers and Hainmueller 2009; and Fisman, Schulz, and Vig 2014) or theoretically (e.g., Diermeier, Keane, and Merlo 2005 and Mattozzi and Merlo 2008).

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

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

## I. Data and Sample Selection

We use Swedish register data for the country's entire working-age population, which contains yearly observations from 1979 to 2012. Each person has a mandatory ID code, which is recorded in interactions with numerous public authorities and thus links individuals to various administrative registers. These registers can be pooled into high-quality datasets with little misreporting and few missing observations.

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

Cohabitation is not perfectly measured in Swedish registers. Joint family ID codes are assigned if a couple has a child together or lives together in a single-family home, i.e., not in an apartment. Couples without children or who live in apartments are thus excluded. Our study does not extend to cohabitation for this reason and because a large share of individuals in our sample—61 percent of women and 70 percent 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 V). Between 1979 and 1989, we extract this information from the Income and Taxation Register. Additional background variables taken from LISA are sex, birth year, birth region, education length, industry code, and occupation code.

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

### *A. 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 (online Appendix Figure W1). Being promoted to one of these two jobs results in a similar average increase in earnings of about 20 percent (see Berg 2018 for an analysis of parliamentarians; for mayors, see Figure 7). Like other top jobs, they also offer a more generous pension plan than middle- or lower-level jobs. Work hours are long, and parliamentarians also frequently commute to Stockholm from their home district (we return briefly to this variation in the robustness checks).

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

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

<sup>5</sup> 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

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

The mayor is nearly always chosen via the bloc-level electoral contest between the top-ranked candidates in the largest parties in the two political blocs. But there are three deviations from this rule in the data. First, in 21 percent of the elections, political parties outside the traditional blocs won enough votes so that neither of the two blocs received a seat majority. Second, in 10 percent of the elections in which a bloc *did* win a seat majority, the largest party did not appoint the mayor. In our sensitivity tests, we show that our results are robust to excluding these elections. The third deviation is that the top-ranked person on the party's ballot fails to become mayor in about 10 percent of the elections.<sup>7</sup> This measurement error biases our results toward a null effect from promotion, since we count some people as promoted even though they were not.

Parties' electoral fortunes can shift over time to allow politicians to first win, then lose, and then get reelected. 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 the robustness checks section). Nevertheless, since the split-sample analysis in the mechanism section quickly suffers from small-sample issues, we keep the repeating losers throughout to avoid going back and forth between different estimation samples.<sup>8</sup>

---

candidates is elected and is therefore unimportant for our analysis. First, only one-third of voters utilize their voluntary vote. Second, the vast majority of those who do use their voluntary vote select the top candidates on the ballot rather than marginal candidates. Third, the threshold of votes needed to win a seat is prohibitively high (see Folke, Persson, and Rickne 2016 for a detailed description).

<sup>6</sup>In-depth descriptions of the positions of mayor and opposition leader can be found in Nilsson (2001), Jonsson (2003), and Montin (2007).

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

<sup>8</sup>A potential concern with including the repeat losers is that we might get an overrepresentation in the sample of people with more stable relationships. This would happen if a person who lost an election becomes more likely to reappear in the analysis sample if they did not divorce after the first loss. But bias from this sample selection is likely small, since nearly all repeated losers run again in the election immediately following the first loss. As these repeat losers are selected for the analysis sample in  $t = -4$ , which is the previous election year, the selection is done before their relationships are (potentially) affected by that election outcome. Our robustness tests also show that the estimated treatment effect is larger, not smaller, if we remove the repeated losers (see online Appendix Figure W6).



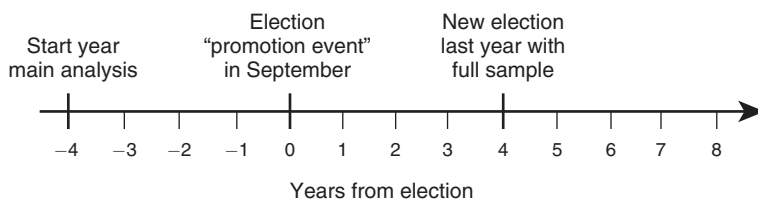


FIGURE 1. TIMING OF EVENTS

We pool the rival candidates for the positions of mayor and parliamentarian in all six elections between 1991 and 2010.<sup>9</sup> We then go back in time four years before the election in which a particular person was a candidate ( $t = -4$ ). In this year, we drop politicians who were not married, leaving 70 percent of the men and 61 percent of the women (robustness checks show that the results are not driven by the choice of starting year).<sup>10</sup> We also exclude people who reached the age of 65 before the end of the election period (10 percent of the sample), a potential confounder for divorce if promoted politicians stay longer in the workforce (although the sample restriction does not affect the estimated results). The final sample includes 641 women and 1,246 men. As illustrated in Figure 1, we follow the relationship statuses of this sample over time, starting four years prior to the election in  $t = -4$ , up to the election in  $t = 0$ , and eight years afterwards to  $t = 8$ . We conduct the analysis on a pooled sample of promotion winners and losers from *different* electoral ballots or municipal elections (following, e.g., Eggers and Hainmueller 2009 and Kotakorpi, Poutvaara, and Terviö 2017) because two married women rarely appear as (close) contestants on the same ballot or municipal election.

### B. Sample Selection: CEOs

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

<sup>9</sup> We exclude the 1994 parliamentary election because of erroneous recording of the list-rank variable for this election.

<sup>10</sup> Three same-sex partnerships among women politicians were excluded from the sample to facilitate our later analysis of heterogeneity between gender-equal and gender-traditional couples. There were no registered same-sex partnerships among the men in the estimation sample.

## II. Descriptive Statistics

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

We start by comparing the socioeconomic characteristics of job candidates prior to the election that assigns the promotion. Relevant variables are derived from previous research on labor market and marriage durability and from sociological research on the drivers of divorce (see, e.g., Becker, Landes, and Michael 1977; Weiss and Willis 1997; and Amato and Previti 2003). Some of these variables cannot be affected by the election/campaign work and are therefore measured in the election year ( $t = 0$ ). These individual-level variables include age, age at marriage, and a dummy variable for having completed tertiary education. At the household level, we measure marriage length, a dummy for whether the marriage is not the politician's first, and a dummy for whether both spouses were born in Sweden. Family structure is measured using two dummies that measure whether the household has children: one indicates at least one child aged 0–17, and the other denotes at least one child aged 0–6.

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

We compute the division of parental leave based on insurance payments, which is available for the full period, and cross-check this information against data for days of leave, available from 1993.<sup>11</sup> Payments are summed for the three first years of each child's life; if the couple has more than one child, they are averaged across all children. We only consider joint children with the current (pre-promotion) spouse.

Table 1 compares the means of the pre-promotion traits within genders and between persons who were (subsequently) promoted and those who were not.

<sup>11</sup> Section W1 of the online Appendix shows that these measurements are highly correlated.



TABLE 1—COMPARISON OF PRE-PROMOTION TRAITS

	Women		Men	
	Yes	No	Yes	No
<i>Subsequently promoted</i>				
<i>Couple characteristics</i>				
Marriage length (years)	20.76	21.01	20.91	20.78
Second marriage (share)	0.03	0.04	0.01	0.01
Same birth region (share)	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)	<b>0.66</b>	<b>0.53</b>	0.90	0.87
Politician's share of parental leave	0.83	0.81	0.14	0.15
Age difference (politician-spouse)	−3.70	−4.01	1.70	1.85
<i>Individual characteristics</i>				
Politician's age	48.60	48.94	49.81	50.89
Politician's age at marriage <sup>a</sup>	28.10	28.14	29.02	30.17
Politician's earnings <sup>b</sup>	<b>335.4</b>	<b>290.6</b>	<b>370.4</b>	<b>344.3</b>
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 percent level or lower, using ordinary least squares (OLS) regressions with standard errors clustered at the individual level. All measures of earnings are in units of 1,000s SEK (1 SEK  $\approx$  0.12 USD).

<sup>a</sup> This is a dummy variable that indicates whether the spouses are from different birth regions: Sweden; other Nordic countries; EU27, excluding Nordic countries; Asia; Europe, excluding Nordic countries and EU27; South America; Africa; North America; Soviet Union; and Oceania.

<sup>b</sup> 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).

A simple regression is used to detect statistically significant differences at the 5 percent level.<sup>12</sup>

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

Differences in traits *between* men and women cannot be omitted variables in the main analysis, which assesses men and women separately. But such differences are relevant for understanding differences in the size of the treatment effect for women and men. The men and women in our sample have a similar average age (roughly 50), and a similar length of marriage (20 years). Close to 10 percent of both men

<sup>12</sup> We run a regression instead of a *t*-test so that we can cluster the standard errors at the individual politician level.

and women have children under 6 years old in the household. There are also traits that differ. Men appear to be in relationships that focus on their careers: they are older than their spouse, earn more, and do less care work. The women appear to be in relationships with a more equal distribution of earnings, but they are generally younger and have used more than 80 percent of the couple's total parental leave. This means that even among the women in our sample, who are candidates for top jobs, many are in relationships that specialize in the husband's career (we return to these differences in Section VI).

We also compare the women and men in the estimation sample to the general Swedish population in terms of civil status (percent married/divorced) and family structure (percent with any children, percent with at least one non-adult child, percent with at least one small child). The population data are weighted to correspond to the composition of birth cohorts and year of observation for the estimation sample (the detailed analysis can be found in online Appendix Section W2 and Table W1).<sup>13</sup> The key takeaway from the description is that men with high-powered careers deviate from the population by having more stable relationships, while women with similar careers do not. Women in our estimation sample instead have highly similar family structures to the general population, echoing previous descriptions of Swedish professional women as equally likely as those with less education to have children or to ever marry (e.g., Bertrand et al. 2016 and Boschini et al. 2011).

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

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

<sup>13</sup> We also weigh the population sample to match politicians in terms of their municipality of residence and education level, and we do a separate comparison between political candidates and people who hold upper-level jobs (as indicated by the occupation code).

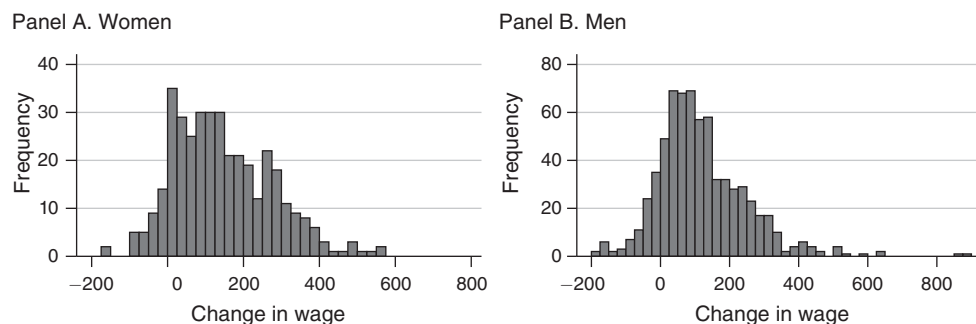


FIGURE 2. DISTRIBUTION OF CHANGES IN EARNINGS BEFORE AND AFTER PROMOTION (1,000 SEK)

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

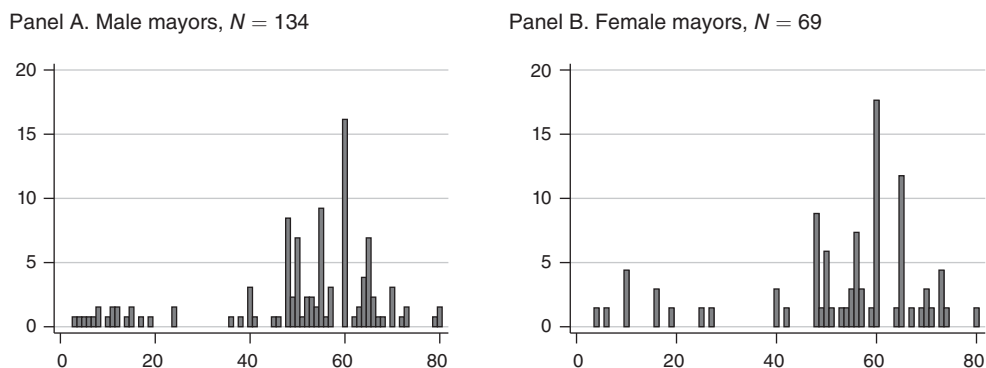


FIGURE 3. SELF-REPORTED WORK HOURS IN A TYPICAL WEEK BY MALE AND FEMALE MAYORS

*Source:* Data from the authors' survey of mayors in 2013

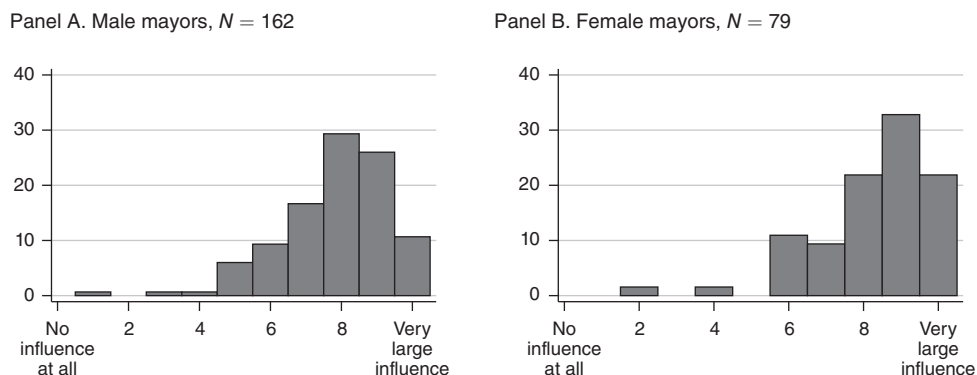
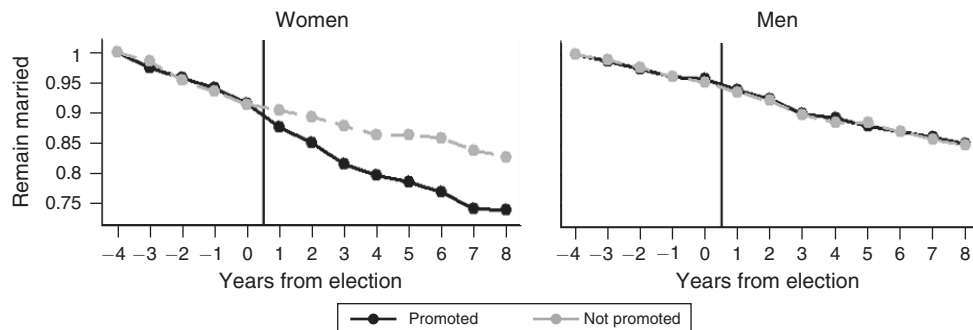


FIGURE 4. COMPARISON OF MALE AND FEMALE MAYORS' SELF-PERCEIVED POLITICAL INFLUENCE

*Note:* The survey question asked the politician to rate his or her own influence over "policy outcomes at the municipal level."

*Source:* Data from the 2012 KOLFU survey (Karlsson and Gilljam 2014) of all municipal politicians (response rate among mayors = 83 percent, 241/290)

Panel A. Descriptive statistics, women and men separately



Panel B. Difference-in-difference estimates, women and men combined

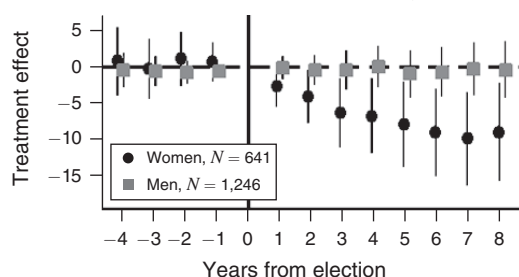


FIGURE 5. THE EFFECT OF POLITICAL PROMOTION ON REMAINING MARRIED

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

### III. Promotion and Divorce among Mayors and Parliamentarians

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

Inspecting the rate of marriage durability in the years leading up to the promotion event in  $t = 0$ , we see no differences between men and women who are subsequently promoted (or not). This validates the key identifying assumption of our design—the lack of differential trends in divorce rates between the treatment and control groups prior to the promotion. In the years after the promotion, the trend lines for women start to diverge. Promoted women get divorced at twice the rate of non-promoted women, resulting in a 7 percentage point divorce gap three years after the election. Looking at the long-term development, we can also see that the

gap continues to grow, rather than converge, over time. For men, there is no difference in divorce rates between promoted and non-promoted in either the short or long run.

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

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

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 ( $\beta_t$ ) 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 preexisting difference in the trend in divorce rates between *subsequently* promoted or non-promoted people *before* the promotion. Correspondingly, if the promotion causes a divergence in marriage durability between the promoted and non-promoted, this should show up as positive or negative estimates for  $\beta_t$  in the years after the election ( $t = 1$  to  $t = 8$ ).

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

Figure 5 plots the estimates of  $\beta_t$  together with 95 percent confidence intervals. The gray points and lines show the estimated promotion effect for men, and the black dots and lines show the estimates for women. These estimates show the exact same patterns as in the descriptive evidence in the top half of the figure. For both men and women, the pre-promotions estimates are close to zero and lack statistical significance. After promotions, there is no indication of an effect for men, but a sizeable negative effect for women. Three years after the election, promoted women are 7 percentage points less likely to remain married than their non-promoted counterparts.

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

#### *A. Robustness Test in Close Elections*

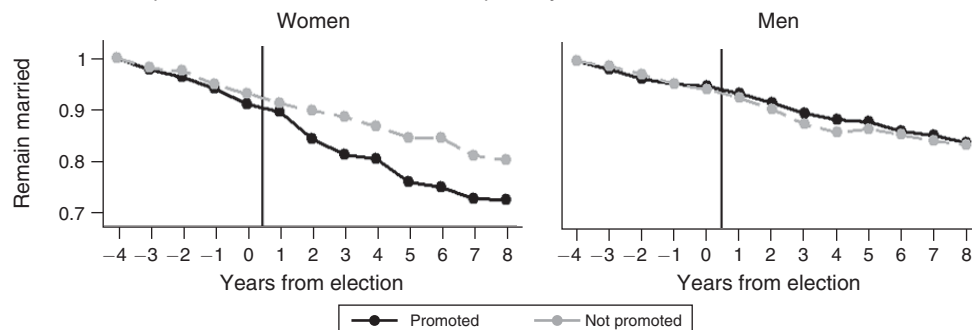
Despite the parallel time trends in marriage durability before promotion and the balance of most pre-promotion traits, a concern about our main analysis could be that women—but not men—decide to pursue a promotion when their marriage is on the rocks. We address this concern by narrowing the sample of 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 percent. 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 percent of the simulations. Online Appendix Section W3 contains a detailed description of how we calculate the municipal and parliamentary indicators of close elections. Online Appendix Table W2 replicates Table 1, the descriptive statistics for pre-promotion traits, in the close election sample. As expected, differences in traits between winning and losing candidates are (even) smaller than in the full sample.

Figure 6 replicates the main analysis (Figure 5) for the sample of close elections. The results are very similar to the main analysis, with one exception: the treatment effect is postponed by one year. This is logical, given that the average promotion in this sample becomes evident on election night rather than when the ballot rank order is set by the party 10–12 months before. Although the treatment effect is postponed by one year, the size of the estimated effect is still the same at the end of the election period ( $t = 3$ ). The descriptive evidence suggests that the



Panel A. Descriptive statistics, women and men separately



Panel B. Difference-in-difference estimates, women and men combined

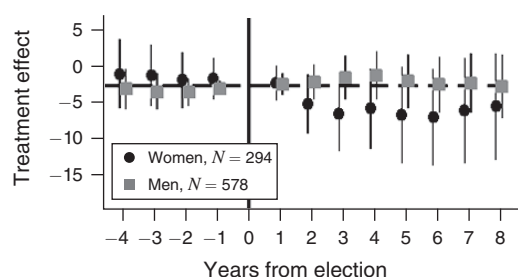


FIGURE 6. THE EFFECT OF POLITICAL PROMOTION ON REMAINING MARRIED IN A SUBSAMPLE OF CLOSE ELECTIONS

*Notes:* The structure of the plot is described in the note under Figure 5. For municipalities, a close election is defined as having a win margin less than 5 percent. 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 percent of the simulations described in online Appendix Section W3.2.

promotion doubles a woman's probability of getting divorced in the three years after the election. But compared to the main analysis, the effect is less precisely estimated due to the smaller sample size. The estimate at  $t = 3$  is barely below the 10 percent significance level.

### B. Robustness Tests for Sample Selection and Pre-trends in Earnings

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

First, we re-estimate equation (1) but include control variables for all the predetermined characteristics and traits that were examined in Table 1 (except the division of parental leave, which is missing for the 45 percent of the sample with children born before 1980). All controls are included as an interaction with the

time dummy variables, and the results replicate the baseline findings (see online Appendix Figure W5). The second test is to re-estimate equation (1) excluding candidates who previously ran for either of the offices but lost. Losing about one-fourth of the sample produces slightly larger standard errors, but the size of the point estimates is unaffected (see online Appendix Figure W6). The third robustness check (see Figure W7) shows that the results are unaffected across six alternative starting years for the sample selection of married politicians, ranging from  $t = -6$  to  $t = 0$ , and excluding the starting year used in the main analysis ( $t = -4$ ). The fourth test demonstrates the robustness of the results to excluding candidates with spouses who have retired, i.e., reached the age of 65, or will retire while the candidate is in office (online Appendix Figure W8).

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

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

A seventh and final robustness check examines the development of earnings, which are separately estimated for politicians and their spouses. The purpose is to test for differential trends in labor market behavior that can indicate sorting of people with more or less stable marriages into promotion and, subsequently, divorce. Women with less stable marriages might compete more fiercely to get elected and perhaps simultaneously strive harder for a promotion in their job outside of politics. This sorting would result in a positive, or possibly even a negative, pre-trend in the earnings difference between promoted and non-promoted women. It could also show up as a pretreatment trend in the differences in earnings between the politicians' spouses. If the politician increases his or her labor supply, the spouse may also increase their career input if joint leisure time is reduced, or reduce their labor input to take on more household responsibilities, or increase it to match the politician's increased work hours.

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

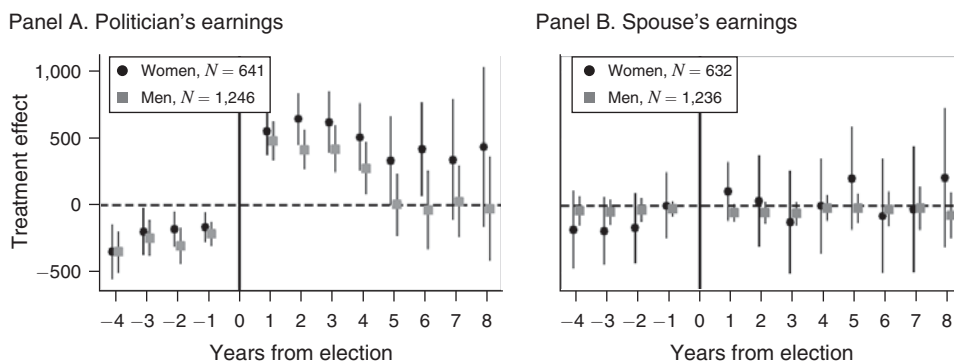


FIGURE 7. PRE-TRENDS IN ANNUAL EARNINGS OF POLITICIANS AND THEIR SPOUSES

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

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

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

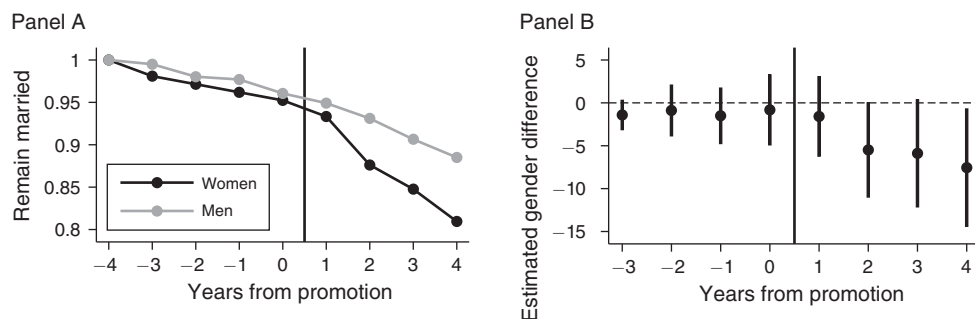


FIGURE 8. EVENT STUDY OF CEO PROMOTION AND REMAINING MARRIED

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

#### IV. 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 ninety-ninth percentile of the distribution of earnings within the working-age population of employed persons (authors' own calculations; see online Appendix Figure W1).

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

Panel A of Figure 8 plots the proportions of men and women who remain married to their spouse in each year. A shorter post-promotion window, four years, is chosen to accommodate the shorter sample period compared to the political jobs. Panel B 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 percent level. Prior to the promotion, the sample shows no clear gender difference in rates of divorce. Although the descriptive event study of CEO promotions does not permit causal inference, the similarity to the political promotions analysis suggests that the baseline finding extends to the private sector.

Extending the analysis to more sectors is difficult because promotions are not readily measurable in register data. An attempt to further generalize the findings can be found in Section 5 in the online Appendix. We select five cohorts of university graduates in 1989–1993 from four university programs: medical doctors, priests, police, and pharmacists. We then go forward in time 20 years and measure each

person's career success relative to their graduating cohort. High career success is measured as an income above the cohort median and low success as below this amount. Comparing the divorce rate for men and women with high and low career success reveals patterns that support the main finding of the paper. In three out of four professions (pharmacists are the exception), successful women have higher divorce rates than unsuccessful women. For men, the relationship is the opposite: a high level of career success is associated with lower divorce rates.

## V. Mechanisms

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

### A. The “Temptation Effect”

A promotion can change a person's work environment and introduce them to new potential partners. If women's divorces are motivated by exposure to new potential partners, they should be concentrated among women who were the least exposed to opposite-sex coworkers prior to their promotion (following McKinnish 2004). We measure the proportion and number of opposite-sex coworkers in the politician's previous workplace by leveraging the plant ID code in the register data.<sup>14</sup> We exclude the politicians themselves from this calculation, and to reduce noise, we drop workplaces with fewer than 10 employees (less than 10 percent of the data). The sample is then split by the median of the share, or absolute number, of colleagues of the opposite sex. The two resulting groups have either “low” or “high” expected temptation effects. Estimating equation (1) in the two samples does not support the temptation effect (see online Appendix Figure W12). If anything, the promotion effect on divorce is larger for women who enter politics from relatively male-heavy environments.

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

<sup>14</sup> A workplace is defined by its Cfar code in LISA. This code ensures that we capture colleagues who were physically working together since it codifies the plant rather than the firm. Note here that the level of opposite-sex coworkers *after* the promotion is highly similar across all promoted politicians, so we can rely solely on the pre-promotion share for variation.

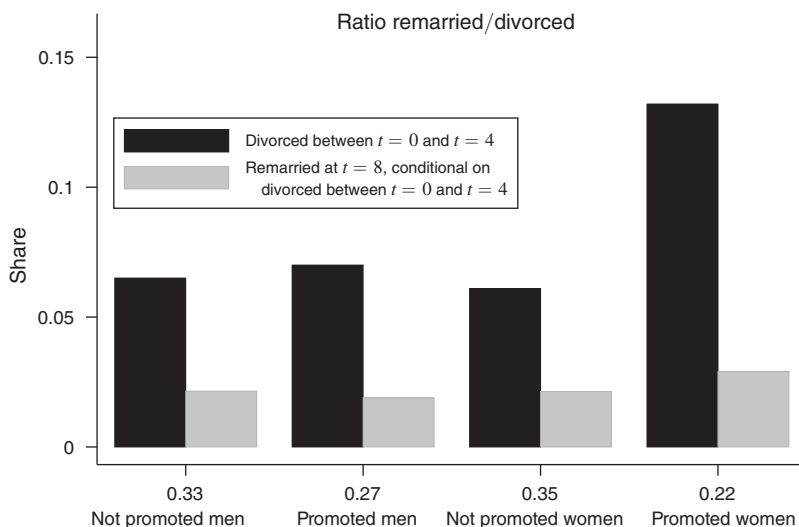


FIGURE 9. PROMOTION, DIVORCE, AND REMARRIAGE

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

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

### B. Traditional and Gender-Equal Marriages

Economic theory suggests that marriages may be destabilized if job promotions move the division of paid (or unpaid) labor away from the spouses' expectations of those divisions (e.g., Becker, Landes, and Michael 1977 and Weiss and Willis 1997). These expectations are formed early in the relationship, at the time of the "match." If a husband is promoted in a gender-traditional couple that has specialized around his career since its early stages, this conforms with expectations. If, however, the wife is promoted in a gender-traditional couple, this deviates from the expectation. In the context of our study, we expect women's divorces to be concentrated in traditional marriages rather than more gender-equal marriages.<sup>15</sup>

<sup>15</sup> A similar prediction can be drawn from sociology. A promotion may be seen as a critical transition point in a person's career. Such events can lower marital satisfaction by creating stress and conflict around the (expected)



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

We use two variables to capture gender-traditional marriages. The first is the spousal age gap, an indicator of gender-based specialization within the couple (Becker 1981, Eagly 1987).<sup>16</sup> The second is the division of parental leave, described in Section III. Like the spousal age gap, the split of parental leave measures gender-based specialization in the early phases of a relationship. This variable captures large time investments, since the Swedish leave policy covers 480 days of paid leave per child, and the vast majority of couples utilize the policy for at least 12 months (Statistics Sweden 2016).<sup>17</sup> For the people in our sample, parental leave and couple formation happened decades ago since their average age is 50 and the average marriage length is 20 years (recall Table 1).

Figure 10 shows the distributions of the spousal age gap and the parental leave split. Although they are candidates for jobs that fall in the top 5 percent of the earnings distribution, the women's relationships show clear signs of prioritizing the husband's career. Four out of five women in the sample are married to an older man and utilized more than 80 percent of the family's total parental leave themselves. Among the male politicians, we see a similar pattern of gender-based specialization. Two-thirds are married to a younger woman, and fewer than one in five utilized more than 20 percent of the couple's total parental leave.<sup>18</sup>

We divide the sample into three groups based on the spousal age gap: (i) the politician is younger by four years or more, (ii) the age gap is relatively small (three years or less),<sup>19</sup> and (iii) the politician is older by four or more years. For the split of parental leave, we are forced to take a liberal view about what constitutes a more or less traditional marriage. A gender-traditional marriage is defined as a couple in which she took more than 90 percent of the leave, and remaining marriages are considered gender-equal.

The results in Figures 11 and 12 show that divorce among promoted women is concentrated in couples that were more specialized around the husband's career

---

roles of the husband and wife (e.g., Coverman 1989). In our context, such conflicts could be expected to be the greatest for couples that started out with a more gender-traditional division of roles, but the wife is later promoted to a top job.

<sup>16</sup> Using the terminology of social exchange theory, a couple in which the husband is older than the wife is organized to benefit from social exchanges that transfer economic resources from the husband to the wife and resources such as sex and children from the wife to the husband (following Thibaut and Kelley 1959).

<sup>17</sup> The first 390 days have a wage-replacement rate of 80 percent and the remaining 180 days a fixed payment rate of 20 euros. People whose earnings do not qualify them for the wage-replacement part of the benefits receive the fixed payment throughout.

<sup>18</sup> 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.

<sup>19</sup> The empirical findings remain the same if we change the cutoff points by one year in either direction.

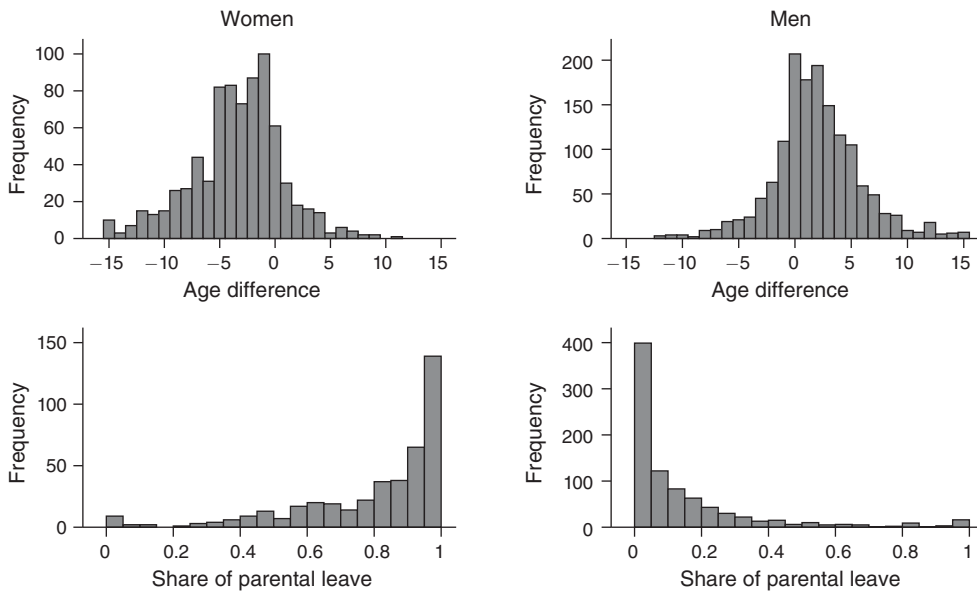


FIGURE 10. DISTRIBUTIONS OF THE SPOUSAL AGE GAP AND THE POLITICIAN’S SHARE OF PARENTAL LEAVE

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

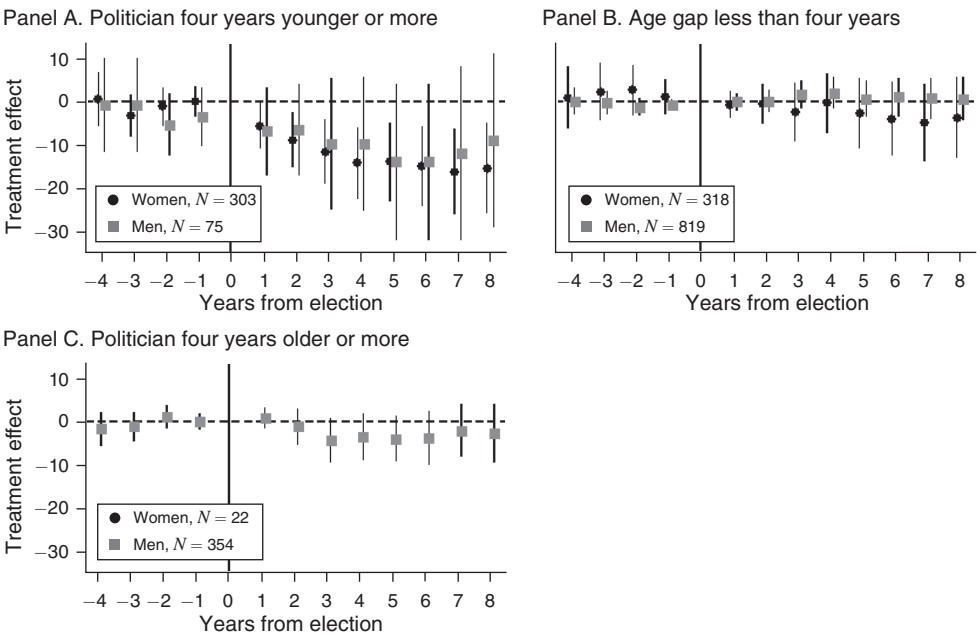


FIGURE 11. THE EFFECT OF POLITICAL PROMOTION ON REMAINING MARRIED IN SUBSAMPLES BASED ON THE SPOUSAL AGE GAP

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

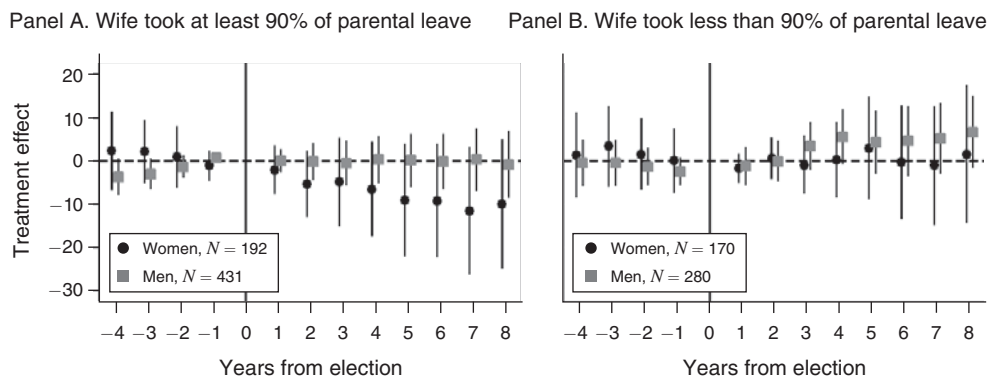


FIGURE 12. THE EFFECT OF POLITICAL PROMOTION ON REMAINING MARRIED IN SUBSAMPLES BASED ON THE WIFE'S SHARE OF TOTAL PARENTAL LEAVE

*Notes:* The figure shows DID estimates from equation (1) for two subsamples 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 III.

in the early stages of the relationship. Divorce is more likely to occur when the promoted woman (i) is younger by her spouse by a larger margin and (ii) took a relatively larger share of the parental leave. Strikingly, we find no divorce effect in the subsample of women in more gender-equal couples. When the couple is closer in age and when the husband took relatively more parental leave, there is no increase in the divorce rate after the woman's promotion. Comparing Figures 11 and 12, the precision is smaller when we split the sample based on the parental leave division, but the pattern is similar to that for the spousal age gap: there is a larger divorce effect in gender-traditional couples. Recall also that the sample size is smaller for this variable due to the unavailability of parental leave data for children born before 1980.

### C. Economic Independence and Other Explanations

A promotion may give women economic independence from their spouse and enable them to get a divorce (e.g., Oppenheimer 1997). Potential support for this mechanism comes from research showing that marriages are more sensitive to women's than to men's economic outcomes (Heckert, Nowak, and Snyder 1998; Jalovaara 2003; Liu and Vikat 2004; Kesselring and Bremmer 2006; but c.f. Rogers 2004).

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

The results, presented in online Appendix Figure W13, suggest that economic independence is *not* a key mechanism for women's increased divorce risk. The estimates for the two groups of earnings increases, above and below the median, are

similar in size and not statistically different from each other, for either absolute or relative earnings. The lack of evidence of an economic independence effect is not surprising given the level of pre-promotion earnings of the women in our sample. Most of them already had high earnings before the promotion, and many also out-earn their spouse (recall Table 1).

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

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

A related interpretation of the findings in this section is that promoted women divorce men who are less supportive of their careers. Some additional descriptive statistics support this interpretation, while others do not. In support, promoted and divorced women are slightly more likely to be reelected to office than promoted and divorced men. However, we find no correlation between divorce and characteristics of the husband that could proxy for a lower marriage utility for the wife.<sup>20</sup>

<sup>20</sup> We tested the husbands' level of earnings prior to the promotion, the husband's education level, and the husband's cognitive and noncognitive skills measured in Sweden's military draft (results available from the authors). Unfortunately, there are too few remarriages among promoted and divorced women and their spouses for us to examine "updated" spousal choices.

## VI. Conclusions

We study the consequences for men's and women's relationships of being promoted to top jobs. The main result is that promotions destabilize women's marriages but not men's. This finding can help explain the persistently small proportion of women in top jobs. In our data, married women and men who obtained top jobs had both been married for 20 years, on average—quite a significant time investment. At the time of their promotion, women were substantially more likely to lose their spousal support system than men. Giving up on the relationship may very well be the woman's choice and may be a positive outcome for her. But the results still highlight a large gender inequality in access to the first-best option for most: a functioning relationship *and* a successful career.<sup>21</sup> It is reasonable to expect that the candidate pool for top jobs would be skewed by a condition for women, but not for men, to put their relationship at risk.

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

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

Arguably, the Swedish welfare state and gender-egalitarian norms should provide an ideal environment for equal career opportunities. Household labor is more equally divided in Sweden than in most other countries, and affordable universal child and elder care relieves career-oriented families of substantial demands on their time. If the family is a source of tension for career women in this context, it might present even greater challenges in places with less

<sup>21</sup> In Sweden, more than 98 percent of women and men in the 2010 World Values Survey reported that “family” is “important” or “very important” in their lives.

generous policy conditions or social norms. However, a less permissive context could prohibit professional women from getting married in the first place (e.g., Bertrand et al. 2016). More research is needed to explore the joint developments of marriage and labor market across contexts and over time.

## REFERENCES

- Albrecht, James, Anders Björklund, 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 D. Cohen. 1997. *Political Cycles and the Macroeconomy*. Cambridge: 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." <https://www.scb.se/contentassets/b1ae4493fd1404987a4d32cbf213ae5/foretagsledarna-i-sverige---en-algoritm-for-att-peka-ut-foretagens-operativa-ledare-i-naringslivet.pdf>.
- 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 (ILR) Review* 60 (2): 163–86.
- Beaman, Lori, Raghabendra Chattopadhyay, Esther Duflo, Rohini Pande, and Petia 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. 1981. *A Treatise on the Family*. Cambridge: 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 (6): 1141–87.
- Berg, Hélène. 2018. "Politicians' Payments in a Proportional Party System." Stockholm University Department of Economics Research Paper 2018:3.
- Bertrand, Marianne. 2018. "Coase Lecture—The Glass Ceiling." *Economica* 85 (338): 205–31.
- Bertrand, Marianne, Patricia Cortés, Claudia Olivetti, and Jessica Pan. 2016. "Social Norms, Labor Market Opportunities, and the Marriage Gap for Skilled Women." NBER Working Paper 22015.
- Bertrand, Marianne, Claudia Goldin, and Lawrence F. Katz. 2010. "Dynamics of the Gender Gap for Young Professionals in the Financial and Corporate Sectors." *American Economic Journal: Applied Economics* 2 (3): 228–55.
- Bertrand, Marianne, Emir Kamenica, and Jessica Pan. 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." *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." Institute for Evaluation of Labour Market and Education Policy Working Paper 2011:15.
- Bursztyn, Leonardo, Thomas Fujiwara, and Amanda Pallais. 2017. "'Acting Wife': Marriage Market Incentives and Labor Market Investments." *American Economic Review* 107 (11): 3288–3319.
- 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 (3): 696–722.
- Diermeier, Daniel, Michael Keane, and Antonio Merlo. 2005. "A Political Economy Model of Congressional Careers." *American Economic Review* 95 (1): 347–73.



- Doiron, Denise, and Silvia Mendolia.** 2012. "The Impact of Job Loss on Family Dissolution." *Journal of Population Economics* 25 (1): 367–98.
- Eagly, Alice H.** 1987. *Sex Differences in Social Behavior: A Social Role Interpretation*. Hillsdale: Lawrence Erlbaum Associates.
- Eggers, Andrew C., and Jens Hainmueller.** 2009. "MPs for Sale? Returns to Office in Postwar British Politics." *American Political Science Review* 103 (4): 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): 101–09.
- Esping-Andersen, Gösta.** 2016. *Families in the 21st Century*. Stockholm: SNS Förlag.
- Fisman, Raymond J., Sheena Sethi Iyengar, Emir Kamenica, and Itamar Simonson.** 2006. "Gender Differences in Mate Selection: Evidence from a Speed Dating Experiment." *Quarterly Journal of Economics* 121 (2): 673–97.
- Fisman, Raymond J., Florian Schulz, and Vikrant Vig.** 2014. "The Private Returns to Public Office." *Journal of Political Economy* 122 (4): 806–62.
- Folke, Olle, Torsten Persson, and Johanna Rickne.** 2016. "The Primary Effect: Preference Votes and Political Promotions." *American Political Science Review* 110 (3): 559–78.
- 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." Stockholm University Swedish Institute for Social Research Working Paper 1/2010.
- Goldin, Claudia.** 2014. "A Grand Gender Convergence: Its Last Chapter." *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 (1): 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*. Nora: Nya Döxa.
- Karlsson, David, and Mikael Gilljam, eds.** 2014. *Svenska politiker: Om de folkvalda i riksdag, landsting och kommuner*. Stockholm: Santérus Förlag.
- Kesselring, Randall G., and Dale Bremmer.** 2006. "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.
- Kotakorpi, Kaisa, Panu Poutvaara, and Matti Terviö.** 2017. "Returns to Office in National and Local Politics: A Bootstrap Method and Evidence from Finland." *Journal of Law, Economics, and Organization* 33 (3): 413–42.
- 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): S106–23.
- 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." Max Planck Institute for Demographic Research Working Paper 2004-010.
- 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.
- Montin, Stig.** 2007. *Moderna Kommuner*. Malmö: Liber.
- Newman, Andrew F., and Claudia Olivetti.** 2018. "Career Women and the Durability of Marriage." <http://people.bu.edu/afnewman/papers/irony.pdf>.
- Nilsson, Thorbjörn.** 2001. "Den lokalpolitiska karriären: En socialpsykologisk studie av 20 kommunalråd." PhD. diss. 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.
- Rege, Mari, Kjetil Telle, and Mark Votruba.** 2007. "Plant Closure and Marital Dissolution." Statistics Norway Research Department Discussion Paper 514.
- Rogers, Stacy J.** 2004. "Dollars, Dependency, and Divorce: Four Perspectives on the Role of Wives' Income." *Journal of Marriage and Family* 66 (1): 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." *Industrial and Labor Relations Review* 66 (2): 380–408.
- Statistics Sweden.** 2016. "Women and Men in Sweden 2016: Facts and Figures." <https://www.scb.se/publication/34336>.
- Thibaut, John. W., and Harold Kelley.** 1959. *The Social Psychology of Groups*. Oxford: Wiley.
- Weiss, Yoram, and Robert J. Willis.** 1997. "Match Quality, New Information, and Marital Dissolution." *Journal of Labor Economics* 15 (1): S293–329.