Online Appendix of "The Ex-Factor: Examining the Gendered Effect of Divorce on Voter Turnout"

Details on data and measures

This section provides a description of the data availability, data sources and the main variables used in this study.

Data availability and replication

We use individual level data from Swedish registers. The data material is located on an encrypted server to which we have to log in through a remote desktop application in order to perform all of our data analyses. Due to the extreme sensitivity of the data, we are under contractual and ethical obligation not to distribute these data to others.

For those researchers who want to replicate our results there are two ways to get access to the administrative data. The first way is to order the data directly from Statistics Sweden (SCB). Statistics Sweden presently requires that researchers obtain a permission from a Swedish Ethical Review Board before data can be ordered (a description, in Swedish, of how to order data from Statistics Sweden is available at: https://www.scb.se/en/services/guidance-for-researchers-and-universities/). We will also make available a complete list all of the variables that we ordered from Statistics Sweden for this project, together with our computer scripts.

The second way to replicate our analyses is to come to Sweden and reanalyze these data through the same remote server system that we used. Researchers interested in using this option should reach out to us prior to coming to Sweden so that we can apply for approval from the Ethical Review Board for the researcher to temporarily be added to our research team, which is mandatory in order to get access to the remote server system.

Variables and data sources

Turnout — Individual level turnout information from five elections - the national elections in 1970, 1982, 1994, 2010 and 2018. The information on the first four elections is retrieved from scanned electoral rolls and is currently stored at Statistics Sweden. The data for the 1982 election were collected, and generously shared with us, by Magnus Carlsson and Dan-Olof Rooth. The data from the 1970, 1994, and 2010 election were collected by Karl-Oskar Lindgren and Sven Oskarsson and the data-collecting prcedure is decsribed in Lindgren et al. (2019). As from the general election in 2018 Statistics Sweden collects population-wide turnout data as part of the official statistics.

Female — Equal to 1 if female. Information is retrieved from the Swedish Population Register.

Birth Year — Information is retrieved from the Swedish Population Register.

Start Marriage — Start-date of marriage. Information is retrieved from the Swedish Population Register.

End Marriage — End-date of marriage. For couples who live together with children that are 16 years or younger, there is a waiting period of six months between the filing of a divorce and its completion. We therefore predate the divorce date of these couples in our data with six months. Information is retrieved from the Swedish Population Register.

Couple ID — Two divorced individuals are defined as a couple if they have been observed as living in the same household in 1980, 1985 or during the period from 1990-2018 (the household variable is only available every 5th year before 1990) and have the same divorce date. In the small number of cases where individuals cannot be mapped to a couple through this procedure two individual are recorded as a couple if they are the only two unmatched individuals in the data with the exact same marriage and divorce dates.

Years of Education Educational attainment at the time of the election according to the three-digit Swedish standard classification of education (SUN 2000). Following the manual for classifying educational programmes in OECD countries (ISCED-97), we assigned the following years of schooling to each category: (old) primary school (7); (new) compulsory school (9); (old) junior secondary education (9.5); high school (10-12 depending on the program); short university (13); longer university (14-16 depending on the program); post-graduate (17–19 depending on type of post-graduate degree). The information on educational attainment is retrieved from the 1991–2010 waves of the Longitudinal integration database for health insurance and labour market studies (LISA by Swedish acronym).

Immigrant Background – Equal to 1 if the individual or at least one of his/her parents are born abroad; equal to 0 if the individual is born in Sweden by two Sweden-born parents. The information retrieved from the Swedish Population Register.

Family Income – The sum of individual disposable income (DispInk) for all individuals belonging to the same household. To increase comparison over time, income is expressed in 2010 prices. The information is retrieved from Longitudinal integration database for health insurance and labour market studies (LISA by Swedish acronym).

Number of Children – The number of children aged 0-17 living in the household. The information is retrieved from Longitudinal integration database for health insurance and labour market studies (LISA by Swedish acronym).

Social Allowance – Equal to 1 if the individual receives social allowance. The information is retrieved from Longitudinal integration database for health insurance and labour market studies (LISA by Swedish acronym).

Housing Allowance – Equal to 1 if the individual receives housing allowance. The information is retrieved from Longitudinal integration database for health insurance and labour market studies (LISA by Swedish acronym).

Municipality of residence – Code for the municipality. The information retrieved from the Swedish Population Register.

A.1 Sample and Sample Restrictions

The data used in this study has been gathered from various administrative registers maintained by Statistics Sweden. One of these registers contain yearly information on the civil status of all Swedish inhabitants together with the date when the individual first obtained this civil status code. So for individuals coded as married this date indicates the date of marriage, whereas it indicates the divorce date for the individuals who are divorced (an individual is coded as divorced in the registers until he or she remarries). We have access to these data for the period from 1972–2019.

From this universe of divorces we then identify all individuals who divorced in the period 1985–2019. The end date of a marriage in the administrative registers records the date when the marriage was legally dissolved. However, for couples who live together with children that are 16 years or younger, there is a waiting period of six months between the filing of a divorce and its completion. We therefore predate the divorce date of these couples in our data with six months. The reason why we choose 1985 as the start period is that 1994 is the first year for which we have access to information on both voter turnout and individual socio-economic characteristics. By using 1985 as the start date we can get a sufficiently long before period before the first election and make sure that a sufficiently large share of the individuals divorcing early in the period are still alive in the 2010 and 2018 elections. As can be seen from the first row in table A.1 there is a total of 1,384,089 unique individuals who have a divorce date recorded between 1985 and 2019 (a small share of these individuals may have been going through a divorce in other countries before moving to Sweden).

If it was possible to observe all these individuals in each of the three elections under study, i.e., 1994, 2010, and 2018, we could multiply the number of individuals by 3 to obtain the theoretical upper number of observations. However, many of the individuals divorcing late in the period, i.e., close to 2019, were too young to vote in 1994 and 2010, and some of those divorcing early in the period, i.e., close to 1985, were not alive in the latter elections in 2010 and 2018. Some individuals may also have resided abroad in some of the election years and then they cannot be included in our data.

Table A.1: The Impact of Various Sample Restrictions

Step	Restrictions	Individuals	Observations
(1)	Divorced 1985–2019	1,384,089	—
(2)	(1) + Swedish resident in the election year and eligible to vote	1,226,312	3,477,556
(3)	(2) + non-missing turnout data	1,218,777	3,330,795
(4)	(3) + non-missing couple ID	1,133,190	3,093,811
(5)	(4) + divorced only once	947,691	2,366,297
(6)	(5) + election year > marriage year	941,284	2,115,934
(7)	(6) + non-missing socio-economic data	938,068	2,109,334
(8)	(7) + two observed individuals in the couple after invoking (1)–(7)	739,664	1,684,967

To be included in our sample the individuals divorcing between 1985 and 2019 must have been residents and eligible to vote in at least one of the elections for which we have data. As can be seen from the second row of table A.1, imposing this restriction reduces the number of individuals to 1,226,312 and the number of observations (N$\times$T) to 3,477,556.

The scanned turnout data is, however, not perfectly complete in all years because some of the election rolls have disappeared in the archives. In step (3), we therefore require that we have access to turnout data to analyze. This reduces the number of individuals in our data to 1,218,777 and the number of observations to 3,330,795. That is we loose about 4 % of our observations and about 1 % of our individuals due to missing turnout data. The sample defined in Step (3) of table A.1 is thus the largest possible sample that we can use for our analysis, and we therefore present some results for this sample as a robustness check.

However, to study mechanisms and strengthen the credibility of our identification strategy we invoke four additional sample restrictions. First, in our preferred specification we use within-couple variation to identify the effects of interest. We must therefore link the two partners of a divorce to one another. Unfortunately, couple identifiers are not available in the divorce registry, but we have access to household identifiers for all adult Swedes for 1980 and 1985 annually between 1990 and 2018. By using this information together with information on divorce dates (see the data section above for more details) we have been able to create couple identifiers for most individuals in our data. As can be seen from the fourth row of table A.1 we loose about 7 % of our individuals and observations due to missing couple information. In some cases the missing couple information may be due to the fact that only one of the partners in a couple have been living in Sweden during the years that we observe. In this step we also drop a small number of divorces occurring in same-sex couples (same-sex marriage has been legal in Sweden since May 2009), since we only create the couple ID for male-female couples. The reason for excluding same-sex marriages is that there is no variation in gender within couples that can be used to identify the effect of interest.

In the fifth step we exclude all individuals who divorced more than once during the period 1972–2019 from the data. The reason for this is that an individual divorcing more than once could be simultaneously treated by two (or more) different divorces, e.g., an election could for instance occur three years after one divorce and five years prior to another divorce. By excluding individuals divorcing more than once we thus circumvent the problem of bundled treatments. Once we remove all individuals divorcing more than once the number of individuals in the data decreases from 1,133,190 to 947,691 and the number of observations from 3,093,811 to 2,366,297.

In Step (6) we exclude all observations of voting that occurred before an individual was married. The reason for invoking this restriction is that we want to use married individuals as the comparison group for estimating the divorce effect, rather than using a mixture of married and yet to be married individuals as the comparison group. Invoking this restriction reduces the number of individuals in the data to 941,284 and the number of observations to 2,115,934.

In some of the main analyses we control for various demographic and socio-economic characteristics. To make sure that the models with and without these controls are estimated on the same observations we drop a small number of observations for which data on one or more of these controls are missing. As described by Step (7) in the table we loose about

3000 individuals and about 6000 data points due to this.

Finally, to ease the interpretation of our within-couple model we require that we have at least one observations for each individual in a couple after having invoked the sample restrictions above. Not doing so, would imply that the couple fixed effects would instead be individual fixed effects for the couples made up of a single individual (and then we would have to drop all individuals without a spouse in the data for which we only have one observation because they would not contribute to identifying the effects of interest).

As described in the main text our final sample thus includes 1,684,967 observations for 739,664 individuals in 369,832 unique couples. For the reasons discussed above the number of observations for each individual and couple will thus vary. Table A.2 describes how many observations we have for each individual and couple respectively. For instance, the final dataset includes 3 observations for 323,117 individuals, and 6 observations for 133,616 couples.

Table A.2: Number of Observations by Individual and Couple

Observations	Individuals	Couples
1	117,478	–
2	299,069	36,098
3	323,117	24,691
4	–	140,133
5	–	35,294
6	–	133,616

Although we believe the sample restrictions presented here to be well justified, not all of them are required to examine how divorces impact turnout. To check the robustness of our results we therefore present a number of analyses, which invokes only a subset of these restrictions (see figures A.2 and A.6 later in this appendix).

A.2 Supplementary Analyses

In this section we provide some details on the auxiliary results and robustness checks briefly discussed in the main text.

A.2.1 Presentation of Regression Estimates

For reasons of completeness table A.3 displays the regression coefficients used to construct the figures displayed in the main text. The column headings indicate the name of the figure corresponding to the different results. In case a figure contains two subgraphs the leftmost subgraph is denoted *a*, and the rightmost subgraph is denoted *b*. For the first four models the year variable refers to the time elapsed from the divorce to the election, whereas the last model (Fig. 6b) refers to time elapsed between the date of marriage and the date of divorce. As described in the main text, the variable $\bar{y}_{g,c,t}$ denotes average turnout for each unique combination of birth year, sex, and election year. Standard errors are clustered on the couple identifier.

Table A.3: Regression Estimates. Dependent Variable: Turnout

	Fig. 3	**Fig. 5b**	**Fig. 7a**	**Fig. 7b**	**Fig. 6b**
Year (-8)	0.000	−0.002	−0.010	0.025	−0.030***
	(0.003)	(0.003)	(0.016)	(0.019)	(0.004)
Year (-7)	0.000	−0.001	0.006	−0.007	0.000
	(0.003)	(0.003)	(0.021)	(0.026)	(0.004)
Year (-6)	0.001	0.000	−0.008	0.038	−0.012**
	(0.003)	(0.003)	(0.021)	(0.026)	(0.005)
Year (-5)	−0.004	−0.004	−0.023	0.014	−0.003
	(0.003)	(0.003)	(0.020)	(0.027)	(0.005)
Year (-4)	0.003	0.003	−0.006	0.024	−0.003
	(0.003)	(0.003)	(0.021)	(0.025)	(0.005)
Year (-2)	−0.007**	−0.006*	0.004	0.011	0.001
	(0.003)	(0.003)	(0.020)	(0.026)	(0.004)
Year (-1)	−0.031***	−0.025***	−0.023	−0.041*	0.005
	(0.003)	(0.003)	(0.019)	(0.024)	(0.004)
Year (0)	−0.074***	−0.054***	−0.052***	−0.108***	0.011***
	(0.003)	(0.003)	(0.018)	(0.023)	(0.004)
Year (1)	−0.055***	−0.041***	−0.043**	−0.090***	0.021***
	(0.003)	(0.003)	(0.019)	(0.023)	(0.004)
Year (2)	−0.046***	−0.034***	−0.058***	−0.068***	0.018***
	(0.003)	(0.003)	(0.019)	(0.023)	(0.004)
Year (3)	−0.040***	−0.030***	−0.040**	−0.032	0.025***
	(0.003)	(0.003)	(0.019)	(0.024)	(0.004)
Year (4)	−0.035***	−0.026***	−0.031	−0.051**	0.013***
	(0.003)	(0.003)	(0.019)	(0.024)	(0.004)
Year (5)	−0.033***	−0.025***	−0.022	−0.037**	0.016***
	(0.003)	(0.003)	(0.014)	(0.018)	(0.004)
Year (6)	−0.026***	−0.019***	0.005	−0.054**	0.017***
	(0.003)	(0.003)	(0.018)	(0.024)	(0.004)
Year (7)	−0.029***	−0.022***	−0.024	−0.023	0.013***
	(0.003)	(0.003)	(0.019)	(0.023)	(0.004)
Year (8)	−0.014***	−0.009***	0.010	0.007	0.010***
	(0.002)	(0.002)	(0.015)	(0.019)	(0.003)
Male x Year (-8)	0.000	−0.003	−0.007	−0.012	0.003
	(0.002)	(0.002)	(0.016)	(0.015)	(0.003)
Male x Year (-7)	0.005*	0.004	0.012	0.047**	−0.001
	(0.003)	(0.003)	(0.018)	(0.021)	(0.004)
Male x Year (-6)	0.001	0.000	0.013	−0.005	−0.004
	(0.003)	(0.003)	(0.019)	(0.019)	(0.005)
Male x Year (-5)	0.001	0.000	−0.007	0.021	−0.004
	(0.003)	(0.003)	(0.020)	(0.020)	(0.004)
Male x Year (-4)	0.002	0.001	−0.011	−0.002	0.000
	(0.003)	(0.003)	(0.019)	(0.020)	(0.004)
Male x Year (-2)	−0.003	−0.002	−0.025	0.011	0.002

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Table A.3: Regression Estimates. Dependent Variable: Turnout

	Fig. 3	**Fig. 5b**	**Fig. 7a**	**Fig. 7b**	**Fig. 6b**
	(0.003)	(0.003)	(0.020)	(0.020)	(0.004)
Male x Year (-1)	−0.005	−0.002	−0.034*	0.019	0.006
	(0.003)	(0.003)	(0.018)	(0.019)	(0.004)
Male x Year (0)	−0.007**	−0.003	−0.090***	0.073***	0.008**
	(0.003)	(0.003)	(0.019)	(0.020)	(0.003)
Male x Year (1)	−0.018***	−0.011***	−0.115***	0.089***	0.010***
	(0.003)	(0.003)	(0.020)	(0.020)	(0.003)
Male x Year (2)	−0.020***	−0.013***	−0.090***	0.083***	0.009***
	(0.003)	(0.003)	(0.020)	(0.021)	(0.003)
Male x Year (3)	−0.027***	−0.019***	−0.078***	0.035*	0.009***
	(0.003)	(0.003)	(0.020)	(0.021)	(0.003)
Male x Year (4)	−0.024***	−0.015***	−0.103***	0.053**	0.010***
	(0.003)	(0.003)	(0.020)	(0.021)	(0.003)
Male x Year (5)	−0.023***	−0.015***	−0.079***	0.034*	0.010***
	(0.003)	(0.003)	(0.019)	(0.020)	(0.003)
Male x Year (6)	−0.027***	−0.019***	−0.103***	0.050**	0.009**
	(0.003)	(0.003)	(0.020)	(0.021)	(0.003)
Male x Year (7)	−0.023***	−0.016***	−0.086***	0.025	0.012***
	(0.003)	(0.003)	(0.021)	(0.021)	(0.003)
Male x Year (8)	−0.025***	−0.016***	−0.078***	0.023	0.010***
	(0.002)	(0.002)	(0.015)	(0.014)	(0.003)
Male	0.010**	0.003	−0.236***	0.277***	0.008**
	(0.003)	(0.004)	(0.024)	(0.025)	(0.003)
Education	0.010***	0.010***	0.005***	0.023***	0.005***
	(0.000)	(0.000)	(0.001)	(0.002)	(0.000)
Imm. Background	−0.024***	−0.024***	0.004	−0.028**	−0.016***
	(0.001)	(0.001)	(0.009)	(0.009)	(0.001)
Income		0.000***			
		(0.000)			
Nr. Children		0.007***			
		(0.000)			
Social Allow.		−0.041***			
		(0.002)			
House Allow.		−0.007***			
		(0.001)			
WI Mobility		−0.024***			
		(0.001)			
BW Mobility		−0.050***			
		(0.002)			
Male x Imm. Background	−0.025***	−0.024***	−0.021	0.027	−0.002
	(0.002)	(0.002)	(0.012)	(0.014)	(0.002)
Male x Education	0.001***	0.001***	0.018***	−0.017***	0.001***
	(0.000)	(0.000)	(0.002)	(0.002)	(0.000)
Male x Income		0.000***			
		(0.000)			
Male x Nr. Children		0.008***			
		(0.001)			
Male x Soc. Allow.		−0.028***			
		(0.003)			
Male x House Allow.		0.004*			
		(0.002)			
Male x WI Mobility		0.002			

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Table A.3: Regression Estimates. Dependent Variable: Turnout

	Fig. 3	**Fig. 5b**	**Fig. 7a**	**Fig. 7b**	**Fig. 6b**
		(0.002)			
Male x BW Mobility		0.006*			
		(0.003)			
$\bar{y}_{g,c,t}$	0.922***	0.902***	1.275***	0.921***	0.953***
	(0.013)	(0.013)	(0.090)	(0.101)	(0.016)
Observations	1,684,697	1,684,697	110,422	86,024	1,047,888

Table A.4 reports the regression estimates used to construct the different subgraphs displayed in Table 4 of the main text. The dependent variable changes across columns as follows: within municipality mobility (4a), between municipality mobility (4b), family income (4c), social allowance (4d), housing allowance (4e), and the number of children in the household (4f). The variable $\bar{y}_{g,c,t}$ denotes the average value of the outcome variable for each unique combination of birth year, sex, and election year. Standard errors are clustered on the couple identifier.

Table A.4: Regression Results. Dependent Variable: Various Indicators.

	Fig. 4a	**Fig. 4b**	**Fig. 4c**	**Fig. 4d**	**Fig. 4e**	**Fig. 4f**
Year (-8)	−0.011***	−0.006***	−38.302***	−0.031***	−0.060***	0.237***
	(0.003)	(0.002)	(8.373)	(0.002)	(0.003)	(0.009)
Year (-7)	−0.014***	−0.004	−1.578	−0.006**	−0.002	0.093***
	(0.004)	(0.002)	(8.825)	(0.003)	(0.004)	(0.012)
Year (-6)	−0.003	−0.002	0.140	−0.009***	−0.005	0.085***
	(0.003)	(0.002)	(10.517)	(0.003)	(0.004)	(0.012)
Year (-5)	−0.003	−0.004**	7.105	−0.012***	−0.010**	0.050***
	(0.003)	(0.002)	(8.025)	(0.003)	(0.004)	(0.012)
Year (-4)	−0.003	0.000	−3.248	−0.002	−0.006	0.037***
	(0.003)	(0.002)	(8.285)	(0.003)	(0.004)	(0.012)
Year (-2)	0.015***	−0.001	−10.918	0.005*	0.006	−0.055***
	(0.003)	(0.002)	(8.783)	(0.003)	(0.004)	(0.012)
Year (-1)	0.096***	0.017***	−50.070***	0.021***	0.055***	−0.202***
	(0.003)	(0.002)	(8.604)	(0.003)	(0.004)	(0.011)
Year (0)	0.322***	0.078***	−188.431***	0.058***	0.208***	−0.466***
	(0.004)	(0.002)	(10.632)	(0.003)	(0.004)	(0.010)
Year (1)	0.140***	0.044***	−224.089***	0.054***	0.237***	−0.522***
	(0.003)	(0.002)	(10.801)	(0.003)	(0.004)	(0.011)
Year (2)	0.091***	0.034***	−216.965***	0.045***	0.218***	−0.524***
	(0.003)	(0.002)	(8.774)	(0.003)	(0.004)	(0.011)
Year (3)	0.069***	0.027***	−218.456***	0.037***	0.186***	−0.524***
	(0.003)	(0.002)	(8.240)	(0.003)	(0.004)	(0.011)
Year (4)	0.060***	0.021***	−220.378***	0.028***	0.161***	−0.543***
	(0.003)	(0.002)	(8.732)	(0.002)	(0.004)	(0.010)
Year (5)	0.053***	0.021***	−218.416***	0.028***	0.147***	−0.546***
	(0.003)	(0.002)	(8.147)	(0.002)	(0.003)	(0.008)
Year (6)	0.043***	0.016***	−212.247***	0.027***	0.136***	−0.536***

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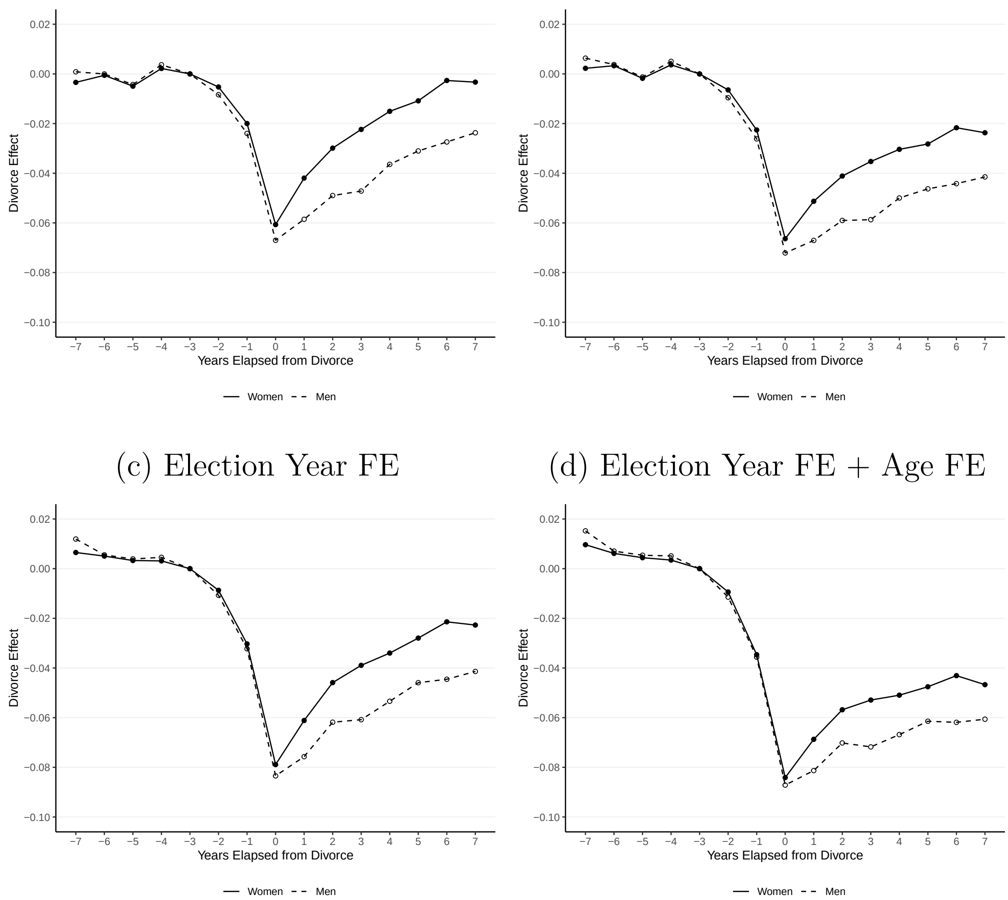
Table A.4: Regression Results. Dependent Variable: Various Indicators.

	Fig. 4a	**Fig. 4b**	**Fig. 4c**	**Fig. 4d**	**Fig. 4e**	**Fig. 4f**
	(0.003)	(0.002)	(8.678)	(0.002)	(0.004)	(0.010)
Year (7)	0.047***	0.019***	−210.619***	0.029***	0.129***	−0.551***
	(0.003)	(0.002)	(8.724)	(0.002)	(0.004)	(0.010)
Year (8)	0.023***	0.013***	−184.827***	0.017***	0.086***	−0.628***
	(0.002)	(0.002)	(9.551)	(0.002)	(0.003)	(0.009)
Male x Year (-8)	0.001	−0.001	−4.911***	0.003***	0.036***	0.046***
	(0.001)	(0.001)	(1.195)	(0.001)	(0.001)	(0.004)
Male x Year (-7)	0.004**	−0.002**	−3.209***	0.000	0.009***	0.019***
	(0.002)	(0.001)	(1.180)	(0.001)	(0.001)	(0.005)
Male x Year (-6)	0.002	−0.001	−4.416***	0.002	0.008***	0.017***
	(0.002)	(0.001)	(0.957)	(0.001)	(0.001)	(0.005)
Male x Year (-5)	0.001	0.000	−2.441**	0.002	0.007***	0.015***
	(0.002)	(0.001)	(1.186)	(0.001)	(0.001)	(0.005)
Male x Year (-4)	0.000	0.000	−3.204***	0.000	0.003*	0.009*
	(0.002)	(0.001)	(0.902)	(0.001)	(0.001)	(0.005)
Male x Year (-2)	−0.004**	0.000	0.570	0.001	−0.003*	−0.012**
	(0.002)	(0.001)	(0.948)	(0.001)	(0.002)	(0.005)
Male x Year (-1)	−0.035***	0.000	12.051***	−0.008***	−0.047***	−0.130***
	(0.003)	(0.002)	(1.889)	(0.001)	(0.002)	(0.006)
Male x Year (0)	−0.088***	0.003	52.972***	−0.034***	−0.161***	−0.416***
	(0.004)	(0.002)	(7.771)	(0.002)	(0.002)	(0.009)
Male x Year (1)	−0.022***	0.011***	41.508***	−0.030***	−0.190***	−0.480***
	(0.003)	(0.002)	(3.851)	(0.002)	(0.003)	(0.009)
Male x Year (2)	−0.015***	0.005**	41.597***	−0.026***	−0.184***	−0.466***
	(0.003)	(0.002)	(9.148)	(0.002)	(0.003)	(0.009)
Male x Year (3)	−0.008***	−0.001	33.104***	−0.020***	−0.171***	−0.454***
	(0.003)	(0.002)	(3.389)	(0.002)	(0.003)	(0.008)
Male x Year (4)	−0.010***	0.002	34.688***	−0.012***	−0.150***	−0.393***
	(0.003)	(0.002)	(4.774)	(0.002)	(0.003)	(0.008)
Male x Year (5)	−0.010***	0.003	40.665***	−0.014***	−0.144***	−0.363***
	(0.003)	(0.002)	(5.183)	(0.002)	(0.003)	(0.008)
Male x Year (6)	−0.005**	0.003*	28.449***	−0.013***	−0.136***	−0.352***
	(0.003)	(0.002)	(3.791)	(0.002)	(0.003)	(0.008)
Male x Year (7)	−0.011***	0.001	27.413***	−0.015***	−0.128***	−0.291***
	(0.003)	(0.002)	(3.816)	(0.002)	(0.002)	(0.008)
Male x Year (8)	−0.005***	−0.003***	37.724***	−0.004***	−0.088***	−0.141***
	(0.001)	(0.001)	(4.718)	(0.001)	(0.001)	(0.004)
Male	0.003	0.007***	−111.270***	−0.033***	−0.008***	−0.068***
	(0.002)	(0.002)	(15.491)	(0.002)	(0.002)	(0.007)
Education	0.000	0.001***	6.157***	−0.006***	−0.011***	−0.010***
	(0.000)	(0.000)	(1.218)	(0.000)	(0.000)	(0.001)
Imm. Background	−0.012***	0.000	2.127	0.006***	0.009***	0.001
	(0.001)	(0.001)	(11.552)	(0.001)	(0.001)	(0.003)
Male x Imm. Background	0.029***	0.006***	−36.736***	−0.012***	−0.020***	−0.036***
	(0.001)	(0.001)	(5.898)	(0.001)	(0.001)	(0.004)
Male x Education	0.000*	0.000**	10.565***	0.003***	0.008***	0.018***
	(0.000)	(0.000)	(1.184)	(0.000)	(0.000)	(0.001)
$\bar{y}_{g,c,t}$	0.920***	0.701***	0.982***	1.262***	1.489***	0.777***
	(0.016)	(0.019)	(0.025)	(0.017)	(0.006)	(0.002)
Dependent variable	WI Mobility	BW Mobility	Income	Soc. Allow.	House Allow.	Nr. Children
Observations	1,684,697	1,684,697	1,684,697	1,684,697	1,684,697	1,684,697

A.2.2 Alternative Ways to Adjust for Age and Election Effects

Figure A.1: Alternative Procedures to Deal with Collinearity

(a) Couple FE (b) Couple FE + Age FE



In the main text we discuss the fact that we are not able to directly control for age and election year fixed effects due to the inclusion of couple fixed effects in the regression models. To obviate this problem we add a control for average turnout within all unique combinations of birth year, sex, and election year.

In figure A.1 we show that the pattern of results is very similar when dropping this control from the model specification and instead include different combinations of couple, age and election year fixed effects among the regressors. More specifically, in panel (a) we only include couple fixed effects whereas the results in panel (b) are based on a model controlling for both couple and age fixed effects. In panels (c) and (d) we display corresponding results from models including controls for election year but dropping the couple fixed effects.

A.2.3 Replacing Couple Effects with Individual Fixed Effects

As explained in the main text, there is some uncertainty involved in identifying the married couples in our data. Therefore, we have re-estimated our main model replacing the couple fixed effects with individual fixed effects that can be directly observed in our data. When estimating the individual effects model we also drop some of the sample restrictions described in table A.1, which increases the effective sample size to 2,052,081 observations. More precisely, we drop restrictions (4), (7), and (8) in table A.1, but in addition we need to drop all individuals for which we only have a single observation in the data. This is because the individual fixed effects model require at least two time periods.

As can be seen from the results displayed in figure A.2 we obtain very similar results when replacing the couple fixed effects with individual fixed effects.

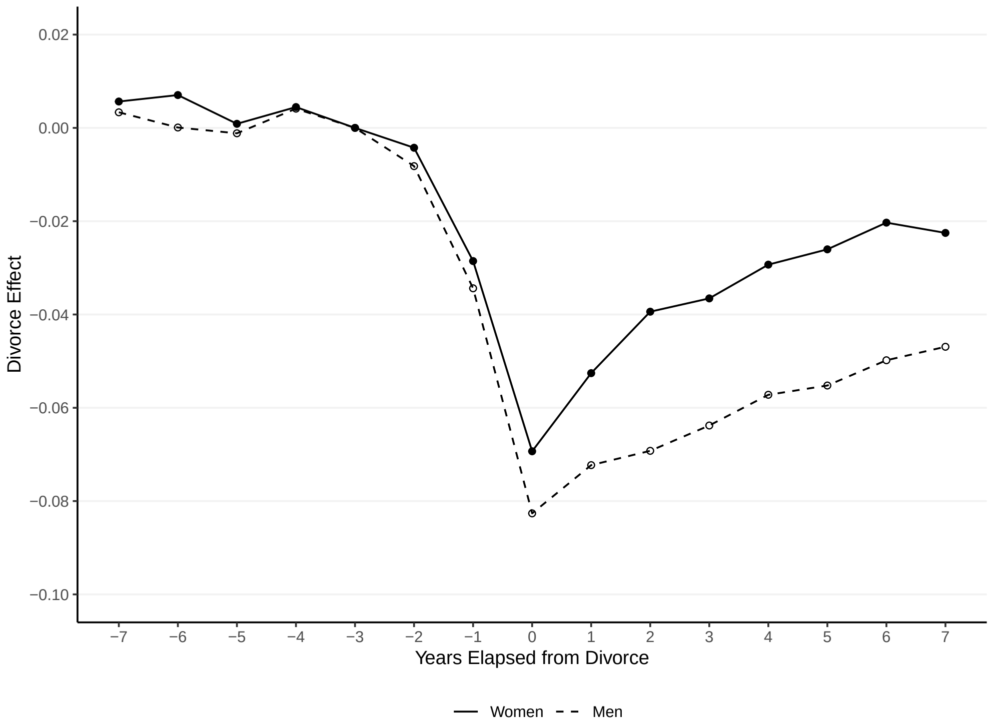


Figure A.2: Results with Individual Fixed Effects

A.2.4 Restricting the Sample to Completely Observed Couples

To be included in the sample used in the main text, it is required that we are able to observe both individuals in a couple in at least one of the three elections for which we have turnout information (1994, 2010, and 2018).[1] In figure A.3 we display results based on a sample restricted to couples for which we can observe the turnout behavior of both spouses in all three elections ($N = 734,376$). Despite the ensuing drop in sample size by approximately 50% the results are very similar to the ones presented in figure 3 in the main text.

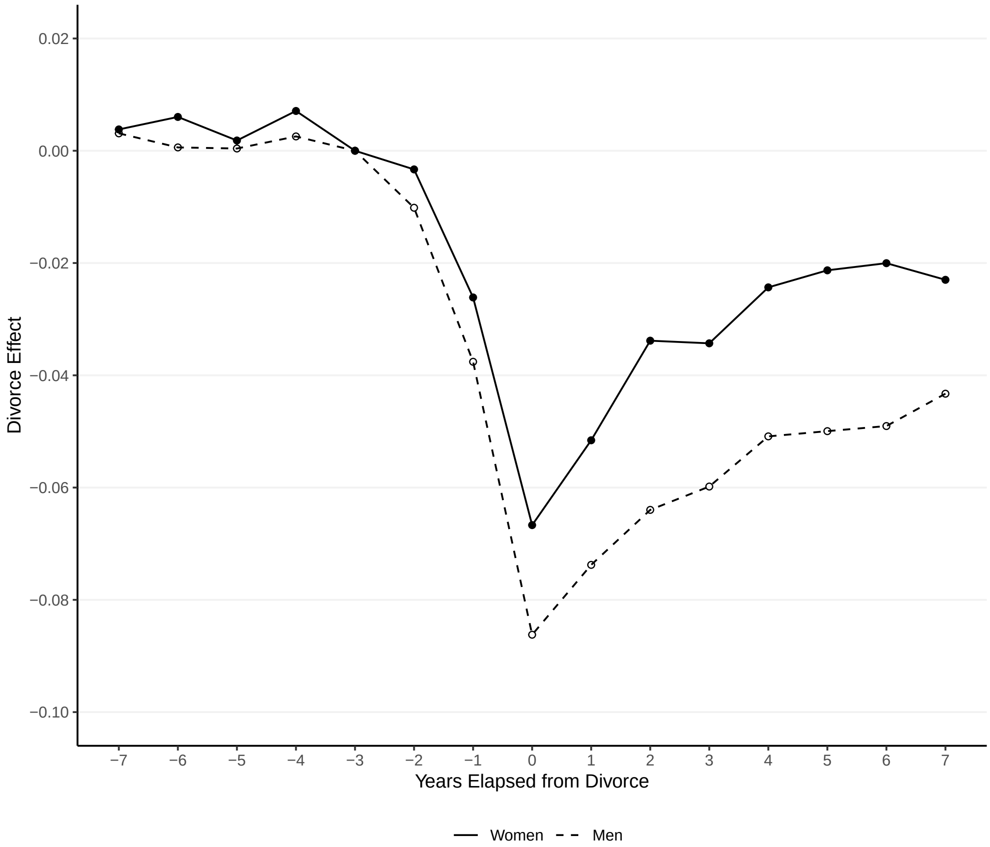


Figure A.3: Results for Completely Observed Couples

A.2.5 Logit Results

In the main text we use a linear probability model to estimate the effects of interest. To check the robustness of these results we have re-estimated our main model using a fixed effects logit model. To do so we use the pseudo-demeaning algorithm with bias-correction (to deal with incidental parameter bias) developed by (Stammann et al. 2016). A downside with the fixed effects logit model is that we are forced to drop all couples in which there is no

[1]To identify the divorce effect data from at least two elections for each couple is necessary. Couples for which we have complete data from a single election will only contribute to the identification of the effects of the control variables.

variation in voting behavior over time. As a consequence of this, the sample size is reduced by two-thirds. This notwithstanding, as can be seen from figure A.4, the pattern of the average marginal effects based on the logit model results closely resembles the corresponding effects estimated using the linear probability model reported in the main text.

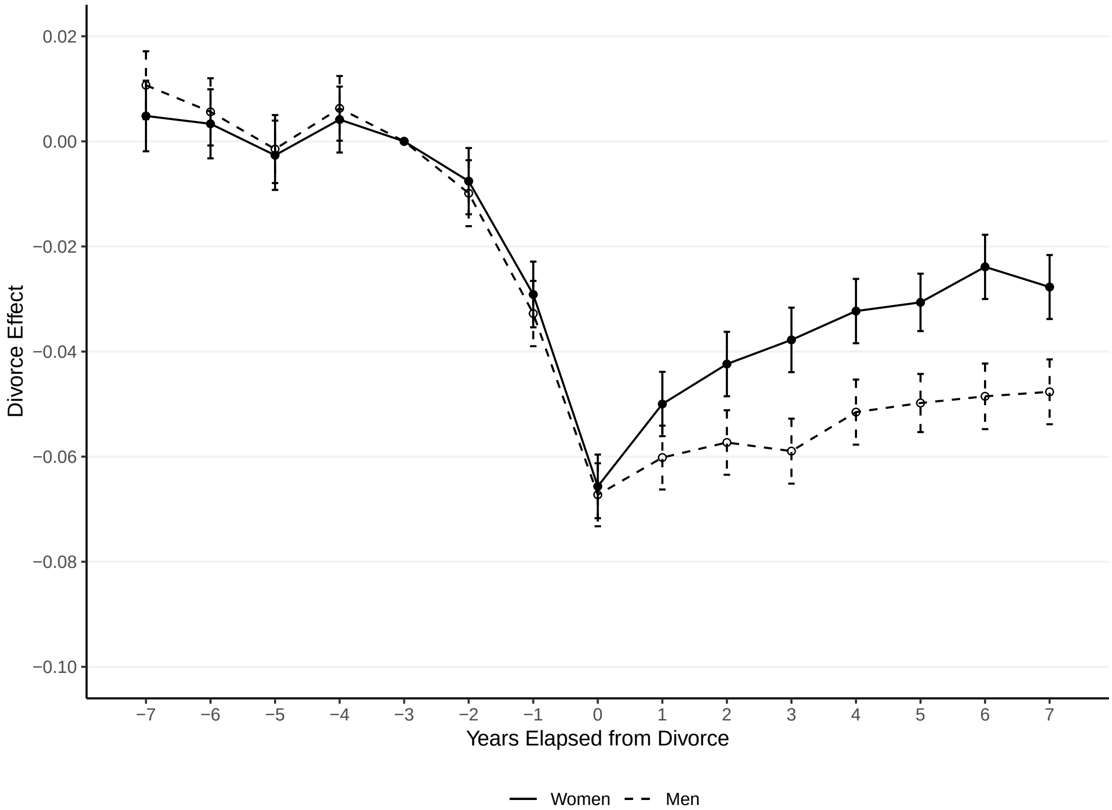


Figure A.4: Marginal Effects, Logistic Regression

A.2.6 Studying Separations Instead of Divorces

In the main text we focus on the impact of formal divorces on turnout, but nowadays many couples live together under marriage-like circumstances without being married. Although the marriage rate in Sweden lies slightly above the EU average, about 30 % of those living together in a relationship in Sweden remain unmarried (Eurostat 2016).

We may therefore be interested in whether our results remain valid for separations more generally. To examine whether this is the case we will use yearly household data for the period 1990–2018. In this data all individuals who live on the same address and are either married or unmarried but have common children are assigned a common household identifier.

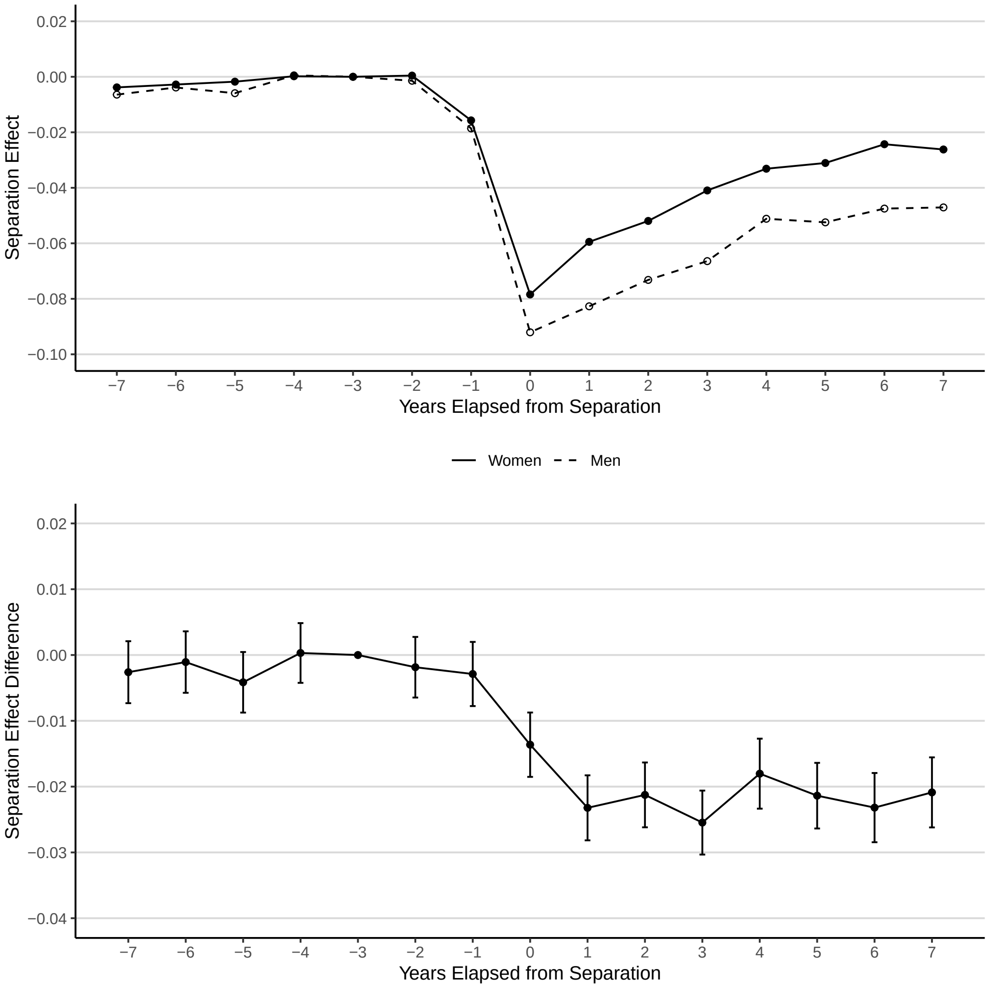


Figure A.5: Impact of Separation on Turnout

A drawback of these data is thus that it will not identify couples that live together as unmarried, but who do not have any common children. By comparing the composition of households from one year to the next we can thus identify couples moving apart during the period 1991 to 2018.

We can then use our standard regression framework to study how turnout varies with the number of years elapsed from the separation. The results from this exercise are displayed in figure A.5 (since we lack information on when many couples started to live together we do not require that the couples live together in the pre-separation period).

As can be seen, the results look very similar when studying separations instead of divorces. If anything the immediate drop at the time of the separation (around year 0) is somewhat more pronounced when studying separations, but the development of the gender differences over time are almost identical to those reported for divorces.

A.2.7 Do the Sample Restrictions Affect the Results?

In table A.1 we describe a number of sample restrictions that we invoke in order to create our main analysis sample. Although we believe these restrictions to be well-justified only a few of these restrictions are strictly necessary in order to study the impact of divorces on turnout. We may therefore investigate how the sample restrictions that we invoke affect the results. In figure A.6 we presents the results from such an exercise.

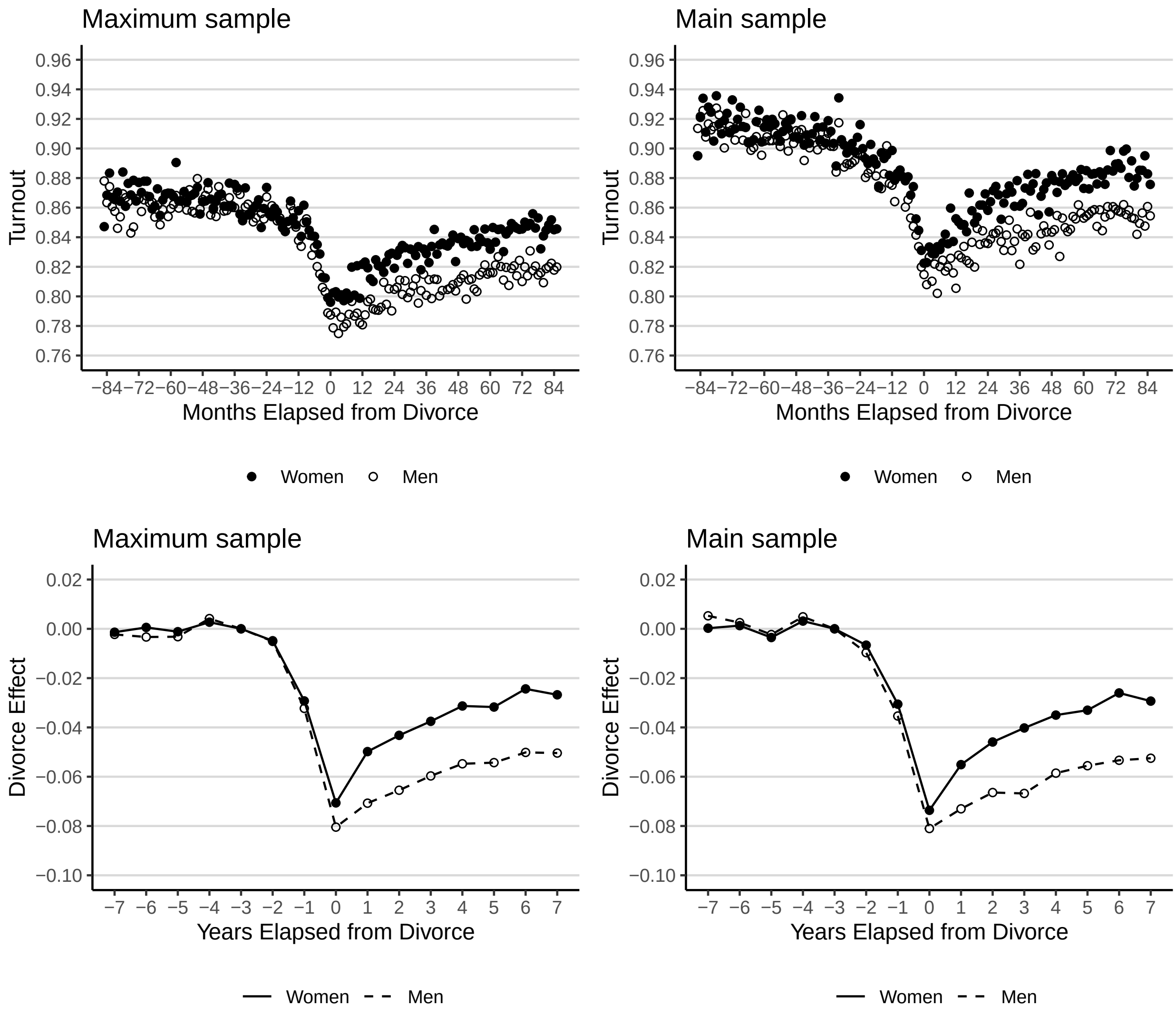


Figure A.6: Impact of Sample Restrictions

The upper two graphs provides the raw plot of turnout by time from divorce. The left of these graphs is the same as that reported in figure 2 of the main text and here we only invoke the restrictions described in steps 1–3 in table A.1. The right upper graph reproduces the same analysis for our main sample. As can be seen the level of turnout differ across the two graphs, mainly because the left graph includes more immigrants (who have divorced before coming to Sweden) and individuals who have divorced more than once. Yet, the dynamic pattern remains very similar across the two samples.

This become even clearer when inspecting the regression results presented in the two lower graphs. As can be seen we obtain almost identical divorce coefficients whether we use the maximum sized sample[2] including more than 3,000,000 observations or our main analysis sample including 1,600,000 observations. Consequently, our sample restrictions appear to be fairly inconsequential for our main results.

A.2.8 Increasing the Observation Window

In our main analyses we restrict attention to the impact of divorces taking place up to seven years before and after the election, whereas the impact of divorces occurring 8 or more years before or after the election are captured by two different dummy variables, which are not reported in the figures. As a result of this that we cannot really tell how long the divorce effect remains (we can only say how much of the effect that remains after seven years). To say more about how long-lasting the effect is we can increase the observation window and study the impact of divorces over a longer time span.

A drawback with increasing the observation window, however, is that the composition of divorces will change with the length of the follow-up period. For instance, if want to use our data to study the impact of divorces taking place more than 25 years before the election only individuals divorcing before 1993 (25 years before the election in 2018) will contribute to the estimate of this effect. If the individuals divorcing early in the period differ from those divorcing later in the period this type of composition effects could affect the results.

With this caveat in mind, we can still examine what the results look like if we extend the observation window from 7 to 15 years, i.e., we now examine the impact of divorces taking place up to 15 years after (before) an election. The results from this exercise are reported in figure A.7. The upper graph extends figure 2 in the main text to 15 years around the divorce, and the lower graph displays the regression results from a model which includes individual dummy variables for divorces taking place within 15 years from an election, whereas the divorces occurring either more than 15 years before or more than 15 years after an election are captured by two distinct dummy variables.

As expected the results from this analysis closely mimics our main results for divorces occurring within seven years from an election. More interesting, however, is that both the

[2] Using the regression framework necessitates that we invoke the additional restriction that the couple indicator is non-missing.

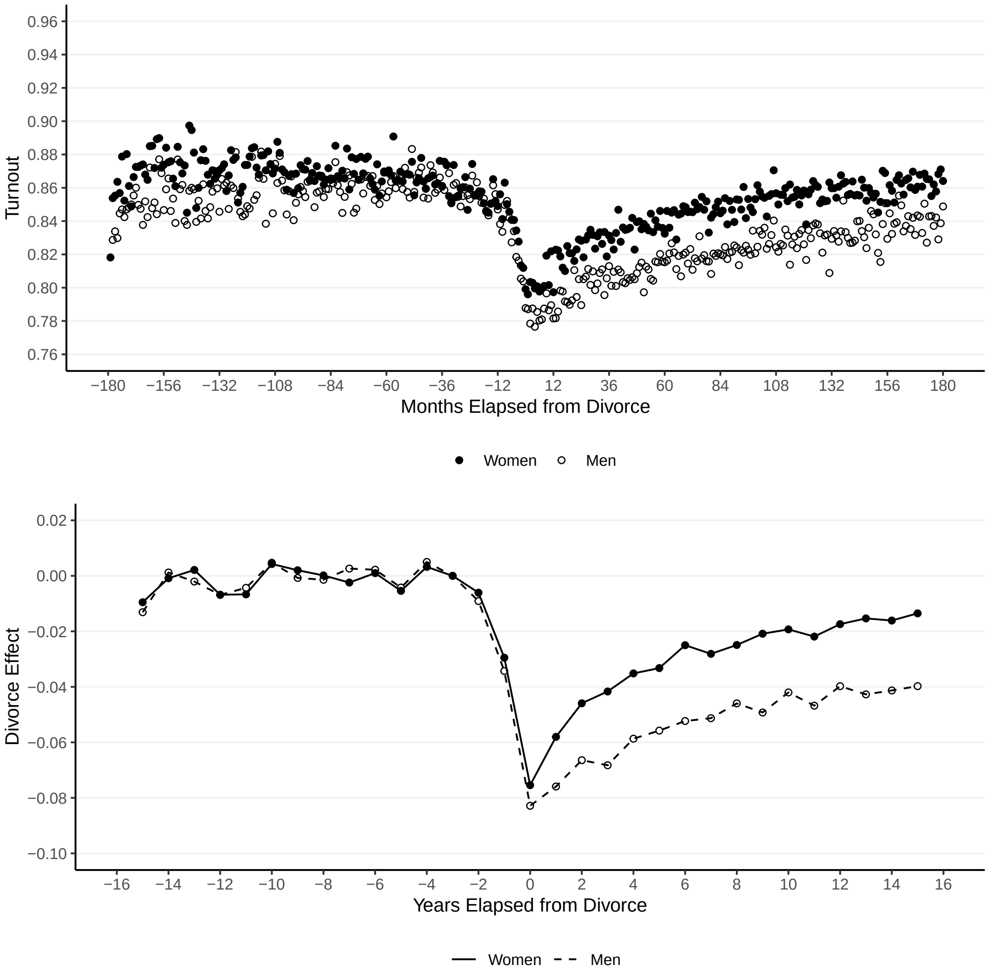


Figure A.7: Results with Larger Observation Window

drop in turnout and the increased gender gap appear to be very long lasting. If we look at the regression results we see that 15 years after a divorce women's turnout lie 1.5 %-points below their pre-divorce level, whereas the corresponding figure for men is almost 4 %-points.

Although we should bear the caveat with compositional changes in mind, these results suggest that our results extend far beyond the observation window used in the main text.

A.2.9 Do the Results Vary by Child Age?

Divorcing is a life changing event for all individuals, but they can be expected to be particularly difficult for individuals who have young children at the time of the divorce. It is therefore interesting to see whether our results differ across couples depending on whether they have young children when divorcing. In figure A.8 we therefore present the results from an analysis in which we have divided our main sample into four different sub-samples depending on the age of the youngest child at the time of the divorce.

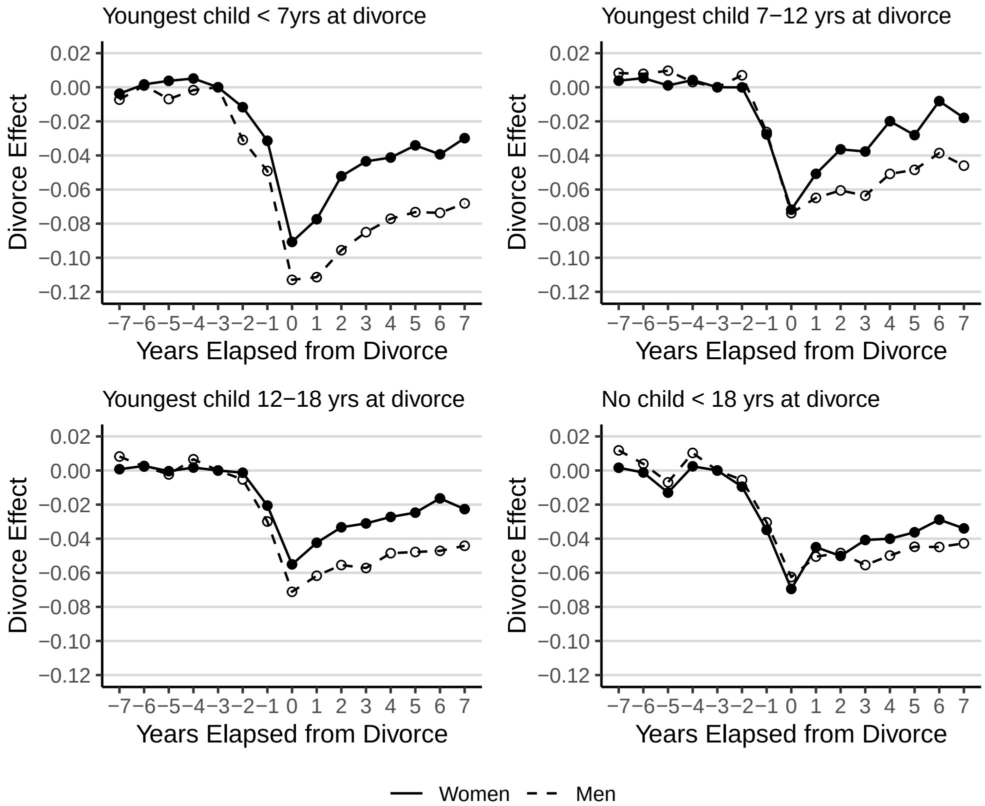


Figure A.8: Results by Child Age at Divorce

As can be seen from the figure, both the immediate drop in turnout and the gender gap is visible in all four graphs. With that said, both of these effects appear to be stronger for couples who have young children at the time of the divorce. A caveat in interpreting these results, however, is that couples who have older children at the time of the divorce will also be older themselves on average. If the impact of divorcing on turnout varies with the age of an individual the differences in effects across the different graphs could also pick up such differences related to age. Whereas divorce effect is statistically significant in all four panels, the gender difference is not statistically significant for all years for the subset of individuals who do not have any children below 18 in the household (complete results are available upon request).

A.2.10 Generalizing the Results to Other Groups

As mentioned in the main text, it is fairly common that marriages in Sweden end in divorce. Among the couples marrying in the 1970s, almost 40 % divorced within 25 years (Stanfors et al. 2020). Divorces are thus something directly affecting large segments of society in all socio-economic groups.

With that said, it could still be interesting to ask whether the group of individuals experiencing a divorce are sufficiently similar to those marrying but never divorcing in order for our results to apply to the latter group as well. That is, is there reasons to believe that the group of individuals who never divorce would have reacted in a similar way as those divorcing, had they actually divorced? In an attempt to shed some light on this issue, the left part of figure A.9 displays turnout by age for the sample of divorcees (the solid line) that are used for our main analyses, and a sample of non-divorcees (the dashed line) who got married between 1975 and 2005, but who had not divorced until 2019.[3] The right part of the graph plots the difference in turnout between the divorcees and the non-divorcees together with 95%-confidence intervals for these differences.

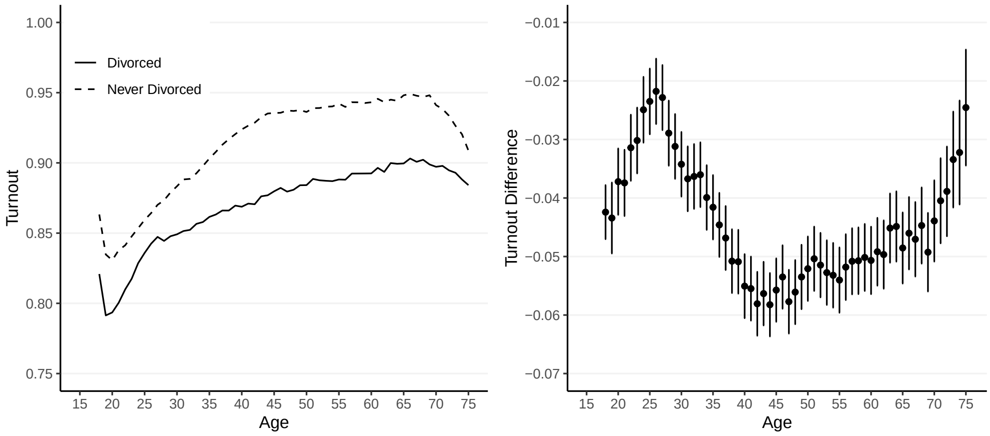


Figure A.9: Turnout by Age for Divorcees and Non-Divorcees

As can be seen, the age profile is fairly similar in both groups, although the non-divorcees have higher turnout in all age groups. Interesting to note, however, is that the turnout differences between the two groups are decreasing up until the late 20s when individuals in the divorcee sample begin to divorce. Then the turnout gap continues to increase until the mid 40s before it starts to decrease again, when most individuals in the divorcee sample have been through their divorce. Consequently, there is reasons to expect that the turnout rates of the two groups would have looked more similar, had the divorcees not divorced, or had

[3]In calculating these turnout rates we have adjusted for election-year fixed-effects.

the non-divorcees divorced.

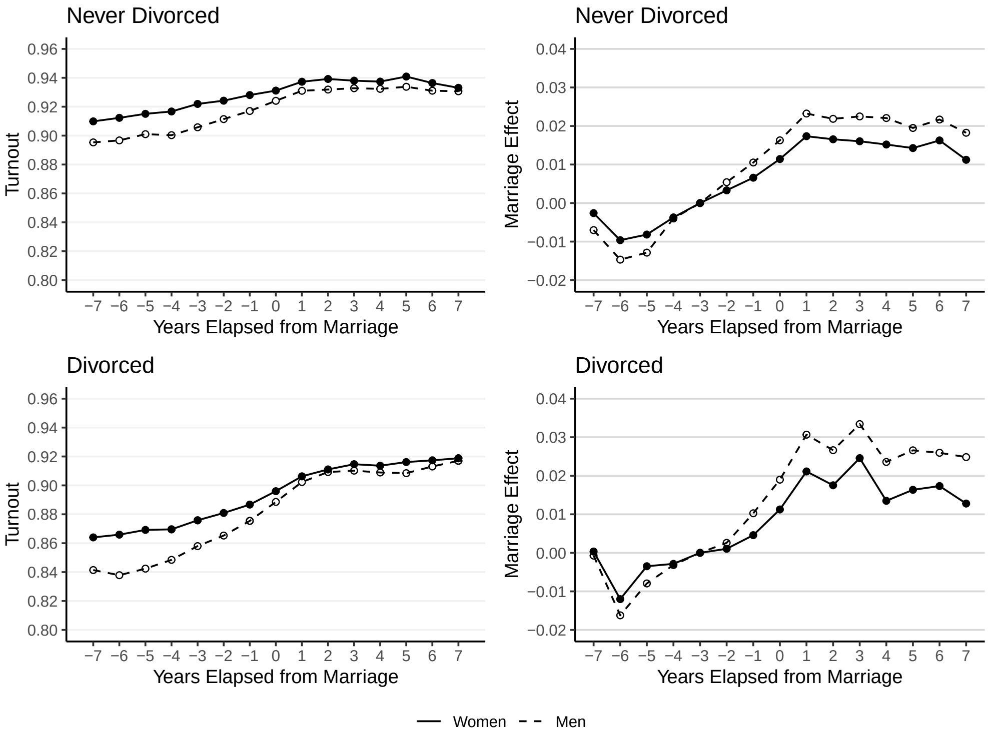


Figure A.10: Marriage Effect for Divorcees and Non-Divorcees

Likewise, the results presented in figure A.10 indicates that the impact of marriage on turnout look very similar for the samples of divorcees and non-divorcees. In the left part of the graph we plot turnout for men and women by time since marriage.[4], and in the right part of the graph we plot the marriage effect obtained from a regression model of the type used for our main analyses. As can be seen, both men's and women's turnout is positively affected by marriage, but the marriage impact is particularly pronounced for men in both the divorcee and the non-divorcee sample.

Overall we thus find that our sample of divorced individuals look fairly similar to the group of married but never divorced individuals in important respects. It could therefore be expected that the group of never divorced individuals would have reacted similarly if they have had to go through a divorce.

[4]In calculating these turnout rates we have adjusted for election-year fixed-effects.

A.2.11 Generalizing the Results to Other Countries

A possible concern is that our results may not generalize to other countries and contexts. Sweden's electorate and political institutions stand out along many dimensions in cross-country comparisons. To explore the external validity of our findings, we report two sets of results.

The first set of results is based on data from the 1979 cohort of the National Longitudinal Study of Youth (NLSY79). The NLSY79 is a nationally representative sample of around 13,000 young men and women who were 14–22 years old when they were first surveyed in 1979. The NLSY respondents have been surveyed annually between 1979 and 1993 and every second year between 1994 and 2018. Each wave they are asked about their current civil status and changes in civil status since the last wave. In 2008, they were asked about their participation in the 2006 midterm election.

Using this information we can calculate average turnout by year since divorce at the time of the midterm election in 2006. The results are displayed in figure A.11 and can be compared to the corresponding monthly averages reported in figure 2 in the main text. The sample is restricted to only include individuals that divorced one time during the period between 1980 and 2018 ($N = 617$). We have capped the x-scale such that divorces taking place more than 8 years after or before the election are set to -/+ 8. Moreover, to increase precision we display 1-year moving averages. That is, each data point is calculated as the average across voter turnout in the focal year and in the year preceding and succeeding the focal year. Despite the much smaller sample the pattern displayed in figure A.11 resembles the one reported in figure 2 in the main text. Above all, men seem to experience a substantially larger drop in turnout when going through a divorce.

The second set of results are based on survey data from the *European Social Survey* (ESS), rounds 1 through 9. The survey was first conducted in 2002 and has been repeated every two years. Over the years, close to 40 countries have participated, and in our analysis we use observations for all years and all countries for respondents that answered a question on whether they voted in the latest election, and their marital status. We estimate a linear probability model using OLS, where the dependent variable takes the value 1 if the respondent indicated that they voted in the latest election, zero otherwise. For each country, we also compute the turnout gender gap (defined as the difference between average female turnout and average male turnout) and the overall country-level turnout, using the ESS data for both measures.

In table A.5 we present the estimates using the ESS survey data. The first column shows the association between individual-level turnout and marital status, represented by the variable *Divorced* taking the value 1 if the respondent was divorced when the survey was conducted, zero otherwise. The estimate for *Divorced* is, as we expect, negative, echoing our main results. This model also includes an indicator variable for the respondent's gender, *Female* taking the value 1 for a female respondent, zero otherwise, as well as an interaction term for *Female* and *Divorced*. In line with our main results, this estimate is positive, meaning that the divorce effect is smaller for women. Moreover, as the estimates in column (2) shows, the gendered divorce effect is influenced by the country-level gender gap: a positive gap, meaning that women are more likely to vote and thus more likely to be the mobilizing partner, is associated with an even stronger gendered divorce effect. This result, which supports the notion that asymmetrical spousal mobilization underlies the gendered divorce effect, is obtained by adding a three-way interaction between *Divorced*, *Female*, and *Gender Gap* (GG).

Column (3) examines whether one can generalize our main results to countries with lower overall turnout. Specifically, we add a three-way interaction between *Divorced*, *Female*, and *Turnout*. The estimated parameters for this specification indicates that the gendered divorce effect is not specific to high-turnout countries, but that one should perhaps expect an even larger gendered divorce effect in countries with a lower overall turnout.

Lastly, in columns (4) and (5) we examine whether the estimated divorce effect in Sweden differs significantly from the corresponding effect in the other countries in the ESS data. This is not the case, as the estimated slope coefficient for the three-way interaction between *Divorced*, *Female*, and *Sweden* (S, taking the value 1 if the respondent lives in Sweden, zero otherwise), is not statistically distinguishable from zero.

Table A.5: Estimates from European Social Survey. Dependent Variable: Turnout

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Constant	0.831***	0.831***	0.831***	0.925***	0.828***
	(0.002)	(0.002)	(0.002)	(0.007)	(0.002)
Divorced (D)	−0.114***	−0.115***	−0.105***	−0.076***	−0.116***
	(0.004)	(0.004)	(0.004)	(0.016)	(0.004)
Female (F)	−0.007***	−0.005**	−0.005**	0.002	−0.007***
	(0.003)	(0.003)	(0.002)	(0.010)	(0.003)
D×F	0.019***	0.018***	0.025***	0.045**	0.019***
	(0.005)	(0.005)	(0.005)	(0.018)	(0.005)
Gender Gap (GG)	−−	−0.001	−−	−−	−−
	−−	(0.001)	−−	−−	−−
Turnout (T)	−−	−−	0.009***	−−	−−
	−−	−−	(0.000)	−−	−−
Sweden (S)	−−	−−	−−	−−	0.082***
	−−	−−	−−	−−	(0.006)
D×F×GG	−−	0.005**	−−	−−	−−
	−−	(0.002)	−−	−−	−−
D×F×T	−−	−−	−0.001*	−−	−−
	−−	−−	(0.001)	−−	−−
D×S	−−	−−	−−	−−	0.030**
	−−	−−	−−	−−	(0.015)
D×F×S	−−	−−	−−	−−	0.025
	−−	−−	−−	−−	(0.019)
Age	0.061***	0.061***	0.058***	0.020***	0.061***
	(0.002)	(0.002)	(0.002)	(0.008)	(0.002)
D×Age	0.024***	0.024***	0.021***	0.012	0.022***
	(0.004)	(0.004)	(0.004)	(0.013)	(0.004)
F×Age	−0.001	−0.001	−0.001	0.005	−0.002
	(0.003)	(0.003)	(0.003)	(0.011)	(0.003)
Adj. R squared	0.030	0.030	0.071	0.012	0.032
Only Sweden	No	No	No	Yes	No
Observations	136777	136777	136777	5214	136777

Notes: Data from European Social Survey, rounds 1 to 9. The dependent variable takes the value 1 if the respondent reported to have voted in the latest election, 0 otherwise. *Divorced*, *Female*, and *Sweden* are indicator variables taking the value 1 if the respondent was divorced, female, or living in Sweden, respectively, at the time of the survey, 0 otherwise. *Gender Gap* measures the country-level turnout gap between female and male respondents while *Turnout* measures the country-level turnout among the respondents. ***, **, and * indicate statistical significance at 0.1%, 1%, and 5% levels, based on heteroscedasticity-consistent standard errors.

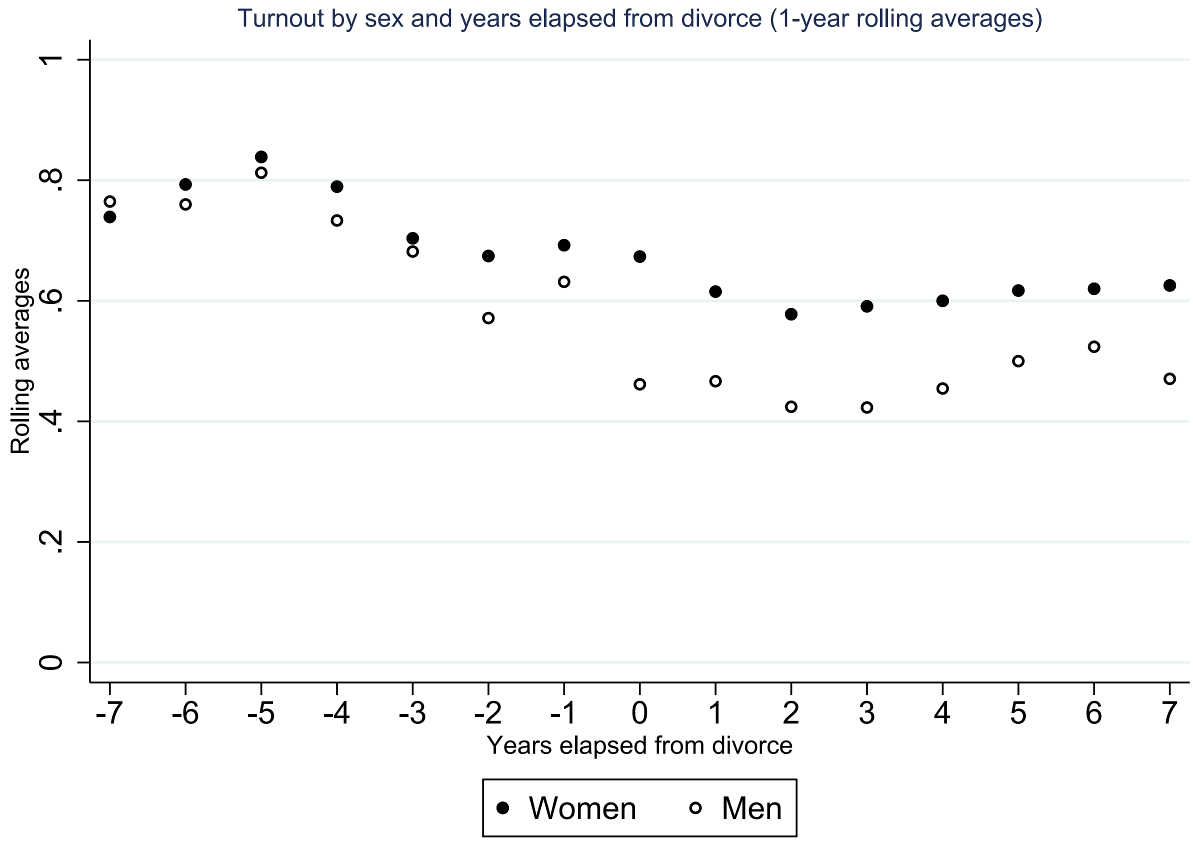


Figure A.11: Turnout and Time since Divorce in NLSY

A.2.12 Previous Estimates of the Divorce Effect

To provide an overview of previous estimates of the divorce effect, table A.6 summarizes the studies that estimate such an effect and that are cited in the section on previous research. All but one do find a divorce effect. Although most of these estimates are based on survey data and the authors make no or little claim to be causal, the magnitude in several of the studies are similar to those we estimate. One study finds an effect only among women and that this effect diminishes over time, while two other studies that estimate separate effects for men and women find no gendered effect.

Table A.6: Literature review: divorce effects

Author(s)	Sample	Location and time period	Method and data	Finding
Stoker and Jennings (1995)	Marital couples among parents of high school seniors.	USA, 1965, 1973, 1982	Panel survey	13 %-points decrease in turnout.
Wolfinger and Wolfinger (2008)	A sample of all individuals 18 or older from the basic Current Population Survey.	USA, 2000	Cross sectional survey	Divorced respondents were 8 %-points less likely to vote than married respondents.
Kern (2010)	Individuals aged 18 or older in England, Scotland and Wales	UK, 1995-2005	Propensity score matching using panel survey data	Divorced respondents were close to 10 %-points less likely to vote than married respondents. No difference between men and women.
Voorpostel and Coffé (2012)	Swiss voters, aged 18-60	Switzerland, 1999-2007	Panel survey	Separated women are 44 % less likely than married women to vote, while no estimated effect for men. The effect for women is diminishing over time.
Bhatti et al. (2020)	All Danish eligible voters that lived together with at leat one other eligible voter in 2009.	Denmark, 2009, 2013, 2014	Panel register data	Losing a partner decreases turnout by 5–6 %-points. This effect is stronger among individuals who, before the separation, went to the polling station together with their partner.
Bellettini et al. (2020)	All marital couples in the city of Bologna that were eligible for voting in the municipal election.	Bologna, Italy, 2004, 2008, 2009, 2013	Panel register data	No divorce effect.

References

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