# The Impact of the Female Advantage in Education on the Family

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**Abstract**

Men's historical advantage in educational attainment has recently been reversed in many countries. I study the implications for family formation of the new female advantage in education in the marriage market, exploiting a Finnish school reform that increased women's relative level of education. I analyze the reduced-form relationship between marriage market exposure to the reform and family outcomes. I find decreases in marriage and fertility in marriage markets with a larger female educational advantage. These results are mostly driven by the increasing mismatch between the educational distributions of men and women, and might have negative consequences for low-educated men's mental health.

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# 1 Introduction

Recent decades have seen a decline and reversal of the traditional gender gap in education in favor of men in many countries. In the United States, for instance, there were over 50% more men than women with university degrees in the working-age population in 1960. This difference gradually eroded during the second half of the twentieth century, and by the 2000's the gap had been reversed (Goldin et al., 2006).[1] Far from being a US-specific phenomenon, women's rates of tertiary education now exceed those of men across all 38 OECD countries among the population aged 25-34, with this advantage continuing to widen over time (OECD, 2022). These historically unprecedented shifts have attracted broad attention from scholars across disciplines (Esteve et al., 2016; Evans et al., 2021; Goldin et al., 2006).

The reversal of the gender gap in education and the emerging female advantage could have far-reaching consequences for the family. Historically, heterosexual mating patterns have been characterized by men marrying women at most as educated as themselves. These patterns are likely to be challenged as women increasingly surpass men in educational attainment but, so far, we lack evidence on how family formation may be affected as a consequence.

The aim of this paper is to understand the causal impact of relative increases in women's educational attainment on marriage and fertility. While the direct consequences of educational attainment for women and men have been widely researched, we know much less about the effects of changes in the gender gap in education in the marriage market. Given the dramatic reversal in these gender gaps across countries, understanding their potential implications for family formation is of particular relevance. To investigate this question, I exploit the gradual implementation of a school reform in Finland that increased the female advantage in education.

Conditional on own educational attainment, changes in the educational composition of the marriage market might affect union formation and family outcomes, as these have been shown to depend on the availability of suitable partners (Abramitzky et al., 2011; Angrist, 2002). In the context of marriage models à la Becker (1973), a larger female advantage could potentially enable more specialization between spouses, and thus increase the gains from marriage.[2] On the other hand, if individuals prefer a partner with their same level of education, we would expect an increasing mismatch between

[1] See this evolution in Figure A1 with data for the US and for the OECD average.

[2] These types of models predict positive assortative matching in education, but this only refers to the ranks of individuals in their gender-specific distribution of traits. Absolute differences in the education levels between men and women play no significant role in this context (Bertrand et al., 2015). Education is seen as an input for both market and non-market sectors. While the closing of the male-female gap in education could reduce the gains from specialization, if the new female advantage in education becomes larger than the former male advantage, gains from specialization could in principle increase, with an inversion of the role of spouses.

the distributions of educational attainment of men and women to lower marriage rates, and potentially fertility. In particular, we might expect there to be an excess number of high-educated women and low-educated men who are unable to find a match. This effect would be reinforced in the presence of gender identity norms that make a situation where the wife has higher education than her husband particularly undesirable (Bertrand et al., 2015; Greitemeyer, 2007; Hitsch et al., 2010).

Finland implemented a large school reform in the 1970s, transforming the former selective school model, where students were separated into different tracks at age 11, into a comprehensive system where they remained together until age 16. The choice between vocational and academic track was thus delayed from age 11 to age 16, and a national curriculum was introduced. This reform has been found to widen the gender differences in education, increasing the female advantage in pursuing the academic track and entering into university (Pekkarinen, 2008).[3]

The reform followed a gradual implementation plan, with different municipalities adopting the new system in different years during the period 1972-1977. This adoption path generates variation in exposure to the new school system within municipalities across cohorts, and within cohorts across municipalities, which can be used to identify the impact of individual exposure to the reform. Crucially, I can also exploit variation in the degree of exposure to the reform of a person's marriage market, even conditional on own exposure. This is because marriage markets do not coincide fully with municipality-cohort groups, given that individuals do not marry only within municipalities or within cohorts—men tend to marry slightly younger women. These features, together with the differential gender effects of the school reform, allow me to study the role of changes in the gender gap in education in the marriage market, above and beyond the impact of increases in an individual's own level of education.

Exploiting these sources of variation and using rich data from Finnish administrative registers, I first show that the reform increased the female advantage in educational attainment. I find that the female-male gap in continuing education beyond secondary school increased by 11%, and the average gender gap in university education was virtually closed. I then estimate the impact of higher marriage market exposure to the reform, conditional on own exposure, on marriage and fertility patterns. In my baseline specification, marriage markets are defined based on region of birth and on the age gaps within couples in pre-reform cohorts. I measure marriage market exposure to the reform as the proportion of people in a person's marriage market who were enrolled

[3]A potential explanation for the differential effect of this reform is related to the gender differences in the timing of puberty, with girls entering adolescence before boys. The gender gap in maturity by age 16 might exacerbate differences in academic performance and aspirations, and educational choices at this age might be affected as a result (Pekkarinen, 2008). Across countries, there is a positive correlation between later tracking systems and a larger female advantage in educational attainment (Pekkarinen, 2008; Scheeren et al., 2018).

in the new school system. In marriage markets with higher exposure there was thus a larger female advantage in education.

My results show that in marriage markets with a larger female advantage in education there were declines in marriage and fertility. In particular, a one standard deviation increase in marriage market exposure to the reform, which leads to a 0.3 pp larger female advantage in education, decreases the probability of being married or cohabiting at age 40 by 1.1% and the number of children by 1.4%.[4] These effects are sizeable compared to the changes in family structure that took place in Finland during this period. A one standard deviation increase in marriage market exposure can account for around 10% of the actual decline in marriage and cohabitation observed during these decades. Importantly, these effects are not driven by a decrease in the propensity to marry of women who became more educated as a result of the reform, as the reform had, if anything, a positive direct effect on women's marriage and fertility.

These results are based on a reduced-form analysis, and do not rely on the assumption that only the gender gap in education changed in marriage markets more affected by the reform.[5] Rather, I claim that changes in the gender gap in education are an important channel driving these findings, and provide suggestive evidence supporting this interpretation.

First, consistent with the effects being driven by the increased dissimilarity between the distributions of education of men and women, I find stronger negative effects for high-educated women and low-educated men. Second, I exploit heterogeneity in the baseline gender gap in education to show that marriage and fertility declined more in marriage markets where this dissimilarity grew more as a result of the reform.

In line with recent evidence linking declines in men's perceived value in labor and marriage markets with deteriorating health (Autor et al., 2019; Case and Deaton, 2017; Coile and Duggan, 2019), my results suggest that these changes in family structure might have had negative consequences for men's mental health. I find that in marriage markets with a larger female advantage in education there is an increase in mental-health related hospital contacts for men, especially for those with low levels of education. I also provide suggestive evidence that in these marriage markets men became more likely to marry a woman more educated than themselves. This finding points to the possibility that the social norm of men marrying women with equal or lower education may change with marriage market conditions (Esteve et al., 2016).

This paper offers the first causal exploration of the impact of the increasing female

[4]Female advantage in education is used hereafter, unless explicitly stated otherwise, to refer to the female to male gap in the probability of having more than secondary education.

[5]Previous studies have found that the Finnish comprehensive school reform increased intergenerational mobility and decreased inequality in mortality and cognitive skills by parental income (Kerr and Pekkarinen, 2013; Ravesteijn et al., 2017; Pekkarinen et al., 2009). We might thus expect that in more affected marriage markets there is also less social inequality.

advantage in education in the marriage market on family outcomes. In doing so, it makes contributions to several strands of the literature. First, it contributes to the studies on the implications of the reversal of the educational gender gap, which have, so far, been descriptive in nature. For instance, Esteve et al. (2012, 2016) study the association between the reversal of the educational gender imbalance and patterns of assortative mating, and show that, as the female advantage in education increases, so does the prevalence of couples in which the wife is more educated. Schwartz and Han (2014) document that, while in the past couples where the wife is more educated than her husband were more likely to divorce, this difference has attenuated over time.[6] I contribute to this literature by providing causal estimates of the effect of a reform which increases the female advantage in education on a set of family outcomes.

Second, this paper speaks to the literature on the causal impact of women's education on fertility and marriage outcomes. This literature generally finds that, in developed countries, increases in educational attainment at the lower end of the distribution (such as those induced by extensions of compulsory schooling) decrease teenage births, but have small or even positive effects on completed fertility (Black et al., 2008; Fort et al., 2016; McCrary and Royer, 2011; Monstad et al., 2008).[7] Regarding marital outcomes, higher female education has been found not to affect the probability of marriage, but to improve spouse quality (Anderberg and Zhu, 2014; Lefgren et al., 2006; McCrary and Royer, 2011).[8] [9] Most closely related to my setting, Virtanen et al. (2024) exploit discontinuities in the centralized admission systems to upper secondary schools and universities in Finland, studying cohorts that are slightly younger than mine, and find that higher education increases women's probability of being in a couple and of having children.

My results on the effect of direct exposure to the reform are in line with this previous evidence. I find that being exposed to the new school system, which leads to higher educational attainment for women, does not have significant effects on their probability of marriage, but has a small positive impact on their probability of being in a couple and of having children. More importantly, a key contribution of this paper is to show that, beyond the impact of one's own level of education, changes in the relative levels of

[6] See Van Bavel et al. (2018) for a comprehensive review of this literature.

[7] The relationship between schooling extensions and fertility seems to depend, at least in part, on the institutional context. For instance, Cygan-Rehm and Maeder (2013) find that extensions of compulsory schooling are related to decreases in total fertility in Germany, where the opportunity cost of childrearing is high. Similarly, Fort et al. (2016) finds that female education has a negative effect on fertility in England, but not in continental Europe.

[8] In developing countries, increased female education has been found to delay (and in some cases decrease) fertility, delay marriage and improve spouse quality (Heath and Jayachandran, 2017).

[9] An exception in this sense is a recent paper by Lennon (2023), who finds that higher education leads to decreases in both marriage and fertility among World War II female veterans in the US.

education of men and women in a given marriage market also affect family outcomes. For women, these effects operate in opposite directions. As a result, failing to account for the marriage market spillovers when studying the impact of the school reform would lead to an underestimation of its positive effects on women's marriage and fertility outcomes.

By focusing on the changes in the relative levels of education of men and women, this paper is also connected with a broad literature on how changes in marriage market conditions, and in particular sex ratios, affect the family (Abramitzky et al., 2011; Angrist, 2002; Baranov et al., 2023; Battistin et al., 2022; Brainerd, 2017; Charles and Luoh, 2010; Lafortune, 2013; Mechoulan, 2011; Grosjean and Khattar, 2019).[10] [11] Across different settings, these papers establish that the marriage market conditions one faces at marriageable age are a crucial determinant of many individual and family outcomes. It is thus relevant to understand the implications that the recent societal changes in terms of educational gaps bear for marriage markets. My work is most closely connected to the scarcer papers within this literature which focus on education-level specific sex ratios (Negrusa and Oreffice, 2010), or even field-of-study specific ratios (Pestel, 2021).

Finally, this study is related to the literature exploring the consequences for the family of changes in the relative position of men and women that violate traditional gender norms. Bertrand et al. (2015) study the causes and implications of relative income within spouses, and find evidence consistent with social aversion to a situation in which the wife outearns her husband. Using a Bartik-style instrument, they show that when, in a given marriage market, women are more likely to earn more than men, marriage rates decline. Using a similar strategy, Shenhav (2021) finds that increases in women's relative wages more generally also reduce marriage rates, while increasing women's spouse quality. Autor et al. (2019), in turn, exploit trade shocks to show that relative decreases in men's earnings lead to lower marriage rates and fertility, and to increased premature mortality among men.[12] Tur-Prats (2021) and Bergvall (2024) show that improvements in women's relative economic position, measured by relative unemployment levels or potential earnings, can lead to increases in intimate-partner violence. Lastly, Folke and Rickne (2020) study the tension between women's career

[10]These are some of the papers which try to identify the causal effect of changes in sex ratios on the family. There is an even broader literature spanning different fields which documents correlations between sex ratios and family outcomes. Relevant to the context of this paper, for instance, Lainiala and Miettinen (2013) study the association between regional sex ratios and marriage and fertility in Finland.

[11]To the best of my knowledge, the only other paper which uses a similar school reform as a source of variation in marriage market conditions is Holmlund (2022), who studies the impact of the Swedish comprehensive school reform on assortative mating by socioeconomic background.

[12]In a related paper, Kearney and Wilson (2018) use the fracking boom and find that increases in men's earnings potential increase marital and non-marital births, but not marriage.

success and marital stability, and find that women's promotions, but not men's, increase their probability of divorce.

In this paper, I study the implications of changes in the relative position of men and women in educational attainment. This is a closely-related but different dimension, which has been ignored so far, despite being highly relevant in the context of most developed countries in light of the recent shifts.[13] My findings corroborate that relative advances in women's economic position can generate frictions in marriage markets.

The rest of the paper is structured as follows. In section 2 I describe the content and implementation of the Finnish comprehensive school reform. I section 3 I lay out the identification strategy. Section 4 describes the data used and provides descriptive statistics. Section 5 shows the results, section 6 provides supplementary analyses and robustness checks to corroborate the main findings, and section 7 concludes.

## 2 Background: the Finnish comprehensive school reform

In the 1970s, Finland transformed its school system and adopted a comprehensive school model, with the aim of equalizing educational opportunities for all students. Similar reforms had taken place some years before in Sweden (Fischer et al., 2021; Meghir and Palme, 2005; Meghir et al., 2018) and Norway (Aakvik et al., 2010; Monstad et al., 2008).

Before the reform, Finland had a selective school system. Children entered in primary school at age 7, and there were only four years of common education for all students. At age 11, they could choose to apply for admission to a general secondary school or to continue in primary school. Admission was based on teacher recommendations, an entrance exam, and primary school grades. Those admitted continued their education in a general secondary school for five more years, and at age 16 were eligible to attend an upper secondary school (for two years) and, later, university. Those who were not admitted, or did not apply, stayed in primary school for two more years. By the beginning of the 1970s, most primary schools offered continuation classes (civic schools), which offered a more practically-oriented education, such that virtually all students remained in school until age 16 (Pekkarinen, 2008). After civic school, students could finish their education or continue with vocational training, but could not attend upper secondary schools.

With the implementation of the reform, the former primary, general secondary, and civic schools disappeared and were replaced by comprehensive schools. Comprehensive

[13]The reversal of the gender gap in education has been a common phenomenon in most developed and some developing countries in recent years, certainly more common than the closing of the gender wage gap. In fact, while educational attainment is related to earnings potential, changes in the gender gap in education might not necessarily lead to a reversal of the wage gap: education and labor market segregation, motherhood penalties, and gender norms might all complicate this relation (Klesment and Van Bavel, 2017).

schools offered the same educational content to all students for nine years, from age 7 to 16. After this compulsory education, students could choose to either apply to an upper secondary school, apply to a vocational school,[14] or stop studying.

The reform thus implied several changes. First, it delayed the choice of academic or vocational track from age 11 to age 16. Second, it meant that all students would now be together in the same facilities and exposed to the same national curriculum for nine (instead of four) years. However, it did not, in practice, extend compulsory schooling, as most students were already enrolled in school for nine years before the reform (Pekkarinen, 2008).

The adoption of the reform was approved by parliament and legislated in the 1968 School Systems Act (467/1968). The reform was mandated to be implemented gradually from 1972 to 1977, with the order of adoption being determined geographically. It started with the northern municipalities, which had lower levels of educational attainment. The plan of adoption is described in Figure 1. The transition was overseen by regional school boards (Pekkarinen et al., 2009). In the year of implementation of the reform in a given municipality, all students in the first five grades were enrolled directly in the comprehensive school, while those in the sixth grade and above continued their education in the pre-reform system.

Several papers have exploited the gradual implementation of this reform to study its effects on educational attainment (Pekkarinen, 2008), cognitive skills (Kerr and Pekkarinen, 2013), intergenerational mobility (Pekkarinen et al., 2009), health inequalities (Ravesteijn et al., 2017), labor market outcomes (Ollikainen, 2021), or mental health (Böckerman et al., 2021).

## 3 Identification strategy

This section lays out the identification strategy. Section 3.1 first describes the empirical strategy to estimate the impact of the reform on the gender gap in education, while section 3.2 focuses on the estimation of the effects of marriage market exposure to the reform on family outcomes.

### 3.1 Impact of the reform on the gender gap in education

The gradual adoption of the comprehensive school system, as described in section 2, generated variation in exposure to the new system across municipalities within cohorts, and across cohorts within municipalities. This variation is illustrated in Table 1. All students turning 11 in the year of adoption of the reform (who would start their fifth grade in that academic year) and all the younger ones were enrolled in the new system,

[14] Admission to either track was based on comprehensive school grades only.

while those turning 12 or more were never exposed. For instance, among students living in municipalities that implemented the reform in 1972, all those born in 1960 and before were never in the new system, while all those born in 1961 and afterwards were exposed to it.[15]

I will leverage this variation to first identify the impact of the reform on individual educational attainment and on the gender gap in education. I use a two-way fixed effects regression (in the spirit of a difference-in-differences with variation in treatment timing),[16] as well as an extended specification where the impact of the reform is allowed to differ by gender:

$$y_{im(r)c} = \alpha_0 + \alpha_1 OwnExposure_{mc} + \mu_c + \delta_m + \gamma_r \times t + \epsilon_{im(r)c} \quad (1)$$

$$y_{im(r)c} = \beta_0 + \beta_1 OwnExposure_{mc} + \mu_c + \delta_m + \gamma_r \times t + (\beta_2 + \beta_3 OwnExposure_{mc} + \mu_c + \delta_m + \gamma_r \times t) \times F_i + \epsilon_{im(r)c} \quad (2)$$

where $y_{im(r)c}$ is an indicator of educational attainment of individual $i$, born in municipality $m$ (located in region $r$) in cohort $c$; $OwnExposure_{mc}$ takes value 1 if cohort $c$ from municipality $m$ was affected by the school reform; $\mu_c$ are cohort fixed effects; $\delta_m$ are municipality of birth fixed effects; $\gamma_r \times t$ are region-specific linear trends (in cohort year), and $F_i$ is an indicator for female gender. Standard errors are clustered at the municipality of birth level. I will present results on the direct impact of the reform separately on men and women using specification (1), and on the gender gap in education (which will be captured by $\hat{\beta}_3$ from (2)).

One necessary condition for the causal interpretation of these results is that the timing of the adoption of the reform for different municipalities was unrelated to trends in educational attainment. In this sense, there are some potential caveats when using the variation generated from the adoption of the comprehensive school system (Pekkarinen, 2008). First, as shown in Figure 1, there were some municipalities in southern parts of the country which were assigned to implement the reform earlier than the rest of municipalities surrounding them. Although Table A2 shows that these localities did not present different educational characteristics than others within their region, one could still be worried that this choice might have been not random. Second, in the Helsinki

[15] All those born from 1961 on were exposed to the change in the tracking age from age 11 to 16. The years of exposure to the new curriculum depended on their age at the time of the reform. For instance, those that were in fifth grade when the reform was implemented were exposed to the new curriculum for five years, those in fourth grade were exposed to it for six years, and so on. This information is summarized in Table A1.

[16] Similar specifications have been used by papers studying the effects of the Finnish comprehensive school reform (Kerr and Pekkarinen, 2013; Pekkarinen, 2008; Pekkarinen et al., 2009) and other similar reforms in other Nordic countries (e.g. Meghir and Palme, 2005; Meghir et al., 2018; Monstad et al., 2008).

region, which was assigned to implement the reform in 1977, some municipality-run general secondary schools deviated from the existing selective system by taking in whole cohorts of students already some years before the official creation of comprehensive schools. As a result, in this region the reform might have been redundant. This would potentially lead to underestimation of the effects of the reform, given that "treated" units will serve as controls. To assess the impact these two features have on the results, in section 6.2 I perform robustness checks in which I exclude individuals from the Helsinki region and from these "outlier" municipalities that implemented the reform before their neighboring localities did.

To explore more generally whether this assumption is likely to hold, I perform an event study exercise in which I estimate changes in educational attainment by cohort, with cohorts normalized with respect to the first exposure to the reform in each municipality.[17] I estimate the event study using Callaway and Sant'Anna (2021)'s estimator. The results from this exercise are presented in section 5.1 and show no evidence of differential trends in education for municipalities implementing the reform at different times. Figure A2 further shows that municipalities that adopted the reform earlier (in years 1972-74) and those that adopted it later (in 1975-1977) were following similar marriage and fertility trends in pre-reform cohorts.[18]

Recent work on difference-in-differences methods by Goodman-Bacon (2021), Callaway and Sant'Anna (2021), and de Chaisemartin and D'Haultfoeuille (2020), among others, highlights other potential concerns with specifications like that in (1). Goodman-Bacon (2021) shows that, in models with variation in treatment timing, the two-way fixed effects difference-in-differences estimator can be seen as a weighted average of all the 2x2 difference-in-differences estimators that compare timing groups to each other (and to always-treated and never-treated units, if these exist). When treatment effects vary over time, relying on comparisons that use earlier-treated units as controls might bias the estimator. Callaway and Sant'Anna (2021) provide an alternative estimator that overcomes these concerns and is preferable in these settings.

In order to assess the extent to which the estimates of (1) are affected by these issues, I perform the Goodman-Bacon (2021) decomposition, which allows one to see which types of comparisons have the most weight for the aggregate estimator.[19] The results show that 84% of the weight comes from comparisons that use earlier-treated units as treatment and later-treated units as controls. Moreover, comparisons with earlier-treated

[17] For example, in municipalities implementing the reform in 1972, the 1960 cohort would have value -1, as it was the last cohort not exposed to the reform; the 1961 cohort would have value 0, and so on.

[18] More generally, Figures A3 and A4 show event studies for different family outcomes separately for men and women. There is no evidence of significant pre-trends for any of the outcomes.

[19] The decomposition was performed using the `bacondecomp` Stata package (Goodman-Bacon et al., 2019).

units as controls, which account for the remaining 16% weight, give almost identical point estimates (see Table A3). In consequence, time-varying effects are unlikely to be a source of bias in my specification. In any case, in section 5.1 I also present estimates of the impact of the reform on education using Callaway and Sant'Anna (2021)'s estimator.

### 3.2 Impact of marriage market exposure to the reform on family outcomes

In order to study how reform-induced changes in the gender gap in education in the pool of potential mates affect marriage and fertility, I regress different family outcomes on a measure of marriage market exposure to the reform. Marriage market exposure to the reform is calculated as the proportion of people in a person's marriage market who were enrolled in the new school system.

Crucially, these regressions also control for whether a given person was herself enrolled in the new system, as this in itself could affect their family outcomes, either through changes in their level of education or through changes in the set of peers to which they were exposed. We can separate marriage market exposure from own exposure to a certain extent, given that marriage markets do not fully coincide with municipality-cohort groups. This is because individuals do not marry only within cohorts—men tend to marry slightly younger women, while women tend to marry slightly older men—and because marriage patterns are broader than municipalities in geographical terms. For instance, among those who marry from pre-reform cohorts, only 24% of people marry someone born in the same municipality, while 53% of them marry someone born in the same region; less than 12% are married to someone from the same cohort, while more than 50% are in couples where the husband is from 0 to 3 years older than the wife.[20] The gradual implementation of the reform, together with these standard features of the marriage market, generate variation in the degree to which someone's marriage market is exposed to the reform, conditional on that person's individual exposure.[21]

I thus run the following type of regressions:

[20]The distribution of the age difference within couples, calculated as husband's minus wife's age, for men and women in pre-reform cohorts is shown in Figure A5.

[21]To see this, consider for instance the case of men born in 1960. These men were not exposed to the reform in any part of Finland. However, in municipalities that implemented the reform in 1972, women born in 1961 or later were enrolled in the new system. Hence, the marriage market of 1960 men was substantially exposed to the reform. This exposure was lower in municipalities that adopted the reform later. For example, in municipalities that implemented the reform in 1977, the marriage market of the 1960 cohort of men was barely affected by the reform. Moreover, the fact that not all contiguous municipalities implemented the reform in the same year gives rise to additional variation in marriage market exposure. Figure A6 shows how even within regions (with borders marked in thicker lines) there is variation in reform timing.

$$y^g_{im(r)c} = \lambda_0 + \lambda_1 MarriageMarketExposure^g_{rc} + \lambda_2 OwnExposure_{mc} \quad (3)$$
$$+ \mu_c + \delta_m + \gamma_r \times t + (\lambda_3 + \mu_c + \delta_m + \gamma_r \times t) \times F_i + v^g_{im(r)c}$$

where $y^g_{imrc}$ is the outcome of individual $i$, of gender $g$, born in municipality $m$ of region $r$ in cohort $c$; $MarriageMarketExposure^g_{rc}$ indicates the proportion of women (men) in a man's (woman's) marriage market who were exposed to the new school system; $OwnExposure_{mc}$ takes value 1 if cohort $c$ from municipality $m$ was affected by the school reform; $\mu_c$ are cohort fixed effects; $\delta_m$ are municipality of birth fixed effects; $\gamma_r \times t$ are region-specific linear time (cohort) trends, and $F_i$ is a dummy equal to 1 for women. Standard errors are clustered at the municipality of birth level. I present results from equation (3) for the pooled sample, and also run these regressions separately for men and women. In section 6.2 I further discuss and show results with alternative specifications, such as including municipality-specific trends instead, partialling out region-specific linear pre-trends, or allowing for different sources of effect heterogeneity.

I measure marriage market exposure in different ways. In my preferred measure, I consider individuals born in the same region as belonging to the same marriage market.[22] I then use the distribution of the age difference within couples in pre-reform cohorts, separately for men and women (see Figure A5), to impute the probability that person $j$ belongs to person $i$'s marriage market based on the age gap between the two. These probabilities are used as the weight that person $j$ has on $i$'s marriage market.[23] Specifically, marriage market exposure for individuals of gender $g$, born in region $r$ in cohort $c$, is calculated as a weighted average of exposure to the reform in their marriage market, as follows:

$$MarriageMarketExposure^g_{rc} = \sum_{m' \in r} \sum_{c'} (\widehat{\omega}^{g'}_{c',c} \times w^{Pop}_{m'c'}) OwnExposure_{m'c'} \quad (4)$$

where $\widehat{\omega}^{g'}_{c',c}$ is the estimated probability that an individual of gender $g'$ and from cohort $c'$ belongs to the marriage market of individuals of gender $g$ from cohort $c$, based on the age difference between the two (and their gender); $w^{Pop}_{m'c'}$ are weights for the population size of cohort $c'$ in municipality $m'$, and $OwnExposure_{m'c'}$ is an indicator equal to 1 if individuals from cohort $c'$ and municipality $m'$ in region $r$ were exposed to the reform (where $c'$ can be equal to $c$, and $m'$ can be equal to $m$).

[22] There are currently 19 regions in Finland, with the number of municipalities per region varying from 9 to 57 (median of 27). Figure A6 shows the map of Finland with the delimitation of regions and municipalities, together with the reform implementation year.

[23] Figure A7 shows, as an example, the resulting weights that men have for 1960 women's marriage market (in panel a) and that women have for 1960 men's marriage market (panel b) based on their year of birth.

Figure A8 shows the distribution of marriage market exposure separately for those exposed and not exposed to the reform themselves, as well as the effective available variation after controlling for gender, cohort, and municipality of birth. As one can see, there is substantial variation in marriage market exposure to the reform, even conditional on own exposure. This variation stems from two facts: i) the fact that marriage markets include other cohorts than a person's own, and ii) the fact that not all municipalities in a region implemented the reform at the same time (see Figure A6). Figure A9 further illustrates the share of variation stemming from each of these two facts, by showing how the available variation changes when shutting down each of these two sources one at a time. The largest share of the variation arises from the fact that marriage markets include other cohorts, but the contribution of the differences in reform timing within regions is not negligible either. In section 6.2 I discuss in more depth the identification threats posed by the correlation between the two exposure variables and show that the results are robust to a variety of sensitivity checks.

One potential additional concern is that the definition of the relevant marriage market changes as a result of the reform itself. In Table A4 I explore whether this is likely to be the case. Using the specification in equation (1), I check if exposure to the reform changed the average age gap within the couples or the probability of marrying someone from the same region. The results show that the reform did not significantly affect any of these aspects.

Nevertheless, I also explore the sensitivity of the results to using alternative marriage market definitions, including the following: a) considering only individuals born in the same region and with an age difference of 0 to 3 years in favor of the man; b) using the weights based on the age difference as in the baseline definition, and also weights based on the distance between municipalities of birth;[24] c) using weights based on age difference (as in baseline definition), together with weights for the surrounding municipalities of birth based on the frequency of marriage of people from those municipalities in pre-reform cohorts. In section 6.1 I discuss how results vary with these different measures of exposure.

Finally, I conduct a randomization inference exercise, both in the spirit of a placebo check and to test the robustness of the main results to clustering standard errors at a coarser level. In the main specification in (3), standard errors are clustered at the municipality level, given that own exposure to the reform varies by municipality and cohort. However, the baseline definition of marriage market exposure to the reform changes by region, cohort, and gender. Since there are only 18 regions in the sample,

[24] In particular, I calculate the probability that a person from municipality $m'$ belongs to the marriage market of a person from municipality $m$ as the (normalized) inverse of the distance between the two municipalities. Figure A10 shows, as an example, the weight that individuals from each municipality have in the marriage market of people from Tampere depending on the distance.

clustering at the region level is likely to lead to invalid inference. MacKinnon and Webb (2020) propose randomization inference for these cases.

To implement the randomization inference test, I randomly permute the values of marriage market exposure across region×year of birth×gender groups 1,000 times. I then regress the different dependent variables on these placebo marriage market exposure variables, controlling for own exposure to the reform, gender, and for year of birth fixed effects, region fixed effects, and region-specific linear trends (interacted with gender), with standard errors clustered at the region level.[25] I save the resulting coefficients and t-statistics and compare the distribution of effects from these permutations to the actual estimates. The fraction of placebo coefficients or t-statistics that are more extreme than the observed ones yields the randomization inference p-value.[26] These results are discussed in section 6.2.

# 4 Data and descriptive statistics

## 4.1 Data

The main data source for the analysis is the FLEED-FOLK (Finnish Longitudinal Employer– Employee Data) dataset provided by Statistics Finland. It contains rich information about all individuals permanently living in Finland at the end of a given year. For the main part of the analysis, I use the files for years 1988-2006 and select all individuals born in Finland and aged 40 in each year. Hence, my sample consists of all Finnish-born individuals from cohorts 1948-1966 who are still living in Finland by age 40.[27] The region of Åland islands is excluded from the sample due to lack of information about the year of adoption of the reform. As a result, my sample consists of 1,460,448 individuals from 430 municipalities in 18 different regions.

The database contains basic information about the year, municipality and region of birth, as well as the following variables regarding each statistical year: municipality of residence, civil status and family structure, educational attainment, and labor market status, among others. Besides the basic file, I use the supplementary marriage module, which contains more detail about the history of marriages and divorces (including the spouse identifier), and the intergenerational module, which allows me to link

[25] This exercise was implemented using the `ritest` command by Heß (2017).

[26] MacKinnon and Webb (2020) discuss that whether inference based on t-statistics or that based on coefficients performs better depends on the specific case; inference based on t-statistics tends to dominate when there are few treated clusters.

[27] The selection of age 40 allows me to have data on the relevant cohorts—those for which there is variation in exposure to the reform—and on a good number of pre-reform cohorts. At the same time, it is a reasonable age at which to study family outcomes: for the 1957 cohort, for instance (at the middle point of the sample), the average age of first marriage was 25 for women and 27 for men, and the mean ages of first-time parenthood were 26 and 27 for women and men, respectively.

individuals to their children and to their parents.

I combine the information about the year and municipality of birth with the year of adoption of the reform in each municipality (as depicted in Figure 1) to construct a binary variable indicating if individuals were exposed to the new school system or not. Since in the FLEED-FOLK dataset I only observe the municipality of birth, rather than the municipality where children were living at school age, estimates of this exposure variable could be affected by measurement error. I supplement the main dataset with information from the 1970 Census to check if defining exposure to the reform based instead on municipality of residence in 1970, just before the implementation of the reform, makes a difference. These results are discussed in section 5.1. For each person, I then construct a measure of their marriage market's exposure to the reform as a weighted average of the individual exposure indicators of the people in their marriage market, as explained in section 3.2.

In order to study the impact of the reform on educational attainment, I construct an indicator variable for having more than secondary education, and an indicator for having at least a bachelor's degree or equivalent level.[28] In terms of marriage outcomes, I use the history of marriages to construct indicators for having married and for having divorced by age 40, to calculate the age at first marriage, to construct an indicator for being married or cohabiting at this age, and to obtain the identifier of the first spouse. Using the spouse identifier I collect information about their year and place of birth and their educational attainment. This allows me to construct indicators for whether a person is equally, more, or less educated than their spouse, and for the age difference between them. The analysis focuses on heterosexual couples, given that there are virtually no same-sex couples in the data for the cohorts of the sample.[29] I examine the following fertility-related variables: the number of children a person has by age 40, and indicator for childlessness at this age, and a measure of age at first birth. In robustness checks I further explore fertility outcomes at later ages.

In supplementary analyses I also explore annual labor earnings and an indicator for being employed at age 30. Due to data limitations, these analyses only include individuals from cohort 1958 onwards. Finally, I combine these datasets with the Finnish Hospital Discharge Register, which contains information about the diagnosed medical conditions coded with the International Classification of Diseases (ICD-10), medical operations, and the date of diagnoses. I use data from outpatient and inpatient visits from 1998 to 2011 and construct individual indicators for having a visit with a

[28]The available variables for educational attainment are left-censored, and only distinguish among education levels starting from the upper secondary level. As a result, for lower levels, one can only know that a person did not achieve upper secondary education, but one cannot tell whether they finished compulsory schooling or dropped out.

[29]Registered partnerships for same-sex couples were introduced in Finland in 2002, and same-sex marriage was not legalized until 2017.

given diagnosis at ages 40-45.[30] This analysis is thus also restricted to individuals born from 1958 onward. I look at the following groups of diagnoses: mental health problems and abnormal emotional symptoms (ICD10 F09-F99 and R45), alcoholic liver disease, chronic hepatitis, and cirrhosis (K70, K73,K74), and drug overdoses (T36-T51).

### 4.2 Descriptive statistics

Table A5 shows descriptive statistics for the full sample, and separately for men and women, for all variables used in the analyses. In order to visualize how the main variables of interest changed across over time, Figure 2 presents the aggregate trends in education and family structure in Finland from 1948 to 1970. While at the beginning of this period there were more men than women with university degrees, the gender gap in university education closed with the cohorts born around 1960, and for cohorts born by 1965 there was already a female advantage, which continued to grow thereafter. At the same time, there were substantial changes to family structure. Marriage rates declined over this period: the percentage of men who were ever married by age 40 declined by 14%, while there was an 8% decrease for women. Similar declines are observed in the share of men and women who are married or cohabiting at age 40. Finally, the average number of children per woman, which was increasing until the 1960 cohort, plateaued and then started to decrease for younger cohorts.

Figure A11 further shows the distribution of educational attainment for men and women just before (cohorts 1956-60) and just after the reform (cohorts 1966-70). It plots the percentage of men and women in each cohort group with three levels of education: basic (with at most upper secondary education), medium (more than secondary education, but less than university degree), and high (university degree or higher). In the pre-reform cohorts, there were substantially more men than women with low level of education, but also slightly more men than women with university degree. Post-reform cohorts had in general higher educational attainment, with decreases in the percentage of men and women with low education and increasing prevalence of university degrees. This increase was larger for women: the gender gap in having low educational attainment increased from 9.8 to 16.3 percentage points, and the gap in university education was reversed, such that in post-reform cohorts there is a 4 percentage point female advantage.

[30] Given the low prevalence of some of these outcomes, I consider not only age 40 but ages 40-45.

# 5 Results

## 5.1 Impact of the reform on the gender gap in education

The estimates of the impact of the reform on educational attainment for women and men and on the resulting gender gap, using the specifications of equations (1) and (2), are shown in Table 2. The first three columns show the results for the probability of having more than secondary education, while the last three columns have an indicator for having at least university education as dependent variable.

The results show that the reform had a positive effect on women's educational attainment, but virtually no impact on men's education. Women exposed to the reform had a 1.2 pp higher probability of having more than secondary schooling (a 3.1% increase with respect to the pre-reform average), and 0.7 pp higher probability of having university education (a 5% increase). As a result, the female advantage in having more than secondary education increased by 1.1 pp (a 11% increase). The former average gender gap in university education in favor of men (1 pp) was virtually closed, as the female advantage increased by 0.8 pp.[31]

These findings are consistent with previous results by Pekkarinen (2008) showing that the reform increased the female advantage in choosing the academic track and in entering into tertiary education. He discusses that this differential effect on boys and girls is likely related to gender differences in the timing of puberty, with girls entering into adolescence before boys. While up to age 11 boys and girls have on average developed at the same pace, around this age their trajectories temporarily diverge, and by age 16 the gender gap in maturity might exacerbate the gender differences in academic performance and educational choices. This is in line with cross-country evidence of a positive association between late tracking and a larger female advantage in educational attainment (Pekkarinen, 2008; Scheeren et al., 2018)

As discussed in section 3, to evaluate the extent to which the timing of the adoption of the reform for different municipalities was unrelated to trends in educational attainment, I perform an event study exercise. In particular, I estimate changes in female educational attainment by cohort, with cohorts normalized with respect to the first exposure to the reform in each municipality. The estimation sample is restricted to make it balanced in event time, to avoid compositional changes in the groups when establishing comparisons across different leads and lags, as recommended by Callaway and Sant'Anna (2021). To be able to estimate effects two years after first adoption of the reform, the sample only includes municipalities implementing the reform from 1972 to 1974. The results of this exercise, estimated using Callaway and Sant'Anna (2021)'s estimator, are shown

[31] Table A6 shows that results are similar if the variable of exposure to the reform is constructed based on municipality of residence in 1970, obtained from the 1970 Census, rather than on municipality of birth.

in Figure 3.[32] We see no significant differences in education across cohorts before the reform, and a significant increase in female education after its implementation. Table A7 further presents Callaway and Sant'Anna (2021)'s average treatment effects of the impact of the reform on the probability of high education, separately for women and men (see section 3.1). The results are similar in magnitude to those in Table 2, and suggest that the reform led to a significant 1.5 pp increase in the probability of having more than secondary education for women, while it did not significantly affect men's educational attainment.

Finally, Table A8 explores the effect of the reform on gender gaps in the labor market by age 30, using the same specification as in (1) and (2).[33] The first three columns show results for the effect on labor earnings, while the last three columns show results for the probability of being employed at age 30. We see that the reform led to a small and non-significant increase in women's earnings, consistent with the findings by Pekkarinen (2008). As a result, the gender earnings gap decreased non-significantly by around 90 euro or 1%. The reform did not affect the probability of being employed at age 30 significantly for either women or men.[34] Thus, the change in the gender gap in education seems to have led to small changes in the relative labor market position of men and women.

### 5.2 Impact of marriage market exposure to the reform on marriage and fertility

This section presents the main results for the impact of marriage market exposure to the reform on family outcomes. The first panel of Table 3 shows pooled results for the whole sample, while panels B and C show results separately for men and women. The first two columns show the estimates of the effect of marriage market exposure on marriage outcomes: on the probability of having ever married by age 40 (column 1) and on the probability of being in a couple, either married or cohabiting, at this age (column 2). The last two columns show results for the impact of marriage market exposure on the probability of not having had any children by age 40 and on the number of children by this age.

The results show that marriage market exposure to the reform did not significantly affect the probability of having been in a formal marriage, although the coefficients are negative in all panels. However, the probability of being in a couple at age 40 decreases

[32] Results on the full, unbalanced sample, are presented in Figure A12.

[33] Ideally we would like to observe labor market outcomes as early as possible, before individuals "enter" into the marriage market. However, labor and marriage decisions are likely to be almost simultaneous in many cases, and due to data limitations the earliest the 1960 cohort is observed is at age 28.

[34] Ollikainen (2021) explores the dynamic effects of the reform on labor market outcomes and finds that it led to a lower probability of working at age 21 for both men and women, but that this negative effect disappears with age.

by 0.8 pp (a 1.1% decrease), with these effects being stronger on average for men (who see a 1.4% decrease). Marriage market exposure to the reform also had negative effects on fertility: the probability of not having any children increases by 1.1 pp (5%) and the number of children decreases by 1.4%. These effects are visible for both men and women.[35]

On the other hand, own exposure to the reform does not seem to have affected these outcomes on average. Interestingly, the estimates in Panel C show that women who were directly exposed to the reform, and had thus on average higher education, were if anything more likely be in a couple and had more children. These results are in line with previous literature showing that increases in women's education have small effects on completed fertility in industrialized countries, which are even positive in some cases (Fort et al., 2016).[36] They are also consistent with recent evidence for younger cohorts in Finland by Virtanen et al. (2024), who find that higher education increases women's probability of having children and a partner. This suggests that the negative effects of marriage market exposure are not simply driven by the high-educated women in these more affected marriage markets being less likely to marry and having lower fertility. The "mismatch" between the distributions of educational attainment of men and women seems a more plausible explanation, which I explore further in section 5.3.[37]

In order to put the magnitude of these effects in context, I compare the effect sizes with the observed change during the period of study, and with the estimates from Bertrand et al. (2015) on the impact on the family of changes in the gender gap in earnings. Among the cohort born in 1948 in Finland, 80% of individuals where married or cohabiting at age 40. This number declined to 72% for those born in 1966. A one standard deviation increase in marriage market exposure to the reform, which would lead to a 3% increase in the female educational advantage, can account for around 10% of this decrease. Compared to the results by Bertrand et al. (2015), in turn, I find that the effect on the share of married males of a one standard deviation increase in marriage market exposure to the reform would be roughly equivalent to the effect of a 2.8 pp increase in the probability that a woman earns more than a man in the marriage

[35]The effects on fertility at age 40 could potentially be driven by changes in assortative mating by age or education, or by delays in the age at first birth. However, Table A9 shows that results are similar if I study fertility outcomes at ages 45 or 50. In section 5.4 I further examine assortative mating and the timing of fertility as outcomes.

[36]Own exposure to the reform, as discussed earlier, could capture not only changes in a person's own level of education, but also the effects of having the whole cohort exposed to the reform. This observation is also true for a large part of the previous literature which has used the variation from staggered implementation of school reforms to study the impact of increases in education on family outcomes (e.g. Black et al., 2008; Fort et al., 2016; Monstad et al., 2008). In this sense, the results in this paper should thus be largely comparable to those of prior work, while acknowledging that they might reflect changes not only to a person's own level of education but also to that of their peers.

[37]Table A10 in the Appendix shows the results when including the own exposure and marriage market exposure coefficients separately one at a time.

market.[38]

### 5.3 Interpretation of results

The results from the last subsection show that, on average, higher marriage market exposure to the reform leads to decreases in marriage and cohabitation and in fertility. Due to the reduced-form nature of the analysis, these findings do not rely on the claim that only the gender gap in education is changing in more affected marriage markets. I argue, however, that changes in the female advantage in education in these markets are an important driver of these effects. This subsection provides several pieces of evidence that support this interpretation.

First, if in more affected marriage markets there is a larger "mismatch" or dissimilarity between the educational distributions of men and women, such that it becomes more difficult to find a partner with the same level of education as oneself, we would expect larger declines in marriage and fertility for high-educated women and low-educated men. Similarly, if we consider educational level-specific sex ratios (Negrusa and Oreffice, 2010), in marriage markets with greater exposure to the reform there will be a decrease in the ratio of men to women with high levels of education, and an increase in the ratio of men to women with low levels of education. Low-educated men and high-educated women thus become relatively more abundant and, according to previous literature, we would expect them to experience decreases in marriage and fertility (Abramitzky et al., 2011; Angrist, 2002). In order to see if this is the case, I explore heterogeneous effects by level of education.

There is a caveat when perform this heterogeneity analysis, especially for women: because the reform had a direct effect on educational attainment for them, conditioning on level of education for the whole sample would lead to biased estimates. When studying women's outcomes, I will therefore focus on cohorts not exposed to the reform themselves, and exploit variation in degree of exposure in their marriage market only. Given that this substantially limits the available variation in marriage market exposure and may compromise statistical power, in order to corroborate results I perform a separate exercise where I split the sample by parental education.[39]

The first four columns of Table 4 show results for the effect of higher marriage market exposure on marriage and fertility outcomes separately for high- and low-educated

[38]The definitions of the outcome variables in Bertrand et al. (2015) differ slightly from mine. In their case, the share of married males refers to the proportion of males who are currently married in each marriage market, which is defined for broad age groups (e.g. men aged 24-33), so the estimate refers to an average effect across different ages. In my analysis, in turn, this estimate refers to the probability for men of being in a couple (married or cohabiting) at age 40.

[39]Interpreting the results from this analysis also requires some caution, as the reform may have changed the elasticity of children's education with respect to that of their parents (Pekkarinen et al., 2009).

men and women, where low-educated individuals are those with at most secondary education. For men, we see significant negative effects on the probability of being in a couple and the number of children only for low-educated men, while the coefficients are smaller in magnitude and not significant for those with high level of education. In fact, we see a marginally significant decrease in the probability of being childless for high-educated men, while this probability increases non-significantly for low-educated ones. For women, in turn, we see a significant decline in the probability of having married by age 40 for high-educated women, as well as a non-significant decrease in the probability of being in a couple at age 40. In contrast, I do not detect any significant negative effect for low-educated women.

The results by parental level of education, presented in the last four columns of Table 4, corroborate these patterns. For men, we see significant declines in the probability of being in a couple and the number of children only for those with low level of education (although the estimates for the number of children are very similar for both groups). For women, we see declines in marriage only for those with high level of education. The results on childlessness are similar for both groups of women, but we see larger declines in the number of children for the highly-educated ones.

All in all, this evidence is consistent with more exposed marriage markets having a larger mismatch among the distributions of educational attainment of men and women, such that there are "excess" numbers of high-educated women and low-educated men who are unable to find a suitable match.

Following this same line of reasoning, we would expect stronger effects in marriage markets where the size of the gender gap in education in absolute value increased more as a result of the reform—the larger this absolute difference, the harder it is to find a partner with the same level of education as oneself. The male-female gap in (university) education before the reform varied across regions: while in some regions men had a large advantage, in others women had already caught up to a great extent. As a result, the increase in women's education induced by the reform led, in absolute terms, to decreases in educational mismatch in some markets, to increases in others, and to little change in some (but to a reversal of the gap). I classify regions into two groups: regions in which the gender educational mismatch would be predicted to increase in absolute terms with the reform, and regions in which it would be predicted to decrease or not to change.[40]

It should be noted that, if the increase in educational mismatch was the only force driving the results, we would not expect to see negative effects on marriage or fertility in marriage markets where mismatch did not increase. In those markets, the only change

[40]Specifically, in my classification the mismatch (gender gap in university education in absolute terms) is predicted to increase in regions where the share of women with university education was greater or equal than that of men already in pre-reform (1956-1960) cohorts.

induced by the reform was making women more educated than men. The presence of negative effects also in those regions would suggest that not only the size of the gender gap, but also its sign, matter, consistent with the importance of gender identity norms.[41]

I explore heterogeneity by the predicted change in the gender gap in education induced by the reform at the marriage market level in Table 5. For each outcome, the first column displays the estimates for regions in which the gender gap in education is not predicted to increase in absolute terms, while the second column shows results for those in which it is predicted to increase. The results show that, for all outcomes, the effects are stronger in marriage markets where the reform is predicted to increase educational mismatch: higher marriage exposure leads in these regions to larger declines in both marriage and fertility. The difference across groups is statistically significant (at the 5% level) for the probability of being in a couple, childlessness, and the number of children.

Overall, the results from this exercise suggest that the increase in educational mismatch in marriage markets more affected by the reform seems to be an important channel driving the results. However, the fact that marriage market exposure has a negative impact also in regions where mismatch is not predicted to increase implies that a potential role of gender identity norms cannot be ruled out.

### 5.4 Other family outcomes: Assortative mating, marital dissolution, and timing of family formation

Higher marriage market exposure to the reform might also affect other family-related outcomes, such as assortative mating, the probability of marital dissolution, or the timing of family formation. However, the causal pathway to these outcomes is mediated by the impact of marriage market exposure on the probability of marriage itself. With these caveats in mind, in this section I provide some suggestive evidence about the relationship between marriage market exposure and assortative mating by education and age, marital dissolution, and the timing of marriage and fertility.

The first three columns of Table 6 present estimates of the association between marriage market exposure and the relative level of education within married couples. For ease of interpretation, I present results for men. Higher marriage market exposure is related to an increased probability for men of being less educated than their spouse. This is consistent with previous descriptive evidence by Esteve et al. (2012, 2016) showing that, as the female advantage in education increases in the population, so does the prevalence of couples where the wife is more educated.

The fourth column shows results for the age difference within couples, expressed as

[41] Akin to the social norms about relative earnings discussed by Bertrand et al. (2015), there might be a resistance to a situation in which the wife has higher education than her husband.

husband's minus wife's age, such that it is on average positive. The estimates suggest that higher marriage market exposure is correlated with a smaller average age difference, and the inspection of different margins reveals that this comes from a decrease in the number of couples where the wife is 4 or more years younger than her husband.[42] Finally, the last columns show that marriage market exposure is not related to any significant change in the probability of (formal) divorce for men. Column 5 shows results for all men, and column 6 restricts the sample to those who married by age 40. When considering this latter group, we see a larger and positive coefficient (which is still not statistically different from zero). Overall, we cannot rule out that higher marriage market exposure is associated with an increase in marital instability, which might contribute to the observed decrease in the probability of being in a couple at age 40.[43]

In Table A12, I further explore the relationship between marriage market exposure to the reform and the timing of family formation. The first column shows results for the age at first marriage; the second column, for the age at first birth, and the third column explores the probability of having at least a nonmarital birth, defined as any birth that happened prior to the person's first marriage. The first panel shows that marriage market exposure to the reform is not related to any significant change in these outcomes for men. The second panel reveals that, for women, higher marriage market exposure to the reform is associated with significant delays in the ages at first marriage and first birth. The delay in the age at first birth is larger than that in the age at first marriage; as a result, the probability of having a nonmarital birth decreases. These changes are consistent with the idea that marriage markets more affected by the reform present more adverse conditions for finding a suitable match. However, given that the timing decisions can only be studied for those who marry or have at least one child, respectively, where that margin is impacted by exposure to the reform, these results

[42]Given the higher probability of exposure to the reform, and the higher average levels of education among these younger women, it might become more difficult for men to find a partner among this group. These results are available upon request.

[43]An inspection of the divorce probability by level of education shows no evidence of significant changes for any group (see Table A11). When splitting by parental education, there is some indication of a decreased probability of divorce for women with highly-educated parents, marginally significant at the 10% level. On the one hand, this could be in part mechanically driven by the decrease this group experiences in the probability of ever getting married. On the other, to the extent that there is a decrease in the probability of divorce conditional on marriage, this could explain why this group presents no change in the probability of being in a couple at age 40, despite the decrease in the probability of having ever formally married. For context, access to divorce was relatively easy in Finland during this period. Since 1988, the relevant period for most cohorts in the analysis, the principle governing access to divorce was that of "divorce on demand." In order to obtain a divorce, one or both spouses must simply file for divorce, with no justification being needed. There is a mandatory six-month reconsideration period, after which the divorce is finalized upon confirmation from one or both spouses. If the spouses have been living apart for at least two years, the reconsideration period is not necessary and immediate divorce can be granted (Savolainen, 2002).

cannot be given a causal interpretation.

### 5.5 Health implications

Finally, declines in men's value in labor and marriage markets have been associated with negative health consequences, like increases in premature mortality, especially from "deaths of despair"; i.e., suicides, and alcohol and drug related problems (Autor et al., 2019; Case and Deaton, 2017; Coile and Duggan, 2019). The combination of data from administrative and hospital registers allows me to explore whether men's health outcomes are affected in marriage markets with a larger female advantage in education, and to study less extreme health measures than mortality.

The results are shown in Table 7. Each row shows the coefficient of marriage market exposure to the reform from separate regressions with indicators for different health problems as dependent variables: mental health problems, alcoholic liver disease, and drug overdose. These indicators take value 1 if the person had a hospital visit (inpatient or outpatient) at ages 40-45 with one of these diagnoses. The first column shows results for all men, while columns 2-3 present heterogeneous results by level of education. We would expect low-educated men to be the most affected, given that the effects on family outcomes were stronger for them.

The estimates suggest that in marriage markets with a higher exposure to the reform, and thus with a larger female advantage in education, men have on average a higher probability of having mental health hospital contacts, but do not present more hospital visits with alcoholic liver or substance abuse diagnoses. The heterogeneity analysis in columns 2-3 reveals that these negative effects are entirely driven by low-educated men.[44]

These results suggest that the increasing female advantage in education, and its associated changes in family structure, might have negative consequences for men's mental health. This is consistent with Bergvall (2024)'s findings that relative increases in women's potential earnings increase their husbands' probability of hospital visits due to stress, anxiety, substance abuse, or assault.

[44]None of the coefficients of own exposure to the reform are significant in these regressions. A recent paper by Böckerman et al. (2021) looking at the "direct" effects of the comprehensive school reform on mental health finds no discernible effects either. In Table A13 I also explore the effect of marriage market exposure to the reform on women's mental health, both for all women and by level of education, for the sample not directly affected by the reform. I find positive but insignificant results, which seem to be driven by women with low level of education.

# 6 Supplementary analyses

## 6.1 Measuring marriage market exposure

As discussed in section 3, in my baseline estimation the definition of marriage market exposure consists of a weighted average of individuals' exposure to the reform in someone's marriage market, geographically defined as their region of birth. The weight that different individuals have for someone's marriage market depends on the age difference between them (and gender), based on the distribution of the age gap within couples in pre-reform cohorts.

In this section I discuss how the main results differ when alternative specifications of the marriage market are used. In particular, I consider the following alternatives: 1) focusing only on individuals born in the same region and within the most common age gap, that is, 0-3 years in favor of the man; 2) using weights for the probability that $j$ belongs to $i$'s marriage market based on their age difference (as in the baseline) and their municipality of birth, using the frequency of marriages across different municipalities in pre-reform cohorts; and 3) using weights for the probability that $j$ belongs to $i$'s marriage market based on their age difference (as in the baseline) and the inverse distance of their municipalities of birth.

Results for the different family outcomes using the baseline (column 1) and these alternatives definitions of marriage market exposure are compared in Table 8. The main conclusions are not affected by changing the definition of marriage market. The measure of exposure that yields the most different results is the one that uses the age distribution from pre-reform cohorts (as in the baseline) and the normalized inverse distance between municipalities of birth as weights. The estimates using this measure are in most specifications substantially larger than the baseline estimates. The definition that uses the frequency of marriage across municipalities in pre-reform cohorts yields similar but noisier results. Finally, the definition that restricts the marriage market to those born in the same region and within an age gap of 0-3 years also gives consistent, yet slightly smaller estimates. Part of this difference could be explained by the rigidity of this definition, which captures effects only for a part of the marriage market. This is likely to introduce measurement error that biases the estimates downwards. Overall, however, using one or another definition of marriage market does not affect the qualitative conclusions.

## 6.2 Robustness tests

In this section I check the sensitivity of the main results to alternative control strategies and sample choices. Table 9 compares the coefficient of marriage market exposure (expressed in standard deviations) in the baseline specification (column 1) with several

alternatives. Each row shows results from separate regressions with different dependent variables. The first column also shows the Romano-Wolf stepdown adjusted p-values to correct for multiple hypothesis testing in the baseline specification. All main results survive this adjustment.

In column 2 region-specific linear trends are replaced with municipality-specific linear trends. The results remain virtually unaltered. In column 3, instead of including linear trends, I instead de-trend the dependent variable of gender- and region-specific linear pre-trends. To do so, I follow Goodman-Bacon (2021) and estimate pre-trends by regressing the dependent variable on gender- and region-specific linear trends for cohorts up to 1960. These trends are next subtracted from the full panel. The specification then includes only municipality and cohort of birth fixed effects. Standard errors are bootstrapped to account for the two-step estimation. Using this method has no visible effect on most results, except for the coefficient on the probability of being in a couple at age 40. Next, column 4 shows results including region by year of birth fixed effects (instead of gender- and region-specific linear pre-trends), together with gender and municipality fixed effects. The results are largely consistent with the baseline estimates, albeit a bit larger in magnitude for the probability of having ever married and the number of children.

In the next two columns I show results using the baseline specification but restricting the sample in different ways. First, as discussed in section 5.1, municipalities in Helsinki region had started to implement the reform before they were supposed to according to the adoption plan. To check whether this affects the results, in column 5 I exclude individuals from this region. In spite of the reduced sample size, the estimates remain consistent, albeit a bit smaller, suggesting that the potentially different trends of the capital region are not completely driving the results. Next, some municipalities were assigned to adopt the reform earlier than most of their surrounding localities (see section 3). As discussed by Pekkarinen (2008), the choice of these municipalities is unlikely to have been random. In column 6 I drop individuals from these municipalities and find that results are unaffected. This indicates that the combination of municipality fixed effects and region-specific trends effectively controls for any potential differences in levels or trends. In column 7 I employ an alternative measure of marriage market exposure based on the municipality of residence in 1970 (just prior to the implementation of the reform), instead of the municipality of birth. The results using this alternative measure are similar and in general larger than the baseline estimates. Finally, in column 8, I control for the economic conditions individuals face at age 40 by adding the regional unemployment rate of that year as a control.[45] The coefficients are not sensitive to this addition.

[45]Source: Statistics Finland, Employment, available at `https://stat.fi/en/statistics/tyokay`. Last accessed: November 18, 2024.

Next, I explore the implications of the correlation between the measures of own and marriage market exposure to the reform for the identification of treatment effects. Given how marriage markets are defined and how marriage market exposure is calculated, one particular concern is that the measure of women's marriage market exposure to the reform is positively correlated with the length of exposure to the reform in a municipality. If, for instance, the effects of own exposure on marriage or fertility grow with length of exposure, one might worry about women's marriage market exposure variable partly capturing that. Appendix Figures A3 and A4, which display event study estimates for the different family outcomes for men and women, show little evidence of dynamic treatment effects of own exposure to the reform for either gender. We see small and insignificant effects of direct exposure to the reform on these outcomes. These are consistent with the results in columns 4 and 7 of Table A10, which estimate the effect of own exposure on family outcomes with the usual specification but without controlling for marriage market exposure.[46] Overall, the event study results suggest that it is unlikely that dynamic treatment effects significantly affect the estimation of the effects of marriage market exposure.

In any case, in Table 10 I further explore the sensitivity of the marriage market exposure coefficients to allowing for different sources of heterogeneity in the effects of own exposure. Table 10 compares the baseline estimates in column 1 with the estimates when considering different functional forms. In particular, column 2 allows the effect of own exposure to change linearly with the number of years a municipality has been exposed to the new school system, while column 3 controls non-parametrically for the number of years of exposure. Even more generally, column 4 allows for dynamic treatment effects, by substituting the coefficient of Own exposure with dummies indicating the number of cohorts relative to the first exposure to the reform in each municipality. Lastly, column 5 evaluates the robustness of the main estimates to allowing for heterogeneous effects of own exposure by treatment cohort, by interacting the indicator of Own exposure with indicators of the year of implementation of the reform in the municipality. The first panel shows results for the whole sample, while the middle and bottom panel show results separately for men and women. The coefficients are remarkably stable across columns and similar to the estimates from the baseline specification, suggesting that identifying the effect of marriage market exposure does not require to assume away relevant functional forms on the effect of own exposure to the reform.

[46] Note that, while event studies are estimated with Callaway and Sant'Anna (2021)'s estimator, the static specification in Table A10 is estimated with two-way fixed effects. Moreover, event studies are estimated on a sample balanced in event time that only includes municipalities for which we can estimate treatment effects for at least two years after first adoption of the reform (municipalities implementing the reform from 1972 to 1974). Estimates of the overall treatment effect of own exposure on family outcomes with Callaway and Sant'Anna (2021)'s estimator are similar to those reported in Table A10 and available upon request.

A related concern is that the opposite-signed effects on the exposure variables across all outcomes in Table 3 could be capturing regression to the mean in family outcomes across cohorts. In order to assess if my specification leads to a systematic negative relationship between own exposure and marriage market exposure to the reform, I conduct a placebo exercise where I randomly permute the year of the reform across municipalities, calculate measures of own exposure and marriage market exposure based on these placebo reform dates, and estimate the main regressions. I repeat this exercise 500 times. The results are presented in Figure A13, which shows, for each outcome, the share of times the coefficients on the placebo exposure variables have the same and the opposite sign (left panel). These proportions are all very close to 50%, with no evidence of a systematic negative relationship between both effects. On the right panel, I plot the coefficients of marriage market exposure against the coefficients of own exposure across placebo regressions and report the correlation coefficient. These correlations are very close to zero across the board, and positive for two out of the four outcomes. Taken together, the results from this placebo exercise suggest that the main coefficients are not capturing regression to the mean across cohorts.

Finally, Figure 4 presents the results from the randomization inference exercises described in section 3.2. These figures compare the distribution of estimated t-statistics from the placebo regressions with the actual t-statistic of marriage market exposure (parallel figures for coefficients instead of t-statistics are shown in Figure A14). The figures also report the resulting p-values, which indicate the proportion of placebo estimates that are more extreme than the actual estimate. Reassuringly, the estimated t-statistics of "fake" marriage market exposure are small and centered around zero for all variables. The p-values suggest that the effects on the probability of being in a couple and the number of children are significant at the 5% level, while the effect on childlessness is significant at the 10% level. As shown in Figure A14, inference based on the coefficients leads to very low p-values for all outcomes. Overall, the results from this exercise support the validity of the main findings.

## 7 Conclusion

This paper provides evidence on the effects of the female educational advantage on marriage and fertility outcomes. Exploiting changes in the gender gap in education in the marriage market induced by the Finnish comprehensive school reform, I show that in marriage markets with a larger female educational advantage there are declines in marriage and fertility rates. The size of these effects is substantial. A one standard deviation increase in marriage market exposure to the reform can explain 10% of the decline in marriage and cohabitation that took place in Finland during the period of study.

My findings suggest that an important driver of the effects is the increasing mismatch between the distributions of educational attainment of men and women resulting from the reform. As such, the effects are stronger for low-educated men and high-educated women, and larger in marriage markets where the reform would increase mismatch more. Overall, these results are consistent with the sociological hypothesis that changes in the economic roles of men and women have profound implications for family structure (Goldscheider et al., 2015), and with previous evidence showing that relative advances by women can generate frictions in marriage markets (Bertrand et al., 2015).

Finally, even though a welfare assessment is outside the scope of this paper, the results suggest that the changes in family structure affecting, in particular, low-educated men, might have had negative consequences in terms of their mental health. The question remains as to whether these effects would persist in younger cohorts, for whom the female advantage in education has increasingly become the norm.

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## Tables

Table 1: Cohorts exposed to the new school system by reform year of municipality

| | *Reform year* | | | | | |
|---|---|---|---|---|---|---|
| *Year of birth* | 1972 | 1973 | 1974 | 1975 | 1976 | 1977 |
| <=1960 | | | | | | |
| 1961 | X | | | | | |
| 1962 | X | X | | | | |
| 1963 | X | X | X | | | |
| 1964 | X | X | X | X | | |
| 1965 | X | X | X | X | X | |
| >=1966 | X | X | X | X | X | X |

Table 2: Reform impact on the gender gap in education

| | Post-secondary | | | University | | |
|---|---|---|---|---|---|---|
| | (1) Women | (2) Men | (3) Female adv. | (4) Women | (5) Men | (6) Female adv. |
| Reform | 0.012*** | 0.000 | 0.011** | 0.007*** | -0.000 | 0.008** |
| | (0.004) | (0.004) | (0.005) | (0.003) | (0.003) | (0.003) |
| Observations | 716537 | 743911 | 1460448 | 716537 | 743911 | 1460448 |
| Adjusted $R^2$ | 0.038 | 0.016 | 0.034 | 0.016 | 0.015 | 0.016 |
| Pre-reform mean | 0.39 | 0.30 | 0.10 | 0.14 | 0.15 | -0.01 |

Standard errors (in parentheses) are clustered at the municipality of birth level. This table shows estimates for the impact of direct exposure to the reform on the educational attainment of women and men, and on the female advantage in education (expressed as the interaction of female with own exposure). The first three columns have as dependent variable an indicator for more than secondary education, and the last three columns an indicator for university degree. The specification includes cohort and municipality of birth F.E., as well as region-specific linear trends. Own exposure takes value 1 for cohorts and municipalities affected by the reform. Pre-reform mean refers to average of the dependent variable in the sample of each column for cohorts born in 1956-1960. * p<0.1, ** p<0.05, *** p<0.01

Table 3: Marriage market exposure impact on family outcomes by age 40

| | Marriage | | Fertility | |
|---|---|---|---|---|
| | (1) Ever married | (2) Married/cohab | (3) Childless | (4) Num children |
| *Panel A: Full sample* | | | | |
| Marriage market exposure (sd) | -0.005 | -0.008** | 0.011*** | -0.025** |
| | (0.003) | (0.004) | (0.004) | (0.011) |
| Own exposure | 0.003 | 0.003 | -0.003 | 0.012* |
| | (0.002) | (0.002) | (0.002) | (0.006) |
| Observations | 1460448 | 1460448 | 1460448 | 1460448 |
| Adjusted $R^2$ | 0.015 | 0.009 | 0.018 | 0.021 |
| Pre-reform mean | 0.70 | 0.73 | 0.22 | 1.78 |
| *Panel B: Men* | | | | |
| Marriage market exposure (sd) | -0.005 | -0.010** | 0.007 | -0.034** |
| | (0.006) | (0.005) | (0.006) | (0.016) |
| Own exposure | 0.004 | 0.000 | -0.001 | 0.002 |
| | (0.004) | (0.003) | (0.003) | (0.010) |
| Observations | 743911 | 743911 | 743911 | 743911 |
| Adjusted $R^2$ | 0.011 | 0.008 | 0.006 | 0.011 |
| Pre-reform mean | 0.66 | 0.72 | 0.27 | 1.64 |
| *Panel C: Women* | | | | |
| Marriage market exposure (sd) | -0.004 | -0.004 | 0.013*** | -0.012 |
| | (0.004) | (0.005) | (0.003) | (0.010) |
| Own exposure | 0.002 | 0.005* | -0.005* | 0.022** |
| | (0.003) | (0.003) | (0.003) | (0.009) |
| Observations | 716537 | 716537 | 716537 | 716537 |
| Adjusted $R^2$ | 0.007 | 0.009 | 0.004 | 0.014 |
| Pre-reform mean | 0.74 | 0.74 | 0.17 | 1.92 |

Standard errors (in parentheses) are clustered at the municipality of birth level. This table shows the effect of higher marriage market exposure to the reform on marriage (the probability of having ever been married by age 40 and the probability of being either married or cohabiting at this age) and fertility outcomes (probability of not having had any children by age 40, and the total number of children by this age). The specification controls for gender and includes cohort and municipality of birth F.E., as well as region-specific linear trends, all of them interacted with gender. Marriage market exposure (in standard deviations) indicates the proportion of people in someone's marriage market affected by the reform. Own exposure takes value 1 for cohorts and municipalities affected by the reform. Panel A shows results for the whole sample, while Panels B and C show results separately for men and women, respectively. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 4: Heterogeneous effects of marriage market exposure by level of education

| | By own education | | | | By parental education | | | |
|---|---|---|---|---|---|---|---|---|
| | Men | | Women | | Men | | Women | |
| | Low | High | Low | High | Low | High | Low | High |
| Ever married | -0.005 | 0.004 | -0.000 | -0.013** | -0.006 | -0.007 | -0.000 | -0.026** |
| | (0.008) | (0.008) | (0.004) | (0.006) | (0.008) | (0.012) | (0.004) | (0.011) |
| Mean of Y | 0.61 | 0.77 | 0.73 | 0.78 | 0.65 | 0.74 | 0.74 | 0.75 |
| Married/cohabiting | -0.011* | -0.005 | 0.001 | -0.007 | -0.016*** | 0.006 | -0.002 | -0.015 |
| | (0.006) | (0.007) | (0.004) | (0.006) | (0.005) | (0.011) | (0.006) | (0.009) |
| Mean of Y | 0.68 | 0.81 | 0.74 | 0.78 | 0.72 | 0.76 | 0.75 | 0.73 |
| Childless | 0.013 | -0.011* | -0.001 | 0.004 | 0.010 | -0.002 | 0.011*** | 0.010 |
| | (0.008) | (0.006) | (0.003) | (0.006) | (0.007) | (0.014) | (0.004) | (0.008) |
| Mean of Y | 0.30 | 0.22 | 0.15 | 0.18 | 0.28 | 0.25 | 0.17 | 0.21 |
| Number of children | -0.044** | -0.005 | 0.014 | 0.002 | -0.039** | -0.037 | -0.007 | -0.022 |
| | (0.022) | (0.021) | (0.013) | (0.018) | (0.019) | (0.046) | (0.011) | (0.030) |
| Mean of Y | 1.58 | 1.79 | 2.01 | 1.85 | 1.64 | 1.69 | 1.93 | 1.85 |
| Observations | 528571 | 215340 | 329638 | 166352 | 621790 | 84002 | 579698 | 78763 |

Standard errors (in parentheses) are clustered at the municipality of birth level. This table shows the coefficients of marriage market exposure in separate regression where the dependent variable is the one indicated in each row. The first four columns show heterogenous results by own level of education, separately for men and women, where the sample of women is restricted to individuals not directly exposed to the reform. The last four columns show heterogenous results by parental level of education for the whole sample of men and women. Low level of education implies at most secondary education, whereas high level of education indicates more than secondary education. The specification includes cohort and municipality of birth F.E., as well as region-specific linear trends. * p<0.1, ** p<0.05, *** p<0.01

Table 5: Heterogeneous effects of marriage market exposure by predicted change in educational mismatch

| | Ever married | | Married/cohabit. | | Childless | | Num children | |
|---|---|---|---|---|---|---|---|---|
| | NI | I | NI | I | NI | I | NI | I |
| Marriage market | -0.004 | -0.033 | -0.008** | -0.066*** | 0.010** | 0.056*** | -0.024** | -0.412*** |
| exposure (sd) | (0.003) | (0.033) | (0.004) | (0.024) | (0.004) | (0.020) | (0.011) | (0.128) |
| Observations | 1268347 | 192101 | 1268347 | 192101 | 1268347 | 192101 | 1268347 | 192101 |
| Adjusted $R^2$ | 0.015 | 0.018 | 0.009 | 0.007 | 0.018 | 0.022 | 0.021 | 0.023 |
| Pre-reform mean | 0.703 | 0.700 | 0.731 | 0.738 | 0.223 | 0.226 | 1.785 | 1.747 |

Standard errors (in parentheses) are clustered at the municipality of birth level. This table shows the coefficients of marriage market exposure for the pooled sample separately for regions where the gender gap in university education was not predicted to increase in absolute terms (NI), and for those where it was predicted increase (I). In particular, for each outcome, the first column show results for regions where there was a male advantage in university education in pre-reform cohorts, while the second column show results for regions where there was no gender difference or already a female advantage in education prior to the reform. The specification includes the indicator for own exposure, as well as cohort and municipality of birth F.E. and region-specific linear trends, all of them interacted with gender. * p<0.1, ** p<0.05, *** p<0.01

Table 6: Marriage market exposure impact on assortative mating and divorce

| | Relative level of education | | | Age difference | Divorced by 40 | |
|---|---|---|---|---|---|---|
| | Equal | More | Less | with spouse | All | Married |
| Marriage market exposure (sd) | -0.008 | -0.004 | 0.007** | -0.135** | 0.002 | 0.005 |
| | (0.005) | (0.003) | (0.003) | (0.061) | (0.004) | (0.006) |
| Observations | 743911 | 743911 | 743911 | 574397 | 743911 | 496958 |
| Adjusted $R^2$ | 0.011 | 0.003 | 0.007 | 0.004 | 0.011 | 0.023 |
| Pre-reform mean | 0.453 | 0.077 | 0.127 | 1.743 | 0.142 | 0.210 |

Standard errors (in parentheses) are clustered at the municipality of birth level. The dependent variable in columns 1-3 is an indicator equal to 1 if the man's level of education is equal, higher, or lower than that of their spouse, respectively. The dependent variable in column 4 is the age difference between the husband and the wife, and in columns 5-6, an indicator equal to 1 if the man has divorced by age 40, for the full sample in column 5 and for those who ever got married by age 40 in column 6. The specification includes the indicator for own exposure, cohort and municipality of birth F.E., and region-specific linear trends. Marriage market exposure (in standard deviations) indicates the proportion of people in someone's marriage market affected by the reform. * p<0.1, ** p<0.05, *** p<0.01

Table 7: Marriage market exposure impact on men's health outcomes

| | | By education | |
|---|---|---|---|
| | (1) All | (2) Low | (3) High |
| Mental health | 0.007** | 0.011** | -0.001 |
| | (0.004) | (0.005) | (0.005) |
| Mean of Y | 0.08 | 0.09 | 0.04 |
| Alcoholic liver | -0.000 | -0.000 | -0.000 |
| | (0.001) | (0.001) | (0.001) |
| Mean of Y | 0.00 | 0.00 | 0.00 |
| Substance abuse | 0.000 | -0.000 | 0.001 |
| | (0.001) | (0.002) | (0.001) |
| Mean of Y | 0.01 | 0.01 | 0.00 |
| Observations | 329408 | 225024 | 104384 |

Standard errors (in parentheses) are clustered at the municipality of birth level. This table shows the coefficients of marriage market exposure in separate regressions where the dependent variable is the one indicated in each row. Results for all men in column (1), for those with low level of education (at most secondary) in (2), and for those with high level of education in (3). Mental health, alcoholic liver, and substance abuse are indicators equal to 1 if the person had any hospital visit with those groups of diagnoses between ages 40-45. The specification includes the indicator for own exposure, cohort and municipality of birth F.E., and region-specific linear trends. * p<0.1, ** p<0.05, *** p<0.01

Table 8: Marriage market exposure coefficient with alternative marriage market definitions

| | (1) Baseline | (2) Region & 0-3 years | (3) Age dist. & freq. marriage | (4) Age dist. & inv. distance |
|---|---|---|---|---|
| Ever married | -0.005 | -0.002 | -0.002 | -0.010* |
| | (0.003) | (0.002) | (0.006) | (0.005) |
| Married/cohabiting | -0.008** | -0.004** | -0.007 | -0.012*** |
| | (0.004) | (0.002) | (0.005) | (0.005) |
| Childless | 0.011*** | 0.006*** | 0.005 | 0.018*** |
| | (0.004) | (0.002) | (0.007) | (0.005) |
| Number of children | -0.025** | -0.012** | -0.017 | -0.025* |
| | (0.011) | (0.005) | (0.017) | (0.014) |
| Observations | 1460448 | 1460448 | 1460448 | 1460448 |

Standard errors (in parentheses) are clustered at the municipality of birth level. This table shows the coefficients of marriage market exposure in separate regression where the dependent variable is the one indicated in each row. Different columns use different definitions of the marriage market, as indicated by column titles. All specifications include the indicator for own exposure as well as cohort and municipality of birth F.E. and region-specific linear trends, all of them interacted with gender. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 9: Robustness of marriage market exposure impact on family outcomes

| | (1) Baseline | (2) Municipality trends | (3) Region pre-trends | (4) Region x Cohort FE | (5) W/o Helsinki | (6) W/o outliers | (7) Based on 1970 residence | (8) Regional unemployment |
|---|---|---|---|---|---|---|---|---|
| Ever married | -0.005 | -0.006 | -0.001 | -0.025*** | -0.003 | -0.005 | -0.005 | -0.005 |
| RW p-value=0.050 | (0.003) | (0.004) | (0.003) | (0.002) | (0.004) | (0.004) | (0.004) | (0.003) |
| Married/cohabiting | -0.008** | -0.008** | -0.002 | -0.007*** | -0.004 | -0.008* | -0.011*** | -0.008** |
| RW p-value=0.020 | (0.004) | (0.004) | (0.003) | (0.003) | (0.004) | (0.002) | (0.003) | (0.004) |
| Childless | 0.011*** | 0.011*** | 0.009** | 0.015*** | 0.007** | 0.011** | 0.015*** | 0.010*** |
| RW p-value=0.010 | (0.004) | (0.004) | (0.004) | (0.001) | (0.003) | (0.004) | (0.004) | (0.004) |
| Number of children | -0.025** | -0.022** | -0.016* | -0.078*** | -0.016 | -0.023** | -0.022* | -0.025** |
| RW p-value=0.010 | (0.011) | (0.011) | (0.009) | (0.004) | (0.011) | (0.011) | (0.012) | (0.011) |
| Observations | 1460448 | 1460448 | 1460448 | 1460448 | 1320040 | 1395839 | 1420992 | 1460448 |

Standard errors (in parentheses) are clustered at the municipality of birth level, and boostrapped in column (3). This table shows the coefficients of marriage market exposure in separate regressions where the dependent variable is the one indicated in each row. RW p-value refers to the Romano-Wolf stepdown adjusted p-value to correct for multiple hypothesis testing in the baseline specification. Column (1) presents results from the baseline specification; column (2) includes (gender-specific) municipality linear trends, instead of region trends; column (3) instead de-trends the dependent variable of gender and region-specific pre-trends; column (4) substitutes gender- and region-specific linear trends with interactions between region and birth year indicators, along with separate birth year fixed effects (to capture time patterns in the omitted region), municipality fixed effects, and controls for gender; column (5) excludes observations from Helsinki and column (6) from "outlier" municipalities; column (7) uses an alternative measure of exposure to the reform based on the municipality of residence in 1970 instead of the place of birth, and column (8) controls for the regional unemployment rate in the year the individuals are 40 years old. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 10: Robustness of marriage market exposure impact on family outcomes

| | (1) Baseline | (2) Years of exposure (linear) | (3) Years of exposure | (4) Dynamic | (5) Cohort-specific |
|---|---|---|---|---|---|
| *Panel A: Full sample* | | | | | |
| Ever married by 40 | -0.005 | -0.005 | -0.005 | -0.007* | -0.004 |
| | (0.003) | (0.003) | (0.003) | (0.004) | (0.003) |
| Married/cohabiting at 40 | -0.008** | -0.008** | -0.008* | -0.006* | -0.008* |
| | (0.004) | (0.004) | (0.004) | (0.003) | (0.004) |
| Childless | 0.011*** | 0.012*** | 0.012*** | 0.009*** | 0.011*** |
| | (0.004) | (0.004) | (0.004) | (0.003) | (0.004) |
| Number of children | -0.025** | -0.025** | -0.023** | -0.027** | -0.024** |
| | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) |
| Observations | 1460448 | 1460448 | 1460448 | 1460448 | 1460448 |
| *Panel B: Men* | | | | | |
| Ever married by 40 | -0.005 | -0.010 | -0.009 | -0.012 | -0.004 |
| | (0.006) | (0.007) | (0.007) | (0.010) | (0.006) |
| Married/cohabiting at 40 | -0.010** | -0.015** | -0.015** | -0.012 | -0.011** |
| | (0.005) | (0.006) | (0.007) | (0.007) | (0.005) |
| Childless | 0.007 | 0.016** | 0.016** | 0.010 | 0.012** |
| | (0.006) | (0.008) | (0.008) | (0.008) | (0.006) |
| Number of children | -0.034** | -0.039* | -0.033 | -0.040* | -0.041** |
| | (0.016) | (0.020) | (0.022) | (0.024) | (0.017) |
| Observations | 743911 | 743911 | 743911 | 743911 | 743911 |
| *Panel C: Women* | | | | | |
| Ever married by 40 | -0.004 | -0.003 | -0.003 | -0.012** | -0.001 |
| | (0.004) | (0.004) | (0.005) | (0.006) | (0.004) |
| Married/cohabiting at 40 | -0.004 | -0.004 | -0.005 | -0.003 | -0.003 |
| | (0.005) | (0.006) | (0.006) | (0.006) | (0.005) |
| Childless | 0.013*** | 0.010** | 0.011** | 0.008* | 0.011*** |
| | (0.003) | (0.004) | (0.004) | (0.005) | (0.004) |
| Number of children | -0.012 | -0.012 | -0.015 | -0.010 | -0.014 |
| | (0.010) | (0.012) | (0.013) | (0.016) | (0.011) |
| Observations | 716537 | 716537 | 716537 | 716537 | 716537 |

Standard errors (in parentheses) are clustered at the municipality of birth level. This table shows the coefficients of marriage market exposure in separate regression where the dependent variable is the one indicated in each row, for the pooled sample (panel A), for men (panel B) and for women (panel C). Column (1) presents results from the baseline specification; column (2) controls linearly for years of exposure to the new school system; column (3) controls non-parametrically for years of exposure; column (4) substitutes the control for Own exposure with dummies indicating the number of cohorts to the first exposure to the reform in each municipality, and column (5) allows for heterogenous effects of own exposure by year of implementation of the reform. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

## Figures

Figure 1: Year of adoption of the reform by municipality

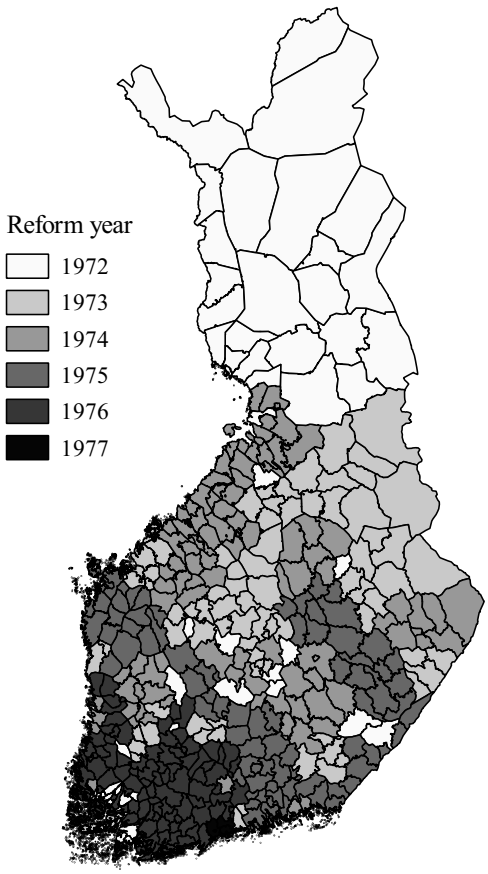
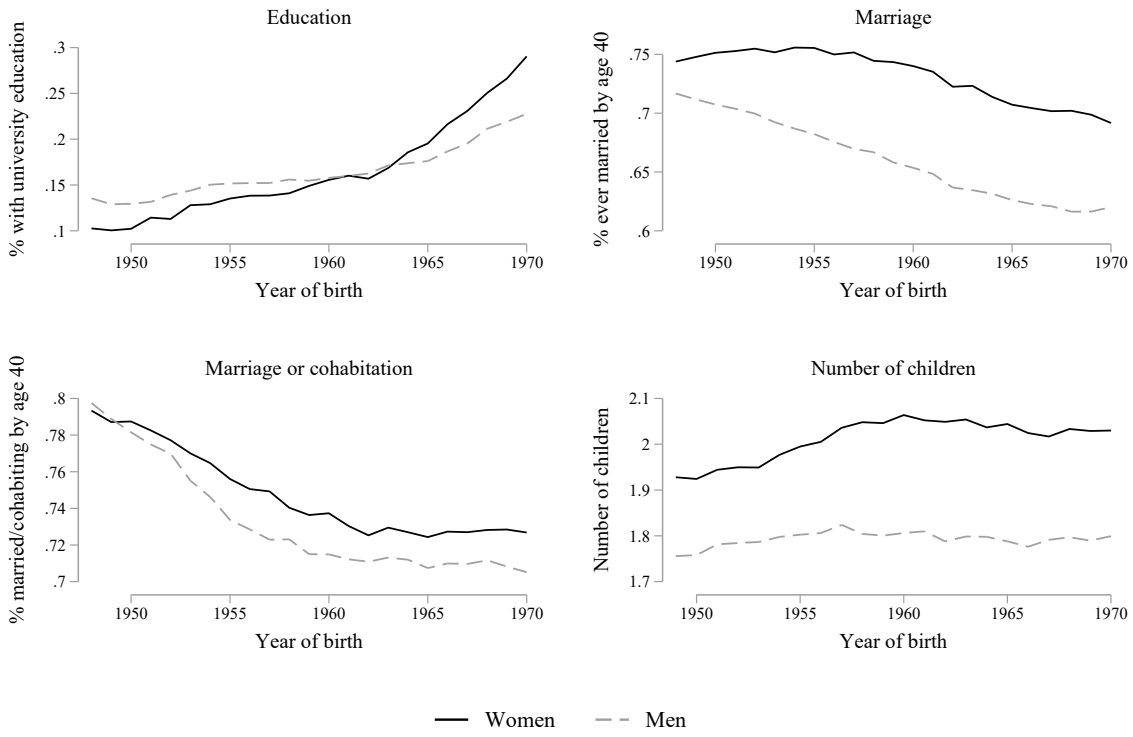
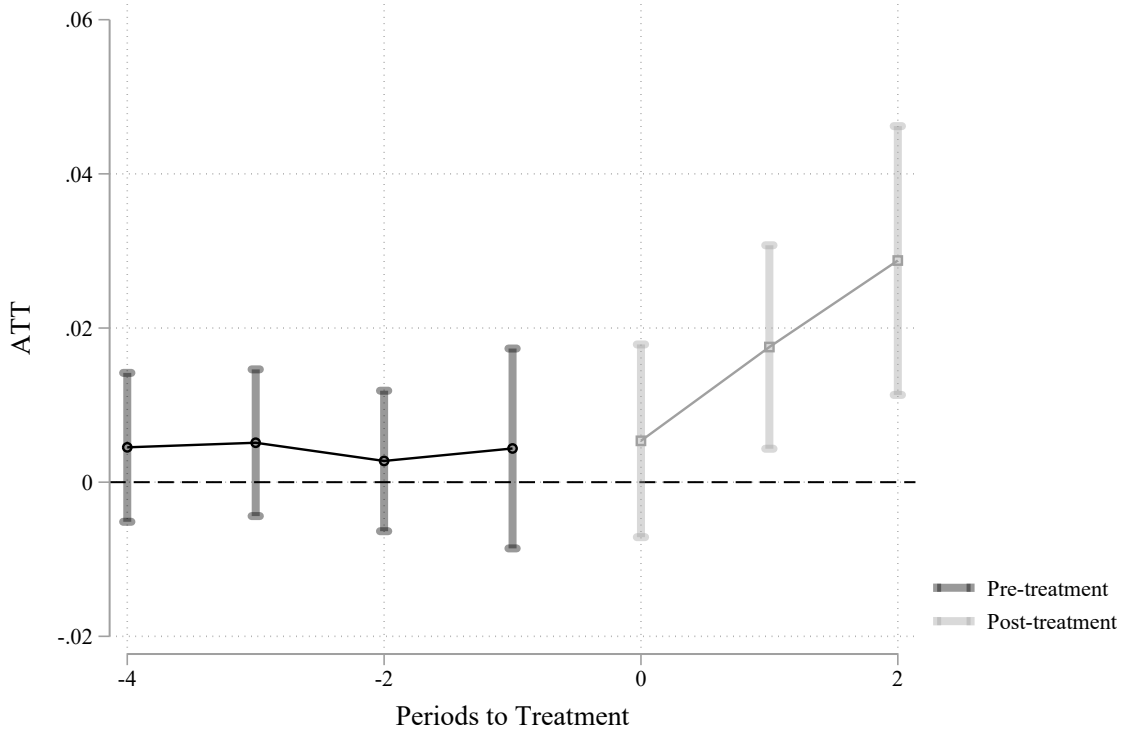


Figure 2: Aggregate education and family trends in Finland




*Notes:* This figure plots the percentage of men and women with university education, the percentage of men and women who were ever married by age 40, the percentage of men and women who are either married or cohabiting at age 40, and the average number of biological children in Finland by year of birth.

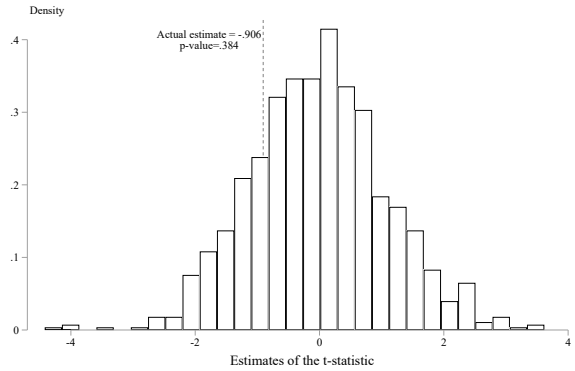
Figure 3: Female high education by cohort relative to first exposure to the reform



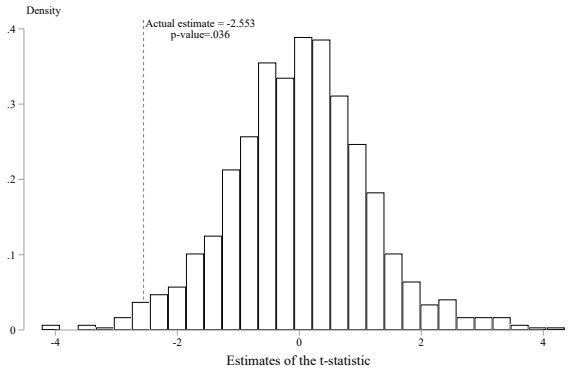

*Notes:* This figures plots the coefficients and 95% confidence intervals of the event study estimates of the probability of having more than secondary education for females with respect to the cohort first exposed to the reform in each municipality, estimated using Callaway and Sant'Anna (2021)'s estimator. The sample is restricted to make it balanced in event time, and only includes municipalities that implemented the reform from 1972 to 1974, for which effects two years after first adoption can be estimated.

Figure 4: Randomization inference results – t-statistics

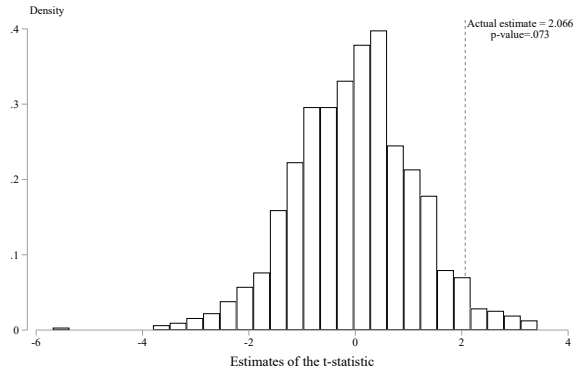
(a) Ever married



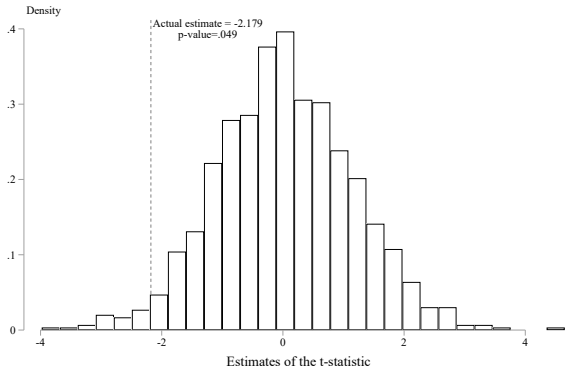

(b) Married or cohabiting




(c) Childless




(d) Number of children




*Notes:* This figure plots the results of the randomization inference exercise conducted with `ritest` (Heß, 2017). The different panels show the distribution of estimated t-statistics of marriage market exposure across 1,000 permutations for the probability of having ever been married by age 40, for the probability of being in a couple at age 40, for the probability of having no children and for the number of children at that age. The dashed line in each panel represents the actual t-statistic, and the p-value is the fraction of placebo estimates that are more extreme than the actual estimate. See section 3.2 for more details. P-values in panels (a)–(d): 0.384,0.036, 0.073, 0.049.

# Appendix

Table A1: Years of exposure to new curriculum by year of birth and reform year of municipality

| | *Reform year* | | | | | |
|---|---|---|---|---|---|---|
| *Year of birth* | 1972 | 1973 | 1974 | 1975 | 1976 | 1977 |
| <=1960 | | | | | | |
| 1961 | 5 | | | | | |
| 1962 | 6 | 5 | | | | |
| 1963 | 7 | 6 | 5 | | | |
| 1964 | 8 | 7 | 6 | 5 | | |
| 1965 | 9 | 8 | 7 | 6 | 5 | |
| 1966 | 9 | 9 | 8 | 7 | 6 | 5 |

Table A2: "Outlier" municipalities' education levels in pre-reform cohorts

| | Post-secondary | | University | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Outlier | 0.002 | -0.001 | 0.006* | 0.004 |
| | (0.005) | (0.004) | (0.003) | (0.003) |
| Observations | 430 | 430 | 430 | 430 |
| Adjusted $R^2$ | -0.002 | 0.177 | 0.007 | 0.170 |
| Region F.E. | No | Yes | No | Yes |

Robust standard errors in parentheses. This table compares the education level of pre-reform cohorts (1956-1960) in "outlier" municipalities and the rest. The dependent variable is the proportion of people in the municipality with more than secondary education in columns 1-2, and the proportion of people with university education in columns 3-4. Outlier is an indicator equal to 1 if the municipality implemented the reform in a different year than most municipalities in the same region. * p<0.1, ** p<0.05, *** p<0.01

Table A3: Goodman-Bacon decomposition results

| DD Comparison | Weight | Avg DD estimate |
|---|---|---|
| Earlier T vs Later C | 0.842 | 0.017 |
| Later T vs Earlier C | 0.158 | 0.016 |
| Diff-in-diff estimate: | | 0.017 |

T=Treatment, C=Control. This table shows the results from the Goodman-Bacon decomposition of the difference-in-differences estimate of the effect of own exposure to the reform on female high education. The decomposition was performed using the `bacondecomp` Stata package (Goodman-Bacon et al., 2019) on data collapsed at the municipality-gender-cohort level.

Table A4: Reform impact on definition of marriage market

| | Age gap within couple | | Spouse from same region | |
|---|---|---|---|---|
| | (1) Women | (2) Men | (3) Women | (4) Men |
| Reform | -0.002 | -0.049 | -0.001 | -0.001 |
| | (0.033) | (0.031) | (0.003) | (0.003) |
| Observations | 587401 | 574397 | 716537 | 743911 |
| Adjusted $R^2$ | 0.005 | 0.004 | 0.021 | 0.019 |
| Pre-reform mean | 2.38 | 1.74 | 0.40 | 0.36 |

Standard errors (in parentheses) are clustered at the municipality of birth level. This table shows estimates for the impact of direct exposure to the reform on the age gap within couples and on the probability of having a spouse born in the same region as oneself, separately for women and men. The specification includes cohort and municipality of birth F.E., as well as region-specific linear trends. Own exposure takes value 1 for cohorts and municipalities affected by the reform. Pre-reform mean refers to average of the dependent variable in the sample of each column for cohorts born in 1956-1960. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A5: Descriptive statistics

| | Full sample | | Men | | Women | |
|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD |
| *Education* | | | | | | |
| Post-secondary education | 0.327 | 0.469 | 0.289 | 0.454 | 0.366 | 0.482 |
| University education | 0.147 | 0.354 | 0.152 | 0.359 | 0.141 | 0.348 |
| Parents with post-secondary education | 0.119 | 0.324 | 0.119 | 0.324 | 0.120 | 0.325 |
| *Family outcomes* | | | | | | |
| Ever married by 40 | 0.702 | 0.457 | 0.668 | 0.471 | 0.738 | 0.440 |
| Married/cohabiting at 40 | 0.748 | 0.434 | 0.741 | 0.438 | 0.755 | 0.430 |
| Childless | 0.218 | 0.413 | 0.264 | 0.441 | 0.170 | 0.375 |
| Number of children | 1.759 | 1.337 | 1.638 | 1.346 | 1.884 | 1.316 |
| Childless at 45 | 0.206 | 0.405 | 0.247 | 0.431 | 0.163 | 0.370 |
| Number of children at 45 | 1.824 | 1.387 | 1.732 | 1.413 | 1.920 | 1.354 |
| Childless at 50 | 0.204 | 0.403 | 0.242 | 0.428 | 0.163 | 0.370 |
| Number of children at 50 | 1.840 | 1.402 | 1.761 | 1.438 | 1.922 | 1.358 |
| Same level of education as spouse | 0.493 | 0.500 | 0.470 | 0.499 | 0.517 | 0.500 |
| Higher level of education than spouse | 0.101 | 0.301 | 0.080 | 0.272 | 0.122 | 0.327 |
| Lower level of education than spouse | 0.101 | 0.301 | 0.114 | 0.318 | 0.087 | 0.281 |
| Age gap within couple | 1.978 | 3.992 | 1.734 | 3.909 | 2.217 | 4.058 |
| Divorced | 0.137 | 0.344 | 0.124 | 0.330 | 0.149 | 0.357 |
| Spouse from same region | 0.371 | 0.483 | 0.357 | 0.479 | 0.386 | 0.487 |
| Age at first marriage | 27.708 | 7.904 | 28.917 | 7.917 | 26.540 | 7.712 |
| Age at first birth | 26.815 | 5.491 | 28.126 | 5.541 | 25.581 | 5.147 |
| Any birth before marriage | 0.361 | 0.480 | 0.336 | 0.472 | 0.386 | 0.487 |
| *Health outcomes* | | | | | | |
| Mental health | 0.078 | 0.268 | 0.078 | 0.269 | 0.077 | 0.267 |
| Alcoholic liver | 0.003 | 0.055 | 0.004 | 0.060 | 0.002 | 0.048 |
| Substance abuse | 0.010 | 0.097 | 0.009 | 0.096 | 0.010 | 0.098 |
| *Labor market outcomes* (*age 30*) | | | | | | |
| Annual labor earnings (1985 prices) | 22678.005 | 14228.727 | 26371.415 | 15239.533 | 18723.212 | 11845.488 |
| Working | 0.754 | 0.431 | 0.790 | 0.407 | 0.717 | 0.451 |
| *Reform variables* | | | | | | |
| Own exposure | 0.161 | 0.368 | 0.161 | 0.367 | 0.162 | 0.368 |
| Marriage market exposure (%) | 17.341 | 26.054 | 24.846 | 30.710 | 9.549 | 16.889 |
| Observations | 1460448 | | 743911 | | 716537 | |

Table A6: Reform impact on the gender gap in education – 1970 residence

| | Post-secondary | | | University | | |
|---|---|---|---|---|---|---|
| | (1) Women | (2) Men | (3) Female adv. | (4) Women | (5) Men | (6) Female adv. |
| Reform | 0.010** | -0.001 | 0.010** | 0.007** | 0.001 | 0.007* |
| | (0.004) | (0.004) | (0.005) | (0.003) | (0.003) | (0.003) |
| Observations | 696624 | 724372 | 1420992 | 696624 | 724372 | 1420992 |
| Adjusted $R^2$ | 0.034 | 0.015 | 0.031 | 0.013 | 0.013 | 0.013 |
| Pre-reform mean | 0.39 | 0.30 | 0.10 | 0.14 | 0.15 | -0.01 |

Standard errors (in parentheses) are clustered at the municipality of residence in 1970 level. This table shows estimates for the impact of direct exposure to the reform on the educational attainment of women and men, and on the female advantage in education (expressed as the interaction of female with own exposure), with exposure defined based on municipality of residence in 1970. The first three columns have as dependent variable an indicator for more than secondary education, and the last three columns an indicator for university degree. The specification includes cohort and municipality of residence in 1970 F.E., as well as region-specific linear trends. Own exposure takes value 1 for cohorts and municipalities affected by the reform. Pre-reform mean refers to average of the dependent variable in the sample of each column for cohorts born in 1956-1960. * p<0.1, ** p<0.05, *** p<0.01

Table A7: Impact of the reform on education: Callaway and Sant'Anna (2021) estimator

| | (1) Women | (2) Men |
|---|---|---|
| Own exposure | 0.015** | 0.007 |
| | (0.007) | (0.006) |
| Observations | 716537 | 743911 |

This table shows estimates of the impact of direct exposure to the reform on the probability of having more than secondary education separately for women and men, using Callaway and Sant'Anna (2021)'s estimator. The estimation was performed using Sant'Anna and Zhao (2020) doubly robust DiD estimator based on stabilized inverse probability weighting and ordinary least squares, with not-yet-treated units as controls. Own exposure refers to the weighted average of all group-time average treatment effects, obtained with the simple aggregation. Bootstrapped standard errors (in parentheses) are clustered at the municipality of birth level. * p<0.1, ** p<0.05, *** p<0.01

Table A8: Impact of the reform on labor market outcomes at age 30

| | Earnings | | | Working | | |
|---|---|---|---|---|---|---|
| | (1) Women | (2) Men | (3) Female adv. | (4) Women | (5) Men | (6) Female adv. |
| Reform | 78.260 | -11.789 | 90.048 | -0.001 | 0.004 | -0.005 |
| | (96.807) | (127.915) | (158.645) | (0.003) | (0.003) | (0.004) |
| Observations | 263500 | 282148 | 545648 | 317145 | 327439 | 644584 |
| Adjusted $R^2$ | 0.035 | 0.049 | 0.113 | 0.026 | 0.039 | 0.039 |
| Pre-reform mean | 16500.98 | 24463.80 | -7962.82 | 0.80 | 0.89 | -0.09 |

Standard errors (in parentheses) are clustered at the municipality of birth level. This table shows the effect of own exposure to the reform on annual labor earnings at age 30 (columns 1-3) deflated to 1985 prices using the CPI, and on the probability of being employed at this age (columns 4-6), for men and women and the gender gap (expressed as the interaction of female with own exposure). The specification includes cohort and municipality of birth F.E, as well as region-specific linear trends. Own exposure takes value 1 for cohorts and municipalities affected by the reform. Pre-reform mean refers to average of the dependent variable in the sample of each column for cohorts born in 1956-1960. * p<0.1, ** p<0.05, *** p<0.01

Table A9: Marriage market exposure impact on fertility at later ages

| | Age 45 | | Age 50 | |
|---|---|---|---|---|
| | (1) Childless | (2) Num children | (3) Childless | (4) Num children |
| Marriage market exposure (sd) | 0.011*** | -0.025** | 0.011*** | -0.025** |
| | (0.004) | (0.011) | (0.004) | (0.012) |
| Observations | 1460448 | 1460448 | 1460448 | 1460448 |
| Adjusted $R^2$ | 0.015 | 0.018 | 0.014 | 0.016 |
| Pre-reform mean | 0.21 | 1.85 | 0.21 | 1.86 |

Standard errors (in parentheses) are clustered at the municipality of birth level. This table shows the effect of higher marriage market exposure to the reform on the probability of not having had any children and on the total number of children by ages 45 and 50. The specification controls for gender and own exposure to the reform, and includes cohort and municipality of birth F.E., as well as region-specific linear trends, all of them interacted with gender. Marriage market exposure (in standard deviations) indicates the proportion of people in someone's marriage market affected by the reform. * p<0.1, ** p<0.05, *** p<0.01

Table A10: Family outcomes by age 40: own and marriage market exposure coefficients

| | Full sample | | | Men | | | Women | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| *Ever married by 40* | | | | | | | | | |
| Own exposure | 0.002 | | 0.003 | 0.004 | | 0.004 | 0.001 | | 0.002 |
| | (0.002) | | (0.002) | (0.004) | | (0.004) | (0.003) | | (0.003) |
| Marriage market exposure (sd) | | -0.003 | -0.005 | | -0.003 | -0.005 | | -0.003 | -0.004 |
| | | (0.003) | (0.003) | | (0.006) | (0.006) | | (0.004) | (0.004) |
| Observations | 1460448 | 1460448 | 1460448 | 743911 | 743911 | 743911 | 716537 | 716537 | 716537 |
| Adjusted $R^2$ | 0.015 | 0.015 | 0.015 | 0.011 | 0.011 | 0.011 | 0.007 | 0.007 | 0.007 |
| Pre-reform mean | 0.70 | 0.70 | 0.70 | 0.66 | 0.66 | 0.66 | 0.74 | 0.74 | 0.74 |
| *Married/cohabiting at 40* | | | | | | | | | |
| Own exposure | 0.001 | | 0.003 | -0.001 | | 0.000 | 0.004 | | 0.005* |
| | (0.003) | | (0.002) | (0.003) | | (0.003) | (0.003) | | (0.003) |
| Marriage market exposure (sd) | | -0.007 | -0.008** | | -0.010** | -0.010** | | -0.003 | -0.004 |
| | | (0.004) | (0.004) | | (0.004) | (0.005) | | (0.005) | (0.005) |
| Observations | 1460448 | 1460448 | 1460448 | 743911 | 743911 | 743911 | 716537 | 716537 | 716537 |
| Adjusted $R^2$ | 0.009 | 0.009 | 0.009 | 0.008 | 0.008 | 0.008 | 0.009 | 0.009 | 0.009 |
| Pre-reform mean | 0.73 | 0.73 | 0.73 | 0.72 | 0.72 | 0.72 | 0.74 | 0.74 | 0.74 |
| *Childless* | | | | | | | | | |
| Own exposure | -0.000 | | -0.003 | 0.000 | | -0.001 | -0.000 | | -0.005* |
| | (0.003) | | (0.002) | (0.003) | | (0.003) | (0.003) | | (0.003) |
| Marriage market exposure (sd) | | 0.010** | 0.011*** | | 0.006 | 0.007 | | 0.011*** | 0.013*** |
| | | (0.004) | (0.004) | | (0.006) | (0.006) | | (0.003) | (0.003) |
| Observations | 1460448 | 1460448 | 1460448 | 743911 | 743911 | 743911 | 716537 | 716537 | 716537 |
| Adjusted $R^2$ | 0.018 | 0.018 | 0.018 | 0.006 | 0.006 | 0.006 | 0.004 | 0.004 | 0.004 |
| Pre-reform mean | 0.22 | 0.22 | 0.22 | 0.27 | 0.27 | 0.27 | 0.17 | 0.17 | 0.17 |
| *Number of children* | | | | | | | | | |
| Own exposure | 0.007 | | 0.012* | -0.004 | | 0.002 | 0.018* | | 0.022** |
| | (0.007) | | (0.006) | (0.009) | | (0.010) | (0.009) | | (0.009) |
| Marriage market exposure (sd) | | -0.019* | -0.025** | | -0.033** | -0.034** | | -0.003 | -0.012 |
| | | (0.010) | (0.011) | | (0.015) | (0.016) | | (0.009) | (0.010) |
| Observations | 1460448 | 1460448 | 1460448 | 743911 | 743911 | 743911 | 716537 | 716537 | 716537 |
| Adjusted $R^2$ | 0.021 | 0.021 | 0.021 | 0.011 | 0.011 | 0.011 | 0.014 | 0.014 | 0.014 |
| Pre-reform mean | 1.78 | 1.78 | 1.78 | 1.64 | 1.64 | 1.64 | 1.92 | 1.92 | 1.92 |

Standard errors (in parentheses) are clustered at the municipality of birth level. This table shows the effect of own exposure and marriage market exposure to the reform on marriage (the probability of having ever been married by age 40 and the probability of being either married or cohabiting at this age) and fertility outcomes (probability of not having had any children by age 40, and the total number of children by this age). The specification controls for gender and includes cohort and municipality of birth F.E., as well as region-specific linear trends, all of them interacted with gender. Marriage market exposure (in standard deviations) indicates the proportion of people in someone's marriage market affected by the reform. Own exposure takes value 1 for cohorts and municipalities affected by the reform. * p<0.1, ** p<0.05, *** p<0.01

Table A11: Heterogeneous effects of marriage market exposure on divorce by level of education

| | Men | | Women | |
|---|---|---|---|---|
| | Low | High | Low | High |
| **By own education** | | | | |
| Marriage market exposure (sd) | -0.002 | 0.009 | -0.004 | 0.004 |
| | (0.005) | (0.007) | (0.004) | (0.004) |
| Observations | 528571 | 215340 | 329638 | 166352 |
| Mean of Y | 0.15 | 0.12 | 0.18 | 0.14 |
| **By parental education** | | | | |
| Marriage market exposure (sd) | 0.001 | 0.008 | -0.006 | -0.016* |
| | (0.005) | (0.009) | (0.004) | (0.008) |
| Observations | 621790 | 84002 | 579698 | 78763 |
| Mean of Y | 0.14 | 0.15 | 0.17 | 0.16 |

Standard errors (in parentheses) are clustered at the municipality of birth level. This table shows the coefficients of marriage market exposure on the probability of being divorced by age 40. The first panel shows heterogeneous results by own level of education, separately for men and women, where the sample of women is restricted to individuals not directly exposed to the reform. The second panel shows heterogeneous results by parental level of education for the whole sample of men and women. Low level of education implies at most secondary education, whereas high level of education indicates more than secondary education. The specification includes cohort and municipality of birth F.E., as well as region-specific linear trends. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A12: Marriage market exposure impact on other family outcomes by age 40

| | (1) Age first marriage | (2) Age first birth | (3) Any nonmarital birth |
|---|---|---|---|
| *Men* | | | |
| Marriage market exposure (sd) | -0.082 | -0.059 | -0.002 |
| | (0.094) | (0.084) | (0.006) |
| Own exposure | 0.088 | 0.061 | -0.004 |
| | (0.068) | (0.044) | (0.003) |
| Observations | 543207 | 564696 | 743911 |
| Adjusted $R^2$ | 0.022 | 0.038 | 0.005 |
| Pre-reform mean | 29.20 | 28.50 | 0.33 |
| *Women* | | | |
| Marriage market exposure (sd) | 0.211*** | 0.356*** | -0.025*** |
| | (0.071) | (0.081) | (0.004) |
| Own exposure | 0.007 | -0.050 | 0.003 |
| | (0.061) | (0.041) | (0.003) |
| Observations | 561516 | 599619 | 716537 |
| Adjusted $R^2$ | 0.019 | 0.043 | 0.006 |
| Pre-reform mean | 26.62 | 25.70 | 0.39 |

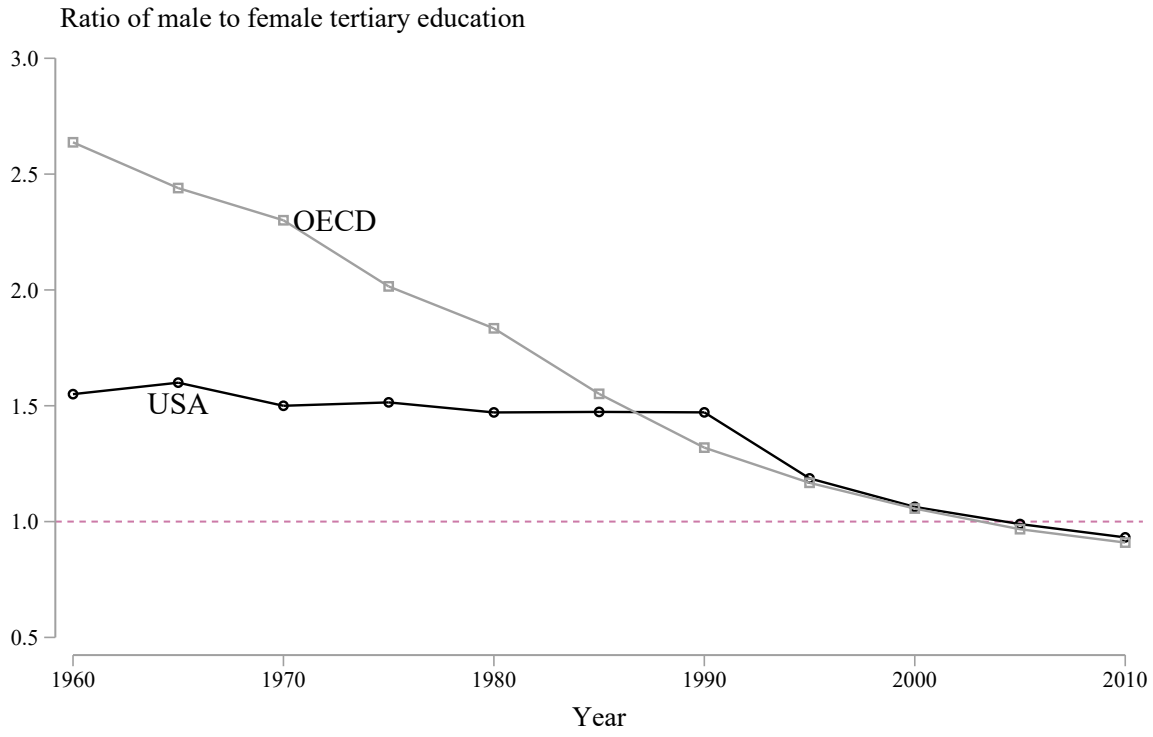
Standard errors (in parentheses) are clustered at the municipality of birth level. This table shows the effect of higher marriage market exposure to the reform on other marriage outcomes by age 40. The specification includes cohort and municipality of birth F.E., as well as region-specific linear trends. Marriage market exposure (in standard deviations) indicates the proportion of people in someone's marriage market affected by the reform. Own exposure takes value 1 for cohorts and municipalities affected by the reform. The top and bottom panels show results separately for men and women, respectively. * p<0.1, ** p<0.05, *** p<0.01

Table A13: Marriage market exposure impact on women's health outcomes

| | | By education | |
|---|---|---|---|
| | (1) All | (2) Low | (3) High |
| Mental health | -0.004 | -0.009 | 0.002 |
| | (0.009) | (0.012) | (0.011) |
| Mean of Y | 0.07 | 0.08 | 0.05 |
| Alcoholic liver | -0.000 | -0.000 | 0.000 |
| | (0.001) | (0.002) | (0.002) |
| Mean of Y | 0.00 | 0.00 | 0.00 |
| Substance abuse | 0.003 | 0.005 | -0.001 |
| | (0.003) | (0.004) | (0.003) |
| Mean of Y | 0.01 | 0.01 | 0.00 |
| Observations | 154547 | 89037 | 65510 |

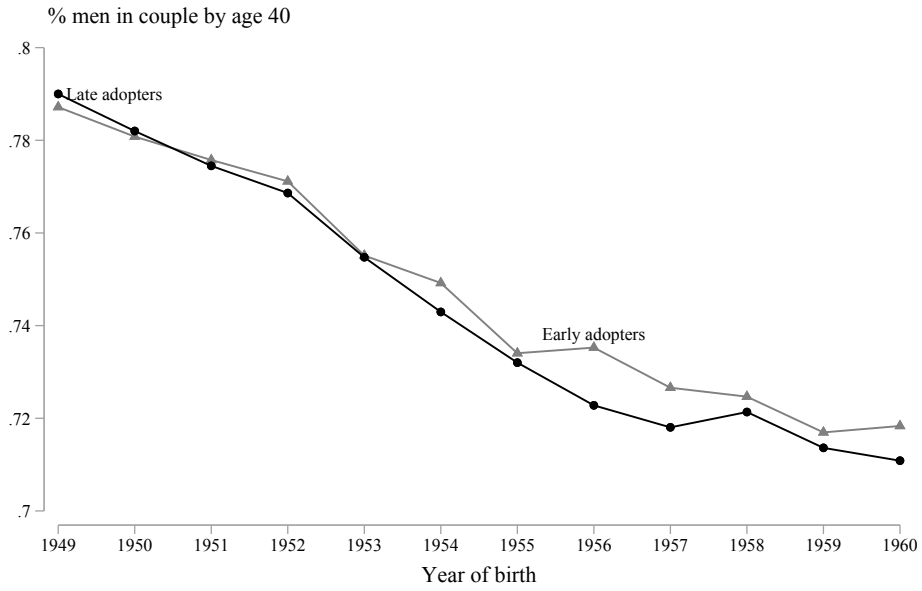
Standard errors (in parentheses) are clustered at the municipality of birth level. This table shows the coefficients of marriage market exposure in separate regressions where the dependent variable is the one indicated in each row. Results for all women in column (1), for those with low level of education (at most secondary) in (2), and for those with high level of education in (3), for those not directly affected by the reform. Mental health, alcoholic liver, and substance abuse are indicators equal to 1 if the person had any hospital visit with those groups of diagnoses between ages 40-45. The specification includes the indicator for own exposure, cohort and municipality of birth F.E., and region-specific linear trends. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Figure A1: Ratio of percentage of men to percentage of women (ages 20-64) with tertiary education in the US and on average in the OECD

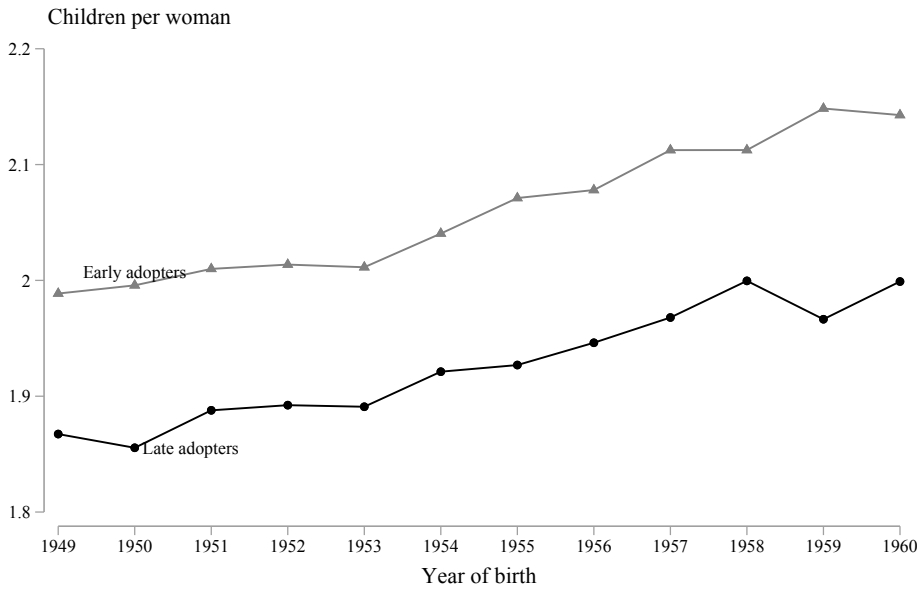



*Notes:* This figure shows the evolution of the ratio of the percentage of men to the percentage of women with tertiary education among the population aged 20-64 in the US (black line) and on average for OECD countries (gray line). Data from Barro and Lee (2013).

Figure A2: Trends in family outcomes in pre-reform cohorts – early vs. late reform municipalities



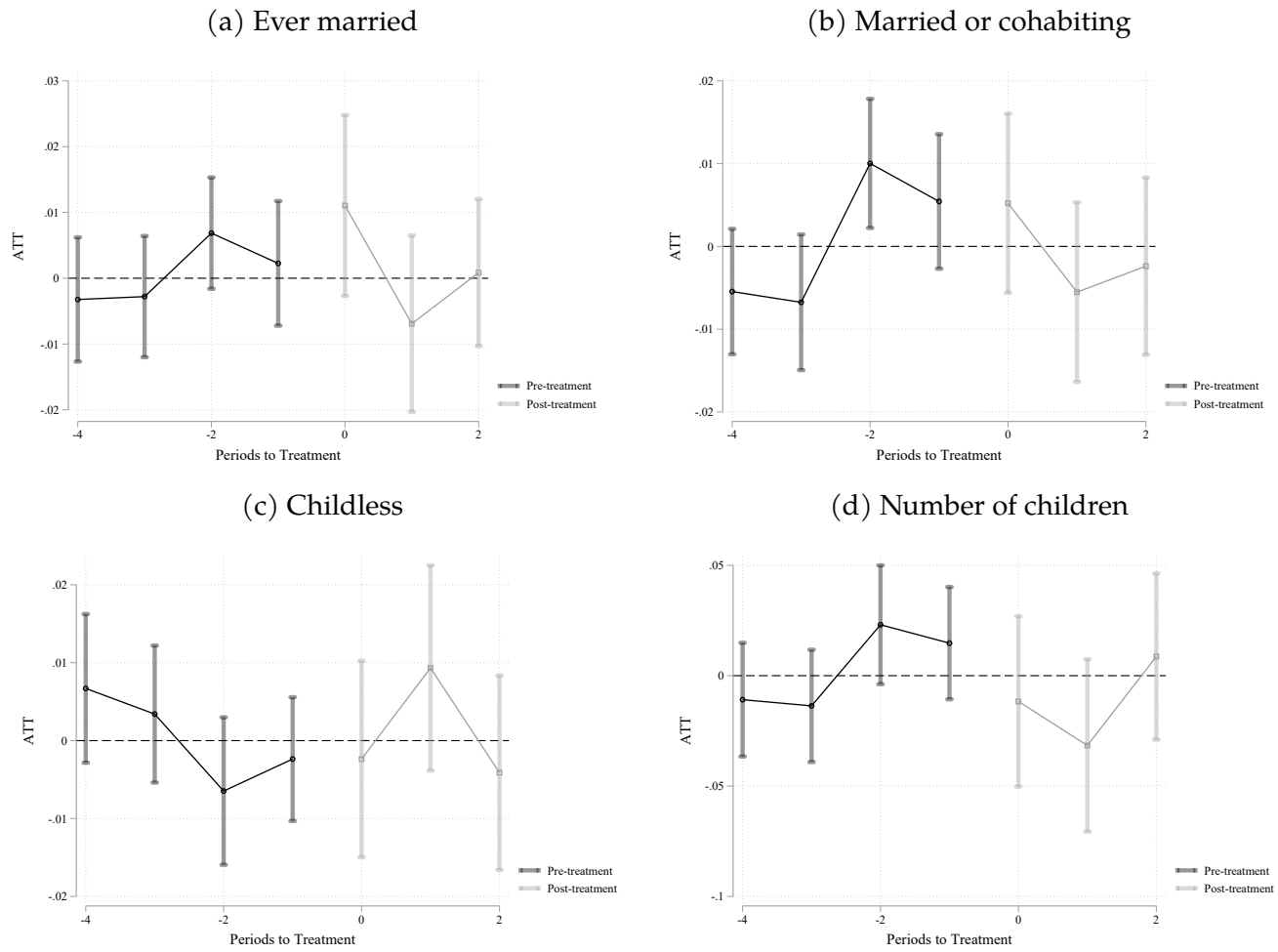

(a) Percentage of men married or cohabiting by age 40




(b) Average number of children per woman

*Notes:* This figure presents the evolution of trends in fertility and marriage outcomes in early-adopter municipalities (those that implemented the reform in 1972-1974) and in late-adopter municipalities (those that implemented it in 1975-1977). Panel (a) shows the the percentage of men who were married or cohabiting by age 40 by cohort, and panel (b) shows the average number of children per woman by cohort.

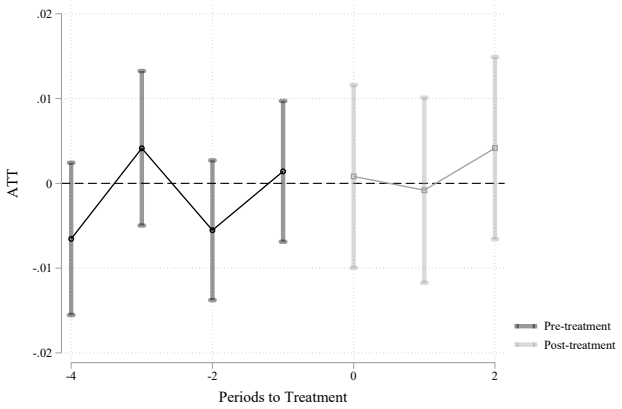
Figure A3: Event studies for men's family outcomes



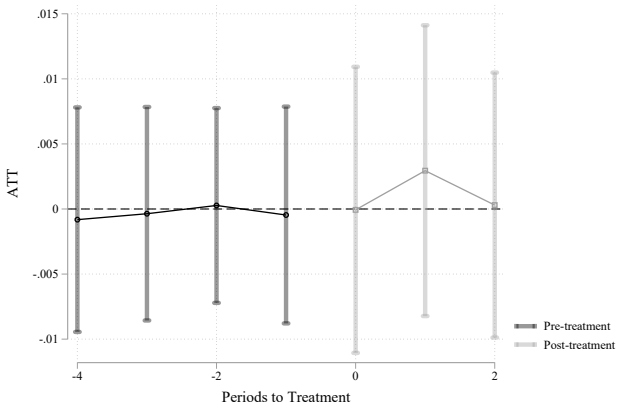

*Notes:* These figures plot the coefficients and 95% confidence intervals of the event study estimates for the main family outcomes for men with respect to the cohort first exposed to the reform in each municipality, estimated using Callaway and Sant'Anna (2021)'s estimator. The sample is restricted to make it balanced in event time, and only includes municipalities that implemented the reform from 1972 to 1974, for which effects two years after first adoption can be estimated.

Figure A4: Event studies for women's family outcomes

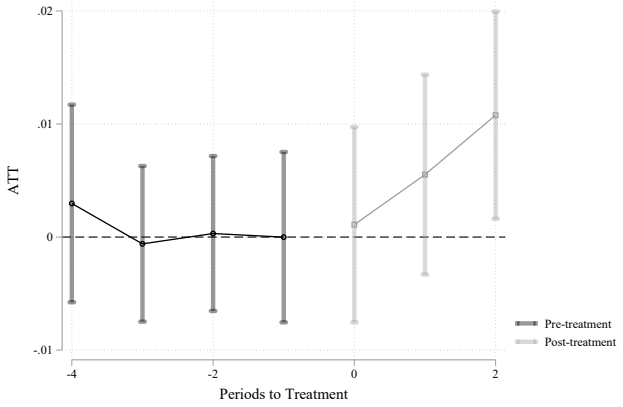
(a) Ever married



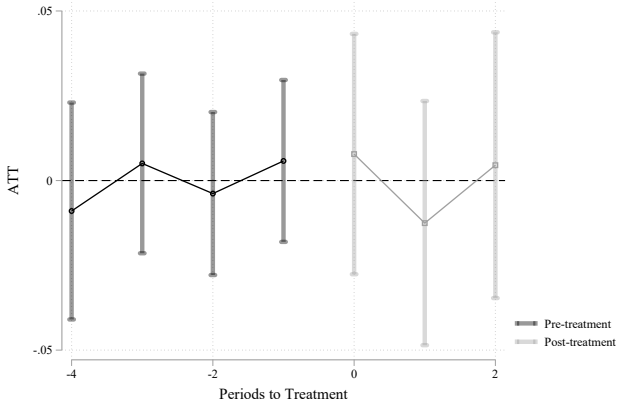

(b) Married or cohabiting




(c) Childless

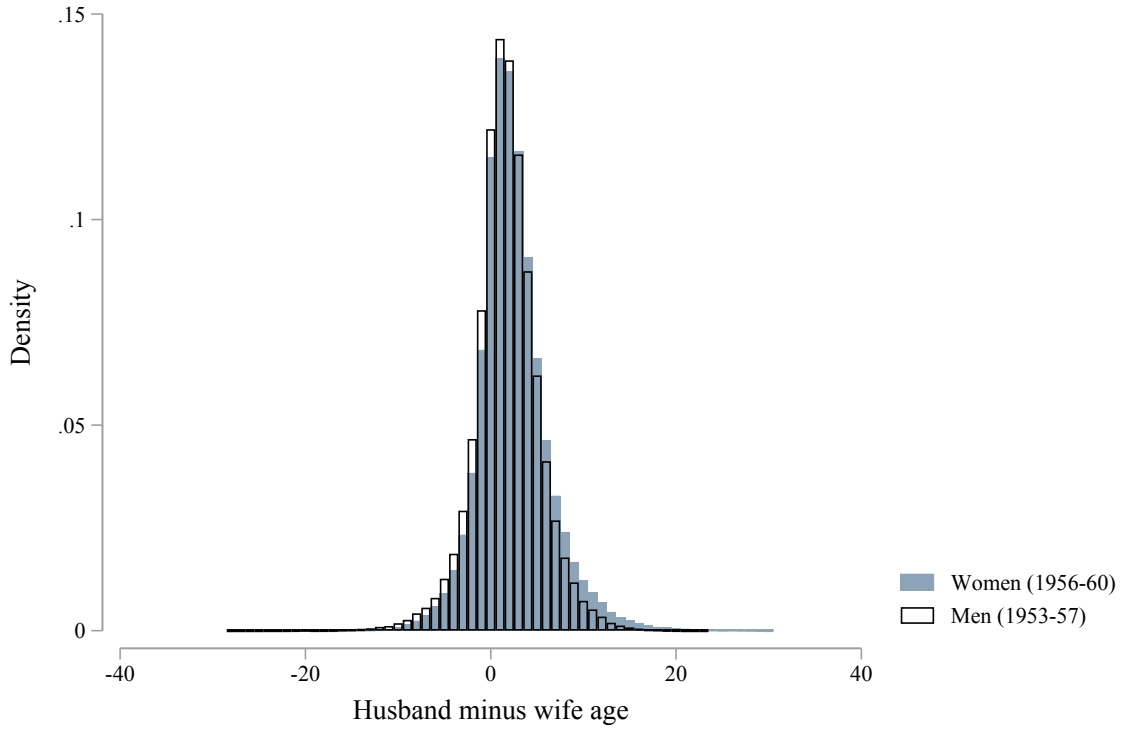



(d) Number of children



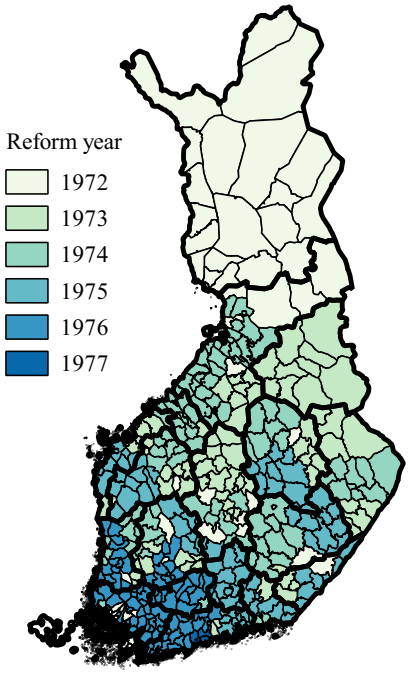

*Notes:* These figures plot the coefficients and 95% confidence intervals of the event study estimates for the main family outcomes for women with respect to the cohort first exposed to the reform in each municipality, estimated using Callaway and Sant'Anna (2021)'s estimator. The sample is restricted to make it balanced in event time, and only includes municipalities that implemented the reform from 1972 to 1974, for which effects two years after first adoption can be estimated.

Figure A5: Distribution of age difference between husband and wife in pre-reform cohorts



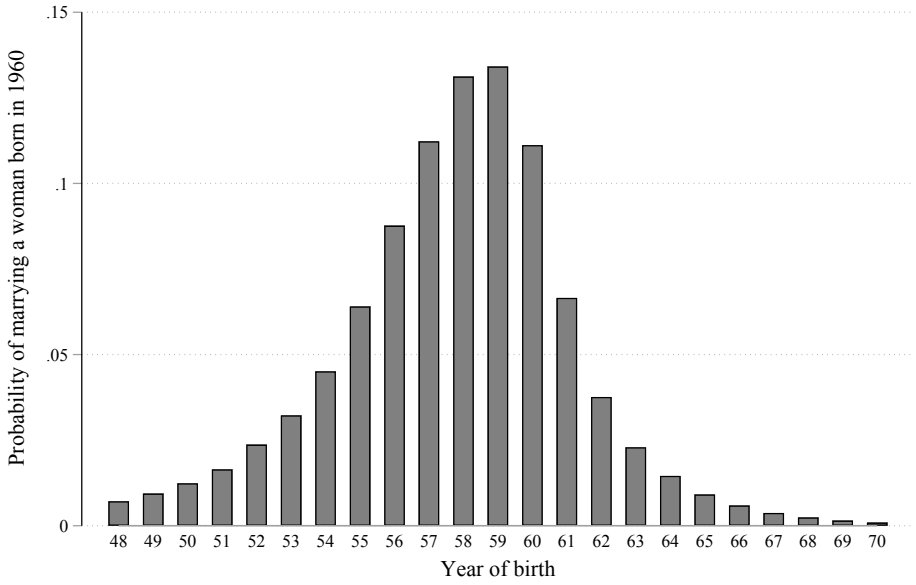

*Notes:* This figure shows the distribution of the age difference within married couples in pre-reform cohorts (1956-60 for women and 1953-57 for men).

Figure A6: Variation in year of reform implementation by municipality and region

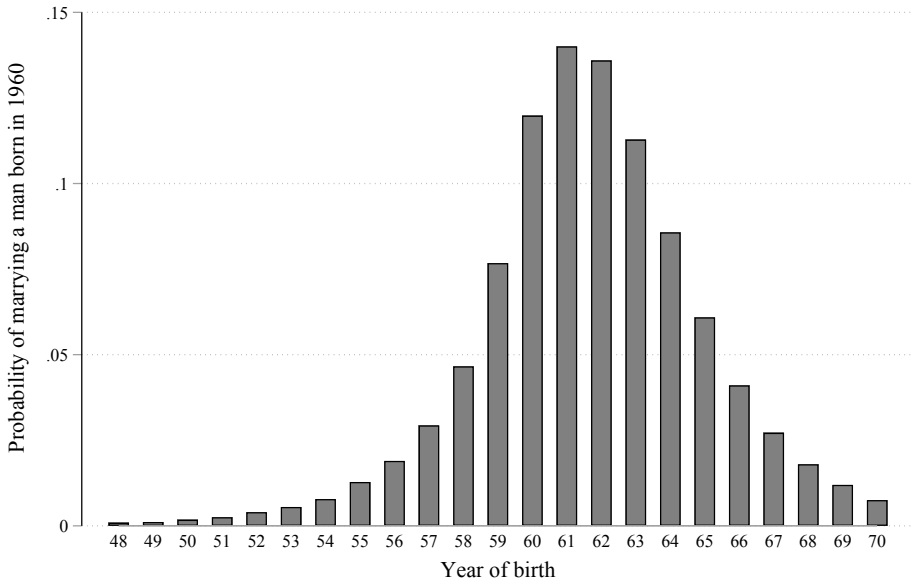



*Notes:* This map shows the year of adoption of the reform by municipality. Thicker lines indicate region boundaries.

Figure A7: Example of imputed probability of belonging to the marriage market – 1960 cohort



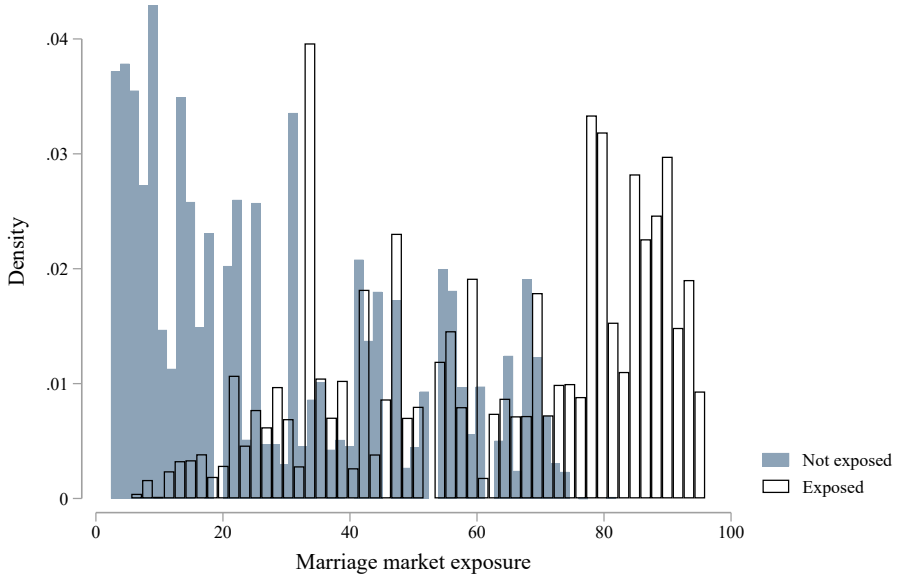

(a) Probability of belonging to the marriage market of a woman born in 1960



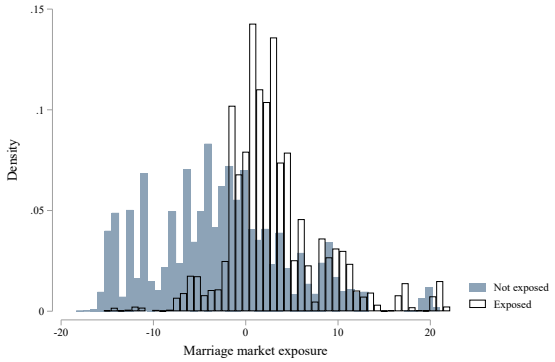

(b) Probability of belonging to the marriage market of a man born in 1960

*Notes:* This figure represents the weight given to men and women of each cohort for constructing the marriage market of 1960 women in panel (a), and of 1960 men in panel (b). The calculation is based on the distribution of the age difference within couples in pre-reform cohorts (1956-60 for women and 1953-57 for men), which is shown in Figure A5.

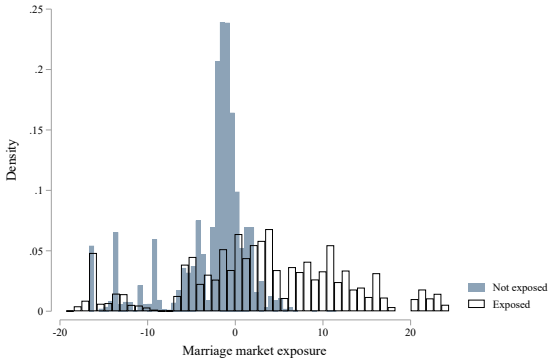
Figure A8: Variation in marriage market exposure to the reform for individuals affected and not affected by the reform themselves




(a) Full sample: raw data



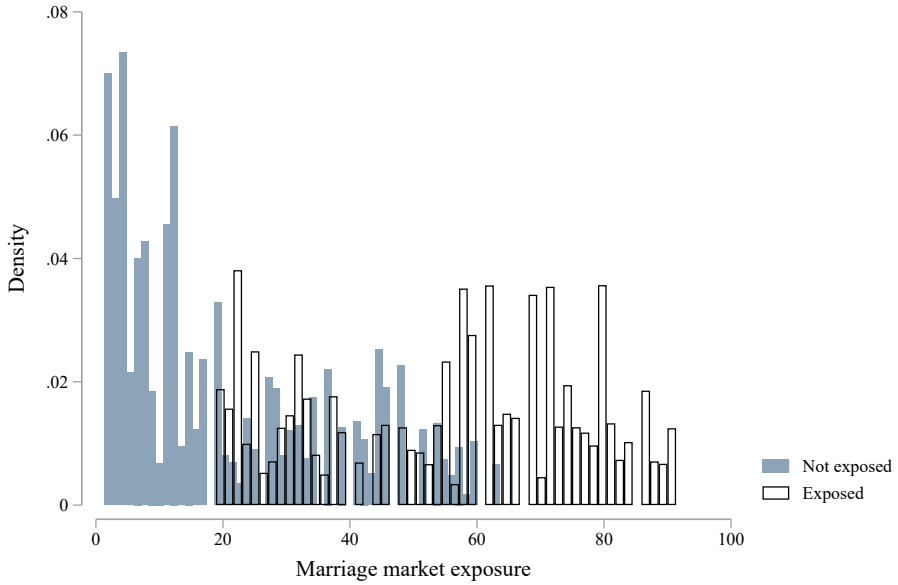

(b) Men: residualized variation



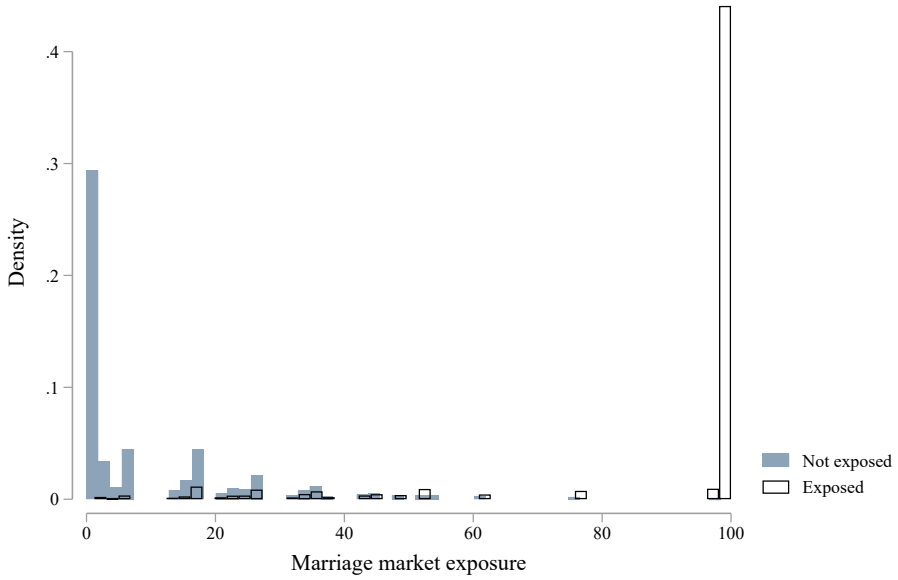

(c) Women: residualized variation

*Notes:* This figure shows, in panel (a) the distribution of the variable marriage market exposure to the reform, separately for those directly exposed to the reform and those not exposed. Panels (b) and (c) show the variation in marriage market exposure effectively used in the main analyses: conditional on own exposure, separately by gender, after accounting for year and municipality fixed effects.

Figure A9: Sources of variation in marriage market exposure to the reform



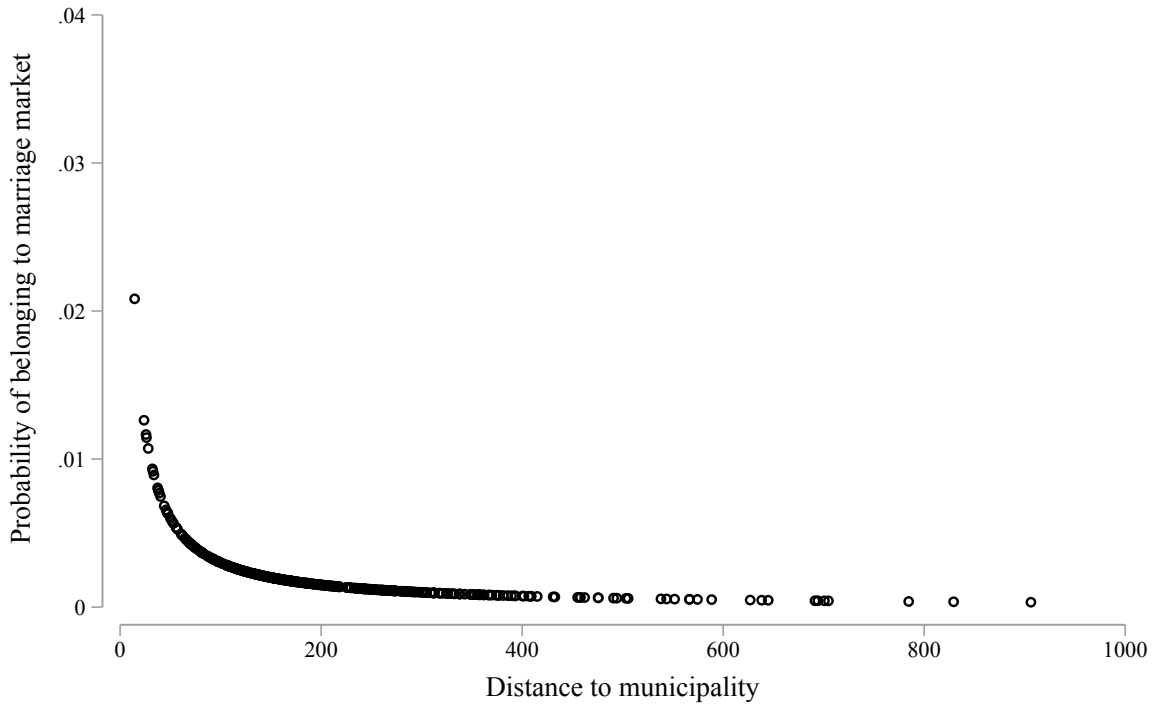

(a) All municipalities in the region implement the reform at the same time




(b) Marriage market includes only own cohort

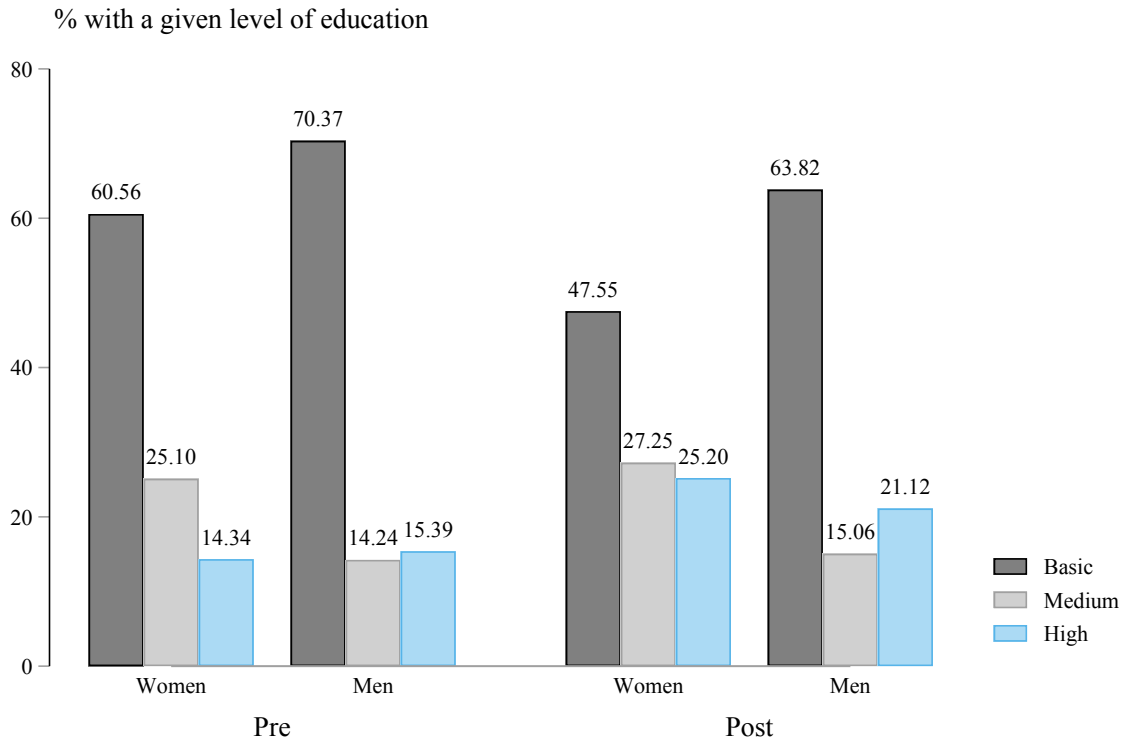
*Notes:* This figure explores the sources of variation in marriage market exposure to the reform, conditional on own exposure. Both panels show the distribution of marriage market exposure for those exposed and not exposed to the reform themselves, while shutting down the two main sources of variation, one at a time. In panel (a), I eliminate the variation in the timing of implementation of the reform across municipalities within regions by assigning the modal year of the reform in the region to all municipalities. In panel (b), I restrict the marriage market definition to include only individuals of the same cohort of the focal person.

Figure A10: Probability of belonging to marriage market by distance between municipalities: Tampere (example)



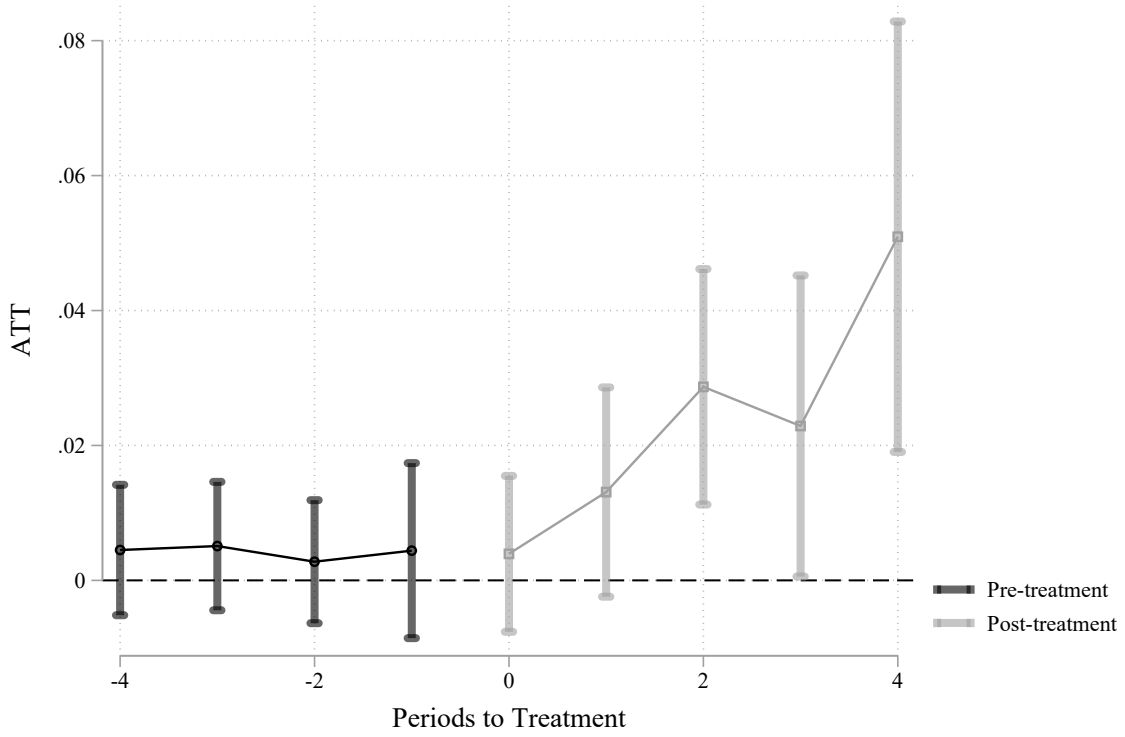

*Notes:* This figure plots the imputed probability for people in each municipality of belonging to the marriage market of individuals from Tampere (as an example). This probability is based on the inverse of the distance between each municipality and Tampere. Inverse distance probabilities are rescaled such that they add up to 1.

Figure A11: Distribution of educational attainment by gender and cohorts




*Notes:* This figure plots the percentage of men and women with basic, medium, and high level of education in pre-reform (1956-60) and post reform (1966-70) cohorts. Basic education is defined as upper secondary education at most; medium education is defined as more than secondary, but less than university education, and high education refers to university degree or higher.

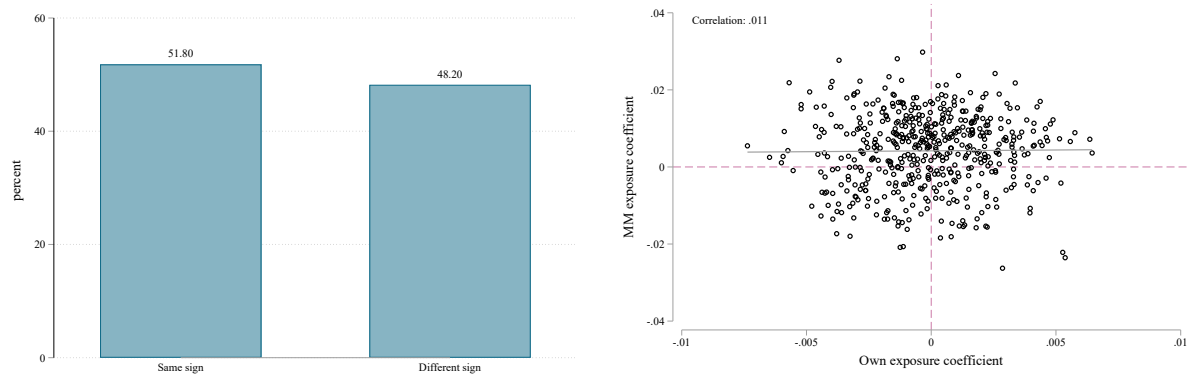
Figure A12: Female high education by cohort relative to first exposure to the reform (full sample)



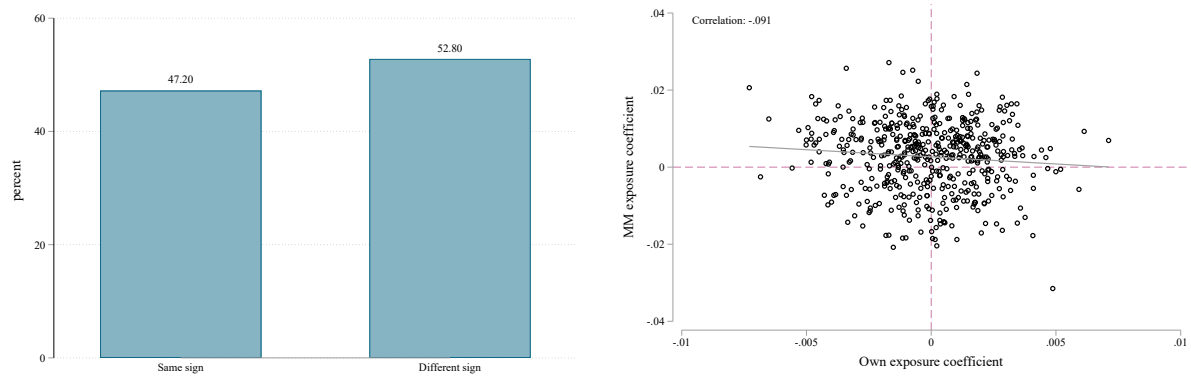

*Notes:* This figures plots the coefficients and 95% confidence intervals of the event study estimates of the probability of having more than secondary education for females with respect to the cohort first exposed to the reform in each municipality, estimated using Callaway and Sant'Anna (2021)'s estimator. The estimation sample is not restricted and is thus unbalanced in event time.

Figure A13: Placebo results (first part)

Ever married



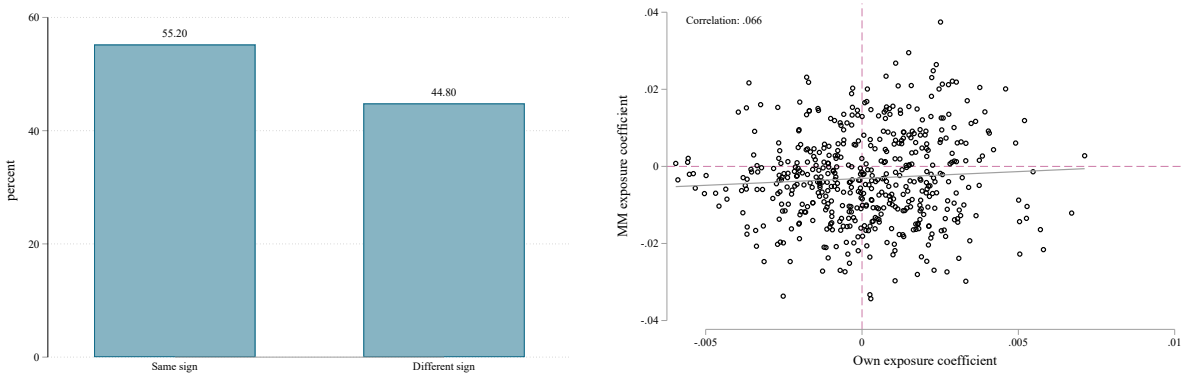

Married or cohabiting



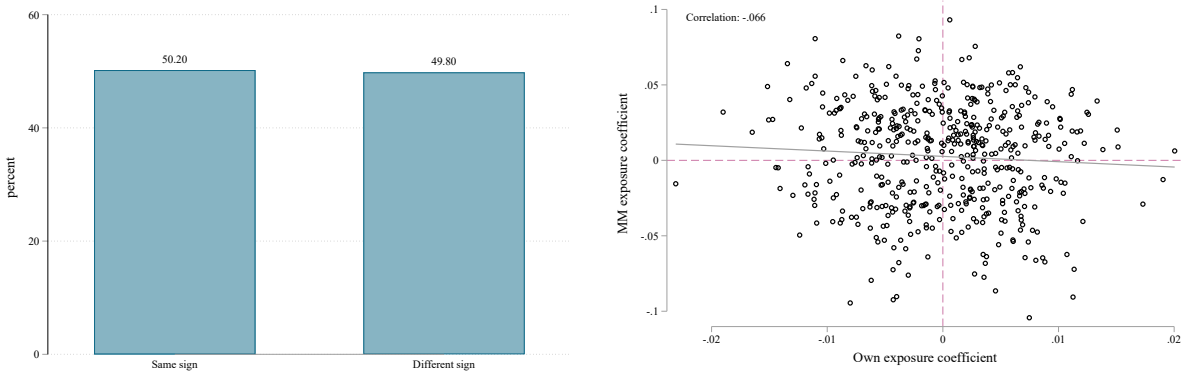

*Notes:* Continues on the next page.

Figure A12: Placebo results (continued)

Childless



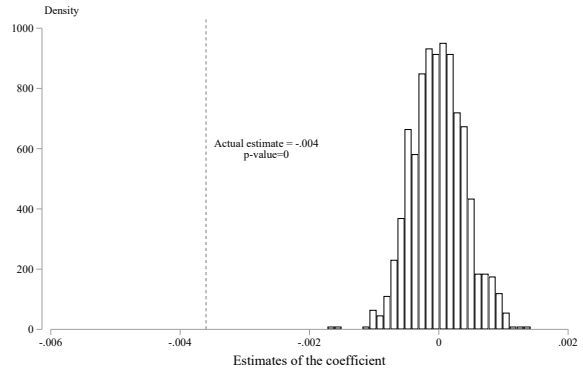

Number of children



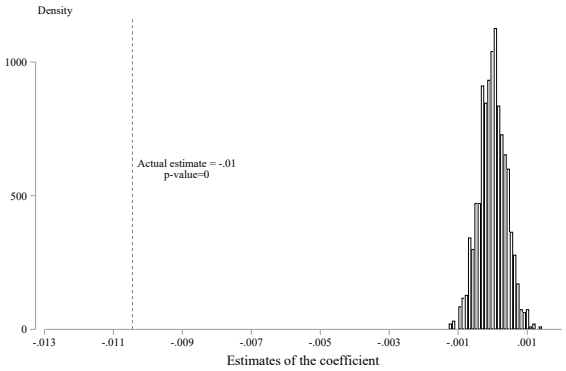

*Notes:* These figures summarize the results from 500 placebo regressions for each outcome variable. In each of them, the year of the reform was randomly shuffled across municipalities, and the measures of own and marriage market exposure to the reform were constructed based on the fake reform year. For each outcome, the left panel shows the proportion of times both exposure coefficients (own and marriage market) had the same versus the opposite sign in the regression for the pooled sample. The right panel displays the relationship between both coefficients across the 500 regressions.

Figure A14: Randomization inference results – Betas

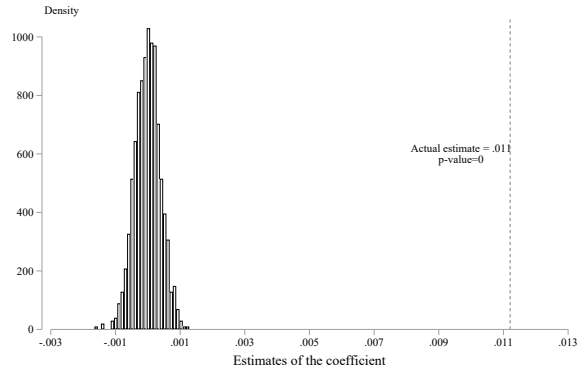
(a) Ever married



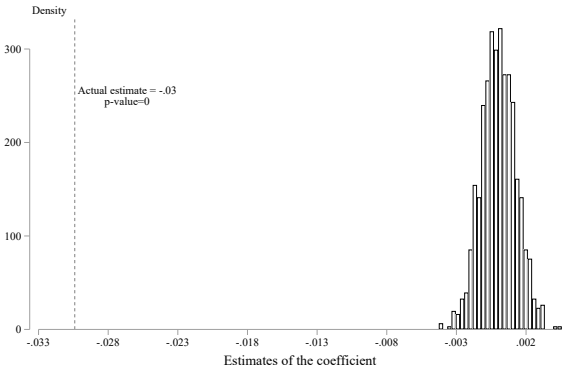

(b) Married or cohabiting




(c) Childless




(d) Number of children




*Notes:* This figure plots the results of the randomization inference exercise conducted with `ritest` (Heß, 2017). The different panels show the distribution of estimated coefficients of marriage market exposure across 1,000 permutations for the probability of having ever been married by age 40, for the probability of being in a couple at age 40, for the probability of having no children and for the number of children at that age. The dashed line in each panel represents the actual coefficient, and the p-value is the fraction of placebo estimates that are more extreme than the actual estimate. See section 3.2 for more details. P-values are $<0.001$ in all panels.