The impact of the female advantage in education on the marriage market

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April 2021

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Abstract

In recent years, the traditional gender gap in educational attainment in favor of men has been reversed in many countries. This development may have far-reaching consequences for the family, challenging traditional patterns of union formation and potentially affecting marriage and fertility outcomes. I study the implications of the female advantage in education on family formation through changes in the marriage market. My empirical strategy exploits the gradual implementation of a large school reform in Finland that increased women's relative level of education. I analyze the reduced-form relationship between marriage market exposure to the reform and marriage and fertility outcomes. The results show that in marriage markets with a larger female advantage in education men had fewer children and were less likely to be in a couple by age 40. I provide suggestive evidence that these results are mostly driven by the mismatch between the distributions of educational attainment of men and women, and that they might have negative consequences for low-educated men's health behaviors and 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 above 50% more men than women with university degrees in the working-age population in 1960. This difference gradually declined during the second half of the twentieth century, and by the 2000's the gap had been reversed.[1] This period also witnessed dramatic changes to family structure. Fertility rates fell, dropping below replacement levels in many countries; the age at first birth increased, and marriage now takes place later and less often. This transformation of the family and the increases in female education and labor force participation have been studied as closely connected phenomena (Goldin, 2006; Goldscheider et al., 2015; Oláh et al., 2018).

The reversal of the gender gap in education and the emerging female advantage could have far-reaching consequences for the family. Traditional 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's education levels exceed those of men 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. 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

[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

other hand, if individuals prefer a partner with their same level of education, we would expect an increasing mismatch between 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 were kept 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.

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

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.

[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).

that the female-male gap in continuing education beyond secondary school increased by 19%, and the gender gap in university education was reversed. 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 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, so that men were more likely to be single by age 40 and had fewer children. In particular, a one standard deviation increase in marriage market exposure to the reform, which leads to a 0.5 pp larger female advantage in education, decreases the probability of being married or cohabiting by 1.4% and the number of children by 1.7% for men. These effects are sizeable compared to the changes in family structure that took place in Finland during this period. An increase in marriage market exposure from the 25th to the 75th percentile of the distribution can account for around 20% of the actual decline in the share of men who are in a relationship observed during these decades. Importantly, this increase in bachelorhood is 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.[4] 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

[4]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.

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.

I also provide suggestive evidence that in marriage markets with a larger female advantage in education men became more likely to marry a woman more educated than themselves, and the average age gap within couples decreased. I do not find any effect on the probability of divorce. Lastly, my results suggest that these changes in family structure might have had negative consequences for men's mental health and health behaviors, especially for those with low level of education. This is 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).

This paper contributes to several strands of the literature. It first contributes to the studies on the implications of the reversal of the gender gap in education. So far, these works have 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 has more education. 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.[5] I contribute to this literature by providing causal estimates of the effect of an increase in 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).[6] Regarding marital outcomes, higher female education has been found not to affect the

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

[6]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)

probability of marriage, but to improve spouse quality (Anderberg and Zhu, 2014; Lefgren et al., 2006; McCrary and Royer, 2011).[7]

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 led to higher educational attainment for women, does not have significant effects on the probability of marriage, and has a small positive impact on fertility. More importantly, my findings show that, beyond the impact of individual changes in education, changes in the relative levels of education of men and women in a given marriage market also affect family outcomes. In this sense, this paper is also related to 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., 2020; Brainerd, 2017; Charles and Luoh, 2010; Lafortune, 2013; Mechoulan, 2011; Grosjean and Khattar, 2019).[8] My work is more closely connected to the scarce papers within this literature which focused on education-level specific sex ratios (Negrusa and Oreffice, 2010), or even field-of-study specific ratios (Pestel, 2017).

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. 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.[9] Tur-Prats (2017) and Ericsson (2020) show that improvements in women's relative economic position, measured by relative unemployment

finds that female education has a negative effect on fertility in England, but not in continental Europe.

[7] 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).

[8] 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.

[9] 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.

levels or potential earnings, can lead to increases in intimate-partner violence. Lastly, Folke and Rickne (2020) study the tension between women's career success and marital stability. They find that women's promotions, but not men's, increase their probability of divorce, based on the analysis of just-winning and just-losing candidates in parliamentarian and mayor elections in Sweden, and CEO promotions.[10]

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.[11] 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: 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 (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

[10] Similarly, Stuart et al. (2018) find that winning a Best Actress Oscar increases actresses' probability of divorce, while the same is not true for Best Actor Oscar winners.

[11] 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).

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.

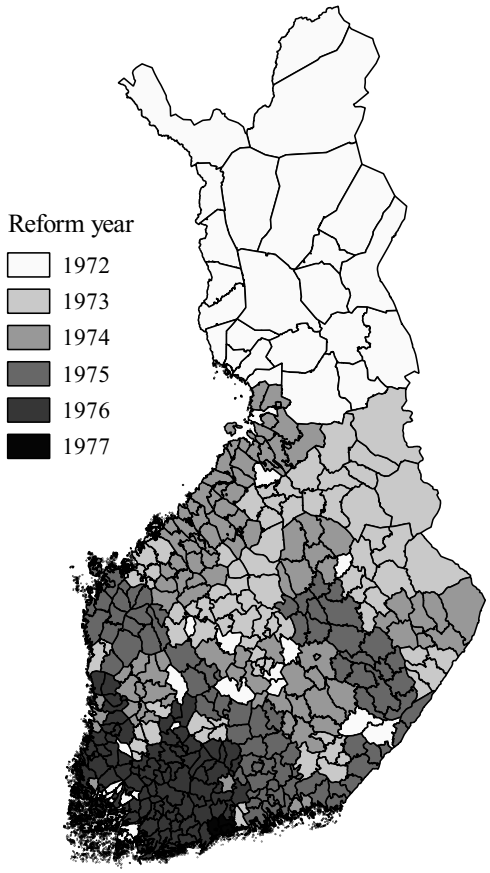
With the implementation of the reform, the former primary, general secondary and civic schools disappeared and were replaced by comprehensive schools. Comprehensive schools offered the same educational content to all students for nine years, from age 7 to 16. After this compulsory education, students could choose to either apply to an upper secondary school, apply to a vocational school,[12] 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

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

Figure 1: Year of adoption of the reform by municipality



grade and above continued their education in the pre-reform system.

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 Effect 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, 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.[13]

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

I will leverage this variation to first identify the impact of the reform on individual educational attainment and the gender gap in education, using a two-way fixed effects regression (in the

[13] All of them 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 four years, those in fourth grade were exposed to it for five years, and so on. This information is summarized in Table A1.

spirit of a differences-in-differences with variation in treatment timing):[14]

$$y_{imrc} = \beta_0 + \beta_1 OwnExposure_{mc} + \mu_c + \gamma_r \times t + (\beta_2 + \beta_3 OwnExposure_{mc} + \nu_c + \lambda_r \times t) \times F_i + \delta_m + \epsilon_{imrc} \quad (1)$$

where y_{imrc} 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; μ_c are cohort fixed effects; $\gamma_r \times t$ are region-specific linear trends (in cohort year); F_i is an indicator for female gender, and δ_m are municipality of birth fixed effects. 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, and on the gender gap in education (which will be captured by $\hat{\beta}_3$).

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. To study 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. 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, the 1962 cohort would have value 1, and so on. I run the following regression:

$$y_{imc} = \sum_{t \neq -1} \gamma_t + \mu_c + \delta_m + \epsilon_{imc} \quad (2)$$

where y_{imc} is an indicator of educational attainment, γ_t are coefficients on indicators for number of cohorts relative to first exposure to the reform, and t runs from -10 to 4. The indicator for $t = -1$ is excluded, such that coefficients represent changes in educational attainment with

[14] 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).

respect to the last non-affected cohort in a municipality. μ_c and δ_m are cohort and municipality of birth fixed effects, respectively. The results of this exercise are shown in section 5.1. 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.

There are some potential caveats when using the variation generated from the adoption of the comprehensive school system in a differences-in-differences setting (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 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 surrounding localities did.

Recent work on differences-in-differences methods by Goodman-Bacon (2018), Callaway and Sant'Anna (2020), and de Chaisemartin and D'Haultfoeuille (2020), among others, highlights other potential concerns with this type of estimators. Goodman-Bacon (2018) shows that, in models with variation in treatment timing, the two-way fixed effects differences-in-differences estimator can be seen as a weighted average of all the 2x2 differences-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 (2020) 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 (2018) decomposition, which allows one to see which types of comparisons have the most weight for the aggregate estimator.[15] 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 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 the gender gap in education using Callaway and Sant'Anna (2020)'s estimator.

3.2 Effect 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 coincide fully 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.[16] The

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

[16] The distribution of the age difference within couples, calculated as husband's minus wife's age, for men and

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.[17]

I thus run the following type of regressions:

$$y^g_{imrc} = \alpha_0 + \alpha_1 MarriageMarketExposure^g_{rc} + \alpha_2 OwnExposure_{mc} + \mu_c + \delta_m + \gamma_r \times t + v^g_{imrc} \quad (3)$$

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; μ_c are cohort fixed effects; δ_m are municipality of birth fixed effects; $\gamma_r \times t$ are region-specific linear time (cohort) trends, and standard errors are clustered at the municipality of birth level. These regressions are run separately for men and women. In section 6.2 I also discuss and show results with alternative specifications, such as including municipality-specific trends instead, or partialling out region-specific linear pre-trends.

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.[18] I then use the distribution of the age difference within couples in pre-reform cohorts, separately for men and women (see Figure A3), 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

women in pre-reform cohorts is shown in Figure A3.

[17] 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 A4 shows how even within regions (with borders marked in thicker lines) there is variation in reform timing.

[18] There are currently 19 regions in Finland, with the number of municipalities per region varying from 9 to 57 (median of 27). Figure A4 shows the map of Finland with the delimitation of regions and municipalities, together with the reform implementation year.

person j has on i's marriage market.[19] 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:

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

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). Figure A6 shows the distribution of marriage market exposure separately for those exposed and not exposed to the reform themselves.

One potential 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, the probability of marrying someone from the same region, or the probability of living by age 40 in a different region than that of birth. 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;[20] 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

[19] Figure A5 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.

[20] 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 A7 shows, as an example, the weight that individuals from each municipality have in the marriage market of people from Tampere depending on the distance.

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, 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 5,000 times. I then regress the different dependent variables on these placebo marriage market exposure variables, controlling for own exposure to the reform, year of birth fixed effects, region fixed effects, and region-specific linear trends, with standard errors clustered at the region level.[21] 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.[22] There 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

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

[22] 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.

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. 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 and family modules, which contain more detail about the history of marriages and divorces (including the spouse identifier), and about children (including their year of birth and identifiers).

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 those 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.[23] 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

[23]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.

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.[24] I also examine the following fertility-related variables: the number of children a person has by age 40, and an indicator for childlessness at this age.[25]

In supplementary analyses I also explore annual labor earnings and an indicator for being employed at age 30. Finally, I combine these datasets with the Finnish Hospital Discharge Register, which contains information about the diagnosed medical conditions coded in ICD10, 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 given diagnosis at ages 40-45.[26] This analysis is thus 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 and cirrhosis (K70, K74), and drug overdoses (T36-T51).

4.2 Descriptive statistics

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

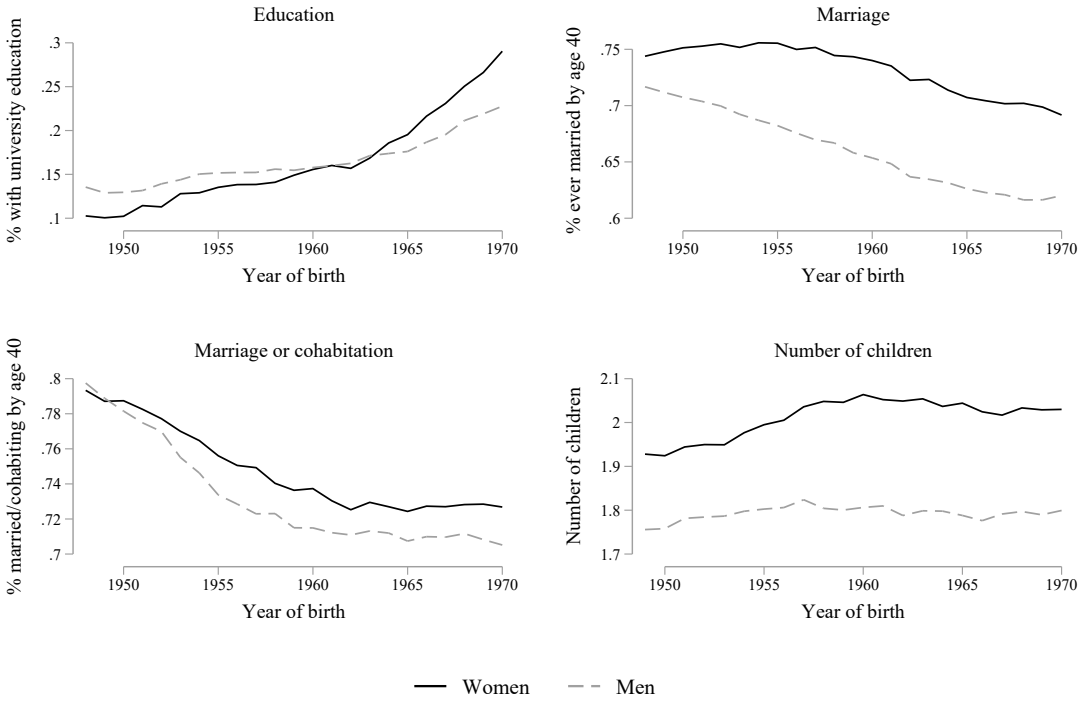
[24] Registered partnerships for same-sex couples were introduced in Finland in 2002, and same-sex marriage was not legalized until 2017.

[25] Information on biological children is only available in the register from 1989 onwards. In the analysis of fertility outcomes I thus focus on cohorts from 1949 to 1966.

[26] I consider not only age 40 but ages 40-45 in order to have smoother variables

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 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 A8 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.

Finally, Figure A9 shows the frequency of different types of couples, by relative level of education, in the same pre- and post-reform cohorts. Couples are classified into four groups: couples where none has university education (L-L), couples were both have university education (H-H), couples where only the husband has university education, and couples where only the wife has university education. The most remarkable changes from the pre-to the post-reform cohorts are the decrease in the frequency of low-educated couples, and the increased prevalence of couples where both have university education, and of couples where the wife is more educated than her husband.

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 specification of equation (1), 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 1.4 pp higher probability of having more than secondary schooling, a 3.6% increase with respect to the pre-reform average, and 0.9 pp higher probability of having university education (a 6% increase). As a result, the female advantage in having more than secondary education increased by 1.7 pp (a 19% increase). The former gender gap in university education in favor of men (1 pp) was reversed, as the female advantage increased by 1.1 pp.[27]

[27]Table A5 shows that results are similar if the variable of exposure to the reform is constructed based on

Table 2: Reform impact on the gender gap in education

	Post-secondary			University		
	Women	Men	Female adv.	Women	Men	Female adv.
Own exposure	0.014***	-0.002	0.017***	0.009***	-0.002	0.011***
	(0.004)	(0.004)	(0.005)	(0.003)	(0.003)	(0.003)
Observations	1460448	1460448	1460448	1460448	1460448	1460448
Adjusted R^2	0.034	0.034	0.034	0.016	0.016	0.016
Pre-reform mean	0.39	0.30	0.09	0.14	0.15	-0.01

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. 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. Standard errors (in parentheses) are clustered at the municipality of birth level. 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 means refers to average of the dependent variable in the sample of each column for cohorts born in 1956-1960. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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 consistent with studies showing that late pubertal development is associated with worse academic performance at age 16 and lower total educational attainment (Koerselman and Pekkarinen, 2018).

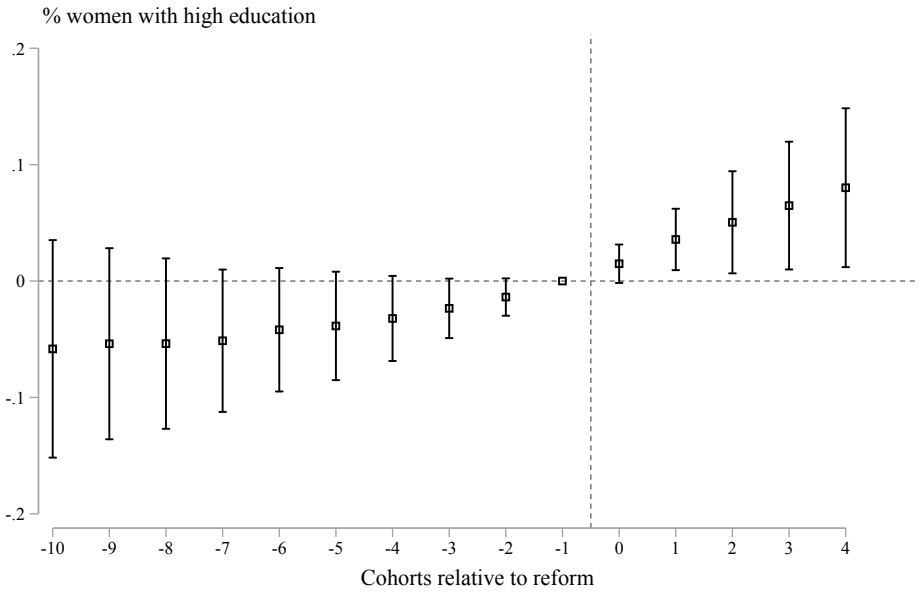
As discussed in section 3, to evaluate the extent to which the timing of the adoption of the reform for different municipalities was unrelated to trends in educational attainment, I perform an event study exercise. In particular, I estimate changes in female educational attainment by cohort, with cohorts normalized with respect to the first exposure to the reform in each municipality.[28]

The results of this exercise are shown in Figure 3. Panel (a) presents the results for the whole sample. While none of the pre-trend coefficients are significant, there seems to be an upward

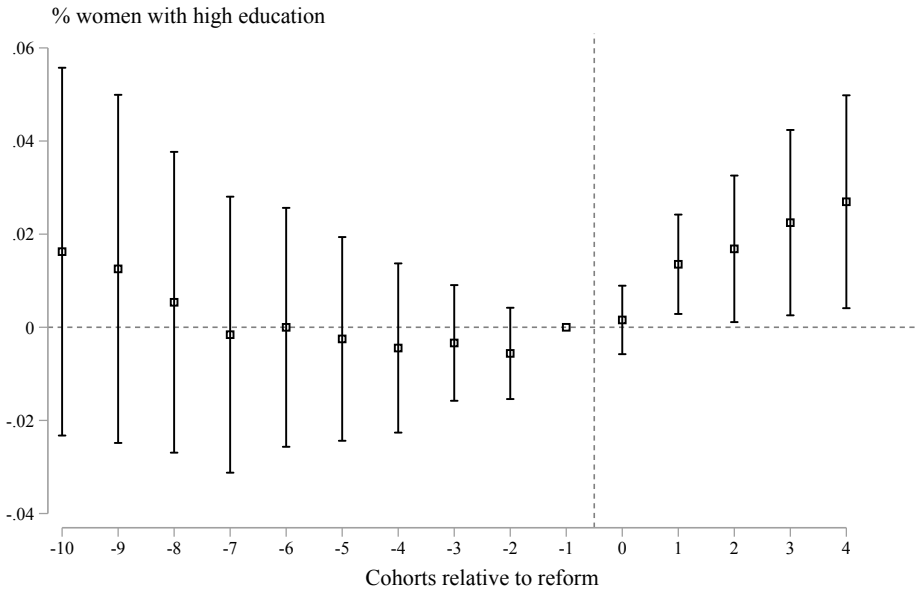
municipality of residence in 1970, obtained from the 1970 Census, rather than on municipality of birth.

[28] Cohorts up to 1970 are used in order to have a balanced sample.

Figure 3: Percentage of women with high education by cohorts relative to first exposure to the reform



(a) Full sample



(b) Without Helsinki region

Notes: These figures plot the coefficients of a regression of female high (post-secondary) education on indicators for number of cohorts relative to the one first exposed to the reform in a municipality, following the specification in (2). The coefficient on $t = -1$ is omitted, such that coefficients represent changes with respect to the last non-exposed cohort. Panel (a) shows results for the full sample, while in panel (b) the Helsinki region is excluded.

trend in female education before the reform took place. This finding could be related to the fact that schools in the Helsinki region, in spite of being scheduled to be among the last to implement the reform, had in practice already started to adopt it some years before. To check if this explains the observed pre-trends, in panel (b) I repeat this exercise excluding observations from the Helsinki region. In this case one cannot see any clear patterns for the cohorts preceding exposure to the reform, and the increases in female education start clearly only after its implementation. This suggests that an important robustness check will be to test the sensitivity of the results to excluding the capital region.

Table A6 presents the estimates of the impact of the reform on the probability of high education, separately for women and men, using Callaway and Sant'Anna (2020)'s estimator (see section 3.1). The results are similar in magnitude to those in Table 2, and suggest that the reform led to a 1.5 pp increase in the probability of having more than secondary education for women (significant at the 10% level), while it did not significantly affect men's educational attainment.

Finally, Table A7 explores the effect of the reform on gender gaps in the labor market by age 30, using the same specification as in (1).[29] The first three columns show results for the effect on earnings. We see that women affected by the reform earned on average 180 euro more by age 30. While no significant effect is found for men, the gender wage gap decreased as a result by around 280 euro (a 5% decrease). The last three columns show that the reform did not affect the probability of being employed by age 30 for either women or men.[30] These results are again consistent with the findings by Pekkarinen (2008) that the changes in the gender gap in education induced by the reform also translated to a certain extent into gender differences in earnings. These changes in relative earnings might thus be part of the channel through which marriage market exposure to the reform affects family outcomes (Autor et al., 2019; Bertrand et al., 2015).

[29] 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.

[30] 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.

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. I first show that marriage market exposure to the reform, conditional on individual exposure, does not itself affect a person's own level of education. The results are shown in Table A8: the coefficient of own exposure is not affected by the inclusion of marriage market exposure in the regression, and marriage market exposure does not have any significant impact on men's and women's level of education. The results in this section can thus be interpreted as the effect of changes in the educational composition of the marriage market, separate from changes in own level of education.

The first two columns in Table 3 show the estimates of the effect of marriage market exposure on men's 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 results show that marriage market exposure to the reform did not significantly affect the probability of having been in a formal marriage, but decreased the probability of being in a couple at age 40: a one standard deviation increase in marriage market exposure to the reform decreases the probability of being in a couple by 1 pp (a 1.4% decrease). Own exposure to the reform, on the other hand, does not seem to have affected these outcomes.

The last two columns of Table 3 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. Men whose marriage market was more affected by the reform had on average fewer children: a one standard deviation increase in marriage market exposure decreases the number of children by 1.7%. The probability of having at least one child, in turn, does not seem to be affected.[31]

Table A10 in the Appendix shows that conclusions are similar if I instead examine women's outcomes. There are also negative, although not significant, effects on the probability of having ever married and the probability of being in a couple at age 40, and a significant increase in the

[31] The effect on the number of children at age 40 could potentially be driven by changes in assortative mating by age or education. If in more affected marriage markets men become, for instance, more likely to marry younger women, they might also have a higher probability of having children after age 40. 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 as an outcome.

probability of not having any children. Interestingly, the estimates for own exposure 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). This suggests that the negative effects of marriage market exposure for men 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.

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 men born in 1950 in Finland, 78% of them where married or cohabiting at age 40. This number declined to 71% for men born in 1970. An increase in marriage market exposure to the reform from the 25th to the 75th percentile of the distribution, which would lead to an 8% increase in the female educational advantage, can account for around 20% 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 market.[32]

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 reduced-form nature of the analysis, these findings do not rely on the claim that only the gender gap in

[32]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) by age 40.

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

	Marriage		Fertility	
	Ever married	Married/cohab	Childless	Num children
Marriage market exposure (sd)	-0.003	-0.010**	0.004	-0.031**
	(0.006)	(0.005)	(0.004)	(0.014)
Own exposure	0.004	0.000	-0.002	0.003
	(0.004)	(0.003)	(0.003)	(0.010)
Observations	743911	743911	638569	638569
Adjusted R^2	0.010	0.008	0.011	0.015
Pre-reform mean	0.66	0.72	0.20	1.81

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 men's outcomes: the probability of having ever been married by age 40 (column 1), the probability of being either married or cohabiting at this age (column 2), the probability of not having had any children by age 40, and the total number of children by this age in the last column. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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 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. In order to see if this is the case, I explore heterogeneous effects by level of education.

Table 4 shows results for the effect of higher marriage market exposure on marriage and fertility outcomes separately for high- and low-educated men, where low-educated men are those with at most secondary education. These results show significant negative effects on the probability of being in a couple and the number of children only for low-educated men, while for those with higher level of education none of the coefficients are significant.

The analysis for women requires further caution: because the reform had a direct effect on

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

	Ever married		Married/cohabiting		Childless		Number of children	
	Low	High	Low	High	Low	High	Low	High
Marriage market exposure (sd)	-0.004 (0.007)	0.005 (0.008)	-0.011* (0.006)	-0.005 (0.007)	0.008 (0.006)	-0.007 (0.007)	-0.035* (0.018)	-0.018 (0.023)
Observations	528571	215340	528571	215340	448030	190539	448030	190539
Adjusted R^2	0.012	0.012	0.011	0.007	0.011	0.010	0.017	0.014
Pre-reform mean	0.61	0.77	0.69	0.81	0.22	0.16	1.76	1.92

Standard errors (in parentheses) are clustered at the municipality of birth level. This table shows heterogeneous effects of marriage market exposure by level of education for men. High-education men are defined as those with more than secondary schooling. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

educational attainment for them, conditioning on level of education for the whole sample would lead to biased estimates. I will therefore focus on cohorts not exposed to the reform themselves, and exploit variation in degree of exposure in their marriage market only. Table A11 shows results separately for high- and low-educated men and women, respectively, in the sample of those who were never exposed to the new school system.[33] The results suggest that, in this sample, higher marriage market exposure leads to decreases in the probability of having ever married among women with high level of education, but not among the low-educated ones. Results are similar, although a bit smaller, for the probability of being married or cohabiting by age 40. Consistent with this, albeit not always significant, the estimates for fertility outcomes suggest that both increases in childlessness and decreases in the number of children are concentrated in women with high level of education and men with low level of education.

All in all, this evidence is consistent with more exposed marriage markets having a larger mismatch among the educational distributions 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 absolute gender difference in education increased more as a result of the reform. The

[33]Individuals from Helsinki region are also excluded given that, as discussed in section 3, some were exposed to the new system before the date assigned in the adoption plan.

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 the educational mismatch in some markets, to increases in others, and to little change in some (but to a reverse of the gap). I classify regions into two groups: regions in which the gender educational mismatch increased in absolute terms after the reform, and regions in which it did not change or it decreased.[34]

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 the mismatch did not increase. In those markets, the only change 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.[35]

I explore heterogeneity by the change in the gender gap in education induced by the reform at the marriage market level in Table 5. The first two columns display the estimates for regions in which the gender gap in education did not increase in absolute terms, while the last columns show results for those in which it increased. Each row presents estimates of the effect of marriage market exposure from separate regressions with the different dependent variables. In general, we see that the effects are stronger in marriage markets where the reform led to an increase in educational mismatch: higher marriage exposure leads in these regions to declines in the probability of having ever married, and to a lower probability of being in a couple by age 40. However, even in regions where the reform did not lead to an increase in mismatch, higher marriage market exposure has negative effects. In particular, we see that the increase in female childlessness is the same in both groups of regions.

The results from this exercise suggest that, even though increases in educational mismatch seem

[34]Specifically, I define the change in the gender gap as the difference between the gender gap in absolute value after the reform, and the gender gap in absolute value before the reform. To do so I estimate the impact of the reform on the gender gap in university education separately for each region.

[35]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.

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

	No increase		Increase	
	Women	Men	Women	Men
A. Marriage outcomes				
Ever married by 40	-0.003	0.000	-0.009**	-0.003
	(0.006)	(0.007)	(0.004)	(0.011)
Mean of Y	0.74	0.66	0.75	0.67
Married/cohabiting by 40	-0.000	-0.006	-0.011*	-0.018**
	(0.006)	(0.007)	(0.006)	(0.007)
Mean of Y	0.74	0.72	0.74	0.73
N	364908	378492	351629	365419
B. Fertility outcomes				
Childless	0.010***	-0.002	0.009**	0.007
	(0.003)	(0.005)	(0.004)	(0.006)
Mean of Y	0.12	0.21	0.12	0.20
Number of children	-0.012	-0.017	-0.001	-0.036
	(0.015)	(0.018)	(0.014)	(0.024)
Mean of Y	2.01	1.77	2.08	1.84
N	322464	324535	310729	314034

This table shows the coefficients of marriage market exposure in separate regressions where the dependent variable is the one indicated in each row. The sample in the first two columns consists of regions where the gender gap in university education decreased or did not change as a result of the reform, while the last two columns show results for regions where it increased. Standard errors (in parentheses) are clustered at the municipality of birth level. The specification includes the indicator for own exposure, cohort and municipality of birth F.E., and region-specific linear trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

to be an important driving force, they are not enough to explain the main findings. The fact that higher exposure to the reform in the marriage market has a negative impact on fertility, even where mismatch did not increase, suggests that gender identity norms might also play a role.

5.4 Assortative mating and marital dissolution

Higher marriage market exposure to the reform might also affect other family-related outcomes, such as assortative mating or the probability of marital dissolution. 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, and marital dissolution.

The first three columns of Table 6 present estimates of the impact of marriage market exposure on the relative level of education within married couples. 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.

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

	Relative level of education			Age difference	Divorced
	Equal	More	Less	with spouse	by 40
Marriage market exposure (sd)	-0.008 (0.005)	-0.004 (0.003)	0.007** (0.003)	-0.124** (0.060)	0.003 (0.004)
Observations	743911	743911	743911	570897	743911
Adjusted R^2	0.010	0.003	0.007	0.004	0.011
Pre-reform mean	0.45	0.08	0.13	1.74	0.14

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 column 5, an indicator equal to 1 if the man has divorced by age 40. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The fourth column shows results for the age difference within couples, expressed as husband's minus wife's age, such that it is on average positive. The estimates suggest that higher marriage market exposure decreases the 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.[36] Finally, the last column shows that marriage market exposure does not affect the probability of (formal) divorce for men. It might thus be that the decreased probability of being in a couple comes from separations, instead of divorces, or that it is driven

[36]These results are available upon request.

by couples that would have never been formally married in the first place.

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 in marriage markets with a larger female advantage in education men's health outcomes are affected, and to look at 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 and alcohol problems, but do not present more hospital visits with substance abuse diagnoses. The heterogeneity analysis in columns 2-3 reveals that these negative effects are entirely driven by low-educated men.[37]

These results suggest that the increasing female advantage in education, and its associated changes in family structure, might have negative consequences for men's health behaviors and mental health. This is consistent with Ericsson (2020)'s findings that relative increases in women's potential earnings increase their husbands' probability of hospital visits due to stress, anxiety,

[37] None of the coefficients of own exposure to the reform are significant in these regressions. A recent paper by Böckerman et al. (2019) looking at the "direct" effects of the comprehensive school reform on mental health finds no discernible effects either. In Table A12 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.

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

	(1) All	(2) Low educated	(3) High educated
Mental health	0.007*	0.011**	-0.001
	(0.004)	(0.005)	(0.005)
Mean of Y	0.08	0.09	0.04
N	329408	225024	104384
Alcoholic liver	0.006*	0.010*	-0.002
	(0.003)	(0.006)	(0.004)
Mean of Y	0.05	0.06	0.03
N	329408	225024	104384
Substance abuse	0.000	-0.000	0.001
	(0.001)	(0.002)	(0.001)
Mean of Y	0.01	0.01	0.00
N	329408	225024	104384

This table shows the coefficients of marriage market exposure in separate regressions where the dependent variable is the one indicated in each row. 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. Standard errors (in parentheses) are clustered at the municipality of birth level. The specification includes the indicator for own exposure, cohort and municipality of birth F.E., and region-specific linear trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

substance abuse, or assault.

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 mar-

riage 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. On the contrary, the definition that restricts the marriage market to those born in the same region and within an age gap of 0-3 years 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.

Table 8: Marriage market exposure coefficient with alternative marriage market definitions – men's outcomes

	Baseline	Region & 0-3 years	Age dist. & freq. marriage	Age dist. & inv. distance
	(1)	(2)	(3)	(4)
A. Marriage outcomes				
Ever married by 40	-0.003	-0.001	-0.008	-0.009
N=743911	(0.006)	(0.003)	(0.008)	(0.009)
Married/cohabiting by 40	-0.010**	-0.005**	-0.015**	-0.013**
N=743911	(0.005)	(0.002)	(0.006)	(0.005)
B. Fertility outcomes				
Childless	0.004	0.002	-0.002	0.008
N=638569	(0.004)	(0.002)	(0.007)	(0.005)
Number of children	-0.031**	-0.013*	-0.025	-0.025
N=638569	(0.014)	(0.007)	(0.024)	(0.019)

This table shows the coefficients of marriage market exposure in separate regressions 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. Standard errors (in parentheses) are clustered at the municipality of birth level. The specification includes the indicator for own exposure, cohort and municipality of birth F.E., and region-specific linear trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The results remain virtually unaltered. In column 3, instead of including linear trends, I instead de-trend the dependent variable of region-specific linear pre-trends. To do so, I follow Goodman-Bacon (2018) and estimate pre-trends by regressing the dependent variable on region-specific linear trends for cohorts up to 1960. These trends are then substracted 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 number of children.

In the last 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 4 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. Finally,

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

	Baseline	Municipality trends	Region pre-trends	W/o Helsinki	W/o outliers
	(1)	(2)	(3)	(4)	(5)
A. Marriage outcomes					
Ever married by 40	-0.003	-0.005	-0.001	0.001	-0.004
RW p-value=0.604	(0.006)	(0.006)	(0.004)	(0.006)	(0.006)
Married/cohabiting by 40	-0.010**	-0.010**	-0.010**	-0.008	-0.010**
RW p-value=0.039	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
N	743911	743911	743911	671868	711116
B. Fertility outcomes					
Childless	0.004	0.004	0.004	0.002	0.003
RW p-value=0.574	(0.004)	(0.004)	(0.003)	(0.005)	(0.004)
Number of Children	-0.031**	-0.029**	-0.007	-0.025	-0.029**
RW p-value=0.039	(0.014)	(0.014)	(0.011)	(0.016)	(0.014)
N	638569	638569	638569	577743	610398

This table shows the coefficients of marriage market exposure in separate regressions where the dependent variable is the one indicated in each row. Standard errors (in parentheses) are clustered at the municipality of birth level, and bootstrapped in column (3). RW p-value refers to the Romano-Wolf stepdown adjusted p-value to correct for multiple hypothesis testing in the baseline specification. All specifications include the indicator for own exposure, cohort and municipality of birth F.E., and additional controls as indicated in column titles. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

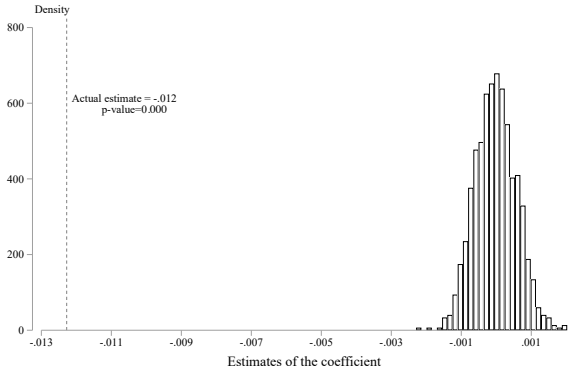
some municipalities were assigned to adopt the reform earlier than most of their surrounding localities (see section 3). As discussed by Pekkarinen (2008), the choice of these municipalities is unlikely to have been random. In column 5 I drop individuals from these municipalities and find that results 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.

Finally, Figure 4 presents the results from the randomization inference exercises described in section 3.2 for the probability of being in a couple at age 40 (first column) and for the number of children (second column). The upper figures compare the distribution of estimated coefficients from the placebo regressions with the actual estimate of marriage market exposure, while the bottom figures do the same for the t-statistics. The figures also report the resulting p-values, which indicate the proportion of placebo estimates that are more extreme than the actual

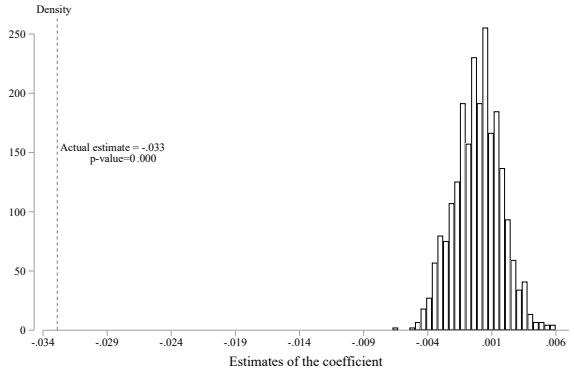
estimate. Reassuringly, the estimated coefficients of "fake" marriage market exposure are small and centered around zero for both variables. Inference based on these coefficients leads to very low p-values for both outcomes. In contrast, the p-values based on the t-statistics are somewhat larger than those in the main analysis. These suggest that the effect on the probability of being in a couple is significant at the 10% level, while the p-value for the number of children is 0.13. Overall, the results from this exercise support the validity of the main findings.

Figure 4: Randomization inference results

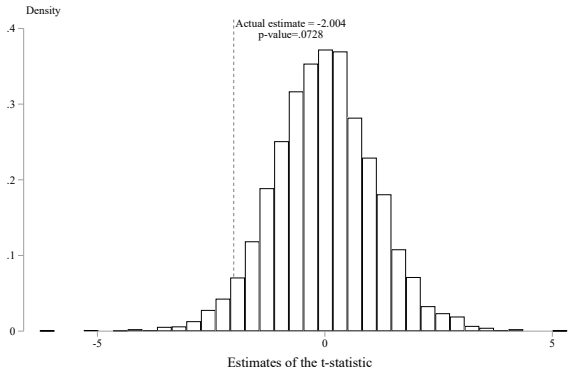
(a) Married or cohabiting at 40 – Coefficients



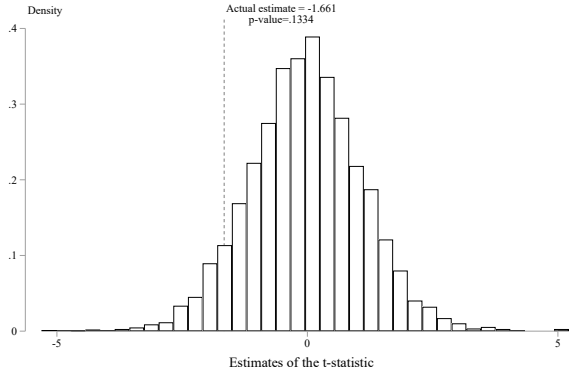
(b) Number of children – Coefficients



(c) Married or cohabiting at 40 – t-statistics



(d) Number of children – t-statistics



Notes: This figure plots the results of the randomization inference exercise conducted with `ritest` (Heß, 2017). Panels (a) and (b) show the distribution of estimated coefficients of marriage market exposure across 5,000 permutations for men's probability of being in a couple at age 40 and for their number of children, respectively. Panels (c) and (d) do the same for the t-statistics. The dashed line in each panel represents the actual coefficient or 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.

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 men are more likely to be single by age 40, and have fewer children. The size of these effects is substantial. An increase in marriage market exposure from the 25th to the 75th percentile of the distribution can explain 20% of the decline in the share of men who are in a couple at age 40 that took place in Finland during this period.

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 increased mismatch more. However, my analysis also highlights that the sign of the gender gap in education, and not only its size, matters. In particular, there are negative effects on family outcomes even in marriage markets where the absolute size of the educational mismatch did not increase, but women became more educated than men. This is consistent with recent work highlighting the importance of gender identity norms (Bertrand et al., 2015; Folke and Rickne, 2020; Tur-Prats, 2017), and with previous evidence from online dating sites showing that men shy away from women more educated than themselves (Hitsch et al., 2010).

One limitation of this analysis is that it does not allow us to quantify the extent to which the estimated effects are driven by an advancement of women's position in the labor market, as opposed to other changes in social status induced by an increase in the level of education. 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 health behaviors and 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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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 for pre-reform cohorts

	Post-secondary		University	
	(1)	(2)	(3)	(4)
Outlier==1	0.002	-0.001	0.006**	0.004
	(0.004)	(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

Standard errors in parentheses. This table compares the education levels 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 at least a university degree in columns 3-4. Outlier is an indicator equal to 1 if the municipality implemented the reform in a different year than most municipalities of 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, performed using the `bacondecomp` Stata package (Goodman-Bacon et al., 2019).

Table A4: Reform impact on definition of marriage market

	Age gap within couple		Spouse from same region		Living in different region	
	Women	Men	Women	Men	Women	Men
Own exposure	-0.006	-0.046	0.000	-0.003	0.006	0.006
	(0.033)	(0.031)	(0.003)	(0.003)	(0.005)	(0.004)
Observations	1460448	1460448	1460448	1460448	1460448	1460448
Adjusted R^2	0.003	0.003	0.021	0.019	0.074	0.074
Mean of Y	2.39	1.74	0.39	0.36	0.34	0.34

Standard errors (in parentheses) are clustered at the municipality of birth level. This table shows the effect of own exposure to the reform on the average age gap within couples (columns 1-2), on the probability of having a spouse born in the same region (columns 3-4), and on the probability of living in a different region than the region of birth by age 40 (last two columns), for men and women. 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 * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Reform impact on the gender gap in education – with 1970 Census data

	Post-secondary			University		
	Women	Men	Female adv.	Women	Men	Female adv.
Own exposure	0.014***	-0.005	0.018***	0.010***	-0.002	0.012***
	(0.004)	(0.004)	(0.005)	(0.003)	(0.003)	(0.003)
Observations	1420659	1420659	1420659	1420659	1420659	1420659
Adjusted R^2	0.031	0.031	0.031	0.013	0.013	0.013
Pre-reform mean	0.39	0.30	0.09	0.14	0.15	-0.01

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, with exposure defined based on the 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. Standard errors (in parentheses) are clustered at the level of the municipality of residence in 1970. The specification includes cohort and municipality F.E., as well as region-specific linear trends. Own exposure takes value 1 for cohorts and municipalities affected by the reform. Pre-reform means 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 A6: Impact of the reform on education – Callaway and Sant'Anna (2020)'s estimator

	Women	Men
Own exposure	0.015*	0.004
	(0.008)	(0.008)
Observations	8152	8153

This table shows the results from the estimation 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 (2020)'s estimator. The estimation was performed using the `did` R package (Callaway and Sant'Anna, 2020), using the doubly-robust method with not-yet treated units as controls, with the data collapsed at the municipality-year of birth-gender level. The coefficient of Own exposure refers to the weighted average of all group-time average treatment effects, obtained with the simple aggregation provided in the package. Bootstrapped standard errors (in parentheses) are clustered at the municipality of birth level. The * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Impact of the reform on labor market outcomes at age 30

	Earnings			Working		
	Women	Men	Female adv.	Women	Men	Female adv.
Own exposure	184.020***	-97.125	281.146***	-0.001	0.004	-0.004
	(64.083)	(77.252)	(99.713)	(0.003)	(0.003)	(0.004)
Observations	263500	282148	545648	317145	327439	644584
Pre-reform mean	11062.13	16398.02	-5335.89	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 earnings by age 30 (columns 1-3) and on the probability of being employed by 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 * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Own vs. Marriage market exposure: impact on high level of education

	Women		Men	
	(1)	(2)	(3)	(4)
Own exposure	0.014***	0.014***	-0.002	-0.001
	(0.004)	(0.004)	(0.004)	(0.004)
Marriage market exposure (sd)		-0.000		-0.002
		(0.002)		(0.003)
Observations	716537	716537	743911	743911
Adjusted R^2	0.038	0.038	0.016	0.016
Pre-reform mean	0.39		0.30	

Standard errors (in parentheses) are clustered at the municipality of birth level. This table shows that marriage market exposure to the reform does not affect individuals' level of education, once own exposure to the reform is accounted for. The dependent variable is a dummy taking value 1 if the person has more than secondary schooling, and 0 otherwise. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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

	Age 45		Age 50	
	Childless	Num children	Childless	Num children
Marriage market	0.003	-0.037**	0.001	-0.025
exposure (sd)	(0.005)	(0.018)	(0.005)	(0.018)
Observations	582109	582109	593225	593225
Adjusted R^2	0.099	0.029	0.055	0.021
Pre-reform mean	0.03	2.27	0.13	2.06

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 men's fertility outcomes (probability of not having any children and number of children) at age 45 (columns 1–2) and at age 50 (columns 3–4). 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 A10: Marriage market exposure impact on women's marriage and fertility by age 40

	Marriage		Fertility	
	Ever married	Married/cohab	Childless	Num children
Marriage market exposure (sd)	-0.004	-0.004	0.009***	-0.008
	(0.004)	(0.005)	(0.002)	(0.010)
Own exposure	0.002	0.005*	-0.001	0.017*
	(0.003)	(0.003)	(0.002)	(0.009)
Observations	716537	716537	633193	633193
Adjusted R^2	0.007	0.009	0.012	0.019
Pre-reform mean	0.74	0.74	0.12	2.05

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 women's marriage (probability of having ever married by age 40 and probability of being currently married or cohabiting by this age) and fertility outcomes (probability of not having any children by age 40 and number of children by this age). 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A11: Heterogeneous effects of marriage market exposure by level of education – sample not directly exposed

	Women		Men	
	Low	High	Low	High
A. Marriage outcomes				
Ever married by 40	0.000	-0.012*	-0.002	0.008
	(0.004)	(0.006)	(0.009)	(0.012)
Mean of Y	0.73	0.78	0.62	0.78
Married/cohabiting by 40	0.001	-0.007	-0.010	-0.004
	(0.004)	(0.006)	(0.007)	(0.012)
Mean of Y	0.74	0.78	0.69	0.81
N	329638	166352	374126	139927
B. Fertility outcomes				
Childless	-0.000	0.001	0.015*	-0.004
	(0.003)	(0.005)	(0.008)	(0.014)
Mean of Y	0.11	0.12	0.22	0.16
Number of children	0.007	0.004	-0.007	-0.000
	(0.014)	(0.018)	(0.029)	(0.042)
Mean of Y	2.11	1.99	1.77	1.93
N	287439	146516	314773	122854

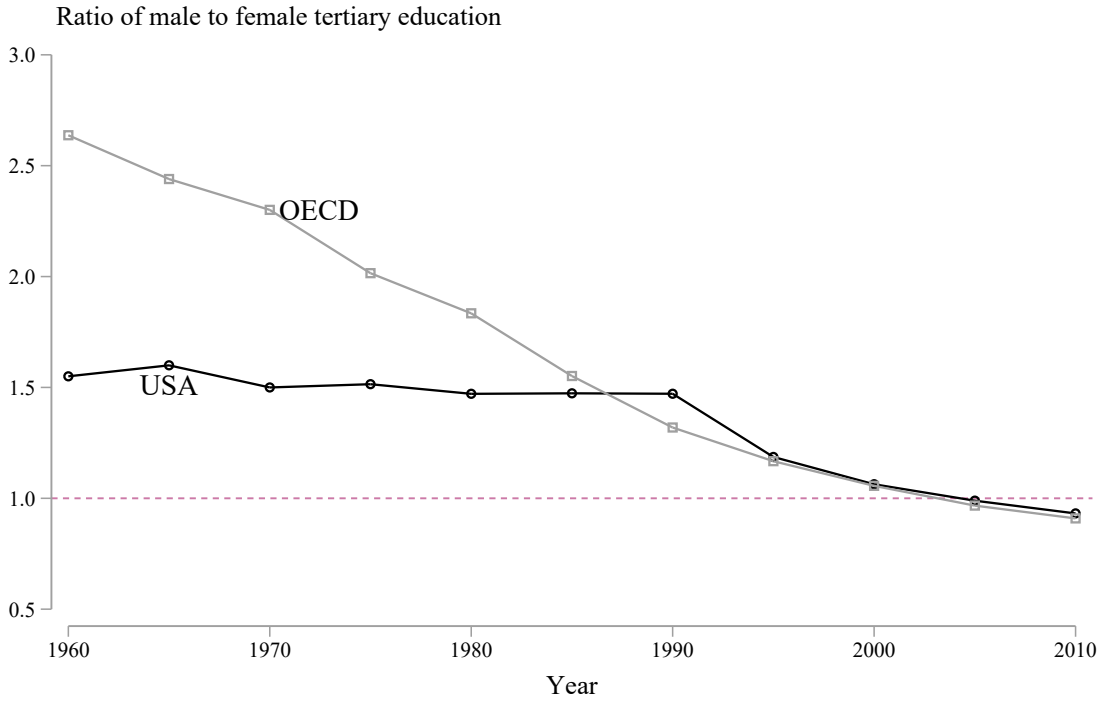
This table shows the coefficients of marriage market exposure in separate regressions where the dependent variable is the one indicated in each row. Sample is restricted to individuals not directly exposed to the reform, and divided into men and women with low (at most secondary education) and high (more than secondary education) education level. Standard errors (in parentheses) are clustered at the municipality of birth level. 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 women's health outcomes

	(1) All	(2) Low educated	(3) High educated
Mental health	-0.004	-0.009	0.002
	(0.009)	(0.011)	(0.011)
Mean of Y	0.07	0.08	0.05
N	154547	89037	65510
Alcoholic liver	-0.008	-0.018**	0.005
	(0.007)	(0.009)	(0.010)
Mean of Y	0.05	0.06	0.04
N	154547	89037	65510
Substance abuse	0.003	0.005	-0.001
	(0.003)	(0.004)	(0.003)
Mean of Y	0.01	0.01	0.00
N	154547	89037	65510

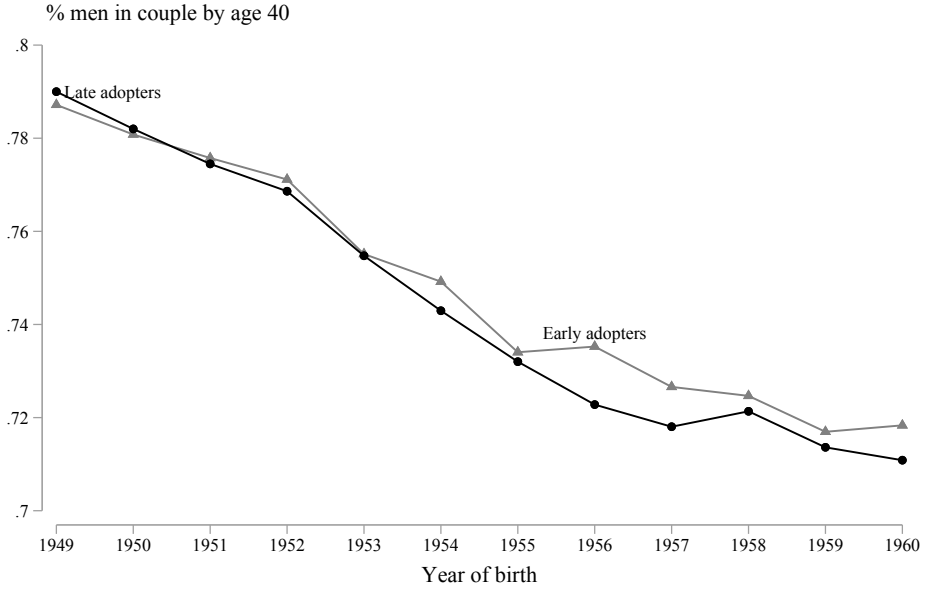
This table shows the coefficients of marriage market exposure in separate regressions where the dependent variable is the one indicated in each row, for all women in column (1), for those with low level of education (at most secondary) in (2), and for those with high level of education in (3), for those 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. Standard errors (in parentheses) are clustered at the municipality of birth level. 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

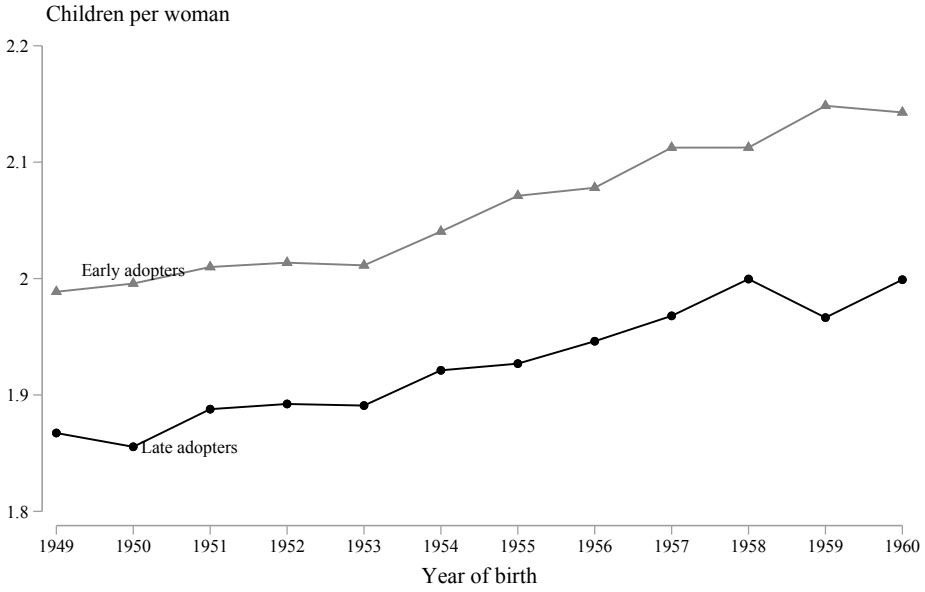


Notes: This figure shows the evolution of the ratio of the percentage of men to the percentage of women with tertiary education among the population aged 20-64 in the US (black line) and on average for OECD countries (gray line). Data from Barro and Lee (2013).

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



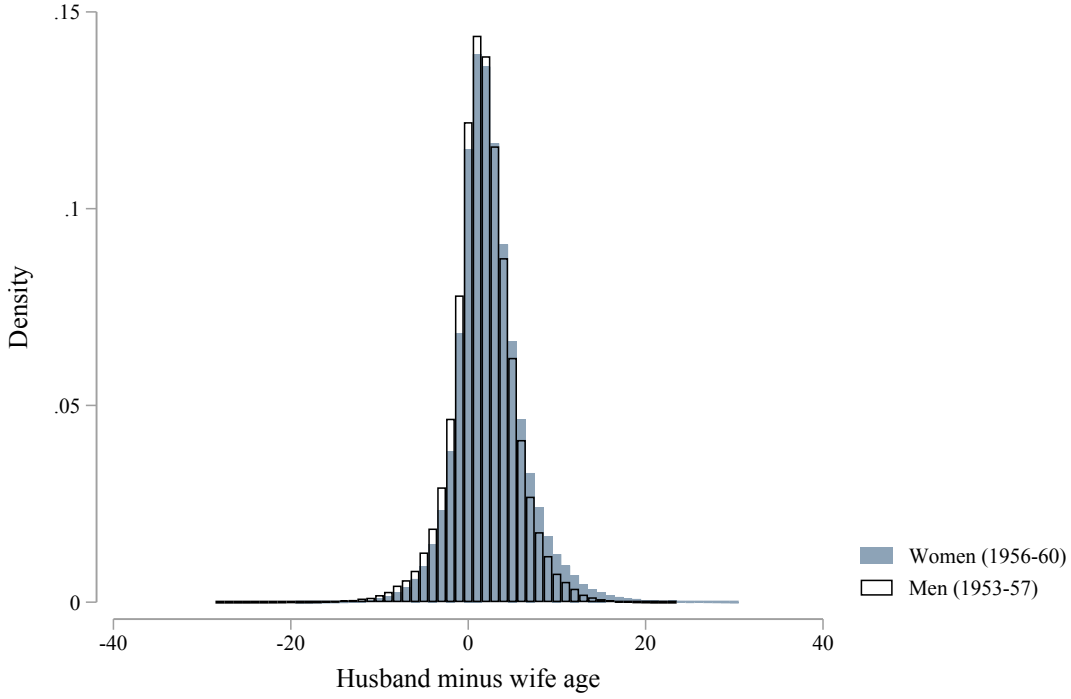
(a) Percentage of men married or cohabiting by age 40



(b) Average number of children per woman

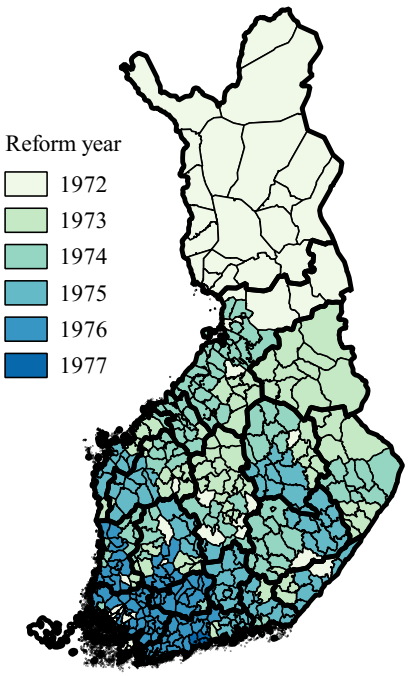
Notes: This figure presents the evolution of trends in fertility and marriage outcomes in early-adopter municipalities (those that implemented the reform in 1972-1974) and in late-adopter municipalities (those that implemented it in 1975-1977). Panel (a) shows the the percentage of men who were married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

Figure A3: Distribution of age difference between husband and wife in pre-reform cohorts



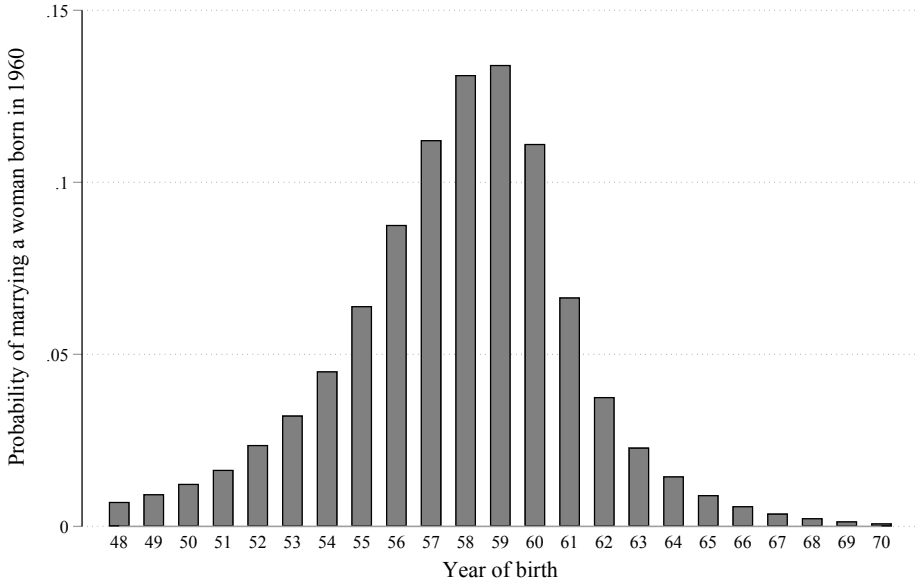
Notes: This figure shows the distribution of the age difference within married couples in pre-reform cohorts (1956-60 for women and 1953-57 for men).

Figure A4: Variation in year of reform implementation by municipality and region

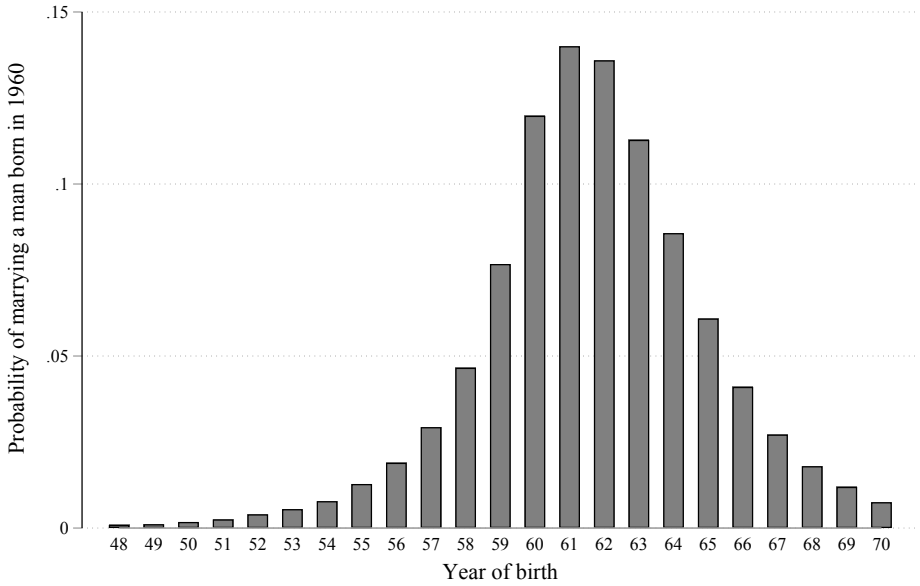


Notes: This map shows the year of adoption of the reform by municipality. Thicker lines indicate region boundaries.

Figure A5: Example of imputed probability of belonging to the marriage market – 1960 cohort



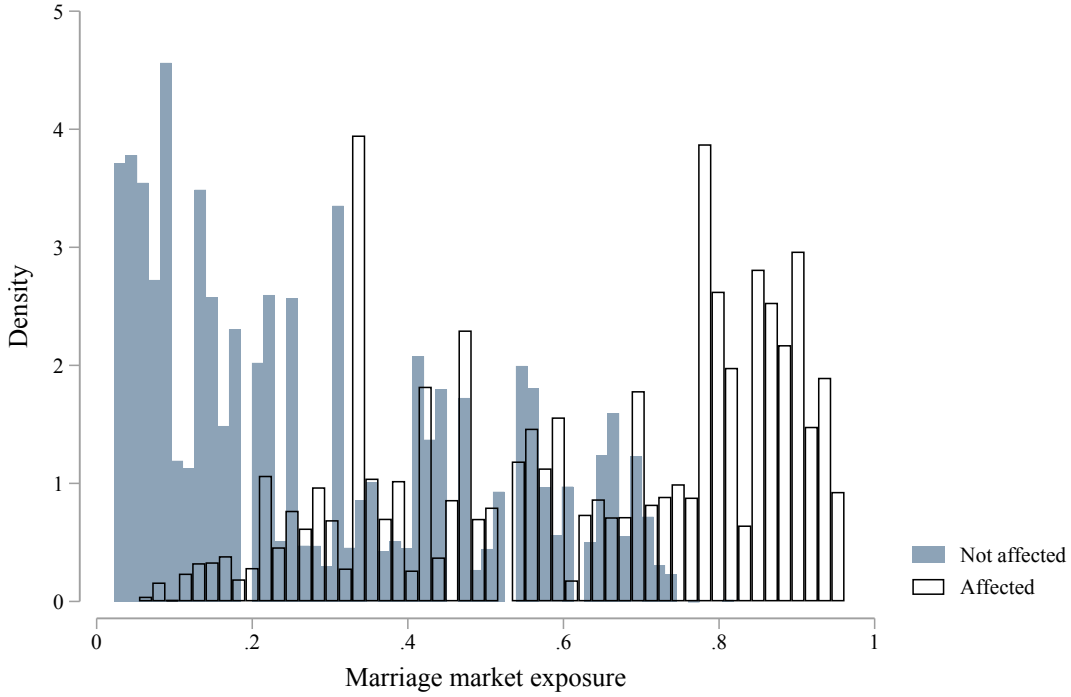
(a) Probability of belonging to the marriage market of a woman born in 1960



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

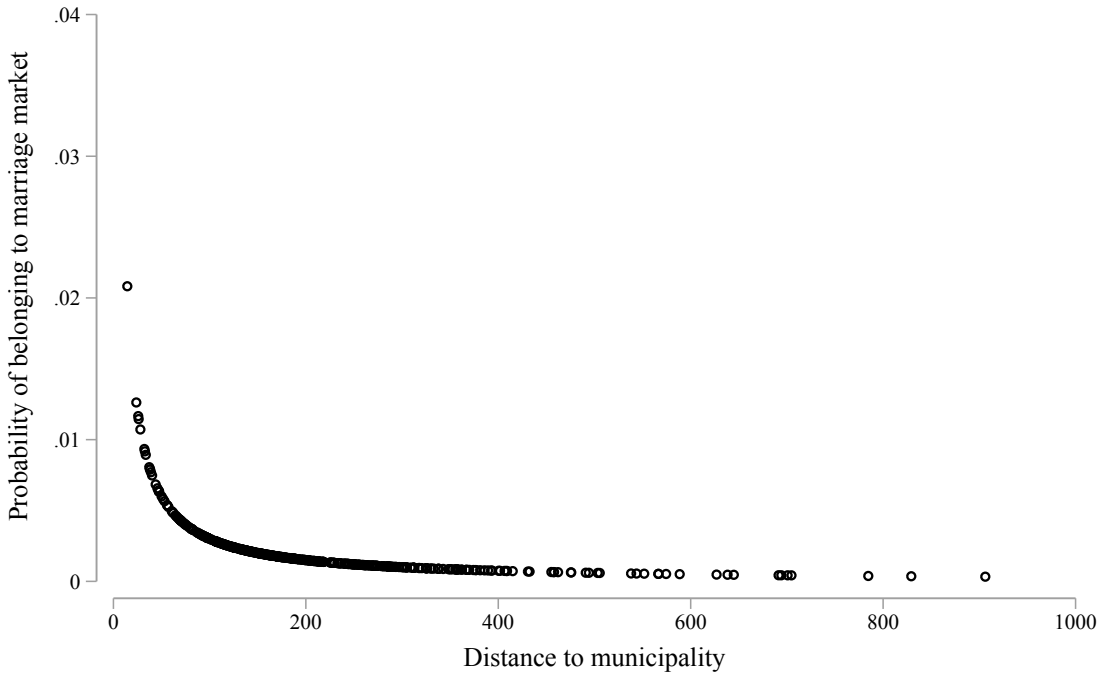
Notes: This figure represents the weight given to men and women of each cohort for constructing the marriage market of 1960 women in panel (a), and of 1960 men in panel (b). The calculation is based on the distribution of the age difference within couples in pre-reform cohorts (1956-60 for women and 1953-57 for men), which is shown in Figure A3.

Figure A6: Variation in the proportion of an individual's marriage market exposed to the reform for individuals affected and not affected by the reform themselves



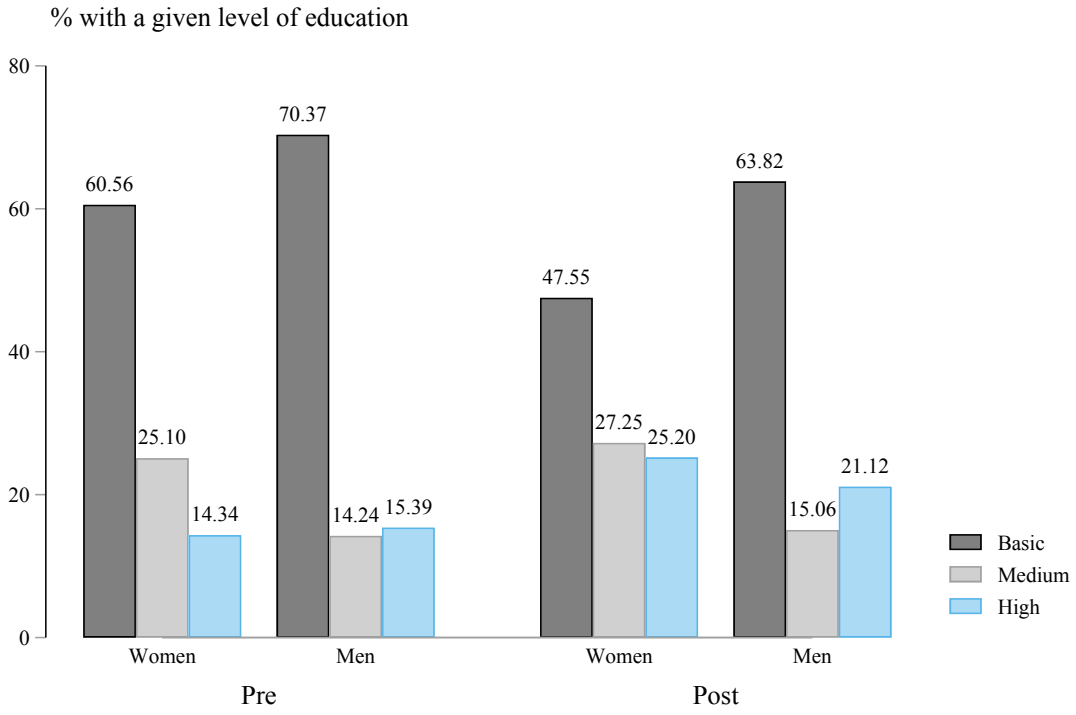
Notes: This figure shows the distribution of the variable marriage market exposure to the reform, separately for those directly exposed to the reform and those not exposed.

Figure A7: Probability of belonging to marriage market by distance between municipalities: Tampere (example)



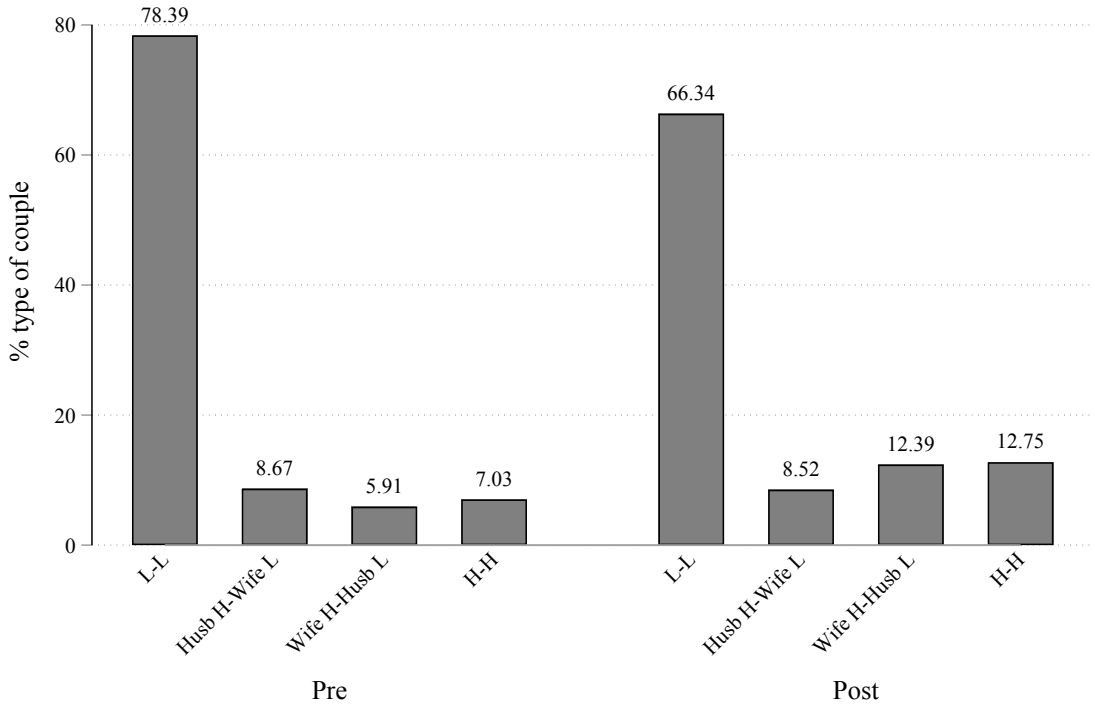
Notes: This figure plots the imputed probability for people in each municipality of belonging to the marriage market of individuals from Tampere (as an example). This probability is based on the inverse of the distance between each municipality and Tampere. Inverse distance probabilities are rescaled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

Figure A8: 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 A9: Frequency of different types of couples by relative education – pre- and post-reform cohorts



Notes: This figure plots the frequency of different types of couples, by relative level of education, in pre-reform (1956-60) and post reform (1966-70) cohorts. Couples are classified into four groups: couples where none of the spouses have a university degree (L-L), those in which both spouses have a university degree (H-H), couples where the husband has a university degree and the wife does not (Husb H-Wife L), and couples where the wife has a university degree and the husband does not (Wife H-Husb L).