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Preferences and opportunities in the marriage market.

How comprehensive schooling made the wealthy marry down

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Preferences and opportunities in the marriage market

How comprehensive schooling made the wealthy marry down[a]

by

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Abstract

This paper documents that a shift from a selective to a comprehensive education system had implications for marriage market outcomes. By exploiting an education reform in Sweden, I show that comprehensive education reduced assortative mating both because children from poor backgrounds started to marry up, and because those from wealthy backgrounds became more likely to find a spouse who had grown up in the bottom of the income distribution. The latter result is not explained by higher competition for wealthier partners, nor by increased partnership formation within the immediate peer group which offered more opportunities to meet partners from poorer backgrounds. Instead, the results point to the explanation that comprehensive education exposed the rich to a more diverse set of peers and therefore weakened their taste for homogamy. This finding suggests that familiarity among kids with different backgrounds may affect inter-group closeness and interactions in the long run.

Keywords: assortative mating; homogamy; education reform; peer group
JEL-codes: I24; I26; J11

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1 Introduction

Assortative mating is a persistent and well documented phenomenon that has recently spurred new interest in economic research.[1] In particular, assortative mating is quantitatively an important contributor to cross-sectional household income inequality (Eika, Mogstad, and Zafar 2019) and has implications for intergenerational mobility across generations (Chadwick and Solon 2002; Ermisch, Francesconi, and Siedler 2006; Holmlund 2022). It has also been suggested that the prevalence of mating across socio-economic or ethnic boundaries is an indicator of a society's level of tolerance and integration (Fryer 2007). Understanding mating patterns is therefore highly important – because of its possible consequences for inequality, social integration, and the accumulation of skills in the offspring generation.

Marital sorting, whether by education, income, parental background or ethnicity (or any other trait), is often explained as the result of three factors: preferences, opportunities (i.e. constraints in the marriage market) and "third party" interference in the selection process (Kalmijn 1994; 1998). In a Western context, individuals have often met their partner within networks created through education institutions, workplaces or friends.[2] Such networks set opportunity constraints, i.e. they define the pool of potential partners, but networks may also shape preferences for homogamy and affect assortativeness among spouses beyond direct opportunity constraints (Mare 1991; 2011). The structure of for example education institutions and workplaces may therefore have consequences for marriage market outcomes. The purpose of this paper is to provide evidence on exactly this topic by studying an education reform, and by answering the following questions: How does a major change to the education system affect assortative mating? Whose behaviour was affected and through which mechanisms?

The paper studies how a shift from a selective two-tier education system to an inclusive comprehensive system affected marital outcomes by exploiting quasi-random variation induced by a compulsory schooling reform in Sweden. The gradual implementation of the school reform allows for a difference-in-differences analysis (see e.g. Meghir and Palme (2005)), and to the best of my knowledge, this is the first paper to study this reform

[1] See e.g. Mare (1991) and Schwartz and Mare (2005) who document marital sorting in the U.S. in the 20th century.
[2] Online dating services have become a common meeting platform that arguably relaxes many of the earlier constraints in the market. See Hitsch, Hortaçsu, and Ariely (2010) and Rosenfeld and Thomas (2012) for studies that focus on this phenomenon.

using stacked-by-event analysis to address recent concerns with staggered differences-in-differences (Cengiz et al. 2019; de Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021).

The reform affected school-aged children mainly in two ways: compulsory education was prolonged by two years, and ability tracking was postponed. The latter implied that the reform forced students from different socio-economic backgrounds and with different abilities to attend the same schools for 3–5 more years in their early teens. Any reduced form effects of the reform on marital sorting should therefore be interpreted as the effect of changes in preferences and marriage market opportunities that arise as a result of the combined reform package: more compulsory schooling (with potential spill-over effects to higher education), postponed tracking, and an increase in exposure to peers from different socio-economic backgrounds. All-in-all, marriage outcomes can be affected through a variety of mechanisms, including a change in the immediate network while in compulsory school, changes to future networks that arise if the reform pushes individuals into further education and/or different workplaces, increased attractiveness in the marriage market through higher education, and finally through updated preferences regarding partner choice. While the empirical analysis in this paper cannot pin down the exact mechanisms for all, it is for some subgroups of the population – whose education and labour market prospects were largely unaffected by the reform – possible to get closer to an explanation.

Studying effects of an intervention on assortative mating is challenging since individuals in the control group operate on the same marriage market as the treated individuals. The sorting patterns in the control group might be affected by changes to marriage market competition and the relative supply and demand for spouses that occur as a result of the intervention. However, the marginal comparison of treated and control individuals that face the same pool of potential partners (i.e. close in age and geographic distance) will net out any supply and demand shifts from the estimate.

The paper draws on methods commonly used in the literature on intergenerational mobility to estimate marital sorting, in combination with difference-in-differences exploiting the gradual roll-out of the school reform. Marital sorting is quantified by estimating the between-spouse elasticity and rank correlation in father's earnings. In other words, assortative mating is defined on the basis of socio-economic background and not on individuals' own traits. This ensures that marital sorting is measured using pre-determined characteristics and that the reform effects on sorting are not mechanically related to the reform-induced changes to the education distribution.[3]

The first key finding of the paper is a decline in assortative mating over time, for the cohorts affected by the comprehensive school reform. For cohorts born in Sweden 1945–1955, the elasticity of income between fathers and fathers-in-law dropped from 0.21 to 0.17, and the corresponding numbers for the rank correlation are 0.21 and 0.19.[4] More importantly, the income relationship between fathers and fathers-in-law is highly non-linear. Marital sorting is relatively flat throughout the bottom and middle of the distribution, but very steep in the top quintile of the income distribution. Homogamy among the wealthy is thus prevalent in the sample, and it is also at the top that the decline in sorting is most visible.

The second key finding of the paper is that exposure to the new comprehensive school reduced marital sorting. The assortative mating elasticity and rank correlations fell by 9 and 7 percent respectively as a consequence of the reform, and this effect should be attributed both to individuals from low socioeconomic status (SES) backgrounds marrying up, and to those from high SES backgrounds marrying down. Interestingly, men from the very top of the income distribution came to marry down to a larger extent; this effect is explained by a 12 percent increase in the probability to marry women from the lowest quintile of the income distribution. This very selective group of individuals at the top of the distribution was to a large extent unaffected by the extension of compulsory education – they would have remained in education anyway – and there is no evidence of negative human capital effects which suggests that the results cannot be explained by lower marriage market attractiveness. Instead, this group faced a less advantaged peer group which supports that either opportunities or preferences may explain the increased prevalence to marry down. Since the results are fully explained by an increased probability of finding a partner with low SES background from a different municipality (and not at all by marrying down within the immediate peer group from the same municipality), they are consistent with a change of preferences in the long run. Although

[3] See Bratsberg et al. (2018) for a discussion on how to measure trends in assortative mating when the underlying distributions are not constant.
[4] This finding complements earlier research that has shown negative trends in assortative mating for the same cohorts using spouses' own characteristics (education and potential earnings), see Boschini et al. (2011) and Holmlund (2022).

suggestive, my findings are corroborated by evidence from other (arguably very different) settings showing that rich kids change attitudes towards the poor once they attend socially mixed schools (Rao 2019), and show that diversity in schools also may have long-lasting consequences for intimate interactions between individuals from different family backgrounds (see Merlino, Steinhardt, and Wren-Lewis (2019) for related evidence on mixed-race relationships). The results are also robust to a number of sensitivity tests and alternative explanations that are addressed in the paper. In particular, the results hold up when only using untreated units as controls in a stacked-by-event regression as suggested by Goodman-Bacon (2021), and when accounting for the fact that potential spouses were also affected by the reform.

Recent empirical research has given a lot of attention to the interplay between education systems and intergenerational transmission in the Nordic countries (Meghir and Palme 2005; Black, Devereux, and Salvanes 2005; Pekkarinen, Uusitalo, and Kerr 2009; Holmlund, Lindahl, and Plug 2011; Nybom and Stuhler 2014; Lundborg, Nilsson, and Rooth 2014; Fischer et al. 2018), but there is less causal evidence on how education systems contribute to patterns of marital sorting.[5] This paper fills a gap in the literature by addressing exactly this question, and by extending our understanding of how the education system can affect inequality.

The remainder of the paper is structured as follows. Section 2 presents the institutional background and the reform, Section 3 focuses on the data and sample restrictions, Section 4 presents descriptive evidence on sorting patterns in Sweden, Section 5 presents the empirical strategy and results. Finally, Section 6 offers conclusions. Supporting material and sensitivity analyses are available in the Appendix.

2 The Swedish context and education reform

The Swedish educational reform increased the compulsory years of schooling from seven to nine years and implied a shift from a selective two-tier system to a comprehensive school system. This section provides a description of the reform; more details can be found in Holmlund (2020).

[5] One exceptoin is Kaufmann, Messner, and Solis (2015) who study admissions to elite university programs and find that being admitted has positive effects on spouse quality. There is also a large theoretical literature on assortative mating, education and preferences, see e.g. Chiappori, Salanié, and Weiss (2017) and Chiappori, Dias, and Meghir (2018).

In the selective system, all students went through grades 1 to 4 or 1 to 6 in primary school (folkskolan). From fourth or sixth grade, more able students were selected (based on their GPA) to attend the five or three/four-year junior-secondary school (realskolan). This was the high tier of the education system. Remaining students continued their education in the low tier until compulsory education was completed. In most cases, compulsory education comprised of seven years, but in some municipalities, mainly the big cities, the minimum was eight years.

In 1948, a parliamentary committee suggested to introduce a nine-year compulsory comprehensive school, where students of all abilities and social backgrounds were to attend the same schools and classes longer than in the earlier school system. As a compromise between the opponents of early tracking and its advocates, the committee proposed tracking in 9th grade; pupils would follow either a vocational track, a general track, or a theoretical track preparing for upper-secondary school. The 9th grade streaming was later abandoned in favour of a completely comprehensive system.

The purposes underlying the proposal were among others to postpone the tracking decision to higher grades, in an effort to increase equality of opportunity, and to meet the demand for junior secondary education among the baby boom cohorts of the mid-1940s. To evaluate the appropriateness and whether the proposed nine-year comprehensive school would serve its purpose, the committee suggested that the comprehensive school should be tested during an assessment period, in a number of selected municipalities.
The assessment programme started in 1949/1950. The new comprehensive school was introduced throughout an entire municipality, or in certain schools within a municipality. 14 (of about 1000) municipalities were selected for the first year of the assessment (1949/50).

The following years, the National Board of Education continued a gradual implementation of the comprehensive school. Year by year, more municipalities joined the reform assessment programme. In 1962, the parliament came to a final decision to permanently introduce the nine-year school throughout the country. At this point, the implementation came to be a matter for each municipality; by 1969 they were obliged to have the new comprehensive school running. Since the timing was much in the hands of each municipality, the implementation was far from a randomized experiment, but nevertheless provides a source of variation in schooling laws that may be explored with a differences-in-differences approach.

The school reform affected cohorts born 1938–1955. Among the first cohorts affected, the roll-out was small scale and very slow. Holmlund (2020) describes how reform status can be matched to individuals and that assignment based on the 1960 and 1965 censuses introduces measurement error for cohorts born before 1945. The target sample of this study is therefore cohorts born 1945–1955. Figure A 1 shows how the fraction treated individuals increases over time. Individuals with unknown treatment status are dropped from the sample.

3 Data

The paper uses data compiled from registers held by Statistics Sweden. The multi-generation register identifies individuals residing in Sweden at some point after 1960, and all their parents and children. The population of interest, individuals born in Sweden in 1945–1955, is extracted from this register. The spouses/partners of these individuals are identified as the "other" biological parent of an individual's first-born child. In the Swedish context, where it is common that partners live together and have children without being formally married, this is the most common definition of a spouse/partner used in empirical analyses.[6] Although a non-negligible fraction of these relationships will break up (and new relationships will form), the other parent of the first-born child is still a relevant choice since it will determine the economic and social context of the child's life. Throughout the paper, the terms partner/spouse and partnership/marriage will be used interchangeably to describe the partnerships defined through the first-born child.

After defining the population of interest and identifying spouses, information from income tax registers, education registers, military enlistment registers and censuses is merged to the data. Below, the main variables of the data are described in more detail.

Father's income. Father's income (and income of fathers-in-law) is calculated as the mean over total labour income observed in 1968, 1971 and 1973, including zero incomes. Mean incomes are residualised netting out variation related to father's birth year, in order

[6] See e.g. Boschini et al. (2011) and Holmlund (2022) for previous applications.

to remove life-cycle variation in earnings. Percentile ranks of father's income are obtained by ranking each father's average income within his cohort-specific distribution.

Ideally, father's income should be measured before treatment. 1968 is the earliest observation available in the register data, and using the 1968, 1971 and 1973 income years thus relies on the assumption that the schooling reform targeting the offspring generation did not affect their fathers' future incomes, an assumption that is supported by balancing tests in Appendix Table A 2. Moreover, the incomes are observed rather late in fathers' careers, and it is necessary to assume that late-career income observations are good proxies for long-run income. Although Nybom and Stuhler (2016) show that rank-based measures are less sensitive to attenuation and stable to age-earnings variations than the elasticity, there is still a concern these income years do not proxy well for socioeconomic background and will bias the estimates of assortative mating. The paper discusses these issues further in Section 4 and Section 5.

Outcomes. A number of outcomes are investigated in order to shed light on potential mechanisms. Human capital outcomes are explored using years of schooling (derived using highest achieved level observed in the education register) and men's cognitive and non-cognitive skills observed at military conscription. The military enlistment data include IQ test scores and a psychological profile and are described in detail in Lindqvist and Vestman (2011).[7] The IQ and psychological profile scores are normalized to mean zero and unit variance.

Effects on marriage market outcomes are addressed by studying the socioeconomic background of the spouse, i.e. log income and income rank of the father of the spouse, in combination with geographic indicators of whether spouses come from the same or different municipalities as the index individual. The terminology SES/income background will be used interchangeably throughout the paper. Additional hypotheses are explored by studying the supply of high SES spouses and the social mobility of spouses. These variables are described in detail in Section 5.2.

[7] Military enlistment took place at age 18 or 19, and enlistment was universal for all men at the time. The IQ test consists of four different parts (synonyms, inductions, metal folding and technical comprehension), each of which is graded on a scale from 1 to 9. These scores are transformed into a general measure of cognitive ability with values 1 to 9, following a normal (Stanine) distribution. The psychological profile is based on a 25-minute long personal interview with a psychologist, who as a basis for the interview has information on the conscript's results from the IQ and physical fitness tests, school grades, and answers from a questionnaire on life outside the military (family, friends etc.). The psychological profile has the purpose to capture the individual's ability to cope with the military service, and characteristics such as responsibility, independence, persistence, emotional stability and social skills are highly valued. The psychological assessment is also graded on a Stanine scale from 1 to 9.

Peer group composition. To assess whether the reform implied exposure to peers from different socio-economic circumstances, exposure to peers from low socioeconomic background (<= 20th percentile in father's earnings distribution) is calculated as the share of low SES peers in cells defined by cohort, municipality and reform status in combination with reported education level in the 1970 census. Post-reform cohorts are assigned to the full peer group defined by municipality and cohort since all students attended the comprehensive school. Pre-reform cohorts that were in the two-tier selective system are assigned to peer groups that in addition are defined by education group: basic education (folkskola) or post-compulsory education (realskola or above). This is a rough measure of peer groups since it is not observed at the school level but is useful for understanding to what extent exposure to peers from different family backgrounds was affected by comprehensive schooling.

Comprehensive reform indicator. The analysis of the effects of comprehensive schooling is based on cohorts born 1945–1955 who were exposed to the gradual roll-out across Sweden's about 1,000 municipalities. The sample is mapped to treatment status by birth cohort and municipality of residence in the 1960 (cohorts born 1945–1950) or 1965 (cohorts born 1951–1955) census. Holmlund (2020a) provides a detailed description of the data collection and the mapping of individuals to the reform.

Sample restrictions. The analysis sample is restricted to individuals born in Sweden to which it has been possible to assign reform status. In addition, fathers (and fathers-in-law) must have at least one income observation in the years 1968, 1971 or 1973, and fathers must be aged between 15 and 45 at the birth of their child. This is to ensure that the sample has the same age structure over the full time period. Table A 1 in the Appendix presents descriptive statistics of the estimation sample.

4 Socio-economic homogamy: descriptive evidence

This section presents evidence on the existence of assortative mating for cohorts born in Sweden 1945–1955. Marital sorting is estimated using the following equation:

$$y_i^{sp} = \alpha + \beta y_i + \varepsilon_i \qquad (1)$$

where y_i^{sp} is a measure of the income background of the spouse, and y_i represents the income background of the index individual. Using logs and ranks of fathers' (and fathers'-

in-law) income, β will represent the assortative mating income elasticity/rank correlation (see Bratsberg et al. (2018) for a similar assortative mating statistic).

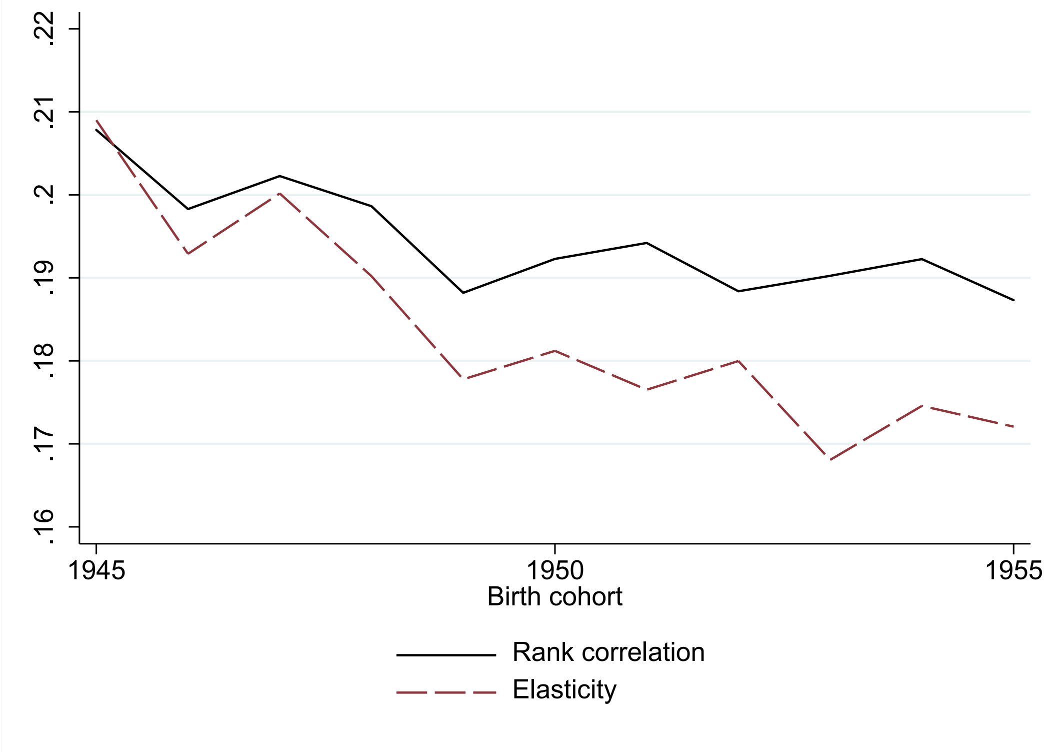
Figure 1 shows the time trends of the elasticity and rank correlation for cohorts born 1945–1955 and indicates a negative trend over time. Figure 2 presents kernel-plots of the income of the father-in-law over the distribution of father's income for the 1945 and 1955 cohorts. Both the elasticity and rank correlation are at 0.21 in the 1945 cohort, and drops to 0.17 and 0.19 in the cohort born 10 years later.[8] As is evident from Figure 2B, marital sorting is weak in the bottom and middle of the distribution but increases sharply in the top quintile, and the drop in sorting should be fully attributed to a change in marriage patterns at the top quintile as children from well-of backgrounds started to marry down to a larger extent.

When studying trends over time, the quality of the income measure is of particular concern. Late-career observations might be a poor proxy for long-run income and introduce attenuation bias (Solon 1999). This would imply that for earlier cohorts (which have older parents when income is observed), the sorting parameters are estimated downwards. It is thus possible that the negative trend is underestimated – and unlikely that the bias reverses the direction of the trend. The drop in marital sorting over time is further corroborated by studies that have focused on sorting by offspring characteristics such as education and potential earnings (Boschini et al. 2011; Holmlund 2022). The decline at the top of the distribution when characterizing sorting by family background is however a new finding, and differs from the pattern shown in Holmlund (2022) which describes sorting in terms of own earnings potential (e.g. predicted earnings). A similar negative trend has also been found in Norway (Bratsberg et al. 2018).

The 1945–1955 cohorts cover the main expansion period of the new Swedish compulsory school, moving from a selective to a comprehensive system. In the 1945 cohort, only 6 percent were in the new comprehensive system, whereas virtually all in the 1955 cohort belonged to the new system. Can the drop in socio-economic homogamy at the top of the distribution to some extent be attributed to this change in the education system? The remainder of the paper explores this hypothesis and tries to shed light on a number of potential mechanisms and alternative explanations.

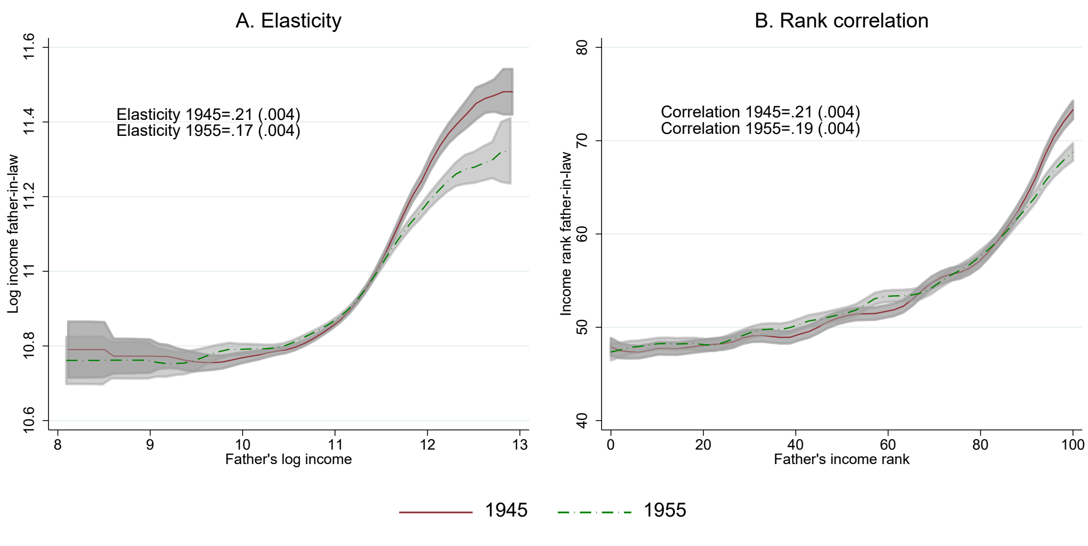
[8] These estimates are not far from the corresponding measures of intergenerational earnings mobility in Sweden (Nybom and Stuhler 2016).

Figure 1 Trend in assortative mating



Note: The trends are based on the yearly estimates of elasticity/rank correlation between earnings of fathers and fathers-in-law.

Figure 2 Assortative mating by parental background, cohorts born 1945 and 1955



Note: The numbers indicate the elasticity/rank correlation between earnings of fathers and fathers-in-law. The graphs show kernel-plots of the log/rank income of fathers-in-law over that of fathers. Grey areas indicate 95 percent confidence intervals.

5 Empirical strategy and results

The empirical strategy relies on the staggered implementation of the school reform, where each year additional municipalities shifted from the selective to the comprehensive system.

Equation (1) presents the baseline regression interacting father's income with reform status in a difference-in-differences specification:

$$y_{itm}^{sp} = \alpha + \beta_1 y_{itm} + \beta_2 R_{tm} + \beta_3 R_{tm} \times y_{itm} + \theta_t + \gamma_m + \Phi(\theta_t \times y_{itm}) + \Gamma(\gamma_m \times y_{itm}) + \varepsilon_{itm} \quad (2)$$

y_{itm}^{sp} is the earnings rank/log of father-in-law of individual i, of cohort t, growing up in municipality m. y_{itm} is earnings rank/log of own father, R_{tm} is a 0/1 indicator that takes the value 1 for municipalities and cohorts that had implemented the reform. The model is fully interacted such that cohort and municipality-fixed effects θ_t and γ_m are also allowed to vary by y_{itm}. The parameter of interest is β_3, which shows how the assortative mating parameter is affected by the school reform. The specification also includes linear time trends interacted with implementation year.

The difference-in-differences design relies on the assumption that treated and control areas would follow similar trends in the absence of the reform. While this assumption is untestable, balancing tests and pre-reform 'placebo' estimates show i) that the reform was not correlated with fathers' birth year, education, log income and income rank (see Appendix Table A 2) and b) that pre-reform trends were parallel (see Figure A 2 which presents estimates from an event-study specification). The fact that the reform is uncorrelated with father's birth year is particularly relevant since it implies that the amount of measurement error in father's income is likely to be balanced across treated and control individuals.

There are two additional (and interrelated) concerns regarding the empirical design that are important to address. The staggered difference-in-difference estimate constitutes a weighted average of all possible two-group/two-period DD estimators in the data (de Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021). First, since the characteristics of potential partners and social norms regarding homogamy might be trending with time, younger post-reform cohorts likely faced a different marriage market compared to the older and first affected cohorts within a municipality. If such trends are municipality-specific and not fully captured by the control group, the comparison of cohorts born far apart could pick up that the marriage market has evolved, an effect that should not be attributed to the reform. Second, recent papers highlight some difficulties with pooled/staggered difference-in-differences. The pooled estimator builds on the assumption that the treatment effect is constant while event-time treatment effect heterogeneity is a possibility. Using early-treated municipalities as controls for late-treated municipalities can lead to a biased estimate if there are time-varying treatment effects (Goodman-Bacon 2021). To address these concerns, I present results from a stacked-by-event regression on a restricted sample, where in each municipality I compare the first two reform cohorts with the adjacent two pre-reform cohorts, and only use untreated municipalities as controls.[9] This comparison within a set of four consecutive cohorts serves to alleviate concerns that early vs. late reform cohorts might be facing different marriage markets (under the assumption that the marriage market is stable over four years) and ensures that only 'clean' controls are used. In addition, I present event-study estimates of the baseline effects and show how they evolve over time.

A final concern, which relates to interpretation rather than internal validity, is the fact that the potential partners on the marriage market were also affected by the education reform. Their attractiveness and/or preferences are therefore also likely to change. I return to this issue in section 5.2 when I discuss mechanisms.

5.1 Baseline results

Table 1 presents the baseline assortative mating parameters and reform interactions for elasticities and rank correlations, respectively. Column 1 confirms the magnitudes of marital sorting by father's income that we observed in Figure 2. Column 2 includes the interaction between reform exposure and father's income and shows that comprehensive schooling reduced the elasticity of marital sorting by 0.013; about 9 percent. Similarly, panel B shows that the rank correlation dropped by 0.011 or 7 percent. These findings support the hypothesis that long-run partnership formation was affected and that marital sorting declined as a consequence of the reform. The coefficients indicate that the school reform can explain about 30 (55) percent of the decline in the assortative mating elasticity

[9] See e.g. Cengiz et al. (2019) for a previous application. For each implementation cohort q, only control municipalities which implement in q+2 or later are retained. Stacking the data (i.e. the treated and their "clean" control municipalities) for each implementation cohort q and keeping only cohorts born between q-2 to q+1, the stacked-by-event regression incorporates interactions with all controls variables in Equation 2 and event q: $y^{sp}_{itmq} = \alpha + \beta_{1q} y_{itmq} + \beta_2 R_{tmq} + \beta_{3q}(R_{tmq} \times y_{itmq}) + \theta_{tq} + \gamma_{mq} + \Phi_q(\theta_t \times y_{itmq}) + \Gamma_q(\gamma_m \times y_{itmq}) + \varepsilon_{itmq}$.

(rank correlation) over the 1945–1955 cohorts that was observed in Figure 1 and Figure 2.

Column 3 presents the corresponding estimates using a stacked-by-event regression on the restricted sample (using only untreated observations as controls and comparing cohorts born +/- 2 years around the reform year), and shows that the results remain unchanged. Columns 4 and 5 show results split by men and women; the effects are present among both women and men but estimated with more precision in the male sample. Table A 3 in the Appendix shows that these results are also robust to a specification without time trends interacted with implementation year.

Figure A 2 in the Appendix presents time-varying estimates in an event study (without and with time trends), where t-2 is the baseline year. First, the pre-reform estimates are all close to zero and not statistically significant. Second, Panels A and C show that the post-reform effect sizes are relatively constant over time when estimating the reform impact on the elasticity, while Panels B and D show that the effects are less stable for the rank correlation with smaller estimates (in absolute terms) for later cohorts. As emphasized above, it is however the short-term impact is likely to be the more relevant comparison in this application (since marriage markets are more comparable).

Table 1 Effects of school reform on assortative mating

	(1)	(2)	(3)	(4)	(5)
	Full sample		Restricted sample & Stacked-by-event regression	Full sample Men	Full sample Women
			A. Log income father-in-law		
Log income father	0.181***	0.144***		0.128***	0.183***
	(0.010)	(0.017)		(0.018)	(0.027)
Reform exposure x		-0.013***	-0.013**	-0.014**	-0.012*
Log income father		(0.005)	(0.006)	(0.006)	(0.007)
Observations	605,829	605,829	1,090,446	314,925	290,904
R-squared	0.033	0.020	0.055	0.018	0.021
Control group mean		10.86	10.85	10.84	10.88
			B. Income rank father-in-law		
Income rank father	0.192***	0.159***		0.160***	0.163***
	(0.008)	(0.011)		(0.014)	(0.014)
Reform exposure x		-0.011***	-0.013**	-0.014**	-0.008
Income rank father		(0.004)	(0.006)	(0.006)	(0.007)
Observations	616,086	616,086	1,108,966	320,123	295,963
R-squared	0.037	0.019	0.060	0.018	0.020
Control group mean		51.49	51.27	50.80	52.24
Number of municipalities		1,020	971	1,020	1,020
Municipality f.e.		Yes	Yes	Yes	Yes
Cohort f.e.		Yes	Yes	Yes	Yes
Full interaction		Yes	Yes	Yes	Yes

Note: Regressions in columns 1 and 2 include controls for gender. Regressions in columns 2–5 include the following controls: main reform effect, municipality and cohort-fixed effects (main effects and interactions with father's income), reform implementation year-fixed effects (main effect and interaction with father's income and interaction with linear time trend). The restricted sample includes only cohorts born +/-2 years around the reform year in each municipality and only uses untreated municipalities as controls in a stacked regression (see section 5 for details). Robust standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

The next step of the analysis is to understand whose marriage patterns have changed. Motivated by Figure 2 and the drop in sorting at the top of the distribution, it is particularly interesting to focus on the top 20 percent of the distribution. Table 2 presents reduced-form reform effects on partner choice, defined by the log/rank income of the father-in-law, for different subgroups (below and above median in father's income distribution, and the top quintile). The upper panel shows that for children growing up with fathers belonging to the bottom half of the income distribution (<p50), reform exposure did not affect partner choice in terms of log income. The lower panel however tells us that in terms of income ranks, reform exposure has "improved" marriage market outcomes and lead to spouses from slightly more advantaged backgrounds (by 0.5 percentile ranks). The marriage patterns do not seem to have been affected on average in the upper half (>=p50) of the income distribution. But by focusing on the top quintile (where sorting is particularly strong as depicted in Figure 2) it turns out that reform exposure has implied a shift to marrying individuals from lower income groups. The effects correspond to having a father-in-law with 1.8 percent lower income (panel A, column 3) or 0.65 lower earnings rank, a drop of about 1 percent (panel B, column 3). The effects in panel B are stronger among men, but the overall pattern is similar for men and women.

Attending comprehensive schooling made children from the wealthiest families marry down, but not unexpected, we also see a tendency that low SES groups married up. How should this result be interpreted? If the marriage market is closed (no entrants or exits), could the two-sided nature of the market lead to a 'summing-up' constraint, where e.g. the increased opportunities of the poor to marry the rich forces the rich to marry down? Under the assumption that treated and untreated cohorts (and the control group) operate on the same marriage market and choose their partners from the same pool of people, the competition for partners will affect all these individuals equally. The marginal comparison of adjacent treated and control cohorts (close in age) will net out any supply and demand shifts from the estimate. In other words, even if the 'summing-up' constraint does affect marriage outcomes, it does so equally for treated and control individuals, and will not mechanically introduce mirror image estimates at the bottom and top of the distribution.

If, however, partnership formation to a large extent is cohort specific, the assumption that adjacent cohorts face the same marriage market is flawed. But by maintaining the assumption that a specific cohort, across treated and control municipalities, is part of the same marriage market – and thus similarly affected by shifts in supply and demand – it is still possible to hold constant the 'mechanical' effects.

Although these assumptions are hard to verify, we can indirectly address them by narrowing down the comparison of treated and control cohorts, and control areas, to those that are more likely to be part of the same marriage market. Table A 4 (panels A and C) presents estimates (corresponding to those in Table 2), using the restricted sample and stack-by-event specification. By comparing cohorts close in age, it is more likely that they face the same pool of potential partners. Panels B and D extend the specification by introducing controls for event time*cohort*county, which means that the effects are estimated by comparing municipalities within the same county (the county is a larger geographic region).[10] The comparison serves to limit the control group to municipalities that are geographically close, and thus more likely to constitute a joint marriage market.

The results in Table A 4 are remarkably stable and show that a much tighter comparison – both in time and space – does not alter the estimates. It therefore seems unlikely that the findings at the top of the distribution should solely be attributed to higher competition for high SES partners (see also section 5.2 for further robustness tests related to the supply-side mechanism).

Although the average effects presented in Table 2 and Table A 4 are small, some interesting patterns emerge if we study the probability to marry into different quintiles of the in-law distribution (see Table A 5). It turns out that the top quintile exposed to the reform increased their probability to marry individuals growing up in the 20 percent poorest families. For men, where the effect is strongest, it implies an increase by 12 percent. Table A 5 also presents evidence that children growing up in the bottom half of the distribution shifted from partners in the second quintile to partners in the fourth quintile.

To sum up, the evidence in Table 1 and Table 2 points to intriguing conclusions regarding the effects of an education reform on assortative mating. Since the reform targeted the low educated and increased years of schooling the most at the bottom of the distribution (Meghir and Palme 2005), it is not surprising that those from low income backgrounds also ended up with more advantaged spouses. What is more surprising is that the wealthiest started to marry down. The next section explores potential mechanisms that may explain these shifts in partner choice.

[10] Sweden was divided into 25 counties in the relevant time period.

Table 2 Reform effects on partner choice (defined by income of father-in-law). Heterogeneous effects by father’s income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<p50	>=p50	>=p80	<p50 Men	>=p50 Men	>=p80 Men	<p50 Women	>=p50 Women	>=p80 Women
					A. Log income father-in-law				
Reform exposure	0.005	-0.004	-0.018***	0.007	-0.002	-0.018**	0.002	-0.007	-0.019**
	(0.004)	(0.004)	(0.007)	(0.006)	(0.005)	(0.009)	(0.006)	(0.006)	(0.010)
Observations	280,536	325,293	129,163	144,940	169,985	68,318	135,596	155,308	60,845
R-squared	0.000	0.001	0.003	0.000	0.000	0.001	0.000	0.001	0.003
Control group mean	10.78	10.94	11.08	10.77	10.92	11.06	10.79	10.96	11.12
					B. Income rank father-in-law				
Reform exposure	0.491**	-0.158	-0.648**	0.453*	-0.141	-0.921**	0.479*	-0.176	-0.419
	(0.197)	(0.193)	(0.330)	(0.265)	(0.282)	(0.452)	(0.290)	(0.255)	(0.423)
Observations	288,190	327,896	130,102	148,842	171,281	68,788	139,348	156,615	61,314
R-squared	0.001	0.001	0.001	0.000	0.000	0.000	0.000	0.001	0.001
Control group mean	47.53	55.41	61.79	46.88	54.63	60.87	48.23	56.27	62.85
Number of municipalities	1,020	1,020	1,019	1,020	1,020	1,017	1,020	1,020	1,019
Municipality f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Regressions include the following controls: main reform effect, municipality and cohort-fixed effects, and reform implementation year-fixed effects interacted with a linear time trend. Robust standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

5.2 Mechanisms

How can the effects on marital sorting be reconciled? Did the reform change individuals’ attractiveness in the marriage market as an indirect consequence of its effects on human capital? Did it push individuals into higher levels of education beyond compulsory schooling, which in turn exposed them to new peer networks? Or did the new peer group in compulsory school, with a mix of children from different socioeconomic backgrounds, affect the short- and/or long-run peer networks in which partnerships are formed? Finally, did familiarity with children from other social circumstances change preferences for homogamy, in particular among the rich where marital sorting was so strong?

The analysis in this section offers supporting evidence that is helpful for disentangling some of these mechanisms. But before exploring the mechanisms, I begin by ruling out that the reform changed marriage outcomes more generally. Table A 6 presents evidence that the reform did not affect the probability of having children (and in this setting therefore the probability to be observed in the sample with a spouse), the birth year and age difference between spouses, nor age at first birth.

Table 3 presents findings on how the reform affected long-run human capital outcomes. Panel A shows that the reform effect on years of schooling is much larger at the bottom of the distribution, and in general also larger for men than for women. Men in the top quintile of father's earnings distribution also benefitted from the reform, but by no means as much as those in the bottom. As mentioned above, a likely outcome for the low-income groups is that increased human capital boosted their marriage market prospects, through increased attractiveness or a change of network. For this group it is therefore not possible to separate whether effects should be attributed to human capital formation and marriage market attractiveness, or to a change in behavior as a consequence of new marriage market opportunities or updated preferences.

Turning to the top of the distribution, one potential explanation for why the wealthy started to marry down is that the comprehensive system negatively affected their human capital through lower education quality or through socio-emotional stress related to the new school organization or to loss of status. Lager et al. (2017) study the effects of the reform on men's cognitive skills and emotional control and find negative effects on emotional control among men from high SES backgrounds. This finding is corroborated by Böckerman et al. (2020) who study the Finnish comprehensive school reform and find that women from highly educated families that went to comprehensive school are more likely to suffer from depression. One possible explanation is therefore that the reform reduced cognitive and non-cognitive skills among the economically advantaged children in a way that lead to lower attractiveness in the marriage market. Using data from Swedish military enlistment for a subset of my sample (men born 1950–1955) I can address this potential explanation. Panels B and C of Table 3 show that there are no negative effects on either cognitive or non-cognitive skills for the top quintile, which suggests that lower attractiveness is not a likely explanation. Next, I therefore explore several outcomes that shed light on opportunities and preferences.

Table 3 Reform effects on human capital outcomes. Heterogeneous effects by father's income.

	(1)	(2)	(3)	(4)	(5)	(6)
	<p50 Men	>=p50 Men	>=p80 Men	<p50 Women	>=p50 Women	>=p80 Women
			A. Years of schooling			
Reform exposure	0.420***	0.209***	0.085**	0.278***	0.096***	0.018
	(0.025)	(0.026)	(0.037)	(0.024)	(0.028)	(0.032)
Observations	148,842	171,281	68,788	139,348	156,615	61,314
R-squared	0.016	0.005	0.002	0.018	0.009	0.003
Number of municipalities	1,020	1,020	1,017	1,020	1,020	1,019
Muni f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Cohort f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Control group mean	10.37	11.74	12.77	10.85	11.92	12.82
			B. Cognitive skills (standardized)			
Reform exposure	0.007	0.036*	-0.011			
	(0.021)	(0.020)	(0.034)			
Observations	59,199	70,981	29,203			
R-squared	0.002	0.003	0.005			
Number of municipalities	1,000	995	966			
Muni f.e.	Yes	Yes	Yes			
Cohort f.e.	Yes	Yes	Yes			
Control group mean	-0.137	0.173	0.402			
			C. Non-cognitive skills (standardized)			
Reform exposure	-0.044*	0.002	0.014			
	(0.024)	(0.027)	(0.044)			
Observations	59,199	70,982	29,203			
R-squared	0.001	0.002	0.004			
Number of kommun60	1,000	995	966			
Municipality f.e.	Yes	Yes	Yes			
Cohort f.e.	Yes	Yes	Yes			
Control group mean	0.0203	0.214	0.334			

Note: Regressions include the following controls: main reform effect, municipality and cohort-fixed effects, and reform implementation year-fixed effects interacted with a linear time trend. Robust standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4 begins in Panel A, presenting effects on the peer group composition by focusing on the share of low SES peers at the cohort, municipality and school form level. Children growing up in the bottom half of the distribution and affected by the reform were on average exposed to a more positively selected peer group – the share of low SES peers (defined as the bottom 20 percent of father's income distribution) decreased by 1.6 percentage points or 9 percent for men. On the contrary, men in the top quintile were faced with an increase by 9 percent in share of low SES peers. The pattern is similar for women. These results are expected and serve mainly as a starting point for understanding how different groups were affected by the postponement of ability tracking.

Panel B explores the hypothesis that the reform induced individuals to move away from their home region (to a different county), which could have implied new networks and in turn affected partner choice. However, all coefficients are close to zero and precisely estimated, suggesting that this mechanism is not a likely explanation. Panel C explores the probability to find a spouse from the same municipality. There is a small and marginally significant negative effect for men in the lower half of the distribution, but the overall picture does not indicate any effects on the geographic origin of spouses.

Panels D and E explore the probability to marry low SES individuals (lowest 20 percent of the in-law distribution) from the same vs. a different municipality. The rationale fore this split is that it may shed light on opportunities vs. preferences. It turns out that the increased prevalence among men from a wealthy background to marry down cannot be explained by partner formation within the home municipality (Panel D) – instead the effect is almost fully explained by partnership formation with individuals from low SES backgrounds originating from other municipalities (Panel E). This result is *not* consistent with increased direct opportunities to meet a partner from a low-income background, since opportunities to meet should have increased to the same degree (or most likely even more) within the home municipality where children went to school together and formed their first networks. Although suggestive, the result is consistent with a change in attitudes and preferences when it comes to partner choice.[11] As emphasized above, the interpretation however relies on the assumption that reform-exposed and non-

[11] If preferences change it is to some extent surprising that it does not affect the probability to marry low SES individuals from the same municipality. As the baseline of this outcome is very low in column 3 (Panel D, Table 4) it however seems like the preference for marrying within the same municipality is very low to begin with – and unaffected by treatment.

exposed individuals on the margin were faced with similar marriage markets. In other words, the top 20 percent group across treated and untreated cohorts within the same municipality, should face potential marriage partners with similar characteristics.

One possibility is that wealthy reform-exposed individuals were pushed into a marriage market (even outside their own municipality) where there was a lower relative supply of potential partners from high-income households. Other explanations are that they met potential partners that were more socially mobile, or that the reform (which obviously also affected potential partners) made low SES individuals more attractive as spouses or reduced the signaling value of socioeconomic background. The latter explanation is particularly relevant for men, who often marry younger women who therefore are more likely to have attended the new comprehensive school themselves. Because of the geographic correlation between spouses and the proximity in age, there is a mechanical correlation in reform status between spouses, which could give rise to such a result.

Panels F and G explore these hypotheses by estimating the reform effect on the share of high SES opposite-sex individuals in the same age group (+/- 2 years) in the home municipality at age 25, and by estimating the reform effect on the social mobility of the spouse.[12] The results in Panel F show that the reform did not reduce the supply of high SES women available to wealthy men, but there is a small and marginally significant negative effect for women at the top of the distribution. In Panel G I adopt a social mobility index consisting of the residuals from a regression of spouse's years of schooling on gender and indicators for the income deciles of the father-in-law. This index is informative of spouse's upward/downward mobility with respect to his/her predicted education based on parental background. The baseline in the control group shows that children from low-income backgrounds tend to marry spouses who exhibit downward mobility, while those from wealthier backgrounds marry individuals who experience upward mobility. While the reform pushed low SES men to marry upwardly mobile spouses, there is no reform-induced difference in spouses' social mobility among high SES groups.

[12] High SES is for consistency defined as individuals whose father's income is above the 20th percentile in the distribution.

Table 4 Reform effects on peer group and partner characteristics. Heterogeneous effects by father's income.

	(1)	(2)	(3)	(4)	(5)	(6)
	<p50 Men	>=p50 Men	>=p80 Men	<p50 Women	>=p50 Women	>=p80 Women
			A. Share low SES school peers			
Reform exposure	-0.016***	0.002	0.009***	-0.008***	0.007***	0.012***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Control group mean	0.172	0.122	0.103	0.165	0.118	0.101
			B. Mobility out-of region			
Reform exposure	0.005	0.001	-0.003	0.004	0.008	0.005
	(0.004)	(0.005)	(0.007)	(0.005)	(0.005)	(0.008)
Control group mean	0.244	0.348	0.449	0.304	0.395	0.497
			C. Spouse from same municipality			
Reform exposure	-0.008*	-0.001	0.002	-0.002	-0.001	0.002
	(0.005)	(0.004)	(0.006)	(0.005)	(0.004)	(0.007)
Control group mean	0.228	0.244	0.233	0.229	0.255	0.241
			D. Low SES spouse from same municipality			
Reform exposure	0.000	-0.001	0.002	0.001	0.002	0.001
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
Control group mean	0.0419	0.0303	0.0229	0.0415	0.0309	0.0230
			E. Low SES spouse from different municipality			
Reform exposure	-0.004	0.003	0.011**	0.004	-0.002	0.002
	(0.004)	(0.003)	(0.005)	(0.004)	(0.003)	(0.004)
Control group mean	0.146	0.109	0.0890	0.137	0.101	0.0826
			F. Share high SES partners in residential location at age 25			
Reform exposure	0.000	-0.000	-0.000	-0.000	-0.001**	-0.001*
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)
Control group mean	0.855	0.867	0.874	0.858	0.871	0.878
			G. Spouse's social mobility index (standardized)			
Reform exposure	0.024***	-0.005	-0.014	0.013	0.014	0.005
	(0.009)	(0.009)	(0.014)	(0.013)	(0.012)	(0.018)
Control group mean	-0.175	0.104	0.330	-0.228	0.155	0.494
Municipality f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Cohort f.e.	Yes	Yes	Yes	Yes	Yes	Yes

Note: For brevity, information on number of observations, R-squared and number of municipalities included in the regressions have been omitted from the table. Regressions include the following controls: main reform effect, municipality and cohort-fixed effects, reform implementation year interacted with linear time trend. Low SES spouse is defined as father-in-law belonging to the lowest quintile of the earnings distribution. Share low SES school peers is the fraction of same-cohort municipality peers attending the same school type (folkskola/realskola before reform; comprehensive school after reform) whose father belongs to the lowest quintile of the earnings distribution. Supply of low SES partners in residential location at age 25 is calculated as the share of opposite-sex individuals (aged +/- 2 years with respect to index individual) residing in the same municipality, whose fathers belong to the lowest quintile of the earnings distribution. Spouse's social mobility index constitutes the standardized residuals from a regression of partners' years of schooling on partners' birth year and income decile of father-in-law. Robust standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

A final concern is related to the fact that potential spouses on the marriage market were also treated. Reform exposure might make low SES individuals more attractive as partners and change their preferences as well. While this mechanism is fully consistent with the conclusion that the reform reduced marital sorting, it is not supportive of the conclusion that it changed the preferences among the richest individuals, who exhibited very strong marital sorting. Because of geographical and age proximity between partners, their treatment status is positively correlated. Controlling for spouse's own reform status is somewhat like controlling for an outcome but can be informative and may shed light on this possible interpretation. Table A 7 presents the reform estimates on spouse's income background (similar to Table 2) additionally controlling for the spouses' 'exogenous' reform status (i.e., including fixed effects for the municipality and birth cohort of the spouse). Although the estimates come with somewhat lower precision overall, the downward mobility observed at the top of the distribution is robust to this inclusion. That is, even when controlling for altered preferences and attractiveness among partners, the results hold.

All in all, comprehensive schooling reduced homogamy among the wealthy, and the results laid out in Table 3 and Table 4 indicate that reduced attractiveness or increased opportunities to meet individuals from other social strata of society cannot explain this result. A residual explanation is therefore that exposure to peers from lower socioeconomic groups could have changed their preferences.

6 Discussion

Does familiarity between different groups, whether defined by income, ethnicity, or other traits, affect prejudice, attitudes, and preferences towards 'the other'? Allport's contact theory suggests that under certain conditions, contact between groups can affect attitude formation and reduce prejudice (Allport 1954). A recent meta-study summarizing the most recent evidence based on random group assignment concludes that the evidence is limited, but in favor of contact theory (Paluck, Green, and Green 2018), and Rao (2019) convincingly shows that when rich students are forced to interact with poor, attitudes are affected and become more positive towards poor students. However, most studies rely on outcomes that assess attitudes and behavior in the short run and do not follow up on how long-run attitudes and interactions between groups are affected. One exception is

Merlino, Steinhardt, and Wren-Lewis (2019) who show that interracial peer groups in childhood lead to more interracial romantic relationships in adulthood. Another example of long-term outcomes is found in Billings, Chyn, and Haggag (2021) who study racial diversity in schools and find that exposure to minority peers affects white students' future political affiliation.

This paper has demonstrated remarkably strong socio-economic homogamy among the wealthiest 20 percent of the parental income distribution. In addition, it has shown that an education reform that forced children of wealthy parents to interact more with children from poorer backgrounds reduced marital sorting at the top of the distribution. Men whose fathers were among the 20 percent richest became much more likely to marry women who came from the poorest 20 percent. As the results are not explained by human capital formation or partnership formation within the immediate peer group, they are consistent with a weakening taste for homogamy among the rich and add to the small literature on the effects of childhood interactions on outcomes that capture long-term revealed preferences.

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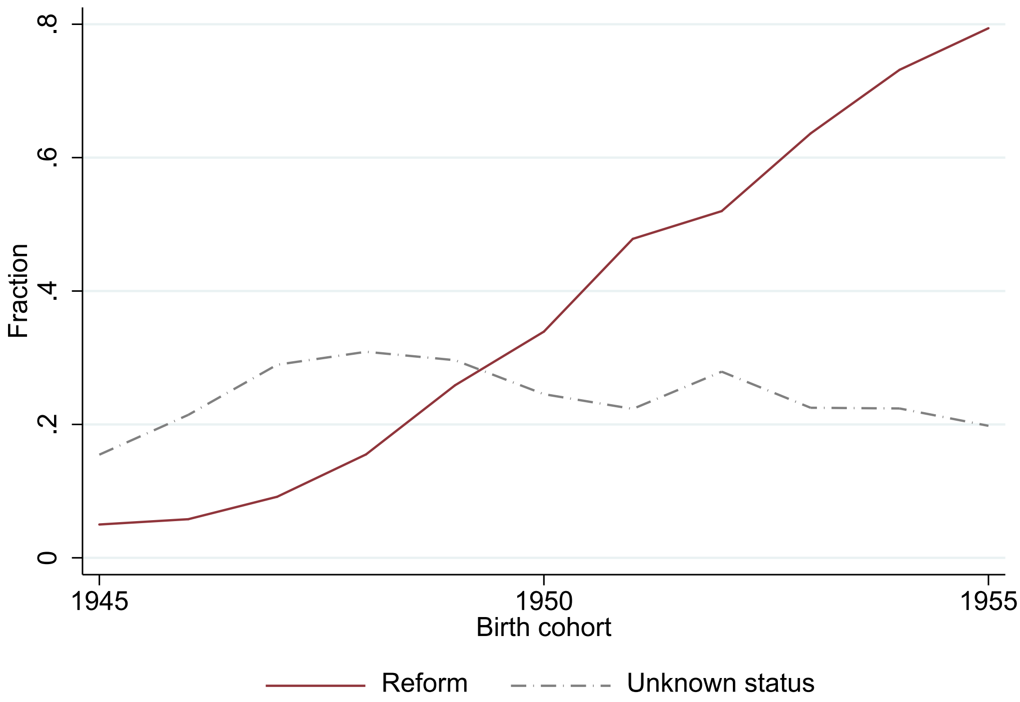
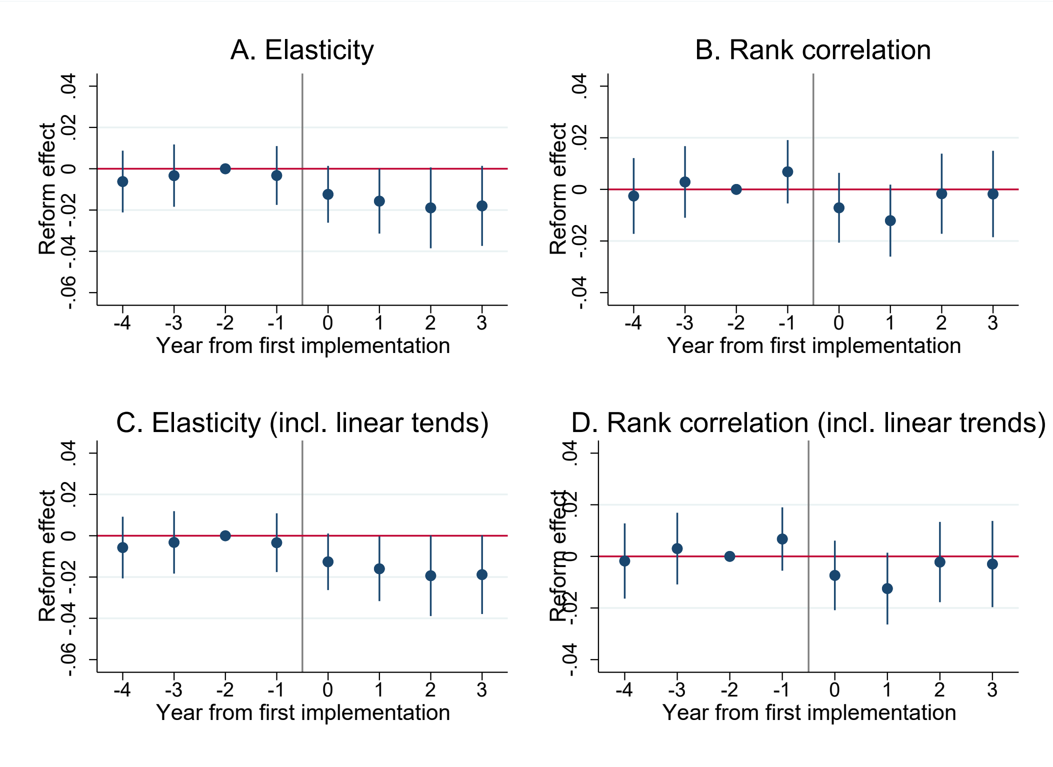
Appendix

Figure A 1 Reform status among cohorts born in Sweden 1945–1955



Note: The figure shows the fraction of each birth cohort that was treated by the reform, and the fraction where reform status is unknown. Calculations are based on the estimation sample used in the current paper, i.e. individuals observed with a spouse.

Figure A 2 Event study. Time-varying effects of the reform on assortative mating.



Note: The figures show point estimates of reform exposure interacted with fathers' a) log income; b) income rank, interacted with time from first reform implementation year, corresponding to time-varying effects of the estimate in column 2 of Table 1. Vertical bars show confidence intervals calculated at the 95 percent confidence level using robust standard errors clustered at the municipality level.

Table A 1 Descriptive statistics

	(1)	(2)	(3)
	Mean	Std.dev.	N
Birth year	1950	3.16	616,086
Reform=1	0.48	0.50	616,086
Rank father's income	10.88	0.60	605,829
Log father-in-law income	52.67	27.65	616,086
Rank father-in-law income	10.88	0.60	605,829
Rank father's income	52.64	27.75	616,086
Years of schooling	11.53	2.48	616,086
Cognitive skills	0.08	0.91	130,183
Non-cognitive skills	0.11	0.96	130,184
Share low SES peers	0.13	0.08	616,086
Mobility out-of-region	0.32	0.47	615,104
Partner from same municipality	0.25	0.43	597,184
Low SES partner from same municipality	0.04	0.19	616,086
Low SES partner from different municipality	0.12	0.32	616,086
Share high SES partners (<20 percentile) in municipality at age 25	0.86	0.04	615,091
Social mobility index of spouse	0.01	1.08	606,107

Note: Incomes and earnings are expressed in SEK 2006 values.

Table A 2 Balancing test: reform effect on pre-determined background characteristics

	(1)	(2)	(3)	(4)
	Father's birth year	High educated father	Log father's income	Father's income rank
Reform effect	-0.015	0.003	0.003	0.159
	(0.035)	(0.002)	(0.003)	(0.134)
Observations	616,086	513,714	605,829	616,086
R-squared	0.242	0.002	0.001	0.000
Number of municipalities	1,020	1,020	1,020	1,020
Muni f.e.	Yes	Yes	Yes	Yes
Cohort f.e.	Yes	Yes	Yes	Yes
Control group mean	1917	0.036	10.86	51.31

Note: Regressions include the following controls: municipality and cohort-fixed effects, reform implementation year interacted with linear time trend). Robust standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table A 3 Effects of school reform on assortative mating. Specification without linear time trends.

	(1)	(2)	(3)	(4)
	Full sample	Restricted sample & Stacked regression	Full sample Men	Full sample Women
		A. Log income father-in-law		
Log income father	0.146***		0.129***	0.185***
	(0.017)		(0.018)	(0.027)
Reform exposure x	-0.014***	-0.013**	-0.015**	-0.012*
Log income father	(0.005)	(0.006)	(0.006)	(0.007)
Observations	605,829	1,090,446	314,925	290,904
R-squared	0.019	0.055	0.018	0.020
Control group mean	10.86	10.85	10.84	10.88
		B. Income rank father-in-law		
Income rank father	0.159***		0.161***	0.163***
	(0.011)		(0.014)	(0.014)
Reform exposure x	-0.012***	-0.013**	-0.015**	-0.008
Income rank father	(0.004)	(0.006)	(0.006)	(0.007)
Observations	616,086	1,108,966	320,123	295,963
R-squared	0.019	0.060	0.018	0.020
Control group mean	51.49	51.27	50.80	52.24
Number of municipalities	1,020	971	1,020	1,020
Municipality f.e.	Yes	Yes	Yes	Yes
Cohort f.e.	Yes	Yes	Yes	Yes
Full interaction	Yes	Yes	Yes	Yes

Note: Regressions in columns 1 and 2 include controls for gender. Regressions in all columns include the following controls: main reform effect, main father's income effect and its interaction with the reform, municipality and cohort-fixed effects (main effects and interactions with father's income), reform implementation year-fixed effects (main effect and interaction with father's income). The restricted sample includes only cohorts born +/-2 years around the reform year in each municipality and only uses untreated municipalities as controls in a stacked regression (see section 5 for details). Robust standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table A 4 Reform effects on partner choice (defined by income of father-in-law). Heterogeneous effects by father's income. Restricted sample & stacked regression.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<p50	>=p50	>=p80	<p50 Men	>=p50 Men	>=p80 Men	<p50 Women	>=p50 Women	>=p80 Women
					Outcome: Log income father-in-law				
					A. Restricted sample & stacked-by-event regression				
Reform exposure	0.002	-0.010*	-0.024***	0.002	-0.005	-0.023**	0.002	-0.019**	-0.025**
	(0.005)	(0.006)	(0.008)	(0.007)	(0.007)	(0.010)	(0.007)	(0.008)	(0.012)
					*B. Restricted sample & stacked-by-event regression, controlling for cohort*county*				
Reform exposure	0.003	-0.008	-0.021*	0.001	-0.005	-0.021	0.006	-0.013	-0.013
	(0.005)	(0.006)	(0.011)	(0.008)	(0.008)	(0.015)	(0.008)	(0.009)	(0.016)
Observations	539,968	550,474	202,981	280,024	289,266	107,899	259,940	261,169	94,311
Control group mean	10.77	10.93	11.08	10.76	10.91	11.05	10.79	10.96	11.11
					Outcome: Income rank father-in-law				
					C. Restricted sample & stacked-by-event regression				
Reform exposure	0.391*	-0.293	-0.690**	0.313	-0.252	-1.165**	0.474	-0.416	-0.172
	(0.223)	(0.242)	(0.348)	(0.309)	(0.325)	(0.508)	(0.321)	(0.327)	(0.487)
					*D. Restricted sample & stacked-by-event regression, controlling for cohort*county*				
Reform exposure	0.555**	-0.269	-0.775*	0.371	-0.243	-1.292*	0.843**	-0.403	0.055
	(0.255)	(0.275)	(0.456)	(0.354)	(0.396)	(0.699)	(0.371)	(0.398)	(0.654)
Observations	554,141	554,821	204,450	287,175	291,413	108,669	266,962	263,373	95,016
Control group mean	47.46	55.12	61.46	46.79	54.32	60.49	48.17	56.01	62.63
Number of municipalities	971	971	957	971	970	927	971	969	927
Municipality f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Regressions include the following controls: main reform effect, municipality and cohort-fixed effects (interacted with event-time), and reform implementation year-fixed effects interacted with a linear time trend and event-time. The regressions are based on cohorts born +/-2 years around the reform year in each municipality and only uses untreated municipalities as controls in a stacked regression (see section 5 for details). Robust standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table A 5 Effects of reform exposure on partner choice, measured as income quintile of father-in-law. Heterogeneous effects by father's income.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<p50	>=p50	>=p80	<p50 Men	>=p50 Men	>=p80 Men	<p50 Women	>=p50 Women	>=p80 Women
				A. Father-in-law income in quintile 1					
Reform expousure	0.000	0.001	0.008**	-0.004	0.002	0.013**	0.005	0.000	0.003
	(0.003)	(0.002)	(0.004)	(0.004)	(0.003)	(0.005)	(0.004)	(0.003)	(0.005)
Control group mean	0.184	0.136	0.109	0.188	0.140	0.112	0.179	0.132	0.106
				B. Father-in-law income in quintile 2					
Reform exposure	-0.008***	0.003	0.002	-0.002	-0.001	-0.003	-0.015***	0.006	0.008
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)
Control group mean	0.242	0.192	0.153	0.245	0.197	0.159	0.238	0.185	0.145
				C. Father-in-law income in quintile 3					
Reform exposure	-0.003	-0.002	-0.002	-0.003	0.001	0.002	-0.004	-0.004	-0.007
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.007)
Control group mean	0.232	0.208	0.175	0.235	0.212	0.179	0.229	0.204	0.170
				D.Father-in-law income in quintile 4					
Reform exposure	0.009***	-0.001	-0.003	0.007*	-0.002	-0.006	0.010**	-0.001	0.001
	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)	(0.008)	(0.004)	(0.004)	(0.006)
Control group mean	0.200	0.216	0.210	0.197	0.216	0.212	0.203	0.216	0.208
				D. Father-in-law income in quintile 5					
Reform exposure	0.003	-0.001	-0.005	0.001	-0.000	-0.006	0.004	-0.001	-0.005
	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.006)	(0.004)	(0.004)	(0.007)
Control group mean	0.143	0.248	0.353	0.134	0.236	0.337	0.151	0.262	0.371
Observations	288,190	327,896	130,102	148,842	171,281	68,788	139,348	156,615	61,314
Number of municipalities	1,020	1,020	1,019	1,020	1,020	1,017	1,020	1,020	1,019
Municipality f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Regressions include the following controls: main reform effect, municipality and cohort-fixed effects, and reform implementation year-fixed effects interacted with a linear time trend. Robust standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table A 6 Reduced form reform effects – alternative mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<p50	>=p50	>=p80	<p50 Men	>=p50 Men	>=p80 Men	<p50 Women	>=p50 Women	>=p80 Women
				A. Being in the sample (=having a child)					
Reform expousure	0.002	-0.001	0.001	-0.001	-0.002	0.002	0.002	-0.001	0.001
	(0.003)	(0.004)	(0.005)	(0.003)	(0.004)	(0.006)	(0.003)	(0.004)	(0.005)
Observations	213,943	238,402	95,260	205,627	226,847	89,768	213,943	238,402	95,260
R-squared	0.001	0.001	0.001	0.000	0.000	0.000	0.001	0.001	0.001
Control group mean	0.801	0.820	0.824	0.868	0.860	0.842	0.801	0.820	0.824
				B. Birth year spouse					
Reform exposure	-0.030	0.026	0.045	0.001	-0.044	-0.043	-0.030	0.026	0.045
	(0.035)	(0.032)	(0.058)	(0.034)	(0.032)	(0.048)	(0.035)	(0.032)	(0.058)
Observations	148,842	171,281	68,788	139,348	156,615	61,314	148,842	171,281	68,788
R-squared	0.491	0.502	0.496	0.491	0.498	0.483	0.491	0.502	0.496
Control group mean	1951	1950	1950	1946	1946	1946	1951	1950	1950
				C. Age difference between spouses					
Reform exposure	0.030	-0.026	-0.045	-0.001	0.044	0.043	0.030	-0.026	-0.045
	(0.035)	(0.032)	(0.058)	(0.034)	(0.032)	(0.048)	(0.035)	(0.032)	(0.058)
Observations	148,842	171,281	68,788	139,348	156,615	61,314	148,842	171,281	68,788
R-squared	0.000	0.000	0.001	0.005	0.004	0.005	0.000	0.000	0.001
Control group mean	-2.449	-2.189	-2.050	2.259	1.982	1.803	-2.449	-2.189	-2.050
				D. Age at first child					
Reform exposure	0.045	0.071	0.082	-0.014	0.050	0.066	0.045	0.071	0.082
	(0.055)	(0.053)	(0.088)	(0.051)	(0.050)	(0.077)	(0.055)	(0.053)	(0.088)
Observations	148,842	171,281	68,788	139,348	156,615	61,314	148,842	171,281	68,788
R-squared	0.012	0.015	0.017	0.006	0.009	0.012	0.012	0.015	0.017
Control group mean	26.59	27.37	28.15	23.73	24.86	25.95	26.59	27.37	28.15
Number of municipalities	1,020	1,020	1,018	1,020	1,020	1,019	1,020	1,020	1,018
Municipality f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Regressions include the following controls: municipality and cohort-fixed effects, reform implementation year interacted with linear time trend). Robust standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table A 7 Sensitivity analysis: Reform effects on spouse characteristics, controlling for reform status of spouse. Heterogeneous effects by father's income.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<p50	>=p50	>=p80	<p50 Men	>=p50 Men	>=p80 Men	<p50 Women	>=p50 Women	>=p80 Women
				A. Log income father-in-law					
Reform exposure	0.001	-0.002	-0.016**	-0.000	-0.006	-0.017	0.001	0.001	-0.013
	(0.005)	(0.005)	(0.008)	(0.006)	(0.006)	(0.011)	(0.007)	(0.006)	(0.010)
Observations	224,740	253,652	98,771	113,596	130,895	51,774	111,143	122,756	46,979
R-squared	0.075	0.103	0.124	0.079	0.107	0.138	0.090	0.115	0.145
Control group mean	10.77	10.92	11.07	10.76	10.91	11.04	10.77	10.94	11.10
				B. Income rank father-in-law					
Reform exposure	0.421*	-0.142	-0.504	0.280	-0.201	-0.695	0.483	-0.065	-0.229
	(0.220)	(0.241)	(0.405)	(0.291)	(0.340)	(0.563)	(0.318)	(0.287)	(0.468)
Observations	230,818	255,644	99,472	116,601	131,865	52,113	114,216	123,778	47,341
R-squared	0.100	0.119	0.132	0.106	0.126	0.150	0.111	0.128	0.154
Control group mean	47.04	54.54	60.91	46.53	53.84	60.01	47.54	55.24	61.85
Number of municipalities	1,020	1,020	1,019	1,020	1,020	1,015	1,020	1,020	1,017
Reform spouse	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Regressions include the following controls: reform status of spouse, municipality and cohort-fixed effects (both for the index individual and the spouse), reform implementation year interacted with linear time trend (both for the index individual and the spouse). Robust standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.