ISSN 2045-6557

CEE DP 91

Intergenerational Mobility and Assortative Mating: Effects of an Educational Reform

Helena Holmlund

November 2008

Published by
Centre for the Economics of Education
London School of Economics
Houghton Street
London WC2A 2AE

November 2008

The Centre for the Economics of Education is an independent research centre funded by the Department for Children, Schools and Families. The views expressed in this work are those of the author and do not reflect the views of the DCSF. All errors and omissions remain the authors.

Intergenerational Mobility and Assortative Mating: Effects of an Educational Reform

Helena Holmlund

Acknowledgments

Helena Holmlund is a Research Economist at the Centre for Economic Performance, London School of Economics and a CEE Associate.

1. Introduction

Despite numerous studies focusing on the degree and measurement of intergenerational income mobility, relatively little is known about the mechanisms that may explain differences in mobility across countries and over time.[1] This paper provides new evidence on the role of the educational system for intergenerational mobility. I evaluate an educational reform, implemented in Sweden in the 1950s and 1960s, which abolished tracking and extended compulsory education from seven to nine years.[2] The Swedish reform was part of a wave of compulsory-school expansions that took place in Europe in the 1960s and 1970s: several other countries also undertook major changes in the spirit of the Swedish reform, so the lessons from this particular policy intervention are relevant for many European countries.[3]

The reform may have influenced intergenerational mobility by several different mechanisms. First, there is the possibility of a direct effect of extending compulsory education; it is likely that children from poor backgrounds benefited more than children growing up under more advantageous socio-economic conditions. Second, the age at which tracking takes place can be crucial for the educational choice. In particular, the earlier the tracking, the more likely it is that the schooling decision is made by the parents, based on their preferences for education, and not on the child's actual ability or on his or her preferences. One can also assume that parents' information on the child's ability is noisier, the younger the child; parents may therefore choose according to their own preferences and not given the actual ability of their child. Hence, a postponement of ability tracking implies higher intergenerational mobility.[4]

[1] See for example Solon (1992), Björklund and Jäntti (1997) and Haider and Solon (2006).
[2] In fact, in the final 9th grade of the new comprehensive school, pupils were divided into three different tracks. However, even though in different tracks, pupils were still all attending the same school establishment, which is strikingly different from the pre-reform tracking system where pupils were also sorted into different schools.
[3] Other countries affected by reforms are England, Finland and Norway.
[4] Ermisch et al. (2006) argue that differences in intergenerational mobility between Great Britain and Germany might be explained by the lower tracking age in Germany.

A third mechanism operates through assortative mating. Recent studies on intergenerational mobility recognize the fact that what determines an individual's economic standard of living is the income of his/her family (Chadwick and Solon 2002, Blanden 2005, Ermisch et al. 2006). Intergenerational mobility with respect to family income thus incorporates the income of an individual's spouse, and the degree of assortative mating in a society will naturally affect economic mobility. Clearly, if the degree of assortative mating is high, intergenerational mobility will be lower, whereas if couples are formed randomly, mobility will be higher.[5] As suggested in Mare (1991), I argue that the school shapes the peer group of individuals, i.e., the peer group in which people meet and form couples. A school that sorts pupils early based on ability and/or family background gives rise to homogenous peer groups where individuals meet and mate with their own kind. Postponing tracking to later ages implies that the peer group is more heterogeneous and couples may be formed across ability and parental background. Thus, later tracking implies lower assortative mating, and higher intergenerational mobility.

The Swedish school reform postponed ability tracking by three years and kept all pupils in one comprehensive school throughout nine years. It is possible but less likely that partners meet already in the last years of the nine year comprehensive school, but the reform may have affected an individual's peer group/educational group later in life, and if so, possibly also assortative mating. In addition, supporting the hypothesis that the reform may have influenced marital patterns, surveys show that a large proportion of couples actually meet in school. Evidence from the Netherlands indicate that 15 percent of the surveyed couples attended the same school: 5 percent attended the same elementary school and 7 percent the same secondary school (Kalmijn and Flap 2001). Similarly, U.S. figures point to that 23 percent of married couples met in school (Lauman et al. 1994).

[5] Previous studies show that about 40-50 percent of the covariance between parents' and own permanent family income can be attributed to assortative mating; see Ermisch, Francesconi and Siedler (2006).

The purpose of the study is to evaluate the effects of the Swedish compulsory school reform on intergenerational mobility, and to assess the extent to which the effect operates through assortative mating. A better understanding of the mechanisms underlying intergenerational mobility is important for the design of educational policies. In particular, if ability sorting has quantitatively large effects on mating patterns, then we might take this as evidence of the importance of sorting of individuals on different characteristics in general. Holding the age of ability tracking constant, the organization of schools, that is, how pupils are sorted within the school, may influence intergenerational mobility in itself.

The compulsory school reform was implemented gradually across the country's around 1000 municipalities, starting in 1948. Thus, for a given birth cohort, some individuals went through the old school system and others went in the new comprehensive school. The nature of the implementation allows me to adopt a differences-in-differences approach to evaluate the effects of the reform on intergenerational income elasticities, both for own income (a direct effect) and spouse's income (the assortative-mating effect) with respect to own parents' permanent family income. Given that the rich data at my disposal also include biological siblings, I test the robustness of my results by adopting a sibling-difference approach. Identification is in this case obtained by the fact that siblings who grow up in the same municipality might be subject to different educational systems; the younger sibling naturally the one being affected by the educational reform. This empirical approach controls for family background shared by siblings, which is particularly important in the case where the educational reform is not exogenous with respect to municipality or family characteristics.

I use a unique set of data compiled from Swedish administrative records that links generations and siblings, and that contains detailed earnings-histories for all individuals. My main finding is that the reform led to sizeable increases in intergenerational income mobility, i.e., it reduced the intergenerational income elasticities between children and parents. The

evidence concerning the impact on assortative mating is not as clear, however. The analysis shows that the reform reduced marital sorting for only for men, not for women, but this did not translate into higher mobility when taking partner's income into account.

The paper unfolds as follows: section 2 presents previous literature, section 3 describes the Swedish educational reform, section 4 presents a simple model of intergenerational mobility and assortative mating, and also the empirical specification, section 5 focuses on the data and section 6 presents the results. Finally, section 7 offers conclusions.

2. Previous Literature

This paper contributes by combining two branches of the literature on economic mobility: the first one has its focus on the role of the education system for intergenerational mobility, whereas the second one devotes attention to the importance of assortative mating for economic persistence across generations. There has been a recent upsurge of empirical evidence on the role of the education system in the mobility process. For example, the Swedish compulsory school reform and its long-run consequences for inequality have been studied previously by Erikson (1996) and Meghir and Palme (2005). Erikson's study relates changes over time in inequality of educational opportunity to several factors, one of which is the educational reform. He finds that the introduction of the nine year comprehensive school coincided in time with reduced inequality in education. Meghir and Palme find that the reform increased education and earnings for those individuals that were directly affected by the reform. In particular, the reform significantly increased earnings for those with low educated fathers, and high-ability individuals, also with low-skilled fathers, attained levels of education higher than the new compulsory minimum. This is an indication that intergenerational mobility was affected by the introduction of the reform, and serves as a benchmark to the results offered by this study, which (unlike Meghir and Palme 2005) estimates

intergenerational income elasticities and introduces the possibility of reform effects on assortative mating.

The evidence is not limited to the Swedish case, however. Riphahn and Bauer (2005) study the timing of ability tracking and its consequences for intergenerational educational mobility, taking advantage of regional variation in the age of tracking across Swiss cantons. They find that the impact of parental education on the education of the child varies with the age of tracking, in such a way that later tracking increases intergenerational mobility. Pekkarinen et al. (2006) make use of an educational reform in Finland, similar to the Swedish reform, to assess the effects on intergenerational income correlations of a postponement of ability tracking. They find that the reform reduced the intergenerational income correlation with seven percentage points, which corresponds to 20 percent of the correlation of 0.32. Their empirical approach is similar to the one in this paper, but they do not take into consideration the effects operating through changes in assortative mating.

A number of studies also use cross-country differences in the educational system in order to say something about its role for mobility. For example, Ammermueller (2005), Brunello and Checchi (2006), and Schuetz et al. (2005) all focus on how the coefficient of parental background on children's outcomes varies with different tracking regimes. Their findings are somewhat contradictory, but all point in the direction of early tracking reinforcing the role of parental background in determining children's outcomes. Furthermore, Hanushek and Woessman (2005) focus on the effect of ability sorting on inequality. Adopting a cross-country differences-in-differences strategy, their main finding is that early tracking increases inequality in achievement.[6] And finally, the previous literature also provides evidence from interventions at later stages of education: Machin (2005) studies changes over time in intergenerational mobility in Britain, and links it to changes in the educational system.

[6] Further evidence on tracking is found in Dustmann (2004) and Restuccia and Urrutia (2004).

Comparing two birth cohorts, born in 1958 and 1970, he finds that mobility has fallen, mainly due to the fact that the expansion of post-secondary education has benefited children from advantaged backgrounds more than children from low-income families.

Turning to the literature on marital sorting and mobility, it is clear that if we measure the individual's economic status with family income instead of own income, the higher the degree of positive assortative mating, the lower is the intergenerational mobility. In two early studies, Lam and Schoeni (1993, 1994) find strong effects of the schooling of father-in-law on own wages. Chadwick and Solon (2002) estimate permanent family income elasticities for daughters and sons, and find that income elasticities with respect to parents-in-law are similar in size to those with respect to own parents, which confirms that assortative mating contributes to intergenerational persistence. Hirvonen (2006) replicates Chadwick and Solon's study on Swedish data, and finds lower income elasticities than in the US, but likewise that assortative mating contributes to intergenerational immobility, more so for daughters than for sons. The latter result is also confirmed in Blanden's (2005) results for Canada. Ermisch, Francesconi and Siedler (2006) also find that assortative mating is contributing to immobility in income. Using German and British data, they estimate that around 40-50 percent of the intergenerational mobility estimate can be accounted for by assortative mating.

3. The Swedish Compulsory School Reform

Prior to the school reform, compulsory education mounted to seven (or in some cases eight) years of education. Ability tracking started in the 5th grade, with a five year junior-secondary school (*realskola*) following, or in the 7th grade, with a three or four year junior-secondary school following. Those pupils who did not select into junior-secondary school remained in the basic comprehensive school (*folkskola*) until the 7th or 8th grade. Importantly, the two

parallel school systems were entirely separated; the pupils spent their school days in different establishments and could not interact during school hours.

In 1950, the Swedish parliament committed to the introduction of a nine-year comprehensive school, and approved of the idea of an experimental period preceding the final implementation of the reform. The National Board of Education (*Skolöverstyrelsen*) administered the reform. The purpose of the reform was to increase compulsory education and equality of opportunity, but also to meet the increasing demand for junior-secondary education throughout the country. At the outset of the experimental period, municipalities willing to participate were selected on several criteria, one being that the chosen municipalities should form a group representing the country in terms of both size and urban development. Other aspects considered were the availability of teachers and the local demand for education. During the course of the experimental period, each year a number of new municipalities introduced the new school system. 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. The reform was introduced either in 1st and 5th grade, or in all grades 1 through 5. Pupils in grade 6 or higher in the first year of implementation were not subject to any changes.

As already mentioned, the educational reform was implemented gradually at different times in different municipalities (or sometimes parts of municipalities). Implementation of the new comprehensive school started first in the school year 1949/1950, introduced a nine-year comprehensive school, and postponed tracking until the final 9th grade of school. In 9th grade, pupils were sorted into three different tracks: one vocational, one theoretical preparing for upper-secondary school, and a third general track.[7] However, the 9th

[7] In a later curriculum from 1969, tracking in 9th grade was abolished; from now on pupils went through the whole comprehensive school without ability sorting.

grade tracking took place within the school, and did not separate pupils into different schools in different neighbourhoods as did the ability tracking in the earlier school system. The reform also revised the curriculum; one major change was to introduce English in 5th grade; one year earlier than before. For a more extensive overview of the educational reform and the Swedish school system, see the National Board of Ecucation (1960) and Marklund (1980, 1981).

4. The Educational System and Intergenerational Mobility: Model and Econometric Framework

4.1 The Model

In the following, I present a model of intergenerational mobility and assortative mating that combines the modelling approaches found in Solon (2004) and Ermisch, Francesconi and Siedler (2006). These models are both in the spirit of the Becker and Tomes model of parental investment in their child's human capital (Becker and Tomes 1979, 1986). In particular, my contribution lies in incorporating the role of assortative mating (as suggested by Ermisch, Francesconi and Siedler (2006)), into the Solon (2004) approach, which shows that public investments in education can affect intergenerational income mobility. The model is stylized and involves a few simplifying assumptions in order to derive equations that are estimated empirically. The model shows how intergenerational mobility is affected by changes in the educational system – both through direct effects and through assortative mating. A main idea is that the peer group of an individual, containing potential marriage partners, can be affected by the educational system.[8]

The conceptual framework is as follows: Parents care about their own consumption, (C_{t-1}), and about the expected future economic status of their (only) child, which is captured by the sum of the log permanent income of the child and his or her partner, $E(\log y_t + \log y_t^P)$,

[8] Note also, that the model does not incorporate the direct effect of ability tracking and its consequences for intergenerational mobility. In the model, tracking only has an effect through mating.

where t indicates the generation and P denotes the partner.[9] The child's human capital, h_t, is determined by public investment, $\bar{H}_{t-1}$, as well as by the parents' own investment, I_{t-1}, according the function

$$h_t = \theta \log(\bar{H}_{t-1} + I_{t-1}), \qquad \theta > 0 \qquad (1)$$

where the functional form implies positive but decreasing marginal product of investing in human capital (see Solon 2004). For simplicity, and unlike in Solon (2004), the child's human capital is *only* determined by investments, and not influenced by other factors such as nature or role models.

I define assortative mating in terms of human capital. Given that parents care about their child's income as well as the income of the spouse, they are sensitive to the degree of assortative mating in society. Parents are uncertain about the human capital of the future spouse of their child, h_t^P, but know that matching of partners takes place according to the following matching process (assuming positive assortative mating):

$$E\left(h_t^P\right) = \alpha h_t + (1-\alpha)\bar{h}_t^P, \qquad \alpha \in (0,1) \qquad (2)$$

where $E\left(h_t^P\right)$ is the expected level of human capital of a prospective partner. With probability α the child will meet someone with human capital equal to his or her own level, and with probability $(1-\alpha)$ the child will meet a randomly drawn partner from the peer group, where the peer group mean of human capital is $\bar{h}_t^P$. The parameter α will therefore represent the degree of assortative mating. Mating on human capital here refers to completed human capital, not necessarily to human capital at the time of mating.

Consider now the parents' maximization problem. Permanent income of the child is increasing in human capital (at the same rate for both spouses):

[9] Parent's utility, including the sum of the log of child's and child's partner's income, indicates altruism towards the partner, and that parents care about the partner's income per se. It is important to them not only to maximize total family income, but that both spouses have high earnings.

$$\log y_t = \gamma_0 + \gamma_1 h_t \tag{3}$$

$$\log y_t^P = \gamma_0 + \gamma_1 h_t^P \tag{4}$$

Parents choose the optimal investment in their child's human capital, I_t, in order to maximize the utility function

$$U = b\left[E(\log y_t + \log y_t^P)\right] + (1-b)\log(C_{t-1}), \qquad b \in (0,1) \tag{5}$$

where b indicates the relative preference for future earnings of the child and his/her partner compared to the parents' own consumption. Parents maximize their utility while recognizing equations (1) -- (4) and the budget constraint $y_{t-1} = C_{t-1} + I_{t-1}$. The optimal parental investment in the child's human capital is obtained as:

$$I_{t-1} = \left[b\gamma_1\theta(1+\alpha)y_{t-1} - (1-b)\bar{H}_{t-1}\right]\left(\frac{1}{b\gamma_1\theta(1+\alpha)+1-b}\right) \tag{6}$$

As is clear from (6), parental investment is increasing in parents' relative preference for their child's future economic status b, in the assortative mating parameter α, and in the return to human capital investments $\theta\gamma_1$. Clearly, holding public investment constant, higher-income families invest more in their child's human capital. The term $-(1-b)\bar{H}_{t-1}$ in (6) represents a negative compensation effect: parents internalize the positive effect of higher public investment and reduce their investment accordingly. The stronger their preference for own consumption is, the more they reduce their investment in the child's human capital as a response to higher public investment.

Using equations (1)-(4) and the optimal investment as in (6), the permanent income expressions of the child and his or her spouse are obtained as:

$$\log y_t = \gamma_0 + \gamma_1\theta\log\pi + \gamma_1\theta\log\left(y_{t-1} + \bar{H}_{t-1}\right) \tag{7}$$

$$\log y_t^P = \gamma_0 + \gamma_1(1-\alpha)\bar{h}^P_t + \alpha\gamma_1\theta\log\pi + \alpha\gamma_1\theta\log\left(y_{t-1} + \bar{H}_{t-1}\right) \tag{8}$$

where $\pi \equiv [b\gamma_1\theta(1+\alpha)]/[b\gamma_1\theta(1+\alpha)+1-b]$. It is clear that public investment in human capital affects intergenerational mobility positively. In other words, the larger is $\bar{H}_{t-1}$, the smaller the intergenerational elasticity $d\log y_t / d\log y_{t-1}$ as well as the elasticity corresponding to the partner, $d\log y_t^P / d\log y_{t-1}$. [10]

I make two further assumptions in order to derive explicit expressions for the intergenerational elasticities and make the model tractable for empirical implementation. Following Solon (2004), I first specify a "policy rule" of the form

$$\frac{\bar{H}_{t-1}}{y_{t-1}} = \varphi - \sigma \log y_{t-1}, \qquad \sigma \in (0,1) \tag{9}$$

where the parameter σ describes the rate at which public investment relative to parental income is decreasing in parental income. The more positive is σ, the larger the effect of the policy on the income of children from low-income parents compared to its effect for children from high-income families. The parameter thus captures the degree of "progressivity" of the educational policy. The second assumption is that the ratio $\bar{H}_{t-1} / y_{t-1}$ is "small" so that we can invoke the approximation $\log\left(1+\bar{H}_{t-1}/y_{t-1}\right) \approx \bar{H}_{t-1}/y_{t-1}$. [11] Armed with these assumptions we can rewrite the income expressions for the child as follows:

$$\log y_t \cong \gamma_0^* + \gamma_1\theta(1-\sigma)\log y_{t-1} \tag{10}$$

---

[10] The intergenerational elasticity pertaining to the child can be written as $d\log y_t / d\log y_{t-1} = \gamma_1\theta / \left(1+\bar{H}_{t-1}/y_{t-1}\right)$, whereas the analogous elasticity for the partner is $d\log y_t^P / d\log y_{t-1} = \alpha\gamma_1\theta / \left(1+\bar{H}_{t-1}/y_{t-1}\right)$. The intuition explaining why a larger public investment reduces the intergenerational elasticity is that it affects children from low-income households more than children from high-income families. The latter group obtain the desired (high) level of education (with or without public investment) through private investment, whereas the former is always on the compulsory minimum. Therefore, an extension of compulsory education will affect the length of poor children's education, but not necessarily children from higher-income families, since their optimum is beyond both the old and new compulsory minimum.

[11] The assumption that the ratio $\bar{H}_{t-1} / y_{t-1}$ should be small can be interpreted in the following way: the public investment in a child's education should be small compared to the parents' lifetime earnings.

where $\gamma_0^* = \gamma_0 + \gamma_1\theta\log\pi + \gamma_1\theta\varphi$. The income equation for the partner is analogously obtained as

$$\log y_t^P \cong \gamma_0^{P*} + \alpha\gamma_1\theta(1-\sigma)\log y_{t-1} \qquad (11)$$

where $\gamma_0^{P*} = \gamma_0 + \gamma_1(1-\alpha)\bar{h}_t^P + \alpha\gamma_1\theta\log\pi + \alpha\gamma_1\theta\varphi$. The stylized model has thus established an intergenerational link between parents and their child, and also arrived at equations that are commonly estimated in the empirical intergenerational income mobility literature. The intergenerational elasticity measures, expressed in terms of the structural parameters of the model, show that intergenerational income mobility (defined as $1-\gamma_1\theta(1-\sigma)$) is decreasing in the returns to human capital $\gamma_1\theta$, and increasing in the progressivity of public investments in children's human capital, σ . The elasticity with respect to parents-in-law depends positively on the degree of assortative mating, α ; if mating is random ($\alpha = 0$), the incomes of the partner and the parents will be uncorrelated.

Finally, it should be noted that the educational system and assortative mating enter the child's income equation not only through the interaction with parental income, but also through a direct effect on income (see φ and α in the intercept terms of equations (10) and (11)). Assortative mating enters directly since it affects parents' optimal investment in their child's human capital.

4.2 Interpreting the Model: The Swedish Compulsory School Reform

The Swedish compulsory school reform increased mandatory education from seven to nine years, and postponed tracking, keeping a heterogeneous group of pupils together for three more years. In the light of the above model, the extension of compulsory education can be interpreted as an increase in the public investment in children's human capital. Increasing compulsory education implies a more progressive policy, meaning that σ increases and

society becomes more mobile across generations. That is, the first implication of the model is that the educational reform should lower the intergenerational elasticity.

Second, postponing ability tracking has important implications for the child's peer group, which after the introduction of the reform will be more mixed with respect to both ability and parental background. I assume that before the reform was in place, there is perfect sorting, $\alpha = 1$, which implies that $E(h_t^P) = h_t$. With certainty, individuals will meet and mate with their own kind. Introducing the reform, the probability of mating with someone with the same human capital goes down, $\alpha < 1$. That is, a more heterogeneous peer group implies a lower degree of assortative mating. The second implication of the model is therefore that the reform should reduce assortative mating, and thereby increase mobility with respect to parents-in-law by two mechanisms. The first one is the same as above; the reform has a stronger impact on children from low-income families. However, this effect is now filtered through the strength of assortative mating, so that if assortative mating is lower, the intergenerational elasticity is lowered even further.

Just to be clear, it is not necessary that mating takes place at the time the reform is in effect; as long as the peer group is affected, the reform may have impacts on assortative mating. Nevertheless, there is evidence supporting that couples may form at an early age. In the 1949 birth cohort, around 15 percent of Swedish women were cohabiting at age 18. At age 20, 40 percent of the women and 20 percent of the men were cohabiting (Statistics Sweden 1995). Also keep in mind that if postponing tracking in itself results in more social mobility, future peer groups in later stages of the educational system will presumably also be more heterogeneous, with possible implications for assortative mating.

4.3 Empirical Specifications

The gradual implementation of the educational reform allows the estimation of a differences-in-differences model where the log income of the child and the log income of the partner of the child are regressed (in two separate regressions) on log family income of the parents. The effects of the reform on mobility are represented in the following baseline reduced-form specifications:[12]

$$\begin{aligned}\log y_{t,icfm} = \beta_0 + \beta_1 \log y_{t-1,icfm} + \beta_2 R_{cm} + \beta_3 (\log y_{t-1,icfm} * R_{cm}) + \Pi v_c + \Omega x_f + \Psi z_m + \\ + \Phi(v_c * \log y_{t-1,icfm}) + \Gamma(x_f * \log y_{t-1,icfm}) + \Lambda(z_m * \log y_{t-1,icfm}) + e_{t,icm}\end{aligned} \quad (12)$$

where $\log y_{t,icfm}$ represents the log of permanent income for individual *i*, belonging to cohort *c*, with a father born in year *f*, going to school in municipality *m*. Index *t* indicates the generation to which the individual belongs: *t* is the child generation and *t-1* represents the parental generation. R_{cm} is an indicator variable that takes the value 1 if in cohort *c*, municipality *m*, the reform was in effect. $\log y_{t-1,icfm}$ represents the log of permanent parental family income, v_c and z_m capture cohort and municipality effects, respectively. x_f stands for birth year effects of the father. Allowing for a full set of interactions with parental income, the intergenerational income elasticity is allowed to vary by cohort (both the child's and the father's) and municipality. However, the model does not capture municipality-specific trends in the elasticity estimate. The reduced-form coefficients and the structural parameters are related as follows: $\beta_1 + \beta_3 * R_{cm} = \gamma_1 \theta (1 - \sigma)$.

The corresponding equation for the partner, describing the intergenerational relationship with respect to parents-in-law is given by:

$$\begin{aligned}\log y^{P}_{t,icm} = \delta_0 + \delta_1 \log y_{t-1,icfm} + \delta_2 R_{cm} + \delta_3 (\log y_{t-1,icfm} * R_{cm}) + \Pi v_c + \Omega x_f + \Psi z_m + \\ + \Phi(v_c * \log y_{t-1,icfm}) + \Gamma(x_f * \log y_{t-1,icfm}) + \Lambda(z_m * \log y_{t-1,icfm}) + e_{t,icm}\end{aligned} \quad (13)$$

[12] The intergenerational elasticity, i.e., the coefficient of a regression of the child's log permanent income on the log permanent income of the parent, is identical to the correlation coefficient between the two in the case log incomes of parents and their children have the same variance.

where $\log y^{P}_{t,icm}$ is the log of permanent income of the partner of individual *i*. The coefficients in equation (13) can be written in terms of their structural counterparts in the following way: $\delta_1 + \delta_3 * R_{cm} = \alpha\gamma_1\theta(1-\sigma)$. From this equality it is clear that with the empirical specification (13), the reform effect on the partner-parent elasticity may be affected independently by both changes in i) α (assortative mating) and ii) changes in the educational system, σ.[13] The first mechanism is just a consequence of a more heterogeneous peer group, holding constant other changes. The second mechanism reflects that the pool of partners of an individual is also affected by changes in the education system, and therefore, by the strength of assortative mating, the actual spouse may also have been affected by the reform. Equation (13) does not separate the two mechanisms; in fact, an important aspect of the empirical specification is that the partner's individual characteristics are taken as endogenous with respect to the educational reform. That is, equation (13) contains no controls for the partner's age or own reform participation. The motivation behind this approach is that if assortative mating is affected by reform, be it by reducing the amount of sorting on age, income or home municipality, this is part of the effect that I want to capture. However, in order to determine whether potential effects are capturing changes in partner sorting, or whether they are purely driven by the fact that spouse's may also have been affected by the reform, I also present results controlling for the reform status of the spouse. Holding this variable constant enables me to isolate the effect of a child's reform participation, operating through changes in assortative mating, on the economic association between his/her partner and parents.

In addition to the differences-in-differences result, I also present sibling-difference estimates, in order to control for all unobserved family background characteristics that are shared by the siblings. In this case, the effect of the educational reform on intergenerational

[13] As argued in Lefgren and McIntyre (2006), changes in schooling laws will change the schooling of everybody in the marriage market, so that it is problematic to use them when studying the effects of education on marriage outcomes.

mobility is identified by making use of the fact that within a family, siblings of different ages went through different school systems. Within a sibling-pair where the siblings went to different types of schools, naturally it is always the younger one that was affected by the implementation of the new compulsory school. Using the sibling-difference approach is an appealing extension of my analysis; to the extent that the reform is not exogenous across municipalities, it is likely to be so within a family. This particular application of the sibling-difference technique is convincing; the variation within a sibling pair is imposed from changes on municipality level, and is unlikely to be endogenous within the family.[14] One assumption has to be maintained however: parents should not change the allocation of resources or the time investments in the child not affected by the reform, for compensatory or other reasons. One potential parental response to the reform could be to move to a different municipality, but by restricting the sample only to siblings who grew up in the same municipality, I rule out this source of bias. It should also be noted that sibling-estimates refer to the specific population of siblings, which might be different from the population as a whole.

Note that since younger siblings are the ones affected by the reform, any reform estimates could be spuriously driven by birth order effects. Therefore birth order dummies are included in all sibling specifications (both the main effect and its interaction with parental income).[15]

5. The Data

The data used in this study are collected from registers administered by Statistics Sweden. First, I start out with a 35 percent random sample of each cohort born in Sweden in 1943-1955. Those cohorts were affected by the educational reform, and to those I am also able to

[14] Holmlund (2005) shows that heterogeneity within the family can indeed bias sibling estimates, in an application of the consequences of teenage motherhood. Within a sibling pair, it is not random who becomes a teen mother, but a reform imposed by the school system will be.

[15] Lindahl (2002) shows that the intergenerational income elasticity decreases with birth order for a given family size.

assign a reform indicator stating whether the individual was subject to the reform or not.[16] By means of population registers, parents, siblings and children of the individuals in the random sample have been matched to the data. In addition, for all individuals in the data, information from the bi-decennial censuses, in the years 1960 to 1990, has been collected. The censuses provide information on which individuals that reside together, on municipality of residence, and on parental background of the 1943-1955 cohorts.

For the sampled individuals, I also use the education register in 1990, which contains information on each individual's highest educational degree.[17] And importantly, the data contain earnings histories for all individuals in the sample, starting in the year 1968. Income is measured as the sum of labour earnings, taxable transfers and capital income. For the cohorts born 1943-1955 (the child generation in this study), permanent income is measured as the mean of log total income in 1987, 1990, 1993 and 1996. That is, I use income observations when the individuals are in the age range 32-53 years old. In this age range, the observed income should properly represent the long-run income, at least for men (see Haider and Solon (2006) for US and Böhlmark and Lindquist (2006) for Swedish results on biases in estimates of lifetime income). All incomes are measured in 1990 prices and incomes below 10,000 SEK have been dropped.[18]

Permanent income of the parents of the 1943–1955 cohorts is measured as the average of log family income in the years 1968, 1969 and 1970.[19] This implies that I observe the parents' income for the first time when the children are 13 to 25 years of age. For the

[16] Appendix A provides an extensive description of the coding of the educational reform, and its quantitative development.
[17] The information on levels of schooling in the 1990 education register is translated into years of education in the following way: 7 years for the old compulsory school, 9 years for the new compulsory school, 9.5 years for the old junior-secondary school, 11 years for short upper-secondary school, 12 years for long upper-secondary school, 14 years for short university, 15.5 years for long university and 19 years for a doctoral degree. Parental education level is found in the 1970 census and translated into years in a corresponding way.
[18] Excluding incomes below 10,000 SEK implies that the permanent income measure is based on an average of those income observations >=10,000. An individual is dropped from the sample only if income is below 10,000 in all four years (1987, 1990, 1993 and 1996).
[19] Family income is defined as the sum of mother's and father's total income.

older cohorts, this income measure might not reflect the economic status of the family as they grew up.[20] More worrisome however, is that for the older cohorts in the sample, it is likely that their parents are too old for the income measure to be a good proxy for their permanent incomes. Observations for parents older than 55 in 1970 are dropped, which might lead to a non-representative sample since individuals with old parents are more likely to be excluded from the sample. Also for parents, family incomes below 10,000 SEK (in 1990 prices) have been excluded.

In Table B1 (Appendix B), I provide descriptive statistics for a non-restrictive sample (including individuals whose parents' income is missing or has been excluded due to sample restrictions), compared to the samples used for estimation. The table shows that the individuals remaining in the sample used for estimation of income elasticities are somewhat younger, and with younger parents, but that income is the same as in the non-restrictive sample. Although the samples seem similar on average, it is clear that the sample restrictions imposed reduce the number of observations by a great amount, from 203000 observations to around 125000 observations. The loss is mainly due to the restriction on parental age: parents are not allowed to be older than 55 in 1970 when their income is observed. This means that of individuals belonging to the oldest cohort in the sample (born in 1943), only those whose parents were 27 years or younger at childbirth, are included in the sample. Unfortunately, the need to impose restrictions on the sample gives rise to a trade-off between a representative sample on the one hand, and measuring permanent parental income without error on the other. By allowing for observations where parental age is allowed to be as high as 60 when observing income in 1970 reduces the loss of observations to around 45000, but most likely increases the error in the measure of permanent parental income, since the income is observed at the end of parents' career or even after retirement. Given that there are no striking

[20] Ideally, I would have liked to observe parental income when the children in this study were younger. The reason this is not possible is that the administrative income registers start in 1968.

differences between the restrictive and non-restrictive samples in Table B1, in the paper I choose to present results where the cut-off of parental age is set at 55 in 1970. Raising this cut-off up to 60 gives results in line with those found in the paper, and can be obtained from the author upon request.

The data do not contain direct information on the spouse of the individuals in the sample. However, it is possible to match spouses by means of the population censuses.[21] An individual's partner is in this study defined as the partner with which the individual lives shortly after the birth of his/her first-born child. Only individuals with a partner are part of the sample and it is not possible to observe marital status (although I use both the terms partner and spouse interchangeably in the paper). Since long-lasting consensual unions are very common in Sweden, cohabiting couples should preferably be included in the analysis even in the presence of information on marital status, whereby the lack of this information is less of a problem. But a number of questions arise with respect to the sample restrictions. First, the main intergenerational estimates presented in this paper refer to individuals with children. (For the elasticity between individuals and their parents, estimates on a less restrictive sample including all individuals, also those without children and spouses, are presented in Appendix B). And second, when considering the results relating to assortative mating, couples without children are not included in the sample. These restrictions are to be kept in mind when interpreting the results. However, when studying assortative mating, one has to settle for a definition of what constitutes a couple, and in absence of data on all partners (both marital and cohabiting), the partner at childbirth seems to be a second-best alternative. It is also reassuring that fertility outcomes were not affected by the reform (results for fertility are not presented in

[21] In order to do so, I first find the first-born child of an individual. In the first census after the child was born, I find the two household adults, one of which I will know is the biological parent. The other household adult is most likely also the biological parent of the child (or a new partner after separation), and thus the spouse of the individual. In some cases, the age difference between the spouses is unreasonably large, indicating that the household member is not a partner but more likely some other family member. I restrict partners so that the age difference between the two is maximum 10 years.

the paper), and therefore on mating, as defined in this study.[22] The economic outcome of the spouses is measured in 1987, 1990, 1993 and 1996, without considering whether couples had separated at that time.

For the purposes of this study, I compile two samples of data. One is the random sample, which includes the sampled individuals of the 1943-1955 cohorts. The other sample is a sibling sample, which singles out the individuals from the random sample who have siblings born in 1943-1955, and matches them with their siblings.

Finally, Appendix A explains how I assign a reform indicator to each individual, and Figure A1 describes the quantitative development of the reform, by birth cohort. As further explained in the appendix, I am not able to assign to all individuals in the data the correct information on whether they went to the old school system, or whether they were affected by the reform. Those individuals are also excluded from my sample.

6. Findings

6.1 Descriptive Statistics

Table 1 presents descriptive statistics for the random sample and the sibling sample. Log of parents' family income (in 1990 prices) is in general higher than the log of the child's income, which reflects that parental income is the sum of both parents' income. In the random sample (panel A), about 48 percent of the individuals were affected by the compulsory schooling reform. The siblings in panel B are somewhat older and therefore also a lower share went to the reform school.

[22] The fact that I observe couples shortly after childbirth also has the advantage of not introducing a bias due to subsequent divorce. Divorce in itself has important implications for economic well-being, but is not within the scope of this study.

6.2 Reform Effects on Assortative Mating

I start out by presenting reduced-form estimates of the structural parameter α (educational assortative mating), as described in section 4. The purpose is to illustrate one of the parameters of the simple model lined out in section 4, and to obtain an idea of the degree to which assortative mating might influence intergenerational mobility. Moreover, I examine the impact of the educational reform on the degree of assortative mating.[23] In order to find an estimate of educational sorting for the cohorts in my sample, I regress the child's partner's education on the child's own education, and to estimate reform effects on this association, I include an interaction term between the reform and the child's own schooling.

Since the reform introduced a shift in the lower tail of the education distribution, reform estimates based on years of schooling will be plagued by this "mechanic" change in the distribution. Instead, Table 2 reports estimates based on a dummy variable that takes the value one for two years of upper-secondary school or more.[24] This measure of schooling is not affected by changes in the distribution of education, and enables the isolation of reform effects only due to changes in educational sorting.

Turning to the results, Table 2 presents estimates of educational assortative mating, using a differences-in-differences specification.[25] Comparing the educational associations for women and men (column 1, panels A and B), some interesting differences between the sexes emerge. If a woman has 11 or more years of education, the probability that her partner also has 11 or more years of schooling increases by 0.25. For men, the corresponding increase in the probability is 0.19. This result indicates that moving up the educational distribution, women are more likely than men to find an equally educated partner -- a finding mirroring

[23] Assortative mating is widely studied in the sociological literature. For example, Mare (1991) studies trends in educational assortative mating in the US. For Sweden, Henz and Jonsson (2003) find that assortative mating has decreased over time.

[24] Two years of upper-secondary school corresponds to a vocational degree: two additional years of education after completion of compulsory school in the post-reform school system.

[25] Note that the regressions include cohort effects and municipality effects for the child, and in columns 3 and 6 also a full set of interactions between child's education and the other controls.

that traditionally it has been more socially accepted for men than for women to find a partner from a lower social class.

Column 3 in Table 2 presents coefficients from the interaction between reform participation and the child's own education. The purpose of this estimate is to investigate whether the association between partners' education has been reduced by the educational reform, as suggested in the theoretical section of the paper. The results show that for women subject to the new school system, there is no effect on their mating patterns. For men, however, participating in the new comprehensive school, where tracking was postponed and a more heterogeneous peer group was created, indeed seems to have reduced marital sorting. Going through the new school system reduces the predicted probability that a man with 11+ years finds a partner with 11+ years of schooling by 0.029, which corresponds to 16 percent of the baseline probability of 0.185. This result is also robust to the inclusion of controls for the partner's reform participation as shown in columns 4 and 5.

Moving to Table 3, reporting the corresponding sibling-difference estimates, the higher educational association for women compared to men is confirmed, although the levels are lower than those of Table 2. More importantly, it is clear that the previous finding that the reform reduces marital sorting for men is not robust to the sibling specification. Therefore I conclude that there is an indication that the educational reform had an impact on marital sorting for men, but the finding is not robust and should be interpreted with caution.

6.3 Reform Effects on Intergenerational Income Mobility

I now turn to the estimation of reform effects on intergenerational economic mobility. I estimate intergenerational elasticities of the child's (and the partner's) log permanent income on parental log permanent family income, and evaluate whether reform participation had an impact on the income elasticity between child and parents.

Table 4 reports on the differences-in-differences results. Panel A shows the coefficients from a pooled sample of women and men, whereas panels B and C present separate estimates by gender. I find intergenerational elasticities of 0.14 for women and 0.21 for men (column 1).[26] This indicates that women are more mobile than men, a finding confirmed in Hirvonen (2006). Turning to the intergenerational elasticities with respect to parents-in-law (column 4), I find that women and their spouses have elasticities similar in magnitude (both around 0.14) with respect to women's parental family income. Strikingly different are the results for men, in panel C of Table 4. Men exhibit much higher elasticities with respect to their parents' income than do their partners (0.21 vs. 0.09). Once again, this finding is in line with Hirvonen (2006), and is also an indicator that women are more economically mobile than men.

Next, column 2 of Table 4 reports on the main earnings effect of the education reform. When pooling men and women together, I find that the reform did not increase earnings on average: the reform is estimated to increase earnings by 0.1 percent, but the estimate is far from significant (a point estimate of 0.001 with a standard error of 0.004). As an interesting benchmark, Meghir and Palme (2005) estimate the average earnings gain from the reform to be 1.42 percent (a point estimate of 0.0142 with a standard error of 0.0089).[27] Comparing these two results, it is clear that this study contributes by providing a rather precisely estimated effect around zero. Reassuringly, the corresponding reform effects adopting a sibling difference, reported in Table 5, confirm the differences-in-differences finding.

[26] These estimates are in line with those found in Österberg (2000), but in general lower than what is usually found for Sweden (Björklund and Jäntti 1997, Hirvonen 2006). A possible explanation to low elasticities is found in Grawe (2006); the older is the parent when his/her income is observed, the lower is the intergenerational elasticity. The reason is that as parents get older, the variance in their permanent earnings is increasing, and thus a lower coefficient will explain the same outcome.

[27] Note that the specifications and the birth cohorts differ between the Meghir and Palme (2005) study and this study.

Establishing that the reform had no average effect on individual earnings, I now turn to the main analysis of interest: did the educational reform have an effect on intergenerational income mobility, and did changes in educational sorting translate into increased economic mobility when considering also the spouse's contribution to household income? Column 3 in Table 4 presents the reform effects on the intergenerational elasticities between child and parent (corresponding to equation 12), and indeed the intergenerational income elasticity is lower for those individuals who were affected by the new education system. Participation in the reform reduces the elasticity by 0.021, which represents 12 percent of the baseline elasticity of 0.17. Moving down the table, it is also clear that the increase in mobility is present for both men and women, although not statistically significant when splitting the sample. As an alternative identification strategy, Table 5 summarizes the corresponding findings using a sibling-difference approach. When using within-family variation in reform participation, the results indicate that mobility has increased in particular for men: panel C of Table 5 shows that the intergenerational income elasticity was reduced by 0.058 for men in the reform school.

Summarizing the findings so far, it is clear that even though the average earnings effect is zero, the reform has reduced the economic persistence between parents and their children, and this result is especially robust for men. (This result is confirmed also by the results in Tables B2 and B3, where the results for a non-restrictive sample including individuals also without children and partner are presented). I now turn to examine an alternative mechanism through which mobility might have been affected: did the reform have an impact on the elasticity of income between the partner and the parents of an individual, estimated as in equation 13? Columns 6 and 8 in Table 4 and Table 5 provide an answer: when a child is affected by a change in the peer group implied by the educational reform, it does not translate into choosing partners that are economically less associated with the child's

parents. The differences-in-differences estimates are all negative as expected, but statistically insignificant, and in the case of women the point estimate is very small.[28] [29]

6.4 Heterogeneity in the response to the reform

Given that there is no average earnings gain from attending the reform school, but that income mobility was affected, it is obvious that there is heterogeneity in the response to the reform. In order to examine what is underlying the changes in intergenerational income mobility, Table 6 and 7 present estimates of the effect of the reform on income at different quartiles of the parental income distribution. The first three panels of each table show the effects on child's income and the last three panels present the results for spouse's income. First, focusing on the reform effects on the child's own income, it turns out that most of them are insignificant, but an interesting general pattern of differences at different quartiles of parental income emerge. First, the reform actually reduced earnings for individuals from high-income families. This effect is statistically significant and large, in particular for women, who subject to the reform earn 2.2 percent less (Table 6). The sibling-difference estimates in Table 7 point to positive effects at the low end of the parental income distribution, and negative effects at the top of the distribution, although the effects are not statistically significant. Reconciling these estimates with the reform effects on the intergenerational income elasticites, it is clear that the reduction in the intergenerational elasticity is driven by mobility at both ends of the parental income distribution. This finding is also in line with that of Meghir and Palme (2005), who find negative earnings effects for individuals with high skilled fathers, and with the results for

[28] Columns 7 and 8 include controls also for the reform participation of the spouse, in order to isolate only the effect on the elasticity that operates through changes in assortative mating.

[29] The results laid out in Table 4 allows for a structural identification of the assortative mating parameter α as definde by the model: $\alpha_{R=0} = \frac{\delta_1}{\beta_1}$ and $\alpha_{R=1} = \frac{\delta_1 + \delta_3}{\beta_1 + \beta_3}$. Using the parameter values in columns 3 and 6, Table 4, I find that assortative mating for women has increased with the reform, from 1.42 to 1.56. For men, on the other hand, the degree of assortative mating has decreased, from 0.24 to 0.17. Although I do not present standard errors of the change based on this approach, the reduced sorting for men is in line with the findings in Table 2.

Finland showing negative reform effects for children from the high end of the parental income distribution (Pekkarinen et al. 2006). Surprising as it may be to find negative effects of the reform under study, it is worth noting that at the time the new comprehensive school was introduced it was commonly perceived that it did not meet the standards of the selective junior-secondary school which it replaced. The reform thus seems to have lowered the quality of education for those individuals who in its absence would have continued beyond compulsory levels.

The three lower panels of Tables 6 and 7 focus on the reform effects operating through assortative mating. The results in Tables 4 and 5 did not support the hypothesis that mobility was affected through mating, but studying the effect of the reform on spouse's income at different parts of the parental income distribution might reveal some interesting heterogeneity in the response to the reform. Recalling that the estimates in section 6.2 indicate that the reform reduced marital sorting for men but not for women, one should expect to find a reduced partner-parent elasticity for men only. Although the average reform effects on the partner-parent relationships are insignificant, panels E and F of Table 6 give some support that for men who attended the reform school mobility increased also through choice of partner, whereas women were not affected in this way. Men whose parents belong to the third income quartile (see Table 6) have responded to the reform by finding a partner with lower income (this result is robust to controlling for partner's own reform status – see Table B4 in Appendix B). However, this finding is not confirmed by the sibling-difference results in Table 7. Therefore, the overall conclusion is that the reform indeed reduced intergenerational earnings persistence, but mainly through effects on the individual's own income and not through changes in assortative mating.

6.5 Remarks on the robustness of results

A major concern when evaluating policy changes such as the Swedish educational reform is that the policy change is endogenous. In the case of this paper it translates into the risk that a correlation between the reform and income mobility is driving the results. Note, however, that the differences-in-differences estimates include municipality fixed effects, so that any correlation between time-invariant municipality-specific factors and the reform are controlled for. Furthermore, the inclusion of interactions between municipality and parental income, allow for differential mobility across reform and non-reform municipalities. The empirical strategy does not control for differential trends in mobility across municipalities, but luckily the sibling-difference method is in itself a robustness check: assuming that parents treat their children in a similar way and do not reallocate resources as a consequence of the reform, the identification implies that the reform is uncorrelated with background characteristics and trends.

A second source of bias could be that families respond to the introduction of the reform by moving to a different municipality. It has been documented that children from higher socio-economic background were sometimes placed with a relative in another municipality in order to avoid the new comprehensive school, since it was thought to be of lower quality than the junior-secondary school (Marklund 1981). The sibling-difference analysis conditions on that siblings grew up in the same municipality, and as long as children's mobility is captured in a correct way in the censuses, the sibling-difference estimates are robust to mobility caused by the reform.

Finally, one concern may be that the result that women are more mobile than men is purely driven by the fact that women's labour supply varies more than men's. However, by measuring income as the mean over several years, the risk that the estimated mobility

coefficient is reflecting labour-supply effects should be reduced, and I should be more confident that I have a good measure of the woman's lifetime earnings.

7. Conclusions

This paper explores the educational system as a mechanism explaining intergenerational mobility. Studying the Swedish school reform that in the 1950s extended compulsory education and postponed ability tracking, both a differences-in-differences and a sibling-difference specification find that the reform had no average effect on individual earnings; an effect that is precisely estimated around zero. Nevertheless, I find a sizable increase in income mobility as a result of the reform. The intergenerational income elasticity was reduced by 12 percent, the result being more robust for men than for women. A policy that targeted the lower end of the educational distribution clearly had the implication to increase intergenerational mobility. And interestingly, the reason the average income effect sums to zero is that the positive gains for individuals from poor backgrounds are counteracted by negative reform effects for individuals from more well-off families.

The paper also considers the fact that the economic standard of living is determined by the household, meaning that the economic position of one's partner is an important parameter of economic well-being. Assortative mating is thus a contributor to intergenerational income persistence – if people were to mate randomly, intergenerational mobility with respect to family income would be higher. I argue that mating takes place in the peer group, which can be affected by the educational system. The educational reform under study in this paper changed the peer group of the individual; the postponement of tracking had the consequence of keeping a more heterogeneous group of pupils together for a longer time. This might have resulted in a reduction in assortative mating.

The empirical results concerning reform effects on mating patterns are mixed, however; the analysis shows that the reform likely reduced educational sorting for men, but there is only very weak evidence that this translated into lower partner's earnings. Although this finding is not robust, I suggest it as a first indicative result of the importance of peer group composition for mating patterns, and hopefully future research will return to this topic.

Overall, my findings indicate that the reform was successful in one of its purposes, namely to increase equality of opportunity, and therefore I conclude that the educational system indeed plays a major role in shaping social mobility.

References

Ammermüller, Andreas (2005), "Educational Opportunities and the Role of Institutions", ZEW Discussion Paper No. 05-44.

Bauer, Philipp and Regina T. Riphahn (2006), "Timing of School Tracking as a Determinant of Intergenerational Transmission of Education", *Economics Letters* 91:1, pp. 90-97.

Becker, Gary S. and Nigel Tomes (1979), "An Equilibrium Theory of the Distribution of Income and Intergenerational Mobility", *Journal of Political Economy* 87:6, pp. 1153-1189.

Becker, Gary S. and Nigel Tomes (1986), "Human Capital and the Rise and Fall of Families", *Journal of Labor Economics* 4:3, pt 2, pp. S1-S38.

Björklund, Anders and Markus Jäntti (1997), "Intergenerational Income Mobility in Sweden Compared to the United States", *American Economic Review* 87:5, pp. 1009-1018.

Blanden, Jo (2005), "Love and Money: Intergenerational Mobility and Marital Matching on Parental Income", Research Paper, Family and Labour Studies, Statistics Canada.

Bureau of Educational Statistics (1960-1964), Tables of pupils in the compulsory school.

Böhlmark, Anders and Matthew J. Lindquist (2006), "Life-Cycle Variations in the Association between Current and Lifetime Income: Replication and Extension for Sweden", *Journal of Labor Economics* (forthcoming).

Chadwick, Laura and Gary Solon (2002), "Intergenerational Income Mobility among Daughters", *American Economic Review* 92:1, pp. 335-344.

Dustmann, Christian (2004), "Parental Background, Secondary School Track Choice, and Wages", *Oxford Economic Papers* 56, pp. 209-230.

Erikson, Robert (1984), "Social Class of Men, Women, and Families", *Sociology* 18:4, pp. 500-14

Erikson, Robert (1996), "Explaining Change in Educational Inequality – Economic Security and School Reforms", in Robert Erikson and Jan O. Jonsson (eds.), *Can Education Be Equalized?*, Westview Press.

Ermisch, John, Marco Francesconi and Thomas Siedler (2006), "Intergenerational Mobility and Marital Sorting", *Economic Journal* 116, pp. 659-679.

Haider, Steven and Gary Solon (2006), "Life-Cycle Variation in the Association between Current and Lifetime Earnings", *American Economic Review*, (forthcoming).

Hanushek, Eric A. and Ludger Woessmann (2006), "Does Educational Tracking Affect Performance and Inequality? Differences-in-Differences Evidence across Countries", *Economic Journal* 116, pp. C63-C76.

Henz, Ursula and Jan O. Jonsson (2003), "Who Marries Whom in Sweden?", in Hans-Peter Blossfeld and Andreas TImm (eds.), *Who Marries Whom? Educational Systems as Marriage Markets in Modern Societies*, Kluwer Academic Publishers.

Hirvonen, Lalaina (2006), "Intergenerational Earnings Mobility Among Daughters and Sons: Evidence from Sweden and a Comparison with the United States", mimeo, Institute for Social Research, Stockholm University.

Holmlund, Helena (2005), "Estimating Long-Term Consequences of Teenage Childbearing – An Examination of the Siblings Approach", *Journal of Human Resources* 40:3, pp. 716-743.

Lam, David and Robert F. Schoeni (1993), "Effects of Family Background on Earnings and Returns to Schooling: Evidence from Brazil", *Journal of Political Economy* 101:4, pp. 710-740.

Lam, David and Robert F. Schoeni (1994), "Family Ties and Labor Market in the United States and Brazil", *Journal of Human Resources* 29:4, pp. 1235-1258.

Lauman, Edward O., John H. Gagnon, Robert T. Michael and Stuart Michaels (1994), "The Social Organization of Sexuality. Sexual Practices in the United States", University of Chicago Press.

Lefgren, Lars and Frank McIntyre (2006), "The Relationship Between Women's Education and Marriage Outcomes", Journal of Labor Economics 24:4, pp. 787-830.

Lindahl, Lena (2002), "Do Birth Order and Family Size Matter for Intergenerational Income Mobility? Evidence from Sweden", Working Paper 5/2002, Swedish Institute for Social Research.

Machin, Steven (2005), "Education Expansion and Intergenerational Mobility in Britain", in Ludger Woessman and Paul Peterson (eds.), *Schools and the Equal Opportunity Problem*, MIT Press (forthcoming).

Mare, Robert D. (1991), "Five Decades of Educational Assortative Mating", *American Sociological Review* 56, February, pp. 15-32.

Marklund, Sixten (1980), "Från reform till reform: Skolsverige 1950-1975, Del 1, 1950 års reformbeslut", Skolöverstyrelsen och Liber UtbildningsFörlaget.

Marklund, Sixten (1981), "Från reform till reform: Skolsverige 1950-1975, Del 2, Försöksverksamheten", Skolöverstyrelsen och Liber UtbildningsFörlaget.

Meghir, Costas and Marten Palme (2005), "Educational Reform, Ability, and Family Background", *American Economic Review* 95:1, pp. 414-424.

National Board of Education (Skolöverstyrelsen) (1954-1962), "Redogörelse för försöksverksamhet med enhetsskola", Aktuellt från Skolöverstyrelsen.

National Board of Education (Skolöverstyrelsen) (1960), "Enhetsskolan under 10 år, Kort redogörelse för försöksverksamheten läsåren 1949/50 – 1958/59", Kungliga skolöverstyrelsens skriftserie 46.

Pekkarinen, Tuomas, Roope Uusitalo and Sari Pekkala (2006),"Education Policy and Intergenerational Income Mobility: Evidence from the Finnish Comprehensive School Reform", IZA Discussion Paper No. 2204.

Restuccia, Diego and Carlos Urrutia (2004), "Intergenerational Persistence in Earnings: The Role of Early and College Education", *American Economic Review* 94:5, pp. 1354-1378.
Solon, Gary (2002), "Intergenerational Income Mobility in the United States", *American Economic Review* 82:3, pp. 393-408.

Schuetz, Gabriella, Heinrich W. Ursprung and Ludger Woessmann (2005), "Education Policy and Equality of Opportunity", CESIfo Working Paper No. 1518.

Solon, Gary (1992), "Intergenerational Income Mobility in the United States", *American Economic Review* 82:33, pp. 393-408.

Solon, Gary (2004), "A Model of Intergenerational Mobility Variation over Time and Place", forthcoming in Miles Corak (ed.), *Generational Income Mobility in North America and Europe*, Cambridge University Press, Cambridge.

Statistics Sweden (1968-1969), Tables of pupils in the compulsory school, Statistiska meddelanden U1968:2, U1969:5.

Statistics Sweden (1995), "Kvinnors och mäns liv, del 2, Parbildning och separationer", Demografiska rapporter 1995:2.2.

Österberg, Torun (2000), "Intergenerational Income Mobility in Sweden: What do Tax-Data show?", *Review of Income and Wealth* 46:4, pp. 421-436.

Table 1

Descriptive statistics for the random sample and the sibling sample

	All	Women	Men
	(1)	(2)	(3)
Variable	Mean (St. Dev)	Mean (St. Dev)	Mean (St. Dev)
A. The random sample			
Child's log income	11.85	11.62	12.09
	(0.48)	(0.42)	(0.42)
Parents' log family income	12.12	12.11	12.13
	(0.49)	(0.49)	(0.49)
Log partner's income	11.84	12.09	11.59
	(0.49)	(0.43)	(0.42)
Reform	0.48	0.49	0.48
	(0.50)	(0.50)	(0.50)
Reform of partner	0.46	0.31	0.61
	(0.50)	(0.46)	(0.49)
Child >10 years of edu	0.75	0.78	0.73
	(0.43)	(0.42)	(0.45)
Partner >10 years of edu	0.74	0.70	0.79
	(0.44)	(0.46)	(0.41)
Child's year of birth	1950.15	1950.18	1950.13
	(3.48)	(3.50)	(3.46)
Father's year of birth	1921.88	1921.89	1921.88
	(4.66)	(4.67)	(4.64)
Woman	0.50	1.00	0.00
	(0.50)	(0.00)	(0.00)
n	124996	63091	61905
B. The sibling sample			
Child's log income	11.85	11.62	12.07
	(0.47)	(0.41)	(0.41)
Parents' log family income	12.07	12.05	12.06
	(0.49)	(0.49)	(0.50)
Log partner's income	11.83	12.08	11.57
	(0.49)	(0.42)	(0.42)
Reform	0.41	0.41	0.40
	(0.49)	(0.49)	(0.49)
Reform of partner	0.41	0.26	0.56
	(0.49)	(0.44)	(0.50)
Child >10 years of edu	0.72	0.74	0.68
	(0.45)	(0.44)	(0.47)
Partner >10 years of edu	0.72	0.66	0.77
	(0.45)	(0.47)	(0.42)
Child's year of birth	1949.65	1949.65	1949.55
	(3.44)	(3.45)	(3.45)
Father's year of birth	1920.95	1920.87	1920.86
	(4.13)	(4.10)	(4.09)
Woman	0.50	1.00	0.00
	(0.50)	(0.00)	(0.00)
n	110907	32315	32782

Note: Income is expressed in 1990 prices.

Table 2

Assortative mating on education
Differences-in-differences estimates
Dependent variable: Indicator for partner having >10 years of education (vocational upper-secondary school or higher)

	(1)	(2)	(3)	(4)	(5)
	A. Women				
Child >10 years of edu	0.252	0.252	0.320	0.248	0.321
	(0.005)**	(0.005)**	(0.023)**	(0.005)**	(0.023)**
Reform		0.004	0.001	-0.004	-0.008
		(0.007)	(0.016)	(0.007)	(0.016)
R* Child >10 years of edu			0.004		0.005
			(0.017)		(0.017)
Reform of partner				Yes	Yes
Observations	63091	63091	63091	63091	63091
R-squared	0.10	0.10	0.12	0.11	0.12
	B. Men				
Child >10 years of edu	0.185	0.185	0.210	0.186	0.216
	(0.004)**	(0.004)**	(0.023)**	(0.004)**	(0.023)**
Reform		-0.000	0.022	-0.002	0.020
		(0.006)	(0.013)+	(0.006)	(0.013)
R* Child >10 years of edu			-0.029		-0.028
			(0.015)+		(0.015)+
Reform of partner				Yes	Yes
Observations	61905	61905	61905	61905	61905
R-squared	0.08	0.08	0.10	0.08	0.10

Notes: Robust standard errors in parentheses are clustered on municipality.
+ significant at 10%; * significant at 5%; ** significant at 1%
R denotes reform status.
All estimates include child cohort effects and municipality effects, columns 3 and 5 also include a full set of interactions between child's education and the cohort and municipality effects. Column 5 also includes the interaction between child's education and partner's reform.

Table 3

Assortative mating on education
Sibling-difference estimates

Dependent variable: Indicator for partner having >10 years of education (vocational upper-secondary school or higher)

	(1)	(2)	(3)	(4)	(5)
	A. Women				
Child >10 years of edu	0.155	0.155	0.231	0.153	0.232
	(0.013)**	(0.013)**	(0.050)**	(0.013)**	(0.050)**
Reform		0.016	0.019	0.008	0.010
		(0.015)	(0.030)	(0.015)	(0.030)
R* Child >10 years of edu			-0.001		-0.000
			(0.032)		(0.032)
Reform of partner				Yes	Yes
Observations	32315	32315	32315	32315	32315
R-squared	0.56	0.56	0.56	0.56	0.56
	B. Men				
Child >10 years of edu	0.097	0.097	0.102	0.097	0.103
	(0.012)**	(0.012)**	(0.046)*	(0.012)**	(0.046)*
Reform		-0.001	0.006	-0.003	-0.003
		(0.014)	(0.025)	(0.014)	(0.025)
R* Child >10 years of edu			-0.008		-0.007
			(0.027)		(0.027)
Reform of partner				Yes	Yes
Observations	32782	32782	32782	32782	32782
R-squared	0.53	0.53	0.53	0.53	0.53

Notes: Robust standard errors in parentheses are clustered on family.
+ significant at 10%; * significant at 5%; ** significant at 1%
R denotes reform status.
All estimates include child cohort effects, birth order dummies and family-fixed effects effects, columns 3 and 5 also include a full set of interactions between child's education and the cohort and municipality effects. Column 5 includes the interaction between partner's reform and child's education.

Table 4
Intergenerational income elasticities
Differences-in-differences estimates

Dependent variable	(1) Child's log income	(2) Child's log income	(3) Child's log income	(4) Partner's log income	(5) Partner's log income	(6) Partner's log income	(7) Partner's log income	(8) Partner's log income
Independent variable								
	A. All							
Parents' log family income	0.171	0.171	0.214	0.114	0.114	0.119	0.114	0.121
	(0.003)**	(0.003)**	(0.022)**	(0.003)**	(0.003)**	(0.019)**	(0.003)**	(0.020)**
Reform		0.001	0.255		-0.004	0.136	-0.004	0.128
		(0.004)	(0.117)*		(0.004)	(0.113)	(0.004)	(0.111)
Reform*Family income			-0.021			-0.012		-0.011
			(0.010)*			(0.009)		(0.009)
Partner's reform							Yes	Yes
Observations	124996	124996	124996	124996	124996	124996	124996	124996
R-squared	0.29	0.29	0.30	0.30	0.30	0.31	0.30	0.31
	B. Women							
Parents' log family income	0.135	0.135	0.175	0.141	0.141	0.248	0.140	0.252
	(0.004)**	(0.004)**	(0.026)**	(0.006)**	(0.006)**	(0.025)**	(0.006)**	(0.025)**
Reform		-0.003	0.242		-0.000	0.073	-0.004	0.053
		(0.006)	(0.174)		(0.006)	(0.169)	(0.006)	(0.166)
Reform*Family income			-0.020			-0.006		-0.005
			(0.014)			(0.014)		(0.014)
Partner's reform							Yes	Yes
Observations	63091	63091	63091	63091	63091	63091	63091	63091
R-squared	0.07	0.07	0.09	0.07	0.07	0.08	0.07	0.08
	C. Men							
Parents' log family income	0.207	0.207	0.177	0.088	0.088	0.042	0.087	0.043
	(0.006)**	(0.006)**	(0.035)**	(0.005)**	(0.005)**	(0.027)	(0.005)**	(0.028)
Reform		0.005	0.259		-0.008	0.170	-0.006	0.156
		(0.006)	(0.164)		(0.006)	(0.153)	(0.006)	(0.154)
Reform*Family income			-0.021			-0.015		-0.013
			(0.013)			(0.013)		(0.013)
Partner's reform							Yes	Yes
Observations	61905	61905	61905	61905	61905	61905	61905	61905
R-squared	0.10	0.10	0.12	0.07	0.07	0.09	0.07	0.09

Notes: Log income for child and partner is a measure of permanent income; the average of log income in 1987, 1990, 1993 and 1996. Parent's log family income is the average of the log of family income in the years 1968, 1969 and 1970. Family income is defined as the sum of mother's and father's income.
Standard errors in parentheses are clustered on municipality. + significant at 10%; * significant at 5%; ** significant at 1%.
All estimates include child cohort effects, father cohort effects and municipality effects. In columns 3, 6 and 8 all controls are interacted with parents' log family income. The upper panel also controls for gender.

Table 5

Intergenerational income elasticities
Sibling-difference estimates

Dependent variable	(1) Child's log income	(2) Child's log income	(3) Partner's log income	(4) Partner's log income	(5) Partner's log income	(6) Partner's log income
Independent variable						
	A. All					
Reform	0.002	0.323	-0.003	-0.047	-0.002	-0.056
	(0.007)	(0.188)+	(0.007)	(0.197)	(0.007)	(0.197)
Reform*Family income		-0.027		0.004		0.004
		(0.016)+		(0.016)		(0.016)
Reform of partner					Yes	Yes
Observations	110907	110907	110907	110907	110907	110907
R-squared	0.66	0.66	0.64	0.64	0.64	0.64
	B. Women					
Reform	-0.000	0.223	0.013	-0.180	0.012	-0.164
	(0.013)	(0.361)	(0.014)	(0.376)	(0.014)	(0.376)
Reform*Family income		-0.018		0.016		0.015
		(0.030)		(0.031)		(0.031)
Reform of partner					Yes	Yes
Observations	32315	32315	32315	32315	32315	32315
R-squared	0.58	0.58	0.55	0.55	0.55	0.55
	C. Men					
Reform	-0.002	0.700	-0.014	-0.035	-0.013	-0.071
	(0.012)	(0.331)*	(0.014)	(0.368)	(0.014)	(0.367)
Reform*Family income		-0.058		0.002		0.005
		(0.028)*		(0.030)		(0.030)
Reform of partner					Yes	Yes
Observations	32782	32782	32782	32782	32782	32782
R-squared	0.63	0.63	0.54	0.54	0.54	0.54

Notes: Log income for child and partner is a measure of permanent income; the average of log income in 1987, 1990, 1993 and 1996. Parent's log family income is the average of the log of family income in the years 1968, 1969 and 1970. Family income is defined as the sum of mother's and father's income. Standard errors in parentheses are clustered on family. + significant at 10%; * significant at 5%; ** significant at 1%.
All estimates include child cohort effects, birth order effects and family fixed effects. In columns 2, 4 and 6 cohort and birth order effects are interacted with parents' log income. In column 6, partner's reform is also interacted with parental income. The upper panel controls for gender.

Table 6

The effect of the reform at different quartiles of parental income
Differences-in-differences estimates

	(1) 1st quartile	(2) 2nd quartile	(3) 3rd quartile	(4) 4th quartile
Independent variable	A. Dependent variable: Child's log income - all			
Reform	0.007	0.003	0.012	-0.016
	(0.009)	(0.008)	(0.008)	(0.009)+
Observations	31249	31249	31249	31249
R-squared	0.25	0.30	0.30	0.29
	B. Child's log income - women			
Reform	-0.003	0.007	0.011	-0.022
	(0.013)	(0.012)	(0.011)	(0.013)+
Observations	15773	15773	15773	15772
R-squared	0.10	0.10	0.08	0.10
	C. Child's log income – men			
Reform	0.013	-0.000	0.012	-0.008
	(0.013)	(0.011)	(0.012)	(0.014)
Observations	15477	15476	15476	15476
R-squared	0.09	0.08	0.08	0.09
	D. Partner's log income - all			
Reform	-0.001	0.003	-0.010	-0.007
	(0.008)	(0.008)	(0.008)	(0.009)
Observations	31249	31249	31249	31249
R-squared	0.30	0.32	0.32	0.30
	E. Partner's log income - women			
Reform	-0.006	0.010	0.005	-0.009
	(0.011)	(0.012)	(0.012)	(0.015)
Observations	15773	15773	15773	15772
R-squared	0.08	0.09	0.08	0.08
	F. Partner's log income - men			
Reform	0.006	-0.011	-0.022	-0.000
	(0.012)	(0.011)	(0.011)*	(0.011)
Observations	15477	15476	15476	15476
R-squared	0.11	0.10	0.11	0.09

Notes: Standard errors in parentheses are clustered on municipality. + significant at 10%; * significant at 5%; ** significant at 1%. All estimates include child cohort effects, father cohort effects and municipality effects.

Table 7

The effect of the reform at different quartiles of parental income
Sibling-difference estimates

	(1) 1st quartile	(2) 2nd quartile	(3) 3rd quartile	(4) 4th quartile
Independent variable	A. Dependent variable: Child's log income - all			
Reform	0.016	0.019	-0.010	-0.012
	(0.014)	(0.013)	(0.013)	(0.015)
Observations	27729	27726	27727	27725
R-squared	0.60	0.64	0.66	0.68
	B. Child's log income - women			
Reform	0.016	0.018	-0.020	-0.008
	(0.026)	(0.026)	(0.025)	(0.028)
Observations	8080	8078	8080	8077
R-squared	0.56	0.55	0.57	0.60
	C. Child's log income – men			
Reform	0.020	0.019	-0.013	-0.034
	(0.025)	(0.022)	(0.023)	(0.027)
Observations	8196	8196	8195	8195
R-squared	0.59	0.58	0.59	0.65
	D. Partner's log income - all			
Reform	0.001	-0.011	-0.000	0.001
	(0.014)	(0.014)	(0.014)	(0.016)
Observations	27729	27726	27727	27725
R-squared	0.62	0.62	0.63	0.65
	E. Partner's log income - women			
Reform	0.012	0.022	0.008	0.013
	(0.026)	(0.026)	(0.027)	(0.031)
Observations	8080	8078	8080	8077
R-squared	0.51	0.52	0.52	0.58
	F. Partner's log income - men			
Reform	-0.018	-0.029	0.012	-0.016
	(0.028)	(0.027)	(0.027)	(0.028)
Observations	8196	8196	8195	8195
R-squared	0.53	0.51	0.54	0.55

Notes: Standard errors in parentheses are clustered on family. + significant at 10%; * significant at 5%; ** significant at 1%. All estimates include child cohort effects, birth order effects and family fixed effects.

Appendix A

The Educational Reform – Coding and Quantitative Development

The first cohort affected by the educational reform was the cohort born in 1938. For cohorts born before 1943 it is not possible to identify the reform status of individuals, whereby I am obliged to drop these cohorts. The reason it is not possible to identify the reform status of the pre-1943 cohorts is the following: I assign reform status based on home municipality in the 1960 or 1965 censuses (when the individuals are 10-17 years old), and based on year of birth.[30] Observing the pre-1943 cohorts in the census of 1960 is too late – individuals might have left home for work or studies, so they might not be assigned to the right municipality. Another potential alternative would be to assign individuals based on their municipality of birth. To obtain information on municipality of birth, it is possible to use parish of birth and then map that to the respective municipalities. However, pre-1947, parish of birth states the parish of the hospital where the individual was born, which can be different from the parish where the individual lived.

After concluding that for pre-1943 cohorts it is a difficult task to assign the reform based on municipality and year of birth, I now turn to the coding of the reform. The coding is not straightforward, mainly for two reasons. First, the documentation on the implementation is scarce, and second, the reform was in some cases implemented in parts of municipalities at different points in time, which introduces error when assigning the reform based on municipality.

I use four sources to obtain a reliable coding of the reform implementation. The first two are the documentation of participating municipalities (and parts of municipalities) in Marklund (1981) and in National Board of Education (1954-62) (a yearly publication describing the development of the reform). With this information it is possible to code cohorts

[30] For cohorts born until 1950 I use the 1960 census, for cohorts born 1951-1955 I use the 1965 census to assign their reform status.

born until 1949. For later cohorts I use educational statistics on municipality level, describing the number of pupils in each grade and school system (the old *folkskola* and the new comprehensive school) (Bureau of Educational Statistics 1960-64, Statistics Sweden 1968, 1969). From the tables it is possible to see for which cohort the reform is implemented at large – that is, the first cohort where all pupils are in the new school system and there are no more pupils of that cohort in the old school. In most cases this is a clear-cut distinction, whereas in some cases the transition into the new school is gradual over two cohorts. In those cases, the reform applies to the majority of pupils.

In some cases, it has been impossible to determine the timing of the reform. A few municipalities have been excluded for this reason. The excluded municipalities are the following: Södertälje, Sundbyberg, Linköping, Jönköping, Hälsingborg and Skellefteå.

The three big cities Stockholm, Gothenburg and Malmö were all early implementers, but only in parts of the municipalities. Based on information on parish level in the 1960 and 1965 censuses, I am able to exclude individuals residing in parts of the cities that implemented before the 1943 cohort, and the remaining parts of the municipalities are coded according to a uniform implementation year.

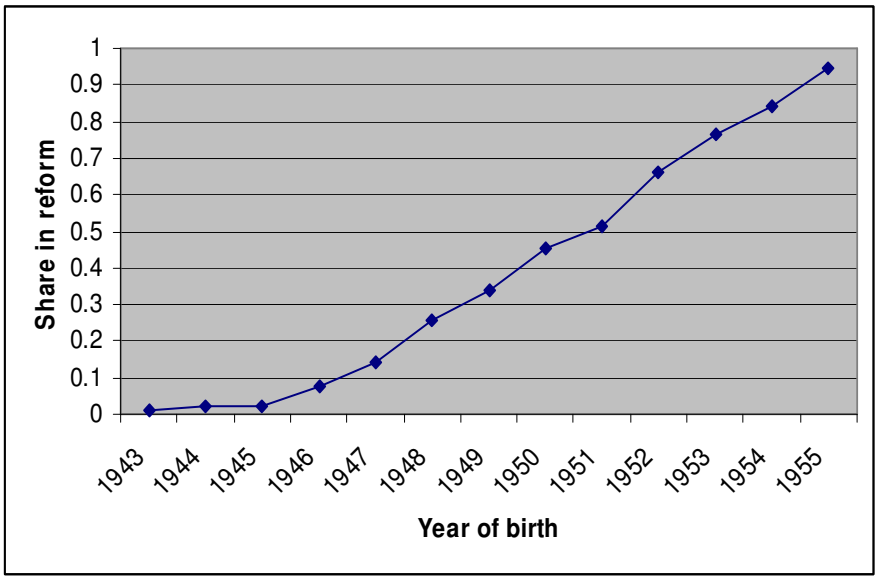
In order to assess the reliability of the coding, I match the reform coding to the IS-data (individual statistics) used in the Meghir and Palme (2005) study of the Swedish educational reform. The IS-data contain information on reform participation for cohorts born in 1948 and 1953; the reform is assigned on individual level by information from the respective schools. The Meghir and Palme (2005) data set (available on www.aeaweb.org) provide information on the municipality in which the individual went to school at age 12. This is to be compared to the municipality of residence at age 10 to 17 in the data set used in this study. Assuming that municipality of residence is a good indicator for school municipality, I match "my" code to the Meghir and Palme data. With two independent measures of reform

status, I obtain a reliability ratio of 0.94. This is a high reliability ratio and points to two facts: a) the quality of the coding used in this study is good and b) attenuation bias caused by measurement error in the reform coding should be relatively low.

Finally, to get an idea of the implementation of the reform, Figure A1 depicts the quantitative development of the reform as in the sample of this study.

Figure A1

Quantitative development of the reform



Appendix B

Table B 1

Descriptive statistics for the income samples

Variable	All The sample used for estimation (1) Mean (St. Dev)	All The non-restrictive sample (2) Mean (St.Dev)
The random sample		
Child's log income	11.85	11.86
	(0.48)	(0.49)
Parents' log family income	12.12	12.12
	(0.49)	(0.49)
Log partner's income	11.84	11.85
	(0.49)	(0.50)
Reform	0.48	0.38
	(0.50)	(0.49)
Reform of partner	0.46	0.38
	(0.50)	(0.49)
Child >10 years of edu	0.75	0.74
	(0.43)	(0.44)
Partner >10 years of edu	0.74	0.73
	(0.44)	(0.44)
Child's year of birth	1950.15	1948.99
	(3.48)	(3.72)
Father's year of birth	1921.88	1917.05
	(4.66)	(7.77)
Woman	0.50	0.50
	(0.50)	(0.50)
n	124996	202951

Notes: All incomes are expressed in 1990 prices.

Table B2

Intergenerational income elasticities
Differences-in-differences estimates
Results for all individuals - including also childless and single individuals

Dependent variable	(1)	(2)	(3)
	Child's log income	Child's log income	Child's log income
Independent variable			
	A. All		
Parents' log family income	0.178	0.178	0.227
	(0.003)**	(0.003)**	(0.019)**
Reform		0.000	0.152
		(0.003)	(0.078)+
Reform*Family income			-0.013
			(0.006)+
Observations	226743	226743	226743
R-squared	0.20	0.20	0.21
	B. Women		
Parents' log family income	0.136	0.136	0.158
	(0.003)**	(0.003)**	(0.018)**
Reform		-0.001	0.139
		(0.004)	(0.119)
Reform*Family income			-0.011
			(0.010)
Observations	110937	110937	110937
R-squared	0.06	0.06	0.07
	C. Men		
Parents' log family income	0.217	0.217	0.226
	(0.005)**	(0.005)**	(0.028)**
Reform		0.002	0.179
		(0.005)	(0.121)
Reform*Family income			-0.015
			(0.010)
Observations	115806	115806	115806
R-squared	0.08	0.08	0.09

Notes: Log income for child is a measure of permanent income; the average of log income in 1987, 1990, 1993 and 1996. Parent's log family income is the average of the log of family income in the years 1968, 1969 and 1970. Family income is defined as the sum of mother's and father's income.
Standard errors in parentheses are clustered on municipality. + significant at 10%; * significant at 5%; ** significant at 1%.
All estimates include child cohort effects, father cohort effects and municipality effects. In column 3 all controls are interacted with parents' log family income. The upper panel also controls for gender.

Table B3

Intergenerational income elasticities
Sibling-difference estimates
Results for all individuals - including also childless and single individuals

	(1)	(2)
Dependent variable	Child's log income	Child's log income
Independent variable		
	A. All	
Reform	0.002	0.142
	(0.004)	(0.115)
Reform*Family income		-0.012
		(0.010)
Observations	274060	274060
R-squared	0.60	0.60
	B. Women	
Reform	0.005	0.084
	(0.008)	(0.214)
Reform*Family income		-0.007
		(0.018)
Observations	81873	81873
R-squared	0.56	0.56
	C. Men	
Reform	-0.003	0.413
	(0.008)	(0.215)+
Reform*Family income		-0.034
		(0.018)+
Observations	89620	89620
R-squared	0.59	0.59

Notes: Log income for child is a measure of permanent income; the average of log income in 1987, 1990, 1993 and 1996. Parent's log family income is the average of the log of family income in the years 1968, 1969 and 1970. Family income is defined as the sum of mother's and father's income. Standard errors in parentheses are clustered on family. + significant at 10%; * significant at 5%; ** significant at 1%.
All estimates include child cohort effects, birth order effects and family fixed effects. In column 2 cohort and birth order effects are interacted with parents' log income. The upper panel controls for gender.

Table B4

The effect of the reform at different quartiles of parental income
Differences-in-differences and sibling-difference estimates
All estimates include controls for the reform of the partner

	(1)	(2)	(3)	(4)
Independent variable	1st quartile	2nd quartile	3rd quartile	4th quartile
Differences-in-differences	A. Dependent variable: Partner's log income - all			
Reform	-0.000	0.002	-0.010	-0.005
	(0.008)	(0.008)	(0.008)	(0.009)
Observations	31249	31249	31249	31249
R-squared	0.30	0.32	0.32	0.30
	B. Partner's log income - women			
Reform	-0.010	0.005	0.002	-0.012
	(0.011)	(0.012)	(0.012)	(0.015)
Observations	15773	15773	15773	15772
R-squared	0.08	0.09	0.08	0.08
	C. Partner's log income – men			
Reform	0.009	-0.010	-0.019	0.003
	(0.012)	(0.011)	(0.011)+	(0.011)
Observations	15477	15476	15476	15476
R-squared	0.11	0.10	0.11	0.09
Sibling differences	D. Dependent variable: Partner's log income - all			
Reform	0.001	-0.010	-0.001	0.002
	(0.014)	(0.014)	(0.014)	(0.016)
Observations	27729	27726	27727	27725
R-squared	0.62	0.62	0.63	0.65
	E. Partner's log income - women			
Reform	0.011	0.020	0.006	0.011
	(0.026)	(0.027)	(0.028)	(0.031)
Observations	8080	8078	8080	8077
R-squared	0.51	0.52	0.52	0.58
	F. Partner's log income – men			
Reform	-0.018	-0.029	0.012	-0.013
	(0.028)	(0.027)	(0.027)	(0.028)
Observations	8196	8196	8195	8195
R-squared	0.53	0.51	0.54	0.55

Notes: Standard errors in parentheses are clustered on municipality in panels A-C, on family in panels D-F.
+ significant at 10%; * significant at 5%; ** significant at 1%. All estimates include reform status of the spouse, child cohort effects and father cohort effects. Panels A-C include municipality fixed effects, panels E-F include family fixed effects.