Swedish Institute for Social Research (SOFI)

Stockholm University

WORKING PAPER 4/2006

INTERGENERATIONAL MOBILITY AND ASSORTATIVE MATING
EFFECTS OF AN EDUCATIONAL REFORM

by

Helena Holmlund

Intergenerational Mobility and Assortative Mating[*] Effects of an Educational Reform

Helena Holmlund[†]
Swedish Institute for Social Research
Stockholm University

May 10, 2006

Abstract

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, which postponed ability tracking and extended compulsory education from seven to nine years. The reform may have influenced intergenerational mobility by several different mechanisms. First, there is the possibility of a direct effect of extending compulsory education. Second, the age at which ability 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. Third, recognizing that economic well-being is determined by the income of the household, assortative mating plays a major role in the mobility process. I argue that the peer group in which couples form can be affected by the educational system, and evaluate how the reform affects intergenerational mobility through changes in assortative mating. Differences-in-differences estimates and sibling-difference estimates indicate that the reform indeed resulted in a sizeable increase in intergenerational income mobility, and in a lower educational association between children and parents. The reform also contributed to reducing the association in education between an individual's partner and parents, which I interpret as an effect operating through reform effects on mating patterns.

[*]I wish to thank Anders Björklund, Bertil Holmlund, Mikael Lindahl, Åsa Rosén, Marianne Sundström, Björn Öckert and seminar participants at the Swedish Insitute for Social Research and at Växjö University for helpful comments. Thanks also to Valter Hultén for help with the coding of the reform. Financial support from the Swedish Council for Working Life and Social Research is gratefully acknowledged.
[†] Swedish Institute for Social Research, Stockholm University, SE-106 91 Stockholm, SWEDEN. Phone: +46-8-16 22 30, e-mail: Helena.Holmlund@sofi.su.se.

1. Introduction

Various measures of intergenerational mobility have been used to assess the degree of equality of opportunity in a society. High mobility, meaning a low association between parent and child in terms of economic outcomes, has been interpreted as equal life chances, since parental background plays a small role in determining an individual's economic success.

Despite numerous papers focusing on the degree and measurement of intergenerational income mobility (Solon 1992, Björklund and Jäntti 1997, Haider and Solon 2006), less is known about the mechanisms underlying mobility, that may potentially explain differences in mobility across countries and over time. 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, which abolished ability tracking that before the reform started either in 5th or 7th grade, and which extended compulsory education from seven to nine years.[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. 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. At an early stage, one can also assume that information on ability is noisy such that parents 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]

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

[3] 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.

[4] Ermisch, Francesconi and Siedler (2006) argue that differences in intergenerational mobility between Great Britain and Germany might be explained by the lower tracking age in Germany.

Solon 2002, Blanden 2005, Ermisch, Francesconi and Siedler 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. Previous studies show that about 40-50 percent of the covariance between parents' and own permanent family income can be attributed to assortative mating (Ermisch, Francesconi and Siedler 2006).

Turning back to the mechanisms through which the educational reform may affect intergenerational mobility, the third and final mechanism I have in mind operates through assortative mating. I argue that the school shapes the peer group of individuals; 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.

Since the Swedish school reform postponed ability tracking by three years and kept all pupils in one comprehensive school throughout nine years, I evaluate whether this has any implications for assortative mating patterns, and thus for intergenerational mobility. 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 later in life, and if so, possibly also assortative mating.

The purpose of the study is to evaluate the effects of the Swedish compulsory schooling reform on intergenerational mobility, and to assess the extent to which the effect operates through assortative mating. From a policy perspective, one might argue that the underlying mechanisms (i.e., a direct effect or assortative-mating effect) are of less

importance; the policy maker cares more about the overall policy outcome. I would like to argue, however, that a better understanding of the underlying mechanisms will help us shape future 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.[5]

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. I use the same specification to also study intergenerational mobility in education. 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.

[5] Assortative mating-effects may be policy relevant in the context of intergenerational mobility in yet another way (albeit out of the scope of this study). Following Erikson (1984), it is possible to assume that the highest level of education among the two parents in a family will be transferred to the children. Now, assume that our educational reform decreases assortative mating. This implies that high educated individuals to a lesser extent will be coupled together, and more likely to be matched with a lower educated spouse. This means that the effect on the education of *their* children will be affected. There is no extra gain for the children if both parents are highly educated, compared to only one. If the highly educated individuals are spread out and matched with lower educated ones, more children will be subject to the positive effect of having a highly educated parent.

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 in the data.

I find that the reform lead to sizeable increases in intergenerational income mobility and educational mobility. The reform reduced the intergenerational income elasticities and the intergenerational transmission of education between children and parents. Also, the education coefficient between an individual's partner and his/her parents was reduced, indicating that mating patterns were affected in a way that contributed to increased economic mobility.

The paper unfolds as follows: section 2 presents previous literature, section 3 describes the Swedish educational reform, section 4 presents a structural 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

The Swedish compulsory school reform and its long-run consequences for inequality have been studied previously by for example 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 a clear indication that intergenerational mobility was affected by the introduction of the reform.

A number of studies from different countries focus more explicitly on the link between the educational system and intergenerational mobility or inequality. For example, Machin (2004) studies changes over time in intergenerational mobility in Britain, and links it to changes in the educational system. 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. 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. Pekkala, Pekkarinen and Uusitalo (2006) make use of an educational reform in Finland, similar to the Swedish reform, to assess the effects of postponing ability tracking and increasing compulsory education, on intergenerational income correlations. 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. 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. Further evidence on tracking is found in Dustmann (2004) and Restuccia and Urrutia (2004).

Also related to this paper is the literature on the importance of assortative mating for intergenerational mobility. 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 schooling reform, compulsory education mounted to seven (or in some cases eight) years of education. Ability tracking took place either starting in 5^{th} grade, with a five year junior-secondary school (*realskola*) following, or starting in 7^{th} 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 7^{th} or 8^{th} 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 ability 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.[6] However, the 9th 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).

[6] 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.

4. The Educational System and Intergenerational Mobility: Structural 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 simple and stylized; its purpose is not to fully describe the intergenerational mechanisms, but rather to make use of a few simplifying assumptions (in particular regarding the choice of functional form) in order to derive the equations that I estimate empirically later on in the paper. Also, the purpose is to show 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.

The conceptual framework is the following: parents care about their own consumption, (C_{t-1}), and about the expected future economic status of their child, which is the sum of the log permanent income of the child and his or her partner, $E(\log y_t + \log y_t^P)$, where *t* indicates the generation and *P* denotes the partner.[7] Note that this model assumes only one child. Parents can increase their child's earnings potential by investing in the child's human capital:

[7] 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.

$$h_t = \theta \log(\overline{H}_{t-1} + I_{t-1}) \quad (1)$$

$\overline{H}_{t-1}$ represents the public investment in a child's human capital, which we can think of as the compulsory education level. I_{t-1} reflects the parental investment in the child. The investments in the child are transformed into human capital through the function (1); $\theta > 0$ indicates a positive marginal product of investing in the child's human capital, and the functional form implies decreasing marginal product. For simplicity, 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.

In this model, I define assortative mating in terms of human capital. Given that parents care about the expected sum of the log of both their child's income and the income of his/her spouse, $E(\log y_t + \log y_t^P)$, parents are sensitive to the degree of assortative mating in society, since it will determine the earnings potential of their child's partner. 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:

$$E\left(h_t^P\right) = (1-\alpha)\overline{h}_t^P + \alpha h_t \quad (2)$$

With probability α their child will meet someone with human capital equal to their own, and with probability $(1-\alpha)$ they will meet a randomly drawn partner from the peer group, where the peer group mean of human capital is $\overline{h}_t^P$. $0 < \alpha < 1$ will therefore represent the degree of assortative mating. For clarity, mating on human capital here refers to completed human capital, not necessarily to human capital at the time of mating.

After having defined the matching process, and the assortative mating parameter α, I will turn to the parents' maximization problem. Permanent income of the child is increasing in human capital (at the same rate for both spouses):

$$\log y_t = \gamma_0 + \gamma_1 h_t \quad (3)$$

$$\log y_t^P = \gamma_0 + \gamma_1 h_t^P \quad (4)$$

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

$$\text{Max } U = b\left[E(\log y_t + \log y_t^P)\right] + (1-b)\log(C_{t-1}) \quad (5)$$

where $b \in (0,1)$ indicates the relative preference for future earnings of the child and his/her partner compared to own consumption. Parents maximize their utility subject to equations (2) and (3)-(4) and the budget constraint $y_{t-1} = C_{t-1} + I_{t-1}$.

Solving the parents' maximization problem, the optimal parental investment in the child's human capital is obtained as:

$$I_{t-1} = \frac{b\gamma_1\theta(1+\alpha)}{1-b(1-\gamma_1\theta(1+\alpha))} y_{t-1} - \frac{(1-b)}{1-b(1-\gamma_1\theta(1+\alpha))} \overline{H}_{t-1} \quad (6)$$

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 $\theta\gamma_1$. Clearly, holding public investment constant, higher income families invest more in their child's human capital. Finally, the second term of (6) represents a negative compensation effect: parents internalize the positive effect of higher public investment and reduce their investment accordingly, by a factor of their preference for own consumption. The stronger their preference for own consumption, the more they reduce their investment in the child's human capital.

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

$$\log y_t = \gamma_0 + \gamma_1\theta \log\left[\frac{b\gamma_1\theta(1+\alpha)}{1-b(1-\gamma_1\theta(1+\alpha))}\right] + \gamma_1\theta \log\left[y_{t-1} + \overline{H}_{t-1}\right] \quad (7)$$

$$\log y_t^P = \gamma_0 + \gamma_1(1-\alpha)\overline{h}^P_t + \alpha\gamma_1\theta \log\left[\frac{b\gamma_1\theta(1+\alpha)}{1-b(1-\gamma_1\theta(1+\alpha))}\right] + \alpha\gamma_1\theta \log\left[y_{t-1} + \overline{H}_{t-1}\right] \quad (8)$$

I will continue to follow the model in Solon (2004), in order to derive an intergenerational income elasticity as a function of the educational system. First, equation (7) can be rewritten as:

$$\log y_t = \gamma_0 + \gamma_1\theta \log\left[\frac{b\gamma_1\theta(1+\alpha)}{1-b(1-\gamma_1\theta(1+\alpha))}\right] + \gamma_1\theta\log\left\{y_{t-1}\left[1+\frac{\overline{H}_{t-1}}{y_{t-1}}\right]\right\}$$

If the ratio $\frac{\overline{H}_{t-1}}{y_{t-1}}$ is small, the permanent log income of the child can be approximated in the following way:

$$\log y_t \cong \gamma_0 + \gamma_1\theta \log\left[\frac{b\gamma_1\theta(1+\alpha)}{1-b(1-\gamma_1\theta(1+\alpha))}\right] + \gamma_1\theta\log y_{t-1} + \gamma_1\theta\frac{\overline{H}_{t-1}}{y_{t-1}} \qquad (9)$$

Equation (9) shows that the child's income is a function of the ratio of public investment to parental income, that is, public policy (or in my interpretation, the investment in compulsory education) has an effect on intergenerational mobility. The mechanism explaining this relationship is that $\frac{\overline{H}_{t-1}}{y_{t-1}}$ is decreasing and concave in y_{t-1}. An increase in the public investment $\overline{H}_{t-1}$ will have a larger impact on the child's income for children from low-income families. Once again following Solon (2004), I assume that the public investment is given by:

$$\frac{\overline{H}_{t-1}}{y_{t-1}} = \varphi - \sigma\log y_{t-1} \qquad (10)$$

The parameter $0 < \sigma < 1$ describes the rate at which public investment relative to parental income is decreasing in parental income. The more positive is σ, the larger is the effect of the policy on the income of children from low-income parents compared to its effect for children from high-income families.

Defining the public investment as in equation (10) allows us to re-write the income expressions for the child and his or her spouse:

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

where $\gamma_0^* = \gamma_0 + \gamma_1\theta \log\left[\frac{b\gamma_1\theta(1+\alpha)}{1-b(1-\gamma_1\theta(1+\alpha))}\right] + \gamma_1\theta\varphi$

$$\log y_t^P \cong \gamma_0^{P*} + \alpha\gamma_1\theta(1-\sigma)\log y_{t-1} \quad (12)$$

where $\gamma_0^{P*} = \gamma_0 + \gamma_1(1-\alpha)\bar{h}^P_t + \alpha\gamma_1\theta \log\left[\frac{b\gamma_1\theta(1+\alpha)}{1-b(1-\gamma_1\theta(1+\alpha))}\right] + \alpha\gamma_1\theta\varphi$

The stylized model has thus, in equations (11) and (12), 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. Deriving the intergenerational elasticities, with respect to own parents and with respect to parents-in-law, gives the following:

$$\frac{d\log y_t}{d\log y_{t-1}} = \gamma_1\theta(1-\sigma) \quad (13)$$

$$\frac{d\log y_t^P}{d\log y_{t-1}} = \alpha\gamma_1\theta(1-\sigma) \quad (14)$$

The intergenerational elasticity measures, expressed in terms of the structural parameters of the above model, show that the intergenerational mobility (defined as (1- $\gamma_1\theta(1-\sigma)$) is decreasing in the return to human capital $\gamma_1\theta$, and increasing in σ, the progressivity of public investments in children's human capital. The elasticity with respect to parents-in-law depends positively on the degree of assortative mating α. If mating is random, such that $\alpha = 0$, the incomes of the partner and the parents will be uncorrelated.

Finally, it should be noted that 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 to equations (11) and (12)).

4.2 Interpreting the Model: The Swedish Compulsory School Reform

The Swedish compulsory schooling reform increased mandatory education from seven to nine years, and postponed ability 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. The reform also shifts up the policy parameter φ, which in the above equations enters as a level effect of the reform on the child's income.

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 to mate 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 by doing so, increasing 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 (see equation (14)).

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

Turning to the empirical specifications, I now introduce the reduced-form counterparts of the above equations that relate parental permanent income to the permanent income of their children. The intergenerational elasticity, 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. Assuming equal variances, the elasticity measures mobility in an individual's position in the income distribution.

The empirical results of the paper are based on the following baseline specifications:

$$\log y_{t,icm} = \beta_0 + \beta_1 \log y_{t-1,icm} + \beta_2 R_{cm} + \beta_3 (\log y_{t-1,icm} * R_{cm}) + v_c + z_m + e_{t,icm} \qquad (15)$$

where $\log y_{t,icm}$ represents the log of permanent income for individual *i*, belonging to cohort *c*, going to school in municipality *m*. R_{cm} is an indicator variable that takes the value 1 if in cohort *c*, municipality *m*, the reform was in effect. $\log y_{t-1,icm}$ represents the the log of permanent parental family income, v_c and z_m capture cohort and municipality effects, respectively. Omitted from (15) but included in all regressions are birth year indicators for the father of individual *i*.

Now, it is clear that we can relate the coefficients to the structural equations above: $\beta_1 + \beta_3 * R_{cm} = \gamma_1\theta(1-\sigma)$.

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

$$\log y^{P}_{t,icm} = \delta_0 + \delta_1 y_{t-1,icm} + \delta_2 R_{cm} + \delta_3 (\log y_{t-1,icm} * R_{cm}) + \nu_c + z_m + e_{t,icm} \qquad (16)$$

Omitted from (16) but included in all regressions are birth year indicators for the father of individual *i*. The coefficients in equation (16) can be written in terms of their structural counterparts in the following way: $\delta_1 + \delta_3 * R_{cm} = \alpha\gamma_1\theta(1-\sigma)$. In the empirical section of this paper, this specification is augmented with controls for the reform status of the partner, and also with cohort and municipality effects for the partner.

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 1932 to 1967. Of these I will use the 1945-1955 cohorts; those cohorts were affected by the educational reform, and to those I am also able to assign a reform indicator stating whether the individual was subject to the reform or not.[8] 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 1945-1955 cohorts.

[8] Appendix A provides an extensive description of the coding of the educational reform, and its quantitative development.

For the sampled individuals, I also use the education register in 1990, which contains information on each individual's highest educational degree.[9] 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 1945-1955 (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 32-51 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.

Permanent income of the parents of the 1945–1955 cohorts is measured as the average of log family income in the years 1968, 1969 and 1970.[10] This implies that I observe the parents' income for the first time when the children are 13 to 23 years of age. For the older cohorts, this income measure might not reflect the economic status of the family as they grew up.[11] 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 income. Any observations for parents older than 55 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.

[9] 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.

[10] Family income is defined as the sum of mother's and father's total income.

[11] 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.

Parental education is taken from the 1970 census. The census provides information on completed levels of schooling and is converted to years of schooling in the same way as child's schooling. One advantage of the education outcome is that once education is completed, it is constant over the life cycle. Therefore, the education sample is less restrictive and does not exclude older parents. In Table B1 (Appendix B), I provide descriptive statistics for non-restrictive samples (including individuals whose parents' income is missing or has been excluded due to sample restrictions, or whose parental education is missing), compared to the samples used for estimation. The upper panel of 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.

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.[12] 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.[13] 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 1945-1955 cohorts. The other sample is a sibling sample, which singles out the individuals from the random sample who have siblings born in 1945-1955, 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

[12] 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 20 years.

[13] One potential mechanism is that the reform effects whether individuals mate in itself, which would introduce a sample selection problem. However, there is no effect of the reform on mating, as defined in this study.

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

The main purpose of the empirical analysis is to estimate the effect of the compulsory schooling reform on the intergenerational elasticities, as specified in equations (15) and (16). 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. 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 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. Therefore, any bias due to endogeneity of the reform should be eliminated. 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] Also important, by using sibling-differences rather than a cross-section, the causal effects of the reform can be more precisely estimated. Note that birth-order effects on mobility are automatically

[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.

controlled for, since the sample also consists of sibling pairs where there is no variation in their reform status.[15]

The upper panel of Table 1 reports on descriptive statistics for the income sample. Log of parents' family income (in 1990 prices) is higher than the log of the child's income, which reflects that parental income is the sum of both parents' income. About 53 percent of the individuals were affected by the compulsory schooling reform, and due to the age difference between spouses, men are more likely than women to have a partner that went through the new compulsory school.

6.1 Reform Effects on Assortative Mating

Before turning to the results on intergenerational mobility, I start out by presenting estimates for the structural parameter α (assortative mating), as described in section 5. The purpose is to illustrate one of the parameters of the model, and to obtain an idea of the degree to which assortative mating might influence intergenerational mobility. Moreover, I take a look at the impact of the educational reform on the degree of assortative mating. The estimates are based on the income sample.[16]

The model in section 4 assumes assortative mating on human capital. When estimating assortative mating, I use years of schooling as a measure of human capital, but for completeness, I also explore assortative mating on income. I estimate α by regressing partner's schooling on own schooling, and include an interaction term (reform*own schooling) to estimate the effect of the reform on assortative mating. When estimating reform effects, I use the differences-in-differences and sibling differences models, as described above.

[15] Lindahl (2002) shows that the intergenerational income elasticity decreases with birth order for a given family size. However, this is not a concern in this study where birth order effects are captured by differencing also over siblings with the same reform status.

[16] 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.

Table 2, columns 1 and 4, show that the educational assortative mating parameter is higher for women than for men (0.54 compared to 0.38 for the differences-in-differences specification in panel A, and 0.32 compared to 0.22 for the sibling differences in panel B), meaning that moving up the educational distribution, a woman is more likely than a man to mate with an equally educated partner. This result confirms previous findings that assortative mating plays a larger role for women than for men in Sweden (Hirvonen 2006). The result can also be compared to the correlation of 0.68 between partners' education that is found for Sweden in Björklund (1992).

Turning to the interaction effect (reform*own schooling), in order to assess the extent to which the reform affects assortative mating, columns 2 and 5 in panel A show that the reform actually *increases* assortative mating, contradictory to the initial hypothesis. In the lower panel, which presents results from using a sibling difference approach to identify the effect, there is no indication that the reform affects the degree of assortative mating.

The identification of reform effects on spouse's outcomes is non-trivial. The main reason is that the child's reform status and other characteristics are positively correlated with that of the spouse, also in the absence of any reform effects on mating patterns. This is the case since partners often come from the same region and are close in age. Therefore, explaining spouse's outcome (as in the assortative mating regression and in the intergenerational elasticity estimates below) with the child's own reform status, will capture both possible mating effects, but also spouse's own reform effect on his/her outcome. One remedy to this problem is to fully control for the spouse's own reform status, age and municipality. This should capture effects on the spouse's outcome that are related to his/her own characteristics. However, it is not a fully satisfying strategy, since partner characteristics might be endogenously determined by the child's reform participation. The idea is to empirically estimate potential mating effects from changing the distribution of potential

partners; then it is not ideal to fully control for changes in the partner distribution. But without these controls, I am at risk of capturing an effect that stems from a mechanical correlation between partners' characteristics.

By providing a complete set of results, both with and without controls for the partner, I am however able to obtain upper and lower bounds of the parameters of interest. Excluding controls for the spouse I might overestimate (in absolute terms) the reform effect on mating, and this will provide the upper bound of the effect. Including the controls for the spouse, I likely obtain (in absolute terms) a lower bound on the effect.

That said, columns 3 and 6 in Table 2 present the reform effects on mating with a complete set of controls also for the partner. The controls for the partner are the following: partner's reform status, its interaction with child's own schooling and cohort effects and municipality effects of the partner. In the upper panel of the table, the inclusion of these controls does not influence the parameter estimates. In the lower panel, the sibling analysis shows that for women, there is now a positive effect of the reform on assortative mating, but no such effect for men.

Assortative mating can also be characterized with respect to income. Table 3 reports on the estimates of assortative mating on permanent income of the children in this study, and their partners. First, the gender difference in assortative mating, indicated in Table 2, is no longer so clear. The differences-in-differences in the upper panel show no effects of the reform on assortative mating, whereas the sibling differences in the lower panel show that the reform reduces the degree of assortative mating for men; a reduction in the range 0.05-0.06 of a baseline assortative mating coefficient of 0.10. That is, when it comes to assortative mating on income, the initial hypothesis of the effects of the reform is confirmed.

To sum up, I find mixed evidence of the effects of the reform on assortative mating. The reform seems to increase assortative mating on education for women, while it decreases

assortative mating on income for men. This result is not surprising, however. Traditionally, it is more socially accepted for men to find a partner from a lower social class, while it is less so for women.

6.2 Reform Effects on Intergenerational Income Mobility

Now I turn to the main part of the empirical analysis in this paper; that is to estimate the reform effects on intergenerational economic mobility. I estimate intergenerational elasticities of child's (or child's partner's) log permanent income on parental log permanent family income, and evaluate whether reform participation has an impact on the income elasticity between child and parents.

Table 4 reports on the differences-in-differences results. I find intergenerational elasticities of 0.13 for women and 0.21 for men (column 1).[17] This indicates that women are more mobile than men, a finding confirmed in Hirvonen (2006). Next, turning to the intergenerational elasticities with respect to parents-in-law (column 3), I find that women and their spouses have elasticities similar in magnitude (0.13 vs. 0.14) with respect to women's parental family income. Strikingly different are the results for men, in the lower panel of Table 4. Men exhibit much higher elasticities with respect to their parents' income than do their partners (0.21 vs. 0.10). Once again, this finding is in line with Hirvonen (2006), and is also an indicator that women are more economically mobile than men. Columns 1 and 3 also include the main effect of the educational reform, which surprisingly has no effect on earnings on average.

Did the educational reform have an effect on intergenerational income mobility? Looking at the reform effects on the intergenerational elasticities between child and parent

[17] 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.

(the interaction term in column 2), the differences-in-differences strategy finds that there is no effect of the reform on income mobility. However, allowing for income mobility to work through mating patterns, columns 4 and 5 report on the upper and lower bounds of the reform effects on the partner-parent income elasticities. If the child's participation in the reform lead to a reduced relationship between his or her partner and parents (net of any effects due to mechanical correlations between partner characteristics), this is an interesting contribution to our understanding of the role of the educational system for assortative mating, and thus the distribution of income. The upper panel of the table, where women and men are pooled together, shows that a child that is affected by the reform will mate with a partner whose income will be less correlated with the child's parental income, at most. The effect of -1.2 percentage points represents a 10 percent decrease in the baseline elasticity of 0.12. When including controls for partner's own reform, this effect disappears (column 5).

As an alternative identification strategy, Table 5 summarizes the corresponding findings using a sibling difference approach. Now, these results indicate that the reform indeed lead to a significant and sizeable increase in intergenerational income mobility; the intergenerational elasticities are reduced by 3.7 and 3.8 percentage points for women and men, respectively. Comparing these to the baseline intergenerational income elasticities of 0.13 and 0.21 for women and men in Table 4, it is clear that the reform lead to large reductions in the economic persistence between generations; the decline representing 28 percent of the initial elasticity for women and 18 percent for men.[18]

The sibling difference analysis also indicates a significant upper bound (in absolute terms) on the estimate of the reform effect on the economic association between the spouse and the parents. This effect is significant for the full sample and for women, but not for men.

[18] I am here comparing the reform effects on the sibling samples with the baseline intergenerational elasticity for the random sample used in the differences-in-differences estimation. The reason is that I cannot identify the elasticity with sibling fixed effects since siblings parental income is constant and drops out. Using the sibling sample as a cross-section and estimating the intergenerational elasticities produces point estimates very close to those in Table 4.

When the women are subject to the educational reform, the elasticity between their spouse and their partner is reduced by 3.1 percentage points – a reduction almost as big as their own reform effect. This 3.1 percentage point reduction could be the result of the reform changing mating patterns, but it could also be driven by the fact that own reform and partners reform status are positively correlated. Controlling fully for the reform status and other characteristics of the spouse (column 5), the point estimate is somewhat reduced, and no longer statistically different from zero.

Summing up the findings so far, of the effects of the Swedish compulsory schooling reform on intergenerational income mobility, the sibling difference analysis shows that the reform indeed lead to quantitatively large increases in intergenerational mobility. This effect is clear when studying the income elasticities between child and parents. The evidence on whether the reform affects assortative mating, and therefore indirectly economic mobility, is less robust. There is some evidence that reform participation reduces the partner-parent elasticity, but it is not clear whether this reflects an actual mating effect or if it is driven by spouse's own characteristics.

To complete the empirical analysis of the reform effects on income mobility, Tables B2 and B3 in Appendix B present results for child's log family income. That is, the child's family income is the sum of child's own and his/her partner's income. The sibling analysis in Table B3 reveals that taking part of the new school system, the intergenerational income elasticities are reduced by 2-4 percentage points from a baseline elasticity of around 0.16. The differences-in-differences analysis comes out with insignificant point estimates apart from a reform interaction estimated with a positive sign for men. To study family income instead of spouses' individual contributions is interesting in the sense that it gives us the overall picture of the reform effects on income mobility. They are of secondary interest, however, if we want to study the mechanisms underlying this effect; these results do not enable a distinction

between the own effect the mating effect. Therefore, I concentrate on the separate effects on child's and partner's incomes in this paper.

Comparing my findings with those of Meghir and Palme (2005), I conclude that they point in the same direction. Meghir and Palme (2005) find small or insignificant average income effects of the reform, but positive effects for individuals with low-educated fathers. This is in line with the results obtained in this study; it indicates that the income return to the reform is not the same across the distribution of parental background.

A few words on the robustness of the results presented so far. One concern may be that the reform is not exogenous, and that a correlation between the reform and income mobility is driving the results. Keep in mind 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. The reform may still be correlated with regional trends, however. But including municipality-specific linear time trends in the differences-in-differences produces very similar results.[19] The sibling-difference method is also in itself a robustness check; assuming that parents treat their children in a similar way, the identification implies that the reform is uncorrelated with background characteristics and trends.

Another potential concern is related to the findings of Grawe (2006). He finds that the size of the intergenerational income correlation is decreasing in the age at which father's income is observed. The reason is that the variance in permanent earnings is increasing the older the parent is, and this implies a lower elasticity to explain the same degree of mobility. If the reform is correlated with parental age (because of regional variation in the demographic structure), we could expect this to bias the estimates. A tentative test of exogeneity is therefore presented in Table B4 (Appendix B); I regress reform status on parents' family

[19] The results are not reported in the paper but can be obtained from the author upon request.

income and on dummies for father's birth year. The regression also includes municipality dummies and cohort controls for the child. Neither parental income nor father's age can predict the reform status of the children. This is a comforting result; the reform is at least not correlated with these observed variables.

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.

6.3 Intergenerational Transmission of Education

Although the model of intergenerational mobility and assortative mating presented in section 4 refers to income, an empirical analysis similar to the one above can be made with respect to education. Education is just another concept of labour marked success, which exhibits strong intergenerational correlations, and therefore I extend my analysis to also include this concept. It is appealing because it captures mobility with respect to "full income" – it holds labour supply constant. That is, any differences in mobility between women and men that depend on labour supply differentials are held constant. All specifications are similar to those of income, with income replaced by years of schooling.

A word of caution when it comes to interpreting the results on educational mobility. The reform shifts up the lower tail of the educational distribution, and the regression analysis compares individuals in the "old" educational distribution with those in the "new" distribution. This will mechanically produce the result that the individuals subject to the reform were more mobile. From this result, we can not draw policy conclusions about positional mobility; a policy that shifts the lower tail for all individuals simultaneously will not affect the

individual's position in the distribution. However, if there is an economic return to the two more years of education that the reform implied, it will indeed affect mobility. And in the case where I study reform effects on the association between partner's and parents' education, holding spouse's characteristics and reform status constant (column 5), the change in the distribution is actually controlled for, which should enable an interpretation in terms of positional mobility.

Tables 6 and 7 present results for the random sample and the sibling sample where parental education is the mean of mother's and father's years of schooling (family education).[20] First, looking at Table 6, we find that one year of parental education is transmitted to the child with 0.46 years for men and 0.40 years for women, once again pointing to that women are more mobile than men (and that women's higher mobility is not only a labour supply effect). The education coefficients relating partners to their parents-in-law show that for women it is almost as big as women's own coefficient, whereas for men it is lower. This finding is in line with the results for income in the previous section. Turning to the reform effects, the results in Table 6 point to that the reform reduced the intergenerational association between parents' and their children's education, both for own children and for children-in-law. The results for women and men are similar in that the effect for the spouse is always lower, in particular so when controlling fully for the characteristics of the spouse. The average effect for both women and men is a decrease of 0.12 years (compared to the baseline transmission coefficient of 0.43); this corresponds to a 29 percent reduction in the educational transmission coefficient between parents and children (column 2). The findings in column 5 reveal that also when fully controlling for the characteristics of the spouse, the reform reduces the intergenerational association in education between partner and parents, by 0.02 years (a reduction of 6.5 percent). This I interpret as an effect operating through assortative mating;

[20] Descriptive statistics for the education sample are found in Table 1, panel B.

the reform changes mating patterns in a way that increases economic mobility. The sibling analysis in Table 7 confirms these findings, and finds even larger mating effects.[21]

7. Conclusions

This paper explores the educational system as a mechanism explaining intergenerational mobility. Evaluating the Swedish compulsory school reform, that extended compulsory education and postponed ability tracking, I find sizable increases in both income and educational mobility. That is, a policy that targeted the lower end of the educational distribution clearly had the implication to increase intergenerational mobility.

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 ability tracking had the consequence of keeping a more heterogeneous group of pupils together for a longer time. This might lead to a reduction in assortative mating. Although the results concerning reform effects on mating patterns are mixed, I find evidence that the reform changed the intergenerational associations between an individual's partner and parents, which I interpret as an effect working through changes in assortative mating. These results are particularly strong when it comes to the intergenerational association in education.

My findings indicate that the reform was successful in one of its purposes: increasing equality of opportunity. A final conclusion is then that the educational system

[21] Differences-in-differences results including municipality-specific time trends give similar results. They are not reported in the paper, but can be obtained from the author upon request.

indeed plays a major role in shaping social mobility. This paper offers some evidence of an intervention at the lower end of the distribution; other types of policies might be equally important in the mobility process, but are left for future research to evaluate. And as for the results of this study, there is still room to further analyse the reform effects on mobility. For example, it would be interesting to explore where in the distribution the reform increases mobility.

8. References

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 (1992), "Rising Female Labour Force Participation and the Distribution of Family Income – the Swedish Experience", *Acta Sociologica* 35:4, pp. 299-309.

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 Economic Mobility and Assortative Mating", *Economic Journal* (forthcoming).

Grawe, Nathan D. (2006), "Lifecycle Bias in Estimates of Intergenerational Earnings Persistence", *Labour Economics* (forthcoming).

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.

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 (2004), “Educational Systems and Intergenerational Mobility”, draft paper prepared for CESifo/PEPG Conference, Munich, September 2004.

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.

Pekkala, Sari, Tuomas Pekkarinen and Roope Uusitalo (2006),”Intergenerational Income Correlation and the Expansion of Secondary Education: Evidence from the Finnish Comprehensive School Reform”, preliminary draft, Uppsala University.

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.

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 samples

Variable	All (1) Mean (St. Dev)	Women (2) Mean (St. Dev)	Men (3) Mean (St. Dev)
A. The income sample			
Child's log income	11.84	11.62	12.08
	(0.48)	(0.42)	(0.42)
Parents' log family income	12.12	12.12	12.13
	(0.49)	(0.49)	(0.49)
Log partner's income	11.84	12.09	11.58
	(0.50)	(0.44)	(0.42)
Reform	0.53	0.53	0.53
	(0.50)	(0.50)	(0.50)
Reform of partner	0.49	0.33	0.66
	(0.50)	(0.47)	(0.48)
Child's year of birth	1950.68	1950.72	1950.64
	(3.05)	(3.07)	(3.03)
Father's year of birth	1922.18	1922.19	1922.17
	(4.67)	(4.68)	(4.65)
Partner's year of birth	1950.36	1948.11	1952.69
	(4.91)	(4.38)	(4.32)
Woman	0.51	1.00	0.00
	(0.50)	(0.00)	(0.00)
n	120911	61501	59410
B. The education sample			
Child's years of schooling	11.53	11.59	11.47
	(2.56)	(2.42)	(2.69)
Parents' family education	8.36	8.33	8.38
	(2.01)	(1.99)	(2.03)
Partner's years of schooling	11.42	11.23	11.61
	(2.62)	(2.88)	(2.30)
Reform	0.47	0.48	0.47
	(0.50)	(0.50)	(0.50)
Reform of partner	0.45	0.29	0.61
	(0.50)	(0.45)	(0.49)
Child's year of birth	1950.12	1950.17	1950.08
	(3.14)	(3.16)	3.12
Father's year of birth	1918.93	1918.95	1918.92
	(6.84)	(6.87)	(6.82)
Partner's year of birth	1949.83	1947.55	1952.17
	(5.00)	(4.47)	(4.39)
Woman	0.51	1.00	0.00
	(0.50)	(0)	(0)
n	167211	84816	82395

Notes: All incomes are expressed in 1990 prices.

Table 2

Estimates of assortative mating on education

Dependent variable: Partner's schooling

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Differences-in-differences estimates						
Child's years of schooling	0.536	0.514	0.488	0.384	0.362	0.397
	(0.006)**	(0.007)**	(0.008)**	(0.005)**	(0.006)**	(0.007)**
Reform		-0.614	-0.957		-0.716	-1.083
		(0.110)**	(0.122)**		(0.087)**	(0.089)**
R*Years of schooling		0.049	0.079		0.052	0.084
		(0.009)**	(0.010)**		(0.007)**	(0.008)**
Reform of partner			1.082			1.221
			(0.128)**			(0.088)**
R^p*Years of schooling			-0.069			-0.091
			(0.010)**			(0.007)**
Observations	60935	60935	60935	58980	58980	58980
R-squared	0.25	0.26	0.30	0.24	0.24	0.27
B. Sibling differences estimates						
Child's years of schooling	0.316	0.311	0.299	0.221	0.220	0.253
	(0.011)**	(0.013)**	(0.013)**	(0.008)**	(0.010)**	(0.011)**
Reform		0.006	-0.509		-0.174	-0.404
		(0.216)	(0.230)*		(0.162)	(0.170)*
R*Years of schooling		0.011	0.054		0.005	0.023
		(0.018)	(0.019)**		(0.014)	(0.015)
Reform of partner			1.419			0.995
			(0.237)**			(0.158)**
R^p*Years of schooling			-0.091			-0.068
			(0.020)**			(0.013)**
Observations	29271	29271	29271	28726	28726	28726
R-squared	0.65	0.65	0.69	0.63	0.63	0.66

Notes: Robust standard errors in parentheses are clustered on municipality (panel A) and family (panel B).
+ significant at 10%; * significant at 5%; ** significant at 1%.
R denotes reform status, R^p denotes reform status of the spouse.
The number of observations is different from the family income sample due to missing observations in the education variables.
Estimates in panel A include cohort effects and municipality effects, estimates in panel B include cohort effects and family fixed effects. Estimates in columns 3 and 6 also include controls for municipality and cohort of the partner.

Table 3

Estimates of assortative mating on income

Dependent variable: Partner's log income

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Differences-in-differences estimates						
Child's log income	0.175	0.179	0.167	0.158	0.161	0.150
	(0.008)**	(0.007)**	(0.007)**	(0.005)**	(0.007)**	(0.008)**
Reform		0.088	0.062		0.064	0.041
		(0.163)	(0.158)		(0.121)	(0.138)
R*Log income		-0.007	-0.005		-0.006	-0.004
		(0.014)	(0.014)		(0.010)	(0.011)
Reform of partner			0.070			0.051
			(0.132)			(0.137)
R^P*Log income			-0.005			-0.004
			(0.011)			(0.011)
Observations	61501	61501	61501	59410	59410	59410
R-squared	0.07	0.07	0.11	0.09	0.09	0.12
B. Sibling differences estimates						
Child's log income	0.114	0.121	0.118	0.103	0.126	0.123
	(0.010)**	(0.013)**	(0.013)**	(0.010)**	(0.013)**	(0.017)**
Reform		0.199	0.048		0.643	0.680
		(0.207)	(0.225)		(0.225)**	(0.246)**
R*Log income		-0.016	-0.004		-0.054	-0.058
		(0.018)	(0.019)		(0.019)**	(0.020)**
Reform of partner			0.268			0.077
			(0.249)			(0.256)
R^P*Log income			-0.021			-0.005
			(0.021)			(0.021)
Observations	29544	29544	29544	28934	28934	28934
R-squared	0.55	0.55	0.58	0.56	0.56	0.59

Notes: Log income for child and partner is a measure of permanent income; the average of log income in 1987, 1990, 1993 and 1996.
Robust standard errors in parentheses are clustered on municipality (panel A) and family (panel B).
+ significant at 10%; * significant at 5%; ** significant at 1%
R denotes reform status, R^P denotes reform status of the spouse.
The number of observations is different from the family income sample due to missing observations in the education variables.
Estimates in panel A include cohort effects and municipality effects, estimates in panel B include cohort effects and family fixed effects.
Estimates in columns 3 and 6 also include controls for municipality and cohort of the partner.

Table 4

Intergenerational income elasticities
Differences-in-differences 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
Independent variable					
			All		
Parents' log family income	0.169	0.167	0.120	0.120	0.123
	(0.004)**	(0.005)**	(0.004)**	(0.005)**	(0.005)**
Reform	0.002	-0.040		0.146	-0.027
	(0.004)	(0.076)		(0.075)+	(0.077)
Reform*Family income		0.003		-0.012	0.002
		(0.006)		(0.006)*	(0.006)
Reform of partner			0.006		0.426
			(0.004)		(0.068)**
Reform of partner*Family income					-0.035
					(0.006)**
Observations	120911	120911	120911	120911	120911
R-squared	0.29	0.29	0.31	0.30	0.32
			Women		
Parents' log family income	0.131	0.133	0.140	0.145	0.135
	(0.004)**	(0.007)**	(0.006)**	(0.007)**	(0.007)**
Reform	-0.001	0.053		0.114	-0.015
	(0.006)	(0.107)		(0.119)	(0.118)
Reform*Family income		-0.004		-0.009	0.001
		(0.009)		(0.010)	(0.010)
Reform of partner			0.010		0.221
			(0.006)		(0.111)*
Reform of partner*Family income					-0.018
					(0.009)+
Observations	61501	61501	61501	61501	61501
R-squared	0.07	0.07	0.08	0.07	0.10
			Men		
Parents' log family income	0.207	0.200	0.099	0.096	0.098
	(0.006)**	(0.007)**	(0.004)**	(0.006)**	(0.007)**
Reform	0.003	-0.154		0.174	0.076
	(0.006)	(0.097)		(0.124)	(0.094)
Reform*Family income		0.013		-0.015	-0.007
		(0.008)		(0.010)	(0.008)
Reform of partner			0.001		0.283
			(0.006)		(0.097)**
Reform of partner*Family income					-0.023
					(0.008)**
Observations	59410	59410	59410	59410	59410
R-squared	0.10	0.10	0.08	0.07	0.10

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 cohort effects, municipality effects and controls for birth year of father (for column 3 cohort and municipality effects for partner). Column 5 includes birth year and municipality effects for both spouses.

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
Independent variable					
			All		
Reform	0.006	0.470		0.312	0.160
	(0.005)	(0.107)**		(0.112)**	(0.117)
Reform*Family income		-0.038		-0.026	-0.014
		(0.009)**		(0.009)**	(0.010)
Reform of partner			0.013		0.361
			(0.006)*		(0.103)**
Reform of partner*Family income					-0.029
					(0.009)**
Observations	101460	101460	101460	101460	101460
R-squared	0.66	0.66	0.65	0.64	0.65
			Women		
Reform	0.004	0.445		0.387	0.313
	(0.010)	(0.198)*		(0.208)+	(0.223)
Reform*Family income		-0.037		-0.031	-0.025
		(0.016)*		(0.017)+	(0.019)
Reform of partner			0.023		0.012
			(0.012)+		(0.215)
Reform of partner*Family income					0.001
					(0.018)
Observations	29544	29544	29544	29544	29544
R-squared	0.58	0.58	0.58	0.55	0.58
			Men		
Reform	0.005	0.466		0.264	0.140
	(0.009)	(0.193)*		(0.204)	(0.216)
Reform*Family income		-0.038		-0.023	-0.013
		(0.016)*		(0.017)	(0.018)
Reform of partner			0.019		0.326
			(0.011)+		(0.197)+
Reform of partner*Family income					-0.025
					(0.016)
Observations	28934	28934	28934	28934	28934
R-squared	0.63	0.63	0.59	0.55	0.59

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 cohort effects, family fixed effects and controls for birth year of father. Columns 3 and 5 include also birth year and municipality effects for the partner.

Table 6

Intergenerational transmission of education
Differences-in-differences estimates

Dependent variable	(1) Child's years of schooling	(2) Child's years of schooling	(3) Partner's years of schooling	(4) Partner's years of schooling	(5) Partner's years of schooling
Independent variable					
			All		
Parents' family education	0.431	0.499	0.337	0.375	0.387
	(0.005)**	(0.005)**	(0.005)**	(0.006)**	(0.007)**
Reform	0.234	1.270		0.633	0.197
	(0.029)**	(0.060)**		(0.074)**	(0.076)**
Reform*Family education		-0.123		-0.073	-0.022
		(0.006)**		(0.009)**	(0.009)*
Reform of partner			0.231		1.161
			(0.027)**		(0.056)**
Reform of partner*Family edu					-0.111
					(0.006)**
Observations	167211	167211	167211	167211	167211
R-squared	0.17	0.17	0.15	0.12	0.16
			Women		
Parents' family education	0.398	0.453	0.365	0.429	0.405
	(0.005)**	(0.006)**	(0.007)**	(0.008)**	(0.009)**
Reform	0.197	1.027		0.779	0.264
	(0.030)**	(0.073)**		(0.099)**	(0.101)**
Reform*Family education		-0.099		-0.087	-0.026
		(0.008)**		(0.012)**	(0.012)*
Reform of partner			0.275		1.231
			(0.049)**		(0.086)**
Reform of partner*Family edu					-0.114
					(0.009)**
Observations	84816	84816	84816	84816	84816
R-squared	0.16	0.16	0.18	0.13	0.19
			Men		
Parents' family education	0.463	0.540	0.304	0.322	0.342
	(0.007)**	(0.007)**	(0.004)**	(0.007)**	(0.008)**
Reform	0.269	1.481		0.509	0.282
	(0.041)**	(0.094)**		(0.087)**	(0.096)**
Reform*Family education		-0.144		-0.062	-0.036
		(0.009)**		(0.009)**	(0.011)**
Reform of partner			0.214		0.775
			(0.027)**		(0.082)**
Reform of partner*Family edu					-0.068
					(0.009)**
Observations	82395	82395	82395	82395	82395
R-squared	0.20	0.20	0.12	0.12	0.14

Notes: Standard errors in parentheses are clustered on municipality. + significant at 10%; * significant at 5%; **

significant at 1%. All estimates include cohort effects, municipality effects and controls for birth year of father (for column 3 cohort and municipality effects for partner). Column 5 includes birth year and municipality effects for both spouses.

Table 7

Intergenerational transmission of education
Sibling difference estimates

Dependent variable	(1) Child's years of schooling	(2) Child's years of schooling	(3) Partner's years of schooling	(4) Partner's years of schooling	(5) Partner's years of schooling
Independent variable					
			All		
Reform	0.271	1.472		0.831	0.404
	(0.023)**	(0.076)**		(0.089)**	(0.091)**
Reform*Family education		-0.147		-0.099	-0.050
		(0.009)**		(0.010)**	(0.011)**
Reform of partner			0.251		1.352
			(0.029)**		(0.081)**
Reform of partner*Family edu					-0.134
					(0.009)**
Observations	139858	139858	139858	139858	139858
R-squared	0.68	0.68	0.61	0.59	0.61
			Women		
Reform	0.219	1.270		1.221	0.848
	(0.041)**	(0.131)**		(0.181)**	(0.189)**
Reform*Family education		-0.130		-0.141	-0.097
		(0.015)**		(0.021)**	(0.022)**
Reform of partner			0.353		1.173
			(0.061)**		(0.177)**
Reform of partner*Family edu					-0.100
					(0.020)**
Observations	41005	41005	41005	41005	41005
R-squared	0.71	0.71	0.66	0.62	0.66
			Men		
Reform	0.340	1.622		0.329	0.108
	(0.046)**	(0.147)**		(0.147)*	(0.156)
Reform*Family education		-0.157		-0.050	-0.027
		(0.017)**		(0.017)**	(0.018)
Reform of partner			0.203		0.870
			(0.049)**		(0.145)**
Reform of partner*Family edu					-0.081
					(0.017)**
Observations	39917	39917	39917	39917	39917
R-squared	0.71	0.71	0.63	0.61	0.63

Notes:Standard errors in parentheses are clustered on family. + significant at 10%; * significant at 5%; ** significant at 1%. All estimates include cohort effects, family fixed effects and controls for birth year of father. Columns 3 and 5 include also birth year and municipality effects for the partner.

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 1945 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-1945 cohorts is the following: I assign reform status based on home municipality in the 1960 or 1965 censuses, and based on year of birth.[22] Observing the pre-1945 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-1945 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 born until 1949. For later cohorts I use educational statistics on municipality level, describing

[22] 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.

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 1945 cohort, and the remaining parts of the municipalities are coded according to a uniform implementation year.

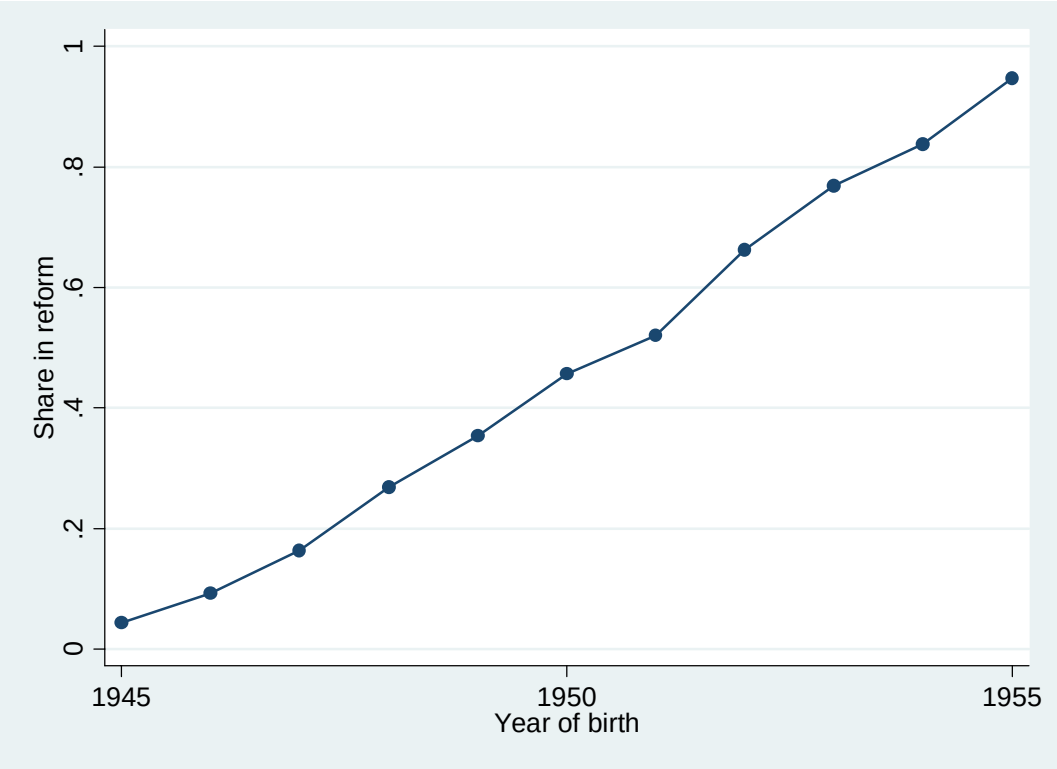
In order to assess the reliability of the coding, I match the reform coding to the IS-data (individual statistics) used in the Meghir and Palme (2005) study of the Swedish educational reform. The IS-data contain information on reform participation for cohorts born in 1948 and 1953; the reform is assigned 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 15 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 family income 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)
A. The income sample		
Child's log income	11.84	11.85
	(0.48)	(0.49)
Parents' log family income	12.12	
	(0.49)	
Log partner's income	11.84	11.84
	(0.50)	(0.50)
Reform	0.53	0.46
	(0.50)	(0.50)
Reform of partner	0.49	0.44
	(0.50)	(0.50)
Child's year of birth	1950.68	1949.96
	(3.05)	(3.17)
Father's year of birth	1922.18	1918.06
	(4.67)	(7.55)
Partner's year of birth	1950.36	1949.65
	(4.91)	(5.03)
Woman	0.51	0.51
	(0.50)	(0.50)
N	120911	180396
B. The education sample		
Child's years of schooling	11.53	11.51
	(2.56)	(2.58)
Parents' family education	8.36	
	(2.01)	
Partner's years of schooling	11.42	11.40
	(2.62)	(2.63)
Reform	0.47	0.46
	(0.50)	(0.50)
Reform of partner	0.45	0.44
	(0.50)	(0.50)
Child's year of birth	1950.12	1950.0
	(3.14)	(5.02)
Father's year of birth	1918.93	1918.06
	(6.84)	(7.55)
Partner's year of birth	1949.83	1949.66
	(5.00)	(5.02)
Woman	0.51	0.51
	(0.50)	(0.50)
N	167211	179807

Notes: All incomes are expressed in 1990 prices.

Table B2

Intergenerational income elasticities
Differences-in-differences estimates

Dependent variable	(1) Child's log family income	(2) Child's log family income	(3) Child's log family income	(4) Child's log family income
Independent variable				
		All		
Parents' log family income	0.161	0.165	0.162	0.157
	(0.003)**	(0.003)**	(0.005)**	(0.006)**
Reform	0.002		0.037	-0.007
	(0.003)		(0.065)	(0.066)
Reform*Family income			-0.003	0.001
			(0.005)	(0.006)
Reform of partner		0.003		0.089
		(0.004)		(0.055)
Reform of partner*Family income				-0.007
				(0.005)
Observations	122468	122468	122468	122468
R-squared	0.10	0.11	0.10	0.12
		Women		
Parents' log family income	0.152	0.149	0.157	0.148
	(0.005)**	(0.005)**	(0.006)**	(0.006)**
Reform	0.004		0.119	0.008
	(0.005)		(0.095)	(0.096)
Reform*Family income			-0.009	-0.000
			(0.008)	(0.008)
Reform of partner		0.003		0.207
		(0.005)		(0.096)*
Reform of partner*Family income				-0.017
				(0.008)*
Observations	62496	62496	62496	62496
R-squared	0.09	0.10	0.09	0.13
		Men		
Parents' log family income	0.171	0.180	0.168	0.170
	(0.004)**	(0.004)**	(0.006)**	(0.005)**
Reform	-0.001		-0.059	-0.151
	(0.005)		(0.089)	(0.075)*
Reform*Family income			0.005	0.012
			(0.007)	(0.006)*
Reform of partner		0.002		0.248
		(0.005)		(0.077)**
Reform of partner*Family income				-0.020
				(0.006)**
Observations	59972	59972	59972	59972
R-squared	0.13	0.13	0.13	0.16

Notes: Child's log family income is the average of log of the sum of both spouses' 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 cohort effects, municipality effects and controls for birth year of father (for columns 2 and 4 cohort and municipality effects for partner).

Table B3

Intergenerational income elasticities
Sibling differences

Dependent variable	(1) Child's log family income	(2) Child's log family income	(3) Child's log family income	(4) Child's log family income
Independent variable				
		All		
Reform	0.004		0.360	0.400
	(0.004)		(0.086)**	(0.090)**
Reform*Family income			-0.029	-0.033
			(0.007)**	(0.008)**
Reform of partner				-0.161
		0.006		(0.078)*
Reform of partner*Family income		(0.005)		0.014
				(0.006)*
Observations	103756	103756	103756	103756
R-squared	0.57	0.59	0.57	0.59
		Women		
Reform	0.012		0.496	0.374
	(0.008)		(0.159)**	(0.168)*
Reform*Family income			-0.040	-0.030
			(0.013)**	(0.014)*
Reform of partner		0.008		0.060
		(0.009)		(0.161)
Reform of partner*Family income				-0.004
				(0.013)
Observations	30492	30492	30492	30492
R-squared	0.58	0.61	0.58	0.61
		Men		
Reform	0.001		0.269	0.270
	(0.008)		(0.154)+	(0.167)
Reform*Family income			-0.022	-0.022
			(0.013)+	(0.014)
Reform of partner		0.011		-0.030
		(0.008)		(0.150)
Reform of partner*Family income				0.004
				(0.012)
Observations	29502	29502	29502	29502
R-squared	0.63	0.65	0.63	0.66

Notes: Child's log family income is the average of log of the sum of both spouses' 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 cohort effects, family fixed effects and controls for birth year of father (for columns 2 and 4 cohort and municipality effects for partner).

	(1) Reform
Family income	0.002
	(0.002)
Birth year of father:	
1916	0.002
	(0.005)
1917	0.002
	(0.005)
1918	0.001
	(0.005)
1919	0.001
	(0.005)
1920	0.001
	(0.004)
1921	0.004
	(0.004)
1922	-0.004
	(0.004)
1923	0.002
	(0.005)
1924	0.004
	(0.005)
1925	-0.002
	(0.005)
1926	-0.005
	(0.005)
1927	0.002
	(0.005)
1928	-0.003
	(0.005)
1929	0.001
	(0.006)
1930	0.010
	(0.006)
1931	0.003
	(0.007)
1932	0.002
	(0.007)
1933	-0.007
	(0.009)
1934	-0.015
	(0.012)
1935	0.001
	(0.014)
1936	-0.011
	(0.022)
1937	-0.012
	(0.027)
1938	0.009
	(0.088)
Constant	0.024
	(0.035)
Observations	120911
R-squared	0.68

Notes: Robust standard errors (in parentheses) are clustered on municipality. * significant at 5%; ** significant at 1%
The regression also includes cohort effects for the 1945-1955 cohorts and municipality effects.