Interpreting Trends in Intergenerational Mobility

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Abstract

We show that events in *previous* generations can explain contemporaneous shifts in intergenerational mobility. We first study the dynamic response of income mobility to structural changes in a model of intergenerational transmission. Mobility today depends on past policies and institutions, such that major reforms may generate long-lasting mobility trends over multiple generations. These trends are often non-monotonic, as mobility tends to be highest when a structural change occurs. Times of change are thus times of high mobility, while declining mobility today may reflect past gains rather than a recent deterioration of "equality of opportunity". We then exploit data over three generations and a compulsory school reform in Sweden to test the dynamic implications of our model. The reform had a large, long-lasting, and non-monotonic effect: it reduced the transmission of disparities in income and education from parents to their offspring in the directly affected generation, but *increased* intergenerational persistence in the next.

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Introduction

The evolution of inequality in economic status over time is a fundamental topic in the social sciences and in public debate. Two central dimensions of interest are the extent of cross-sectional inequality between individuals and its persistence across generations, as status differences are transmitted from parents to their children. Both have important implications for individual welfare and the functioning of political and economic systems.[1]

A significant rise in cross-sectional income inequality from the late 1970s in the US, UK and (more recently) other OECD countries is well documented, but much less is known about trends in *intergenerational mobility*.[2] Yet, we do know that income mobility differs substantially across countries, and the observation that those differences appear negatively correlated with cross-sectional inequality has received much attention.[3] A central theme in the recent literature is thus if income inequality has not only increased, but also become more persistent across generations. This question is debated particularly in countries that experienced rising cross-sectional inequality, such as the US, where commentators argue that low mobility threatens social cohesion and the notion of "American exceptionalism".[4]

But how should evidence on declining mobility be *interpreted* – does it reflect a diminished effectiveness of current policies and institutions in the promotion of "equal opportunities"? In this paper we show theoretically and empirically that mobility trends may instead be caused by events in a more distant past, as structural changes affect mobility over *multiple* generations. We argue that such dynamic responses are of particular importance in the study of intergenerational persistence, since even a single transmission step – one generation – corresponds to a very long time period. Institutional reforms or other systemic changes generate therefore long-lasting mobility trends.

The interpretation of such trends necessitates a dynamic perspective, but existing theoretical work focuses instead on the relationship between causal mechanisms and the implied long-run or *steady-state* level of intergenerational mobility. We thus contribute to the literature by examining the dynamic implications of a simultaneous equations model of inter-

[1]Intergenerational mobility is for example seen to contribute to the stability of liberal democracies, by legitimating income and status inequalities and by reducing the potential for class-based collective action (see Erikson and Goldthorpe, 1992).

[2]Autor and Katz (1999) discuss trends in wage inequality across countries. Atkinson, Piketty, and Saez (2011) find a substantial rise in top income shares in the US and various other countries.

[3]A large empirical literature (see Solon, 1999, and Black and Devereux, 2011) seeks to quantify how intergenerational mobility differs across countries, groups and time. Björklund and Jäntti (2009), Blanden (2011), and Corak (2013) present evidence on the correlation between cross-sectional inequality and mobility.

[4]Exemplary articles are *"Ever Higher Society, Ever Harder to Ascend"* in *The Economist* (Dec. 2004), *"Moving Up: Challenges to the American Dream"* in the *Wall Street Journal* (May 2005), *"The Mobility Myth"* in *The New Republic* (Feb. 2012), or the recent *"Great Divide"* series on nytimes.com. The political importance of the topic is exemplified by a speech of Alan Krueger, Chairman of the Council of Economic Advisers, who warned that intergenerational mobility should be expected to decline further as of the recent rise in income inequality in the US (speech at the Center for American Progress, January 12th, 2012).

generational transmission. We deviate from previous work also by assuming that income depends on human capital through a vector of distinct productive characteristics instead of a single factor. This choice is in accordance with the growing evidence on the importance of distinct, including noncognitive types of skills (e.g., Heckman et al., 2006). We find that such multiplicity also matters in the intergenerational context.

Using our model we first show that the level of intergenerational mobility depends not only on contemporaneous transmission mechanisms, but also on the distribution of income and skills in the parent generation – and thus on past mechanisms. This result leads to a number of implications. First, changes in policies and institutions can generate long-lasting mobility trends. Conversely, changes in mobility today might not be explained by recent structural changes, but by major events in the more distant past. Second, differences in mobility across countries, or across groups within countries, might reflect not only the consequences of current but also of past policies, institutions and conditions.

A fairly general class of changes in transmission mechanisms cause *non-monotonic transitions* between steady states. We show that changes in the relative returns to different types of human capital or endowments generate transitional mobility, as some families gain while others lose. Technological, institutional or other structural change may thus increase mobility initially, followed by a decreasing trend that lasts over multiple generations. We conclude that times of change tend to be times of high mobility, while mobility is likely to decrease when the economic environment stabilizes. A shift towards a more meritocratic society has similar consequences. A rise in the importance of own skill relative to parental status is to the advantage of talented offspring from poor families, providing opportunities that were not yet available to their parents. Intergenerational mobility is thus particularly high in the first affected generation, but is bound to decline in subsequent generations. Even structural changes that are clearly mobility-enhancing in the long-run can therefore cause negative trends over some generations.

Declining mobility today may then not signal that current policies and institutions promote equality of opportunity less effectively, but might instead be a repercussion of major *improvements* in the past. These results are important for policy evaluation and for the interpretation of mobility trends. Observed mobility shifts are commonly related to contemporaneous changes in policy or institutions, which may result in misleading conclusions about determinants of the former and long-run consequences of the latter.

A dynamic view of intergenerational transmission does not only reveal such pitfalls, it may also aid our understanding of causal mechanisms (as different structural shocks have different dynamic implications) and of mobility differences across countries and time that have been documented by the empirical literature. Our main objective is to illustrate the general relationship between causal mechanisms and mobility trends, but we comment briefly also on various practical implications that seem particularly relevant for the recent literature.

We test the dynamic implications of our model empirically in the second part of our paper, examining a school reform in Sweden that raised the compulsory schooling level for cohorts born from the early 1940s. We exploit the reform's gradual implementation over municipalities and the availability of registry data covering three generations to identify its causal effects on both educational and income mobility.

We first confirm that the reform increased intergenerational mobility in those cohorts that were directly subjected to it, reducing the degree to which differences in income or education were transmitted from parents to their offspring. This first-generation effect was particularly strong at the onset of the reform, reducing persistence in education by up to one fourth in the earliest affected cohorts. We then show that the same school reform generated mobility trends in the next generation, increasing the intergenerational elasticity of income and educational coefficient in cohorts born from the mid-1960s. We demonstrate that this second-generation effect is likely to persist up to very recent offspring cohorts, until all of their parents have been subject to the new school reform. The observed non-monotonic response is consistent with the prediction from our theoretical model.

Finally, the empirical application leads to another conceptual insight. While rapid structural changes may initially have a sudden impact on mobility, their effect on mobility trends in subsequent generations will be more gradual due to the variation of parental age at birth of their child. We introduce a cohort dimension into our theoretical model to capture such implications and to provide a closer link between the existing empirical (trends over *cohorts*) and theoretical literature (transmission over *generations*). This extended model highlights that variation in intergenerational persistence by parental age and time can be informative about dynamic effects of past events. We illustrate that a simple estimation of intergenerational persistence conditional on parental age does indeed suffice to identify the onset of the second-generation effect in our empirical application.

The paper proceeds as follows. We next discuss the related literature. In Section 2 we present our model of intergenerational transmission, derive current and steady-state mobility levels in terms of its structural parameters, and analyze the dynamic content of the model. In Sections 2.2 and 2.3 we study three theoretical examples to illustrate our main theoretical findings. Section 3.1 presents our empirical application, which then motivates the introduction of a cohort dimension into our model in Section 4. Section 5 concludes.

1 The Literature

Many studies examine the theoretical relationship between causal transmission mechanisms and the implied long-run or *steady-state* level of intergenerational mobility, but there exists little work on transition paths between those steady states. In the standard simultaneous equations approach as developed by Conlisk (e.g., Conlisk, 1974a) only Atkinson and Jenkins (1984) focus on systems that are not in steady state.[5] While they show that failure of the steady-state assumption impedes identification of invariable parameters of the structural model, we instead consider how changes in structural parameters affect mobility in subsequent generations. Solon (2004) notes that the interpretation of mobility trends would benefit from a theoretical perspective, and examines how structural changes (such as in the return to human capital and the progressivity of public investment) affect mobility in the first affected generation. Davies et al. (2005) compare mobility and cross-sectional inequality under private and public education in a model of human capital accumulation. They note that the observation of mobility trends may help to distinguish between alternative causes of rising cross-sectional inequality.

While theoretical work is sparse, it exists much empirical work on mobility trends in the US and other countries. A long-standing and mostly sociological literature is concerned with occupational and class mobility (see Breen, 2004, Hauser, 2010, and Long and Ferrie, 2013), examining both absolute (subject to changes in the occupational structure at the aggregate level) and relative mobility rates across countries and time. A more recent but fast-growing economic literature examines mobility trends in *income* or *educational attainment,* which are important indicators and potentially key mechanisms for the reproduction of economic advantage (see Black and Devereux, 2011). Most economic studies assess how strongly absolute or relative *differences* among parents are transmitted to their offspring, abstracting from mean changes over generations.

Some of the emerging evidence on income mobility appears conflicting, perhaps as a result of the substantial data requirements that such studies face. Measurement ideally requires income data that span over two generations, but often only sparse data are available or exploited.[6] Hertz (2007) and Lee and Solon (2009) find no evidence of a major trend across cohorts of sons born 1952-1975 in the US, but cannot reject more gradual changes over time. Levine and Mazumder (2007) as well as Aaronson and Mazumder (2008) argue that mobility has fallen in recent decades – the latter based on intergenerational estimates from synthetic families (constructed from census data), the former based on estimates of sibling correlations in various economic outcomes. Such decline has also been found for the UK, in Blanden et al. (2004) and Nicoletti and Ermisch (2007).[7]Other studies examine how educational mobility differs between groups, how it is affected by institutional aspects, or how it changes over time. Hertz et al. (2008) present trends in educational mobility over 50 years for 42 countries,

[5]Moreover, Jenkins (1982) discusses stability conditions for systems of stochastic linear difference equations with constant coefficients, Conlisk (1974b) derives stability conditions for systems with random coefficients.

[6]Nybom and Stuhler (2011) summarize methodological advances in the recent literature, and argue that these can still not fully eliminate *life-cycle bias* in mobility estimates based on incomplete income data. This bias can differ by cohort and may mask gradual changes of mobility, or generate a false impression of such trends.

[7]See Erikson and Goldthorpe (2010) and Blanden et al. (2012) for a debate of divergent findings in measures of income and occupational mobility.

noting that Nordic countries display comparatively high intergenerational mobility.

A central concern in many of these papers, policy-related outlets, and the public press is that mobility may have declined in conjunction with the recent rise in income inequality.[8] Various potential causal factors for observed trends – such as educational expansion, rising returns to education, or changes in welfare policies – are considered in the literature (e.g., Levine and Mazumder, 2007, and further articles in the same issue). Common to all explanations is that they relate trends to recent events that may have directly affected the respective cohorts. We argue that this is only one potential interpretation, and that the key to an understanding of current mobility levels and trends might lie in the more distant past.

2 A Model of Intergenerational Transmission

Measuring intergenerational mobility. In our theoretical analysis we consider the intergenerational elasticity of income, which is a popular descriptive measure of persistence in relative economic status. Our main arguments extend to mobility in other outcomes, such as educational attainment, which we will consider in our empirical analysis. Consider a simplified one-parent one-offspring family structure, with $y_{i,t}$ as log lifetime income of the offspring in generation t of family i and $y_{i,t-1}$ as log lifetime income of the parent. The intergenerational elasticity is given by the slope coefficient in the linear regression

$$y_{i,t} = \alpha_t + \beta_t y_{i,t-1} + \epsilon_{i,t}. \tag{1}$$

The elasticity β_t captures a *statistical* relationship and the error $\epsilon_{i,t}$ is uncorrelated with the regressor by construction. Under stationarity in the variance of $y_{i,t}$ it equals the intergenerational correlation, which adjusts the elasticity for changes in cross-sectional inequality. The intergenerational income elasticity is the most commonly estimated parameter in the empirical literature and captures to what degree percentage differences in parents' incomes tend to be transmitted to the next generation. A low elasticity or correlation indicates high mobility.

A model of intergenerational transmission. We model intergenerational transmission as a system of stochastic linear difference equations, in the tradition of the simultaneous equation approach developed and elaborated by Conlisk (1969, 1974a) and Atkinson and Jenkins (1984). We show in Appendix A.1 that the "mechanical" pathways represented by these equations can be derived from the optimizing behavior of parents in an underlying utility-maximization framework (see Becker and Tomes, 1979, Goldberger, 1989, and Solon, 2004).

[8] See references in footnote 4 for the US, or Blanden (2009) for the UK.

The equations of our baseline model are

$$y_{it} = \gamma_{y,t}\, y_{it-1} + \boldsymbol{\delta}_t'\, \boldsymbol{h}_{it} + u_{y,it} \tag{2}$$

$$\boldsymbol{h}_{it} = \boldsymbol{\gamma}_{h,t}\, y_{it-1} + \boldsymbol{\Theta}_t\, \boldsymbol{e}_{it} + \boldsymbol{u}_{h,it} \tag{3}$$

$$\boldsymbol{e}_{it} = \boldsymbol{\Lambda}_t\, \boldsymbol{e}_{it-1} + \boldsymbol{v}_{it}. \tag{4}$$

From equation (2), income y_{it} in generation t of family i is determined by *parental income* y_{it-1}, own *human capital* $\boldsymbol{h}_{it}$, and *chance* $u_{y,it}$. The parameter $\gamma_{y,t}$ captures a direct effect of parental income that is independent from offspring productivity, which may arise as of nepotism, statistical discrimination under imperfect information on individual productivity, or other reasons.[9] Human capital consists of a $Jx1$ vector $\boldsymbol{h}_{it}$ with elements $h_{1,it}, ..., h_{J,it}$, reflecting distinct characteristics such as formal schooling, health, and cognitive and non-cognitive skills. These characteristics are valued on the labor market according to a $Jx1$ price vector $\boldsymbol{\delta}_t$ with elements $\delta_{1,t}, ..., \delta_{J,t}$. The random shock term $u_{y,it}$ captures factors that do not relate to parental background. For our analysis it makes no difference if these are interpreted as (labor market) luck or as the impact of other characteristics that are not transmitted within families.

From equation (3), human capital $\boldsymbol{h}_{it}$ is affected by parental income y_{it-1}, own *endowments* $\boldsymbol{e}_{it}$, and chance $\boldsymbol{u}_{h,it}$. A role for parental income may for example stem from parental investment into offspring human capital. Elements in the $Jx1$ vector $\boldsymbol{\gamma}_{h,t}$ may differ if parental investments are more targeted or more effective on some types of human capital than others. Parental income may thus affect offspring income directly (through $\gamma_{y,t}$) or indirectly (through $\boldsymbol{\gamma}_{h,t}$).[10] The JxK matrix $\boldsymbol{\Theta}_t$ governs the role that endowments such as abilities or preferences play in the accumulation of different types of human capital. Those endowments, consisting of the $Kx1$ vector $\boldsymbol{e}_{it}$ with elements $e_{1,it}, ..., e_{K,it}$, are partly inherited from parental endowments $\boldsymbol{e}_{it-1}$ and partly due to chance $\boldsymbol{v}_{it}$. The elements of the KxK matrix $\boldsymbol{\Lambda}_t$ with elements $\lambda_{11,t}, ..., \lambda_{KK,t}$ govern the *heritability* of each endowment. We consider $\boldsymbol{\Lambda}_t$ to represent a broad concept of intergenerational transmission potentially working through both nature (e.g. genetic inheritance) and nurture (e.g. family environment). The random shock $u_{y,it}$ and elements of $\boldsymbol{u}_{h,it}$ and $\boldsymbol{v}_{it}$ are assumed to be uncorrelated with each other and past values of $\{y_{it}, \boldsymbol{h}_{it}, \boldsymbol{e}_{it}, u_{y,it}, \boldsymbol{u}_{h,it}, \boldsymbol{v}_{it}\}$.

For convenience we drop the individual subscript i and make a few simplifying assumptions. As we focus on relative mobility assume that all variables are measured as trendless

[9] For example as of credit constraints influencing choices on the labor market, parental information and networks, or (if total market income is considered) returns to bequests. The exact mechanism and the distinction between earnings and income are not central for our purposes.

[10] The distinction may not be sharp in practice; for example, parental credit constraints might affect educational attainment and human capital acquisition of offspring, but might also affect their career choices for a given level of human capital.

indices with constant mean zero (as in Conlisk, 1974a). To avoid case distinctions assume further that those indices measure positive characteristics with a non-negative effect on income (such that $\gamma_{y,t}$ and the elements of $\boldsymbol{\gamma}_{h,t}$ and $\boldsymbol{\delta}_t'\Theta_t$ are non-negative) and that parent and offspring endowments are not negatively correlated (such that elements of $\boldsymbol{\Lambda}_t$ are non-negative), for all t.

Using equation (3) to substitute out $\boldsymbol{h}_{i,t}$ we have

$$y_t = \gamma_t \, y_{t-1} + \boldsymbol{\rho}_t' \, \boldsymbol{e}_t + u_t \tag{5}$$

$$\boldsymbol{e}_t = \boldsymbol{\Lambda}_t \, \boldsymbol{e}_{t-1} + \boldsymbol{v}_t, \tag{6}$$

where the parameter $\gamma_t = \gamma_{y,t} + \boldsymbol{\delta}_t'\boldsymbol{\gamma}_{h,t}$ aggregates the direct and indirect effects of parental income, the $1xK$ vector $\boldsymbol{\rho}_t' = \boldsymbol{\delta}_t'\Theta_t$ captures the returns to inherited endowments and human capital (affected both by the importance of endowments in the accumulation of and the returns to human capital), and where $u_t = u_{y,t} + \boldsymbol{\delta}_t'\boldsymbol{u}_{h,t}$ aggregates the random shocks in income and human capital.

Our model has a similar structure as the model in Conlisk (1974a), but in contrast to the previous literature we assume that income depends on human capital through a vector of distinct productive characteristics. This generalization will prove to be central for some of our findings. Similarity to the existing literature in other dimensions is advantageous since it suggests that our findings do not arise due to non-standard assumptions. The second deviation from previous work is simply the addition of t subscripts to all parameters, reflecting our focus on the dynamic response to changes in the transmission framework. A parameter may change as of various underlying mechanisms. For example, an expansion of public childcare may affect the degree to which human capital is inherited across generations, or technological change may affect relative demand and thus returns to skills on the labor market. For simplicity we do not explicitly model any particular mechanism.

We will consider mobility trends following a single structural change in generation $t = T$, assuming that the moments of all variables were in steady-state equilibrium before the shock. For simplicity we assume that the process is infinite. This assumption (which imposes restrictions on the parameters of our model, see Appendix A.2) nor the existence of pre- and post-shock steady states are necessary for our arguments, but simplify the discussion and facilitate comparisons to the existing literature on steady-state mobility.

For convenience we normalize the variances of y_t and all elements of $\boldsymbol{h}_t$ and $\boldsymbol{e}_t$ in the initial steady state to one. The variances of $u_{y,t}$ and elements of $\boldsymbol{u}_{h,t}$ and $\boldsymbol{v}_t$ are then implicitly a function of the slope parameters of the model, and the requirement for those variances to be non-negative leads to additional constraints on the parameters. Cross-sectional inequality may change after a structural change occurs. However, we will frequently consider changes in the *relative* strength of different transmission mechanisms that do not affect the cross-sectional variances of income, human capital, and endowments. Abstracting from changes in those variances simplifies the discussion and helps to isolate other adjustment mechanisms that are of particular interest.

2.1 The Importance of Past Transmission Mechanisms

We express intergenerational mobility as a function of our model to illustrate some central implications. Consider a simplified example, assuming $\mathbf{\Lambda}_t$ to be diagonal and cross-sectional inequality to remain constant, $Var(y_t) = Var(e_{j,t}) = 1\,\forall j, t$. The intergenerational elasticity then coincides with the intergenerational correlation, and is derived by plugging equations (5) and (6) from our model into equation (1), such that

$$\beta_t = \frac{Cov(y_t, y_{t-1})}{Var(y_{t-1})} = \gamma_t + \boldsymbol{\rho}_t'\mathbf{\Lambda}_t Cov(\boldsymbol{e}_{t-1}, y_{t-1}). \tag{7}$$

Thus, β_t depends on current transmission mechanisms (parameters γ_t, $\boldsymbol{\rho}_t$ and $\mathbf{\Lambda}_t$) and on the cross-covariance between income and endowments in the parent generation. The intuition is simple. If income and other favorable endowments are concentrated in the *same* families then intergenerational mobility will be particularly low (the elasticity will be high). Expression (7) illustrates that two populations subject to the same transmission mechanisms (e.g., institutions and policies) today can still differ in their levels of intergenerational mobility, since current mobility depends also on the joint distribution of income and endowments in the parent generation.

The cross-covariance between income and endowments in the parent generation is in turn determined by past transmission mechanisms, and thus past values of $\{\gamma_t, \boldsymbol{\rho}_t, \mathbf{\Lambda}_t\}$. We can iterate equation (7) backwards to express β_t in terms of parameter values,

$$\begin{aligned} \beta_t &= \gamma_t + \boldsymbol{\rho}_t'\mathbf{\Lambda}_t \left(\mathbf{\Lambda}_{t-1} Cov(\boldsymbol{e}_{t-2}, y_{t-2})\gamma_{t-1} + \boldsymbol{\rho}_{t-1}\right) \\ &= \ldots \\ &= \gamma_t + \boldsymbol{\rho}_t'\mathbf{\Lambda}_t\boldsymbol{\rho}_{t-1} + \boldsymbol{\rho}_t'\mathbf{\Lambda}_t \left(\sum_{r=1}^{\infty}\left(\prod_{s=1}^{r}\gamma_{t-s}\mathbf{\Lambda}_{t-s}\right)\boldsymbol{\rho}_{t-r-1}\right), \end{aligned} \tag{8}$$

assuming that the process is infinite.[11] The level of intergenerational mobility today thus depends on current and past transmission mechanisms.[12] If no structural changes occur,

[11] For a finite process, β_t will depend on past parameter values and the initial condition $Cov(\boldsymbol{e}_0, y_0)$.

[12] If cross-sectional inequality varies over generations, or if $\mathbf{\Lambda}_t$ is not diagonal, the derivation of equation (8) would require backward iteration of the variance of y_t and the variance-covariance matrix of $\boldsymbol{e}_t$. Accordingly, β_t would also depend on the variances of u_t and $\boldsymbol{v}_t$ in past generations.

$\gamma_s = \gamma,\ \boldsymbol{\rho}_s = \boldsymbol{\rho},\ \boldsymbol{\Lambda}_s = \boldsymbol{\Lambda}\ \forall s \leq t$, then equation (9) simplifies to the *steady-state* elasticity

$$\beta \ = \ \gamma + \boldsymbol{\rho}'\boldsymbol{\Lambda}\sum_{s=0}^{\infty}\left(\gamma\boldsymbol{\Lambda}\right)^s\boldsymbol{\rho} \ = \ \gamma + \boldsymbol{\rho}'\boldsymbol{\Lambda}\left(\boldsymbol{I}_{KxK} - \gamma\boldsymbol{\Lambda}\right)^{-1}\boldsymbol{\rho}, \tag{9}$$

where the second step follows since the geometric series $\sum_{s=0}^{\infty}\left(\gamma\boldsymbol{\Lambda}\right)^s$ converges (the absolute value of each eigenvalue of $\gamma\boldsymbol{\Lambda}$ is below one). The literature has almost exclusively focused on how changes in structural parameters affect intergenerational mobility in steady state, as given by (9). We will instead analyze the *transition path* towards the new steady state as determined by equation (8).

Some properties can be readily generalized. The transition path of $Cov(\boldsymbol{e}_{t-1}, y_{t-1})$ is governed by the eigenvalues of the reduced-form coefficient matrix and is thus monotonic (see eq. 44 in Appendix A.2). But from (7) it follows that income mobility in the *first* generation subject to a structural change is directly affected by parameter changes, not indirectly by changes in the covariance between parental income and endowments. Trends in income mobility are thus not necessarily monotonic (even if cross-sectional inequality remains constant), as we will show in the next section. Other properties, such as the speed of convergence, depend on the parameterization of the model and can thus not be generalized.

2.2 From Simple Examples to Non-Monotonic Trends

We start with simplified versions of our baseline model and then move to more general models. For our first examples it is sufficient to consider a single endowment e_t and thus scalar versions of equations (5) and (6), such that

$$y_t = \gamma_t y_{t-1} + \rho_t e_t + u_t \tag{10}$$

$$e_t = \lambda_t e_{t-1} + v_t. \tag{11}$$

Our qualitative findings do not rely on specific parameter choices, but the quantitative implications of our examples will be more plausible if we choose values that are consistent with empirical evidence. The evidence in the literature, and our cross-validations within the model, suggest the following rough order of magnitudes for the US case:

$$0.45 \leq \beta \leq 0.55, \quad 0.15 \leq \gamma \leq 0.25, \quad 0.60 \leq \rho \leq 0.70, \quad 0.50 \leq \lambda \leq 0.65.$$

We discuss these choices in detail in Appendix A.3. It will be useful to first look at an even simpler case in which parental income has no causal effect.

Example 1: A simple meritocratic economy. Assume that the *heritability* of endowments (λ_t) or the *returns* to endowments and human capital (ρ_t) change in a simple meritocratic economy ($\gamma_t = 0\ \forall t$).

Assume first that cross-sectional inequality remains constant.[13] From equation (8), a change in the heritability of endowments in generation T from $\lambda_{t<T} = \lambda_1$ to $\lambda_{t\geq T} = \lambda_2$ shifts the intergenerational elasticity (or correlation) according to

$$\Delta\beta_T = \beta_T - \beta_{T-1} = \rho(\lambda_2 - \lambda_1)\rho. \tag{12}$$

Mobility remains constant afterwards. A change in returns from ρ_1 to ρ_2 in generation T instead shifts β_t over two generations. The first shift equals

$$\Delta\beta_T = \beta_T - \beta_{T-1} = (\rho_2 - \rho_1)\lambda Cov(e_{T-1}, y_{T-1}) = (\rho_2 - \rho_1)\lambda\rho_1, \tag{13}$$

and is induced by the change in returns for the *offspring* generation in T. The second shift,

$$\Delta\beta_{T+1} = \beta_{T+1} - \beta_T = \rho_2\lambda\left(Cov(e_T, y_T) - Cov(e_{T-1}, y_{T-1})\right) = \rho_2\lambda(\rho_2 - \rho_1), \tag{14}$$

is induced by the change in the correlation between income and endowments among the *parents* of the offspring generation $T+1$, in turn caused by changing returns to those endowments in generation T. The second shift is larger than the first if returns increase ($\rho_2 > \rho_1$). Figure 1 gives a numerical example.

Cross-sectional inequality. An additional source of dynamics stems from changes in cross-sectional inequality. Intuitively, if *individual* endowments and skills are linked over generations due to inheritance within families, then *cross-sectional inequality* will also be linked over generations; the variance of equation (11) can be iterated backwards such that

$$Var(e_t) = \lambda_{t-k}^{2k} Var(e_{t-k}) + \sum_{s=0}^{k-1} \lambda_{t-s}^{2s} Var(v_{t-s}) \qquad \forall k \geq 1. \tag{15}$$

Models of intergenerational transmission therefore imply that the impact of a structural change on cross-sectional inequality may propagate in subsequent generations, in turn affecting mobility measures over multiple generations.[14]

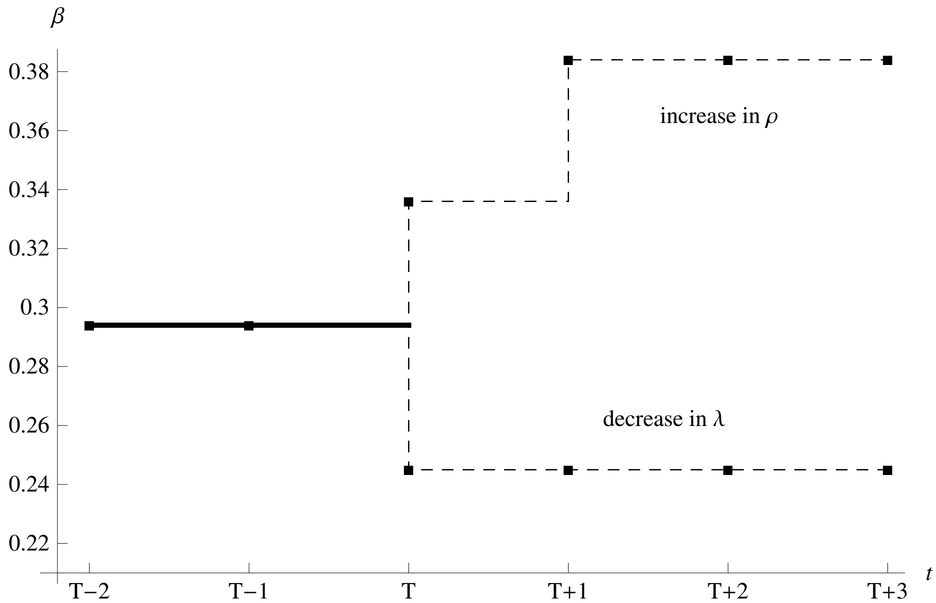
[13] Assume that the importance of parental background *relative* to unrelated factors changes, such that shifts in λ_t or ρ_t are offset by corresponding shifts in the variance of u_t or v_t.

[14] For example, if the changing heritability of endowments affects its cross-sectional variance (because the variance of v_t remains constant) then the elasticity shifts not only in the first but also subsequent generations, as

$$\Delta\beta_{T+1} = \rho\lambda_2\left(\frac{Var(e_T)}{Var(y_T)} - \frac{Var(e_{T-1})}{Var(y_{T-1})}\right) = \rho\lambda_2\left(\frac{1 + (\lambda_2^2 - \lambda_1^2)}{1 + \rho^2(\lambda_2^2 - \lambda_1^2)} - 1\right)$$

is non-zero for $\lambda_1 \neq \lambda_2$.

Figure 1: A change in the heritability of, or returns to, endowments



Note: Mobility trend over generations in two numerical examples. Example 1a: in generation T the heritability of endowments λ decreases from $\lambda_1 = 0.6$ to $\lambda_2 = 0.5$ (assuming $\rho = 0.7$ and $\gamma = 0$). Example 1b: the returns to endowments and human capital ρ increase from $\rho_1 = 0.7$ to $\rho_2 = 0.8$ (assuming $\lambda = 0.6$).

Implications. The example illustrates that the dynamic response of mobility measures can be informative on the type of structural shock that occurred. Changes in the heritability of endowments and skills have a more immediate effect than changes in the returns to those skills, as income mobility depends directly on returns in both parent and offspring generations. The effect of changing returns on steady state mobility levels may thus not become fully evident before both the parent and child generations have experienced the new price regime. We can relate this argument to the evidence on rising skill differentials in wages from the late 1970s in the US, UK, and (more recently) other OECD countries. The notion that widening wage differentials could decrease intergenerational mobility (e.g., Blanden et al., 2004, and Solon, 2004) contributes greatly to the current interest in mobility trends. But recent studies do not yet observe offspring cohorts whose parents have fully experienced the changing wage regime; its impact on mobility may thus become more evident in future empirical work.[15]

Not only will the dynamic response of mobility depend on the type of structural change that occurred; different measures of the importance of family background may also show different dynamic responses. Sibling correlations, which capture influences on economic outcomes that are shared by siblings, depend less directly on conditions in the parent generation and thus respond more immediately to rising returns than intergenerational measures

[15]For example, the last offspring cohort observed in Lee and Solon (2009) were born in 1975. Their parents were not subject to the widening skill differential in their early careers.

of persistence.[16] This argument may explain why US studies find a sharp increase in *sibling* correlations since 1980 (Levine and Mazumder, 2007), while there seems to be less evidence for such shift in *intergenerational* measures of persistence. The former are directly affected by changing wage differentials, but the latter also depend on conditions in the parent generation. Sibling correlations may then be a preferred measure in the analysis of mobility trends over time, as they tend to react more immediately to structural changes.[17]

These results have general implications for the interpretation of mobility trends: shifts in mobility may not reflect a changing effectiveness of current policies and institutions in the promotion of equality of opportunity, but a lagged effect of major changes in the more distant past. The next example illustrates that such repercussions can be both sizable and non-monotonic. We move to a more general model that allows for parental income to have causal effects ($\gamma \neq 0$). Consider first an example of "equalizing opportunities", in which offspring outcomes become less dependent upon parental income.[18]

EXAMPLE 2: EQUALIZING OPPORTUNITIES. Assume that the importance of parental status diminishes ($\gamma_1 > \gamma_2$) while skills that are partially inherited are instead more strongly rewarded ($\rho_1 < \rho_2$).

In other words, assume that in generation T the economy becomes less *plutocratic* and more *meritocratic*. For example, parental status may become less and own merits more important for appointment into jobs and occupations. Mobility then shifts in the first affected generation according to

$$\Delta\beta_T \quad = \quad (\gamma_2 - \gamma_1) + (\rho_2 - \rho_1)\lambda Cov(e_{T-1}, y_{T-1}), \tag{16}$$

affected both by the declining importance of parental income and the increasing returns to endowments or skills. However, the latter effect is attenuated, for two reasons. First, endowments are only imperfectly correlated within families, such that $\lambda < 1$. Second, parental endowments e_{T-1} explain only a fraction of the variation of incomes in the parent generation, such that $Cov(e_{T-1}, y_{T-1}) < 1$. Income mobility thus tends to increase if a generation is subject to a more meritocratic setting than their parents, as might be expected.

However, income mobility will also shift in the second generation, according to

$$\Delta\beta_{T+1} \quad = \quad \rho_2\lambda \left[\frac{Cov(e_T, y_T)}{Var(y_T)} - \frac{Cov(e_{T-1}, y_{T-1})}{Var(y_{T-1})} \right]. \tag{17}$$

Apart from changes in the variance of income, the elasticity may also shift because of changes

[16] The sibling correlation equals $\rho_1^2\lambda^2$ before and $\rho_2^2\lambda^2$ in generations after returns change in the example.

[17] Analysis of trends in sibling correlations, with its weaker data requirements, may also often be more feasible (see Björklund et al., 2009).

[18] As noted by Conlisk (1974a), "opportunity equalization" is an ambiguous term that may relate to different types of structural changes in models of intergenerational transmission.

in the correlation between income and endowments in the parent generation. The relative importance of parameter changes on the latter is now reversed, since

$$\frac{\partial Cov(e_T, y_T)}{\partial \gamma_2} = \lambda Cov(e_{T-1}, y_{T-1}) \quad \text{and} \quad \frac{\partial Cov(e_T, y_T)}{\partial \rho_2} = 1.$$

Changing returns have a strong effect on the correlation between own endowments and incomes. A change towards a more meritocratic society tends to increase the correlation between endowments and income, thereby *decreasing* income mobility from the second affected generation onwards.

The dynamic response of the intergenerational elasticity thus tends to be *non-monotonic*, with an initial rise in mobility and a subsequent decline. Intuitively, a rise in the importance of own skill relative to parental status will be detrimental for offspring with high-income, low-skill parents. In contrast, the shift will benefit talented offspring from poor families, providing opportunities for upward mobility that were not yet available to their parents. Mobility is thus highest when these relative gains and losses occur, when a generation faces new institutions, policies and opportunities that differ markedly from those in their parents' generation. But the offspring of those who thrived under the meritocratic setting will also do relatively well, due to the inheritance of talent; mobility hence decreases subsequently.[19]

Exact conditions for such non-monotonic adjustment can be given if the shifting importance of parental background and own characteristics does not affect cross-sectional inequality, such that $Var(y_t) = 1 \, \forall t$.[20] Figure 2 plots a numerical example, illustrating that the response in mobility trends can be long-lasting; it becomes insignificant only in the third generation, or more than half a century after the structural change.[21]

Implications. The example illustrates that we need to be careful when interpreting mobility trends. Not only may those trends be a response to events that occurred in past generations, this response may also be non-monotonic. Changes that are mobility-enhancing in the long run may nevertheless cause a *decreasing* trend in mobility measures that lasts over several generations. Declining mobility today may then not necessarily reflect a recent deterioration of equality of opportunity, but rather major gains made in the past.

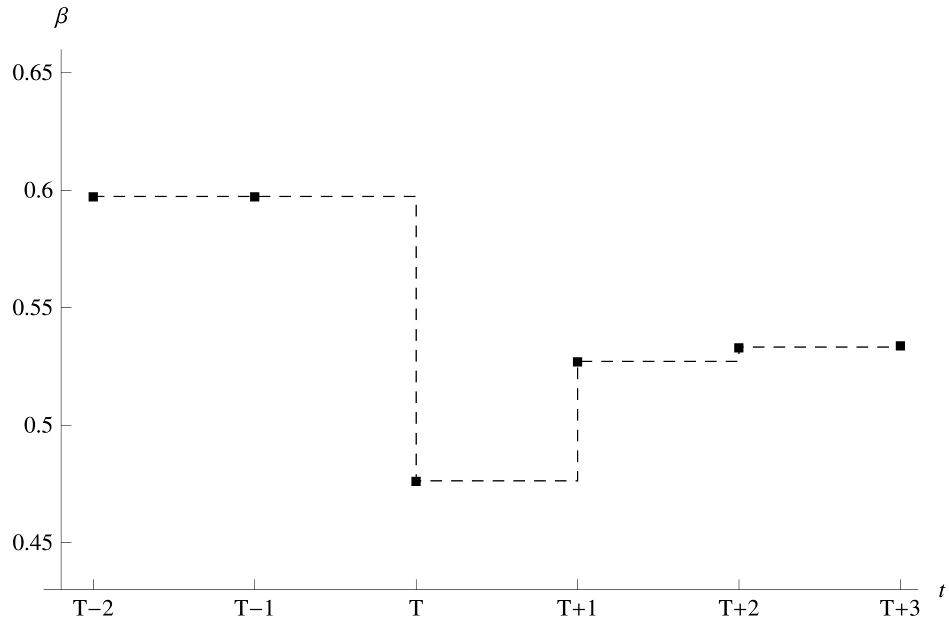
In the numerical example, mobility responded much more strongly in the first two than in subsequent generations. Can we then conclude that more distant events have only a negligible

[19] The idea that a shift towards "meritocratic" principles can also have depressing effects on mobility was already noted by the sociologist Michael Young, who coined the term in the book *The Rise of the Meritocracy* (1958). In contrast to its usage today, Young intended the term to have a derogatory connotation.

[20] From equation (8), a change to a more meritocratic society will then increase mobility initially iff $\frac{\gamma_1 - \gamma_2}{\rho_2 - \rho_1} > \lambda Cov(e_{T-1}, y_{T-1})$. However, mobility *decreases* in subsequent generations iff $\frac{\rho_2 - \rho_1}{\gamma_1 - \gamma_2} > \lambda Cov(e_{T-1}, y_{T-1})$. These conditions will be satisfied for any changes $\gamma_1 - \gamma_2$ and $\rho_2 - \rho_1$ that are of similar magnitude in absolute terms.

[21] We will illustrate the timing of mobility trends over *cohorts* further in Section 4.

Figure 2: A declining impact of parental income and increasing returns to skills



Note: Mobility trend over generations in numerical example. In generation T the impact of parental income γ declines from $\gamma_1 = 0.4$ to $\gamma_2 = 0.2$ while the returns to endowments and human capital ρ increase from $\rho_1 = 0.5$ to $\rho_2 = 0.7$ (assuming $\lambda = 0.6$).

effect on current trends? We believe not, for two reasons. First, plausible extensions of our model would generate slower transitions between steady states (e.g., considering wealth or capital accumulation, and direct causal effects from grandparents). Second, past events may have been more dramatic than more recent changes. For example, in the late 19th and early 20th century the US experienced rapid industrialization and urbanization, a strong decline in agricultural employment, mass migration, and a vast expansion of public schooling. The US participated in two world wars and went through a highly turbulent interwar period. Other countries experienced similarly stark transformations.

Much of the recent empirical literature measures trends in income mobility for offspring cohorts born from around 1950 to the 1970s, which are separated by only one or two generations from those events. Recent trends may thus partly reflect repercussions from such changes in the first half of the 20th century. Finally, our example illustrates that if those changes led to a more meritocratic society, mobility should perhaps be *expected* to decline in more recent cohorts.

2.3 Intergenerational Mobility in Times of Change

Our finding that a change to a more meritocratic society can lead to long-lasting and non-monotonic mobility trends is important for the interpretation of recent trends. But it relates to a rather specific structural change; one may thus expect that non-monotonic responses are more of an exception than a rule.

We next illustrate that such responses are instead quite typical. We now consider multiple types of human capital and endowments, as in equations (5) and (6). The notion of individual ability has recently shifted from a one-dimensional concept primarily related to IQ (as in Herrnstein and Murray, 1994) to a multidimensional set of traits that also recognizes the importance of noncognitive skills. A stream of evidence has supported this idea, showing that several distinct skills affect various labor market outcomes (e.g., Heckman et al., 2006; Lindqvist and Vestman, 2011). Such multiplicity has not yet been stressed in the intergenerational context (an exception is Bowles and Gintis, 2002), but our analysis illustrates that it provides implications that cannot be captured by single-skill models.[22]

EXAMPLE 3: CHANGING RETURNS TO SKILLS. Assume that the returns to different types of human capital or endowments change on the labor market ($\boldsymbol{\rho}_1 \neq \boldsymbol{\rho}_2$).

Changes in the returns to different types of skills could stem from changes in demand (e.g., as of trade, or industrial and technological change) or in relative supplies (e.g., as of immigration or changes in the production of skills). A specific example is the decrease in the demand for physical relative to cognitive ability as a labor market moves from agricultural to white-collar jobs. But relative returns may change also in periods that are much shorter than the time scale underlying our intergenerational analysis – a typical example is the job-polarization literature, which highlights how the IT revolution has implied a shift in demand from substitutable manual skills to complementary abstract skills (e.g., Levy, Murnane, and Autor, 2003).

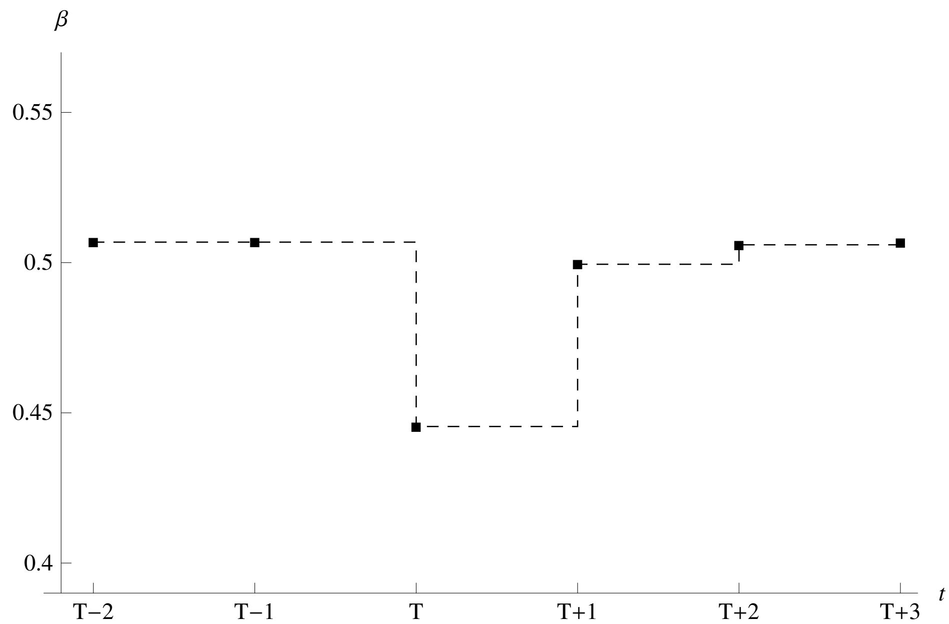
Figure (3) illustrates a simple symmetric case: two endowments k and l are equally transmitted within families ($\lambda_{ij} = \lambda$ for $i = j$ and $\lambda_{ij} = 0$ for $i \neq j$), but their prices on the labor market swap at time T ($p_{2,k} = \rho_{1,l} \neq p_{1,k} = \rho_{2,l}$). Adapting equations (5) and (6) for $K = 2$ endowments and iterating backwards we find that mobility increases in the first affected generation, but decreases in the next.[23]

Intuitively, those endowments or skills that have been more strongly rewarded in past generations are also more strongly correlated with parental income. As a consequence, mobility tends to initially increase if relative prices change, since endowments for which prices increase from low levels are less prevalent among high-income parents than endowments for which prices decrease from high levels. But the endowment for which prices increase becomes increasingly associated with high parental income in subsequent generations, causing

[22] Multiplicity of skills matters also for other questions in the literature. For example, Stuhler (2013) notes that income persistence over generations may decline more slowly than at a geometric rate if the degree of heritability varies across characteristics.

[23] We find $\Delta\beta_T = -\left(\rho_{k,2} - \rho_{k,1}\right)^2 \lambda/(1-\gamma\lambda)$, which is negative. The elasticity in the second generation shifts according to $\Delta\beta_{T+1} = \lambda(\rho_{k,2} - \rho_{k,1})^2 + \lambda(\rho_{k,2}^2 + \rho_{k,1}^2 + (2\rho_{k,1}\rho_{k,2}\lambda\gamma)/(1-\gamma\lambda))(1/Var(y_T) - 1)$, which is positive since $Var(y_T) = 1 - 2\gamma\lambda(\rho_{k,2} - \rho_{k,1})^2/(1-\gamma\lambda) < 1$. These findings are not due to shifts in cross-sectional inequality; if instead $Var(y_T) = 1$ (i.e. changes in ρ_k and ρ_l are offset by changes in the variance of u_t) we still have that $\Delta\beta_T < 0$ and $\Delta\beta_{T+1} > 0$.

Figure 3: A swap in prices



Note: Mobility trend over generations in numerical example. In generation T the returns to skill k increase from $\rho_{k,1} = 0.3$ to $\rho_{k,2} = 0.6$ and the returns to skill l decrease from $\rho_{l,1} = 0.6$ to $\rho_{l,2} = 0.3$ (assuming $\gamma = 0.2$ and $\lambda = 0.6$).

a decreasing mobility trend. The key assumptions underlying these results are that endowments are positively correlated within families and imperfectly correlated within individuals.

We can derive that non-monotonic responses in mobility are also typical when the returns to any number of skills change, by expressing the elasticity in generation T as a function of the steady-state elasticities before and after the structural change (β_{T-1} and $\beta_{t\to\infty}$). We assume here a diagonal heritability matrix $\mathbf{\Lambda}$. The derivation for more general cases (non-diagonal $\mathbf{\Lambda}$ and correlated endowments) is given in Appendix A.4. If the steady-state variance of income remains unchanged we have

$$\beta_{T-1} = \gamma + \boldsymbol{\rho}_1'\mathbf{\Lambda}\left(\boldsymbol{I} - \gamma\mathbf{\Lambda}\right)^{-1}\boldsymbol{\rho}_1 \tag{18}$$

and

$$\beta_{t\to\infty} = \gamma + \boldsymbol{\rho}_2'\mathbf{\Lambda}\left(\boldsymbol{I} - \gamma\mathbf{\Lambda}\right)^{-1}\boldsymbol{\rho}_2, \tag{19}$$

such that

$$\beta_T = \frac{1}{2}\left(\beta_{T-1} + \beta_{t\to\infty}\right) - \frac{1}{2}\left(\boldsymbol{\rho}_2' - \boldsymbol{\rho}_1'\right)\mathbf{\Lambda}\left(\boldsymbol{I} - \gamma\mathbf{\Lambda}\right)^{-1}\left(\boldsymbol{\rho}_2 - \boldsymbol{\rho}_1\right). \tag{20}$$

The quadratic form in the last term is greater than zero for $\boldsymbol{\rho}_2 \neq \boldsymbol{\rho}_1$ since $\mathbf{\Lambda}\left(\boldsymbol{I} - \gamma\mathbf{\Lambda}\right)^{-1}$ is positive definite. Eq. (20) states that intergenerational mobility in the first affected generation can be decomposed into two parts. Mobility in generation T equals the average of the old and the new steady-state mobility (first term), plus a purely *transitional gain* (second term). Price changes then lead to a temporary spike in mobility (β_T is below both the previous steady state β_{T-1} and the new steady state $\beta_{t\to\infty}$) if the steady-state elasticity does not shift too strongly, iff

$$|\beta_{t\to\infty} - \beta_{T-1}| < (\boldsymbol{\rho}_2' - \boldsymbol{\rho}_1')\boldsymbol{\Lambda}\,(\boldsymbol{I} - \gamma\boldsymbol{\Lambda})^{-1}\,(\boldsymbol{\rho}_2 - \boldsymbol{\rho}_1). \tag{21}$$

This argument also holds if cross-sectional inequality is lower in the new than in the old steady state.[24] Any symmetric changes (as in the numerical example) or changes in returns that do not affect long-run mobility much fulfill condition (21) and will thus lead to non-monotonic trends as in Figure 3.

We should thus expect "short-term" mobility gains if returns change, but those gains may not persist. These results have general implications on how we expect institutional or technological change to affect mobility. Previous authors have shown that technological progress can lead to non-monotonic mobility trends through *repeated* changes in skill returns (Galor and Tsiddon, 1997). We find that even a *one-time* change tends to generate such trends.

Implications. We can formulate a more general intuition, which applies to both of our last two examples. A change in the relative importance of different channels of intergenerational transmission will tend to increase mobility temporarily, as it affects the prospects of families differently. For example, a decline in the importance of parental income relative to own skills diminishes the prospects of offspring from high-income parents. The declining relative importance of a particular skill or endowment is to the disadvantage of those families in which it is abundant. Technological, economic, and social changes will often generate such relative gains and losses, generating *transitional* intergenerational mobility in the generation in which they occur.

The implications of our findings are not restricted to those particular types of structural changes that we examined explicitly. This may become more apparent if we allow for a broader definition of the endowment vector. For example, assume that $\boldsymbol{e}_t$ captures also the geographic location of individuals ("inherited" with some probability from their parents). We can then relate our last example to Long and Ferrie (2013), who argue that US occupational mobility may have been comparatively high in the 19th century as of exceptional internal *geographic* mobility. Our framework can support this hypothesis, but with a different emphasis. Intergenerational mobility may not necessarily increase due to internal migration itself (that depends on who migrates), but certainly due to one of its underlying causes: variation in labor

[24] Eq. (20) includes then the additional term $\boldsymbol{\rho}_2'\boldsymbol{\Lambda}\,(\boldsymbol{I} - \gamma\boldsymbol{\Lambda})^{-1}\,\boldsymbol{\rho}_2\,(1 - \frac{1}{Var(y_{t\to\infty})})$, which is negative if $Var(y_{t\to\infty}) < Var(y_{T-1}) = 1$.

demand across areas and time incentivizes internal migration, but it also directly increases intergenerational income mobility by generating different local demand conditions for parents and their (non-migrating) children.

We thus come to a quite general conclusion. First, times of change tend to be times of high intergenerational mobility. Moreover, such gains will be succeeded by a long-lasting decline in mobility, unless further structural changes occur. Countries experiencing a period of stable economic conditions will thus tend to be characterized by negative mobility trends if they were preceded by more turbulent times.

As noted above, countries such as the US may have experienced much greater societal transformations in the first than in the second half of the 20th century. Our findings suggest that such transformations may have strengthened intergenerational mobility in economic status in those generations that were directly affected.[25] Our model also illustrates that these mobility gains diminish in subsequent generations, providing another reason why mobility of more recent cohorts should perhaps be *expected* to decline.

3 Empirical Application

The core implication from our model is that even a single structural change should be expected to affect intergenerational mobility measures over long time periods. We examine now if such dynamic effects can be observed empirically.

We considered intergenerational mobility trends over *generations* in our theoretical framework, but empirical studies estimate mobility trends over *cohorts* (typically offspring cohorts). These two dimensions, which do not match due to variation of parental age at birth, have to our knowledge not yet been linked in the literature. An explicit consideration of cohorts (Section 4) will provide additional implications, some of which will already become apparent in our empirical analysis.

Our objective is to cleanly identify the effects of a major structural reform on mobility not only in the directly affected cohorts, but also in subsequent cohorts and generations. This intention leads to considerable requirements on both data coverage (requiring data on family links and individual outcomes over multiple decades) and identifiability of the reform impact among other determinants of mobility trends. Fortunately, the Swedish compulsory school reform and access to long-run registry data make such analysis possible.

[25] Note that much of the economic literature and our findings relate to *relative* mobility, how differences in economic outcomes among parents relate to differences among their offspring. Economic development or transitions may also generate *absolute* mobility, by generating differences in economic status between generations (see Goldthorpe, 2013).

3.1 The Swedish Compulsory School Reform

We describe here only the most important elements of the Swedish compulsory school reform, which is comprehensively discussed in Holmlund (2007). Gradually implemented across municipalities from the late 1940s, the reform's two main components were to *raise compulsory schooling* from seven (eight in some municipalities) to nine years, and to *postpone tracking* decisions from the fifth or seventh to after the ninth grade. The reform prescribed a unified national curriculum and municipalities received additional funding to cover costs from its implementation.

Our choice of application is motivated by three main reasons. First, education and educational systems are key mechanisms for the reproduction of economic advantage. Family background explains a large share of the variation in educational attainment , and institutional aspects are believed to affect that relationship (Björklund and Salvanes, 2010). Educational reforms or expansion are thus potential determinants of observed mobility changes over time (Machin, 2007), and school reforms are often directly motivated by a desire to increase mobility – indeed, one of the Swedish reform's objectives was to increase educational attainment among students from less advantaged backgrounds (Erikson and Jonsson, 1996). The Swedish and similar reforms in other Scandinavian countries have appeared to achieve this objective, raising income mobility in directly affected generations (see Meghir and Palme, 2005, Holmlund, 2008, and Pekkarinen et al., 2009).

Second, administrative data in Sweden cover an extraordinarily long time span. Coverage over three generations is needed to assess the reform's impact on mobility not only on directly affected but also the subsequent generation. Large sample sizes allow us to exploit fine geographic variation for causal identification and to detect gradual mobility changes over time.

Third, the reform's gradual implementation over municipalities allows separation of the reform from regional or time-specific effects. A number of studies exploit this characteristic to assess the causal effect of the reform on individual outcomes in directly affected, or spillover effects in subsequent generations (see e.g. Meghir and Palme, 2005; Holmlund et al., 2011; Meghir et al., 2011). While we follow a similar identification strategy, our objective is to examine the reform's effect on standard summary measures of intergenerational mobility instead of individual outcomes. Both aspects are related (e.g., Havnes and Mogstad, 2012), but mobility can respond dynamically even in the absence of intergenerational spillover effects, as we showed theoretically in Section 2.2.

We estimate the reform's impact on intergenerational mobility in income and educational attainment over two generations and compare the results against our theoretical predictions.

3.2 Compulsory Schooling in the Intergenerational Model

The impact of a compulsory schooling policy on educational and income mobility can be predicted from our theoretical framework. We first include constants α_y and α_h into the scalar variants of our baseline equations (2)-(3), thus allowing for mean changes in income and education. To capture the main component of the school reform assume then that eq. (3) determines *intended* schooling h^*, while from generation T onwards *actual* schooling h_t is compulsory until x years, such that

$$h_t = \begin{cases} h_t^* & \text{if } t < T \\ max(h_t^*, x) & \text{if } t \geq T \end{cases} . \tag{22}$$

The school reform raises schooling of individuals with particularly low educational attainment. This "mechanical" shift may in turn affect the attainment of others via potential general equilibrium responses. Compositional changes may generate peer effects, and changes in supply may alter the returns to schooling and thus schooling decisions.[26] However, a theoretical discussion of the numerous responses that may occur over such long time intervals can be only incomplete and speculative. We instead focus on the main "mechanical" effect of the school reform, which explains the observed empirical pattern well.

We study the dynamic response in the most popular measure of income and educational mobility, the intergenerational elasticity of income β_{inc} and educational coefficient β_{edu},

$$\beta_{inc,t} = \frac{Cov(y_t, y_{t-1})}{Var(y_{t-1})} \quad \text{and} \quad \beta_{edu,t} = \frac{Cov(h_t, h_{t-1})}{Var(h_{t-1})}. \tag{23}$$

In the previous section we derived this measure by repeated insertion of the structural equations of our model, using linearity of the expectation operator to solve for the required moments. But the compulsory schooling requirement generates a non-linear relationship between h_t and h_{t-1}, which depend also on the distributions of u_y, u_h and v.

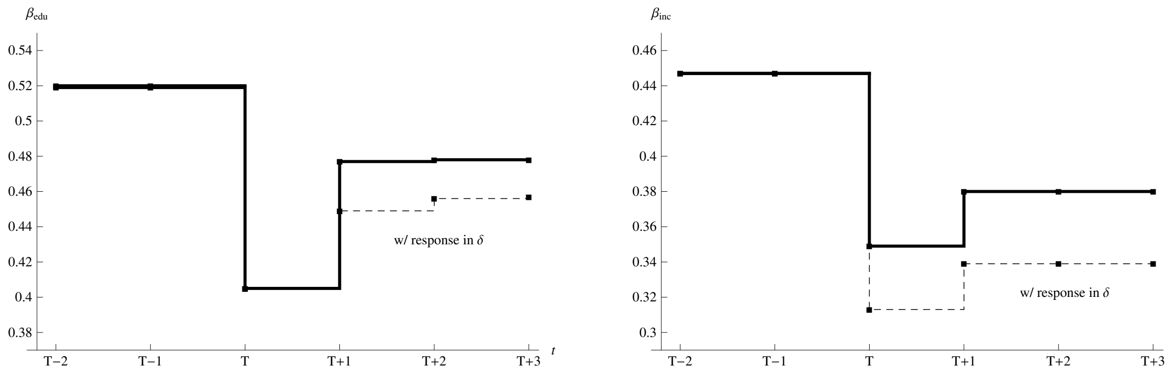
Figure 4 provides a simulated numerical example based on simple parametric assumptions (e.g., normally distributed errors). From generation T schooling becomes compulsory until $x = 9$ years. We assume that parental schooling has only modest indirect intergenerational spillover effects ($\gamma_h = 1$) and choose other parameters such to generate pre-reform first and second moments for income y_t and schooling h_t that are similar to the observed moments in the Swedish data.

Panel A plots the response of the intergenerational educational coefficient β_{edu}. In offspring generation T the reform compresses the variance of schooling strongly, which decreases the numerator of β_{edu} – differences in schooling between parents result into smaller

[26] Spillover effects on educational attainment of individuals not directly affected by the reform were found to be small in Holmlund (2007).

Figure 4: Raising the compulsory schooling level

(a) Intergenerational educational coefficient (b) Intergenerational income elasticity



Note: Income and educational mobility trends in numerical example, with $x = 9$, $\alpha_y = 9$, $\gamma_y = 0$, $\delta = 0.2$ (dashed line: $\delta = 0.18$), $\alpha_h = 10$, $\gamma_h = 1$, $\theta = 2$, $\lambda = 0.6$, and (u_y, u_h, v) normally distributed with variances $(0.1, 2.75, 0.64)$.

differences among their offspring. However, from generation $T + 1$ the variance of schooling is also compressed among *parents*, who were already subject to the school reform in the previous generation. The coefficient β_{edu} is inversely scaled by this variance, and thus tends to rise. The non-monotonic response is thus mainly a consequence of strong changes in the variance of the marginal distributions (a direct and mechanical effect of the reform).

The reform could lead to further substantial compressions of educational attainment in subsequent generations if schooling has very strong causal effects on offspring outcomes ($\gamma_h \gg 1$). However, the existing empirical literature points to modest intergenerational "multiplier" effects of education (see Plug et al., 2011). The dashed line illustrates one important potential general equilibrium response. Increased supply of formal schooling may decrease its returns on the labor market (a decrease in δ), decreasing inequality in income and thus (if human capital accumulation is subject to parental investments) educational inequality and intergenerational persistence.

A reduction in the degree to which differences in educational attainment are transmitted from parents to offspring will also reduce the transmission of income differences, if formal schooling improves an individual's earnings potential – the intergenerational income elasticity β_{inc} decreases in generation T (panel B in Figure 4). General equilibrium responses may affect this prediction. For example, increased supply of formal schooling may reduce its returns, thus decreasing the intergenerational elasticity further (dashed line). The second-generation response in β_{inc} is less clear-cut. Changes in the numerator of β_{inc} in eq. (23) are not as easily dominated by a decrease in the denominator in generation $T + 1$, which will tend to be weaker for β_{inc} than for β_{edu} since differences in formal schooling are not the only source of differences in income. The direction of the second-generation response in β_{inc} is thus an empirical question.

3.3 Data

Our *source data* is based on a 35 percent random sample of the Swedish population born between 1932 and 1967. Using information based on population registers we link sampled individuals to their siblings (all sibling types) as well as their (and their siblings') biological parents and children. We then individually match data on personal characteristics and place of residence based on bi-decennial censuses starting from 1960, as well as education data stemming from official registers. We do not use the sibling-parent subsample in our main analysis: it can provide additional precision in mobility estimates in 1940/50 cohorts, but is not representative for earlier and later cohorts.

Educational registers were compiled in 1970, 1990 and about every third year thereafter, containing detailed information on each individual's educational attainment.[27] Data in 1970 were collected only for those born 1911 and later. We can therefore not observe schooling for parents who were 33 years or older at their child's birth in 1943 (at the onset of the reform implementation). This age limit increases by a year for each subsequent offspring cohort, potentially creating a confounding trend in mobility measures over cohorts due to non-random sample selection. For comparability we thus restrict our *intergenerational sample* to parent-child pairs in which parents were no older than 32 years when their child was born. Educational data may also be missing for other reasons, in particular if parents had died or emigrated before 1970. The probability of such occurrences is potentially related to individual characteristics, but the share of affected observations is small.[28] As the data are collected from official registers there are no standard non-response problems.

The most recent educational register was compiled in 2007, which allows us to consider mobility trends for cohorts born from the early 1940s up until 1972. Attainment of individuals at the top of the educational distribution is not reliably covered for more recent cohorts; only a small population share is affected, but measurement error in the tails of the distribution would have a disproportionately large effect on intergenerational mobility measures.

We construct a measure of long-run income status based on age-specific averages of annual incomes, which are observed for the years 1968-2007.[29] Incomes for parents are necessarily measured at a later age than incomes for their offspring, which may bias estimates of the intergenerational elasticity of lifetime income. Such bias is less problematic for our purposes as we are interested in mobility differences between groups instead of the overall

[27] We consider for each individual the highest attainment recorded across these years. The information on schooling levels is translated into years of education with 7 years for the old compulsory school being the minimum, and 20 years for a doctoral degree the maximum.

[28] Educational information are less often missing among offspring, due to their younger age and the more frequent measurement of education after 1990. The share of missing observations does not vary with reform status (conditional on municipalities and offspring cohorts), and has thus little effect on our causal analysis.

[29] We use total (pre-tax) income, which is the sum of an individual's labor (and labor-related) earnings, early-age pensions, and net income from business and capital realizations. We express all incomes in 2005 prices and exclude observations with average incomes below 10000 SEK.

Table 1: Sample Statistics by Birth Cohort

	Source data			Intergenerational samples				
	# obs.	reform shares		# obs.	with non-missing		reform shares	
		(offspring)	(fathers)		(educ.)	(inc.)	(offspring)	(fathers)
1943	42,138	0.04	0.00	17,211	15,008	11,059	0.04	0.00
1944	44,715	0.06	0.00	18,425	16,179	14,016	0.06	0.00
1945	44,682	0.06	0.00	18,604	16,441	15,984	0.07	0.00
1946	44,299	0.11	0.00	19,124	17,101	16,800	0.11	0.00
1947	43,288	0.18	0.00	19,078	17,103	16,775	0.18	0.00
1948	42,527	0.31	0.00	19,063	17,192	16,881	0.31	0.00
1949	40,628	0.39	0.00	18,449	16,768	16,424	0.40	0.00
1950	38,854	0.53	0.00	19,421	17,657	17,288	0.54	0.00
1951	36,951	0.56	0.00	18,644	17,016	16,693	0.57	0.00
1952	37,031	0.69	0.00	19,102	17,442	17,085	0.70	0.00
1953	37,537	0.79	0.00	19,452	17,904	17,565	0.80	0.00
1954	35,668	0.86	0.00	18,453	16,955	16,589	0.87	0.00
1955	36,440	0.95	0.00	19,122	17,569	17,179	0.96	0.00
1956	36,666	1.00	0.00	20,942	19,217	18,714	1.00	0.00
...	...	...	...	...	...	...	...	...
1965	42,909	1.00	0.01	28,447	26,762	24,657	1.00	0.01
1966	43,050	1.00	0.01	29,043	27,415	25,166	1.00	0.02
1967	42,686	1.00	0.02	28,897	27,366	25,177	1.00	0.03
1968	54,105	1.00	0.04	33,526	32,524	30,124	1.00	0.05
1969	52,317	1.00	0.05	32,157	31,315	28,924	1.00	0.06
1970	53,908	1.00	0.07	32,508	31,788	29,195	1.00	0.08
1971	56,493	1.00	0.09	33,251	32,539	29,783	1.00	0.12
1972	57,035	1.00	0.12	33,081	32,409	29,472	1.00	0.16

Note: Father-child pairs are included in the intergenerational sample if father's age at birth of the child is below 33.

level of income mobility in the population.

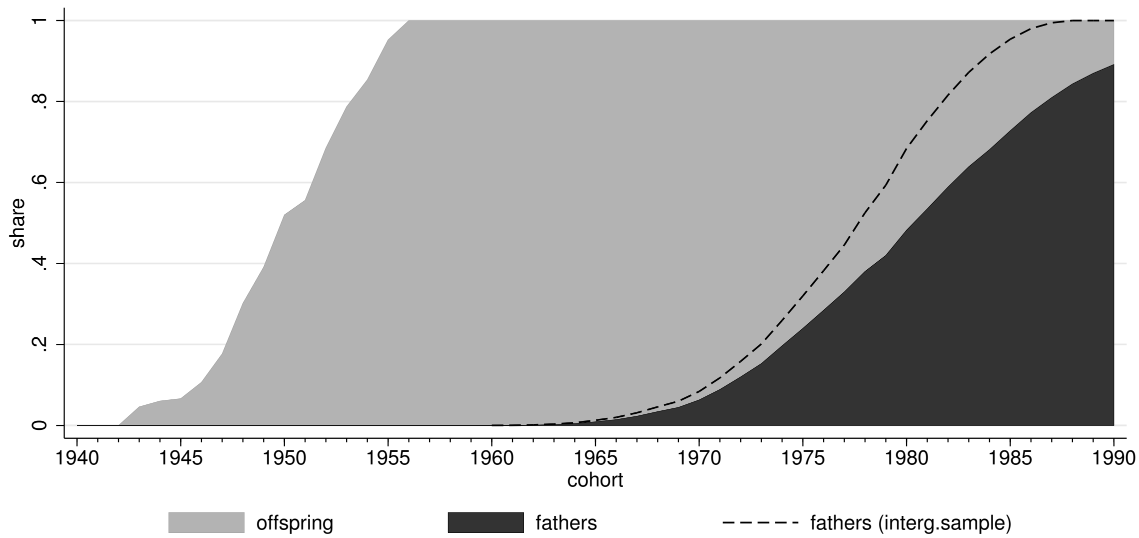
We present evidence on mobility in father-child pairs, but the consideration of maximum parental education and income yields similar results. We test the robustness of our results using other samples with no or different restrictions on parental age, or alternative measures of parental education and income, some of which we will also report below.

To construct the reform dummy, which indicates whether an individual was subject to the new system of comprehensive schooling, we follow the procedure first used by Holmlund (2008). Reform status can be approximated using information on an individual's birth year (from the administrative register) and place of residence during school age (from the censuses).[30] The gradual implementation of the reform affected cohorts born between 1938 and 1955, but the school municipality cannot be reliably determined for individuals born before 1943. As the share of individuals affected by the reform was very small we set the reform dummy to zero for all cohorts before 1943 (and one for all cohorts after 1955).[31]

[30]Reform status across cohort-municipality cells can be inferred by tracing in which cohort, for each municipality, the share graduating from the old school system discontinuously drops to zero (or close to zero). Helena Holmlund has kindly provided us with her coding, and we refer to Holmlund (2007) for further details on the coding procedure and potential measurement issues.

[31]Cohorts born before 1943 were subject to the new school system in 33 out of a total of 1034 municipalities. With the exception of less than a handful mid-sized urban municipalities, all of these were small, rural

Figure 5: Share of Offspring and Fathers Subject to Reform



Note: Share of offspring and fathers subject to school reform over offspring cohorts, in source data (grey and black areas) and intergenerational sample (dashed line).

Table 1 describes, by birth cohort, both the source data and the intergenerational sample, which was drawn according to the conditions described above. The number of observations for each cohort are listed in columns 2 and 5. Columns 6 and 7 describe the number of observations with non-missing education or income information. Columns 3-4 and 8-9 describe how the share of offspring and fathers attending reformed schools increases over cohorts. It increases faster among fathers in the intergenerational sample than in the source data, due to oversampling of younger parents in the former.[32]

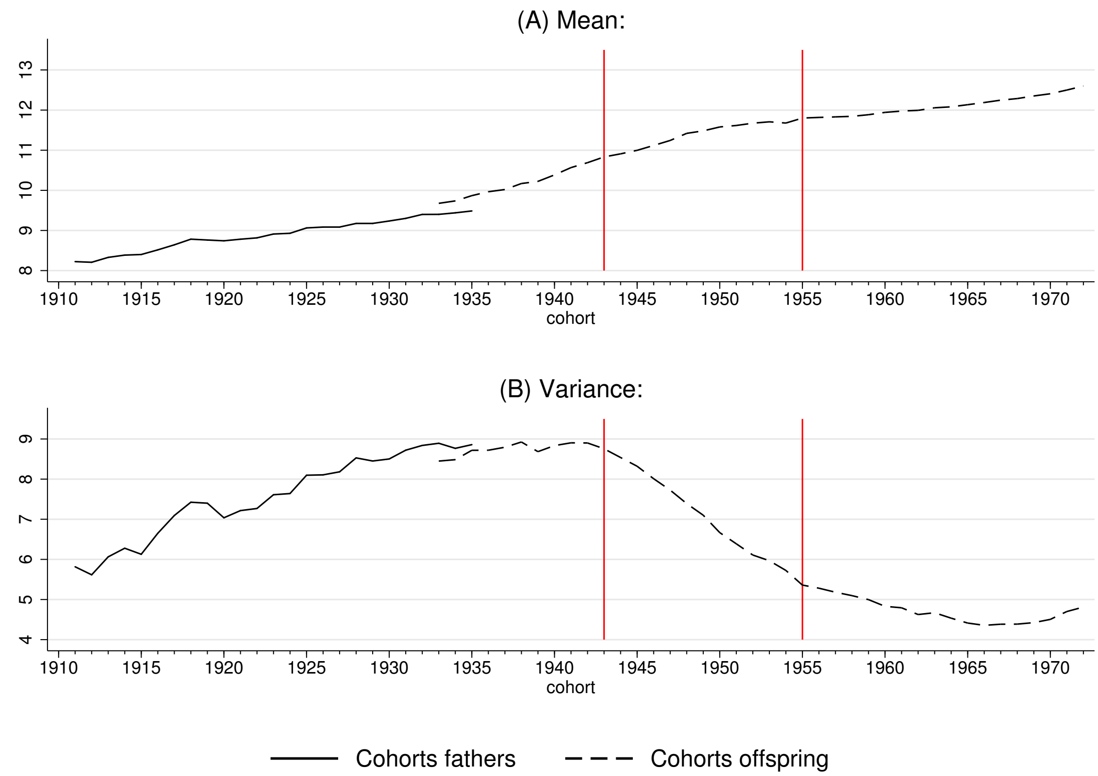
3.4 Empirical Evidence

Descriptive Evidence. To illustrate the timing of the reform further, Figure 5 plots the shares of offspring and fathers attending a reformed school in our source data over half a century of (offspring) birth cohorts. The share of children subject to the reform increases nearly linearly in cohorts 1943-1955 (gray area). These individuals become parents themselves from the early 1960s, but their share among all parents increases more slowly due to variation in parental age at birth (black area). Up until the early 1980s only a minority of fathers had themselves been affected by the compulsory school reform. This observation leads

municipalities. We further drop a small number of municipalities for which the implementation date is unclear.

[32]A smaller share of individuals from the raw data are sampled among earlier cohorts, as their fathers are less likely to be identified in the source data. Identification of the reform effect requires that the probabilities that fathers, education and income are observed do not change systematically with introduction of the reform. While sampling probabilities differ across birth cohorts and municipalities, the correlation with reform status is negligible.

Figure 6: Mean and Variance of Years of Schooling over Cohorts



Note: Moments of years of schooling over cohorts of offspring (dashed line) and their fathers (solid line) in intergenerational sample.

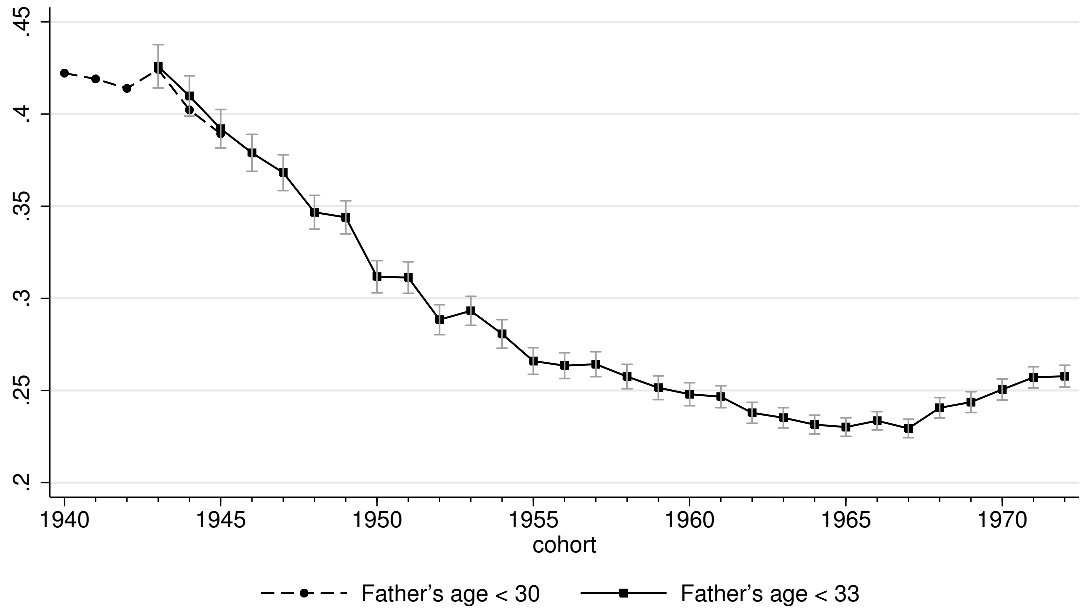
to a first important point: the dynamic effect of structural changes on mobility measures in subsequent generations should be *gradual*, due to variation of parental age at birth. We will discuss this implication in more detail in Section 4. As noted, the share of fathers subject to the reform increases faster in our intergenerational sample, which is restricted to younger parents (dashed line). Our results will therefore understate the longevity of the reform's effect on mobility measures.

The reform had a direct impact on educational attainment, which can be also measured with high precision over long time intervals.[33]

Figure 6 plots the mean and variance of years of schooling of offspring cohorts (1933-1972) and their fathers (1911-1935) in our intergenerational sample. Vertical bars at the 1943 and 1955 cohorts indicate the start and end point of the reform's implementation. A reform effect on *average* years of schooling is not easily discernible from panel (A). Indeed, Holmlund (2007) finds the reform effect on mean schooling to be small (lower bound estimate

[33]A measure of education in later life is likely to capture an individual's entire educational attainment, as most people complete schooling in early life. In contrast, differences in current incomes are poor proxies of differences in lifetime income, such that measures of income mobility (in particular of mobility trends) are sensitive even to small changes in the age at which incomes are observed (the *life-cycle bias* problem, see Jenkins, 1987, Haider and Solon, 2006, and Nybom and Stuhler, 2011).

Figure 7: Trends in the Intergenerational Educational Coefficient over Cohorts



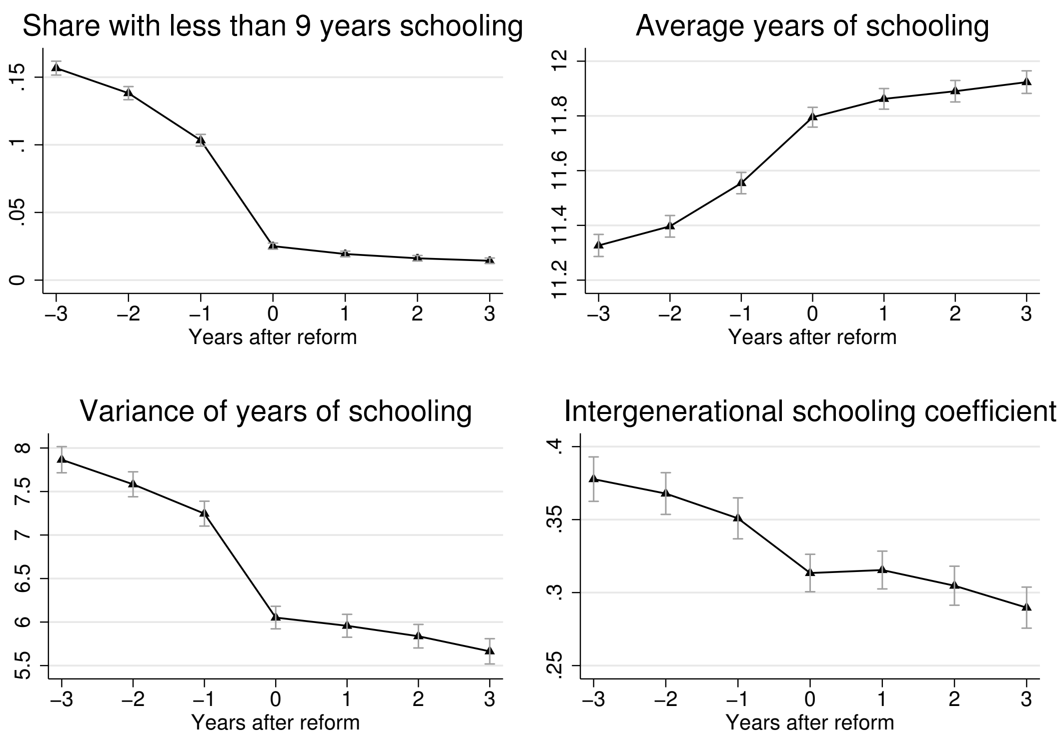
Note: Each dot represents the coefficient from a regression of years of schooling of offspring in the respective birth cohort on years of schooling of their fathers. Based on intergenerational sample (fathers aged below 33, solid line) and subsample (fathers aged below 30, dashed line). Grey bars: 95% confidence intervals.

of 0.19 years), as only a share of children are affected by the compulsory requirement. In contrast, the shift in the *variance* of schooling is more striking: the reform period coincides with a sudden and strong compression of the distribution of schooling. Comparison with earlier trends for their fathers in the first half of the 20th century illustrates the exceptional magnitude of those changes.

Intergenerational Mobility Trend. Figure 7 plots cohort trends in the intergenerational educational coefficient, the slope coefficient in an ordinary least-squares regression of offspring's years on father's years of schooling. The solid line includes estimates from our main intergenerational sample, spanning from 1943 to 1972. The dashed line represents estimates from a restricted sample containing younger fathers (aged below 30), allowing us to plot trends also for earlier cohorts not yet affected by the reform. We find estimated trends to be very robust to changes in sample restrictions concerning parental age, as exemplified by the close overlap for the 1943-1945 cohorts (plotted) and beyond.

The reform's implementation period coincides with a large drop in the intergenerational coefficient, contrasting with stable estimates before the onset of the school reform. The degree to which differences in schooling are transmitted to the next generation declines by more than a third. This decline is consistent with our theoretical expectation: the reform compresses the distribution of years of schooling in the offspring generation, such that differences

Figure 8: Educational Attainment and Intergenerational Mobility, Pre- vs. Post-Reform



Note: We recenter the data such that the reform occurs at time zero for each municipality. Panels (a)-(c) summarize the distribution of offspring educational attainment. Each dot in panel (d) represents the coefficient from a regression of years of schooling of offspring on years of schooling of their fathers. Based on intergenerational sample (fathers aged below 33). Grey bars: 95% confidence intervals.

in parental education correspond to smaller differences in offspring attainment.

Reform Effect. Figure 8 provides more direct evidence on the reform impact. Recentering the data within each municipality, we compare educational attainment and the intergenerational educational coefficient before and after a cohort was first subject to the new school type. The share of individuals with less than 9 years, the variance of schooling and the intergenerational schooling coefficient all drop strongly with local reform implementation.

We can exploit the gradual introduction of the reform to verify its causal impact, adapting a difference-in-differences specification as similarly used in Holmlund (2008) and Pekkarinen et al. (2009). Consider the regression equation for schooling (income)

$$\begin{aligned} h_{cfm,t} \; = \; & \underbrace{\alpha_1 + \beta_1 h_{t-1}}_{\text{baseline}} + \underbrace{\alpha_2 R_{cm} + \beta_2 \left(h_{t-1} \times R_{cm}\right)}_{\text{reform effect}} \\ & + \underbrace{\boldsymbol{\alpha}_3' \boldsymbol{D}_c + \boldsymbol{\beta}_3' \left(h_{t-1} \times \boldsymbol{D}_c\right)}_{\text{offspring cohort effect}} + \underbrace{\boldsymbol{\alpha}_4' \boldsymbol{D}_m + \boldsymbol{\beta}_4' \left(h_{t-1} \times \boldsymbol{D}_m\right)}_{\text{municipality effect}} + \varepsilon_{cfm,t}, \end{aligned} \qquad (24)$$

where $h_{cfm,t}$ represents years of schooling (log income) of the offspring in generation t of

Table 2: Reform Effect on Educational and Income Mobility, Cohorts 1943-1955

Panel A: Education	education offspring (# years)			
	(1)	(2)	(3)	(4)
education father (# years)	0.359***	0.396***	0.454***	0.422***
	(0.00383)	(0.00496)	(0.0233)	(0.00750)
reform		1.407***	0.977***	0.555***
		(0.0577)	(0.0696)	(0.0672)
reform x education father		-0.0969***	-0.0639***	-0.0371***
		(0.00632)	(0.00685)	(0.00722)
constant	8.331***	7.770***	7.298***	7.306***
	(0.0433)	(0.0477)	(0.216)	(0.0683)
N	220335	220335	220335	220335
Panel B: Income	log income offspring			
	(1)	(2)	(3)	(4)
log inc. father	0.164***	0.157***	0.172***	0.139***
	(0.00265)	(0.00402)	(0.0194)	(0.0162)
reform		-0.0111	0.102	0.253**
		(0.0759)	(0.0936)	(0.121)
reform x log inc. father		0.00510	-0.00588	-0.0196**
		(0.00618)	(0.00760)	0.00995)
constant	9.893***	9.947***	9.762***	9.915***
	(0.0324)	(0.0487)	(0.236)	(0.195)
N	199340	199340	199340	199340
municipality controls				x
offspring cohort controls			x	x

Note: Clustered (municipality level) standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Coefficient estimates from equation (24) (column 4) and simplified variants (columns 1-3), based on offspring cohorts 1943-1955 in intergenerational sample.

family i (subscript suppressed) born in cohort c, to a father of generation $t-1$ born in cohort f, attending school in municipality m. The variable h_{t-1} represents years of schooling (log income) of fathers. The indicator R_{cm} equals one if the reform was in effect for cohort c in municipality m. We control for differences in both schooling levels and the intergenerational coefficient across child cohorts (captured in the indicator vector $\boldsymbol{D}_c$) and across municipalities (captured in $\boldsymbol{D}_m$).

The identifying variation that we exploit in this specification are municipality-specific changes in the intergenerational coefficient after local introduction of the reform. While controlling for common time trends and for persistent differences across areas, this strategy is still susceptible to differences in municipality-specific trends. Moreover, the reform indicator is measured with error, which may introduce attenuation bias. We address both issues below.[34]

Table 2 presents OLS estimates from different variants of specification (24), based on a

[34]Some pupils may have moved in response to local reform implementation, but Holmlund (2007) finds that there was little selective mobility with respect to parental background.

pooled sample of those cohorts that were affected by the reform introduction phase (1943-1955). Panel (A) presents our findings on educational mobility. The estimated schooling coefficient for a simple pooled regression (column 1) of 0.359 approximates the average of cohort-specific estimates over that period (see Figure 7).[35] The second column presents separate estimates for children who were and who were not subject to the reform. Differences in parental educational attainment are associated with much smaller differences in attainment among the former. To identify the reform's causal contribution we successively introduce cohort and municipality fixed effects and interactions in the next columns. Standard errors are clustered on the municipality level. Estimates for the full difference-in-differences specification are presented in column 4. We find that the Swedish compulsory school reform reduced the degree to which differences in educational attainment were transmitted from fathers to their children by about ten percent ($\hat{\beta}_2 = -0.0371, p < 0.001$).

Panel (B) of Table 2 presents corresponding estimates on income mobility. Our measure of long-run income of offspring (fathers) is based on average incomes in age 30-35 (age 53-59). Given observation of incomes at such a young (old) age for offspring (fathers), the pooled coefficient of 0.164 is likely to understate the true degree of intergenerational persistence in lifetime income (see Nybom and Stuhler, 2011). We can nevertheless identify if the reform had an effect on income mobility. Our difference-in-differences estimate implies that the degree to which percentage income differences were transmitted from fathers to their children decreased by about ten percent due to the reform ($\hat{\beta}_2 = -0.0196$, $p < 0.05$). These results are consistent with findings by Holmlund (2008).

Our estimates are not sensitive to the inclusion of father cohort effects and remain statistically significant also for a number of alternative specifications, as discussed in more detail below. We conclude that the reform had a clear positive effect on both educational and income mobility in the first affected generation.

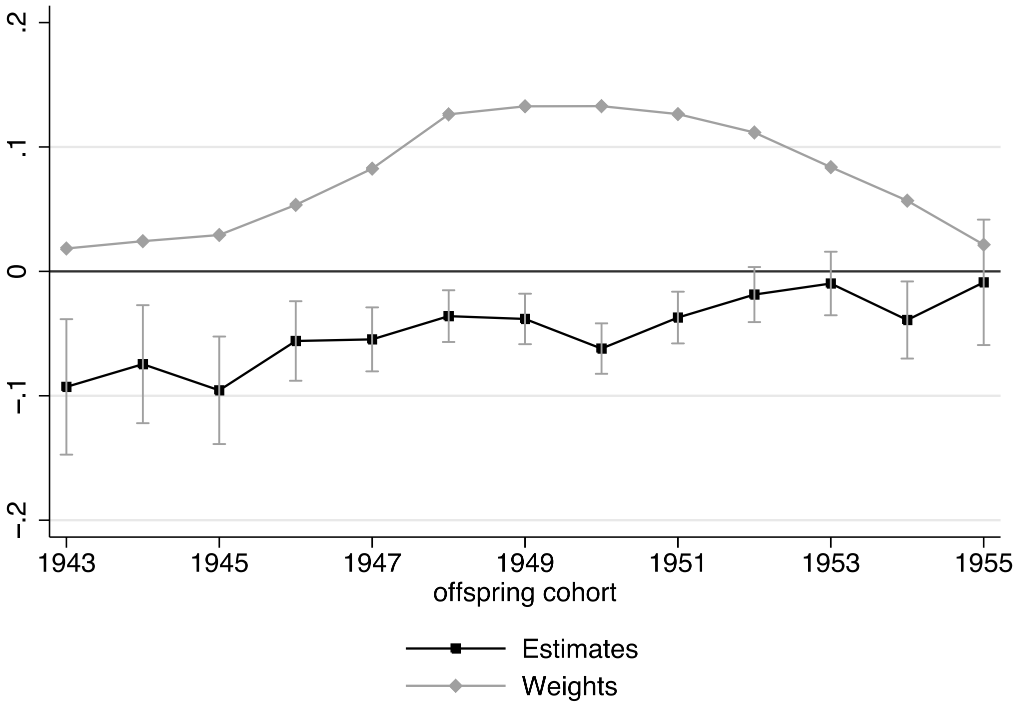
Heterogeneity. Yet, this effect may be smaller than expected. The intergenerational educational coefficient dropped by more than a third during the reform introduction phase (from about 0.42 to 0.27, see Figure 7). Furthermore, a sudden trend change occurred in the mid-1940s, even though few municipalities had yet been subject to the reform. This pattern can be understood if we examine the heterogeneity in the reform's effect over time. We interact the reform with offspring cohort dummies, exploiting that in each cohort additional municipalities switch to the new school system. The reform effect in specification (24) then equals

$$\boldsymbol{\alpha}_2 \left(R_{cm} \times \boldsymbol{D}_c\right) + \boldsymbol{\beta}_2 \left(h_{t-1} \times R_{cm} \times \boldsymbol{D}_c\right). \tag{25}$$

Figure 9 plots the resulting estimates for the elements of $\boldsymbol{\beta}_2$ (black line). The reform had

[35] Differences in yearly means also affect the pooled coefficient (Hertz, 2008), but their contribution is small.

Figure 9: Heterogeneity in the Reform Effect over Cohorts



Note: Estimates of the reform effect on the intergenerational educational coefficient over cohorts (black line), and their respective weight in the pooled coefficient (grey line). Based on intergenerational sample (fathers aged below 33), including sibling subsample. Grey bars: 95% confidence intervals.

a very strong impact in earlier cohorts, reducing the intergenerational coefficient by almost 25 percent in those municipalities that were subject to the reform already in the early 1940s. But coefficient estimates decrease over cohorts, implying that its impact on later cohorts was small. The reason becomes clear from Figure 6. The general trend towards higher educational attainment made the main component of the reform (the rise of the compulsory school level to nine years) less consequential: as by the early 1950s most pupils were attending school for at least nine years anyways. The reform effect can thus be seen as an intention-to-treat estimate, with the share of compliers diminishing over cohorts. We therefore conclude that the reform caused the sudden drop in the intergenerational coefficient in the early 1940s, but that much of its overall decline until the mid 1950s might have occured even in the absence of the reform.[36]

The pooled (difference-in-differences) coefficient that we presented in Table 2 can be

[36]Our estimates may understate the reform effect in later cohorts if it generated anticipation or spillover effects in non-reform schools (individual schooling decisions may depend on the educational attainment of others). Our argument that the reform's impact was larger in earlier cohorts still holds, as educational attainment was steadily increasing even before the reform was introduced.

decomposed as a weighted average of these cohort-specific reform effects,

$$\beta_2(DD) = \sum_{c=1}^{\bar{c}} \beta_{2,c} w(\beta_{2,c}), \tag{26}$$

where c denotes cohorts, $\beta_{2,c}$ the cohort-specific reform effects, and $w(\beta_{2,c})$ the weight assigned to each cohort. These weights are defined as

$$w(\beta_{2,c}) = \frac{Var(h_{t-1} \times R_{cm} \mid h_{t-1}, R_{cm}, \boldsymbol{D}_c = D_c) P(\boldsymbol{D}_c = D_c)}{\sum_{c=1}^{\bar{c}} Var(h_{t-1} \times R_{cm} \mid h_{t-1}, R_{cm}, \boldsymbol{D}_c = D_c) P(\boldsymbol{D}_c = D_c)}. \tag{27}$$

The pooled estimator will assign more weight to large cohorts, and cohorts with greater variance in father's schooling and the reform dummy (conditional on their covariance). Thus, the pooled coefficient is likely to be most affected by cohorts in which the shares of affected and unaffected by the reform are similar in size (i.e. the variance of the reform dummy is maximized). This will especially hold if the variance of father's schooling is relatively stable over the implementation period, which is true in our case. Sample analogs of these weights are plotted in the grey line in Figure 9. As suspected, the weights are highest around the 1950 cohort and still high for later cohorts. In contrast, the weights are close to zero for earlier cohorts. The pooled coefficient (Table 2) reflects therefore mostly the reform impact on later cohorts, which was comparatively small.

Our example points to a general feature of difference-in-differences analyses with gradual (or staggered) treatment implementation. Treatment effects are assumed to be constant over time in a standard specification, but are likely heterogeneous if the counterfactual is subject to trends. The pooled coefficient then gets a more complex interpretation and may to a large extent reflect the reform impact at a particular point in time, which can be quite different from its initial impact.

Second generation effect. Figure 7 documents a second, more gradual but nevertheless pronounced change in mobility over time. After its large and long decline, the coefficient starts rising again among cohorts born in the late 1960s. Incidentally, these were the first cohorts in which some children were born to fathers who already themselves attended a reform school (see Figure 5).

But is the modest increase in the data really the dynamic impact of the reform, and not the product of coincidental (and potentially contemporaneous) factors? We can distinguish

Table 3: Reform Effect on Educational and Income Mobility, Cohorts 1966-1972

Panel A: Education	education offspring (# years)			
	(1)	(2)	(3)	(4)
education father (# years)	0.240***	0.238***	0.195***	0.294***
	(0.00214)	(0.00298)	(0.00929)	(0.00411)
reform (father)		-0.904***	-0.923***	-0.768***
		(0.0894)	(0.0847)	(0.139)
reform x education father		0.0534***	0.0727***	0.0655***
		(0.00893)	(0.00762)	(0.0128)
constant	9.741***	9.813***	10.07***	9.763***
	(0.0233)	(0.0356)	(0.0938)	(0.0471)
N	111173	111173	111173	111173
Panel B: Income	log income offspring			
	(1)	(2)	(3)	(4)
log inc. father	0.207***	0.211***	0.186***	0.244***
	(0.00530)	(0.00601)	(0.0172)	(0.0093)
reform (father)		0.331**	-0.0949	-0.498*
		(0.156)	(0.164)	(0.265)
reform x log inc. father		-0.0286**	0.00814	0.0410*
		(0.0128)	(0.0134)	(0.0216)
constant	9.666***	9.618***	9.874***	9.446***
	(0.0650)	(0.0734)	(0.210)	(0.118)
N	110317	110317	110317	110317
municipality controls				x
father cohort controls			x	x

Note: Clustered (municipality level) standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Coefficient estimates from equation (28) (column 4) and simplified variants (columns 1-3), based on offspring cohorts 1966-1972 in intergenerational sample.

these sources by adapting regression equation 24 for the next generation. We estimate

$$\begin{aligned} h_{cfm',t} =& \underbrace{\alpha_1 + \beta_1 h_{t-1}}_{\text{baseline}} + \underbrace{\alpha_2 R_{fm'} + \beta_2 \left(h_{t-1} \times R_{fm'}\right)}_{\text{father reform effect}} \\ &+ \underbrace{\boldsymbol{\alpha}_3' \boldsymbol{D}_f + \boldsymbol{\beta}_3' \left(h_{t-1} \times \boldsymbol{D}_f\right)}_{\text{father cohort effect}} + \underbrace{\boldsymbol{\alpha}_4' \boldsymbol{D}_{m'} + \boldsymbol{\beta}_4' \left(h_{t-1} \times \boldsymbol{D}_{m'}\right)}_{\text{father municipality effect}} + \varepsilon_{cfm,t}, \end{aligned} \tag{28}$$

where the indicator $R_{fm'}$ equals one if the reform was in effect for *father* cohort f born in municipality m'.

Table 3 presents OLS estimates from variants of specification (24), using offspring cohorts 1966-1972 in which the share of reform fathers is above one percent (adding earlier cohorts has little effect on the estimates). Panel (A) presents our results on educational mobility. Estimates for the full difference-in-differences specification (column 4, $\hat{\beta}_2 = 0.0655$, $p < 0.001$) indicate that the observed rise in the intergenerational educational coefficient is indeed a dynamic response to the school reform that occurred in the previous generation.

Panel (B) presents estimates of the reform's second-generation effect on income mobility.

We can observe parental incomes at an earlier age for later cohorts, and use observations in age 35-45 to construct our measure of long-run status. The pooled coefficient estimate of 0.207 is thus likely to be less biased than the corresponding estimate for the first generation. As with education, the reform's impact on the intergenerational coefficient ($\hat{\beta}_2 = 0.041$, $p < 0.05$) is larger than the corresponding estimate for the first generation. Two factors explain this finding. First, fathers who themselves were subject to the reform had their children at young age. Young fathers tend to have less educational attainment, the reform impact on this group was thus large. A second explanation follows from Figure 9 – children born in the late 1960s are more likely to have parents born in the early 1940s than later We showed that the reform impact was much larger on the former, due to underlying trends in education.

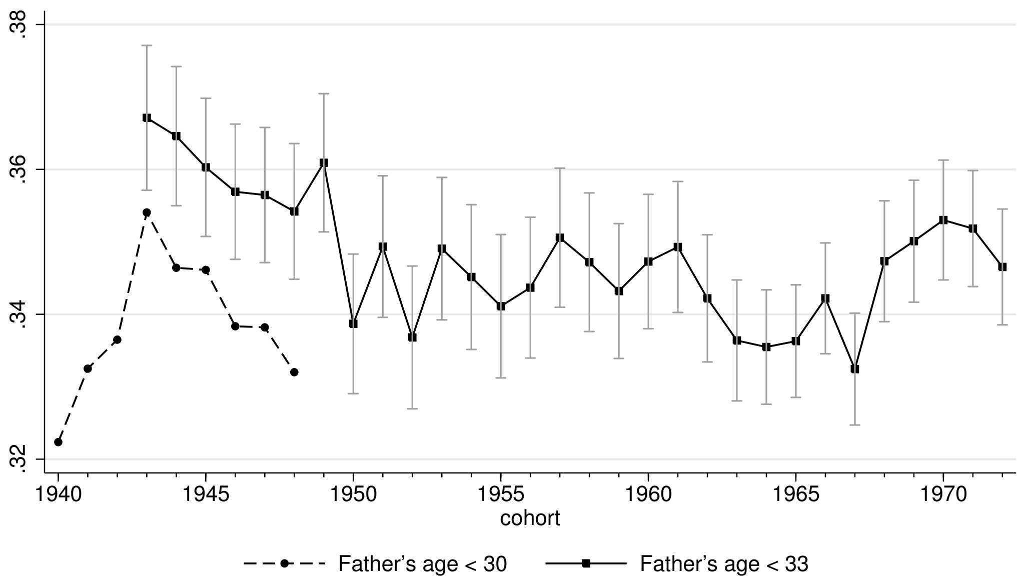
In our data we can track the intergenerational coefficient only up to 1972, but the share of reform fathers will continue to climb until the early 2000s (see Figure 5). Unless dominated by contemporaneous events we thus expect the intergenerational income elasticity and educational coefficient to rise for several decades after our records end.

Intergenerational Correlation. The reform's impact on the intergenerational regression coefficient exemplifies our argument that current mobility levels and trends can be affected by events that occured in a more distant past. The school reform compresses the distribution of years of schooling, first decreasing the regression coefficient when affecting the offspring's distribution, and increasing it in later cohorts when also affecting parents. But it is less obvious what trend we should expect in the intergenerational correlation, which abstracts from differences in cross-sectional inequality over generations.

Figure 10 plots estimates of the intergenerational correlation from 1940 to 1972. Estimates from our main intergenerational sample are represented by the solid line, while the dashed line shows estimates from a restricted sample containing fathers aged below 30 to examine trends also for earlier cohorts. Estimated levels are sensitive to changes in sample restrictions concerning parental age, but the pattern over cohorts appears robust. The intergenerational correlation is strongly increasing among cohorts not yet affected by the reform, but the correlation starts declining shortly after introduction of the reform from 1943 and remains lower until the end of our observation period in the early 1970s. The overall change in the correlation is smaller than the change in the regression coefficient.

Estimates are comparatively low already in the early 1950s. The difference is not statistically significant, but such pattern would not be surprising: our model predicts that the intergenerational correlation should be particularly low when the shares of children subject and not subject to the reform are similar, as a larger part of the variation in schooling is then explained by reform status instead of parental background. The rising coefficient towards the end of our sampling period is not predicted by our model; given its suddenness it is likely due to contemporaneous instead of past events. We return to this argument in our next section.

Figure 10: Trends in the Intergenerational Educational Correlation over Cohorts



Note: Each dot represents the correlation coefficient between years of schooling of offspring in the respective birth cohort and years of schooling of their fathers. Based on intergenerational sample (fathers aged below 33, solid line) and subsample (fathers aged below 30, dashed line). Grey bars: 95% confidence intervals.

Robustness. We perform a number of tests to probe the robustness of our results. Table 4 compares our baseline estimates of the reform effect on the intergenerational educational coefficient and income elasticity with estimates from six alternative specifications. First, we include matched siblings in our sample, which increases its size but also diminishes representativeness for some cohorts (see data subsection). Second, we restrict the sample to younger fathers with age at birth below 30, to probe the sensitivity of our results to such age restrictions. Our third robustness tests address measurement error in the reform indicator. Individuals who have been in a lower than expected grade from delayed school entry or grade repetition may have been subject to the reform before others from the same birth cohort (see Holmlund, 2007). The resulting attenuation bias can be reduced by dropping all individuals born in the cohort just preceding local implementation of the reform. Fourth, we use the maximum of both parents' (instead of the father's) educational attainment or income. Fifth, we include additional controls for the birth cohort of fathers (first generation) or offspring (second generation estimates). Finally, we include municipality-specific linear time trends to support the common trends assumption that is underlying our difference-in-differences analysis.

Our estimates of the reform effect on the intergenerational educational coefficient remain statistically significant on the $p < 0.001$ level across all specifications. Their sizes vary either very little or as expected. In particular, they increase in absolute size when measurement error in the reform indicator is being addressed (column 4). Estimates differ slightly also

Table 4: Robustness Tests

	baseline	with siblings	fathers below 30	pre-reform dropped	parental max.	cohort controls	municip. time trends
Education:							
1st gen.	-0.0371***	-0.0393***	-0.0408***	-0.0434***	-0.0357***	-0.0387***	-0.0364***
	(0.0072)	(0.0054)	(0.0089)	(0.0083)	(0.0064)	(0.0073)	(0.0074)
2nd gen.	0.0655***	0.0651***	0.0655***	0.0710***	0.0307***	0.0655***	0.0622***
	(0.0128)	(0.0122)	(0.0128)	(0.0139)	(0.0093)	(0.0126)	(0.0131)
Income:							
1st gen.	-0.0196*	-0.0078	-0.0181	-0.0195*	-0.0210**	-0.0233**	-0.0239**
	(0.0100)	(0.0068)	(0.0115)	(0.0118)	(0.0088)	(0.0095)	(0.0097)
2nd gen.	0.0410*	0.0148	0.0410*	0.0492**	0.0344**	0.0418**	0.0363*
	(0.0216)	(0.0165)	(0.0216)	(0.0238)	(0.0155)	(0.0212)	(0.0219)

Note: Sensitivity analyses reporting the coefficient on the interaction between reform dummy and parental education and income and clustered standard errors (in parentheses), * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column 1 contains the baseline specification. For the next columns we include the sibling subsample, restrict the sample to fathers with age at birth below 30, drop offspring born in the cohort preceeding the reform implementation, use the maximum of mother's and father's education or income, include father (rows 1 and 3) or offspring cohort dummies (rows 5 and 7), or include municipality-specific linear trends.

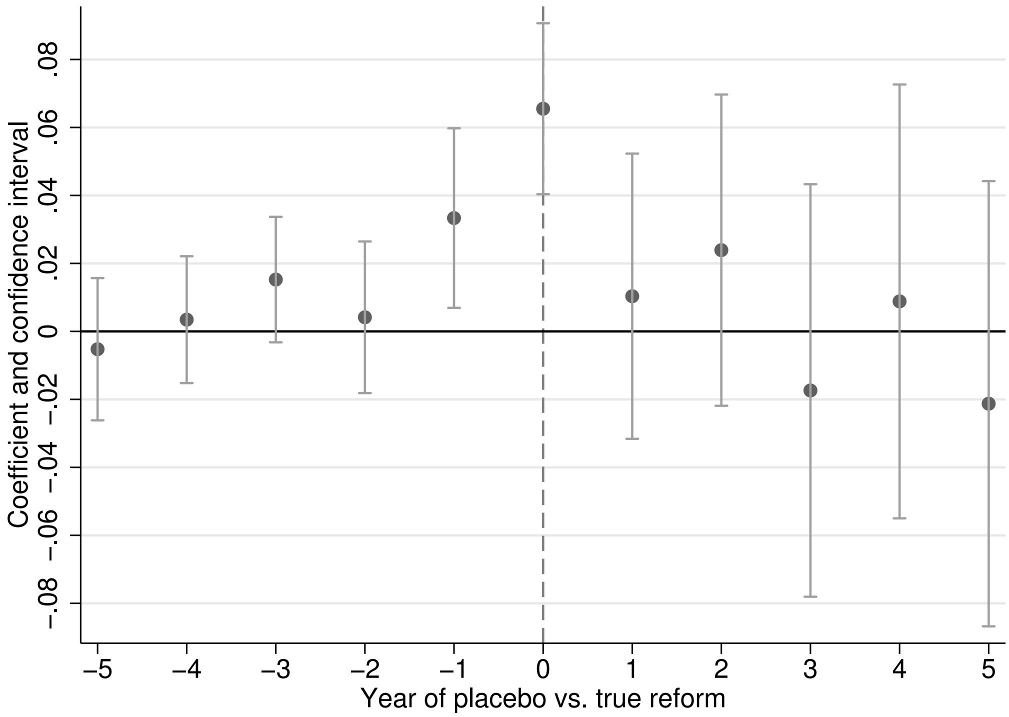
when we estimate a parent-offspring (instead of father-offspring) measure of persistence, using maximum education among both mothers and fathers as independent variable (column 5). Estimates of the reform effect on the intergenerational income elasticity have always the same sign, but vary more strongly and are not always statistically significant on the $p < 0.05$ or even $p < 0.1$ level. Two factors reduce precision. First, long-run income is measured with much larger error than educational attainment. Second, the reform had a mechanic and strong effect on the distribution of educational attainment, while incomes were only indirectly affected.

Overall the tests corroborate the existence and the direction of reform effects on the intergenerational persistence in both education and income, but underscore that the former is more precisely estimated. We provide further evidence on the suitability of our identification strategy and the common trends assumption by performing a number of placebo tests. Following Meghir et al. (2011) we falsely assume that the reform took place before or after the actual implementation date. We first sample only those offspring born in 1966 to 1972 whose fathers were subject to the reform and generate a placebo "non-treated" group by pretending that the school reform was implemented one year later, two years, three years, and so on. Similarly, we sample only those fathers who were not treated and pretend that the reform was implemented earlier, thus generating a placebo "treated" group. The resulting estimates are plotted in Figure 11.[37]

Each dot represents the estimate of the reform effect on the intergenerational educational

[37] Corresponding tests provide supportive evidence also for the first-generation estimates (available upon request).

Figure 11: Placebo Test: Second Generation



Note: Each dot represents an estimate of the reform effect on the intergenerational educational coefficient in cohorts 1966-72 under the assumption that the reform took place at the specified period before or after the actual implementation date. Based on intergenerational sample (fathers aged below 33). Grey bars: 95% confidence intervals.

coefficient assuming the reform took place at the specified period before or after the actual implementation date. The largest estimate is obtained when we use the correct timing for the reform assignment (at zero). We find small and insignificant estimates in all other cases, except when we assume that the reform was implemented one year before the actual date. Measurement error in reform status is a potential explanation for this observation, as discussed above and also visible from Figure 8 – those in a lower than expected grade may have been subject to the reform even though not captured by our reform indicator (see Holmlund, 2007).

4 From Generations to Cohorts

Our model is broadly in line with the previous literature, but motivated by our empirical application we will next relax its coarse generational perspective.[38] The existing theoretical

[38] A more detailed discussion of our theoretical model is given in Nybom and Stuhler (2013), in which we discuss some of its other simplifying assumptions. We demonstrate that our results are not sensitive to the way the influence of parental income is modeled, and that more recursive causal mechanisms (independent effects from grandparents) lead to prolonged dynamic responses of mobility trends to structural shocks.

literature considers intergenerational transmission between *generations*, but empirical studies estimate mobility trends over *cohorts*. These two dimensions, which do not match due to variation of parental age at birth, have to our knowledge not yet been linked in the literature. We therefore introduce a cohort dimension into our model. Our initial motivation was to provide a closer match to the empirical literature, but this extension will also reveal a prospective avenue for identification of past structural changes in mobility levels and trends.

We adopt the following notation to distinguish cohorts and generations. Let the random variable C_t denote the cohort into which a member of generation t of a family is born. Let $A_{t-1,C(t)}$ be a random variable that denotes the age of the parent at birth of the offspring generation t born in cohort C_t. For simplicity assume $A_{t-1,C(t)}$ to be independent of parental income and characteristics, but allow for dependence on C_t, so that its distribution can change over time. Member $t-j$ of a family is then born in cohort

$$C_{t-j} = C_t - A_{t-1,C(t)} - ... - A_{t-j,C(t-j+1)}. \tag{29}$$

Denote realizations of these random variables by lower case letters. For simplicity we consider the scalar case with a single skill. Our reduced two-equations model for intergenerational transmission between offspring born into cohort $C_t = c_t$ and a parent born in cohort $C_{t-1} = c_{t-1}$ is then given by

$$y_{t,c(t)} = \gamma_{c(t)} y_{t-1,c(t-1)} + \rho_{c(t)} e_{t,c(t)} + u_{t,c(t)} \tag{30}$$

$$e_{t,c(t)} = \lambda_{c(t)} e_{t-1,c(t-1)} + v_{t,c(t)}, \tag{31}$$

where we keep the simplifying assumptions as in our baseline model in equations (5) and (6). By considering a single set of equations for each generation we abstract from life-cycle effects within a given generation. The transmission parameters in (30) and (31) can thus be interpreted as representing an average of effective transmission mechanisms over the life-cycle. For example, the price parameter $\rho_{c(t)}$ reflects average returns throughout the working life of an individual born in year c_t.[39]

Consider for simplicity again the special case in which cross-sectional inequality remains constant, such that $Var(y_{t,c(t)}) = Var(e_{t,c(t)}) = 1 \; \forall t, c(t)$. Using (30) and (31), the intergenerational income elasticity of the offspring generation t born in cohort c_t then equals

$$\beta_{t,c(t)} = \frac{Cov\big(y_{t,c(t)}, y_{t-1,C(t-1)}\big)}{Var\big(y_{t,c(t)}\big)} = \gamma_{c(t)} + \rho_{c(t)}\lambda_{c(t)} Cov\left(e_{t-1,C(t-1)}, y_{t-1,C(t-1)}\right), \tag{32}$$

[39] A consideration of life-cycle effects (as in Conlisk, 1969, or Cunha and Heckman, 2007) would be interesting, but the general implications that we discuss here hold as long as some intergenerational transmission mechanisms tend to be effective in early life (e.g., genetic transmission, childhood environment, and education).

where for convenience we do not explicitly condition on $C_t = c_t$. Mobility for a given cohort depends on cohort-specific transmission mechanisms and the covariance of income and endowments in the parent generation. However, this cross-covariance may vary with parental age, since different parental cohorts might have been subject to different policies and institutions. Using eq. (29) and the law of iterated expectations we rewrite eq. (32) as

$$\begin{aligned}
\beta_{t,c(t)} &= \gamma_{c(t)} + \rho_{c(t)}\lambda_{c(t)} E_{A(t-1)}\left(Cov\big(e_{t-1,c(t)-A(t-1)}, y_{t-1,c(t)-A(t-1)} | A_{t-1,c(t)}\big)\right) \\
&= \gamma_{c(t)} + \rho_{c(t)}\lambda_{c(t)} \sum_{a_{t-1}} f_{c(t)}\big(a_{t-1}\big) Cov\big(e_{t-1,c(t)-a(t-1)}, y_{t-1,c(t)-a(t-1)}\big), \qquad (33)
\end{aligned}$$

where $f_{c(t)}$ is the probability mass function for parental age at birth of cohort c_t. Income mobility thus depends on current transmission mechanisms and a *weighted average* of the cross-covariance of income and endowments in previous cohorts, where the weights are given by the cohort-specific distribution of parental age in the population.[40]

We can iterate backwards to express $\beta_{t,c(t)}$ in terms of parameter values only, and find

$$\beta_{t,c(t)} = \gamma_{c(t)} + \rho_{c(t)}\lambda_{c(t)} \sum_{a_{t-1}} f_{c(t)}(a_{t-1})\rho_{c(t)-a(t-1)} + \rho_{c(t)}\lambda_{c(t)} \sum_{r=1}^{\infty} z_r, \qquad (34)$$

where

$$z_r = \sum_{a_{t-1}} \Big(f_{c(t)}(a_{t-1}) \ldots \sum_{a_{t-r-1}} \Big(f_{c(t-r)}(a_{t-r-1}) \prod_{s=1}^{r} \Big(\gamma_{c(t-s)}\lambda_{c(t-s)}\Big) \rho_{c(t-r-1)} \Big)\Big).$$

Equation (34) summarizes how mobility trends across cohorts respond to structural changes. The insights from the generations-only model still hold, but the explicit consideration of cohorts leads to a number of additional implications.[41]

First, while a rapid structural change may have a sudden impact on mobility in the first generation, their effect on mobility trends in subsequent generations will be *gradual* due to variation of parental age at birth. This is exactly the pattern we found in our empirical application (see Figures 5 and 7).

Second, the importance of past institutions and policies on current mobility rises with parental age at birth. Likewise, the impact of structural changes on mobility trends will die out faster in populations in which individuals become parents at younger ages. Cross-country

[40] The decomposition of the cross-covariance of income and endowments into conditional cross-covariances was simplified here by assuming that first moments of the distribution of those variables are constant over cohorts. In the empirical application we consider cases in which those moments are not constant.

[41] In steady state, both equations (8) and (34) simplify to equation (9). The explicit consideration of cohorts has consequences only for transitions between steady states, which may explain why existing steady-state models have not yet been explicitly linked to cohort-specific measures of mobility.

mobility differentials are thus not only driven by differences in both current and past transmission mechanisms, but also by different *weights* on past mechanisms. This argument might be particularly relevant for comparisons between developed and developing countries.[42]

Finally, equation (34) points to a potential avenue for identification of *past* structural changes in *current* mobility trends, exploiting that the influence of the former on the latter is a function of parental age at birth. As an example, assume that from cohort c^* onwards an expansion of public childcare reduces the heritability of endowments from λ_1 to λ_2.[43] Assume that all parents of generation t were not yet subject to the new regime, such that

$$\lambda_{C(t-1)} \quad = \quad \begin{cases} \lambda_1 & for\ C_{t-1} < c^* \\ \lambda_2 & for\ C_{t-1} \geq c^* \end{cases} .$$

Other parameters remain unchanged and all grandparents have been subject to the old regime. From equation (34), the *conditional intergenerational elasticities* among children with old ($C_{t-1} < c^*$) or young ($C_{t-1} \geq c^*$) parents equal

$$\beta_{t,c(t)}\Big|_{C_{t-1}<c^*} = \gamma + \rho\lambda_2\gamma\lambda_1 Cov\left(e_{t-2,C(t-2)}, y_{t-2,C(t-2)}\right) + \rho^2\lambda_1 \tag{35}$$

and

$$\beta_{t,c(t)}\Big|_{C_{t-1}\geq c^*} = \gamma + \rho\lambda_2\gamma\lambda_2 Cov\left(e_{t-2,C(t-2)}, y_{t-2,C(t-2)}\right) + \rho^2\lambda_1. \tag{36}$$

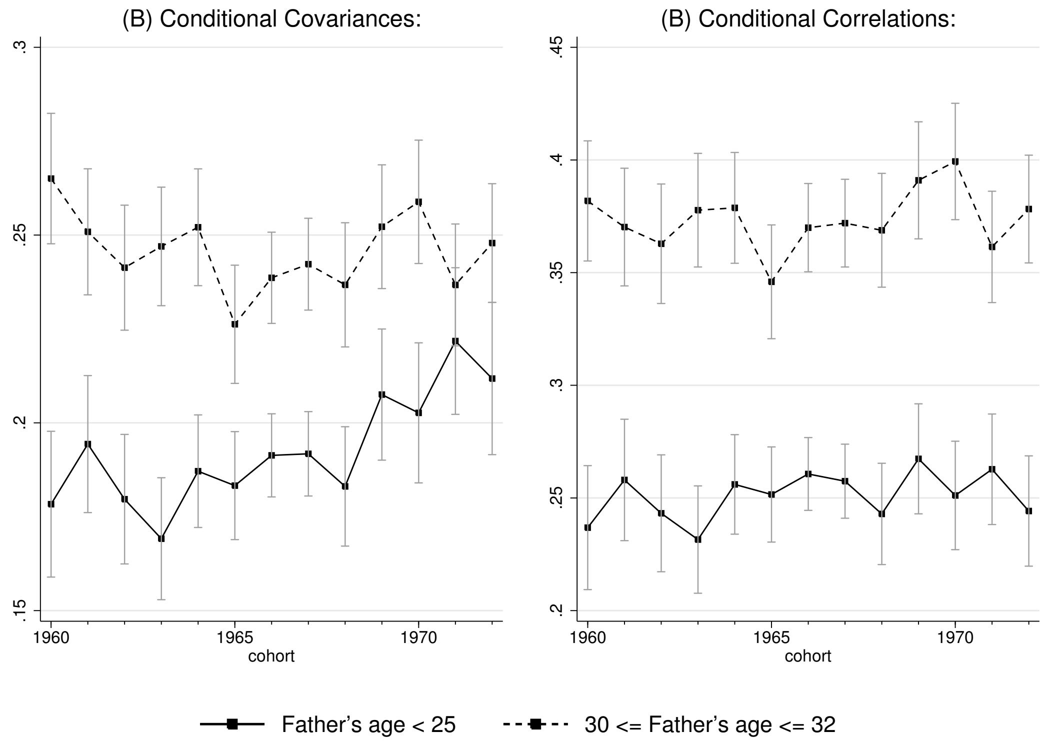
Differencing equations (35) and (36) then reveals the dynamic, or second-generation impact of the reform on current mobility levels. In practice we may of course encounter various obstacles that are ignored in this simple example. In particular, parental age is likely to correlate with other parental characteristics and thus mobility of their offspring.

A more targeted analysis was feasible in our empirical application: we directly conditioned on parental exposure to a particular school reform, exploiting our knowledge of the geographic variation in its time of effectiveness. We can use the same application to illustrate that even in the absence of such direct evidence, a comparison of conditional mobility measures may still provide a first clue about dynamic effects of past events on current trends. Panel (A) in Figure 12 plots conditional coefficients from a regression of offspring on father's years of schooling, for cohorts born from the 1960s until 1972. The pattern is consistent with our previous results: the intergenerational coefficient increases first among families with

[42]Our results imply that mobility in developing countries, in which parents tend to be younger, is less dependent on past institutions. Our example in Section (2.3) points to another potential source for *high* mobility in developing countries, in which returns to certain skills or regional wage levels may be comparatively variable over time (e.g., due to internal conflict or rapid economic and societal change).

[43]For example, Havnes and Mogstad (2012) find that access to subsidized childcare in Norway benefited children from low-income parents the most.

Figure 12: Trends in the Conditional Intergenerational Educational Coefficient and Correlation



Note: Each dot in panel (A) represents the coefficient from a regression of offspring years of schooling in the respective birth cohort on father years of schooling, for fathers aged 18-24 (solid line) or fathers aged 30-32 (dashed line) at offspring birth. Panel (B) presents the corresponding correlation coefficients. Grey bars: 95% confidence intervals.

younger fathers, who were more likely to have been subject to the school reform themselves. Panel (B) shows that the corresponding trend in the intergenerational correlation coefficient is not systematically related to parental age at birth.

5 Conclusions

We examined the dynamic relationship between intergenerational mobility in economic outcomes and its underlying structural factors. We showed, theoretically and empirically, that changes in the economic environment affect intergenerational persistence not only in directly affected but also in *subsequent* generations.

Our objective in the empirical application was to identify such dynamic effects for a particular policy reform. Using administrative microdata over three generations, we showed that a Swedish compulsory schooling reform decreased educational and income persistence in directly affected cohorts – by up to a fourth among earlier cohorts, in which the compulsory requirement affected a larger share of the population. But the reform's impact in the subsequent generation was of comparable magnitude, increasing the intergenerational educational coefficient and income elasticity and thus lowering mobility. This second-generation effect is likely to extend to very recent cohorts, as the majority of parents who were themselves subject to the reform had not yet had children when our sample ends. By looking solely at directly affected cohorts, previous research on similar reforms has thus likely overstated their long-run (or net) mobility effects.

We based our theoretical analysis on a simple simultaneous-equations model, deviating from the existing literature in our focus on its dynamic properties and our consideration of a multidimensional skill vector. We showed that mobility today depends not only on current transmission mechanisms, but also on the joint distribution of income and endowments in past generations – and thus on past mechanisms. Policy or institutional reforms generate therefore long-lasting mobility trends, which are often non-monotonic. Some implications may be surprising, especially our finding that negative mobility trends today can stem from *gains* in equality of opportunity in the past. Other conclusions may have a more intuitive appeal, such that mobility will tend to be higher in times of structural changes.

While the focus was on the general relationship between causal transmission mechanisms and mobility trends, we also noted various practical implications. For example, we showed that the impact of rising wage differentials in US and other countries on mobility may not yet have been fully realized in current data. Changing returns to skills shift intergenerational mobility over at least two generations, while other measures of persistence respond more immediately. This argument may explain why the empirical literature finds increasing sibling correlations in earnings in the US, but less evidence for a corresponding increase in intergenerational persistence. The latter has been surprising as both theoretical (Solon, 2004) and cross-country evidence (e.g., Corak, 2013) suggest a negative relation between cross-sectional inequality and intergenerational mobility.

This implication may be of concern for mobility proponents, as it suggests that a recent decline in mobility might yet to be uncovered by empirical research. But our results also point to a rather innocuous explanation for such observation. We showed that a shift towards a more meritocratic society (a rise in the importance of own skill relative to parental status) tends to generate a non-monotonic response – a mobility gain in the first affected generation, followed by a long-lasting negative trend. We should then perhaps expect mobility to decline in countries that became more meritocratic and mobile in the first half of the 20th century.

Finally, our finding that intergenerational mobility tends to be high in times of change seems consistent with recent evidence from the empirical literature. Long and Ferrie (2013) find that US occupational mobility was comparatively high in the late 19th century, and suggest that an exceptional degree of geographic mobility may have raised intergenerational mobility. Our model points to a potential joint cause for both: strong variation in economic conditions across areas and time not only incentivizes internal migration, it also increases intergenerational mobility by altering the local demand conditions that parents and children face during their lifetimes.

Our model is of course highly stylized, and a thorough discussion of related applications requires careful treatment of issues that we only touched upon (such as the timing of intergenerational transmission over an individual's life-cycle, or the difficulties that hinder reliable estimation of trends in income mobility). We briefly addressed promising avenues for future empirical research, noting that different potential causes of mobility shifts could be distinguished by their divergent dynamic implications; that the covariance between income and endowments in the *parent* generation plays a central role in the evolution of income mobility over generations; and that estimation of mobility measures conditional on parental age at birth may provide initial evidence on the effect of past events on current mobility trends.

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Appendix

A.1 An Economic Model of Intergenerational Transmission

We model the optimizing behavior of parents to derive the "mechanical" transmission equations presented in Section 2. For this purpose we extend the model in Solon (2004), considering parental investments in multiple distinctive types of human capital and statistical discrimination on the labor market.

Assume that parents allocate their lifetime *after tax earnings* $(1-\tau)Y_{t-1}$ between own *consumption* C_{t-1} and *investments* $I_{1,t-1},...,I_{J,t-1}$ in J distinctive types of human capital of their children. Parents do not bequeath financial assets and face the budget constraint

$$(1-\tau)\,Y_{t-1} = C_{t-1} + \sum_{j=1}^{J} I_{j,t-1}. \tag{37}$$

Accumulation of *human capital* h of type j in offspring generation t depends on parental investment, a $Kx1$ vector of inherited *endowments* $\boldsymbol{e}_t$, and chance $u_{j,t}$,

$$h_{j,t} = \gamma_j log I_{j,t-1} + \boldsymbol{\theta}_j' \boldsymbol{e}_t + u_{j,t} \;\; \forall j \in 1,...,J, \tag{38}$$

where γ_j and elements of the vector $\boldsymbol{\theta}_j$ measure the marginal product of parental investment and each endowment. Endowments represent early child attributes that may be influenced by nature (genetic inheritance) or nurture (e.g. parental upbringing). We assume that they are positively correlated between parents and their children, as implied by the autoregressive process

$$e_{k,t} = \lambda_k e_{k,t-1} + v_{k,t} \;\; \forall k \in 1,...,K, \tag{39}$$

where $v_{k,t}$ is a white-noise error term and the heritability coefficient λ_k lies between 0 and 1. We may allow endowments to be correlated within individuals, leading to the more general transmission equation (4). Finally, assume that income of offspring equals

$$logY_t = \begin{cases} \boldsymbol{\delta}' \boldsymbol{h}_t + u_{y,t} & \text{with probability p} \\ \boldsymbol{\delta}' E\left[\boldsymbol{h}_t | Y_{t-1}\right] + u_{y,t} & \text{with probability } 1-p \end{cases}. \tag{40}$$

With probability p employers observe human capital of workers and pay them their marginal product $\boldsymbol{\delta}'\boldsymbol{h}_t$ plus a white-noise error term $u_{y,t}$, which reflects market luck. With probability $1-p$ employers cannot uncover true productivity, and remunerate workers instead for their *expected* productivity given observed parental background. In particular, employers observe that on average parents invest income share s_j in offspring human capital of type j, such that $E\left[I_{j,t-1}|Y_{t-1}\right] = s_j Y_{t-1}$, and that the offspring of high-income parents tend to have more favorable endowments, such that $E\left[e_{k,t}|Y_{t-1}\right]=\gamma_k Y_{t-1}$ (with $\gamma_k\geq 0$) for all $k\in 1,...,K$.

Parents choose investment in the child's human capital as to maximize the utility function

$$U_{t-1}=(1-\alpha)\,logC_{t-1}+\alpha E\left[logY_t|Y_{t-1},\boldsymbol{I}_{t-1},\boldsymbol{e}_t\right], \tag{41}$$

where the altruism parameter $\alpha\in[0,1]$ measures the parent's taste for own consumption relative to the child's expected income. Given equations (37) to (41), the Lagrangian for parent's investment decision is

$$\begin{aligned}\mathcal{L}(C_{t-1},\boldsymbol{I}_{t-1},\mu) &= (1-\alpha)logC_{t-1}+\alpha\boldsymbol{\delta}'\left(pE\left[\boldsymbol{h}_t|Y_{t-1},\boldsymbol{I}_{t-1},\boldsymbol{e}_t\right]+(1-p)E\left[\boldsymbol{h}_t|Y_{t-1}\right]\right)\\ &\quad+\mu\left((1-\tau)\,Y_{t-1}-C_{t-1}-\mathbf{1}'\boldsymbol{I}_{t-1}\right)\end{aligned}$$

The first-order conditions require that

$$\begin{aligned}\frac{\partial\mathcal{L}}{\partial C_{t-1}} &= \tfrac{1-\alpha}{C_{t-1}}-\mu &&=0,\\ \frac{\partial\mathcal{L}}{\partial I_{j,t-1}} &= \tfrac{\alpha(1-p)\delta_j\gamma_j}{I_{j,t-1}}-\mu &&=0\ \ \forall j\in 1,...,J,\\ \frac{\partial\mathcal{L}}{\partial\mu} &= (1-\tau)Y_{t-1}-C_{t-1}-\mathbf{1}'\boldsymbol{I}_{t-1} &&=0.\end{aligned}$$

Optimal investments,

$$I_{j,t-1}=\frac{\alpha p\delta_j\gamma_j}{(1-\alpha)+\sum_{l=1}^{J}\alpha p\delta_l\gamma_l}(1-\tau)Y_{t-1}\quad\forall j\in 1,...,J, \tag{42}$$

increase in parental altruism and income, and in the probability that offspring human capital is observed and acted on by employers. Parents invest more into those skills in which the marginal product of investment or the return on the labor market are large. Plugging optimal investment into equation (38) yields (ignoring constants, which are irrelevant for our analysis) equation (3), which if plugged in turn into eq. (40) motivates equation (2).

A.2 Reduced Form and Stability

The reduced form of equations (5) and (6) is

$$\begin{pmatrix} y_t\\ \boldsymbol{e}_t\end{pmatrix}=\begin{pmatrix}\gamma_{y,t}+\boldsymbol{\delta}_t'\boldsymbol{\gamma}_{h,t} & \boldsymbol{\delta}_t'\boldsymbol{\Theta}_t\boldsymbol{\Lambda}_t\\ \mathbf{0} & \boldsymbol{\Lambda}_t\end{pmatrix}\begin{pmatrix} y_{t-1}\\ \boldsymbol{e}_{t-1}\end{pmatrix}+\begin{pmatrix} u_{y,t}+\boldsymbol{\delta}_t'\boldsymbol{u}_{h,t}+\boldsymbol{\delta}_t'\boldsymbol{\Theta}_t\boldsymbol{v}_t\\ \boldsymbol{v}_t\end{pmatrix}, \tag{43}$$

which we may shorten to

$$\boldsymbol{x}_t = \boldsymbol{A}_t\boldsymbol{x}_{t-1}+\boldsymbol{w}_t. \tag{44}$$

Let subscripts $1, 2$ index parameter values before and after a structural shock occurs in generation T.[44] The stability condition $\lim_{s\to\infty} \boldsymbol{A}_2^s = 0$ is then satisfied by assuming that $\gamma_{y,2} + \boldsymbol{\delta}_2' \boldsymbol{\gamma}_{h,2}$ and all eigenvalues of $\boldsymbol{\Lambda}_2$ are non-negative and below one.[45] These conditions also ensure that the transitions of the first and second moments of the distribution of $\boldsymbol{x}_t$ towards their steady state values are monotonic (see Jenkins, 1982), a property that however does not extend to the transition path of the intergenerational elasticity, as we discuss in Sections 2. Normalization of the variances of y_t and elements of $\boldsymbol{h}_t$ and $\boldsymbol{e}_t$ in the initial steady state leads to additional parameter restrictions. Take the covariance of (44) and denote the covariance matrices of x_t and w_t by $\boldsymbol{S}_t$ and $\boldsymbol{W}_t$, such that

$$\boldsymbol{S}_t = \boldsymbol{A}_t \boldsymbol{S}_{t-1} \boldsymbol{A}_t' + \boldsymbol{W}_t.$$

Denote by γ, $\boldsymbol{\rho}$, and $\boldsymbol{\Lambda}$ the steady-state parameter values before a structural change occurs in generation $t = T$. Note that in steady state $\boldsymbol{S}_t = \boldsymbol{S}_{t-1} = \boldsymbol{S}$, normalize all diagonal elements of $\boldsymbol{S}$ to one, and solve for the variances of $u_{y,t}$ and elements of $\boldsymbol{u}_{h,t}$ and $\boldsymbol{v}_t$. For example, if $\boldsymbol{\Lambda}_t$ is diagonal then $Var(e_{j,t}) = 1 \forall j$ iff $Var(v_{j,t}) = 1 - \lambda_j^2 \forall j$; the variances are non-negative iff $\lambda_{jj} \leq 1 \, \forall j$, as is also required for stability of the system.

A.3 Choice of Parameter Values

Our main findings do not rely on specific parameter choices, but our numerical examples will benefit from parametrizations that are consistent with the empirical literature. One difficulty is that some variables in our model represent broad concepts (e.g., human capital $\boldsymbol{h}_t$ may include any productive characteristic of an individual), which are only imperfectly captured by data. In addition, the parameters of the model reflect total effects from those variables. While estimates of (intergenerational) correlations and other moments are widely reported, there exists less knowledge about the relative importance of the various underlying causal mechanisms. Although only indicative, we can at least choose parameter values that are consistent with the available evidence.

Lefgren et al. (2012) examine the relative importance of different mechanisms in a transmission framework that is similar to ours. Using imperfect instruments that are differentially correlated with parental human capital and income they estimate that in Sweden the effect from parental income (captured by the parameter γ) explains about a third of the intergenerational elasticity, while parental human capital explains the remaining two thirds. In our model we further distinguish between a direct and indirect (through human capital accumulation) effect from parental income, as captured by the parameters γ_y and γ_h, but the total

[44] Conlisk (1974b) derives stability conditions in a random coefficients model with repeated shocks.

[45] For example, if $\boldsymbol{\Lambda}_2$ is diagonal and elements of the endowment vector $\boldsymbol{e}_t$ are uncorrelated then the diagonal elements of $\boldsymbol{\Lambda}_2$ are required to be strictly between zero and one.

effect is sufficient for the parameterization of our examples.

The literature provides more guidance on the transmission of physical traits such as height or cognitive and non-cognitive abilities, for which we use the term *endowments*. Common to these are that genetic inheritance is expected to play a relatively important role. From the classic work of Galton to more recent studies the evidence implies intergenerational correlations in the order of magnitude of about 0.3-0.4 when considering one and much higher correlations when considering both parents.[46] Those estimates may reflect to various degrees not only genetic inheritance but also correlated environmental factors; we capture both in the *heritability* parameter λ (estimates of genetic transmission are then a lower bound), for which values in the range 0.5-0.8 seem reasonable. Note that we use the term "heritability" in a broad sense, while the term refers only to genetic inheritance in the biological literature.

Finally, a reasonable lower-bound estimate of the *returns* ρ to endowments and *human capital* can be approximated by evidence on the explanatory power of earnings equations. Studies that observe richer sets of covariates, including measures of cognitive and non-cognitive ability, typically yield estimates of R^2 in the neighborhood of 0.40.[47] On the one hand, such estimates are likely to underestimate the explanatory power of (broadly defined) human capital as of imperfect measurement and omitted variables. On the other hand, we want to only capture returns to the component of human capital that is not due to parental income and investment; we capture the latter channel instead in the parameter γ_h (and its contribution to offspring income in γ). In any case, values of ρ in the range of 0.6-0.8 should be at least roughly consistent with the empirical evidence.[48]

These parameter ranges are consistent with recent estimates of the intergenerational income elasticity β in the US, which are typically in the range of 0.45-0.55 (see Black and Devereux, 2011). Given reliable elasticity estimates we can also cross-validate and potentially narrow down the implied range for the structural parameters of the model. We write each parameter as a function of the others in steady state,

$$\begin{aligned} \beta &= \gamma + \frac{\rho^2\lambda}{1-\gamma\lambda} \qquad & \gamma &= \frac{\beta\lambda + 1 \pm \sqrt{\beta^2\lambda^2 - 2\beta\lambda + 4\lambda^2\rho^2 + 1}}{2\lambda} \\ \rho &= \sqrt{\frac{(\beta-\gamma)(1-\gamma\lambda)}{\lambda}} \qquad & \lambda &= \frac{\beta-\gamma}{\beta\gamma + \rho^2 - \gamma^2}, \end{aligned} \tag{45}$$

and plug in the discussed values on the right-hand sides to impute parameter ranges that

[46]For estimates of correlations in measures of cognitive ability, see Bowles and Gintis (2002) and the studies they cite; for measures of both cognitive ability and non-cognitive ability, see Grönqvist et al. (2010).

[47]See for example Lindqvist and Vestman (2011) for Sweden. Fixed-effects models yield higher estimates, although some of the difference may be capturing persistent luck rather than unobserved characteristics.

[48]In the initial steady state we standardize $Var(y) = Var(e) = 1$, such that $R^2 = 0.4$ translates into $\rho \approx 0.63$.

are consistent with our reading of the empirical literature. Specifically we rule out too high values of λ and ρ as they cause γ to approach zero, to arrive at

$$0.45 \leq \beta \leq 0.55, \quad 0.15 \leq \gamma \leq 0.25, \quad 0.60 \leq \rho \leq 0.70, \quad 0.50 \leq \lambda \leq 0.65.$$

These implied ranges should not be taken literally, but are sufficient to provide a reasonable illustration of the potential quantitative implications of our findings.

A.4 Correlated endowments

We revisit example 3 under the assumption that $\boldsymbol{\Lambda}_t$ is not diagonal, such that elements of the endowment vector $\boldsymbol{e}_t$ are potentially correlated. Suppose that at generation T the returns to human capital change from $\boldsymbol{\rho}_1$ to $\boldsymbol{\rho}_2$ but that the steady-state variance of income remains unchanged.

By substituting equation (5) for y_{T-1} and income in previous generations we can express the pre-shock elasticity as

$$\beta_{T-1} = Cov(y_{T-1}, y_{T-2}) = \gamma + \boldsymbol{\rho}_1' Cov(\boldsymbol{e}_{T-1}, y_{T-2}) = \gamma + \boldsymbol{\rho}_1' \boldsymbol{\Gamma} \boldsymbol{\rho}_1 \tag{46}$$

where

$$\boldsymbol{\Gamma} = \sum_{l=1}^{\infty} \gamma^{l-1} Cov(\boldsymbol{e}_{T-1}, \boldsymbol{e}_{T-1-l}) \tag{47}$$

is the cross-covariance between the endowment vectors of offspring and parents (if $\gamma = 0$), or a weighted average of the endowment vectors of parents and earlier ancestors ($0 < \gamma < 1$). These cross-covariances measure to what degree each offspring endowment is correlated with the *same* endowment in previous generations (the diagonal elements) and each of the *other* $K-1$ endowments (the off-diagonal elements). Note that $\boldsymbol{\Gamma}$ does not depend on t if these cross-covariances are in steady state.

We can similarly derive the elasticity in the first affected generation and in the new steady state as

$$\beta_T = \gamma + \boldsymbol{\rho}_2' \boldsymbol{\Gamma} \boldsymbol{\rho}_1 \tag{48}$$

$$\beta_{t\to\infty} = \gamma + \boldsymbol{\rho}_2' \boldsymbol{\Gamma} \boldsymbol{\rho}_2. \tag{49}$$

The conditions under which a change in skill prices leads to a non-monotonic response in mobility can be easily summarized if the cross-covariances $Cov(\boldsymbol{e}_{T-1}, \boldsymbol{e}_{T-j})\ \forall j > 1$ are symmetric. Symmetry requires the correlation between offspring endowment k and parent endowment l to be as strong as the correlation between offspring endowment l and parent endowment k, $\forall k, l$. We can then note that

$$\begin{aligned}
2\beta_T &= 2\left(\gamma + \boldsymbol{\rho}_2'\boldsymbol{\Gamma}\boldsymbol{\rho}_1\right) \\
&= \gamma + \boldsymbol{\rho}_1'\boldsymbol{\Gamma}\boldsymbol{\rho}_1 + (\boldsymbol{\rho}_2' - \boldsymbol{\rho}_1')\boldsymbol{\Gamma}\boldsymbol{\rho}_1 + \gamma + \boldsymbol{\rho}_2'\boldsymbol{\Gamma}\boldsymbol{\rho}_2 + \boldsymbol{\rho}_2'\boldsymbol{\Gamma}(\boldsymbol{\rho}_1 - \boldsymbol{\rho}_2) \\
&= \beta_{T-1} + \beta_{t\to\infty} + (\boldsymbol{\rho}_2' - \boldsymbol{\rho}_1')\boldsymbol{\Gamma}\boldsymbol{\rho}_1 - \boldsymbol{\rho}_2'\boldsymbol{\Gamma}(\boldsymbol{\rho}_2 - \boldsymbol{\rho}_1) \\
&= \beta_{T-1} + \beta_{t\to\infty} - (\boldsymbol{\rho}_2' - \boldsymbol{\rho}_1')'\boldsymbol{\Gamma}(\boldsymbol{\rho}_2 - \boldsymbol{\rho}_1), \qquad (50)
\end{aligned}$$

where we expanded and subtracted $\boldsymbol{\rho}_1'$ and $\boldsymbol{\rho}_2$, substituted equations (46) and (49), and finally took the transpose and used the symmetry of $\boldsymbol{\Gamma}$ to collect all remaining terms in a quadratic form.

Let S denote the subset of prices that do not change in generation T, and denote by $\boldsymbol{\Gamma}_S$ and $\boldsymbol{\Lambda}_S$ the minors of $\boldsymbol{\Gamma}$ and $\boldsymbol{\Lambda}$ that are formed by deleting each row and column that correspond to an element in S. The quadratic form $(\boldsymbol{\rho}_2' - \boldsymbol{\rho}_1')'\boldsymbol{\Gamma}(\boldsymbol{\rho}_2 - \boldsymbol{\rho}_1)$ is greater than zero for $\boldsymbol{\rho}_2 \neq \boldsymbol{\rho}_1$ if $\boldsymbol{\Gamma}_S$ is positive definite. A sufficient condition for $\boldsymbol{\Gamma}_S$ to be positive definite is diagonality of the heritability matrix $\boldsymbol{\Lambda}_S$, with positive diagonal elements. More generally, the matrix $\boldsymbol{\Gamma}_S$ is positive definite if the respective minors of the cross-covariances $Cov(\boldsymbol{e}_{T-1}, \boldsymbol{e}_{T-j})\ \forall j > 1$ are strictly diagonally dominant. Strict diagonal dominance requires that the correlation between offspring endowment k and parent endowment k is stronger than the sum of its correlation to all other relevant parent endowments $l \neq k, l \in S$ (i.e., offspring are similar instead of dissimilar to their parents).

Price changes then increase intergenerational mobility temporarily (β_T is below *both* the previous steady state β_{T-1} and the new steady state $\beta_{t\to\infty}$) as long as the steady-state elasticity shifts not too strongly, specifically iff

$$|\beta_{t\to\infty} - \beta_{T-1}| < (\boldsymbol{\rho}_2' - \boldsymbol{\rho}_1')\boldsymbol{\Lambda}\left(\boldsymbol{I} - \gamma\boldsymbol{\Lambda}\right)^{-1}(\boldsymbol{\rho}_2 - \boldsymbol{\rho}_1). \qquad (51)$$