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Understanding Intergenerational Mobility

Inequality, Student Aid and Nature-Nurture Interactions

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IFAU
INSTITUTE FOR EVALUATION OF LABOUR MARKET AND EDUCATION POLICY

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Abstract
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Essay I: A body of evidence has emerged in the literature on intergenerational mobility documenting that unequal countries experience less social mobility: a relationship known as the Great Gatsby Curve. In this paper I estimate the Great Gatsby Curve within Sweden across 125 commuting zones and 20 cohorts, exploiting both cross-sectional and longitudinal variation. I find that children who were exposed to higher levels of inequality during childhood experienced less social mobility as adults, thereby confirming the existence of a Great Gatsby Curve in Sweden. I also present new evidence on the underlying mechanisms of the Great Gatsby Curve. By decomposing intergenerational mobility into separate transmission channels, I find that the Great Gatsby Curve is exclusively driven by the mediating effect that children's educational attainment and development of cognitive and non-cognitive skills has on the persistence of income across generations. Hence, the results suggest that adverse effects of inequality on mobility can be alleviated by policies that target children's educational attainment and development of cognitive and non-cognitive skills.

Essay II: The causal effects of student aid on educational attainment and subsequent labor market outcomes is estimated by exploiting the repeal of the Recruitment Grant in 2006 in a difference-in-differences framework. The purpose of the Recruitment Grant was to increase enrollment in adult education among unemployed adults with incomplete upper secondary education, and thereby improve their prospects on the labor market. The grant replaced the loans in the national student aid system, and as such offers an opportunity to study the effects of student aid when credits constraints are absent. I find that the repeal of the Recruitment Grant reduced enrollment in adult education by 10 percent in the target population relative to the pre-treatment enrollment rate, and that the number of passed credits decreased by 28 percent. In terms of labor market outcomes, the repeal increased the unemployment rate by 3.2 percentage points in the target population in 2008, and by 2.1 percentage points in 2009. Focusing on long term outcomes, I find that the repeal decreased average labor market income between 2012 and 2014 by about $280 while increasing the number of days in unemployment by 27.2 days in the same period. In sum, the repeal of the Recruitment Grant had sizable adverse effects for the target population.

Essay III (with Mikael Lindahl and Björn Öckert): This paper provides evidence on the importance of nature-nurture interactions for socio-economic outcomes, using administrative data on adopted children and their adoptive and biological parents. We study a large sample of adoptees born in Sweden 1932-1970, and use the education and income of the biological and adoptive parents as proxies for pre-birth and post-birth factors, respectively. The estimated interaction effects are typically non-positive and small: they account for around 5–10 percent of the overall intergenerational transmission. However, the interaction effects between pre-birth and post-birth factors are statistically significant and negative for educational attainment for sons and for the earlier cohorts. We find similar results if we instead treat children’s genetic and environmental background as unobserved latent variables. Thus, we do not find that a poor upbringing exacerbates any genetic disadvantages. Instead, a favorable family environment is likely to improve life chances for everyone, regardless of their genetic predisposition.

Keywords: Intergenerational mobility, inequality, student aid, adult education, difference-in-differences, propensity scores

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While studying at the Ph.D. program here in Uppsala I have had the opportunity to spend my time both at the Department of Economics and at the Institute for Evaluation of Labour Market and Education Policy (IFAU), both of which have been stimulating workplaces. I therefore want to thank my awesome office mates, Lucas at the Department of Economics and Stefano, Dagmar, Mathias and Ulfhild at IFAU, for always having a minute to talk, for putting up with my irregular office hours and for providing a familial atmosphere.

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Uppsala, November 2018
Gunnar Brandén

Contents

Introduction

> "All right," said Deep Thought. "The Answer to the Great Question…,"
> "Yes…!"
> "Of life, the Universe and Everything…," said Deep Thought.
> "Yes…!"
> "Is…," said Deep Thought and paused.
> "Yes…!...?"
> "Forty-two" said Deep Thought, with infinite majesty and calm.
> …
> "Forty-two!" yelled Loonquawl. "Is That all you've got to show for seven and a half million years work?"
> "I checked it very thoroughly," said the computer "and that quite definitely is the answer. I think the problem, to be quite honest with you, is that you've never actually known what the question is."
>
> - Douglas Adams[1]
> *The Hitchhiker's Guide to the Galaxy*

I first came across the academic use of the above passage when I studied calculus as an undergraduate student. It appears in the introduction to the chapter on differentiation *in Calculus – a complete course* by Robert A. Adams and Christopher Essex. A few years later, I found the exact same passage quoted in the introductory chapter of *Mostly Harmless Econometrics – an empiricist's companion* by Joshua D. Angrist and Jörn-Steffen Pischke. Seeing as both *Calculus* and *Mostly Harmless Econometrics* have excellent introductions, I figured I'd continue the custom in hopes of a similar achievement. My plan is to first introduce the process of estimating causal effects, and then use the concepts and terminology thus explained to describe the work and findings of the three self-contained essays (chapters) that comprise this thesis.

First of all, in the world of applied microeconomics whenever we talk about causality we almost always mean causality in the *counterfactual* sense: the effect of X on Y *had X not occurred*. To clarify what that means, I'll borrow an example from *Mostly Harmless Econometrics*.

Suppose we are interested in the effect of hospitalization among the elderly on their health. On one hand, exposure to other sick patients might have a negative effect on those whose health is already fragile. On the other hand,

[1] As an economist, I often sympathize with Deep Thought. Coming up answers to difficult questions is essentially what an applied microeconomist is trained to do, but invariably we make mistakes and end up with something just us as frustrating as Deep Thought's 42.

those who are hospitalized receive many valuable health services, and so the net effect of hospitalization is uncertain. To find out truth of the matter, we survey the entire elderly population and ask them to rate their current health status, from 1 = very poor to 5 = very good, and whether they've been hospitalized in the past six months. Suppose the average reported health status is 2.5 among the recently hospitalized, and 3.6 among the non-hospitalized. By comparing these averages, our study suggests that hospitalization has a *negative* effect on health among the elderly. But is this a correct interpretation of the estimate? Not by a mile. The reason is that those who had been hospitalized in the past six months surely had worse health to begin with, which means that we're confusing initial differences in health with the effect of hospitalizations on health.

To obtain a causal estimate of the effect of hospitalization on health, we would ideally like to compare the health status of those who were hospitalized (treated) with their *counterfactual health status* had they not been hospitalized. Obviously, this isn't feasible since we can only observe the same person in either the treated or the non-treated state at a given point in time. Moreover, since this limitation in the powers of our observation generalize to all counterfactual outcomes, estimation of causal effects amounts to an intrinsic missing observation problem where the task is to find a credible substitute for the counterfactual outcome. In the hospitalization example, we implicitly used the health status of those who hadn't been hospitalized to represent the counterfactual outcome of those who had been hospitalized. But we quickly realized that that was a bad idea because those who had been hospitalized were undoubtedly less healthy to begin with.

To resolve the problem of the missing counterfactual, the gold standard in terms of research design is the randomized controlled trial (RCT). In this setting, treatment is *randomly* allocated among participants in the experiment in such a way that the treated and control group is identical to a first approximation. However, RCT's in the social sciences are often not feasible. For example, an RCT in our hospitalization example would imply that we randomly decide whether to allow elderly in need of health care to go to the hospital or force them to take their chances at home. This is of course neither legal or ethical and infeasible, so what do we do? The short answer, which will have to suffice, is that we apply an arsenal of econometric and institutional knowledge to try to come up with a different research design that is both feasible and allows us to construct a control group that credibly represents the counterfactual outcome. The essays in this thesis can be seen as the products of me and my co-authors' attempts to come up with and implement such research designs in order to answer questions that we think are important. The rest of this introduction summarizes the work and findings of these essays.

Inequality and intergenerational mobility

The research questions in the first essay of this thesis can be stated as follows: does regional income inequality have an effect on the socioeconomic mobility of the children that grows up there? If it does, how does it work?

In terms of causal estimation, these research questions are complicated. To obtain a credible substitute for the counterfactual outcomes, one must have access to random variation in the level of regional inequality that children are exposed to during childhood. But since people do not randomly end up where they live, but are guided by social networks, labor markets, preferences and plans for the future, and so on, such random variation doesn't spontaneously arise. Of course, one can imagine an experiment where families are randomly allocated to different regions, but in practice such an experiment is obviously not feasible. Another source of random variation could be an event, such as a natural disaster or structural shock to the economy, that forces families to relocate. However, I haven't been able to detect any events like that on a large enough scale during the period of study (thankfully).[2] Hence, I'm constrained to estimate descriptive associations.

The first part of the essay is devoted to finding out whether the negative association that exists when comparing inequality and mobility across countries – known as The Great Gatsby Curve (see Björklund and Jäntti 1997; Corak, 2006; Andrews and Leigh, 2009; Ermisch et al., 2012; Corak, 2013; Blanden, 2013; Jerrim and Macmillan, 2015) – also exists when comparing across regional units within the same country. There are several reasons why estimates across regions within the same country might differ from cross-country estimates, which can be divided into two main sources of heterogeneity. First, the institutions and conditions that determine the transmission of income from one generation to the next – such as labor markets, taxation, social security, access to health care and education, etc. – varies more across countries than within countries. Second, differences in measurement units and analytic methods further complicates the comparison of mobility and inequality between countries. By studying the relationship between inequality and mobility across regional units within the same country these difficulties are alleviated to the extent that the regions are institutionally and culturally homogeneous.

However, I am not the first to economist to come up with the idea to study The Great Gatsby Curve across regions within the same country. To my knowledge, the first study to estimate the Great Gatsby Curve across regions within the same country is Chetty et al. (2014), who estimates mobility and inequality across commuting zones in the United States. The Great Gatsby

[2] Even if such a large-scale event would've taken place, it's not certain that it would be possible to separate the effects of the event itself from the effect of the change in regional inequality during childhood on future outcomes for the children. Furthermore, it can be argued that differences in inequality during childhood are has a different effect on future outcomes when families are involuntarily exposed to them.

Curve has also been estimated across provinces in China by Fan et al. (2015), across provinces in Italy by Güell et al. (2018), and even across commuting zones in Sweden by Heidrich (2017).

Although the within-country comparisons of China and the United States certainly improve the consistency of statistical measurements and analytic methods, they are still countries with a large cultural heterogeneity and a high degree of legislative autonomy at the regional level. Furthermore, there are some crucial differences between my study and the study by Güell et al. (2018) that are important to mention. First, although they estimate the relationship between inequality and intergenerational mobility in Italy which is an institutionally and culturally rather homogeneous country, their analysis is restricted by the limitations of their data. They only observe income from tax declarations in one year, and cleverly rely on the informational content of the surnames on the forms to estimate mobility. However, since inequality is estimated in the same year as mobility, they estimate the instantaneous relationship between intergenerational mobility and inequality which is conceptually different than estimating the relationship between inequality in the parental generation and its effect on the subsequent intergenerational transmission of income. This last point is also valid for the study by Heidrich (2017) who estimates measures of intergenerational mobility for nine cohorts born between 1968-1976 but only use incomes in 1991 to calculate her inequality metric.[3] Consequently, the relationship between inequality *during childhood* and subsequent rates of intergenerational mobility across homogeneous regions within the same country is uncertain.

I fill this gap in the literature by combining administrative registers to create an income panel spanning 53 years with information on residency and parent-child links that allows for accurate calculations of inequality levels during childhood and approximations of lifetime incomes in consecutive generations born between 1961 and 1980: a feat unparalleled in previous studies. I can therefore estimate the relationship between inequality during childhood and subsequent intergenerational mobility across 125 commuting zones (CZ) and 20 cohorts in Sweden, and thereby exploit both cross-sectional and longitudinal variation.

I find that children who were exposed to higher levels of inequality during childhood experienced less intergenerational mobility as adults. A one standard deviation increase in childhood inequality is associated with a 0.019 increase in intergenerational rank persistence[4], which corresponds to a 7 percent

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[3] It should be noted that the main focus of these papers is not to estimate The Great Gatsby Curve. In particular, the focus in Heidrich (2017) is to investigate regional differences in rates of mobility in the spirit of Chetty et al. (2014), and as such makes an important contribution to the growing literature intergenerational mobility.

[4] The intergenerational rank persistence is based on percentile ranked incomes within cohorts, and measures the correlation between a child and its parent's rank in their respective income distributions. Hence, an intergenerational rank persistence of 0.26 implies that a 1 percentile

increase relative to the average rank persistence of 0.26. To put these numbers into perspective, average childhood inequality in Sweden would have to increase by three standard deviations for the persistence of income ranks across generations to reach the same level as in the United States (Chetty et al., 2014).[5] I also find that inequality is more strongly correlated with mobility at the lower end of the inequality distribution, and that the relationship between inequality and mobility is strongest during the first years of childhood (age -1 to 2).

In the second part of the essay, the goal is to investigate the mechanisms of the Great Gatsby Curve. To do this, I decompose the mobility estimates into four orthogonal transmission channels, following previous work by Rothstein (2017) and Blanden et al. (2007), and investigate their association with childhood inequality. The transmission channels include educational attainment, cognitive skills, non-cognitive skills, and a residual effect that captures the effect of parental income on children's income conditional on educational attainment and skills.

I find that the mediating effect of children's educational attainment and development of cognitive and non-cognitive skills account for about 53 percent of the persistence of income across generations. The remaining persistence is accounted for by the residual effect. However, in spite of the residual effect accounting for almost half of the total persistence, it is completely uncorrelated with childhood inequality. In contrast, all three mediation effects are positively correlated with childhood inequality: a one standard deviation increase in childhood inequality is associated with a 0.22 standard deviation increase in the mediation effect of children's educational attainment on the intergenerational rank persistence, and a 0.26 and 0.28 standard deviation increase in the mediation effect of children's cognitive and non-cognitive skills. Hence, the results suggest that children who grew up in regions with high levels of inequality experienced less social mobility because their parents' income had a stronger effect on their educational attainment and development of cognitive and non-cognitive skills, but not a stronger direct effect on their income.

To summarize, this essay shows that children who grew up in the 70's and 80's in regions with high levels of income inequality experienced less socioeconomic mobility as adults. The essay also shows that this relationship is entirely mediated by children's educational attainment and development of cognitive and non-cognitive skills.

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increase in parental income rank is associated with an expected increase of 0.26 of the child's income rank.

[5] Chetty et al. (2014) estimates the U.S. rank persistence to 0.317 for sons born 1980-1982. See the second column of Table 1.

The effects of replacing student loans with grants

In this essay, I estimate the causal effect of student aid on educational attainment and subsequent labor market outcomes by exploiting the repeal of the Recruitment Grant in 2006 in a difference-in-differences (DD) framework. The idea behind a DD model is to construct a control group whose *outcome trend* is an accurate representation of the counterfactual outcome trend of the treated group. Another way to state this idea is that omitted variables must be either time-invariant group attributes, or time-varying factors that are group invariant.

The Recruitment Grant replaced the loans in the regular student aid with grants and was offered to unemployed adults aged 25-50 who had not completed their upper secondary education, and was restricted to studies at the compulsory or upper secondary levels (Komvux) for a maximum of one year. Furthermore, the recipients were not allowed to have received any other form of student aid in the past five years.

There is a large empirical literature on how student aid affects educational attainment, and it has generally found that student aid has a positive effect on college completion while reducing drop-out rates and retention (see Van der Klaauw, 2002; Dynarski, 2003; Dynarski, 2008; Bettinger, 2004; Goodman, 2008; Angrist et al., 2009; Glocker, 2011). However, eligibility for student aid typically selects on academic merit and/or financial need and therefore, the effect of student aid is often identified locally at the upper end of the skill distribution among recent high school graduates with a financially disadvantaged family background. My study thereby contributes to this literature by studying the effect of student aid on a subset of the population that is rarely featured - adults with incomplete upper secondary schooling. Furthermore, the empirical literature on student aid is almost exclusively focused on enrollment decisions at the college level, whereas the Recruitment Grant was offered for studies at the compulsory and upper secondary level.

In addition, there is also a large literature on financial decision making that point to the possibility that standard economic theory is too simplistic to explain the relationship between student aid and school enrollment. For example, Bettinger et al. (2012) found that providing application assistance to low-income individuals increased aid application rates and college attendance, while Caetano et al. (2011) found that labeling a contract as a “loan” reduced the probability of it being accepted by 8 percent compared to a financially equivalent contract. In contrast, recipients of the Recruitment Grant did not have to accumulate debt and were actively recruited, informed, and assisted with the application procedure by officials at the municipal level. Hence, my study makes an empirical contribution to the literature on financial decision making by analyzing the impact of a student aid reform that not only had a financial component but also entailed efforts to overcome obstacles to rational decision making.

To construct a control group whose outcome trend is an accurate representation of the counterfactual outcome of those who were offered the grant, I create a sample by selecting from the data all individuals aged 25-50 in 2006 and 2007 with incomplete upper secondary education who are unemployed and did not receive any form of student aid in the previous year. I then define as controls those who received some form of student aid in the past five years and therefore were not eligible for the grant, and conversely defined as treated those who were eligible for the grant. Consequently, the key identifying assumption in the study is that the outcome trends for those in the sample who received student aid in the past two to five years is an accurate representation of the counterfactual outcome trends for those who were eligible for the Recruitment Grant. The DD estimate is then defined as the difference between the treated and control group in 2007 minus the difference between the treated and control group in 2006.

I find that the repeal of the Recruitment Grant reduced enrollment in adult education and increased the unemployment rate in the target population. Focusing on long term outcomes, I also find that the repeal decreased subsequent labor market incomes and increased the number of days in unemployment. The repeal of the Recruitment Grant thus had sizable adverse effects on educational attainment and subsequent labor market outcomes for the target population.

The importance of nature-nurture interactions

This essay is co-authored with Mikael Lindahl and Björn Öckert, and our research question is whether gene-environment interactions are quantitatively important in explaining inequality transmission between generations. In other words, does the environment exacerbate or narrow “genetic inequality”? An ideal experiment to answer this question would be to randomly assign children to family environments, which of course isn’t feasible. Instead, we study adoptees who thereby have been assigned to a random family environment in some sense, and approximate pre-birth (i.e. nature) inputs by using data on the biological parents and post-birth (i.e. nurture) inputs by using data on the adoptive parents. Hence, since we cannot directly observe genetic endowments or all aspects of the rearing environment, we must limit ourselves to estimating descriptive associations.

Nevertheless, the significance of gene-environment interactions has important implications for how to optimally design policies. If environmental inputs can compensate for initial differences in genetic endowments, targeting interventions toward the disadvantaged can increase equality of opportunity and at the same time be an efficient way to raise productivity. But if environmental factors tend to exacerbate initial genetic differences, public policies would face a major trade-off between equity and efficiency.

There is a vast inter-disciplinary literature on the importance of gene-environment interactions. For example, studies using adoption and twin designs have shown that gene-environment interactions can be important for outcomes such as development of mental disorders and alcoholism (Rutter et al, 2006). Several studies have also found that IQ is more transmissible at the upper end of the socioeconomic status (SES) distribution (Rowe et al, 1999; Scarr-Salapatek, 1971; Turkheimer et al., 2003). The pioneering studies on gene-environmental interactions using environmental factors and genetic markers are Caspi et al. (2002, 2003) that found evidence of negative interaction effects for antisocial behavior. They used information on specific genes important for this outcome and information on maltreatment in the family. However, interacting genetic markers with environmental conditions can generate interaction estimates that are difficult to interpret. If the environmental factors are not randomly determined, interaction effects may just reflect the fact that the environment is better for those with a positive genetic predisposition for some outcome.

We contribute to the literature on nature-nurture interactions in several ways. In this study, we use a large sample of adoptees that makes it possible to focus on the biological fathers of the adopted children, which provide a cleaner measure of genetic endowment than the biological mother since it is less contaminated by the prenatal environment. We can also study changes over time, where a hypothesis is that the possibility for environmental interventions to narrow genetic inequality possibly has decreased during the end of the period when the Swedish welfare state changed focus as several important reforms designed to decrease inequality of opportunity (e.g., in education) already had been implemented. Finally, we estimate separate associations for daughters and sons, which is of particular interest since there is evidence that boys are more sensitive to negative environmental shocks than girls (see Bohman et al., 1981; Cloninger et al., 1981; Krein and Beller, 1988).

We use a data set based on all adoptees born in Sweden between 1932 and 1970 and their biological and adoptive parents. It is compiled from several Swedish registers and contains information on educational attainment, income and cognitive and non-cognitive results from military tests and evaluations for males born between 1951-1970.

We find that the estimated interaction effects are typically non-positive and small: they account for around 5-10 percent of the overall intergenerational transmission. However, the interaction effects between pre-birth and post-birth factors are statistically significant and negative for educational attainment for sons and for the earlier cohorts. Thus, we do not find that a poor upbringing exacerbates genetic disadvantages. Instead, a favorable family environment is likely to improve life chances for everyone, regardless of their genetic predisposition.

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I. Does inequality reduce mobility? The Great Gatsby Curve and its mechanisms

1. Introduction

Income inequality has been increasing in OECD countries since the 1980's (OECD, 2011). In the wake of that development, concerns have been raised about the adverse effects of inequality on social mobility, as expressed by Alan B. Krueger in a speech to the Center for American Progress:

> "Support for equality of opportunity should be a nonpartisan issue. It is hard not to bemoan the fact that because of rising inequality the happenstance of having been born to poor parents makes it harder to climb the ladder of economic success. There is a cost to the economy and society if children from low income families do not have anything close to the opportunities to develop and use their talents as the more fortunate kin from better off families who can attend better schools, receive college prep tutoring, and draw on a network of family connections in the job market." (Krueger, 2012)

Krueger, building on previous work by Corak (2006), presented a scatter plot of the relationship between inequality and the intergenerational elasticity of income (IGE) across countries. The data points clustered along an upward sloping line indicating that unequal societies experience less social mobility – a relationship he called the Great Gatsby Curve.

An early reference on the relationship between inequality and mobility is Björklund and Jäntti (1997) who compares Sweden and the United States.[6] Since then, a number of studies have corroborated the finding that countries with high levels of inequality experience less intergenerational mobility (Corak, 2006; Andrews and Leigh, 2009; Ermisch et al., 2012; Corak, 2013; Blanden, 2013; Jerrim and Macmillan, 2015). But does the Great Gatsby Curve also exist across regions within the same country?

There are two main sources of heterogeneity which suggests that estimates across regions within the same country can differ from cross-country estimates. First, the institutions and conditions that determine the transmission of income from one generation to the next - such as labor market institutions, taxation policies, social security, access to and quality of education and health care, marital sorting, segregation, cultural norms, etc. - undoubtedly varies more across countries than within countries. Second, differences in measurement units, analytic methods, income definitions, sample frames, inequality metrics and mobility statistics further complicates the comparison of mobility and inequality between countries. But by studying the relationship between inequality and mobility across regional units within the same country these difficulties are greatly mitigated, more so to the extent that the regions exhibit a high degree of institutional and cultural homogeneity.

[6] The paper by Björklund and Jäntti (1997) was preceded by empirical work in the sociological literature (see Erikson and Goldthorpe, 1992).

To my knowledge, the first study to estimate the Great Gatsby Curve across regions within the same country is Chetty et al. (2014), who estimates mobility and inequality across commuting zones in the United States. The Great Gatsby Curve has also been estimated across provinces in China by Fan et al. (2015), across provinces in Italy by Güell et al. (2018), and across commuting zones in Sweden by Heidrich (2017). Although the within-country comparisons of China and the United States certainly improve the consistency of statistical measurements and analytic methods, they are still countries with a large cultural heterogeneity and a high degree of legislative autonomy at the regional level. Furthermore, there are some crucial differences between this study and the study by Güell et al. (2018) that are important to mention. First, although they estimate the relationship between inequality and intergenerational mobility in Italy which is an institutionally and culturally rather homogeneous country, their analysis is restricted by the limitations of their data. They only observe income from tax declarations in one year, and cleverly rely on the informational content of the surnames on the forms to estimate mobility. However, since inequality is estimated in the same year as mobility, they estimate the instantaneous relationship between intergenerational mobility and inequality which is conceptually different than estimating the relationship between inequality in the parents' generation and its effect on the subsequent intergenerational transmission of income for the child generation. This last point is also valid for the study by Heidrich (2017) who estimates measures of intergenerational mobility for nine cohorts born between 1968-1976 in Sweden, but only use incomes in 1991 to calculate her inequality metric. Consequently, the relationship between inequality *during childhood* and subsequent rates of intergenerational mobility across *homogeneous regions* within the same country remain uncertain.

I fill this gap in the literature by combining administrative registers to create an income panel spanning 53 years with information on residency and parent-child links that allows for accurate calculations of inequality levels during childhood and approximations of lifetime incomes in consecutive generations born between 1961 and 1980: a feat unparalleled in previous studies. I can therefore estimate the relationship between inequality during childhood and subsequent intergenerational mobility across 125 commuting zones (CZ) and 20 cohorts in Sweden, and thereby exploit both cross-sectional and longitudinal variation.

The second aim of this study is to investigate the mechanisms of the Great Gatsby Curve. To do this, I decompose the mobility estimates into four orthogonal transmission channels and investigate their association with childhood inequality. The transmission channels include educational attainment, cognitive skills, non-cognitive skills, and a residual effect that captures the effect of parental income on children's income conditional on educational attainment and skills, such as the direct effect of parental income and the effect of social networks, hereditary traits, etc.

To obtain robust measures of intergenerational mobility, I use lifetime incomes that are percentile ranked within cohorts at the national level (Nybom and Stuhler, 2016). Estimates of intergenerational mobility are then obtained as slope coefficients from bivariate regressions of children's income ranks on father's income ranks, hereafter referred to as the intergenerational rank persistence (IRP).[7] To measure the level of inequality that each cohort is exposed to prior to labor market entry, I average annual Gini coefficients between ages -1 and 18 for each cohort. The Great Gatsby Curve is then estimated by regressing intergenerational mobility on childhood inequality at the CZ by cohort level.

I find that children who were exposed to higher levels of inequality during childhood experienced less intergenerational mobility as adults, and that this holds true whether comparing children who grew up in the same commuting zones but were born in different years, or whether comparing children who were born in the same year but grew up in different commuting zones. A one standard deviation increase in childhood inequality is associated with a 0.019 increase in IRP, which corresponds to a 7 percent increase relative to the average rank persistence at 0.26. Taking these estimates at face value, average childhood inequality in Sweden would have to increase by three standard deviations for the persistence of income ranks across generations to reach the same level as in the United States (Chetty et al., 2014).[8] I also find that inequality is more strongly correlated with mobility at the lower end of the inequality distribution, and that the relationship between inequality and mobility is strongest during the first years of childhood (age -1 to 2).

Turning to the mechanisms, I find that the mediating effect of children's educational attainment and development of cognitive and non-cognitive skills account for about 53 percent of the persistence of income across generations. The remaining persistence is accounted for by the residual effect. However, in spite of the residual effect accounting for almost half of the total persistence, it is completely uncorrelated with childhood inequality. In contrast, all three mediation effects are positively correlated with childhood inequality: a one standard deviation increase in childhood inequality is associated with a 0.22 standard deviation increase in the mediation effect of children's educational attainment on the intergenerational rank persistence, and a 0.26 and 0.28 standard deviation increase in the mediation effect of children's cognitive and non-cognitive skills. Hence, the results suggest that the Great Gatsby Curve is entirely driven by the mediating effect that children's educational attainment and development of cognitive and non-cognitive skills has on intergenera-

[7] I refer interchangeably to "mobility" and "persistence" in the paper, where the former is understood to have an inverse relationship to the latter.

[8] Chetty et al. (2014) estimates the U.S. rank persistence to 0.317 for sons born 1980-1982. See the second column of Table 1.

tional mobility. Another way of putting it is that children who grew up in regions (or cohorts) with high levels of inequality experienced less social mobility because their parent's income had a stronger effect on their educational attainment and development of cognitive and non-cognitive skills (and/or the returns to them), but not a stronger direct effect on their income.

Thus, this paper makes two significant contributions to the existing literature on inequality and mobility. First, by estimating the Great Gatsby Curve across regions with an exceptionally high level of institutional and cultural homogeneity, using both spatial and temporal variation by combining administrative registers to create an income panel spanning 53 years with information on residency and parent-child links that allows for accurate approximations of lifetime incomes in consecutive generations for 20 cohorts: a feat unparalleled in previous studies of the relationship between mobility and inequality. The second contribution is to study the mechanisms of the Great Gatsby Curve by decomposing the transmission of income between generations into separate channels using data on educational achievement and cognitive and non-cognitive skills.

The paper proceeds as follows. I present a theoretical framework for the transmission of income across generations in section 2. Section 3 describes the estimation, while the data and sample selection is covered in Section 4. Estimates of the Great Gatsby Curve is presented in section 5, and section 6 presents results on the underlying mechanisms. Robustness checks are presented in section 7, and section 8 concludes.

2. Theoretical framework

There are strong theoretical underpinnings for the Great Gatsby Curve which dates back to seminal papers by Gary Becker and Nigel Tomes (1979; 1986). They establish a link between cross-sectional inequality for the parent generation and the subsequent intergenerational persistence of income by formulating a model where the utility optimizing behavior of families means that parents invest more in the human capital of their children when the returns to those investments are high; i.e. when inequality is high. Since rich parents can afford to invest more in the human capital of their children, increasing levels of inequality in the Becker-Tomes model implies that the intergenerational persistence of income will increase.

Solon (2004) extends the Becker-Tomes model in a way that rationalizes the intergenerational elasticity of income and allows for an analysis of the impact of public investments in children’s human capital. In Solon’s model inequality is decreasing in the progressiveness of public investments in children’s human capital, and increasing in the heritability of biological endowments, the returns to human capital investments, and the earnings return to human capital.

Meanwhile, mobility is increasing in the progressiveness of public investments and decreasing in the heritability of biological endowments, the returns to human capital investments, and the earnings return to human capital. Hence that model also predicts a negative correlation between inequality and mobility.[9]

In this section, I present a theoretical framework that closely follows prior work by Rothstein (2017) and Blanden et al. (2007) to fix ideas about how income is transmitted across generations. Admittedly, this theoretical framework abstracts from much of the theoretical richness in the Becker-Tomes model and its extensions.

2.1 Transmission of income across generations

Let regions be indexed by r and cohorts by t. Then suppose that lifetime income, y, is determined by separate processes in two periods indexed by subscripts 1 and 2. In the first period, a vector of income-generating skills, $\boldsymbol{a}_{1rt}$, is acquired prior to labor market entry as a function of parental lifetime income, y^p:

$$\boldsymbol{a}_{1\mathrm{rt}} = \boldsymbol{g}_{1\mathrm{rt}}(y^p) \tag{1}$$

Here, $\boldsymbol{g}_{1\mathrm{rt}}$ reflects the institutions and conditions that govern the transmission of parental income into children's production of income-generating skills in each region r for each cohort t. Examples of such institutions and conditions range from crime rates, segregation and unemployment, to the quality of and access to education and healthcare. In the next period, income y_{rt} is determined by the acquired skills in the previous period and parental income, again mediated by the regional institutions and conditions at the time:

$$y_{rt} = f_{2rt}(\ _{1rt}, y^p) \tag{2}$$

The reduced form relationship of income across generations can then be expressed as:

$$y_{rt} = f_{2rt}(\boldsymbol{g}_{1\mathrm{rt}}(y^p), y^p) \tag{3}$$

[9] The impact of public investments on intergenerational mobility and cross-sectional inequality is further elaborated upon in a recent model by Becker et al. (2018). In that model, parental human capital and parental investments are complementary, thereby incorporating the very plausible notion that parents with high levels of human capital are better at investing in their children's human capital. Their model thus predicts that the impact of public investments on intergenerational mobility will depend upon if those investments substitute or complement parental investments.

Hence, the intergenerational persistence of income is defined as:

$$\frac{dy_{rt}}{dy^{p}} = \frac{\partial f_{2rt}}{\partial g_{1rt}} * \frac{\partial g_{1rt}}{\partial y^{p}} + \frac{\partial f_{2rt}}{\partial y^{p}} \tag{4}$$

The first term captures the effect of parental income on children's development of skills as mediated by the regional institutions and conditions at the time, multiplied by the effect of skills on income (again as mediated by the regional institutions and conditions at the time). A large effect of this term suggest that parental income mainly affects children's income by investments in their income-generating skills. The second term captures the conditional effect of parental income on children's income, and a large effect of this term implies that parental income either has a large direct effect on children's income, or that parental income affects children's income through channels not captured by income-generating skills. Examples of such channels could be access to social networks that facilitate success in the labor market, or hereditary traits such as good looks and skin-tone.

2.2 Mechanisms and the mobility measure

To see how the transmission mechanisms derived in this framework relates to the standard measure of intergenerational mobility, I will assume for the moment that skill is uni-dimensional and that g_{1rt} and f_{2rt} are linear functions with errors that are uncorrelated with parental income:

$$a_{1rt} = g_{1rt}(y^{p}) = \kappa_{1rt} + \varphi_{1rt}y^{p} + \mu_{1rt} \tag{5}$$
$$y_{rt} = f_{2rt}(a_{1rt}, y^{p}) = \kappa_{2rt} + \rho_{2rt}a_{1rt} + \delta_{rt}y^{p} + \mu_{2rt} \tag{6}$$

Then the reduced form relationship of income across generations can be expressed as:

$$\begin{aligned} y_{rt} &= \kappa_{2rt} + \rho_{2rt}(\kappa_{1} + \varphi_{1rt}y^{p} + \mu_{1rt}) + \delta_{rt}y^{p} + \mu_{2rt} = \\ &\kappa_{2rt} + \rho_{2rt}\mu_{1rt} + \rho_{2rt}\kappa_{1rt} + (\rho_{2rt}\varphi_{1rt} + \delta_{rt})y^{p} + \mu_{2rt} \end{aligned} \tag{7}$$

And the intergenerational persistence of income as:

$$\frac{dy_{rt}}{dy^{p}} = \rho_{2rt}\varphi_{1rt} + \delta_{rt} \tag{8}$$

The returns to the uni-dimensional income-generating skill is captured by ρ_{2rt} and the effect of parental income on the production of the skill is captured by φ_{1rt}, analogous to $(\partial f_{2rt}/\partial a_{1rt}) * (\partial g_{1rt}/\partial y^{p})$ in Equation (4), while δ_{rt} reflects the conditional effect of parental income on children's income analogous to $(\partial f_{2rt}/\, \partial y^{p})$ in Equation (4).

The standard measure of intergenerational income persistence is generally obtained as the OLS estimate of the slope coefficient from regressing parental income on children's income:

$$y = \alpha + \beta y^{p} + \epsilon \tag{9}$$

Assuming a sample of *n* individuals and their parents, the probability limit of the OLS estimator of β as $n \to \infty$ is:

$$\text{plim}\hat{\beta} = \frac{\text{Cov}(y, y^{p})}{V(y^{p})} = \beta + \frac{\text{Cov}(\varepsilon, y^{p})}{V(y^{p})} \tag{10}$$

Here, β is the causal effect of parental income on children's income and the last term, $\frac{Cov(\varepsilon, y^{p})}{V(y^{p})}$, accounts for all other channels that causes income to persist across generations including genetic endowments, social networks and so on. Therefore, $\hat{\beta}$ should be understood as a descriptive measure that incorporates the combined influence of all variables that are correlated with y^{p} and y in in addition to any causal effect.

The standard measure of intergenerational income persistence can be expressed in terms of the framework above as:

$$\begin{aligned} y_{rt} &= \alpha_{1rt} + \beta_{rt} y^{p} + \varepsilon^{*}_{rt} \\ \varepsilon^{*}_{rt} &= \rho_{2rt} a_{1rt} + \mu_{2rt} \end{aligned} \tag{11}$$

where Equation (11) is estimated separately for each cohort in each region. The probability limit of the OLS estimator of β_{rt} is then:

$$\begin{aligned} \text{plim}\hat{\beta} &= \frac{\text{Cov}(\alpha_{rt} + \beta_{rt} y^{p} + \varepsilon^{*}_{rt}, y^{p})}{V(y^{p})} \\ &= \beta_{rt} + \frac{\text{Cov}(\rho_{2rt}(\kappa_{1rt} + \varphi_{1rt} y^{p} + \mu_{1rt}) + \mu_{2rt}, y^{p})}{V(y^{p})} \\ &= \beta_{rt} + \rho_{2rt}\varphi_{1rt} \end{aligned} \tag{12}$$

Recognizing that $\beta_{rt} = \delta_{rt}$ assuming the model is correctly specified, we can see that the sum of the transmission coefficients is equal to the standard measure of intergenerational income persistence, $\hat{\beta}_{rt}$, which means that the transmission coefficients decompose $\hat{\beta}_{rt}$ into orthogonal components that goes through the development of (and returns to) the income-generating vector a_{1rt}, plus the influences that remain in δ_{rt}.

3. Estimation

In this section, I briefly discuss some conceptual differences between common measures of intergenerational mobility before I turn to the estimation of the Great Gatsby Curve. I then describe the decomposition of intergenerational mobility into separate transmission channels and how they're estimated.

3.1 The Great Gatsby Curve

The Great Gatsby Curve is estimated in two steps where the first is to estimate intergenerational mobility and calculate childhood inequality at the CZ by cohort level, and the second is to regress the mobility estimates onto the childhood inequality measures.

However, as pointed out by Chetty et al. (2014), measuring intergenerational income persistence amounts to choosing one out of several statistics that characterize the joint distribution of parent and child income. The most common statistic in the empirical literature has been the intergenerational elasticity of income (IGE), obtained as OLS estimate of the slope coefficient in a regression of children's log lifetime income on parental log lifetime income:

$$\ln(y) = \alpha + \beta \ln(y^p) + \varepsilon \tag{13}$$

A feature of the IGE is that it combines attributes of both the copula and the marginal distributions of parent and child income and will therefore incorporate any changes in inequality across generations. To see how the IGE is related to changes in inequality across generations, recall that the Pearson correlation coefficient is obtained by dividing the covariance between two variables with the product of their standard deviations. Hence the IGE is related to the correlation coefficient through the ratio of the standard deviations of incomes across generations:

$$\beta = \frac{\mathrm{Cov}(y, y^p)}{V(y^p)} = \frac{\mathrm{Cov}(y, y^p)}{SD(y)SD(y^p)}\left(\frac{SD(y)}{SD(y^p)}\right) = \rho\left(\frac{SD(y)}{SD(y^p)}\right) \tag{14}$$

where ρ is the correlation coefficient and β is the IGE. Therefore, increasing inequality across generations will inflate the IGE relative to the correlation. On the other hand, the intergenerational rank persistence (IRP) suggested by Dahl and DeLeire (2008) depends only on the copula (Chetty et al., 2014) since the percentile ranking of lifetime incomes transforms the marginal distributions of parent and child income into $\text{Uniform}\left(\frac{1}{2}, \frac{1}{2}\right)$ distributions.[10] So which measure of intergenerational mobility is preferable?

Since the IGE has been shown to be more susceptible to measurement error and life-cycle bias due to the correlation between income trajectories over the life-cycle and total labor market income (Nybom and Stuhler, 2016), the answer is the IRP for the purpose of this study.[11]

So, from now on let y_{irt} denote the lifetime income rank of individual *i* born in year *t* who grew up in commuting zone *r*, and let y_i^p denote the parental lifetime income rank. The intergenerational rank persistence is then obtained as the OLS estimate of the slope coefficient in a regression of children's income rank on parental income rank:

$$y_{irt} = \alpha_{rt} + \beta_{rt} y_i^p + \varepsilon_{irt} \tag{15}$$

where β_{rt} measures the expected change in children's income rank following a one percentile increase in parental income rank within each commuting zone for each cohort. The next step is to regress intergenerational rank persistence on childhood inequality, z_{rt}:

$$\beta_{rt} = \alpha + \theta z_{rt} + \varepsilon_{rt} \tag{16}$$

The slope of the Great Gatsby Curve is captured by θ which measures the expected change in intergenerational rank persistence following a one unit increase in childhood inequality.

3.2 Mediation effects

To estimate the amount of income persistence that is channeled through educational attainment, cognitive skills, and non-cognitive skills, I first estimate their association with parental income ranks from bivariate regressions (separately for each cohort in each CZ):

$$e_{irt} = \psi_{1rt} + \varphi_{1rt} y_i^p + \upsilon_{1irt} \tag{17}$$
$$c_{irt} = \psi_{2rt} + \varphi_{2rt} y_i^p + \upsilon_{2irt} \tag{18}$$

[10] This is not strictly the case at the CZ level since the percentile ranking is done at the national level.

[11] For example, Haider and Solon (2006) showed that income early in life produces a downward-inconsistent estimate of lifetime income, and that income late in life produces an upward-inconsistent estimate (see Böhlmark and Lindquist (2006) for an application to Swedish data). Therefore, the most common way to deal with measurement error and life cycle bias has been to average income over multiple years at points in life when the income trajectories do a good job of approximating lifetime income. The optimal age to measure income seems to be around 32 to 40 years of age for Swedish males (Böhlmark and Lindquist, 2006).

$$n_{irt} = \psi_{3rt} + \varphi_{3rt} y_i^p + \upsilon_{3irt} \tag{19}$$

where e_{irt}, c_{irt} and n_{irt} is the educational attainment, cognitive skills and non-cognitive skills of individual *i* raised in commuting zone *r* in year *t*, and y_i^p is the parental income rank. Hence, estimating Equations (17), (18) and (19) is amounts to estimating $(\partial g_{1rt}/\partial y^p)$ in Equation (4).

The next step is to estimate the conditional returns in a regression that includes the mediating variables as well as parental income rank, which amounts to estimating $(\partial f_{2rt}/\partial a_{1rt})$ and $(\partial f_{2rt}/\partial y^p)$ in Equation (4):

$$\begin{aligned} y_{irt} &= \gamma_{rt} + \rho_{1rt} e_{irt} + \rho_{2rt} c_{irt} + \rho_{3rt} n_{irt} + \delta_{1rt} y_i^p + \upsilon_{irt} \\ &= \gamma_{rt} + (\rho_{1rt}\varphi_{1rt} + \rho_{2rt}\varphi_{2rt} + \rho_{3rt}\varphi_{3rt} + \delta_{1rt}) y_i^p + \upsilon_{irt}^* \\ \upsilon_{irt}^* &= (\psi_{1rt} + \upsilon_{1irt})\rho_{1rt} + (\psi_{2rt} + \upsilon_{2irt})\rho_{2rt} + (\psi_{3rt} + \upsilon_{3irt})\rho_{3rt} + \upsilon_{irt} \end{aligned} \tag{20}$$

Where y_{irt} is the income rank of individual *i*. The intergenerational rank persistence is then:

$$\frac{dy_{irt}}{d_{y_i^p}} = \rho_{1rt}\varphi_{1rt} + \rho_{2rt}\varphi_{2rt} + \rho_{3rt}\varphi_{3rt} + \delta_{1rt} \tag{21}$$

The mediation effect of children's educational attainment is the product of the conditional returns to education, ρ_{1rt}, times the effect of parental income rank on education, φ_{1rt}. The mediation effects of cognitive and non-cognitive skills are defined analogously as $\rho_{2rt}\varphi_{2rt}$, $\rho_{3rt}\varphi_{3rt}$ while δ_{1rt} captures the remaining association of income across generations after the mediating variables have been controlled for, including any direct effect. However, to estimate the transmission coefficients in Equation (21) without bias[12] one must assume that the errors in Equations (17), (18), and (19) are uncorrelated with the errors in (20), i.e. that:

$$\mathrm{Cov}(\upsilon_{irt}, \upsilon_{1irt}) = \mathrm{Cov}(\upsilon_{irt}, \upsilon_{2irt}) = \mathrm{Cov}(\upsilon_{irt}, \upsilon_{3irt}) = 0 \tag{22}$$

But those assumptions are very strong. To get an idea of what kind of bias that might be present in the estimates, consider the situation where a variable *x* is omitted in Equation (20) that is positively correlated with children's education and income but uncorrelated with the other variables. In this situation, ρ_{1rt} in Equation (20) is biased upwards and δ_{1rt} is biased downwards but only to the extent that *x* is associated with education and income conditional on cognitive skills, non-cognitive skills and parental income. Hence, the scope for bias due to omitted variables diminishes as the richness of the specification of Equation (20) increase.

Bias will also arise if variables are measured with error, which in some sense is inescapably the case when measuring cognitive and non-cognitive

[12] The discussion about bias in this section builds on previous work by Adermon et al. (2016).

skills since the test scores only represent a subset of the latent abilities. But it is also true for education to the extent that educational attainment as measured by years of education fails to capture differences in the quality and returns to education across the distribution of educational practices, subjects, and so on. Measurement error would attenuate the estimates of ρ_{1rt}, ρ_{2rt}, and ρ_{3rt} in Equation (20), and consequently underestimate the mediation of education, cognitive skills and non-cognitive skills and overestimate δ_{1rt}. All things considered, I suspect that attenuation bias due to measurement error is a bigger cause for concern than omitted variable bias in this study, and therefore that the mediating effects are more likely to be underestimated than overestimated.

As previously mentioned, δ_{1rt} captures the conditional association not mediated by the other variables and is in that sense a combination of many different channels in the transmission of income across generations, and I think it is worthwhile to elaborate on what those channels might be. Jerrim and Macmillan (2015), while conducting a similar decomposition exercise for the effect of parental education on children's earnings, propose three different channels. The first operates via the financial resources directly by enabling high income families to support their children during labor market entry. This would be important if it takes a long time to find a job that maximizes income over the whole career, or if such jobs are low- or unpaid internships.

The second operates via connections and networks. Parents with higher income might have more valuable labor market connections that facilitate the labor market success of their children regardless of their children's educational attainment or development of cognitive and non-cognitive skills. Likewise, parents with higher income might also be able to supply their children with a more valuable pool of peers by sending them to select schools or by simply residing in an area of high socioeconomic status.

Finally, the third channel is hereditary endowments such as good looks, height, skin tone, and health endowments, that are unrelated to ability and educational attainment but nevertheless has an effect on labor market success.

4. Data

I use several administrative registers maintained by Statistics Sweden to build my database. The data covers the universe of the Swedish population aged 0-74 years from 1960 until 2012, and their biological parents. All individuals have been linked to the quinquennial national censuses (FoB) 1960-1990; the education register 1985-2012; and the income and tax register (IOT) for a 10 percent sample of the population between 1960-1966 and for the whole population for scattered years between 1968-1984 and all years 1985-2012. As discussed in Jäntti and Jenkins (2015), to measure intergenerational income persistence one must make decisions about when income is to be measured, what kind of income to include and among whom to measure that income. In this section I will elaborate on the choices made in this study.

4.1 Sample selection

This study focuses exclusively on the incomes of fathers and sons. In the parent generation, labor market attachment among mothers is significantly weaker than among fathers and is therefore a poor measure of their socioeconomic status. While this problem is not as severe in the child generation, daughters are still problematic to include because they, to a much larger extent than sons, temporarily detach from the labor market in response to childbirth which means that incomes around 30-35 years of age are not as representative of their lifetime incomes. I also lack military enlistment data on cognitive and non-cognitive skills on women since enlistment into military service has not been mandatory for Swedish women.

To construct my core sample of sons and fathers, I begin by selecting all males born in Sweden between 1961 and 1980 and obtain 1,117,878 sons. I then restrict the sample to sons whose parents are identified in the multi-generation register which contains parental links to all children born in 1932 or later who were a resident in Sweden at some point from 1961 and onward. Since I study cohorts born between 1961 and 1980, I can connect all sons to their fathers if the father is known. I also add the restriction that the fathers must be at least 18 and at most 45 when their son was born. This reduces the sample by about 4.8 percent. I further restrict the sample to sons whose father was born after 1920 and before 1961, which reduces the sample further by about 0.9 percent.[13] This leaves me with a core sample of 1,055,163 sons and their fathers.

I drop all annual incomes below 75 percent of full-time employment on "minimum wage", which in 2012 was about 134,900 SEK (about $19,200). Since Sweden does not have a national minimum wage I have constructed one based on the results in Skedinger (2005) who shows that the minimum wage is approximately 65 percent of the average wage in each branch of industry. Skedinger (2005) also shows that this ratio has been roughly constant between 1970 and 2004. I combine those results with national changes in the hours of work per week and the number of vacation days per year to calculate an annual minimum income level equal to 75 percent of full-time employment on minimum wage. By imposing the minimum income restriction, I get around two

[13] By dropping all fathers born after 1960 I don't have any cohort overlap between the parent and child generations which means that I won't use the same observation as both a son and a father in the sample. Dropping all fathers born before 1921 means that I observe the incomes of the oldest fathers until 1971 (the year they turn 50) and therefore that I potentially observe their income for ten years, but most likely for three years: 1968, 1970 and 1971. Hence, the observation window is similar to that of the youngest sons whose income I also only observe for three years.

problems that is difficult to address by other means. The first is the prevalence of cross-border commuting in some municipalities along the Norwegian and Finnish border, where some workers earn most of their annual income abroad and therefore is not recorded in registries that I can access. This is problematic since I rely on the regional variation of income distributions. The second problem is that students in tertiary education in Sweden tend to supplement the financial aid that they receive while studying with seasonal work over the summer months, and therefore earn an annual income that highly misrepresent their future labor market income. I therefore restrict the sample to fathers and sons with at least three years of observed income above the minimum income threshold, reducing it by a further 16 percent. Finally, to ensure that the sons in my sample are sufficiently exposed to the regional inequality level, I restrict the sample to sons that lived at least 6 consecutive years in the same commuting zone between 2 and 12 years of age. About 98 percent of the remaining sons pass this restriction, leaving me with a final sample of 868,557 sons.

4.2 Variable definitions and descriptive statistics

Following Chetty et al. (2014), I choose to analyze the Great Gatsby Curve using commuting zones as the geographical unit of analysis. I observe residency in 1960 and 1965 and then annually from 1969. I re-code the residency data to map into the 1977 municipality distribution before aggregating the municipalities into a total of 125 commuting zones.[14] [15]

I combine data from the registries to create pre-tax income panels spanning 53 years; from 1960 until 2012. However, I only observe income for 10 percent of the population with a taxable income above one price base amount (roughly $1,600 in 2012 USD) between 1960-19666. Also, data from the income and taxation register is only available for the years 1968, 1971, 1973, 1976, 1979 and 1982. After that, the longitudinal database on education, income and occupation (LOUISE) provides annual data from 1985 and onward. There is also income data in the administrative registries from quinquennial censuses (FOB) between 1970 and 1980. All income has been deflated to 2012 SEK and the year associated with each income corresponds to the year in which the income was earned. The income measure includes wage earnings, business income, taxable benefits and some transfers from the social security system such as sick pay and certain parental benefits. Capital earnings, pensions and parental leave are not included. Furthermore, incomes are measured

[14] The government initiated massive municipality reforms 1952 and 1971. In 1977 the number of municipalities was at an all-time low of 277, compared the 2,532 that existed in 1930 (and the 290 that exists today). By mapping the residency codes into the 1977 municipality distribution I maximize the number of observations in each municipality.

[15] The commuting zones were created by Statistics Sweden based on commuting patterns observed in 1985. The explicit purpose of creating the commuting zones was to form local labor market regions suitable for economic analysis (SCB, 2010).

at the individual level since household income explicitly introduces marital sorting as a mechanism through which income is transmitted between generations (Ermisch et al., 2006). I then approximate father's (son's) permanent labor market income by averaging annual incomes between 30-50 (30-45) years of age. Percentile ranks are then calculated at the national level among all men born in the same cohort.

Just as estimating intergenerational mobility amounts to choosing a statistic to characterize the joint distribution of parent and child income, measuring inequality amounts to choosing a metric to characterize the dispersion in a distribution. The most commonly used metric of inequality is the Gini coefficient and is, since it also readily incorporates changes in the dispersion across the whole distribution, therefore the preferred one in this study. There are numerous mathematically equivalent ways of defining it (Yitzhaki, 1998). The one I prefer is as half of the relative mean absolute difference because of its intuitive interpretation: it is a function of the expected absolute income difference between two random draws from the income distribution. Let y denote annual income, $\bar{y}$ the population average, and n the population size indexed by i and j. Then the Gini coefficient is given by:

$$\mathrm{G} = \frac{\sum_{i=1}^{n}\sum_{j=1}^{n}\left|y_i - y_j\right|}{2n\sum_{i=1}^{n} y_i} \tag{23}$$

The Gini coefficient thus ranges between 0 and 1 where 0 means that everyone has the same income and 1 means that all income as concentrated to one individual. To measure childhood inequality, z_{rt}, I average the annual Gini coefficients (based on labor market incomes as defined in section 4.1 among male residents aged 18 to 64) in the commuting zone, from the year before birth until 18 years of age:

$$\mathrm{z}_{\mathrm{rt}} = \frac{1}{20}\sum_{k=t_{-1}}^{t_{18}} G_{rk} \tag{24}$$

where z_{rt} is the average inequality that cohort t raised in commuting zone r was exposed to from the year prior to birth (in utero) until 18 years of age.

I use data on educational attainment as well as cognitive and non-cognitive skills for the child generation in the decomposition exercise described in Section 2.1. The education data is reported in levels but have been converted into years of education.[16]

The data on cognitive and non-cognitive skills comes from military enlistment tests and is available from 1969 in stanine scale measurements. These

[16] The conversion is as follows; old primary school = 7 years; new primary school = 9 years; short high school = 11 years; long high school = 12 years; short tertiary education = 14 years; long tertiary education = 16 years; and Ph.D. = 20 years.

tests were mandatory for all Swedish men and the enlistment typically took place the year a person turned 18 or 19 years old. Cognitive ability scores were based on tests on verbal, logical, spatial and technical abilities. On the other hand, non-cognitive ability scores were based on semi-structured interviews with a certified psychologist with the explicit aim of assessing the enlistee's ability to cope with the psychological requirements of military service. According to the Swedish National Service Administration as reported by Lindqvist and Vestman (2011), the character traits that gave a high score during the enlistment interview were independence, persistence, willingness to assume responsibility, outgoing character, emotional stability, power of initiative and social skills.

Table 1 reports descriptive statistics across cohorts.

Table 1. Descriptive statistics at the individual level

	(1) 1961-1965	(2) 1966-1970	(3) 1971-1975	(4) 1976-1980
Fathers				
Income	280,924	279,914	284,540	288,798
	(135,498)	(120,975)	(120,250)	(116,137)
Observed incomes	9.3	11.6	13.8	15.4
	(3.3)	(3.4)	(3.4)	(3.7)
Age	42.2	41.3	41.1	40.9
	(2.6)	(2.0)	(1.6)	(1.5)
Sons				
Income	333,867	364,729	369,483	364,253
	(176,854)	(182,089)	(163,456)	(137,255)
Observed incomes	14.1	13.3	9.2	4.9
	(3.2)	(2.9)	(2.1)	(1.4)
Age	37.5	36.9	34.6	32.1
	(1.3)	(1.3)	(1.0)	(0.8)
Education	12.0	12.3	12.8	13.2
	(2.5)	(2.5)	(2.5)	(2.3)
Cognitive ability	5.2	5.3	5.1	5.1
	(1.6)	(1.9)	(1.9)	(1.9)
Non-cognitive ability	5.2	5.2	5.2	5.1
	(1.6)	(1.6)	(1.7)	(1.7)
Cohort size	44,606	47,772	45,687	36,383
	(4,186)	(2,242)	(1,753)	(2,425)

NOTES. Each column represents a cohort span. Age refers to the age at which income is observed. Cohort size refers to the sample size used to estimate the mobility measures. Means and standard deviations.

Average annual income for fathers increases slightly across cohorts; from 280 924 SEK for cohorts born 1961-1965 to 288 798 SEK for cohorts born 1976-1980. Likewise, the average number of observed incomes for fathers increases from 9 to 15, while the average age of fathers at which their income is observed declines slightly from 42 to 41. Son's average annual income also increases slightly over the period; from 333,867 in the first cohort group to 364,253 in the last cohort group. The average number of observed incomes for sons decreases quite rapidly due to the cut-off at 30 years of age: from 14 for the first cohort group to 5 for the last cohort group. At the same time, the average age of sons at which their income is observed only decreases from 38 to 32, while average years of education increases by one from 12 to 13, and since cognitive and non-cognitive skill are measured on stanine scales within each cohort they remain practically constant.

Table 2 reports population weighted descriptive statistics at the CZ level across cohorts. Average rank persistence drops substantially from 0.276 to 0.216, while the average inequality falls from 0.242 to 0.218. The average sample size remains fairly constant across the first three cohort groups and then drops from about 2500 to 1967 in the last cohort group. Notice however that the standard deviation of the sample across commuting zones is very large, reflecting the vast differences in population size in the commuting zones. I will return to that issue in section 5.3 where I present results from estimating the Great Gatsby Curve within Sweden.

Table 2. Descriptive statistics at the regional level

	(1)	(2)		(3)	(4)
	1961-1965	1966-1970		1971-1975	1976-1980
IRP	0.276	0.283	0.259	0.216	0.261
	(0.060)	(0.056)	(0.056)	(0.068)	(0.065)
Gini coefficient	0.242	0.230	0.220	0.218	0.228
	(0.019)	(0.017)	(0.015)	(0.016)	(0.019)
Sample size	2,538	2,614	2,507	1,967	2,432
	(3,167)	(3,209)	(3,089)	(2,447)	(3,030)

NOTES. All variables except sample size are weighted by the CZ sample size of each cohort span. Means and standard deviations.

5. Results

In this section, I first present results on the national inequality and mobility trends during the period. I then show how the regional variation in inequality and mobility looks like, before I move on to estimates of the Great Gatsby Curve. I also present new evidence on features of the Great Gatsby Curve.

5.1 National mobility and inequality trends

Figure 1 plots the national inequality level between 1960 and 1998 the years included in the childhood inequality measures.[17] Inequality was quite high for most of the 1960's at about 0.27 as measured by the Gini coefficient, but then fell to about 0.22 in 1975. Inequality then remained low until the beginning of the 1990's when it started to climb back up.

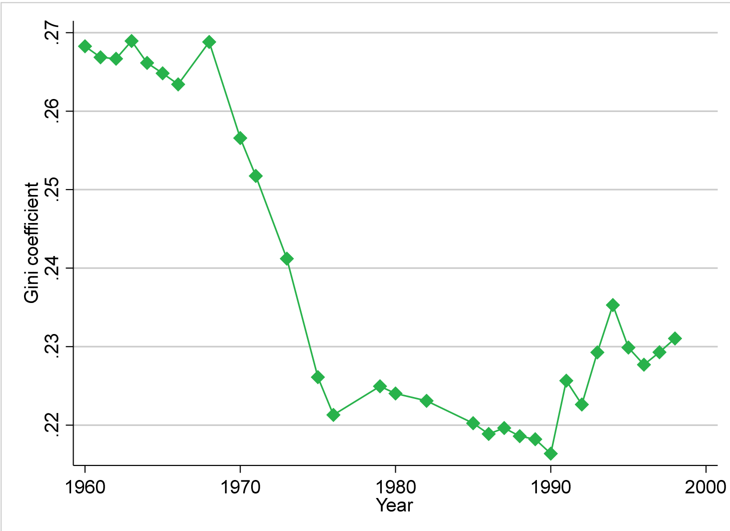

Figure 1. National inequality trend.

NOTES. Inequality is measured as Gini coefficients based on pre-tax labor market incomes (as defined in section 4.2) among the male population aged 18-64.

These patterns are in line with previous studies on inequality in Sweden (Edin and Holmlund, 1993; Johansson et al., 2006; Domeij and Floden, 2010; Björklund and Jäntti, 2011).[18] The sharp wage compression at the end of the 1960's until the mid-1970's was to a large extent caused by decreasing age and education differentials in the labor market and falling returns to education (Edin and Holmlund, 1993). The increase in inequality during the 1990's however, was largely caused by within group dispersion given age, family composition and educational attainment (Domeij and Floden, 2010), and increasing wage dispersion between firms (Skans et al., 2009).

[17] These Gini coefficients are based on pre-tax labor market incomes (as defined in section 4.2) among the male population aged 18-64.

[18] The dissimilarities that do exist are likely caused by differences in sample restrictions and income definitions. For example, Björklund and Jäntti (2011) include capital income in their income measure and find that inequality increased during the 1980's, whereas I don't have information on capital income and find that inequality did not increase until 1990.

Figure 2 shows the intergenerational rank persistence at the national level for cohorts born between 1961 and 1980. For cohorts born in the 1960's the IRP remains quite constant around 0.29. IRP then declines for the rest of the period to about 0.21 for the last cohort born in 1980, which means that later cohorts experienced higher levels of intergenerational mobility.

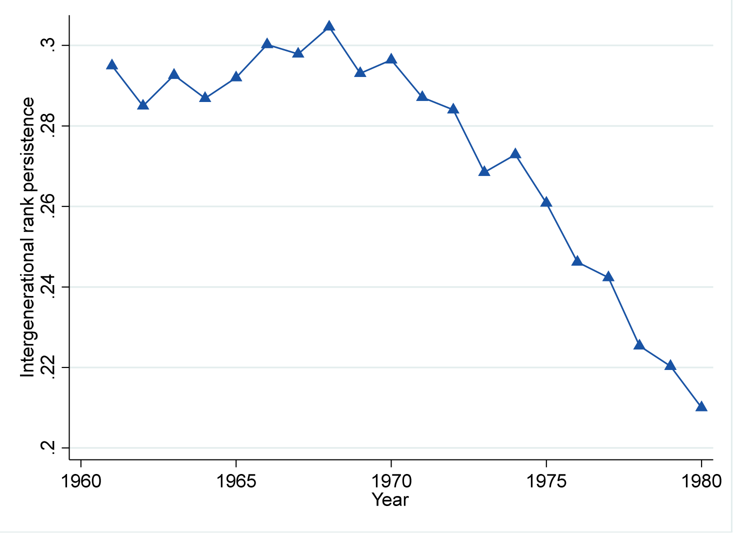

Figure 2. National rank persistence

NOTES. Intergenerational rank persistence is obtained as the slope coefficient from regressing children's income rank on parental income rank for male cohorts born 1961-1980. Hence, the years on the X-axis correspond to year of birth for the cohorts. The income ranks are based on average labor market incomes (as defined in section 4.2) between 30-45 (30-50) years of age for sons (fathers), and then percentile ranked among the male population with the same year of birth.

These mobility patterns are in line with previous results in Björklund et al. (2009). Hence, both inequality and intergenerational rank persistence falls over the period, and Figure 3 shows the striking co-movement between the two.

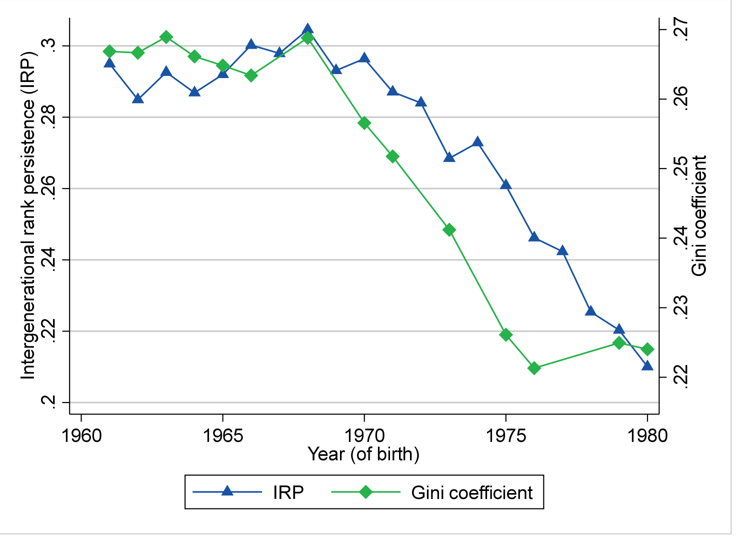

Figure 3. National intergenerational rank persistence and inequality

NOTES. Inequality is measured as annual Gini coefficients based on pre-tax labor market incomes (as defined in section 4.2) among the male population aged 18-64. Intergenerational rank persistence is obtained as the slope coefficient from regressing children's income rank on parental income rank for male cohorts born 1961-1980. Hence, the years on the X-axis correspond to year of birth for the cohorts. The income ranks are based on average labor market incomes between 30-45 (30-50) years of age for sons (fathers), and then percentile ranked among the male population with the same year of birth.

5.2 Regional childhood inequality and mobility

Choropleth maps of the regional distribution of intergenerational rank persistence and childhood inequality are shown in Figure 4. The map is constructed by averaging across cohorts within each commuting zone, and then grouping them into quartiles and shading them so that lighter colors correspond to less intergenerational rank persistence or inequality. With a couple of exceptions, areas with the highest intergenerational rank persistence are located in southern Sweden, including the commuting zones that contain the three largest cities in Sweden: Stockholm, Göteborg and Malmö. The regional differences in mobility are quite substantial, with Munkfors CZ exhibiting the highest level of rank persistence at 0.312, only 0.05 less than what Chetty et al. (2014) found for the United States.

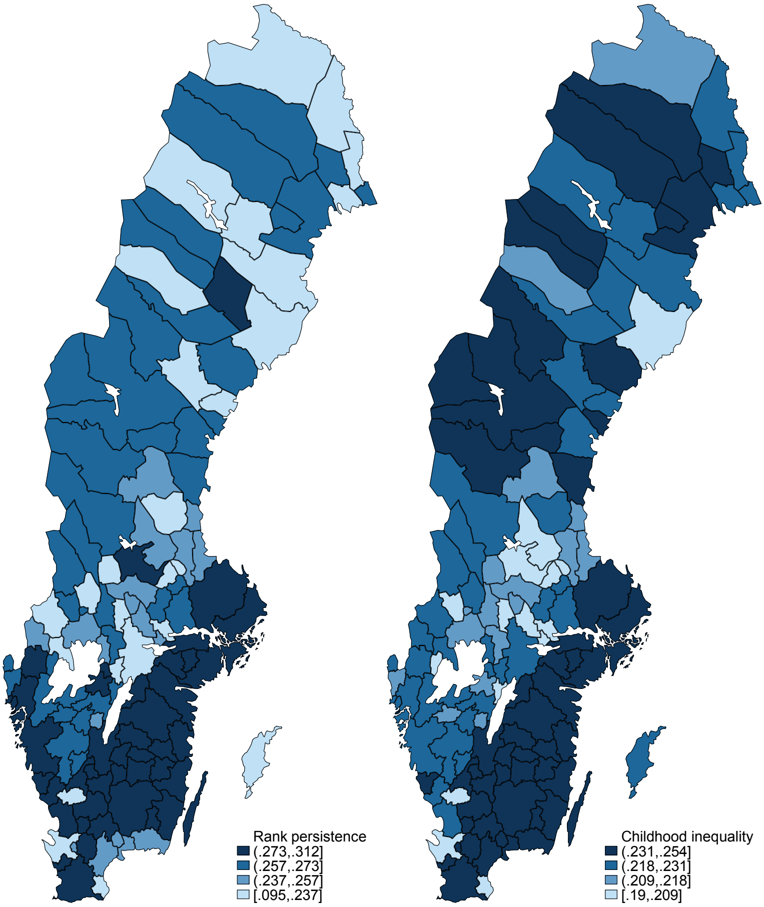

Figure 4. Intergenerational rank persistence and childhood inequality in Sweden

NOTES. The figure is created by averaging rank persistence and childhood inequality over cohorts within commuting zones. The class breaks are defined by the quartiles of the distributions after averaging.

Areas of high inequality are concentrated along the southwestern coastline and in some of the large commuting zones in the north. Stockholm CZ exhibits the highest level of childhood inequality at 0.254, about three standard deviations higher than the unweighted average childhood inequality. In contrast, Olofström exhibits the lowest inequality at 0.19 which is about 1.5 standard deviations lower than the unweighted average. To put these numbers into perspective, the difference between the most equal and the most unequal commuting zones in Sweden is about the same as the difference in inequality between Denmark and Canada (OECD, 2017).

5.3 The Great Gatsby Curve in Sweden

Table 3 reports results from estimating the Great Gatsby Curve in Sweden. Column 1 shows an unweighted OLS estimate of θ in Equation (16), whereas column 2 shows estimates weighted by the sample size in each commuting zone. Weighting reduces the estimated slope from 1.466 to 0.958, possibly indicating that the relationship between inequality and mobility is weaker in commuting zones with relatively large populations or with characteristics that are positively correlated with having a large population (Solon et al., 2015). Weighting also dramatically increases precision – the standard error of the slope coefficient drops from 0.172 to 0.089, indicating that some of the sparsely populated commuting zones have a large impact on the precision of the estimates.

Table 3. The Great Gatsby Curve

	(1) Unweighted	(2) Weighted	(3) Cohort FE	(4) CZ FE
IRP	1.466***	0.958***	0.693***	1.591***
	(0.172)	(0.0899)	(0.0805)	(0.359)
R-squared	0.029	0.082	0.193	0.184
Observations	2,500	2,500	2,500	2,500

NOTES. Sons are born 1961-1980 and their income is observed at approximately 35 years of age. Fathers are born 1920-1960 and their income is observed at approximately 41 years of age. The sample is restricted to sons who lived in the same CZ at least 6 years between 2 and 12 years of age. For each cohort, inequality is equal to the average Gini coefficient in the CZ they grew up in between t_{-1} and t_{+18} where t_0 is the year of birth for the cohort. Standard errors are clustered at the commuting zone level.

Figure 5 illustrates the issue: it plots the Great Gatsby Curve obtained by averaging inequality and mobility across cohorts within commuting zones. The weight (and size) of each data point is proportional to the average sample size in the commuting zone. The Great Gatsby Curve is clearly upward sloping, but the plot reveals a group of outliers in the lower left corner characterized by their low levels of persistence given their levels of childhood inequality, as well as their small populations. These outliers surely effect the precision of the unweighted estimate, but rather than dropping commuting zones from the sample in an ad-hoc fashion I instead opt to use weights throughout the paper.

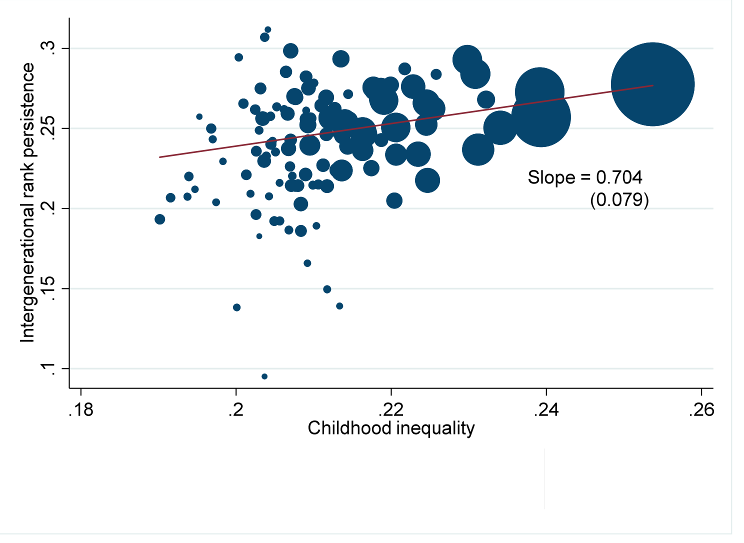

Figure 5. The Great Gatsby Curve across commuting zones in Sweden

NOTES. Each dot in the figure represents the (across cohorts) average intergenerational rank persistence and childhood inequality in a commuting zone (see section 4.2 for definitions). The size of each dot is proportional to the sample size in each commuting zone, i.e. the average number of sons used to estimate intergenerational rank persistence. The fitted line therefore depicts the slope coefficient from a regression of average intergenerational mobility on average childhood inequality weighted by CZ sample size.

Children who were exposed to higher levels of inequality during childhood experienced less intergenerational mobility as adults. I find that a one unit increase in childhood inequality is associated with a 0.958 increase in intergenerational rank persistence, which translates into a standard deviation increase in childhood inequality being associated with a 0.019 increase in intergenerational rank persistence. Taking these estimates at face value, average childhood inequality would have to increase by three standard deviations for the persistence of income across generations in Sweden to reach the same level as in the United States (Chetty et al., 2014).[19]

The third column of Table 3 reports the expected change in intergenerational rank persistence when cohort fixed effects are added to Equation (16), which means that the identifying variation comes from comparing sons born in the same year but in different commuting zones. Revealingly, adding cohort fixed effects decreases the estimated slope which implies that some of the association between inequality and mobility is driven by differences across cohorts. Since the cohorts in the sample are born between 1961-1980, strong candidates for such differences are access to tertiary education, which vastly

[19] Chetty et al. (2014) estimate that the U.S. rank persistence is 0.317 for cohorts born 1980-1982. See the second column of Table 1 where the individual income rank estimate for a sample restricted to male children is presented.

increased after WW2 until the end of the 1970's, and then again in the 1990's and 2000's (SOU, 2007:81), and labor market tightness which remained very low throughout the 1970's and 80's, and then greatly increased during the 1990's (Holmlund, 2003). Other changes at the national level that the cohort fixed effects controls for such as the introduction of financial aid systems for students and the vast expansion of tertiary education, and the diminished role of centralized wage bargaining and the increasing task-based job polarization (Adermon and Gustavsson, 2015). A one unit increase in childhood inequality among sons born in the same year is associated with a 0.693 unit increase in intergenerational rank persistence, compared to 0.958 when cohort fixed effects are not added to the model implying that trends at the national level, such as those mentioned above, are positively associated with intergenerational rank persistence.

Column 4 of Table 3 reports the results from adding fixed effects at the commuting zone level, which means that the variation used to estimate the association between inequality and mobility comes from sons born in different years within the same commuting zones. Therefore, adding CZ fixed effects controls for families selecting into CZ level residency due to differences in the local quality of and access to schools and healthcare, and differences in the local norms and culture as reflected in crime rates, segregation, and so on. In contrast to cohort fixed effects, adding county fixed effects increases the estimated slope, suggesting that selection into residency reduces the association between childhood inequality and intergenerational rank persistence.

The take away from Table 4 is that the association between inequality and mobility holds even after controlling for constant differences across cohorts and commuting zones. The fact that children who experience high levels of inequality during childhood also experience less intergenerational mobility regardless of whether they are compared with children born in the same commuting zone in different years, or with children born in the same year but in a different commuting zone, suggest that the Great Gatsby Curve at least partly reflects a relationship between inequality and fundamental causal processes of income transmission across generations.

5.4 Inequality at different stages of childhood

I now turn to the question whether inequality during specific developmental stages of childhood is differentially associated with income persistence across generations. I average annual Gini coefficients during "baby years" (-1 to 2), "preschool years" (3 to 6), "school years" (7 to 12) and "teen years" (13 to 18), and use them to replace the childhood inequality measure, z_{rt}, in Equation (16). The output is reported in Table 4. Panel A reports estimates for bivariate regressions of mobility on inequality in each period.

Table 4. Inequality at different stages of childhood

	(1) Baby age (-1 to 2)	(2) Pre-school age (3 to 6)	(3) School age (7 to 12)	(4) Teen age (13 to 18)
Panel A				
IRP	0.926***	0.842***	0.910***	0.561***
	(0.0478)	(0.0522)	(0.0686)	(0.0815)
R-squared	0.13	0.09	0.07	0.02
Panel B				
IRP	1.378***	-0.199	0.184	-0.920***
	(0.119)	(0.159)	(0.196)	(0.138)
R-squared	0.16	0.16	0.16	0.16
Observations	2,500	2,500	2,500	2,500

NOTES. Panel A shows estimates from bivariate regressions of intergenerational rank persistence on the average CZ by cohort inequality within the age spans in the columns. Panel reports the conditional effects of inequality within each age span given inequality at all other age spans. All estimates are obtained using CZ-by-cohort sample size weights. Standard errors are clustered at the commuting zone level.

Inequality at all four stages of childhood are positively associated with the persistence of income across generations, but inequality during the baby years explains the most variance with an R-squared of 0.13, compared with 0.09 for preschool years, 0.07 for school years, and only 0.02 for teen years. The point estimate also drops significantly between school years and teen years, from 0.91 to 0.56. Hence, inequality during the earliest stages of childhood appears to have the biggest impact on the persistence of income across generations. This is supported by the results reported in Panel B, which shows estimates of partial effects of inequality on mobility conditional on the level of inequality in the other age spans. Inequality during the baby years is the only partial effect that is statistically significant, indicating that inequality at the earliest stages of life can explain variation in subsequent social mobility that inequality later in life cannot. A one standard deviation increase in inequality is associated with a 0.035 unit increase in intergenerational income persistence, which is larger than the point estimate of 0.019 reported in section 5.3.

5.5 Level effects of childhood inequality

To investigate whether the Great Gatsby Curve is constant across the CZ by cohort inequality distribution, I fit a linear spline in inequality to the data by partitioning the data into three segments using two equidistant points between the minima and maxima of observed childhood inequality. The bottom segment ranges from 0.18 to 0.21, the middle segment from 0.21 to 0.25, and the upper segment from 0.25 to 0.28. The spline coefficients reflect the expected change in intergenerational income persistence following a unit change in inequality given that the change occurs at the specific segment of the inequality distribution. The output is shown in Table 5 where the first row reports the estimated slope coefficients in each segment, while the second row reports the probability that those slopes are equal to the global slope of 0.958, and the third row reports the probability that the slope in the segment is equal to the slope in the preceding segment.

Table 5. Linear spline in inequality

	(1) Global effect	(2) .18 to .21	(3) .21 to .25	(4) .25 to .28
IRP	0.958***	1.955***	0.629***	1.161***
	(0.0899)	(0.413)	(0.203)	(0.168)
$P(\theta_k = \theta_{global})$		0.017**	0.109	0.227
$P(\theta_k = \theta_{k-1})$			0.020**	0.082*
R-squared	0.082	0.086	0.086	0.086
Observations	2,500	1,616	843	41

NOTES. Column 1 shows the estimated Great Gatsby Curve across commuting zones and cohorts in Sweden, i.e. the slope coefficient θ in Equation (16). Columns 2-4 shows the slope coefficient from regressing intergenerational rank persistence on childhood inequality in the respective segments of the inequality distribution. The third row shows the probability that the slope estimate in the segment is equal to the global slope, and the fourth row show the probability that the slope estimate in the segment is equal to the slope estimate in the preceding segment. All estimates are obtained using CZ-by-cohort sample size weights. Standard errors are clustered at the commuting zone level.

Only the estimated slope in the first segment is significantly different from the global slope, with a rather imprecisely estimated point estimate of 1.96. The results should be interpreted with caution, but nevertheless suggests that inequality that increases from a low level has a relatively larger impact on intergenerational income persistence.

6. Mechanisms

In this section, I first present the results from decomposing the transmission of income across generations as described in section 3.2. I then describe how the mediation effects are related to childhood inequality, and how those relationships differ when inequality increases due to changes above or below the median of the income distribution.

6.1 The mediators of mobility

The top row of Table 6 shows the decomposition of β_{rt} in Equation (15) into four orthogonal channels of intergenerational income persistence: children's educational attainment; children's development of cognitive skills; children's development of non-cognitive skills; and a residual effect that captures the conditional effect of parental income on children's income after controlling for the other variables. The three mediating variables collectively account for about 53 percent of the total persistence of income across generations, leaving about 45 percent accounted for by the residual effect.[20]

Table 6. Decomposition results

	(1) Total persistence	(2) Through Education	(3) Through cognitive skill	(4) Through non-cognitive skill	(5) Residual effect
Panel A					
IRP	0.26	0.05	0.04	0.04	0.12
	(100%)	(21%)	(16%)	(16%)	(45%)
Panel B					
IRP		0.095	0.062	0.047	
		(36%)	(24%)	(18%)	
Observations	2,500	2,500	2,500	2,500	2,500

NOTES. Panel A column 1 reports the average intergenerational rank persistence across 20 cohorts and 125 commuting zones. Sons are born 1961-1980 and their income is observed at approximately 35 years of age. Fathers are born 1920-1960 and income is observed at approximately 41 years of age. The sample is restricted to sons who lived in the same CZ at least 6 years between 2 and 12 years of age. Columns 2-4 report the average mediation effect of education, cognitive skill and non-cognitive skill across CZ's in the persistence of income across generations. Column 5 reports the persistence of income across generations that remain after the mediation of the other variables is accounted for. Panel B column 2 reports the mediation effect of education when cognitive and non-cognitive skills are unaccounted for, while columns 3-4 reports the mediation effects of cognitive and non-cognitive skills when education is unaccounted for. Standard errors are clustered at the commuting zone level.

Among the mediating variables, children's educational attainment accounts for the largest part of intergenerational persistence at 21 percent, while children's development of cognitive and non-cognitive skills account for about 16 percent each.

Since children with good social skills, high perseverance and a high capacity for abstract and logical thinking naturally does well in school and therefore select into higher education simply because it comes easy for them, one might wonder to what extent cognitive and non-cognitive skills begets educational attainment, and conversely what the role of schooling is in the formation of

[20] The sum of the percentages do not add up to 100 due to round-off errors

cognitive and non-cognitive skills?[21] To investigate this, I first estimate the returns to education conditional only on parental income:

$$y_{irt} = \gamma_{rt} + \pi_{1rt}e_{irt} + \delta_{2rt}y_i^p + \upsilon_{irt} \tag{25}$$

Here, π_{1rt} is an estimate of the returns to education conditional on parental income whereas ρ_{1rt} in Equation (20) is an estimate of the returns to education conditional on parental income and cognitive and non-cognitive skills. Therefore, the difference between $\pi_{1rt}\varphi_{1rt}$ and $\rho_{1rt}\varphi_{1rt}$ captures the extent that children's cognitive and non-cognitive skills generates intergenerational income persistence by enabling higher educational attainment.

Next, I estimate the returns to cognitive and non-cognitive skills conditional only on parental income:

$$y_{irt} = \gamma_{rt} + \pi_{2rt}c_{irt} + \pi_{3rt}n_{irt} + \delta_{3rt}y_i^p + \upsilon_{irt} \tag{26}$$

Hence, the difference between $\pi_{2rt}\varphi_{2rt}$ and $\rho_{2rt}\varphi_{2rt}$, and $\pi_{3rt}\varphi_{3rt}$ and $\rho_{3rt}\varphi_{3rt}$, captures the extent that schooling contributes to intergenerational income persistence by affecting children's development of cognitive and non-cognitive skills.

The estimates of $\pi_{1rt}\varphi_{1rt}$, $\pi_{2rt}\varphi_{2rt}$ and $\pi_{3rt}\varphi_{3rt}$ is shown in the second row of Table 6. When skills are excluded from the returns estimation, the mediation effect of children's schooling increases from 21 percent of the total persistence to 36 percent. Taken at face value, that means that selection accounts for about 42 percent of the total mediation effect of educational attainment, which is quite substantial. Focusing on the role of schooling for the contribution of children's development of cognitive and non-cognitive skills to intergenerational income persistence, I find that the mediation effect of children's cognitive skills increases from 16 to 24 percent, and that the mediation effect of children's non-cognitive skills increases from 16 to 18 percent. This implies that schooling can account for about a third of the impact that children's cognitive skills have on intergenerational income persistence, but only about 11 percent of the contribution that children's non-cognitive skills have.

6.2 The mediators of mobility and childhood inequality

Panel A of Table 7 shows how the mediation effects and residual effect from the fully specified model ($\rho_{1rt}\varphi_{1rt}$, $\rho_{2rt}\varphi_{2rt}$, $\rho_{3rt}\varphi_{3rt}$, and δ_{1rt}) are related to inequality. The most striking result is that the residual effect is uncorrelated with inequality. Since the residual effect account for nearly half of the total

[21] In the child generation, the correlation is 0.53 between education and cognitive skills, and 0.31 between education and non-cognitive skills. The correlation between cognitive and non-cognitive skills is 0.37.

persistence of income across generations this is a surprising result. It implies that the Great Gatsby Curve is entirely driven by the contribution of children's educational attainment and development of cognitive and non-cognitive skills to the persistence of income across generations.

Table 7. The mechanisms of the Great Gatsby Curve

	(1)	(2)	(3)	(4)	(5)
	Total persistence	Through Education	Through Cognitive skill	Through Non-cognitive skill	Residual effect
Panel A					
Inequality	0.958***	0.318***	0.299***	0.291***	0.0459
	(0.0899)	(0.0780)	(0.0437)	(0.0212)	(0.0697)
Std. Beta	0.287	0.219	0.257	0.276	0.0149
R-squared	0.082	0.048	0.066	0.076	0.000
Panel B					
Inequality	0.958***	0.640***	-	-	0.318***
	(0.0899)	(0.117)			(0.0669)
Inequality	0.958***	-	0.452***	0.319***	0.187***
	(0.0899)		(0.0746)	(0.0227)	(0.0536)
Observations	2,500	2,500	2,500	2,500	2,500

NOTES. Sons are born 1961-1980 and income observed between 30-45 years of age. Paternal income is observed between 30-50 years of age. The sample is restricted to permanent residents, i.e. sons who lived in the same CZ at least 6 years between 2 and 12 years of age. Slope coefficients and standardized coefficients are reported in Panel A. Panel B reports estimates when either education or skills are omitted from the decomposition. Standard errors are clustered at the commuting zone level.

Standardized coefficients are also reported in Panel A to make the size of the coefficients easier to compare, and a one standard deviation increase in childhood inequality is associated with a 0.22 standard deviation increase in the mediation effect of children's educational attainment on the persistence of income across generations. The corresponding estimates for children's cognitive and non-cognitive skills are 0.26 and 0.28 respectively, which suggest that the three channels of income transmission are similarly responsive to changes in childhood inequality.

Panel B of Table 7 shows how the mediation effects related to childhood inequality when either education or cognitive and non-cognitive skills are excluded from the decomposition. As expected, when children's cognitive and non-cognitive skills are omitted some of the transmission of income across generations that goes through those channels is picked up by the mediating effect of educational attainment. The expected change in the mediating effect of children's educational attainment following a one unit increase in childhood inequality increases from 0.32 to 0.64. However, as we saw in section 6.1 educational attainment does not pick up all the income persistence accounted for by cognitive and non-cognitive skills. Hence, the expected change in the residual effect following a one unit increase in childhood inequality also increases, from 0.05 to 0.32 which means that educational attainment absorbs about half of the association between childhood inequality and the mediating effects of cognitive and non-cognitive skills while the rest is soaked up by the residual effect. In an analogous fashion when education is excluded, the expected change in the mediating effect of children's development of cognitive skills following a one unit increase in childhood inequality increases from 0.3 to 0.45, and from 0.29 to 0.32 for non-cognitive skills. The remaining persistence that children's educational attainment accounts for ends up in the residual effect, whose expected change following a one unit increase in childhood inequality increases to 0.19.

The main takeaway from these results are that the Great Gatsby Curve is entirely driven by the mediating effects of children's educational attainment and development of cognitive and non-cognitive skills. Another important result is that failing to account for children's educational attainment or cognitive and non-cognitive skills causes an upward bias of the estimated mediating effects, which in the case of the residual effect could be misinterpreted as evidence of an association between inequality and a direct effect of parental income on children's income.

6.3 Disentangling attainment from returns

Now that we've seen that the mediating effects of children's educational attainment and development of cognitive and non-cognitive skills are all positively correlated with childhood inequality, it is natural to ask whether these positive correlations reflect a relationship between the attainment/development of these mediators, as reflected by the period 1 function $\mathbf{g}_{1rt}$ in Equation (1), or whether they simply reflect a relationship between inequality and the labor market returns to the mediators, as reflected by the period 2 function f_{2rt} in Equation (2)?

Table 8 reports results from separately regressing the slope coefficients φ_1, φ_2 and φ_3 obtained from the bivariate regressions defined by Equations (17)-(19), and the slope coefficients ρ_1, ρ_2 and ρ_3 obtained from Equation (20) on childhood inequality. As can be seen in the first row, childhood inequality is positively correlated with the correlation between parental income and children's educational attainment and development of cognitive and non-cognitive skills. Inequality is also positively correlated with the correlation of parental income and the conditional returns to cognitive and non-cognitive skills, but it is uncorrelated with the correlation between parental income and the conditional returns to educational attainment. This result is perhaps not very surprising: in a highly mobile and functional labor market, everyone should face pretty much the same rate of returns to their education.

Table 8. Attainment and returns to the mediators

	(1) Education	(2) Cognitive skills	(3) Non-cognitive skills
Attainment	0.141***	0.059***	0.034***
	(0.023)	(0.007)	(0.006)
Returns	0.218	10.29***	14.53***
	(1.407)	(2.018)	(1.675)
Observations	2,500	2,500	2,500

NOTES. The first row reports the OLS estimates from regressing the slope coefficients obtained in Equations (17), (18) and (19) on childhood inequality; i.e. it reports the relationship between childhood inequality and the association between paternal income and the mediating variables. The second row reports the OLS estimates from regressing the slope coefficients obtained in Equation (20) on childhood inequality; i.e. it reports the relationship between childhood inequality and the association between paternal income and the conditional returns to the mediating variables. Standard errors are clustered at the commuting zone level.

6.4 Mediators and the childhood inequality distribution

To create Table 9, I have replaced Gini coefficients with percentile ratios as the inequality metric in Equations (16) and (24). A one unit increase of the 50-10 percentile ratio during childhood is associated with a 0.31 increase in intergenerational income persistence, while a one unit increase in the ratio of the 90-50 percentile ratio is only associated with a 0.17 increase in intergenerational income persistence, implying that mobility is more sensitive to changes in inequality at the bottom half of the income distribution.

Table 9. Mediators and inequality above and below the median

	(1) Total persistence	(2) Through Education	(3) Through Cognitive skill	(4) Through Non-cognitive skill	(5) Residual effect
$P_{90/50}$	0.171***	0.0509**	0.0515***	0.0598***	0.00832
	(0.0189)	(0.0197)	(0.00914)	(0.00423)	(0.0173)
$P_{50/10}$	0.309***	0.0405	0.116***	0.100***	0.0510**
	(0.0766)	(0.0409)	(0.0217)	(0.0196)	(0.0254)
Observations	2,500	2,500	2,500	2,500	2,500

NOTES. Sons are born 1961-1980 and income observed between 30-45 years of age. Paternal income is observed between 30-50 years of age. The sample is restricted to permanent residents, i.e. sons who lived in the same CZ at least 6 years between 2 and 12 years of age. Standard errors are clustered at the commuting zone level.

I also find that childhood inequality below the median is uncorrelated with the mediation effect of children's educational attainment. Since the mediation effect involves both the association between parental income and children's education attainment and the subsequent returns to that education, this result suggests that inequality below the median is either unrelated to both effects, or that it is positively correlated with one and negatively correlated with the other. It turns out that the latter is true; inequality below the median is negatively correlated with the effect of parental income on children's educational attainment and positively correlated with the returns to education. Furthermore, I find that inequality below the median is positively correlated with the residual effect which is a result that is difficult to interpret. It either implies that parental income has a larger direct effect on children's income for children who grew up in places where the difference between the relatively poor and the median earner were large, or that things like parental networks and hereditary traits are more important in such rearing environments.

7. Robustness analysis

In this section I show that the results in section 5 are robust to the choice of inequality metric to characterize the dispersion in the income distributions, and to the choice of mobility statistic to characterize the joint distribution of parent and child income. I also show that they are robust to the choice of minimum income level as described in section 4.2.

7.1 Inequality metrics and mobility statistics

Table 10 reports output from estimating Equation (16) using both the intergenerational rank persistence (panel A) and the intergenerational elasticity of income (panel B) as mobility statistics, and the Gini coefficient, the mean log deviation (MLD), and the 90-10 percentile ratio as inequality metrics. The MLD is relatively sensitive to changes near the bottom of the income distribution and in that sense complements the Gini coefficient since it is relatively sensitive to changes in the middle of the income distribution.

Let y denote annual income, $\bar{y}$ the population average, and *n* the population size indexed by i. Then the MLD is defined as:

$$\text{MLD} = \frac{1}{2}\sum_{i=1}^{n} ln\left(\frac{\bar{y}}{y_i}\right) \qquad 27$$

The 90-10 percentile ratio, which incorporates less information about the dispersion of the income distribution since it abstracts from changes in the distribution at all other percentiles, is a common statistic in the literature with the benefit of a clear interpretation: a one unit increase of the 90-10 percentile ratio means that the income of the individual at the 90th percentile has increased by an amount that is equal to the income of the individual at the 10th percentile.

Table 10. Different inequality metrics and mobility statistics

	(1) Unweighted	(2) Weighted	(3) Cohort FE	(4) CZ FE
Panel A				
Ginicoefficient	1.466***	0.958***	0.693***	1.591***
	(0.172)	(0.0899)	(0.0805)	(0.359)
Mean log deviation	2.118***	1.268***	0.930***	1.965***
	(0.249)	(0.130)	(0.102)	(0.450)
$P_{90/10}$	0.126***	0.0817***	0.0579***	0.187***
	(0.0195)	(0.00930)	(0.00643)	(0.0395)
Panel B				
Ginicoefficient	1.910***	1.275***	0.779***	2.450***
	(0.206)	(0.115)	(0.103)	(0.590)
Mean log deviation	2.770***	1.695***	1.049***	3.002***
	(0.295)	(0.174)	(0.124)	(0.744)
$P_{90/10}$	0.174***	0.108***	0.0672***	0.287***
	(0.0237)	(0.0121)	(0.00749)	(0.0647)
Observations	2,500	2,500	2,500	2,500

NOTES. Sons are born 1961-1980 and income observed between 30-45 years of age. Paternal income is observed between 30-50 years of age. The sample is restricted to permanent residents, i.e. sons who lived in the same CZ at least 6 years between 2 and 12 years of age. Panel A reports slope coefficients from regressions using IRP as mobility statistic, and panel B using IGE. Standard errors are clustered at the commuting zone level.

I find that children that were exposed to higher levels of inequality during childhood experienced less intergenerational mobility across all six combinations of mobility statistics and inequality metrics. Furthermore, adding weights and fixed effects has the same qualitative effect in all specifications in terms of precision and the direction of the change in the slope coefficients. In sum, the choice of mobility statistic and inequality metric does not seem to be crucial for the Great Gatsby Curve.

7.2 Minimum income levels

Table 11 also reports output from estimating Equation (16), but this time using mobility and inequality measures that are based on minimum income levels that are either half as large or twice as large as the preferred minimum income level (defined in section 4.2). The first row reports estimates based on the preferred minimum) income as a reference point (it's the same output as in Table 3). The second row reports estimates based on minimum incomes that are half as large, and looking at column 1 and 2 we can see that the unweighted estimate is smaller (1.001 compared to 1.466 in the first row) but that the weighted estimate is larger (1.014 compared to 0.958).

Table 11. Different minimum income levels

	(1) Unweighted	(2) Weighted	(3) Cohort FE	(4) CZ FE
Preferred minimum income	1.466***	0.958***	0.693***	1.591***
	(0.172)	(0.0899)	(0.0805)	(0.359)
Half the minimum income	1.001***	1.014***	0.592***	1.325***
	(0.156)	(0.0818)	(0.154)	(0.249)
Twice the minimum income	1.301***	1.017***	0.997***	0.993***
	(0.165)	(0.0768)	(0.0948)	(0.300)
Observations	2,500	2,500	2,500	2,500

NOTES. The table is created analogously to Table 3, except that the minimum income level (MIL) used to calculate inequality and estimate mobility in the second and third row is equal to half and twice the preferred MIL respectively. All estimates are obtained using CZ-by-cohort sample size weights. Standard errors are clustered at the commuting zone level for columns 1-3 and at the cohort level in column 4.

The effect of doubling the minimum income level is reported in the third row, and it follows the same pattern; the unweighted estimate is slightly smaller at 1.301 compared to 1.466 and the weighted estimate is slightly larger at 1.017 compared to 0.958. Considering the size of standard errors, most of these differences are not statistically significant and I therefore conclude that the results are not particularly sensitive to where the minimum income level is set given that there is one.

8. Conclusion

I have estimated the Great Gatsby Curve across 125 commuting zones within Sweden for 20 cohorts born between 1961 and 1980, and found that children who were exposed to higher levels of inequality during childhood also experienced less intergenerational mobility regardless of whether they grew up in a given commuting zone at a time when inequality was high, or whether they grew up at a given point in time in a commuting zone where inequality was high. Hence, I have found that the Great Gatsby Curve exists in Sweden - a country with much less institutional heterogeneity across regions than China or the United States which means that the results in this study more readily generalize to other European countries. I have also presented new evidence on two features of the Great Gatsby Curve. First, I found that inequality is more strongly associated with intergenerational mobility at the earliest stages of childhood (age -1 to 2). Second, I found that childhood inequality is more strongly associated intergenerational mobility at lower levels of inequality.

To understand the underlying mechanisms that drive the Great Gatsby Curve, I decomposed the transmission of income across generations into four separate channels: children’s educational attainment, children’s cognitive skills, children’s non-cognitive skills, and a residual effect that capture the conditional effect of parental income on children’s income once the other mediating variables have been accounted for. I found that children’s educational attainment accounts for approximately 21 percent of the total persistence of income across generations, while children’s developments of cognitive and non-cognitive skills account for about 16 percent each, leaving almost half of the total persistence accounted for by the residual effect. However, upon investigating the relationship between childhood inequality and the transmission channels I found that childhood inequality is uncorrelated with the residual effect even though it accounts for almost half of the total persistence of income across generations. In contrast, all three mediating variables were found to be positively correlated with childhood inequality.

The results therefore suggest that children who grew up in regions or cohorts with high levels of inequality experienced less social mobility because their parent's income had a stronger effect on their educational attainment and development of cognitive and non-cognitive skills (and/or the returns to them), but not a stronger direct effect on their income. Consequently, the results suggest that adverse effects of inequality on mobility can be alleviated by progressive policies that targets the development of children's cognitive and non-cognitive skills as well as their educational attainment.

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II. Policy of last resort: Replacing student loans with grants

1. Introduction

The educational attainment of the Swedish population has increased drastically in the post-war period. Between 1940 and 2016, the number of students enrolled in tertiary education in Sweden increased from 11 000 to 402 000 (Andrén, 2013; UKÄ, 2018). This massive shift in the educational level of the population has come about in conjunction with the development of new technologies and modes of production, and an unprecedented period of economic growth.

But not everyone thrives under the demands of the modern educational system. Only 72 percent of the students that enrolled in upper secondary education in Sweden in 2012 graduated with a degree within 5 years (Skolverket, 2018). The situation is exacerbated by the fact that recent decades have been characterized by polarized job growth, where the demand for middle-skilled jobs have decreased relative to low-skilled and high-skilled jobs that cannot be automated and performed by machines (Adermon and Gustavsson, 2015). Consequently, the prospects for young adults on the labor market without a complete upper secondary education are rather dismal. To address persistent unemployment rates among the low educated and to adjust the skills of the workforce to the structural changes of the economy, Sweden has developed a comprehensive system of adult education. But the potential benefits of adult education cannot be realized unless people choose to enroll. In an attempt to increase enrollment and thereby improve labor market prospects among unemployed adults with incomplete upper secondary education, the Recruitment Grant was introduced in 2003.

The grant replaced the loans in the national student aid system, and as such offers an opportunity to study the effects of student aid when credits constraints are absent.[22] There were three eligibility criteria: recipients had to be unemployed or at the risk of becoming unemployed; they had to be between 25-50 years of age; and they were not allowed to have received any other form of student aid in the past five years (Prop. 2001/2002:161).[23] Furthermore, the grant was only given for studies at the compulsory or upper secondary level, and for a maximum of one year.

The purpose of this paper is to estimate the causal effects of student aid on educational attainment and subsequent labor market outcomes. The difficulty in identifying the causal effects is to separate the effects of student aid from differences in unobserved characteristics between those who receive the aid

[22] Depending on whether a student was eligible for supplemental aid or not, the loans that were replaced by the grant amounted to about $1,700 or $3,680 per year (Prop. 1999/2000:10).

[23] Since the control group at the student aid margin can potentially switch into treatment in later years if the introduction of the reform in 2003 is used to identify the effect, I restrict the analysis to the repeal of the reform in 2006.

and those who do not. I address this by exploiting the repeal of the Recruitment Grant in 2006 in a difference-in-differences (DD) framework.

To preview the results, I find that the repeal of the Recruitment Grant reduced enrollment in adult education by 10 percent in the target population relative to the pre-treatment enrollment rate, and that the number of passed credits decreased by 28 percent. In terms of labor market outcomes, the repeal increased the unemployment rate by 3.2 percentage points in the target population in 2008, and by 2.1 percentage points in 2009. Focusing on long term outcomes, I find that that the repeal decreased the average labor market income between 2012 and 2014 of the target population by about $280 while increasing the number of days in unemployment by 27.2 days in the same period. In sum, the repeal of the Recruitment Grant had sizable adverse effects on educational attainment and subsequent labor market outcomes for the target population.

There is a large empirical literature on how student aid affects educational attainment. Previous studies have generally found that financial aid has a positive impact on college completion and reduces drop-out and retention (Van der Klaauw, 2002; Dynarski, 2003; Dynarski, 2008; Bettinger, 2004; Goodman, 2008; Angrist et al., 2009; Glocker, 2011). This paper contributes to this literature in two ways. First, by studying the effect of student aid on a subset of the population that rarely features in the literature - adults with incomplete schooling. Typically, eligibility for student aid selects on academic merit and/or financial need. Consequently, the effect of student aid is often identified locally at the upper end of the skill distribution among recent high school graduates with a financially disadvantaged family background. It is not obvious that results from that subset of the population generalize to the rest of the population. This study addresses that shortcoming by estimating the effect of student aid for adults at the lower end of the skill distribution. Second, the empirical literature on student aid is almost exclusively focused on enrollment decisions at the college level, whereas the Recruitment Grant was offered for studies at the compulsory and upper secondary level.[24] Hence, by comparing the estimates in this paper with previous findings, this study will be informative about whether the effects of student aid differ across educational levels.

This study also relates to the behavioral economic literature on financial decision making. Standard economic theory on the effect of student aid on school enrollment is derived from human capital theory, where school enrollment is seen as an investment decision whose soundness depends on the (utility) returns to education and the opportunity cost of enrollment (Becker, 1975). Individuals are assumed to behave rationally in the sense that they are fully informed and form unbiased expectations about outcomes and make decisions that will maximize utility over their lifespan. In the basic model, non-

[24] An exception is Angrist et al. (2002) who studies the effect of a voucher lottery in Colombia on students in secondary school.

pecuniary costs and returns are ignored so that the enrollment decision only depends on whether the present discounted value of the returns exceed the present discounted value of the costs. In contrast, behavioral economic theory offers several insights as to why standard economic theory might be too simplistic to describe the relationship between student aid and school enrollment. First, informational asymmetries seem to have a role to play in the enrollment decision. Bettinger et al. (2012) found that providing application assistance to low-income individuals increased aid application rates and receipt, as well as college attendance. Second, students who are reluctant to take up loans to finance their studies may be over-weighting the risk of defaulting on their loans. Such loan averse behavior is supported by prospect theory which suggests that people overweight extreme events, especially when their probability of occurring is very low (Tversky and Kahneman, 1992). In a study that elicited risk aversion based on a set of survey questions, Hryshko et al. (2011) found that sex and age are strong predictors of risk aversion, with women and older individuals being more risk averse. Third, prospect theory also supports so called framing and labeling effects that leads to loan averse behavior, by suggesting that people make decisions based on a reference point and arrive at different decisions depending on the frame or label of the reference point (Tversky and Kahneman, 1992). For example, Caetano et al. (2011) found that labeling a contract as a "loan" reduced the probability of it being accepted by 8 percent compared to a financially equivalent contract. However, recipients of the Recruitment Grant did not have to accumulate debt and were actively recruited, informed, and assisted with the application procedure by officials at the municipal level. Hence, this study contributes empirically to the literature on financial decision making by analyzing the impact of a student aid reform that not only had a financial component but also entailed efforts to overcome obstacles to rational decision making.

The paper proceeds as follows: the next section describes the institutional background of the reform in terms of the Swedish school system, and section 2 presents the data. Estimation and identification is discussed in section 3, and section 4 presents the results. Section 5 concludes.

2. Institutional background

All public education in Sweden is tuition free. The current school system has 9 years of compulsory school that comprises the primary and lower secondary level. This is followed by 3 years of upper secondary school that upon completion grants admission to tertiary education. However, until 1994 students had the option to enroll in 2-year tracks in upper secondary school that were mostly vocational and did not grant admission to tertiary education (Prop. 1990/1991:85).

The current school system was formally implemented in 1962, but it took until 1972 to complete the roll out. It was preceded by a school structure with early tracking, in which most people attended 7-8 years of primary school and some attended 1-3 years of secondary school. Very few enrolled in tertiary education. Upper secondary education expanded rapidly in the wake of the roll out which generated an educational gap between younger and older cohorts on the labor market. To bridge the gap and meet the increasing demand for education among the adult population, “Komvux” was introduced in 1968.

Komvux literally means “municipal adult education” and is just that – education at the compulsory and upper secondary level for adults, organized by the municipalities but financed by the state.[25] Komvux offers an extensive range of courses available to all Swedish residents above 20 years of age, and anyone who wishes to enroll has a legal right to take a leave of absence from work and receive student aid from the state for the duration of the studies.

The structure of the current student aid system can be traced back to 1965, but the point of departure relevant to this study is the 2001 reform which consolidated the separate forms of student aid that existed at the time into the cohesive system in place today (Prop. 1999/2000:10). Several features of the student aid system are relevant to this study. First, the level of the student aid varies with the inflation and is comprised of both loans and grants. The share of the aid that consists of loans depends on which tier the student is in of which there are two. The first tier entails a 65.5 percent loan share and the second entails a 20 percent loan share, although the total amount of student aid is the same in both tiers.[26] Eligibility for the second tier is reserved for studies at the compulsory and upper secondary level for students that are above 25 years of age. Hence, everyone in the analysis sample is eligible for the second tier. Second, the student aid is means tested. If a student earns an income above a certain threshold, the student aid will be reduced by 50 percent of the exceeding amount.[27] Third, students must pass their courses to continue to receive student aid. The exact amount of credits a student must pass depends on the level and pace of the studies but is usually about 75 percent of what is considered full-time studies. A final feature of the student aid system that is highly relevant to this study, is that students can qualify for “supplemental aid” if their taxable income in past the twelve months exceed a certain threshold.[28] The supplemental aid consists entirely of loans and increases the level of the student aid by 22 percent which for a student studying full-time in 2006 implied an increase from about $8,900 per year to $10,900 (SCB, 2016).

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[25] Courses are also given at a “supplemental” level that typically have a vocational content. Very few students attend courses at this level.

[26] All references to dollar values in the paper have been adjusted to 2014 prices.

[27] In 2006, the threshold for means adjustment was about $6,760 per semester (SCB, 2016).

[28] In 2006, the income threshold for supplemental aid was about $21,500 (SCB, 2016).

2.1 The Recruitment Grant

The Recruitment Grant was introduced in 2003 and repealed in 2006 after the September elections that saw the social democratic government ousted by a conservative alliance (Prop 2006/2007:17). An explicit goal of the Recruitment Grant was to recruit individuals who in the absence of the reform would not have enrolled in adult education. To achieve that goal, officials at the municipal level - social workers, job counselors, and even librarians - were engaged with recruiting potential recipients. In the typical case, a recipient would be informed of the grant by a job counselor who would assist in the application procedure and fill in the necessary paperwork (Hirasawa and Sundelin, 2006).

The Recruitment Grant replaced the regular student aid for those who were eligible, which meant that recipients did not have to accumulate any debt in order to finance their studies. The features of the Recruitment Grant were pretty much identical to those of the regular student aid. To be eligible, one had to be unemployed or at the risk of becoming unemployed and between 25-50 years of age. Finally, one was not allowed to have received any form of student aid in the past five years (Prop. 2001/2002:161). In addition to the eligibility criteria, the grant was also restricted to studies at the compulsory and upper secondary level, and for a maximum of one year.

Figure 1 presents a schematic graph of the Recruitment Grant.

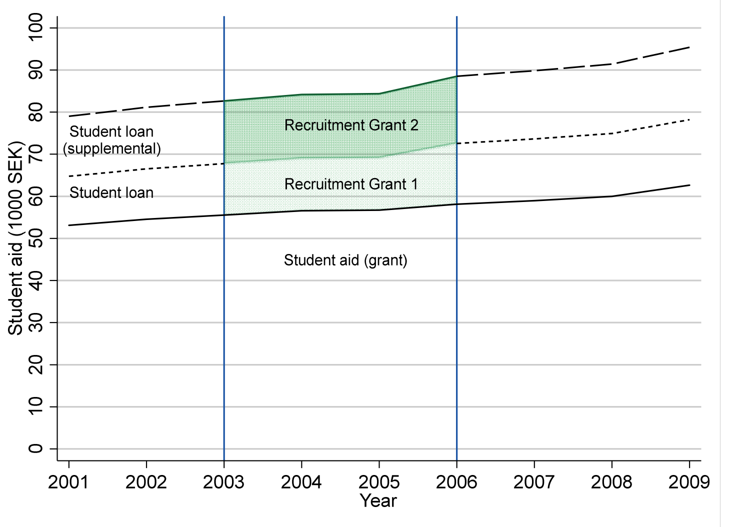

Figure 1. Student aid and the Recruitment Grant

NOTES. The solid black line plots the level of the grants in the second tier of the regular student aid (which everyone in the sample is eligible for), and the dashed lines plots the amount of loans at the regular and supplemental level respectively.

In 2006, the grants in the regular student aid amounted to 58,120 SEK (about $7,100) and the loan level to 14,440 SEK (about $1,800), while the supplemental loan level amounted to 30,400 SEK (about $3,700). Hence, in pure financial terms, the treatment constituted a substitution of grants into loans to the tune of about $3,700 for those with supplemental student aid and $1,800 for the rest.

3. Data

I combine several administrative registers maintained by Statistics Sweden. These include the quinquennial national censuses (FoB); the multi-generation register with parent-child links; the longitudinal database about education, income, and employment (LOUISE); the education register; the graduation register for the 9th grade; the pupil register for Komvux; and the unemployment register (Händel). In this section I will elaborate on the sample construction and describe the data and definitions of key variables.

3.1 Sample selection

I first create a data set that covers the universe of the Swedish population aged 20-58 from 2003 until 2014 and match information on the following variables: year and country of birth, parents' country of birth, gender, employment, unemployment spells, grades, educational attainment, parents' educational attainment, Komvux enrollment, passed credits at Komvux, enrollment in tertiary education, labor market income, and student aid. To create my analysis sample, I select from the data set all individuals aged 25-50 in 2006 and 2007 with less than 12 years of education who are unemployed and did not receive any form of student aid in the previous year. I exclude individuals that received student aid in the previous year address potential bias due to serial correlation in Komvux enrollment. I then define as treated those that did not receive any form of student aid in the past 5 years and hence were eligible for the Recruitment Grant. This leaves me with a sample of 497,029 observations, of which 60,602 belong to the control group and 436,427 belong to the treatment group.[29]

[29] The sample selection process implies that some individuals will appear in the data both in 2006 and 2007. To see this, suppose for example that an individual is 25 years old, unemployed and eligible for the Recruitment Grant in 2006. If this individual remains unemployed in 2007 and does not enroll in adult education in 2006, she will be assigned to the treatment group both in 2006 and 2007. However, I do not exploit the longitudinal structure of the data in this study but instead treat all observations as if from a repeated cross-section. Serial correlation due to repeated observations of the same individual is handled by clustering the standard errors at the individual level throughout the estimation analysis.

3.2 Variable definitions and descriptive statistics

Educational attainment has been converted from levels into years of education.[30] Grades are observed at the 9th grade (the final year of compulsory school) and has been percentile ranked within year of graduation to address the discontinuity caused by the nationwide change in grading system in 1995 (for more details about that reform, see Wikström and Wikström (2005) and Vlachos (2010)).

Information on unemployment spells is gathered from the unemployment register, where entry and exit dates are readily available. I define unemployment on the intensive margin as the number of days in unemployment, and on the extensive margin as a dummy variable equal to 1 if the individual was unemployed at any point during the year. In contrast, information on employment is gathered from LOUISE and only measured at the extensive margin as a dummy variable equal to 1 if the individual was employed at least one hour a week in the month of November (SCB, 2016). Data on Komvux enrollment and the number of passed credits at Komvux comes from the pupil registry for Komvux. The amount of credits associated with a course taken at Komvux is the same as the corresponding course in compulsory or upper secondary school (if applicable), and one week of full-time studies correspond to 20 credits. Labor market incomes have been adjusted to 2014 princes and are based on pre-tax observations of wage earnings, business income, taxable benefits, sick pay and certain parental benefits. Regrettably, I am not able to observe pensions, capital income or income from parental leave.

Descriptive statistics are presented in Table 1 where the analysis sample is juxtaposed with those aged 25-50 in 2006 and 2007 in the population. As expected, the average educational attainment in the sample is much lower than in the population, as is the grade rank average. Looking at previous levels of unemployment, the difference is striking. The sample on average has 528 days of unemployment in the past three years while the population has 132, underscoring the weak labor market attachment of the target population. In terms of ethnicity, only 69 percent of the sample was born in Sweden compared to 83 percent in the population implying a rather stark over representation of immigrants in the sample. Focusing on Komvux enrollment, about 10 percent of the sample enrolled in 2006 or 2007 whereas 4 percent of the population did. In terms of study levels, Komvux students in the sample studied more frequently at the compulsory level as opposed to the upper secondary level compared to the population.

[30] The conversion into years of education is done as follows: old compulsory school = 7 years; current compulsory school = 9 years; short secondary school = 10 years; old vocational secondary school = 11 years; old theoretical/new secondary school = 12 years; vocational tertiary education = 13 years; short tertiary education = 14 years; baccalaureate degree = 15 years; degree of master of one year = 16 years; degree of master of two years = 17 years; Ph. Licentiate = 20 years; Ph. Doctorate = 22 years.

Table 1. Descriptive statistics

	(1) Sample	(2) Population
Age	38.4	37.8
	(7.15)	(7.32)
Woman	0.48	0.49
	(0.50)	(0.50)
Born in Sweden	0.69	0.83
	(0.46)	(0.37)
Years of education	9.90	12.4
	(1.24)	(2.39)
Mother's years of education	9.46	10.6
	(2.37)	(2.95)
Father's years of education	9.30	10.5
	(2.50)	(3.18)
Previous unemployment	528.0	132.2
	(400.1)	(269.9)
Grade rank	19.7	49.6
	(19.2)	(28.8)
Komvux	0.094	0.040
	(0.29)	(0.20)
Primary courses	0.24	0.15
	(0.43)	(0.35)
Secondary courses	0.75	0.84
	(0.43)	(0.37)
Supplemental courses	0.0088	0.015
	(0.093)	(0.12)
Observations	497,029	6,174,388

NOTES. Column 1 reports descriptive statistics in the sample, and column 2 reports descriptive statistics in the population. The grades are observed at the 9th grade (the final year of compulsory school) and have been percentile ranked within graduation year. The course levels are reported conditional on attending Komvux. Previous unemployment is measured as the number of days in unemployment in the past 3 years. Means and standard deviations.

Figure 2 plots the number of students enrolled in Komvux by gender between 2003-2009. Throughout the period, women consistently make up about two-thirds of the enrolled students. Focusing on the general trend, enrollment remained fairly constant until the Recruitment Grant was repealed in 2006, where a rather sharp drop can be seen that continues until 2008. Thus, Komvux enrollment overall has been decreasing over the period which underscores the importance of controlling for the trend.

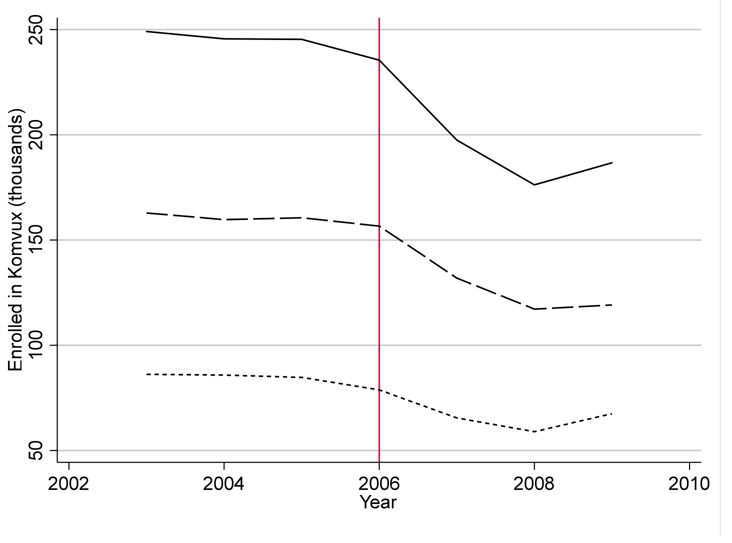

Figure 2. Komvux enrollment over time

NOTES. The y-axis shows the number of students (thousands) enrolled in Komvux. The dotted line plots men and the dashed line plots women, and the solid line plots the total. The vertical red line is drawn at 2006, when the Recruitment Grant was repealed.

Figure 3 plots the enrollment shares at different study levels.

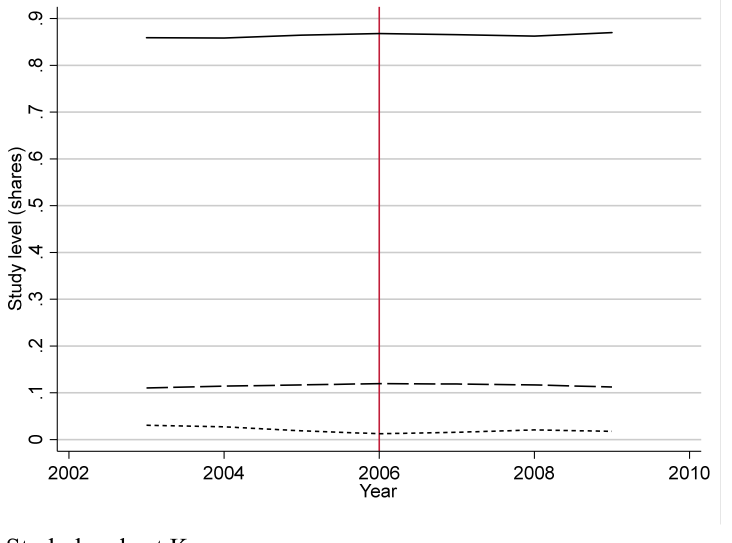

Figure 3. Study levels at Komvux

NOTES. The y-axis shows share of students enrolled at different levels in Komvux. The solid line plots the upper secondary level, the dashed line plots the compulsory level, and the dotted line plots the supplemental level. The vertical red line is drawn at 2006, when the Recruitment Grant was repealed.

The repeal of the Recruitment Grant does not seem to have influenced the relative demand for courses at different levels, as the clear majority studies at the upper secondary level throughout the period.

Figure 4 plots the average number of semesters students enroll at Komvux, conditional on enrolling at least one semester. The solid black line corresponds to the total number of semesters and varies between 4.8 and 5.3 over the period. The dashed blue line corresponds to the number of semesters with passed credits and is roughly constant over the period at 2.5 semesters.

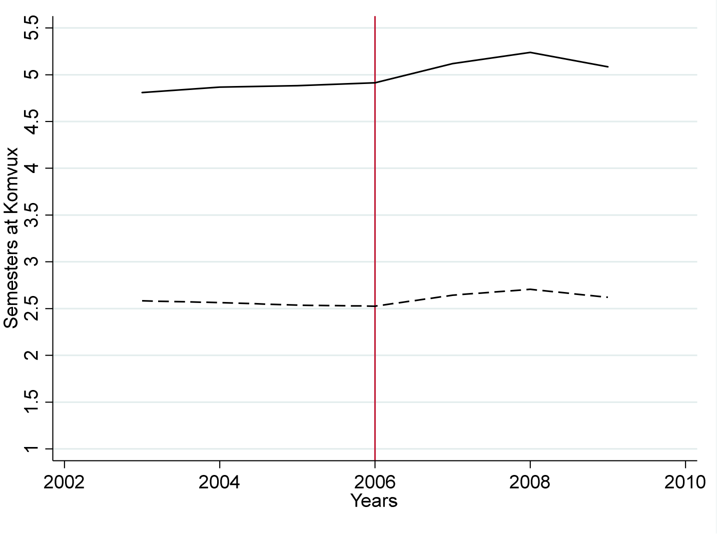

Figure 4. Semesters at Komvux

NOTES. The y-axis shows the average number of semesters a student stays at Komvux conditional on enrollment. The solid black line plots the average including all semesters, and the dashed blue line plots the average number of semesters with passed credit. The vertical red line is drawn at 2006, when the Recruitment Grant was repealed.

The striking gap between the total number of semesters and the number of semesters with passed credits suggests that there's a substantial amount of uncertainty associated with the enrollment decision.

4. Estimation strategy

Student aid at the national level often selects on socioeconomic factors that are negatively related to academic achievement, such as an economically disadvantaged family background (see Seftor and Turner (2002); Kane (2003); Bettinger (2004); Alon (2007); Glocker (2011)). On the other hand, eligibility for grants and scholarships at the state and school level often selects on academic merit, which is positively related to future academic achievement (see

Dynarski (2000); Van der Klaauw (2002); Dynarski (2008); Goodman (2008); Scott-Clayton (2011)). This means that a simple correlation of student aid on educational outcomes will suffer from selection bias that confounds the causal effect of the aid. I address this by estimating the effect of the Recruitment Grant in a difference-in-differences framework. I clarify the causal interpretation of the estimand that this framework identifies in the next section and describe the estimation procedure in detail in section 4.2.

4.1 The identified estimand

In a randomized controlled experiment, subjects do not always comply with the treatment that they're assigned to. Depending on assigned treatment status, an observation will belong to one of four groups. First, compliers are participants that are treated if assigned to the treatment group and non-treated if assigned to the control group. Second, always-takers are participants that are treated irrespective of assigned treatment status. Third, never-takers are participants that are non-treated irrespective of assigned treatment status. Finally, the fourth group are defiers who are non-treated if assigned to the treatment group and treated if assigned to the control group. In this study, the non-compliance comes from the individuals who were eligible for the Recruitment Grant in 2006 but didn't take it.

In an experiment non-compliance, one way to proceed is to estimate the treatment effect by comparing those who actually were treated with the those that were not. But if those that did not comply with their assigned treatment status are systematically different from those that did along dimensions that are unobserved but related to the outcome, the estimate will be biased. Another way to proceed is to ignore the non-compliance and estimate the treatment effect by comparing those assigned to treatment group with those assigned to the control group. This amounts to estimating the so called the intention-to-treat-effect (ITTE), which doesn't estimate the average treatment effect (ATE) but rather the effect of offering treatment (Imbens and Angrist, 1994; Angrist and Pischke, 2008). However, if the non-compliance only occurs in the treatment group, it is possible to retrieve the ATE for the compliers if one is willing to assume that the outcome of the non-compliers is the same regardless of their assigned treatment status, i.e. that the non-compliers are never-takers.[31]

In this study, the assumption that the non-compliers are never-takers amounts to assuming that those who were eligible for the Recruitment Grant in 2006 but didn't take it, did so for reasons that are unrelated to the fact that it's been at least five years since they last received student aid. I don't believe

[31] The average treatment effect for the compliers is called the local average treatment effect (LATE) because it estimates the treatment effect "locally" at the margin that separates the treatment group from the control group. A LATE thusly obtained is an instrumental variable estimate where the compliance rate is the first stage, and the reduced form effect is equal to the ITTE (Angrist and Pischke, 2008).

that's a credible assumption, and therefore the analysis will be limited to identifying the ITTE whose causal interpretation is the effect of discontinuing to offer the Recruitment Grant to the target population.

4.2 Difference-in-differences estimation

To identify the causal effect of the Recruitment Grant on educational attainment and labor market outcomes, simply estimating a correlation between an outcome and an eligibility dummy is inadequate because individuals who are eligible for the Recruitment Grant are likely to be different from those who are not eligible in ways that also affects the outcome. To overcome the problem of unobservable differences between the treatment group and the control group, I exploit the repeal of the Recruitment Grant in the end of 2006 to difference out the omitted variables in a DD model that estimates the difference between the treated and the control group before and after treatment exposure (where treatment refers to the repeal of the Recruitment Grant at the end of 2006).

The eligibility criteria at the student aid margin combined with the introduction of the reform in 2003 and its repeal in 2006 suggests that two DD-models are possible: differences between treated and controls before and after the introduction in 2003 as well as before and after the repeal in 2006. However, I only exploit the repeal of the reform in 2006 to identify the causal effect for two reasons. First, the controls at the introduction in 2003 are likely to switch into treatment in later years which would obfuscate the interpretation of estimated effects that do not follow immediately after the introduction, such as subsequent unemployment status and effects on income. Second, a massive expansion of Komvux called the Knowledge Lift formally ended in 2001 but had transition rules that lasted until June 1st 2003, and hence ran parallel with the Recruitment Grant for a while (see Albrecht et al. (2008) for a description of the Knowledge Lift). For these reasons, I only exploit the repeal in 2006 to identify the effect of the reform.

The DD models are estimated using OLS regression with standard errors clustered at the individual level. The generic model is defined as:

$$\mathrm{Y}_{\mathrm{iat}} = \alpha + \phi_{\mathrm{a}} + \tau_{\mathrm{t}} + \delta \mathrm{D}_{\mathrm{at}} + \varepsilon_{\mathrm{iat}} \tag{1}$$

where Y_{iat} denotes and outcome for observation *i* in group *a* observed in period *t*, ϕ_{a} is a time-invariant treatment effect, τ_{t} is a year fixed-effect hat is constant across eligibility status, and D_{at} is a dummy for treated observations in the post-period. The parameter of interest is δ, which estimates the difference between the between the treated and control groups before and after the repeal.

The key identifying assumption is that in the absence of treatment, the outcome trend for the treatment group would have been parallel with trend for the control group. Another way to state this assumption is that omitted variables are either time-invariant group attributes, or time-varying factors that are group invariant. One way to assess the parallel trends assumption is to plot the outcome trends for the treated and control group over time. Ideally, the trends should be parallel in the pre-treatment period and then diverge in the post-period. Figure 5 plots Komvux enrollment shares and Figure 6 plots passed credits between 2003-2009.

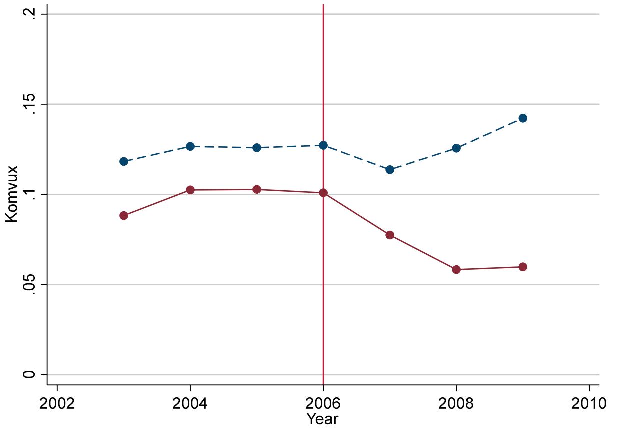

Figure 5. Parallel trends for Komvux enrollment

NOTES. Komvux enrollment rates and credits for treated and controls at the student aid margin. The solid red line plots the treated group and the dashed blue line plots the control group. The vertical red line marks the repeal of the Recruitment Grant in 2006.

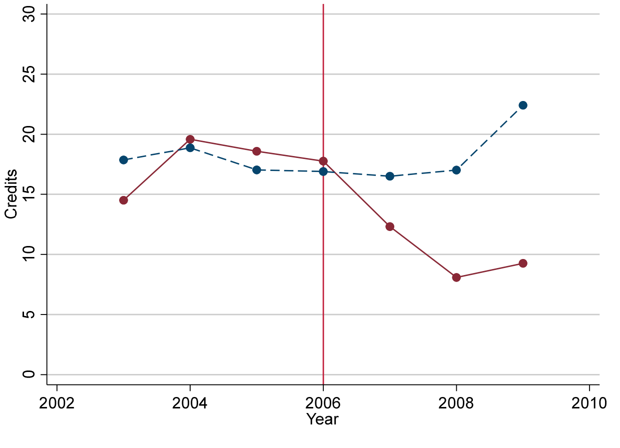

Figure 6. Parallel trends for Komvux credits

In both figures, the trends align quite nicely in the pre-treatment period and there isn't much volatility. After the repeal of Recruitment Grant in 2006, enrollment and credits drop sharply in to the treatment group relative to the control group, which is encouraging for the identifying strategy.

Another way to assess the plausibility of the parallel trends assumption is to gauge the similarity of the treatment and control groups prior to treatment. The idea is that if the groups are similar to each other, it is less likely that selection bias is differentially affecting the outcome trends. Table 2 reports pre-treatment descriptive statistics for the treatment and control groups on selected covariates.

Table 2. Pre-treatment descriptive statistics for treated and controls

	(1) Treated	(2) Control
Age	38.8	35.4
	(6.94)	(7.45)
Woman	0.46	0.62
	(0.50)	(0.49)
Born in Sweden	0.72	0.68
	(0.45)	(0.47)
Years of education	9.95	9.96
	(1.23)	(1.10)
Mother's years of education	9.37	9.86
	(2.34)	(2.48)
Father's years of education	9.20	9.74
	(2.48)	(2.60)
Previous unemployment	498.4	563.7
	(402.0)	(360.3)
Grade rank	20.3	19.8
	(19.3)	(19.7)
Observations	227,561	34,537

NOTES. Column 1 reports pre-treatment descriptive statistics for the treated group and column 2 for the control group. The grades are observed at the 9th grade (the final year of compulsory school) and have been percentile ranked within graduation year. Previous unemployment is measured as the number of days in unemployment in the past 3 years. Means and standard deviations.

The groups appear to be quite similar except for gender composition and previous unemployment. Women make up 62 percent of the control group but only 46 percent of the treated group, which means that the student aid margin binds harder for women than for men. Previous unemployment is also considerably higher in the control group at an average of 564 days in the past three years, compared to 498 in the treated group.

To assess whether these differences are problematic for the identification, I combine the DD framework with a propensity score weighting strategy proposed by Stuart et al. (2014). To see how this works, let G_i denote group membership for observation i so that:

$$G_i = \begin{cases} 1 & if\ treated\ in\ 2006 \\ 2 & if\ a\ control\ in\ 2006 \\ 3 & if\ treated\ in\ 2007 \\ 4 & if\ a\ control\ in\ 2007 \end{cases}$$

Propensity scores that reflect the probability of group belonging to group *j* can then be estimated in a multinomial logit model where the probability of group affiliation is regressed onto a vector of covariates. The response probabilities are given by:

$$\mathrm{P(g_i = j|x_i)} = \frac{e^{x_i \beta_j}}{1 + \sum_{h=2}^{4} e^{x_i \beta_h}} \tag{2}$$

$$\mathrm{P(g_i = 1|x_i)} = \frac{1}{1 + \sum_{h=2}^{4} e^{x_i \;\; h}}$$

where ${}_{\mathrm{i}}$ is a vector of covariates and $\boldsymbol{\beta}$ is the associated vector of slope coefficients. ${}_{\mathrm{i}}$ contains a gender dummy, a dummy for being born in Sweden, a dummy for the father being born in Sweden, a dummy for the mother being born in Sweden, a second degree polynomial in age, and a second degree polynomial in the number of days of unemployed in the past 3 years. Following Stuart et al. (2014), each observation is then assigned a weight equal to the probability of being in the pre-treatment treated group relative to the probability of being in the group that it's actually in. The weight for observation *i* in group *j* is thus defined as:

$$\mathrm{w_i} = \frac{P(g_i = 1|\boldsymbol{x}_i)}{P(g_i = j|\boldsymbol{x}_i)} = \frac{1}{e^{x_i \boldsymbol{\beta}_j}} \tag{3}$$

Hence, $\mathrm{w_i}$ is equal to the inverse of the probability that observation *i* is in the pre-treatment treated group relative to the group it is actually in. Intuitively, observations that are similar to those in the pre-treatment treated group and dissimilar to their own group will receive a large weight, and vice versa. The procedure can therefore be thought of as weighting the covariate distributions of observations in groups 2-4 to reflect the covariate distribution of observations in the pre-treatment treated group. Thus, by fitting a weighted DD model using the weights defined in Equation (3) I can obtain a consistent estimate of the treatment effect even in the presence of selection bias based on the covariates in ${}_{i}$.

Finally, since I have access to data for several years before and after the reform, I will perform a Granger-type causality test by estimating leads and lags of the treatment. When there's no reason to expect any anticipatory effect, the leads in such a test are essentially placebo reforms that shouldn't influence the outcome while the lags will be informative about the effect dynamics. As before, let Y_{iat} denote an outcome for observation i in group a observed in period t. The model is then defined as:

$$Y_{iat} = \alpha + \phi_a + \tau_t + \delta D_{at} + \sum_{s=1}^{S} D_{at+s}\gamma_s + \sum_{m=1}^{M} D_{at-m}\lambda_m + u_{iat} \tag{4}$$

where δ captures the immediate treatment effect and λ_m estimates the effect m years after the treatment occurred. The placebo treatment effects are captured by γ_s which estimates the effect of the treatment s years before it occurred. Given the context of the repeal - which involved an unpredictable general election result in September of 2006 followed by a surprisingly quick shut down of the reform - there's no reason to expect anticipatory effects.

5. Results

Table 3 presents DD estimates of the effect of repealing the Recruitment Grant on Komvux enrollment. The repeal reduced enrollment by 1 percentage point in 2007 and by 1.3 and 1.2 percentage points in 2008 and 2009, which implies that the effects of the repeal were not transitory.

Table 3. DD estimates of Komvux enrollment

	(1) 2007	(2) 2008	(3) 2009
Komvux enrollment	-0.010***	-0.013***	-0.012***
	(0.003)	(0.002)	(0.002)
Before repeal	262,098	259,392	258,331
Control group	0.127	0.0892	0.0817
Treated	0.101	0.0644	0.0601
Difference	-0.0263	-0.0485	-0.0449
After repeal	234,931	233,508	232,401
Control group	0.114	0.113	0.105
Treated	0.0775	0.0532	0.0491
Difference	-0.0362	-0.0360	-0.0326

NOTES. Difference-in-differences estimates of enrolling in Komvux. The sample is restricted to unemployed individuals in 2006 and 2007 with at most 11 years of education. The control group is defined as those who received student aid within the last 2-5 years but not the previous year, and the treatment group is defined as those who have not received student aid within the last 5 years. Standard errors are clustered at the level of the individual.

If the effect is symmetric, i.e. if we assume that the introduction of the grant increased enrollment by 1 percentage point, a back-of-the-envelope calculation suggests that a $1,000 offer of annual student aid will increase enrollment by about 0.5 percentage points.[32] By comparison, Dynarski (2003) finds that a $1,000 offer of annual student aid increases college enrollment by 3.6 percentage points in the U.S., while Nielsen et al. (2010) estimates that the corresponding effect is 1.35 percentage points in Denmark. However, gauging effect sizes measured in percentage points can be misleading if there are large differences in enrollment rates across the samples. Indeed, the college enrollment rate in the final sample used by Nielsen et al. (2010) is 39 percent, and the college enrollment rate in the pre-treatment treatment group in Dynarski (2003) is 50 percent: much higher than the 10 percent enrollment rate in my sample. Converted into "percent of the enrollment rate" effects, the effect of offering $1,000 dollars of student aid is about 3.5 percent in Nielsen et al. (2010) and about 7.2 percent in Dynarski (2003), while I find that it's about 5 percent. Hence, the effect of student aid on enrollment at the compulsory and upper secondary level in Sweden is very similar to the effects of student aid on enrollment at the college level in the U.S and Denmark, despite the institutional differences between the countries.

Though the repeal of the Recruitment Grant decreased enrollment, it is possible that those who chose not to enroll as a consequence of the repeal would've failed and dropped out anyway, and that particularly talented students were unaffected by the prospect of accumulating debt in order to study. However, that does not seem to be the case. Table 4 presents DD estimates of the effect on the number passed credits at Komvux.

Table 4. DD estimates of Komvux credits

	(1) 2007	(2) 2008	(3) 2009
Komvux credits	-5.057***	-2.688***	-3.328***
	(0.789)	(0.633)	(0.653)
Before repeal	262,098	259,392	258,331
Control group	16.90	17.68	16.95
Treated	17.77	11.86	14.09
Difference	0.864	-8.505	-5.409
After repeal	234,931	233,508	232,401
Control group	16.51	21.94	22.83
Treated	12.32	13.44	11.54
Difference	-4.193	-5.817	-8.737

NOTES. Difference-in-differences estimates of being unemployed. The sample is restricted to unemployed individuals in 2006 and 2007 with at most 11 years of education. The control group is defined as those who received student aid within the last 2-5 years but not the previous year, and the treatment group is defined as those who have not received student aid within the last 5 years. Standard errors are clustered at the individual level.

[32] To arrive at 0.4 percent, I assume that only 10 percent of the sample was eligible for the supplemental student aid. Although it's true that 40 percent of all Komvux students in 2006 received the supplemental aid, it's highly unlikely that such a large share of the analysis sample were eligible considering the vast amount of previous unemployment (see Table 2).

The number of passed credits decreased by about 5.05 in 2007 relative to a pre-treatment average of 17.8, amounting to a 28 percent decrease in the number of passed credits for the target population. Comparing this effect to the 10 percent decrease in enrollment, if anything, the repeal had a greater impact on more capable students.

Focusing on labor market outcomes, Tables 5 and 6 reports the estimated effects on employment and unemployment rates.

Table 5. DD estimates of unemployment

	(1) 2008	(2) 2009
Unemployment	0.032***	0.021***
	(0.003)	(0.003)
Before repeal	259,392	258,331
Control group	0.603	0.630
Treated	0.573	0.541
Difference	-0.0300	-0.0174
After repeal	233,508	232,401
Control group	0.723	0.559
Treated	0.725	0.634
Difference	0.00153	0.00355

NOTES. Difference-in-differences estimates of being unemployed. The sample is restricted to unemployed individuals in 2006 and 2007 with at most 11 years of education. The control group is defined as those who received student aid within the last 2-5 years but not the previous year, and the treatment group is defined as those who have not received student aid within the last 5 years. Standard errors are clustered at the individual level.

Table 6. DD estimates of employment

	(1) 2008	(2) 2009
Employment	0.002	0.005
	(0.003)	(0.003)
Before repeal	259 392	258 331
Control group	0.633	0.582
Treated	0.708	0.659
Difference	0.0756	0.0817
After repeal	233 508	232 401
Control group	0.579	0.527
Treated	0.656	0.609
Difference	0.0776	0.0772

Difference-in-differences estimates of being employed. The sample is restricted to unemployed individuals in 2006 and 2007 with at most 11 years of education. The control group is defined as those who received student aid within the last 2-5 years but not the previous year, and the treatment group is defined as those who have not received student aid within the last 5 years. Standard errors are clustered at the individual level.

It is important to remember that unemployment is a dummy variable equal to 1 if the individual was unemployed at any point during the year, and that employment is a dummy variable equal to 1 if the individual was employed at least one hour a week in the month of November. Hence, it's possible for an individual to be both employed and unemployed in the same year.

Strikingly, the repeal of the Recruitment Grant caused a 3.2 percentage point increase in the unemployment rate in the target population in 2008 and a 2.1 percentage point increase in 2009, while the employment rate was unaffected throughout the period. Hence, the repeal of the Recruitment Grant in 2006 had adverse effects on both the educational attainment and labor market outcomes for the target population.

5.1 Robustness analysis

As previously mentioned, the key identifying assumption for causal inference in the DD framework is the assumption of parallel outcome trends for the treated and control group had the treatment not been implemented. One potential threat to that assumption comes from selection into treatment, which can be assessed by gauging the similarity of the treatment and control group. To that end, Table 7 reports propensity score weighted pre-treatment descriptive statistics, using the weights from Equation (3).

Table 7. Propensity score weighted pre-treatment descriptive statistics

	(1) Treated	(2) Control
Age	38.8	38.6
	(6.94)	(6.91)
Woman	0.46	0.48
	(0.50)	(0.50)
Born in Sweden	0.72	0.70
	(0.45)	(0.46)
Years of education	9.95	9.91
	(1.23)	(1.18)
Mother's years of education	9.37	9.57
	(2.34)	(2.46)
Father's years of education	9.20	9.47
	(2.48)	(2.61)
Previous unemployment	498.4	518.7
	(402.0)	(402.4)
Grade rank	20.3	19.4
	(19.3)	(19.7)
Observations	227,561	34,537

NOTES. All observations are weighted by propensity scores that reflect the probability of being in the pre-treatment treated group relative to the group that the observation is actually in. The grades are observed at the 9th grade (the final year of compulsory school) and have been percentile ranked within graduation year. Previous unemployment is measured as the number of days in unemployment in the past 3 years. Means and standard deviations.

The differences in gender composition and previous unemployment between the groups have almost been eliminated as a result of the weighting strategy. The control group is now comprised of 48 percent women compared to 46 percent in the treatment group. As for previous unemployment, the control group now has an average of 519 days of unemployment in the past three years compared to 498 days for the treatment group. Table 8 presents the weighted DD estimates. The adverse effects on enrollment and unemployment rates are sustained, while the effect on credits is still negative in 2007 but drops to zero in 2008 and 2009. Notably, the up-weighting of men and individuals with relatively low previous unemployment in the control group has resulted in a positive effect on employment rates in both 2008 and 2009.

Table 8. Propensity score weighted DD estimates

	(1) 2007	(2) 2008	(3) 2009
Komvux enrollment	-0.008***	-0.007***	-0.005***
	(0.003)	(0.002)	(0.002)
Komvux credits	-3.606***	-0.952	-0.854
	(0.761)	(0.628)	(0.631)
Unemployment	-	0.011***	0.009**
		(0.004)	(0.004)
Employment	-	0.007*	0.010**
		(0.004)	(0.004)
Observations	497,029	492,900	490,732

NOTES. All observations are weighted by propensity scores that reflect the probability of being in the pre-treatment treated group relative to the group that the observation is actually in. Column 1 reports pre-treatment descriptive statistics for treated group and column 2 reports pre-treatment descriptive statistics for the control group. The grades are observed at the 9th grade (the final year of compulsory school) and have been percentile ranked within graduation year. Previous unemployment is measured as the number of days in unemployment in the past 3 years. Means and standard deviations.

A straightforward interpretation of these changes is that the treatment effect varies across subgroups. To explore that possibility, a heterogeneity analysis will be implemented in the next section.

Table 9 reports the estimated leads and lags from the Granger-type causality test defined by Equation (4). As expected, the placebo treatments estimated by the leads in 2005 and 2006 are all zero. In contrast, the lags indicate that the adverse effects of repealing the grant increased between 2007 and 2009. This isn't very surprising since the Swedish economy at the time was struggling to cope with the financial crises that originated in the U.S. subprime mortgage market in 2007. Nevertheless, neither the propensity score weighted estimates nor the Granger-type causality test indicate any immediate reasons for concern.

Table 9. Effect dynamics and treatment placebos

	(1) Komvux enrollment	(2) Komvux credits
$Repeal_{t+2}$	0.001	0.844
	(0.002)	(0.657)
$Repeal_{t+1}$	-0.002	0.158
	(0.002)	(0.708)
$Repeal_{t}$	-0.012***	-4.899***
	(0.003)	(0.768)
$Repeal_{t-1}$	-0.043***	-9.640***
	(0.003)	(0.819)
$Repeal_{t-2}$	-0.058***	-13.855***
	(0.003)	(0.890)
Observations	1,502,782	1,502,782

NOTES. Leads and lags difference-in-differences estimates of Komvux enrollment. t denotes 2007 - the first year after the repeal of the Recruitment Grant. The sample is restricted to unemployed individuals between 2004-2009 with at most 11 years of education. The control group is defined as those who received student aid within the last 2-5 years but not the previous year, and the treatment group is defined as those who have not received student aid within the last 5 years. Standard errors are clustered at the individual level.

5.2 Heterogeneity analysis

To investigate whether the effect of repealing the Recruitment Grant varies across subgroups of the target population, the sample is split across gender, parental education and ethnicity. Parental education is defined as high if at least one parent has 3 years of upper secondary education or more and defined as low if both parents have less than 3 years of upper secondary education. “Swedish ethnicity” is defined as having been born in Sweden with at least one parent who were also born in Sweden, and “non-Swedish ethnicity” is defined as either having been born abroad or have parents that were both born abroad.

Table 10 presents DD estimates across gender. As expected, the decrease in the enrollment rate and the number of passed credits at Komvux is more pronounced for women than men in the target population. The causes of these gender differences are difficult to disentangle. Stenberg et al. (2014) also finds a gender gap when estimating the returns to Komvux enrollment among older workers and suggest that it stems from differences in the underlying reasons for enrollment after observing that male participation in their sample is associated with increased levels of sick-leave benefits prior to enrollment, whereas female enrollment appears to be driven by a “latent demand” for education.

Table 10. DD estimates by gender

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
	2007	2008	2009	2007	2008	2009
Enrollment	-0.026***	-0.015***	-0.015***	0.003	-0.010***	-0.008***
	(0.004)	(0.003)	(0.002)	(0.004)	(0.003)	(0.002)
Credits	-9.128***	-9.128***	-2.692***	-3.030***	-2.049**	-2.049**
	(1.154)	(1.154)	(0.928)	(0.750)	(0.818)	(0.818)
Unemployment	-	0.027***	0.017***	-	0.034***	0.027***
		(0.004)	(0.004)		(0.005)	(0.005)
Employment		0.001	0.007*		0.004	0.002
		(0.004)	(0.004)		(0.005)	(0.005)
Observations	240,101	238,694	238,016	256,928	254,206	252,716

NOTES. Difference-in-differences estimates of enrolling in Komvux by gender. Standard errors are clustered at the individual level.

Turning to differences across parental education, Table 11 presents the DD estimates. The decrease in enrollment and passed credits is larger for the subgroup with high parental education.

Table 11. DD estimates by parental education

	High parental education			Low parental education		
	(1)	(2)	(3)	(4)	(5)	(6)
	2007	2008	2009	2007	2008	2009
Enrollment	-0.013**	-0.020***	-0.017***	-0.009***	-0.010***	-0.011***
	(0.005)	(0.004)	(0.004)	(0.003)	(0.002)	(0.002)
Credits	-7.676***	-4.340***	-4.947***	-4.260***	-2.112***	-2.862***
	(1.666)	(1.360)	(1.386)	(0.896)	(0.714)	(0.739)
Unemployment	-	0.032***	0.026***	-	0.031***	0.019***
		(0.006)	(0.006)		(0.003)	(0.003)
Employment	-	0.019***	0.023***	-	-0.001	0.000
		(0.006)	(0.006)		(0.003)	(0.003)
Observations	95,846	95,182	94,863	401,138	397,718	395,968

NOTES. Difference-in-differences estimates of enrolling in Komvux by parental education. Parental education is defined as high if at least one parent has 3 years of upper secondary education or more and defined as low if both parents have less than 3 years of upper secondary education. Standard errors are clustered at the individual level.

Also, there is a sizable and significant increase in the employment rate for the group with high parental education. A plausible explanation for these somewhat counter-intuitive results is that individuals with high parental education have access to high quality social networks that improves their prospects on the labor market. If that is the case, they'd have an above average opportunity cost of enrolling in adult and consequently be more responsive to changes in the cost of education. Furthermore, because they have relatively good prospects on the labor market, it is conceivable that they'd experience an increase in the probability of subsequently employment after being "pushed" out of Komvux enrollment.

Finally, Table 12 presents estimates across ethnicity. Overall, the adverse effects of the repeal on enrollment, credits and unemployment are slightly higher for the subgroup with a Swedish ethnicity. However, there's a striking difference between groups in how the repeal affected subsequent employment rates.

Table 12. DD estimates by ethnicity

	Swedish ethnicity			Non-Swedish ethnicity		
	(1) 2007	(2) 2008	(3) 2009	(4) 2007	(5) 2008	(6) 2009
Enrollment	-0.011***	-0.014***	-0.016***	-0.013***	-0.011***	-0.007**
	(0.003)	(0.002)	(0.002)	(0.005)	(0.003)	(0.003)
Credits	-5.188***	-3.398***	-3.556***	-4.575***	-2.402***	-3.151***
	(0.932)	(1.089)	(0.793)	(1.403)	(0.769)	(1.123)
Unemployment	-	0.032***	0.019***	-	0.025***	0.020***
		(0.004)	(0.004)		(0.005)	(0.005)
Employment	-	0.019***	0.019***	-	-0.016***	-0.006
		(0.004)	(0.004)		(0.005)	(0.005)
Observations	324,785	323,070	322,053	172,244	169,830	168,679

NOTES. Difference-in-differences estimates of enrolling in Komvux by ethnicity. Swedish ethnicity is defined as born in Sweden with at least one parent also born in Sweden. Non-Swedish ethnicity is defined as born abroad or both parents born abroad. Standard errors are clustered at the individual level.

Those with a Swedish ethnicity experienced an increase in the probability of being employed similar to the subgroup with high parental employment. These two subgroups overlap to some extent which makes it difficult to assess which of the characteristics that are driving the results. [33] It is of course also possible that parental education and ethnicity are both important moderators of the treatment effects. The fact that those with a non-Swedish ethnicity is the only subgroup that experienced a decrease in employment following the repeal of the grant suggests that Komvux schooling is of greater importance for their success on the labor market than for any of the other subgroups.

[33] 86 percent of those with high parental education also have a Swedish ethnicity. By comparison, the share of the sample with high parental education is 20 percent and the share with a Swedish ethnicity is 65 percent.

5.3 Long term effects

I present estimates across the subgroups defined in the previous section on three long term outcomes: the probability of enrolling in tertiary education between 2007-2014; the number of days in unemployment between 2012-2014; and the average labor market income between 2012-2014. Focusing first on enrollment in tertiary education, Tables 13 and 14 presents the DD estimates.

Table 13. DD estimates on enrollment in tertiary education by gender

	(1) Pooled	(2) Women	(3) Men
Tertiary education	-0.002	-0.002	-0.003
	(0.001)	(0.002)	(0.002)
Before repeal	252,722	122,702	130,020
Control group	0.0460	0.0528	0.0346
Treated	0.0192	0.0287	0.0109
Difference	-0.0268	-0.0241	-0.0237
After repeal	226,796	111,628	115,168
Control group	0.0473	0.0531	0.0377
Treated	0.0184	0.0266	0.0110
Difference	-0.0289	-0.0265	-0.0267

NOTES. Difference-in-differences estimates of enrolling in tertiary education. Enrollment is observed until 2014. Standard errors are clustered at the individual level.

Table 14. DD estimates on tertiary education by parental education and ethnicity

	(1) High parental education	(2) Low parental education	(3) Swedish ethnicity	(4) Non-Swedish ethnicity
Tertiary education	-0.008**	0.000	-0.002	-0.003
	(0.004)	(0.001)	(0.002)	(0.002)
Before repeal	49,767	202,955	171,330	81,392
Control group	0.0749	0.0369	0.0517	0.0362
Treated	0.0334	0.0158	0.0208	0.0153
Difference	-0.0415	-0.0211	-0.0330	-0.0209
After repeal	43,454	183,342	145,175	81,621
Control group	0.0832	0.0356	0.0538	0.0381
Treated	0.0339	0.0149	0.0210	0.0142
Difference	-0.0493	-0.0207	-0.0308	-0.0239

NOTES. Difference-in-differences estimates of enrolling in tertiary education. Enrollment is observed until 2014. Standard errors are clustered at the individual level.

The only subgroup whose enrollment in tertiary education was affected by the repeal were those with high parental education, which experienced a decrease of 0.8 percentage points. Given the already low educational level of the target population and their poor position in the grade rank distribution, it is not surprising that the repeal of the grant did not have an effect on enrollment in tertiary education for any of the other subgroups.

Tables 15 and 16 presents DD estimates on long term unemployment spells.

Table 15. DD estimates on unemployment spells by gender

	(1) Pooled	(2) Women	(3) Men
Unemployment spell	27.208***	21.215***	33.096***
	(2.649)	(3.356)	(4.362)
Before repeal	252,722	122,702	130,020
Control group	416.6	392.7	489.7
Treated	403.7	444.5	414.4
Difference	-12.93	-1.426	-9.073
After repeal	226,796	111,628	115,168
Control group	449.2	424.7	456.6
Treated	463.5	391.3	480.7
Difference	14.28	19.79	-42.17

NOTES. Difference-in-differences estimates of the number of days in unemployment between 2012-2014. Standard errors are clustered at the individual level.

On average, the repeal of the Recruitment Grant increased the number of days in unemployment between 2012-2014 by 27.2 days for the target population. However, the estimated effects vary considerably across subgroups.

Table 16. DD estimates on unemployment spells by parental education and ethnicity

	(1) High parental education	(2) Low parental education	(3) Swedish ethnicity	(4) Non-swedish ethnicity
Unemployment spell	37.166***	23.162***	30.279***	19.267***
	(5.288)	(3.051)	(3.378)	(4.271)
Before repeal	49,767	202,955	171,330	81,392
Control group	364.0	433.1	438.8	464.1
Treated	354.9	415.2	451.9	435.8
Difference	-9.131	5.228	13.11	1.158
After repeal	43,454	183,342	145,175	81,621
Control group	389.6	468.6	406.1	434.6
Treated	417.6	473.9	389.0	484.5
Difference	28.03	-17.93	-17.17	20.42

NOTES. Difference-in-differences estimates of the number of days in unemployment between 2012-2014. Standard errors are clustered at the individual level.

For men, the repeal increased unemployment between 2012-2014 by 33.1 days and for women by 21.2 days. For those with high parental education, I find that unemployment increased by 37.2 days and for those with low parental education by 23.2 days. Finally, I find that unemployment increased by 30.3

days for those with a Swedish ethnicity and by 19.3 days for those with a non-Swedish ethnicity.

Turning finally to the effect of the repeal on income, Tables 17 and 18 presents the DD estimates.

Table 17. DD estimates on income by gender

	(1) Pooled	(2) Women	(3) Men
Income	-2,495***	-34	-4,059 ***
	(777)	(926)	(1,376)
Before repeal	252,722	122,702	130,020
Control group	167,629	166,152	170,099
Treated	196,007	166,978	194,294
Difference	28,378	13,096	40,395
After repeal	226,796	111,628	115,168
Control group	155,417	153,883	157,958
Treated	181,300	179,282	210,494
Difference	25,883	13,129	36,336

NOTES. Difference-in-differences estimates on pre-tax average labor market income between 2012-2014. Standard errors are clustered at the individual level.

Table 18. DD estimates on income by parental education and ethnicity

	(1) High parental education	(2) Low parental education	(3) Swedish ethnicity	(4) Non-swedish ethnicity
Income	-1,948	-2,416***	-312	-2,295*
	(1,703)	(871)	(992)	(1,248)
Before repeal	49,767	202,955	171,330	81,392
Control group	174,006	153,434	170,745	152,376
Treated	213,343	191,925	195,852	154,702
Difference	39,336	23,880	38,324	2,327
After repeal	43,454	183,342	145,175	81,621
Control group	161,495	165,629	157,527	162,294
Treated	198,883	177,314	209,381	166,915
Difference	37,388	26,296	38,636	4,621

Difference-in-differences estimates on pre-tax average labor market income between 2012-2014. Standard errors are clustered at the individual level.

The repeal of the Recruitment Grant decreased average labor market income between 2012-2014 by 2,495 SEK (about $280). Strikingly, women's incomes were not affected by the repeal while men's incomes decreased by 4,056 SEK (about $450). This result is in line with the observation that women (men) sort into low (high) wage occupations. The point estimate of the effect on income for those with high parental education is negative but unfortunately lacks precision. However, for those with low parental education the repeal decreased labor market income by 2,416 SEK (about $270). Across ethnicity, we can see that the repeal had no effect on incomes for those with Swedish ethnicity while those with non-Swedish ethnicity experienced a decrease of 2,295 SEK (about $255).

6. Conclusion

In 2003, the Recruitment Grant was introduced to increase enrollment in adult education among unemployed adults with incomplete upper secondary education. The grant replaced the loans offered to students by the state in the national student aid system, which amounted to a about $3,700 for those that were eligible for supplemental aid and about $1,800 for the rest.

In this paper, I have estimated the causal effects of the Recruitment Grant on educational attainment and subsequent labor market outcomes exploiting the repeal of the grant in 2006 and an eligibility criterion that recipients were not allowed to have received any other form of student aid in the past five years in a difference-in-differences (DD) framework. Furthermore, the grant was only given for studies at the compulsory or upper secondary level, and for a maximum of one year.

I find that the repeal had adverse effects both on educational attainment and labor market outcomes for the target population. The enrollment rate in adult education by 10 percent for the target population relative to the pre-treatment average, and the number of passed credits by 28 percent. In terms of subsequent labor market outcomes, the repeal increased the unemployment rate by 3.2 percentage points in the target population but had no effect on the employment rate. I also find that the repeal had adverse effects on long term labor market outcomes. The average number of days in unemployment between 2012 and 2014 increased by 27.2 days for the target population, and the average labor market income between 2012 and 2014 decreased by 2,495 SEK (about $280). However, the repeal had no effect on enrollment in tertiary education for the target population overall.

I also find that the effect of the repeal varies considerably across gender, parental education and ethnicity. Women and the subgroups with high parental education and a Swedish ethnicity were more adversely affected by the repeal in terms of educational attainment, whereas men and the subgroups with low parental education and a non-Swedish ethnicity were more adversely affected in terms of labor market outcomes. This seems counter-intuitive since one would expect that those who are more adversely affected in terms of educational attainment also experience a larger effect on the labor market. However, a plausible explanation for the differences across parental education and ethnicity is that individuals with high parental education and a Swedish ethnicity have access to social networks of relatively high quality that improve their prospects on the labor market and thereby increase the opportunity cost of enrollment in adult education. As a result, those groups would be more responsive to changes to the cost of enrollment. When it comes to the differences across gender, the social networks interpretation is less plausible as there's no reason to believe that women have access to social networks of higher quality than men. However, it is an established fact that women (men) in Sweden are sorting into low (high) wage occupations which implies that women might expect their returns to enrollment to be lower and for that reason be more responsive to changes to the cost of enrollment. Finally, though I do not find an effect of the repeal on enrollment in tertiary education overall, I do find an effect for the subgroup with high parental education which experienced a 24 percent decrease relative to the pre-treatment enrollment rate.

To assess the validity of the DD framework, I implemented a propensity score weighting strategy suggested by Stuart et al. (2014) that weights the covariate distributions in the control groups and the post-treatment treated group to reflect the covariate distribution in the pre-treatment treated group. The resulting propensity score weighted DD estimates affirmed the results from the unweighted DD models for enrollment and unemployment rates, but also generated a positive effect of the repeal on subsequent employment rates which I later found was caused by heterogeneous treatment effects across parental education and ethnicity. Since I have access to several years of data before and after the Recruitment Grant was repealed, I was able to perform a Granger-type causality test by estimating a DD model with leads and lags between 2005 and 2009. Reassuringly, the placebo treatments in 2005 and 2006 were both zero while the lags in 2008 and 2009 suggested the adverse effects of the repeal on the target population increased over the period which is not surprising given the fact that the Swedish economy at the time was struggling to cope with the financial crisis that originated in the U.S. subprime mortgage market in 2007.

In conclusion, the implication of these results is that it is indeed possible to increase enrollment in adult education and improve subsequent labor market prospects for unemployed adults with incomplete upper secondary education by substituting the student loans offered by the state with grants. In terms of enrollment in adult education, the existence of the Recruitment Grant seems to have been of particular importance for women and the subgroups of the population with low parental education and non-Swedish ethnicity.

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III. The importance of nature-nurture interactions for skill formation and labor market outcomes: Evidence from a large sample of adoptees

With Mikael Lindahl and Björn Öckert

1. Introduction

The significance of gene-environment interactions has important implications for how to optimally design public policies. If environmental inputs can compensate for initial differences in genetic endowments, targeting interventions toward the disadvantaged would not only increase equality of opportunity, but would also be an efficient way to raise productivity. If, on the other hand, environmental factors tend to exacerbate initial genetic differences, public policies would face a major trade-off between equity and efficiency.

The literature on the importance of gene-environment interactions, although more prominent in recent years, is not new. Interaction effects have been inferred from studies using adoption and twin designs, as well as from studies of specific genes. Studies using adoption and twin designs have shown that gene-environment interactions can be important for some outcomes, such as the development of mental disorders and alcoholism, although the results are not entirely consistent across studies (Rutter et al, 2006).[34] Plomin et al. (2016) lists ten replicable findings in the behavioral genetics literature; genotype-environment interactions is not one of them.[35]

There is also a literature using twin decomposition techniques to investigate the importance of genetic endowment for the variation in IQ across the SES distribution. Several influential studies have found that the importance of genetic endowment differs by family background, with IQ being more heritable at the upper part of the SES distribution (Rowe et al, 1999; Scarr-Salapatek, 1971; Turkheimer et al., 2003). One interpretation of this result is that a favorable environment is necessary for some genetic factors to impact IQ. Still, other papers have failed to find support for such a hypothesis (Tucker-Drob and Bates, 2016; Figlio et al., 2017).

Using genetic markers, researchers have recently been able to explain significant variation in some outcomes like education (Rietveld et al, 2013). The pioneering studies on gene-environmental interactions using this approach are Caspi et al. (2002, 2003) that found evidence of negative interaction effects for antisocial behavior. They used information on specific genes important for this outcome and information on maltreatment in the family. However, interacting genetic markers with environmental conditions can generate interaction estimates that are difficult to interpret. If the environmental factors are not exogenously determined, interaction effects may just reflect the fact that the environment is better for those with a positive genetic predisposition for some

[34] For some early studies using the adoption design see Bohman et al, 1981, Cadoret et al., 1997, and Cloninger et al, 1981.

[35] "Fifth, our goal is to describe big behavioral genetic findings that replicate, rather than describing results that have not shown sufficient replication to be included in our list. Examples, which may become more convincing with more research, include (….) "genotype-environment interaction (attempts to show that heritability differs as a function of environment)." (Plomin et al., 2016, page 4)

outcome. Combining polygenic scores with some exogenous variation in the environment is a literature still in its infancy (see e.g. Schmitz and Conley, 2016).

Let us note that the question we ask in this paper is not whether gene-environment interaction effects are present, we know they are, but whether they are quantitative important in explaining inequality transmission between generations.[36] Can the environment, as we observe it through proxies of the adoption family, exacerbate or narrow "genetic inequality", where the latter term is the dispersion in predisposition to do well on some trait, at conception.

In this study we estimate the importance of "nature-nurture" interactions for cognitive and non-cognitive ability, educational attainment and earnings using adopted children and their adoptive and biological parents. More specifically, we regress the outcome for the adopted child on the outcomes for the adoptive parent, the biological parent and the interaction between the two. A negative (positive) interaction term is interpreted as environmental interventions potentially having a larger (smaller) effect for individuals born with disadvantage genetic predisposition for the analyzed outcome.

We contribute to the literature on nature-nurture interactions in several ways. There exist few studies of the importance of gene-environment interactions for skill formation and labor-market outcomes, and, as pointed out above, evidence from these studies is inconclusive. An earlier study for Sweden (Björklund, Lindahl and Plug, 2006) used a smaller sample of Swedish adoptees born in the early 1960s and estimated interaction effects for education and earnings. However, results were inconclusive as some interaction terms was positive (mother's education and father's earnings) and some small and insignificantly different from zero (father's education).[37] In this study, we use a much larger sample of adoptees, making it possible to i) focus on biological fathers of adopted children, which provide a cleaner measure of genetic endowment than the biological mother, since it is less contaminated by the prenatal environment, ii) investigate changes over time, where a hypothesis is that the possibility for environmental interventions to narrow genetic inequality possibly has decreased during the end of the period, when the Swedish welfare state changed focus as several important reforms designed to decrease inequality of opportunity (e.g., in education) already had been implemented,

[36] That genes can be switched on or off and alter gene expression, depending on the environment, is well known (see e.g., Gluckman and Hanson, 2005).

[37] Other authors have also who used this regression based adoption approach to investigate the importance of nature-nurture interaction effects for other outcomes; Hjalmarsson and Lindquist (2011), have looked at the criminal convictions; Cesarini, Johannesson and Oskarsson (2014), have looked at voting outcomes, Lindquist et al. (2015) have looked at entrepreneurship, Black et al. (2015a, 2015b) have looked at financial risk taking and wealth, respectively, and Lindahl et al. (2016) have looked at health outcomes. Generally, interaction effects, when investigated, are not statistically significant.

and iii) look at separate associations f daughters and sons, which is of particular interest since there is evidence that boys are more sensitive to negative environmental shocks than girls (Bohman et al., 1981; Cloninger et al., 1981; Krein and Beller, 1988).

In addition, we look at new outcomes such as cognitive and non-cognitive ability measures and create an index of parental occupation. We are also the first to extend the regression-based adoption approach (Björklund, Lindahl and Plug, 2006) to a latent variable framework where we use several proxies for the socioeconomic influence from the adoption and biological parents on the adopted children's skill formation and earnings. To do this we use administrative data on educational attainment, earnings, and an occupation index that reflect non-cognitive skills and apply the approach developed in Lubotsky-Wittenberg (2006).[38] This proxy variable approach can be used to decompose the transmission of "human capital" as two latent variables, capturing genetic and environmental factors, as well as to infer how much of the transmission that is due to interaction effects between these two factors. The main motivation for using this approach is to control for selective placement on unobservable factors which otherwise can bias our estimates. In addition, the variation and skewness in educational attainment measure changes over time, and by using several proxies for socioeconomic background, we add important information that increase the variation in parent's family background.

We use a data set based on all adoptees born in Sweden 1932-1970, where we can identify the children as well as their adoptive and biological parents. It is compiled from several Swedish registers and contains information on the children's educational outcomes and earnings and on the same characteristics of both their adoptive and biological parents. For adopted sons born 1951-1970 we also have information on cognitive and non-cognitive results from military tests and evaluations. For the parents we also utilize occupational information from national censuses. The intergenerational estimations for the adoption samples are also compared with representative samples of all (non-adopted) children born in Sweden in these periods.

We find that estimated interaction effects are typically non-positive and small in magnitude: around 5-10 percent of the overall transmission coefficients estimated for education and earnings. Interaction estimates are statistically significant and negative for educational transmission for sons and for the

[38] The approach in Lubotsky and Wittenberg (2006) was first applied to estimate intergenerational associations in Voosters (2018) and Nybom and Voosters (2017), estimating income mobility parameters in a latent variable framework (for Us and Sweden, respectively), with the purpose of correcting for parental income measuring some underlying long-run social status variable with error. Their finding is that estimates only increase somewhat (at least for men). In Adermon et al (2018) we do a similar exercise for educational attainment and find a larger increase of the estimates from using approach.

earlier cohorts. If we treat children's genetic and environmental background as unobserved latent variables, by using several proxy variables of the biological and adoptive parents' SES, we find similar results. Hence, a favorable upbringing will not exacerbate genetic inequality due to inherited differences among children, as would be suggested from a model of dynamic complementarity between genetic endowment and early family environmental interventions. This finding is also very important also from a methodological standpoint, since this means that additive models of nature and nurture probably is, for practical purposes, often a good enough approximation when studying educational and labor market outcomes.

The structure of the paper is as follows. In section 2 we present a simple version of the Cunha and Heckman model of skill formation suitable for our setting using the adoption design. In section 3 we present the estimation strategy and discuss the assumptions of the regression-based adoption approach that we use. Next, we discuss the Swedish institutional setting for adoptions, present the data, and discuss the prevalence of selective placement in the adoption process. In section 5, we report estimates using the population of children as well as the adoption sample. We present estimates of the importance of genetic and environmental factors as well as their interactions, in explaining the intergenerational transmission of skills and earnings. We also show trends over time. In section 6, we perform some sensitivity analysis, and section 7 concludes.

2. Theoretical framework

How does parental endowment and investments impact the production of offspring's skills? To understand this with respect to our setting, we here lay out a slightly modified version of the model of skill formation in Cunha and Heckman (2007).[39]

The skills production function is defined as:

$$\theta_{t+1} = f_t(\theta_t, h, I_t) \tag{1}$$

where θ_t and θ_{t+1} are the skills at the beginning of the time periods t and t+1; h is the stock of skills of the parents (when they have finished their education), and can be seen as a composite input capturing everything transferred from

[39] Conti and Heckman (2010) set up a framework for estimating nature-nurture effects with adoptions data in a latent variable framework. See also Lindahl et al. (2016) for a variation of the model presented in this section, although their focus is not on interaction effects between genetic and environmental factors.

parents and not captured by θ_t and I_t;[40] and I_t are the parental investments in the child's skill formation in period t. The main innovation of the basic Cunha and Heckman model, compared to traditional models of skills production, is that it allows for several stages of development where inputs do not have to be perfect substitutes and where "skills beget skills".

In our version of this model we think of investments being possible in 2 periods, in-utero and in childhood, and where the stock of skills (θ_t) exists at the beginning of period 1, 2 and 3: θ_1 is the stock of skills at conception, which is a function of the genetic endowment of the biological mother and father; θ_2 is the stock of skills when the child is born, which depends on the genetic make-up, investments during pregnancy, as well as from interactions between these (pre-natal environmental induced changes in gene expression), and θ_3, which is the stock of skills formed when investments in skills are done, hence $\theta_3 \equiv h'$ which is the stock of skills of the child, when the child has become an adult. θ_3 depends on the factors determining θ_2 as well as investments during childhood by the adoption family and interactions between these investments and earlier investments and/or with the genetic endowment. Since θ_1 constitute the genetic endowment of the biological parents, the stock of skills of parents, h, is a composite measure of inputs stemming from the family environment, net of investments made by the parents. Hence, h is the part of the family environment that "passively" influences the child's stock of skills in each period. Hence, this three-period framework leads to:

$$\theta_3(\equiv h') = m_2(\theta_1, h, I_1, I_2) \qquad (2)$$

In the Cunha and Heckman (2007) model, two features of the concept of skill formation are emphasized: Self-productivity, which means that the stock of skills θ_t are causally related across t:s, so that a high stock of skills in one period (for instance via investments during the earlier period) leads to a higher stock of skills in the next period; Dynamic complementarities, where a high stock of skills in the beginning of a period raises the returns to investments made during this (or later) time periods. In our setting, self-productivity means a positive effect of initial genetic endowment on the stock of skills in later periods, whereas dynamic complementarity means positive nature-nurture interactions, as a high genetic endowment raises the returns to investments in utero or in childhood. Negative nature-nurture interactions, or dynamic substitutability, means that the lower initial genetic endowment, the higher are the returns to investments in later periods.

However, it is important to emphasize that we use the Cunha and Heckman framework more to understand what we can and cannot do with our data, than

[40] Since h is expressed separately from θ_t and I_t it can be seen as representing family environment not captured by the initial stock of skills of the child and separate from the active investments made by the parents. Note that h itself is affected by grandparents' investments and stock of skills.

to lay out a model with parameters that we attempt to estimate. First, years of schooling of the adoptive and biological parents are imperfect measures of the genetic and environmental factors that impact child's skills, since there are unobservable factors that impact child's skills, which are uncorrelated with parent's education, and, more importantly, since there are unobservable factors that impact child's skills, which are correlated with parent's education. Hence, these measures are not broad enough; However, we attempt to deal with this by using three proxies for "human capital" of the biological and adoptive parents, in a latent variable framework. Second, the adoption experiment is unable to estimate treatment effects of specific environmental factors and should instead be seen as estimating the impact of the family environment as proxied for by our parental variables. The exogenous variation is that children are quasi randomly assigned to families with many different characteristics (important for child's education), where families years of education is one of these (and earnings and occupation are others). Hence, the measures are, in this sense, too broad in that the adoption experiment is unable to estimate causal effects of specific inputs. It is, however, able to decompose an overall intergenerational association in some outcome (or a combination of outcomes), into separate factor associated with pre-birth and post-birth factors (which under some assumption will measure the influence from genetic and environmental factors).

So, what can we estimate with our data?[41] We can estimate variations of self-productivity and dynamic complementarity with respect to initial genetic endowment, but only under some strong assumptions. Years of schooling (or any of the other socioeconomic measures: earnings and occupation) of the biological parent is a proxy for the pre-birth factors (the initial genetic endowment, θ_1 and investments in utero) (possibly h^{bp} if the stock of skills of the biological parents influence in utero conditions), while years of schooling of the adoptive parent is a proxy for the post-birth factors: the adoptive parent's stock of skills, h^{ap}, and investments during childhood/upbringing, I_2. However, if we are willing to assume that biological fathers of the adopted children have very limited influence on the prenatal environment of the adopted child, years of schooling of the biological father is a better proxy for the initial genetic endowment, θ_1. Hence, we can then separate out the pre-natal environment from the pre-birth factors. A positive estimate for years of schooling of the biological father would then support self-productivity, and a positive (negative) estimate for the interaction between years of schooling of the biological father and the adoptive parents would support dynamic complementarities (substitutability). Note that the later result only holds if adoptive parent's stock of skills, h^{ap}, and the investments of adoptive parents, I_1or I_2 are uncorrelated. The reason is that these factors are not empirically distinguishable (without

[41] Let us here also abstract from the fact that we can observe a vector of skills for the child but not for the parent.

additional information on, say, some exogenous reform that increases adoptive parents' propensity to invest in their children's skills).[42] If there instead is, as is likely, a positive correlation between these factors, dynamic complementarities/substitutability will be overestimated (biased away from zero).

3. Conceptual framework, econometric specifications and identification issues

A simple linear additively separable model of education production would look like:[43]

$$y^{ac} = f(E, G) = \alpha + \delta \cdot E + \theta \cdot G + u \quad (3)$$

where E is the environment and G is the genetic background of the child. However, as many have argued, this model is over-simplistic. Cunha and Heckman (2007) state that "the "nature versus nurture" distinction is obsolete" because genes express themselves through the environment. However, there can of course also be $E \times G$ interactions due to non-biological mechanisms because environmental factors simply affect individuals with varying genetic predisposition (for some skill outcome) differently. Regardless of what are the mechanisms, the model should be modified to allow for these to be present. A simple way to do this is to allow θ to depend on E, for instance through a linear relation so that $\theta(E) = \gamma_0 + \gamma_1 \cdot E + v$:

$$\begin{aligned} y^{ac} &= \alpha + \delta \cdot E + \theta(E) \cdot G + u \\ &= \alpha + \delta \cdot E + \gamma_0 \cdot G + \gamma_1 \cdot (E \cdot G) + \varepsilon \end{aligned} \quad (4)$$

As is clear from the discussion in the previous section, E contains family environment as well as direct investments and prenatal environmental factors, and G constitutes the genetic endowments of the child. We will use years of education of the adoption parents (y^{ap}) to proxy for E and years of education of the biological parents (y^{bp}) to proxy for G:

$$y^{ac} = \beta_0 + \beta_1 \cdot y^{ap} + \beta_2 \cdot y^{bp} + \epsilon \quad (5)$$

$$y^{ac} = \beta_0 + \beta_1 \cdot y^{ap} + \beta_2 \cdot y^{bp} + \beta_3 \cdot y^{ap} \cdot y^{bp} + \epsilon \quad (6)$$

We are interested in estimating the parameters in equations (3) and (4) through estimation of the regression equations (5) and (6). Note first, as pointed out in

[42] If we extend I to include government investments in child's stock of skills, we can also think about effects through reforms that impact child's skill directly.
[43] For simplicity, the discussion in this subsection is framed in terms of educational attainment.

the last section, that we have to limit ourselves to estimating intergenerational associations, as intergenerational causal effects are unattainable simply because we are using proxies for E and G. Under ideal circumstances (see the assumptions stated below), an OLS estimate of β_1 will at best capture the effect of the education of the adoptive parent's education (y^{ap}) and of other characteristics in the adoption family correlated with y^{ap}. The same reasoning is true for β_2 (and β_3).

To arrive at unbiased estimates of these parameters we need to impose a few assumptions: i) Children are given up for adoption early and will be moved to the adoptive family shortly thereafter, ii) Prenatal and pre-adoption postnatal environment are not correlated with the genetic endowment of the child and not correlated with the postadoption environment of the child. iii) Children are randomly assigned to adoptive families, and iv) Adopted children (and the adoptive parents) do not socially interact with the biological parents post adoption.

Regarding assumption ii): Since adoption took place a few months after the child was born, years of education of the adoptive parents will only capture environmental factors post adoption, and years of education of the biological parent will, in addition to genetic endowment, also capture prenatal environment and early postnatal environment. We will argue that as the biological father of the adopted child had limited involvement during mother's pregnancy and during infancy of the child, years of education of the biological father is a better proxy for the genetic endowment of the child.

Regarding assumption iii): There are two related problems with unobservable factors in this approach. First, in a regression of y^{ac} on y^{ap} and y^{bp}, an OLS estimate of y^{ap} will be upward biased because of insufficient controls for pre-birth factors (G). The reason is that non-random assignment is incorrectly controlled for by including y^{bp}. Second, an OLS estimate of y^{ap} will be downward biased because of measurement error in y^{ap}, as a proxy for E. Our main approach for dealing with unobservables is to treat the biological parents' human capital/skills/SES and adoptive parents' human capital/skills/SES as separate latent variables. We do this by applying the method suggested by Lubotsky-Wittenberg (2006) which is based on estimating the importance of an underlying latent variable, by using proxy variables (more specifically, to form a weighted average of separate proxies). Here we use educational attainment, earnings, a non-employed indicator and occupational rank for the biological parents and adoptive parents, respectively, thereby controlling for unobservable genetic and environmental factors simultaneously under some (admittedly) quite strong assumptions. In the Lubotsky-Wittenberg approach one needs to impose the assumption that the proxy variables are excludable in a model where the dependent variable to be used in the regression is a linear function of the latent variable. Here we use two latent variables, meaning that our proxy variables for adoptive and biological parents each needs to be excludable in the main equation. Hence, we need to assume that there is no selective placement, conditional on these two latent variables. In addition, we also simulate the likely direction and size of the remaining bias in the presence of positive selection (on unobservables) of children to adoptive families.

We will return to the other assumptions below, when we discuss sample restrictions (and selective placement), but in short we will: i) show that most adopted children are adopted very early and move to the adopted families fairly quickly, and iv) restrict the sample of parents and children so as to limit the possibility for social interactions.[44]

4. Institutions, data and descriptive statistics

4.1 Adoptions in Sweden 1930-1975

There are several adoption studies for Sweden, looking at varying outcomes, which present the institutional background for domestic adoptions in Sweden (see, for instance, Lindahl et al, 2016, for a lengthy discussion of adoptions in Sweden during a similar period as used in this paper).[45] Here we just discuss this shortly with a focus on the following issues: Who gave up a child for adoption? Who did adopt? What were the legal rights of the adopted child? How were children matched to adoptive families? What was the experience of the child before adoption?

The mothers who put their children up for adoption were typically young (30 percent were teenagers), unmarried and had low income. Many biological fathers were “unknown”. Although social workers tried to track down fathers, about 58 percent of fathers are not recorded in our data. Mothers often contacted social authorities during pregnancy and typically made the formal decision of giving up the child when she had recovered from the delivery (she could not do so before). Unmarried fathers had no formal say in the adoption decision.

Adoptive parents had to fulfill a number of requirements. They had to be married, be at least 26 years of age and not have children of their own (although there are quite a lot of exceptions to this in the data). The adoptive father had to have a stable income and adoptive mothers were expected to stay at home.

A basic principle of Swedish adoption laws has always been that an adoption should be “in the best interest of the child”. This meant that adoptions and

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[44] Hence, we see the change in environment as being permanent for the adopted children, which also increases the possible impact of the environmental change. However, even temporary changes in the family environment have been shown to have long-lasting effects on a child (see Santavirta, 2012).

[45] For a lengthier discussion using original sources see Bohman (1970) and Nordlöf (2001).

the choice of host family should be motivated by concern for the child. Adoptive children received same legal status as own children and formal connections to biological parents were broken.

Those responsible for the adoption process were local social authorities. They handled the match between biological mothers who wanted to give up child for adoption and adoptive parents who wanted to adopt. Adoptive parents were not selected at random. In fact, the adoption agencies were instructed to match adoptive parents to biological parents' mental abilities (if possible) and physical appearances (if possible). However, the information available to the social worker were likely quite limited (Björklund, Lindahl and Plug, 2006). One concern, for our study, is the degree of non-random matching of adopted children to adoptive families and to what extent this has changed over time (we return to this issue below where we discuss evidence on selective placement and how it has evolved over time).

Newborn children that were given up for adoption rarely stayed with their biological parents. In fact, about 87 (94) percent of these were given up before they were 3(6) months old (Black, et al., forthcoming). Children were placed in different forms of care such as special nursery home, home for unwed mothers, temporary foster care or the home of the adoptive family. The child was placed in the adoptive family on trial basis. Placement was recommended before 6 months of age. The trial lasted 3-6 months and if the trial went well, parents could apply for formal adoption. The formal decision of adoption was then taken by the court. Björklund, Lindahl, Plug (2006), who uses an adoption sample of children born 1962-1966, are able to infer that about 80 percent of the sample of adopted children was adopted before they became 6 months old. It is not possible to infer if this has been constant during the whole period that we study, although we don't think there are any reasons to expect why it would be markedly different.

4.2 Data and variable definitions

The original data set consist of the population of Swedish-born children between 1932 and 1970.[46] The multigenerational registry is used to match the children to their biological and adoptive parents. The multigenerational registry covers individuals born 1932 or later, and their parents, although the coverage is quite poor for the first two, three birth cohorts. Parents and children are available in the register as long as they have survived January 1st 1961

[46] Note that there are Swedish born adoptees also after 1970. However, these are few and come from very non-typical families. Shortly, the supply of unwanted children decreased sharply during these years as abortions become legal and contraceptives widely available. The demand for children to adopt was however unchanged, which is why when the decrease in domestic adoptions is seen, a comparable increase in foreign adoptions is observed. As we are not able to identify the biological parents of foreign adoptees we only use domestic adoptees in this paper.

and lived in Sweden at any time after that date. We use the sample of adopted children and their biological and adoptive parents in our main analysis, and also compare it to results from using a reference sample of biological (non-adopted) children reared by their biological parents.

We use data on educational attainment from the censuses 1960, 1970 and 1990 for parents and from the censuses and administrative registers 1985-2009 for the children. All educational attainment data is reported in levels and we have converted them to years of education based on highest educational attainment observed for the individual at (or around) age 40. The quality of the educational information for parents derived from the censuses has improved significantly over time. Hence, preference is given to later censuses.[47] Educational reforms have also increased the number of years of compulsory schooling, and an increased intake to high school and higher education has been observed as well. To make years of education comparable over time, we standardized it by year of birth and gender in the full population.

We use data on cognitive and non-cognitive skills at age 18 from the military draft records, which is available for men born 1951-1970. Hence, these outcomes can only be used for individuals in the child generation. Cognitive ability at age 18 is based on written tests for logical, spatial, verbal and technical abilities. We use the standardized sum of test scores. Non-cognitive ability at age 18 is based on assessment by certified psychologist through semi-structured interviews following a manual. It is a standardized composite measure of social-interactive ability and has been shown to be highly predictive of future earnings (Lindqvist and Vestman, 2011).[48]

We use data on earnings from administrative registers for a representative sample of 10 percent of the population between 1960-1966, and for the whole population for scattered years 1968-1984[49] and all years from 1985 until 2009. The earnings measure includes wage earnings, business income, taxable benefits and some work-related transfers from the social security system such as sick pay and certain parental benefits.[50] Capital earnings, pensions and parental leave are not included. Given that individuals in the child generation are born between the years 1932-1970, we cover several years of mid-career earnings for individuals from all these cohorts. To limit the problem with life-cycle bias we require earnings to have at least 3 positive observations between 30-50 years of age for individuals in the child generation. We also standardize the

[47] In 1960, only a few levels were available and the distribution is very right skewed. Note that if a parent has survived to 1970, educational data for 1970 is used over 1960, and so forth.
[48] See also Carlstedt, 2000.
[49] Between 1968-1984 we observe incomes in 1968, 1970, 1971, 1973, 1975, 1976, 1979, 1980 and 1982.
[50] Björklund, Lindahl and Plug (2006) used "earnings" and "income" of fathers, both giving qualitatively very similar interaction estimates. Our measure is similar to their earnings measure.

lifetime earnings within year of birth and gender in the full population to obtain estimates that are easy to compare over time and across specifications.

For the parents, the earnings measure has more limited coverage, especially for the parents of the early child generations. We therefore only require earnings of parents to have at least 2 positive observations between 30-64 years of age. This means that we can calculate an earnings measure for those parents that are born as early as 1906, which cover 98 percent of the biological fathers and 92 percent of the adopted fathers (note that very few adopted children born in the 1930s are included in our estimation sample). However, because earnings observed at this late age can be a poor indicator of lifetime earnings, we in our main estimations on the intergenerational transmission of earnings instead use an occupation-based measure. Data on occupation of the parents are available every five years from the censuses starting 1960, and we map earnings to individuals' occupations in the following way: We first calculate lifetime earnings for all individuals observed in the 1990 census and calculate the average lifetime earnings associated with each occupation. We then standardize the within-occupation lifetime earnings w.r.t. the year of birth and gender of the individuals that hold them to obtain our occupation-based earnings values. Finally, we assign each individual to the occupation-based earnings that correspond to the occupation they had at age 45 (or as close as possible to age 45) using three-digit occupational coding from the censuses. Note that our interaction estimates are very similar regardless of whether we use actual earnings or occupational induced earnings for the parents.

We also use parental earnings and occupation, in combination with educational attainment, as proxies for socioeconomic status in the LW approach. This is especially important for earlier cohorts, where we (especially since year of education is very right skewed) need as much information as possible to create a reliable SES measure. In the LW approach, since we want to use as independent information as possible, we use actual earnings that have been percentile ranked in the population w.r.t. year of birth and gender. However, because a non-trivial number of parents lack information on earnings, we use an indicator variable for missing earnings in the LW estimations to represent non-employment (including unemployment). When it comes to occupation, there is no natural rank-order present. There are different ways of creating a rank (and a cardinal measure) using secondary information.[51] Preferably this secondary source of information should not capture education and earnings, but instead creating a measure that capture something else, like social status or skills. We propose a new approach to do this based on the non-cognitive skill measure from military conscription discussed above. This measure is particularly suited here because the main purpose of the tests was to identify potential leaders in the military and to screen those unsuitable for military service.

[51] See Ganzeboom et al., (1992).

The non-cognitive skill based occupational measure is created in the population using the following steps: First, we map people into their occupations (including a category for the non-employed)[52] in 1990 and take the average of NC-skills within occupations. Second, we map the occupational index to occupational distributions in the censuses in the years 1960, 70, 75, 80, 85, 90. Third, each individual is finally assigned the occupational index that corresponds to the occupation he/she had at 45 years of age (or as close to 45 years of age as possible).

As for the creation of the LW-index, we utilize a) parents' education measured as number of years of education (standardized w.r.t. year of birth and gender in the population). b) Parents' lifetime earnings (dummy variable adjusted for those that lack earnings information). c) Parents' NC skills based on their occupational index. [53]

4.3 Sample restrictions and descriptive statistics

We require the adopted children to have been born in Sweden and adopted by two parents (where neither is the biological parent), and that both the biological parents can be identified. Between 1932 and 1970, there are 23,563 adoptees born in Sweden that the multigenerational registry can match to biological mothers, adoptive mothers and adoptive fathers. This implies that the adoptees and their parents lived at least until January 1st 1961 and was a resident in Sweden at some point after that date. Requiring the biological fathers to be identified decreases the sample to 12,227 children. Eliminating those families where none of the adoptive parents is a biological parent further decreases the sample to 9,944. We also require that data from the educational and birth registers are available which brings the sample down to 9,802 children since the coverage is almost 100 percent.

In addition, we require all individuals to have survived long enough to having been able to complete their education (26 years of age). The adoptive mother should be at least 24 years of age and the biological father at most 63 years of age at the time of birth of the adopted child. This decreases the sample to 9,042 children. For later cohorts, we are able to infer how many children that are adopted by relatives (either by a grandparent, an uncle or an aunt). It turns out to be almost 200 individuals, less than 3 percent, decreasing the sample to 8,855 children.

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[52] We use men 25 years or older to calculate the average of NC skills in the non-employed group.

[53] The LW indices are based on residualized variables w.r.t fixed effects for the child's year of birth, linear and squared terms for parents' year of birth, as well as dummy variables if the respective parent income is not observed in accordance with the dummy variable adjustment for parents who lack information earnings (i.e., 4 such dummy variables).

To guarantee that the adoptive parents indeed capture the family environment of the adopted child we also require both adoptive parents to have survived until the child is 15 years of age and to have lived with both adoptive parents between ages 11 and 15. This further limits the sample to 7,980 children.

We also impose several restrictions to limit the possible selective placement of children to adoptive parents. Since we have detailed information on residential location every five years from 1960 and birth parish for every year, we impose the following restrictions on the sample.[54] First, the biological parents should not live in the same parish as the adopted child when the child is between 11 and 15 years of age, and not be born in the same parish as any adoptive parent. Second, the adoptive parents should not live in the same parish as the adopted child were born in and not live in this parish at the time as the child was born. This gives us the sample that we use in the estimations, which consists of 6,788 adopted children.

Table 1 reports descriptive statistics for the main variables used in the estimations. We report means, standard deviations and min and max values for the sample of adoptees and for a random sample of non-adopted Swedish-born children. We see that adopted children are similar to the random sample of non-adopted children when it comes to years of education, but that they possess lower earnings, non-cognitive skills and cognitive skills. Next, we compare the characteristics of non-adopted and adopted children's biological and adoptive parents. We see that age at birth for the biological parents of the adopted children is several years lower than in the population of non-adoptive parents. The reverse is true for adoptive parents as the age at birth (of the adopted child) is several years higher than in the population of non-adoptive parents. A similar pattern is true for years of education, at least for the adopted parents, which has as about half a year longer education than what is observed in the population. Average earnings of the biological father of adopted children is much lower than earnings in the population of fathers. Interestingly, biological mother's earnings are similar to the population of mothers, and actually higher than adoptive mothers. The latter is most likely because of lower employment rates among adoptive mothers, which is why we concentrate on fathers when we estimate intergenerational earnings regressions. Average years of education for the biological parents of the adopted children is similar to the average years of education of the biological parents in the population. The reason for this is twofold. First, the biological parents of the adopted children are younger and therefore educated in later years than the population of parents. Second, we have not adjusted for differences in the adoptee sample across birth cohorts, due to there being much fewer adoptees in the earlier

[54] The fact that we only have information on residential location from 1960 make some of the restrictions less binding for the first 10-15 birth cohorts. However, the fraction of the overall sample born during these years is anyway quite small.

years. In the estimations we always use variables that are standardized by year of birth in the population and include birth cohort controls.

Table 1. Descriptive statistics

	Adoption sample				Population			
	(1) Mean	(2) Sd	(3) Max	(4) Min	(5) Mean	(6) Sd	(7) Max	(8) Min
Rank earnings	44.65	28.29	100	0	50.00	28.87	100	0
Education	11.71	2.26	21	7	11.22	2.87	21	5
Cognitive skill	-0.17	1.00	2.4	-3.9	-0.00	1.00	2.5	-4.1
Non-cognitive skill	-0.05	1.03	3.0	-3.9	-0.00	1.00	3.3	-3.9
Year of birth	1958.06	7.44	1970	1932	1952.18	10.89	1970	1932
AF age at birth	36.98	5.69	61	25	-	-	-	-
AM age at birth	34.19	5.11	49	25	-	-	-	-
BF age at birth	28.04	7.80	73	14	31.47	7.17	79	14
BM age at birth	24.39	6.16	49	13	28.06	6.18	60	13
AF education	9.32	2.97	20	7	-	-	-	-
AM education	8.82	2.53	20	7	-	-	-	-
BF education	8.60	2.12	20	7	8.78	2.58	21	5
BM education	8.67	1.99	20	7	8.64	2.28	21	5
AF rank earnings	58.76	27.42	100	0	-	-	-	-
AM rank earnings	43.45	28.19	100	0	-	-	-	-
BF rank earnings	39.71	28.61	100	0	52.95	27.86	100	0
BM rank earnings	46.82	28.72	100	0	47.42	28.08	100	0
AF occupation	0.10	0.30	1	-1				
AM occupation	-0.13	0.22	1	-1				
BF occupation	-0.09	0.27	1	-1	0.01	0.30	1	-1
BM occupation	-0.15	0.21	1	-1	-0.14	0.22	1	-1
Observations	6,788				5,259,035			

NOTES. Education is measured as years of schooling. Cognitive and non-cognitive skills have been standardized in the population within year of birth and is only observed for men. BF = biological father; BM = biological mother; AF = adoptive father; AM = adoptive mother.

If we compare the means and standard deviations for years of education for the parents of adoptees and non-adoptees, we see that there is a lot of distributional overlaps. To investigate this further, we next compare the fraction of individuals, by parental types, across the whole education distribution. Kernel densities across the education distribution are shown in Figure 1a for fathers and in Figure 1b for mothers. To facilitate comparison across parental types, we have sampled the three types of parents so as they are born in the same years. For fathers, we see a clear pattern where the distribution for adoptive fathers is to the right of the population fathers, and the distribution of biological fathers of adoptees is to the left.

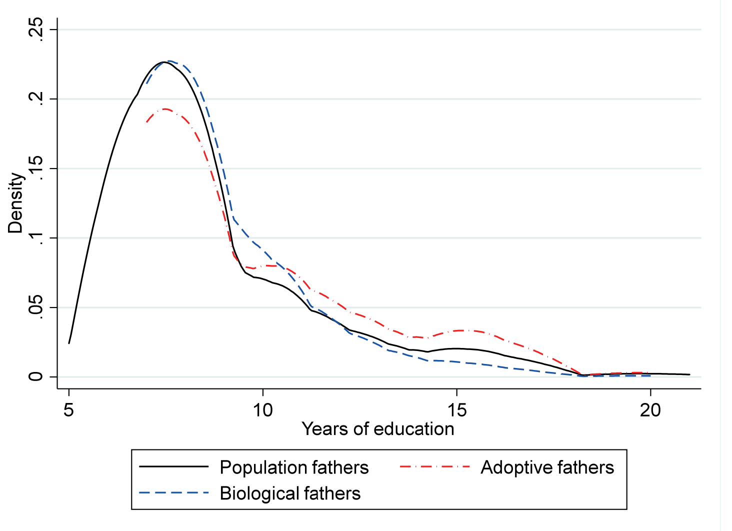

Figure 1a. Empirical education distributions for fathers

For mothers, the pattern is similar for the biological mothers, although for the adoptive mothers, the distribution at the upper part resembles the one for the population of mothers, and in the middle part, the adoptive mothers' educational distribution seems to be somewhat to the left of the distribution for the population of mothers. We show the distributions for percentile ranked earnings (w.r.t. year of birth and gender) in Figures 1c and 1d. Results for father's earnings are in line with those for education, whereas for mother's earnings overlap more, but is also showing biological mothers at the upper part of the distribution having higher earnings than the adoptive and the adoptive mothers and the population of mothers. The most important message from these figures is that there are significant overlaps in the educational and earnings distributions for both mothers and fathers, which 1) speaks in favor of our adoption estimates being externally valid for the population, and 2) that there is a lot of variation in our proxies for family environment and genetic background. This is important since a worry with using data on adoptive parents to infer nurture effects, is that the family environment is always of good quality (since adoptive parents are screened) and hence that there is very little environmental dispersion, making it difficult to detect statistically significant estimates. However, a too small dispersion in the family environment, as measured by adoptive parent's education, is not a worry in our adoption sample.

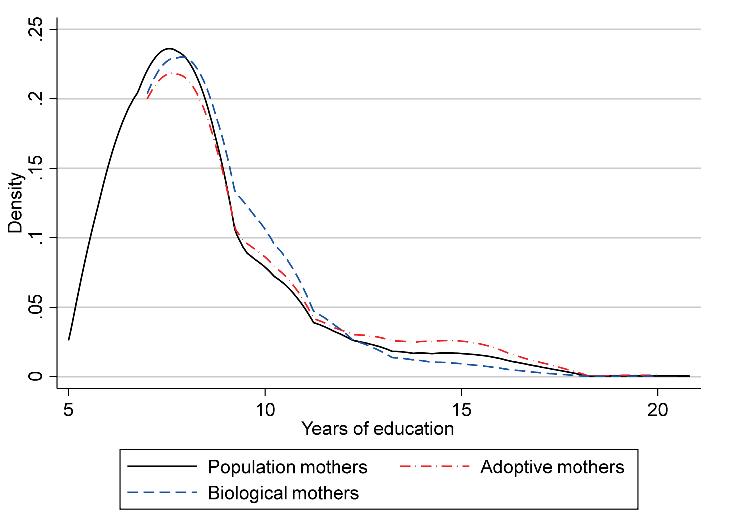

Figure 1b. Empirical education distributions for mothers

4.4 Selective placement

Ideally, adopted children should be randomly allocated to adoptive families. However, there are reasons to be concerned about the existence of adoptions of relatives to the biological family as well as local matching of children to families, which might result in non-random allocation. As we discussed in the previous section, we therefore impose a number of restrictions to the sample with respect to grandparent and cousin adoptions, as well as to the residential location and birth of the parents and children. We note, however, that the association of years of education between adoptive and biological parents (a measure of selective placement used in Björklund, Lindahl and Plug (2006)) only decrease marginally when we impose these restrictions.

As we discussed in section 4.1, the guidelines for the social workers to match children to families on observable traits suggest another reason for non-random assignment. Using the sample of non-related adoptions and with the locations restrictions imposed as well, we show the correlation coefficients for years of education of the adoptive and biological mothers and fathers over time in Figure 2. The education variables are standardized by year of birth and gender. As can be seen the correlations are relatively high, but in line with the results reported in Björklund, Lindahl and Plug (2006) for 1962-1966. The correlation coefficients are around 0.2 for much of the period, although lower for mothers during the first half of the period. Hence, there is still a high degree of selective placement of adopted children to adoptive families remaining even after imposing the sample restrictions. We interpret this result as non-random matching by adoption agencies being the main reason for selective placement.[55]

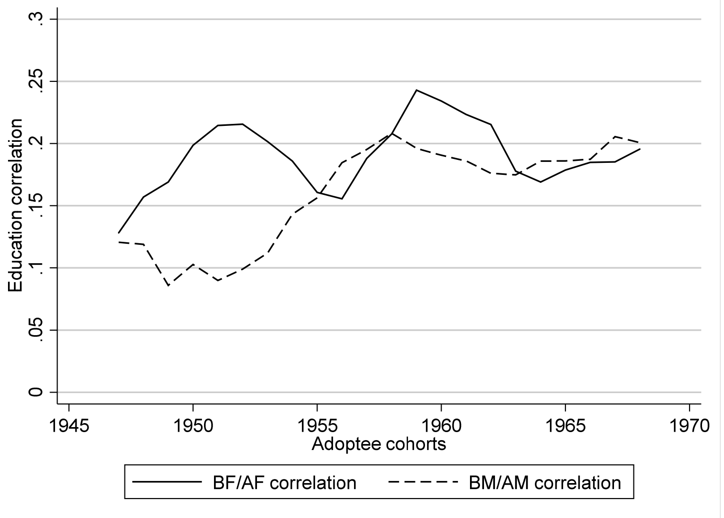

Figure 2. Correlation between fathers' and mothers' education

The main issue for us is how and when we expect our results to be affected by selective placement. First, if we are interested in inferring how estimates of main or interaction effects have changed over time, we are not too worried about selective placement, as long as it has remained roughly constant over time. As we see in Figure 2, it appears to be roughly constant from the mid-1950s to the end of the 1960s, at least for fathers, where a high fraction of our adoption sample was born.

Second, as we include years of education of both the adoptive and the biological parents in the estimations, we only need conditional random assignment. Hence, what we are worried about is matching on unobservable characteristics, and how this will affect our results. This can be seen as a problem of measurement error in the adoptive and biological parent's years of education variables, where the measurement errors are positively correlated. For the additive separable model (without an interaction term), imposing uncorrelated measurement errors, this is modelled in Björklund, Lindahl and Plug (2006), which conclude that the bias is probably quite small. If we include an interaction term, and simulate the bias due to measurement error, we get that the estimates for years of education of an adoptive parent, years of education of a

[55] The high correlations between adoptive and biological parent's educational attainment is in line with adoption studies for other countries, where it has also been argued that matching by adoption agencies is the main explanation (Scarr and Weinberg, 1978).

biological parent and the interaction between these two variables, all are biased towards zero and the bias in the interaction term being about twice as large (in percentage terms) as the bias in the estimates of the main effects.[56]

Third, we also use the Lubotsky-Wittenberg (2006) approach, which (as discussed in section 3) under some assumptions can control for selective placements based on unobservable factors.

5. Results

5.1 Estimations of the intergenerational transmission of educational attainment and skills in the population

Table 2 reports estimates of the intergenerational transmission of skills using the population of biological children. We use samples of sons and daughters born between 1932-1970 for education and sons born 1951-1970 for cognitive and non-cognitive skills. All variables are standardized in the population within cohort and gender to have mean zero and a standard deviation equal to one. We report the results from a model where mother's and father's years of education are entered separately. All regressions include an intercept and controls for year-of-birth fixed effects for the children and a quadratic function of (all) parent's year of birth. Standard errors are clustered on the biological mother. In panel A we report results using parent's education and in Panel B we report estimates using an index based on the Lubotsky-Wittenberg (2006) approach using parents' earnings and occupation in addition to education. This index is measured on the same scale as the education estimates.

The estimates of parents' education in the pooled sample of children show a stronger positive association with child's education for father's education compared to mother's education. A one standard deviation higher years of education for both parents is associated with a 0.4 standard deviation higher years of education of the child. The estimate is somewhat higher for sons than for daughters (0.43 versus 0.38). The association of parent's years of education with the cognitive skills of the child shows a similarly high association as for years of education, whereas the association with non-cognitive skills is about half the size as for cognitive skills.

[56] That classical measurement error in several right-hand side variables bias estimates for all variables towards zero is well known. Since we impose a positive correlation between the measurement errors, it is not very surprising that this results still holds. Also, that the bias of the interaction terms becomes larger makes a lot of sense, sine the measurement error is amplified from taking the product of two variables, where both are measured with error.

Table 2. Main effects in the population

	Pooled	Daughters	Sons		
	(1)	(2)	(3)	(4)	(5)
	Education	Education	Education	Cognitive skill	Non-cognitive skill
Panel A					
BF education	0.236***	0.207***	0.263***	0.201***	0.110***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
BM education	0.173***	0.176***	0.171***	0.175***	0.082***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	3,695,684	1,810,088	1,885,596	938,664	924,756
R-squared	0.150	0.133	0.168	0.126	0.034
Panel B					
BF input	0.281***	0.250***	0.310***	0.253***	0.201***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
BM input	0.182***	0.185***	0.178***	0.192***	0.109***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	3,695,684	1,810,088	1,885,596	938,664	924,756
R-squared	0.159	0.142	0.177	0.138	0.050

NOTES. Panel A reports regression estimates of offspring's education on parental education. Panel B reports regression estimates of offspring's education on parental human capital proxied by a Lubotsky-Wittenberg index (Lubotsky and Wittenberg, 2006) based on parental education, earnings and an occupational index that reflects non-cognitive skills. Outcomes and parental inputs have been standardized w.r.t. year of birth and gender in the population, and the LW-index is measured on the same scale. Controls include year of birth fixed effects as well as linear and quadratic controls for parents' year of birth. Standard errors are clustered on the biological mother.

The estimates of parents' SES, using the Lubotsky-Wittenberg approach, show larger estimates for all outcomes. This is in line with the results in Adermon et al., (2018) for educational transmissions using Swedish data. The increase of the estimate is larger for fathers, probably because mothers have a weaker labor market attachment (implying that education contains more information relative to earnings and occupation for mothers). The LW estimates for fathers increase about 20-30 percent relative to the baseline OSL estimates. An exception is when we use non-cognitive skills as outcome for the children, where the increase is much larger. This is probably because non-cognitive skills are used to create the occupation index that we use in the Lubotsky-Wittenberg approach.

Figure 3 shows trends in the intergenerational transmission of education using all children born 1945-1970. We show results for mothers and fathers separately and for both parents. We also compare the trend of the association for all parents with biological children to the trend using only the sample of adopted children and adoptive parents. The trends are always based on moving averages of estimates for 5-year periods. The first point is for 1945-1949, the second point for 1946-1950, and so forth up to 1966-1970. As the number of adopted children born before 1945 is small (less than 300 children for the whole period), we limit the period to 1945-1970.

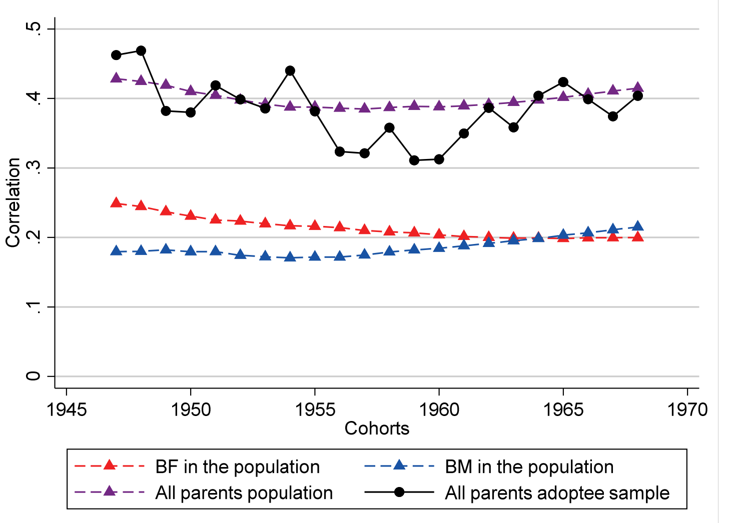

Figure 3. Intergenerational transmission of education

We see that the importance of mother's education has increased over time, whereas the reverse is true for father's education. At the end of the period, it looks like the association with mother's education has slightly overtaken the association with father's education in size. Overall, the intergenerational transmission of education has decreased somewhat up to the second half of the 1950s and increased slightly after that. The magnitude of the change is not very large though: it goes from about 0.42 in the early years to about 0.38, and then reverses to 0.40 at the end of the period. A similar pattern is observed for adoptive families. The U-shape pattern seems more pronounced for adoptees, but it should be kept in mind that the confidence bands are also quite large for this sample. The similar pattern at least suggests that results using the adoption sample, when we decompose the intergenerational association into pre- and post-birth factors, are likely to be representative of the full population.

5.2 Estimations of the importance of pre- and post-birth factors for the intergenerational transmission of educational attainment and skills

Table 3 reports estimates for the intergenerational transmission of skills using the sample of adopted children. We use samples of adopted sons and daughters born between 1932-1970 for education and sons born 1951-1970 for cognitive and non-cognitive skills. All variables are standardized against a population having mean zero and standard deviation one. The estimated models and the structure of the table is similar to Table 2, with the difference that we now report results from a model where the adoptive and biological parents' education are entered separately, and in panel B, from a model where we use the average of the adoptive and biological parents' education. Panel C reports estimates using the Lubotsky-Wittenberg approach.

Table 3. Main effects in the adoption sample

	Pooled	Daughters	Sons		
	(1)	(2)	(3)	(4)	(5)
	Education	Education	Education	Cognitive skill	Non-cognitive skill
Panel A					
BF education	0.098***	0.123***	0.077***	0.137***	0.056**
	(0.015)	(0.022)	(0.020)	(0.026)	(0.027)
BM education	0.127***	0.135***	0.113***	0.169***	0.115***
	(0.015)	(0.021)	(0.022)	(0.030)	(0.031)
AF education	0.104***	0.095***	0.108***	0.119***	0.112***
	(0.011)	(0.015)	(0.015)	(0.020)	(0.022)
AM education	0.057***	0.049***	0.069***	0.069***	0.030
	(0.011)	(0.015)	(0.016)	(0.020)	(0.021)
R-squared	0.092	0.102	0.099	0.102	0.053
Panel B					
BP education	0.225***	0.261***	0.188***	0.307***	0.172***
	(0.0194)	(0.027)	(0.028)	(0.036)	(0.036)
AP education	0.163***	0.146***	0.179***	0.189***	0.142***
	(0.0110)	(0.015)	(0.016)	(0.019)	(0.021)
R-squared	0.091	0.101	0.098	0.101	0.051
Panel C					
BP input	0.245***	0.269***	0.223***	0.464***	0.220***
	(0.0211)	(0.029)	(0.030)	(0.044)	(0.043)
AP input	0.190***	0.173***	0.207***	0.212***	0.204***
	(0.0117)	(0.016)	(0.017)	(0.021)	(0.025)
R-squared	0.089	0.090	0.088	0.108	0.048
Observations	6,788	3,250	3,538	2,493	2,452

NOTES. Panel A reports regression estimates of offspring's education on parental education separately for each parent, and panel B reports estimates using parental averages. Panel C reports regression estimates of offspring's education on average parental human capital proxied by a Lubotsky-Wittenberg index (Lubotsky and Wittenberg, 2006) based on parental education, earnings and an occupational index that reflects non-cognitive skills. Outcomes and parental inputs have been standardized w.r.t. year of birth and gender in the population, and the LW-index is measured on the same scale. Controls include year of birth fixed effects as well as linear and quadratic controls for parents' year of birth. Standard errors are clustered on the biological mother.

The estimates with parents' education in the pooled sample of children show a positive association with child's education for all parental types, but the association is strongest for the biological mother's education and weakest for the adoptive mother's education. These results are qualitatively similar as in Björklund, Lindahl and Plug (2006), even though their estimate for adoptive mother's education was smaller in magnitude and statistical insignificant. Estimating separate models for sons and daughters reveal a stronger association with the biological parents for daughters than for sons, and a stronger association with the adoptive parents for sons than for daughters.

Turning to models where we associate cognitive and non-cognitive skills for adopted sons with the education of their adopted and biological parents we find a similar pattern as for education of sons. The associations with cognitive skills are stronger than for non-cognitive skills, and for non-cognitive skills the association with adoptive mother's education is statistically insignificant. The test for cognitive skills is comparable to an IQ test, and results can therefore be compared to earlier adoption studies looking at IQ as an outcome. Scarr and Weinberg (1978) used a sample of 150 adopted children growing up in white families in Minnesota and found the biological mother's education to be significantly associated (correlation about 0.25) with the child's IQ score, even conditional on adoptive parents' education, earnings, occupation and IQ.

Looking across columns, a general pattern is that estimates for biological mother's education are larger than the estimates for biological father's education (especially for sons!). This result is consistent with prenatal (and very early postnatal) environmental effects being positively correlated with the educational attainment of the biological mothers. This is especially true for adopted children as the involvement of biological fathers during the pregnancy and shortly thereafter typically is very limited. Hence, an estimate for biological the father's education better capture the genetic endowment, compared to an estimate for the biological mother's education, which capture both genetic endowment and the prenatal environment. A similar result was found in Björklund, Lindahl and Plug (2006), but the difference was so small (and statistically insignificant for years of education) that they concluded that prenatal environmental effects were relatively small. In the present paper the difference in the associations between biological mother's and biological father's education is large enough (between 10-50 percent) that we think there is evidence in favor of a large role for prenatal environment. This is also in line with several studies which often have found very large negative effects on children's outcomes from having experienced negative environmental shocks to the mother during pregnancy. We will therefore, when we look at how the environment and genetic endowment interacts, mostly use the biological father's education as a proxy for the genetic endowment of the adopted child.

In panel B, we use the average of the adoptive and biological parents' education, and consistently find larger associations with biological parent's education than with adoptive parent's education. The magnitude of the estimates shows that a standard deviation (SD) higher education for all parents is associated with about a 0.4 SD higher education for the child, with about 40 percent coming from the adoptive parents and 60 percent from the biological parents.[57] The overall associations are slightly higher for cognitive skills, but lower for non-cognitive skills.

In panel C, we use the LW approach and the average of the adoptive and biological parent's SES (so the estimates should be compared to the OLS estimates for education in panel 2). We find that the estimates for both biological and adoptive parents increase with about 20-25 percent after incorporating the influence of earnings and occupation into the estimations, leaving the relative importance of pre- and post-birth factors unchanged. The exception is when we use cognitive and non-cognitive skills as outcomes, where the influence of parents increases more. For cognitive skills, the estimate for biological parents increases significantly, making the pre-birth factors much more important relative to post-birth factors.

Next, we investigate trends in pre-birth and post birth factors for explaining the intergenerational transmission of education.

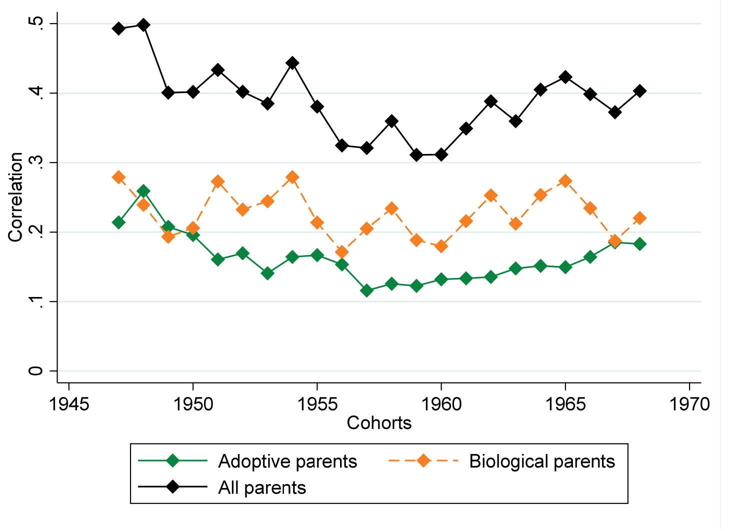

Figure 4. Intergenerational education correlations

Figure 4 show trends for the association of children's education with the education of the biological and adoptive parents (defined as averages of mothers and fathers, as in the second panel of Table 3). We compare these trends by

[57] This result is very much in line with Björklund, Lindahl and Plug (2006)'s results for the sample of Swedish adoptees born 1962-1966. There is an enormous literature estimating the importance of genes and environment for various outcomes, including cognitive and non-cognitive measures (see Plomin et al, 2001, for a review) and educational and labor market outcomes (see Sacerdote, 2011, for a review). Since our main focus is on interaction effects, we will not review the evidence here.

parental types to the sum of the estimates for all parents (the upper line in both figures). The trends are, as in Figure 3, based on moving averages of estimates for 5-year periods.

5.3 Estimations of the importance of interaction effects between pre- and post-birth factors for the intergenerational transmission of educational attainment and skills

Table 4 reports estimates for the intergenerational transmission of skills using the sample of adopted children from models including both main and interaction effects. To limit the number of included variables somewhat, we estimate models where we have combined adoptive parent's education into one variable, based on the average of the standardized education variables for adoptive mother's and father's education.[58] In panel A, we show estimates from models including three main effects for biological mother's education, biological father's education, and adoptive parent's education, and two interaction effects, birth mother's education interacted with adoptive parents education and birth father's education interacted with adoptive parents education. We report F-tests of whether the two interaction effects are equal to zero. In panel B, we do not include the main and interaction effects for biological mother's education. The reason, as argued earlier, is that we believe that biological mother's education is a proxy not only for genetic endowment, but also for prenatal environment, something which also can bias estimates for the other variables.[59] In panel C, we show the LW estimates to be comparable with the estimates in panel B. As we have seen earlier the LW estimates are typically at least 20 percent larger than the education estimates from the OLS regression. Hence, we do indeed capture factors of biological and adoptive parents that are not incorporated in the models for observed education, which could possibly bias or interaction coefficients as well.

In panel A, we see that the estimated effects of the interaction effects for education in the pooled sample of children show negative interaction effects, which are driven by those for the biological fathers. An F-test reveals that they are marginally jointly different from zero. Turning to the other columns, results are confirmed for education and cognitive skills of sons, but not for education for daughters or non-cognitive skills for sons. The difference between results for biological mothers and fathers suggest that prenatal and post birth environmental factors are complements (since biological mother's education capture both genetic and pre-natal environmental factors).

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[58] For estimation of a more general model, see Appendix Table A1, where we show main and interaction effects separately for adoptive mother's and adoptive father's education. Results are then very similar.

[59] Note that the estimates for the main effects are very similar as in Table 3 because we use standardized variables. They are not identical because we standardize the variables against the population distributions.

Table 4. Estimates of nature-nurture interactions

	Pooled	Daughters	Sons		
	(1)	(2)	(3)	(4)	(5)
	Education	Education	Education	Cognitive skill	Non-cognitive skill
Panel A					
BF education	0.115***	0.139***	0.096***	0.175***	0.068**
	(0.016)	(0.025)	(0.022)	(0.028)	(0.029)
BM education	0.130***	0.127***	0.125***	0.204***	0.113***
	(0.017)	(0.024)	(0.023)	(0.031)	(0.034)
AP education	0.162***	0.146***	0.177***	0.176***	0.144***
	(0.011)	(0.016)	(0.016)	(0.021)	(0.022)
BF/AP interaction	-0.025**	-0.020	-0.029*	-0.049**	-0.020
	(0.012)	(0.018)	(0.016)	(0.020)	(0.022)
BM/AP interaction	-0.001	0.013	-0.014	-0.049*	0.013
	(0.013)	(0.018)	(0.017)	(0.028)	(0.028)
F-test	0.099*	0.500	0.085*	0.004***	0.642
R-squared	0.092	0.101	0.100	0.106	0.052
Panel B					
BF education	0.136***	0.166***	0.112***	0.202***	0.085***
	(0.016)	(0.025)	(0.021)	(0.027)	(0.029)
AP education	0.177***	0.160***	0.193***	0.206***	0.154***
	(0.011)	(0.015)	(0.015)	(0.019)	(0.021)
BF/AP interaction	-0.020*	-0.013	-0.027*	-0.053***	-0.011
	(0.012)	(0.018)	(0.016)	(0.020)	(0.022)
R-squared	0.081	0.088	0.092	0.091	0.046
Panel C					
BF input	0.141***	0.154***	0.131***	0.255***	0.089***
	(0.016)	(0.024)	(0.021)	(0.031)	(0.029)
AP input	0.210***	0.193***	0.226***	0.252***	0.223***
	(0.011)	(0.016)	(0.017)	(0.021)	(0.025)
BF/AP interaction	-0.012	-0.008	-0.019	-0.080***	-0.007
	(0.015)	(0.021)	(0.020)	(0.028)	(0.029)
R-squared	0.079	0.077	0.082	0.090	0.041
Observations	6,788	3,250	3,538	2,493	2,452

NOTES. Panel A reports regression estimates of offspring's education on BF, BM and AP education and their interactions. BM education is dropped in panel B. Panel C reports regression estimates of offspring's education on BF and AP human capital proxied by a Lubotsky-Wittenberg index (Lubotsky and Wittenberg, 2006) based on parental education, earnings and an occupational index that reflects non-cognitive skills. Outcomes and parental inputs been standardized w.r.t. year of birth and gender in the population, and the LW-index is measured on the same scale. Controls include year of birth fixed effects as well as linear and quadratic controls for parents' year of birth. Standard errors are clustered on the biological mother.

However, the estimates of the interaction terms for biological mothers and fathers are not statistically different. Because we are worried about that the biological mother's education also captures prenatal environment, our preferred estimates are from models without this variable in panel B. However, we note that a drawback from such a model is that genetic factors of the biological mothers are not incorporated in the estimations. Results from estimating of such models pretty much confirm the results shown in panel A. All interaction effects are estimated negative, although they are only sometimes statistically significant, and only marginally so for education (overall and for sons). The exception is for cognitive skills, where the interaction effect is precisely estimated negative. In panel C, we show LW estimates, which very similar in size to the education estimates in panel B, although somewhat less precisely estimated. Hence, the bias from selective placements for the interaction estimates appears to be negligible.

So, what can we conclude from these interaction estimates? Even though some of them are marginally statistically significant, they are almost always small in magnitude. To see this, we can calculate the interaction estimate as a fraction for the sum of the biological and adoptive parents' estimates. For education in column 1, we see that the interaction estimate is about 3-6 percent of the overall intergenerational transmission. The magnitude is largest for cognitive skills, where it is between 13-16 percent. Hence, with the exception of cognitive skills, we conclude that "nature-nurture" interactions are quantitatively unimportant in the intergenerational transmission of skills.

Figure 5 show trends in estimates from the model underlying the estimates in panel A of Table 4.

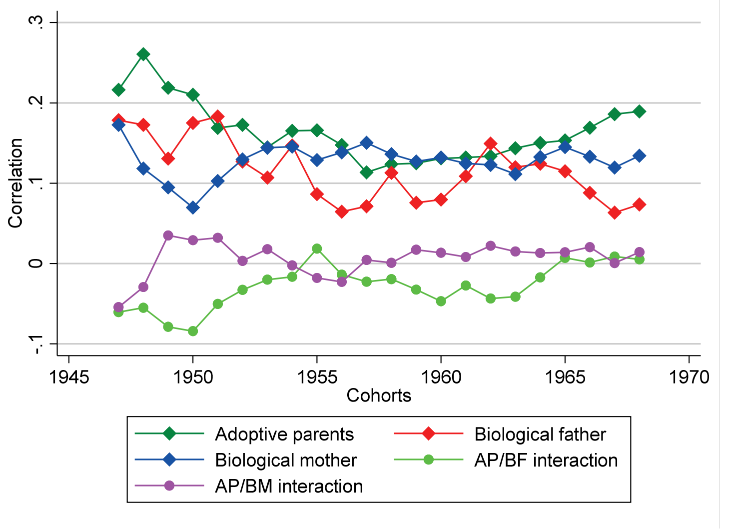

Figure 5. Interaction effects comparing biological fathers and biological mothers

We see that the main effects from adoptive parents' and the biological father's education all show a downward trend, whereas the interaction effect has increased over time for biological fathers but has been roughly constant for biological mothers. We interpret this as some evidence in favor of the interaction between genetic factors and the postnatal environment having gone from being substitutes in the early period to neither reinforcing nor compensating each other at the end of the period.

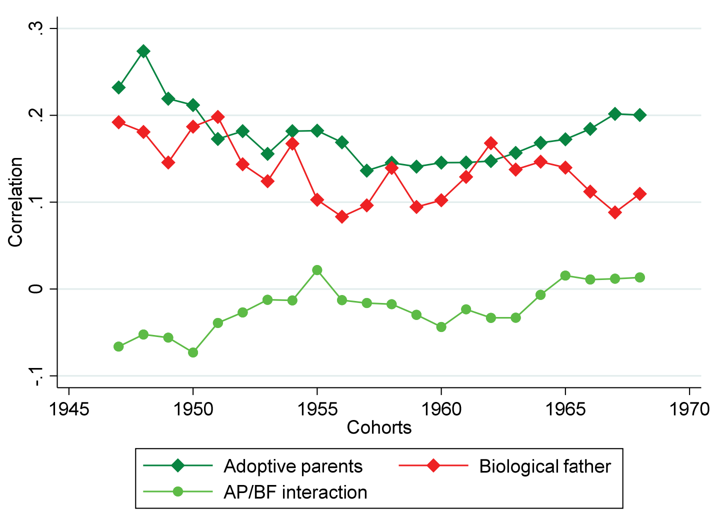

Figure 6. Interaction effect using a clean measure of genetic endowment

Note that the result in the figure is consistent with the results for education in Björklund, Lindahl and Plug (2006), only using data on adoptees born during the latter part of our period (1962-1966), finding positive but statistically insignificant interaction effects for fathers and positive and statistically significant interaction effects for mothers. The difference in the interaction term involving the biological mother might indicate that genetic factors and prenatal environment were complements early in the period but not later in the period. These patterns are confirmed in Figure 6 which shows trends in estimates from the model underlying the estimates in panel B of Table 4.

It is important to remember that the adoption sample is fairly small in the beginning of the period, and therefore that the trends in the interaction terms are quite erratic despite being based on 5-year moving averages. We therefore extend the regression models and interact the main and the interaction effects with a linear time trend. The resulting estimates are shown in Table 5. It is clear that it is difficult to detect statistically significant trends. For years of education, it is positive and statistically insignificant in the pooled sample, but positive and statistically significant for sons. The trend interaction estimates for cognitive and non-cognitive skills for sons are negative, and statistically significantly so for non-cognitive skills. These results are pretty much confirmed in the LW estimations (see Table A2 in the appendix). Given these results we don't want to push too hard for these trends.

Table 5. Estimates of nature-nurture interactions plus a linear trend

	Pooled	Daughters	Sons		
	(1)	(2)	(3)	(4)	(5)
	Education	Education	Education	Cognitive skill	Non-cognitive skill
BF education	0.146***	0.165***	0.130***	0.232***	0.097
	(0.023)	(0.039)	(0.029)	(0.051)	(0.059)
AP education	0.194***	0.181***	0.211***	0.170***	0.136***
	(0.016)	(0.022)	(0.022)	(0.040)	(0.044)
BF/AP interaction	-0.034**	-0.010	-0.058***	-0.031	0.046
	(0.016)	(0.025)	(0.022)	(0.036)	(0.039)
Trends					
BF education	-0.001	0.001	-0.003	-0.003	-0.001
	(0.002)	(0.003)	(0.003)	(0.005)	(0.005)
AP education	-0.002	-0.003	-0.002	0.004	0.001
	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)
BF/AP interaction	0.002	-0.001	0.004*	-0.002	-0.006*
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Observations	6,788	3,250	3,538	2,493	2,452
R-squared	0.082	0.089	0.093	0.091	0.047

NOTES. Outcomes and parental inputs been standardized w.r.t. year of birth and gender in the population. Controls include year of birth fixed effects for the adoptees as well as linear and quadratic controls for parents' year of birth. Education is standardized with respect to year of birth and gender. The trend is an integer centered at the 1951 birth cohort. Standard errors are clustered on the biological mother.

5.4 Estimations of the importance of main and interaction effects between pre- and post-birth factors for the intergenerational transmission of earnings

We now turn to models for the intergenerational transmission of earnings. We use earnings for fathers only, whereas for the child generation we pool the adopted men and women. The results are presented in Table 6. Results using population data is shown in column 1, whereas estimates from using adoption samples are shown in columns 2-4. The intergenerational correlation coefficient for fathers and children is 0.19. This is somewhat lower estimates than typically found for Sweden, probably because of measurement error in father's earnings, especially for the earlier cohorts. In column 2, we use adopted children, and find that post-birth factors are more important than pre-birth factors. These results are very much in line with Björklund et al. (2006).

Table 6. Estimates of the intergenerational transmission of earnings

	Population	Adoption sample		
	(1) Earnings	(2) Earnings	(3) Earnings	(4) Earnings
BF earnings	0.185***	0.051***	0.046**	0.074***
	(0.001)	(0.012)	(0.018)	(0.021)
AF earnings	-	0.101***	0.099***	0.118***
		(0.012)	(0.014)	(0.018)
BF/AF	-	-	0.004	0.001
			(0.013)	(0.017)
BF*trend				
	-	-	-	-0.004
AF*trend				(0.002)
	-	-	-	-0.003
BF/AF*trend				(0.002)
	-	-	-	0.000
Observations	3,468,404	6,308	6,308	6,308
R-squared	0.037	0.026	0.026	0.027

NOTES. Column 1 presents population estimates and columns 2-4 presents estimates in the adoption sample. The outcome is lifetime earnings, standardized w.r.t. year of birth and gender in the population. Parental earningss are based on an occupational index that reflects standardized lifetime earnings. Controls include year of birth fixed effects for the adoptees as well as linear and quadratic controls for parents' year of birth. The trend is centered at the 1951 birth cohort. Standard errors are clustered on the biological mother.

Next, we turn to models with interaction effect in columns 3. Our estimate of the interaction effect is indistinguishable from zero, which differs from Björklund et al. (2006) who found positive and statistically significant effects for two earnings measures used for fathers for an adoption sample born 1962-1966. This suggests that interaction effects for earnings might have increased over time. However, looking at the results in column 4, we find no evidence of such an increase.

Are our results using the occupation created earnings measure for fathers are sensitive to using actual earnings? We check this by estimating models with actual earnings and our occupation-based earnings for the same sample. Results are shown in Table A3 in the appendix, and the interaction estimates are somewhat larger but still not statistically significant. Notable from this table is also that the estimates for the adoptive father's earnings become smaller when using actual earnings. This is probably due to severe measurement error when trying to measure lifetime earnings for many of the oldest fathers. Hence, it demonstrates the importance of combining occupational information with earnings when creating an earnings measure for the fathers, as we do in the main earnings regressions.

6. Sensitivity analysis

In our analysis we have always standardized the variables against the population distributions, by gender and year of birth. One might worry that such transformations affect our conclusions. It might especially be a concern for our interaction effects when we take the product of two standardized variables. However, our results are very stable if we use the actual years of education of the parents in the regressions (see Table A4 in the appendix, where we use the same specification as in panel B of Table 4).

A potential problem for our interaction estimates is if they are picking up non-linear effects of environment and/or genetic factors. One way to check for this is to add non-linear terms of the parental variables to the regression model. We show results from such an exercise in Tables A5 and A6. We see that for education and earnings, the interaction estimates become statistically insignificant. As it can be difficult to interpret quadratic terms of standardized variables, we also checked the interaction terms from using non-standardized variables. This gave qualitatively very similar results (see Table A7) Note, however, that these non-linear terms might pick up some of the interaction effects, making the estimates a bit difficult to interpret.

7. Conclusions

We estimate the importance of nature-nurture interactions for cognitive and non-cognitive ability, educational attainment and earnings, using a large sample of adopted children and their adoptive and biological parents. More specifically, we regress the outcome for the adopted child on the outcomes for the adoptive parent, the biological parents and the interaction between the two.

Focusing on biological fathers of adopted children, which we argue is a cleaner measure of genetic endowment than the biological mother, we find no or small interaction effects for skill formation. The negative interactions are more pronounced for cognitive skills, sons and earlier in the analyzed period.

The most important result is that nature-nurture interactions for skill formation is zero or non- positive. Hence, a beneficial family environment does not help those with a luck of the draw in the genetic lottery more than the most unfortunate ones. This is an important result as environmental intervention would not be expected to widen genetically inherited inequalities.

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Appendix

Table A1. Full specification in the adoption sample

	Pooled	Daughters	Sons		
	(1)	(2)	(3)	(4)	(5)
	Education	Education	Education	Cognitive skill	Non-cognitive skill
BF education	0.118***	0.141***	0.100***	0.175***	0.0712**
	(0.0164)	(0.0247)	(0.0219)	(0.0276)	(0.0294)
BM education	0.125***	0.122***	0.122***	0.198***	0.108***
	(0.0170)	(0.0240)	(0.0239)	(0.0306)	(0.0348)
AF education	0.105***	0.0980***	0.105***	0.120***	0.111***
	(0.0108)	(0.0154)	(0.0156)	(0.0209)	(0.0227)
AM education	0.0548***	0.0453***	0.0708***	0.0536**	0.0333
	(0.0115)	(0.0162)	(0.0163)	(0.0210)	(0.0223)
BF/AF interaction	-0.0295**	-0.0194	-0.0442**	-0.0236	-0.0391
	(0.0123)	(0.0169)	(0.0185)	(0.0228)	(0.0250)
BF/AM interaction	0.00618	-0.00115	0.0187	-0.0249	0.0211
	(0.0140)	(0.0195)	(0.0206)	(0.0258)	(0.0274)
BM/AF interaction	0.0193	0.0338*	0.000169	-0.00386	0.0174
	(0.0130)	(0.0183)	(0.0191)	(0.0288)	(0.0278)
BM/AM interaction	-0.0206	-0.0229	-0.0151	-0.0485	-0.00768
	(0.0133)	(0.0183)	(0.0185)	(0.0296)	(0.0299)
$P > F_{(1,2,3,4)}$	0.055*	0.3111	0.0877*	0.014**	0.6149
$P > F_{(1,2)}$	0.03**	0.3872	0.0416**	0.051*	0.2825
$P > F_{(3,4)}$	0.226	0.1732	0.5931	0.117	0.8193
Observations	6,788	3,250	3,538	2,493	2,452
R-squared	0.093	0.103	0.102	0.108	0.054

NOTES. Education is standardized with respect to year of birth and gender in the population. Controls include year of birth fixed effects for the adoptees as well as linear and quadratic controls for parents' year of birth. Standard errors are clustered on the biological mother.

Table A2. Estimates of nature-nurture interactions plus a linear trend.

	Pooled	Daughters	Sons		
	(1)	(2)	(3)	(4)	(5)
	Education	Education	Education	Cognitive skill	Non-cognitive skill
BF input	-0.051	-0.072	-0.032	0.166**	-0.065
	(0.032)	(0.047)	(0.044)	(0.080)	(0.092)
AP input	0.390***	0.391***	0.388***	0.306***	0.307***
	(0.028)	(0.040)	(0.040)	(0.075)	(0.087)
BF/AP interaction	0.006	0.023	-0.010	0.004	0.050
	(0.022)	(0.033)	(0.030)	(0.064)	(0.069)
Trends					
BF input	0.004	0.008**	0.000	-0.010	-0.001
	(0.003)	(0.004)	(0.004)	(0.008)	(0.008)
AP input	-0.007***	-0.012***	-0.002	0.012*	0.004
	(0.003)	(0.004)	(0.004)	(0.007)	(0.008)
BF/AP input	-0.002	-0.001	-0.003	-0.009	-0.006
	(0.002)	(0.003)	(0.003)	(0.006)	(0.006)
Observations	6,788	3,250	3,538	2,493	2,452
R-squared	0.077	0.075	0.081	0.089	0.039

NOTES. Outcomes have been standardized w.r.t. year of birth and gender in the population and parental inputs are proxied by a Lubotsky-Wittenberg index (Lubotsky and Wittenberg, 2006) measured on the same scale based on parental education, earnings and an occupational index that reflects non-cognitive skills. Controls include year of birth fixed effects for the adoptees as well as linear and quadratic controls for parents' year of birth. The trend is an integer centered at the 1951 birth cohort. Standard errors are clustered on the biological mother.

Table A3. Estimates using actual earnings vs occupation-based earnings

	(1) Pooled	(2) Daughters	(3) Sons
Panel A			
BF earnings	0.041*	0.080***	0.012
	(0.021)	(0.028)	(0.032)
AF earnings	0.089***	0.085***	0.093***
	(0.015)	(0.020)	(0.023)
BF/AF interaction	0.007	-0.020	0.026
	(0.014)	(0.019)	(0.021)
R-squared	0.024	0.032	0.035
Panel B			
BF earnings	0.075***	0.077***	0.075***
	(0.017)	(0.026)	(0.022)
AF earnings	0.044***	0.036**	0.060**
	(0.014)	(0.016)	(0.025)
BF/AF interaction	0.006	0.001	0.023
	(0.012)	(0.012)	(0.026)
R-squared	0.018	0.026	0.028
Observations	5,364	2,539	2,825

NOTES. The outcome is lifetime earnings standardized w.r.t. year of birth and gender in the population. In panel A, parental earnings are based on an occupational index. In panel B, parental earnings are based on actual lifetime earnings that have been standardized w.r.t. year of birth and gender in the population. Controls include year of birth fixed effects for the adoptees as well as linear and quadratic controls for parents' year of birth. Standard errors are clustered on the biological mother.

Table A4. Estimates using (non-standardized) years of education for parents

	Pooled	Daughters	Sons		
	(1) Education	(2) Education	(3) Education	(4) Cognitive skill	(5) Non-cognitive skill
BF education	0.198***	0.197***	0.212***	0.138***	0.0477
	(0.0504)	(0.0727)	(0.0692)	(0.0292)	(0.0328)
AP education	0.283***	0.224***	0.338***	0.158***	0.0852***
	(0.0452)	(0.0617)	(0.0660)	(0.0273)	(0.0309)
BF/AP interaction	-0.00829*	-0.00484	-0.0122*	-0.00747***	-0.00201
	(0.00480)	(0.00676)	(0.00684)	(0.00273)	(0.00313)
Observations	6,788	3,250	3,538	2,493	2,452
R-squared	0.113	0.142	0.108	0.092	0.047

NOTES. Regression estimates non-standardized using years of education for parents. Controls include year of birth fixed effects as well as linear and quadratic controls for parents' year of birth. Standard errors are clustered on the biological mother.

Table A5. Estimates using education and controlling for non-linear main effects

	Pooled	Daughters	Sons		
	(1)	(2)	(3)	(4)	(5)
	Education	Education	Education	Cognitive skill	Non-cognitive skill
BF education	0.132***	0.151***	0.116***	0.188***	0.071**
	(0.018)	(0.027)	(0.023)	(0.032)	(0.034)
(BF education)^2	-0.003	0.013	-0.012	0.005	0.008
	(0.011)	(0.015)	(0.013)	(0.018)	(0.019)
AP education	0.233***	0.205***	0.248***	0.298***	0.221***
	(0.017)	(0.023)	(0.025)	(0.033)	(0.036)
(AP education)^2	-0.024***	-0.018***	-0.027***	-0.049***	-0.035**
	(0.006)	(0.007)	(0.010)	(0.015)	(0.016)
BF/AP interaction	-0.009	-0.011	-0.011	-0.030	0.005
	(0.012)	(0.018)	(0.017)	(0.022)	(0.024)
Observations	6,788	3,250	3,538	2,493	2,452
R-squared	0.083	0.090	0.094	0.095	0.048

NOTES. Outcomes and parental education are standardized w.r.t. year of birth and gender in the population. Controls include year of birth fixed effects as well as linear and quadratic controls for parents' year of birth. Standard errors are clustered on the biological mother.

Table A6. Estimates using earnings and controlling for non-linear main effects

	Pooled	Daughters	Sons
	(1)	(2)	(3)
	Education	Education	Education
BF earnings	0.039*	0.067**	0.019
	(0.021)	(0.029)	(0.030)
(BF earnings)^2	0.003	0.004	0.000
	(0.008)	(0.011)	(0.011)
AF earnings	0.135***	0.137***	0.138***
	(0.024)	(0.032)	(0.036)
(AF earnings)^2	-0.014**	-0.018*	-0.013
	(0.007)	(0.010)	(0.011)
BF/AF interaction	0.006	-0.021	0.028
	(0.013)	(0.018)	(0.018)
Observations	6,308	3,002	3,306
R-squared	0.028	0.038	0.037

NOTES. The outcome is lifetime earnings, standardized w.r.t. year of birth and gender in the population. Parental earnings are based on an occupational index that reflects standardized lifetime earnings. Controls include year of birth fixed effects as well as linear and quadratic controls for parents' year of birth. Standard errors are clustered on the biological mother.

Table A7. Estimates using years of education for parents and non-linear main effects

	Pooled	Daughters	Sons		
	(1)	(2)	(3)	(4)	(5)
	Education	Education	Education	Cognitive skill	Non-cognitive skill
BF education	0.048	0.035	0.067	0.080	0.031
	(0.033)	(0.047)	(0.047)	(0.056)	(0.063)
$(\text{BF education})^2$	0.001	0.002	-0.000	0.002	-0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
AP education	0.181***	0.188***	0.174***	0.309***	0.175***
	(0.036)	(0.052)	(0.050)	(0.054)	(0.056)
$(\text{AP education})^2$	-0.004**	-0.005**	-0.003	-0.008***	-0.005*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
BF/AP interaction	-0.002	-0.001	-0.003	-0.005*	-0.000
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	6,788	3,250	3,538	2,493	2,452
R-squared	0.082	0.089	0.093	0.095	0.048

NOTES. The outcomes are standardized w.r.t. year of birth and gender in the population. Parental education is measured as year of education. Controls include year of birth fixed effects as well as linear and quadratic controls for parents' year of birth. Standard errors are clustered on the biological mother.

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