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Essays on the roles of families, firms, location, and criminal records

Erika Forsberg

IFAU INSTITUTE FOR EVALUATION OF LABOUR MARKET AND EDUCATION POLICY

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Postal address: P O Box 513, 751 20 Uppsala
Visiting address: Kyrkogårdsgatan 6, Uppsala
Phone: +46 18 471 70 70
Fax: +46 18 471 70 71
ifau@ifau.uu.se
www.ifau.se

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Erika Forsberg

Labor-market inequality

Department of Economics, Uppsala University

Visiting address: Kyrkogårdsgatan 10, Uppsala, Sweden
Postal address: Box 513, SE-751 20 Uppsala, Sweden
Telephone: +46 18 471 00 00
Telefax: +46 18 471 14 78
Internet: http://www.nek.uu.se/

ECONOMICS AT UPPSALA UNIVERSITY

The Department of Economics at Uppsala University has a long history. The first chair in Economics in the Nordic countries was instituted at Uppsala University in 1741.

The main focus of research at the department has varied over the years but has typically been oriented towards policy-relevant applied economics, including both theoretical and empirical studies. The currently most active areas of research can be grouped into six categories:

* Labour economics
* Public economics
* Macroeconomics
* Microeconometrics
* Environmental economics
* Housing and urban economics

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Essays on the roles of families, firms, location, and criminal records

UPPSALA
UNIVERSITET

Dissertation presented at Uppsala University to be publicly examined in Lecture Hall 2 Ekonomikum, Kyrkogårdsgatan 10, Uppsala, Friday, 20 September 2024 at 13:15 for the degree of Doctor of Philosophy. The examination will be conducted in English. Faculty examiner: Professor Matthew Lindquist (SOFI, Stockholms universitet).

Abstract
Forsberg, E. 2024. Labor-market inequality. Essays on the roles of families, firms, location, and criminal records. *Economic studies* 217. 218 pp. Uppsala: Department of Economics, Uppsala University. ISBN 978-91-506-3059-6.

Essay 1 Individuals working in larger labor markets tend to earn more than those working in smaller labor markets, but the reason for this is still unclear. This paper studies whether larger cities provide better occupational skill matches by combining machine learning techniques with data on individuals' productive skills matched with employer data to construct a novel measure of match quality. I show that occupational skill-match quality is higher for individuals living in large local labor markets. Conditional on skills, differences in match quality explain around 30 percent of the city-size wage gap. The higher match quality in larger labor markets is related to a more diversified occupation structure and more learning possibilities in these markets.

Essay 2 (with Martin Nybom and Jan Stuhler) To what extent does the sorting of workers across firms contribute to intergenerational persistence and why? We show that socioeconomic disparities in firm pay premia account for about one third of the intergenerational elasticity of income in Sweden. Firm pay gaps open already at career start, implying that children from more privileged backgrounds find more favorable entry points to the labor market. Their pay advantage widens further in their early careers as they climb the firm pay ladder faster, switch firms more frequently, and secure higher pay gains conditional on switching. Skill sorting explains most of the divergence over the career, but not the initial pay gaps at the career start.

Essay 3 (with Akib Khan and Olof Rosenqvist) Family background shapes outcomes across the life cycle. While the importance of family background varies across countries, less is known about heterogeneities across social groups within a country. Using Swedish data, we compare sibling correlations in skills, schooling, and earnings across fine-grained socioeconomic status (SES) groups. The result from the study shows that sibling correlations decline with parental socioeconomic status. This pattern holds for skills, schooling, and earnings.

Essay 4 (with Hans Grönqvist, Susan Niknami and Mårten Palme) We investigate the effect of being included in Sweden's first online criminal database, which facilitates anonymous and free name-based searches for individuals charged with a crime. Leveraging administrative rules that restricted the identification of individuals charged before specific dates, we estimate the effects by comparing outcomes of exposed and non-exposed individuals. We find significant adverse effects of exposure on earnings but not on employment or criminal recidivism. However, there are significantly stronger detrimental effects on both labor market outcomes and recidivism in defendant subgroups such as those with at least a high school degree, acquitted individuals, and those living in areas with a relatively low concentration of ex-criminals. Our results suggest that stigma is a potentially important but previously unappreciated mechanism explaining responses to criminal justice interactions.

Keywords: Inequality, match quality, local labor markets, intergenerational mobility, firms, criminal records

Erika Forsberg, Department of Economics, Box 513, Uppsala University, SE-75120 Uppsala, Sweden.

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To Anton and Otto

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Uppsala, August 2024
Erika Forsberg

Contents

Introduction

It is a well-documented fact that differences in human capital are one explanation for wage differences, where individuals with more education and skills have been shown to get higher wages (see, for example Card, 1999; Lindqvist and Vestman, 2011). However, individual outcomes in the labor market may not solely depend on their skills. Instead, it is possible that equally productive workers end up having different labor market outcomes due to imperfections in labor markets and the specific labor market conditions they encounter. The labor market could affect individuals in different ways and through different channels, and this thesis explores various dimensions of how the labor market influences individuals' labor market outcomes.

One way in which the labor market might affect individuals is that different individuals meet different geographical labor markets. Different geographical labor markets might provide different labor market conditions and career opportunities. If individuals are perfectly mobile, we might expect them to move to the labor market with the best opportunities. However, in practice, that might not be the case. Individuals are likely to have preferences of where to live (see for example Moretti, 2011). For example, they may prefer to live close to their friends and family and stay in the city where they were born. Thus, individuals with similar skills who are born in different places might face different labor markets and thus might have different labor market opportunities.

Geographical labor markets might differ in several dimensions. One dimension of the labor markets that has been shown to be important is the size of the labor market, where individuals who work in larger labor markets have been shown to, on average, earn more than individuals who work in smaller labor markets (see for example Rosenthal and Strange, 2008; Papageorgiou, 2022; Eliasson and Westerlund, 2022). Chapter 1 examines more about how geographical labor markets affect labor market possibilities by studying how labor market size is related to match quality on the labor market, where match quality is high if individuals work in an occupation where their skills are valuable.

In a perfectly competitive labor market, workers who lose their jobs can, without cost, find an identical job (Manning, 2011). However, in practice, this might not be the case. Jobs vary in attributes, and workers may find themselves better off in one firm compared to another. One dimension of how firms differ from each other is how much they pay their workers. Research has shown that some firms pay their workers more, while others pay less (Abowd et al., 1999).

Thus, if workers are able to enter different types of firms, their earnings might be affected, even if the worker possesses the same human capital. Differences in firm pay have been shown to be an important factor for inequality (see for example Card et al., 2013).

In chapter 2, we examine the role of firms, where we look at the role of firms to explain persistence in income across generations. That is, we study if the fact that children from high-income families earn more themselves is explained by the fact that they are more likely to sort into high-paying firms. However, the role of firms might not be unrelated to the role of human capital. It might be the case that individuals with higher human capital are able to enter better-paying firms. In Chapter 2, we therefore also examine how the sorting of children from high-income families to better-paying firms is related to skills.

While research has shown that family background shapes individuals' outcomes, it is possible that family background does not matter equally everywhere. In Chapter 3, we examine how the importance of family background, measured as siblings' correlations, varies depending on family background.

In a world with perfect information, the employees could observe all relevant information about the individual and choose the individual best suited for the job. However, in practice, this might not be the case. There might be information frictions where not all information is available to the employee, or there is a cost of obtaining the information (see Stigler, 1962 for an early discussion about this). Thus, in the case of imperfect information, what information is available in the labor market might affect the worker's job possibilities.

With technological change, information about the worker that was previously unavailable might suddenly become available (see for example Autor, 2001). Depending on what information becomes more available, the worker might either benefit or be harmed by its viability. Chapter 4 studies more about the effect of new information becoming available by studying how the introduction of an online database, which reveals the criminal history of the individuals, affects their labor market possibilities.

Thus, the labor market conditions the individual faces might affect their career opportunities. Different dimensions of the labor market, such as geographical labor markets, firms, and information access, might matter for individuals' labor market outcomes. Below is a short summary of the different chapters in the thesis.

Essay 1: Labor market size and multidimensional skill-mismatch

While earlier research has provided evidence that individuals who work in larger labor markets earn more than individuals who work in smaller labor markets (see, for example Rosenthal and Strange, 2008; Papageorgiou, 2022; Eliasson and Westerlund, 2022), the mechanism behind the higher wage premium in larger labor markets still remain unclear. In Chapter 1, I examine one mechanism behind the city-size wage premium by studying whether larger cities provide better occupational skill matches.

To study how match quality differs depending on city size, I construct a match quality measure using Swedish data on eight different types of skills from the military enlistment test. The tests include both cognitive (inductive, verbal, spatial, and technical ability) and non-cognitive skills (social maturity, intensity, psychological energy, and emotional stability). To measure how useful different skills are in different occupations, I estimate the return to skills in different occupations. Instead of assuming any functional form I estimate the return to skills using a random forest. I also use a random forest to estimate the return to skills on the whole market. The match quality measure is then constructed as the estimated return to skills in the occupation where the individual works minus the market return to skills. Thus, the intuition behind the match quality measure is that individuals are well-matched if they work in occupations with high returns to their skills compared to what they could receive on average.

The results show that match quality is higher in larger labor markets than in smaller ones. Conditional on skills, the difference in match quality explains around 30 percent of the city-size wage gap. The city-size match-quality gap is small for young individuals but increases with age. This result is consistent with initial uncertainty about the optimal match that decreases with experience (see Guvenen et al., 2020; Fredriksson et al., 2018). The increase in match quality over the life-cycle seems to come from both more frequent, and better, occupation switches in larger labor markets. When examining mechanisms for higher match quality in larger labor markets, the analysis suggests that the higher match quality in larger labor markets is driven by both more occupation diversity and by more learning possibilities in larger labor markets.

It should be noted that the finding that match quality is higher in larger labor markets does not necessarily mean individuals would be better off by moving to large labor markets since the utility of the individual is also affected by the cost of living and their preferences about where to live. Thus, while the result here highlights the limited ability to find a good match in smaller labor markets, research is needed on how to best solve this problem and increase match quality in smaller labor markets.

Essay 2: Labor-market Drivers of Intergenerational Earnings Persistence (with Martin Nybom and Jan Stuhler)

Children from high-income families, on average, earn more than children from low-income families. Most of the literature that examines the mechanism behind the persistence in earnings across generations has focused on the role of human capital (see for example Becker and Tomes, 1979; Solon, 2004; Heckman and Mosso, 2014).[1] In Chapter 2, we instead focus on the role of labor market factors, and more precisely, firms, to explain the persistence of income between generations.

We show that sorting to higher-paying firms explains an important part of the persistence in earnings across generations, explaining almost 30 percent of the intergenerational earnings correlation. The contribution of firms rises to 38% if we incorporate differences in returns to experience across firm types in our analysis.

However, part of the sorting to higher-paying firms is still related to skill sorting. Using data on the estimated individual fixed effects, education, and skills, we show that around half of the SES gradient in firm premia is explained by skill sorting. However, even conditional on skills, 50 percent of the SES gradient in skill premia remains, indicating that investments in human capital do not seem to be enough to equalize income for children from families of different incomes.

Moreover, we study the life cycle dynamics of the firm premia depending on family background. The result in Chapter 2 shows that children from high-income families start off at better-paying firms already career start, and the difference in firm premiums continues to widen at the beginning of the career. While most of the widening in skill premium can be explained by skill sorting, most of the initial gap in firm premia cannot be explained by skill sorting. The results indicate that while human capital seems to be an important explanation for why children from high-income families climb further in the job ladder, coming from families with high income might give better starting positions in the career for reasons unrelated to the skill of the child.

Essay 3: Do sibling correlations in skills, schooling, and earnings vary by socioeconomic background? Insights from Sweden (with Akib Khan and Olof Rosenqvist)

In Chapter 3, we examine how the importance of family background, measured as siblings' correlations, varies depending on family background. The sibling correlation shows the correlation in outcome between a pair of siblings. We use Swedish register data to compare sibling correlations in skills, schooling, and earnings across fine-grained groups defined by parental socioeconomic status.

The result from the study shows that sibling correlations decline with parental socioeconomic status. This pattern holds for skills, schooling, and earnings.

For the sibling correlation in income and education, the decline is driven by an increase in within-family variation by parental SES, indicating that siblings become less similar to each other in terms of income and education in families

[1] However, see Dobbin and Zohar (2023) and Engzell and Wilmers (2024) for some notable expectations of studies that also focus on labor market drivers for intergenerational mobility.

with higher socioeconomic status. In contrast, the decline in sibling correlations of skills is driven by a decline in between-family variation. Thus, the increase in within-family variation, in income and education, by parents' socioeconomic background does not seem to be driven by differences in skills. A potential explanation is that high-ability children from low SES families cannot reach their full potential in terms of earnings and educational attainment but instead end up closer to their lower-ability siblings (as also suggested by Papageorge and Thom, 2020 and Ronda et al., 2022).

Essay 4: Making Background Salient: The Effects of Open Access Criminal Databases on Offender Behavior (with Hans Grönqvist, Susan Niknami and Mårten Palme)

Technological advances have led to the creation of large online criminal databases that are easily accessible to the public. However, the impact of this accessibility on the offenders in these databases remains unclear. In chapter 4, we investigate the effect of being included in Sweden's first online criminal database, which facilitates anonymous and free name-based searches for individuals charged with a crime.

To estimate the causal effect of being included in the database, we use the fact that legally, courts were only obliged to provide criminal charges five years back in time, giving a cutoff date for when the company was able to get access to courts. This allows us to compare outcomes between two groups: those charged after the specified cutoff date, whose criminal records are consequently exposed online, and those of non-exposed offenders who were charged prior to the cutoff date and consequently do not have their criminal history online. We compare outcomes for these two groups using a difference-in-difference design.

The result from the study shows that being included in the database has negative effects on earnings but not on employment or criminal recidivism. However, there are significantly stronger detrimental effects on both labor market outcomes and recidivism in defendant subgroups such as those with at least a high school degree, acquitted individuals, and those living in areas with a relatively low concentration of ex-criminals. Our results suggest that stigma is a potentially important but previously unappreciated mechanism explaining responses to criminal justice interactions.

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Essay 1: Labor market size and multidimensional skill-mismatch

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1 Introduction

A large empirical literature has shown that individuals who work in larger labor markets earn more than individuals who work in smaller labor markets (see for example Rosenthal and Strange, 2008; Papageorgiou, 2022; Eliasson and Westerlund, 2022). This city-size wage premium should partly be explained by productivity differences (see for example Glaeser and Maré, 2001), otherwise firms in the tradable sector would increase profits by relocating to smaller areas where both wage and land costs are lower (Moretti, 2011). From the workers' perspective, productivity differences could remain due to mobility frictions, such as higher cost of living in larger cities or individuals having preferences to reside in certain areas.

To be able to address geographical inequalities, it is important to understand the mechanism behind the city-size wage differential. Theoretically, competing mechanisms such as increased opportunities for individuals to augment human capital and higher match quality in larger labor markets have been suggested as explanations for the wage difference (see for example Puga, 2010). While research has provided evidence on learning and city size (see for example De La Roca and Puga, 2016), the empirical evidence on how match quality differs between large and small labor markets is still limited. The main reason is the challenges involved in measuring match quality. Most of the existing literature instead relies on indirect evidence. For instance, Wheeler (2008), Bleakley and Lin (2012), and Korpi and Clark (2019), show job change patterns that are consistent with higher match quality in larger cities.

In this study, I use rich population-based data to construct a novel measure of occupational skill-match quality that allows me to provide direct evidence of the importance of this mechanism in explaining the city-size wage differential. The measure draws from advances in the literature on multidimensional mismatch (see Guvenen et al., 2020; Fredriksson et al., 2018), which highlights that different combinations of skills might be useful in different jobs. To construct the match quality measure, I use Swedish data on eight different types of skills from the military enlistment test. The tests are conducted at age 18 or 19 and include both cognitive (inductive, verbal, spatial, and technical ability) and non-cognitive skills (social maturity, intensity, psychological energy, and emotional stability). As a proxy for skill requirements in each occupation, I estimate the return to skills in different occupations. Because theory gives little guidance on how to model the relationship between skills and wages, I pursue a non-parametric approach to estimation. Specifically, I use recent advancements in machine learning to improve on the earlier match quality measure and estimate a random forest model trained on tenured workers with data on the eight skills.[1] Analogously, I use a random forest to estimate the return to skills on the whole market. The match quality measure is

[1] See recent work by Almgren et al. (2022) who also use a random forest to proxy for skill requirements in occupations.

then constructed as the estimated return to skills in the occupation where the individual works, minus the market return to skills. Thus, individuals are well-matched if they work in occupations with high returns to their skills compared to what they could receive on average based on their skills.

My results show that match quality is higher in larger compared to smaller labor markets. This result holds conditional on skills and in a subset of workers who move across labor markets of different sizes. Conditional on skills, the difference in match quality explains around 30 percent of the city-size wage gap. The magnitude is in line with the result of Papageorgiou (2022), who uses a calibrated model and shows that occupational match quality explains around 35 percent of the city-size wage premium in the United States. The difference in match quality between individuals in larger and smaller labor markets is especially large for high-skilled workers, measured either by their cognitive skills, non-cognitive skills, or education. This is well in line with the fact that the city-size wage premium is higher for highly skilled individuals (see Bacolod et al., 2009).

The city-size match-quality gap is small for young individuals but increases with age. This result is consistent with initial uncertainty about the optimal match that decreases with experience (see Guvenen et al., 2020; Fredriksson et al., 2018). The increase in match quality over the life-cycle seems to come from both more frequent, and better, occupation switches in larger labor markets. The finding of an increase in match quality over the life-cycle is also in line with the finding by Eckert et al. (2022). The authors find that refugees placed in a large labor market initially have similar wages as refugees placed in smaller labor markets and experience faster wage growth through sorting to better (urban) jobs.

I examine two mechanisms behind the higher match quality in larger cities: occupation diversity and learning possibilities. If a more diverse set of occupations exists in large labor markets, this could give individuals more occupations to choose from and, therefore, could increase the likelihood that individuals find a good match (see Papageorgiou, 2022). Theoretically, higher learning possibilities in larger cities could come from a lower cost of switching occupations in larger cities (Wheeler, 2008). This allows individuals to explore different occupations and learn about what their optimal match is. Moreover, working in larger cities gives workers more valuable knowledge (Glaeser and Maré, 2001; De La Roca and Puga, 2016), which in some cases might be needed to be able to enter high-match quality occupations.

The analysis gives suggestive evidence that both of these mechanisms are at work. By constructing a Herfindahl-Hirschman Index (HHI) of occupation concentration in the labor markets, I show that larger labor markets have a more diversified occupation structure. Moreover, the difference in match quality in large and small labor markets partly seems to be explained by the difference in occupation diversity. Following the method in De La Roca and Puga (2016), I also show that experience obtained in larger labor markets is more valuable for future match quality than experience obtained in smaller labor markets. In addition, prior experience from large labor markets is valuable for individuals who work in small labor markets, suggesting that learning might be an important mechanism for higher match quality in larger labor markets.

This paper is related to several strands of literature. First, this paper is related to the strand of research that employs AKM models (see Abowd et al., 1999) to investigate mismatch between individuals and firms in relation to city size. These studies estimate assortative matching based on one-dimensional skills and tend to find that larger cities have a higher degree of assortative matching (see for example Dauth et al., 2022; Andersson et al., 2007; Leknes et al., 2022; Card et al., 2021).[2] While these studies have focused on the mismatch between workers and firms, I shift the focus to occupation mismatch. Occupational match quality plays a vital role in explaining workers' earnings (Guvenen et al., 2020). Consequently, investigating the role of occupation match quality in explaining the city-size wage premium is crucial. Furthermore, while prior studies employing AKM models have examined matching in terms of one-dimensional skills, my research utilizes data on eight distinct types of skills and estimates a multidimensional match quality measure. The match quality measure allows for both horizontal dimensions of mismatch, in terms of workers being overqualified and underqualified for their jobs, and vertical dimensions of mismatch, in terms of workers having the wrong set of skills for their jobs.

Secondly, this paper complements the literature exploring the mismatch between individuals' education and the educational requirements of their jobs, which tends to find higher education mismatch in smaller cities (see Abel and Deitz, 2015; Berlingieri, 2018; Boualam, 2014; Koster and Ozgen, 2021). Moreover, smaller cities seem to have more mismatch in terms of earlier industry experience (Harmon, 2013). However, using survey questions Andini et al. (2013) finds small effects of density on match quality. In contemporaneous work, Moretti and Yi (2024) show that after displacement, workers in larger labor markets are more likely to find a job with higher match quality, as indicated by the fact that they are more likely to work in an industry relevant to their college major, be employed in the same industry as before, and that the new job is likely to last longer.

In contrast to earlier studies, this paper looks at mismatch in terms of skills that are measured at age 18 or 19, when the individuals are unlikely to have entered the labor market. Unlike education choices, which may be influenced by local labor market conditions, pre-labor market skills are unlikely to be shaped by local labor market conditions. Consequently, this study enriches

[2]In contrast Mion and Naticchioni (2009) find a negative association between market size and assortative matching in Italy and Figueiredo et al. (2013) find limited evidence on more associative matching with more firm clustering within the same industry in Portugal.

the existing literature by shedding light on match quality of pre-determined skills, and suitable occupations in markets of different sizes. Moreover, I provide novel evidence on the life-cycle dynamics of match quality in relation to labor market size and direct evidence on how the city-size match quality gap is related to learning possibilities and occupation diversity.

The paper is structured as follows: Section 2 presents the conceptual framework and section 3 describes the data. In section 4, the empirical approach is explained, and section 5 presents background about how wage difference and labor market size look in the Swedish setting. The main results are presented in section 6 and the mechanisms behind the higher match quality in larger labor markets are examined in section 7. Section 8 explores the robustness of the results and the conclusions are discussed in section 9.

2 Conceptual framework

This section presents a simple framework to illustrate the idea behind the difference in match quality between large and small labor markets. Before entering the labor market, the individual has a set of different skills that are useful in different occupations. The more similar the skills obtained by the individual are to the skills valued in the occupation, the higher the match quality. Higher match quality increases productivity, which is shared with the worker in terms of higher wages.

In a world without frictions, individuals would move to labor markets with higher match quality until the match quality between different labor markets equalizes. However, in the presence of mobility frictions, this might not happen. To get the idea behind the mobility friction, consider a utility function similar to Moretti (2011).

$$U_{iL} = W(MQ,X)_{iL} - r_L + A_L + e_{iL}$$

The utility for individual (i) living in the local labor market (L) depends on the wage (W), the cost of living (r), amenities (A) and idiosyncratic preferences e_{iL}. The idiosyncratic preferences e_{iL} show how much the individual values the labor market holding the wage and amenity constant. For example, Moretti (2011) claims that being born in a city or having relatives in the city might give a higher value of e_{iL}. If e_{iL} is high, regional mobility will be low. Yagan (2019) shows that individuals do not seem to migrate to new places to any great extent as a response to economic shocks, indicating that mobility frictions might be high in practice.

Match quality (MQ) is assumed to have an impact on utility by affecting the wage. Productive skills also depend on a vector of other worker characteristics X, which, for example, can be the age of the worker. If workers get direct utility from working in an occupation that matches their skill set, match quality can also enter the utility function through the amenity. If idiosyncratic preferences e_{iL} are high, individuals might not move even in the case of large differences in real wages and amenities. Thus, the implication from the equation above is that in the presence of mobility frictions, match quality might differ between different labor markets.

In the presence of mobility frictions, the possibility of forming good matches in the labor market has been theorized to be higher in large labor markets (Puga, 2010). There are at least three mechanisms for why match quality might be higher in larger labor markets: occupation diversity, frictions, and learning possibilities.

2.1 Occupation diversity

Larger labor markets have been shown to have a more diversified occupation structure in the sense that more different occupations exist in large labor markets (Papageorgiou, 2022; Korpi, 2007), where especially scarce specialist occupations are more common in larger cities (Duranton and Jayet, 2011). A more diversified occupation structure gives the individual more occupations to choose from which could increase the likelihood that individuals find the optimal match given their skills.

2.2 Search frictions

The cost of switching jobs and occupations has been theorized to be lower in larger cities since larger cities have more job openings (Puga, 2010; Bleakley and Lin, 2012). With more job openings, individuals could apply to many jobs at the same time and be able to change jobs when they like instead of waiting for the job opening. Thus, lower search frictions speed up the matching process.

2.3 Learning possibilities

Earlier research about match quality has shown that work experience and job switches are important for learning about the optimal match (Guvenen et al., 2020; Fredriksson et al., 2018). A lower cost of switching occupations in larger labor markets will allow the individual to change occupations more and thus explore more different occupations (Wheeler, 2008). Thus, less search frictions in larger labor markets might give individuals the opportunity to learn about what occupations are a good match for them given their skills. Moreover, working in larger cities has been shown to provide larger human capital learning possibilities compared to working in a smaller city (Glaeser and Maré, 2001; De La Roca and Puga, 2016). Thus, experience from working in larger cities might give workers valuable knowledge, which might open up possibilities to enter some new occupations with higher match quality. Finally, larger labor markets have more education possibilities (Frenette, 2006). For some occupations, a university education might be necessary for entering the occupation. Thus, more learning through university education could open doors to be able to enter more occupations.

Taken together, from theory, we could expect higher match quality in larger labor markets, driven by more occupation diversity in larger cities, lower search frictions, and more learning possibilities. To implement policies to increase match quality in small labor markets, it is vital to know why match quality differs between labor markets of different sizes. Thus, section 7 tries to separate between the mechanisms. If occupation diversity drives the result, controlling for a measure of occupation diversity is expected to reduce the estimated city-size match quality gap. If learning possibilities in larger labor markets are driving the result, we would expect that the gain in match quality from working in a large labor market should not be immediate; instead, match quality is expected to increase over time when working in a large compared to a small labor market.

3 Data

I use administrative wage data collected by Statistics Sweden. Occupation data are available for a large part of the Swedish population, covering almost 50 percent of all private and public sector workers.[3] Skills are measured with scores from tests during the military enlistment for those who enlisted between the years 1969 and 1994 when almost all males participated in the military drafting. The enlistment is done at age 18 or 19. Since military enlistment was only mandatory for males, the data is limited to only include males.

The cognitive measures include four cognitive skills: inductive, verbal, spatial, and technical ability. The non-cognitive measures also capture the four skills: social maturity, intensity, psychological energy, and emotional stability. The evaluation of the non-cognitive score is done by a psychologist in a 20-minute interview (see Mood et al., 2012); before the interview, the psychologist has access to a form where the tested has answered questions about friends and family. In some years, the cognitive tests are graded on a scale of 0-25, and in others years, a scale of 0-40. The non-cognitive scores are measured on a scale of 1-5. To make the test scores comparable, all test scores have been standardized within each year of enlistment to have a mean of 0 and a standard deviation of 1.

[3]The wage and occupation information is collected during a measurement week (in September–November) each year. To be included in the sampling, the individual needs to be employed for at least one hour during the sampling week. Sampling is stratified by firm size and industry, and small firms in the private sector are underrepresented.

Occupation data is used on the three-digit level for the years 1996-2013, with the ssyk96 definition, in turn based on the international isco88. Some individuals have multiple occupation observations in a given year (around 4 percent of the sample). For these individuals, only the main occupation is used, where the main occupation is defined as the occupation with the highest wage expressed in full-time equivalent wages. In the sample, there exist 111 occupations. [4] Wage is measured as full-time equivalent wages. Furthermore, the data is linked to data on education, firms, and the municipality the individual lives in.

To define labor markets, I rely on Statistics Sweden's definition of local labor markets defined from commuting patterns. The local labor market definition 2013 is used, and thus, municipalities are assumed to belong to the same local labor market during the whole sample period. This definition gives 73 local labor markets. In the main specification, the log of the number of inhabitants in the local labor market is used to measure labor market size. In some cases, a categorical definition of the local labor market is used. The labor market is then divided into three size categories. Large labor markets are defined as labor markets with more than 500,000 inhabitants, consisting of three labor markets: Stockholm-Solna, Malmö-Lund, and Göteborg. Medium-sized labor markets are defined as labor markets with more than 100,000 and less than 500,000 inhabitants, consisting of 20 local labor markets. The small labor markets are labor markets with less than 100,000 inhabitants, consisting of 50 local labor markets. The number of inhabitants used is the number of inhabitants in the local labor market in 2013, defined by SCB.

To be able to use the military and occupation data, the sample is restricted to men born between 1951 and 1976, with non-missing occupation data from 1996 to 2013. The sample is then restricted to those with data on the military test scores, which is available for 80 percent of the sample. Of this sample, 17 percent miss test scores data on any of the cognitive or non-cognitive sub-tests. The sample further excludes those individuals. To train the random forest, 20 percent of all tenured individuals are used, where tenured are defined as individuals who have worked in the occupation for at least three years. The test data consists of the remaining 80 percent of the tenured individuals and all individuals who never have tenure. The results in the paper use the individuals in the test data.

Table 1 summarizes the data by labor market size. As can be seen in the table, even if most labor markets are small, the number of observations is larger in large labor markets. Thus, a large part of the population lives in large labor markets. In panel B, population characteristics are shown divided by labor market size. The population in large labor markets is slightly younger.

[4] In total, there exist 113 occupations at the three-digit level. However, two occupations are excluded because they have less than one tenured worker (the excluded occupations are photo models and street market salespersons).

Cognitive skills, non-cognitive skills, and education levels are higher in larger labor markets, in line with individuals with high skills receiving a higher city-size premium (Bacolod et al., 2009; Andersson et al., 2014; Koster and Ozgen, 2021; Carlsen et al., 2016; Neves Jr et al., 2017) and individuals with high skills being more likely to migrate to large labor markets (see Bacolod et al., 2021). The summary statistics also show that descriptively, wages and match quality are highest in large labor markets and smallest in small labor markets.

Table 1. *Summary statistics*

	Large labor markets (1)	Medium sized labor markets (2)	Small labor markets (3)
Panel A. Labor markets			
Number of labor markets	3	20	50
Number of observations	2,660,719 (44%)	2,407,657 (40%)	937,253 (15%)
Panel B. Population characteristics			
Age	41.85	41.90	42.49
Cognitive skill	0.29	0.09	-0.036
Non-cognitive skills	0.14	0.07	-0.01
Share University education	49.40	37.70	28.86
LN Wage	10.35	10.22	10.18
Match quality	0.097	-0.06	-0.11

Notes: This table provides summary statistics separate by the size of the local labor market. I rely on Statistics Sweden's definition of local labor markets based on commuting patterns. The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013. The skill and match quality measure is standardized to have mean 0 and standard deviation 1.

4 Empirical approach

This section describes how the match quality measure is constructed and presents the method for studying the difference in match quality in labor markets of different sizes.

4.1 Match quality measure

To construct the match quality measures, I use data on wages and eight different types of skills, both cognitive (inductive, verbal, spatial, and technical ability) as well as non-cognitive skills (social maturity, intensity, psychological energy, and emotional stability). The skills measures have been shown to be an important predictor of wages (Lindqvist and Vestman, 2011). The idea behind the match quality measure is that different combinations of skills might be useful in different occupations, and individuals have high match quality if they work in an occupation where their skills are highly valued. I follow Fredriksson et al. (2018) and use tenured workers to proxy for skill requirements. The conclusions remain similar when using O*Net data to proxy for skill requirements in the occupation.[5]

The match quality measure is then constructed by estimating returns to skills in each occupation. The functional form of the match quality measure is theoretically unclear. I thus deviate from the linear match quality measure used in Fredriksson et al. (2018) and instead use recent advancement in the machine learning literature and estimate the match quality measure using a random forest (in similarity with Almgren et al., 2022). [6]

The random forest is trained on data on each of the eight skills and residualized wages, where log wages are residualized on age and year-fixed effects. The training sample is constructed from a random sample of 20 percent of all tenured individuals. Individuals from the training sample are then excluded from the test data. All results shown in the paper only include individuals in the test data. The random forest is trained with residualized wages as the outcome and the eight skills as the independent variables. Thus, the random forest predicts wages based on skills. Separate random forest algorithms are estimated for each occupation, allowing different interactions of skills to be differently important for returns in each occupation. Appendix A.1 shows the relationship between predicted wages based on skills from the random forest for the test data and actual residualized wages. As can be seen in Figure A1 there is a clear positive relationship between the predicted wages from the random forest and actual wages.

Highly skilled individuals will have higher returns in all occupations. To take away the direct impact of skill, I construct the match quality measure as the return to skill for the occupation the individual works in minus the market return to skill. The idea is that the match quality is higher if an individual works in an occupation with higher returns to skills than they could receive on average on the labor market. To estimate the market return, I train the random forest on all workers in the training sample. Match quality is then defined as predicted earnings based on skills in the occupation the individual works in minus market returns:

[5] Section 8.3 provides results where the match quality measure instead are constructed using O*NET data to proxy for skill requirement in each occupation, following Guvenen et al. (2020).

[6] Fredriksson et al. (2018) use two different match quality measures, one using the skills of tenured workers and one using the estimated return to skills. The match quality measure in this paper is more similar to the estimated return to skills match quality measure, where match quality is defined as $MQ_{ij} = \sum_{s=1}^{8}(\hat{\beta}_{js} - \hat{\beta}_s)X_{is}$, where $\hat{\beta}_{js}$ is the estimated return to skill s in job j, $\hat{\beta}_s$ is the estimated return to skill s on the market, and X_{is} is the skill level of skill s for individual i. In difference to Fredriksson et al. (2018), this study focuses on match quality on the occupation level instead of job level and uses a random forest instead of assuming any functional form of the match quality measure. Section 8.3 presents results for when using the linear version of the match quality measure.

$$MQ_{io} = Return_{io} - Return_{iM}$$

where $Return_{io}$ is the predicted return estimated with the random forest based on the skills for individual i working in occupation o, and $Return_{iM}$ is the estimated market return for individual i. To ease interpretation, the match quality measure is then normalized to have a mean zero and standard deviation equal to one. Figure A2 in appendix A.1 shows match quality plotted against skills. The figure shows a negative relationship between match quality and skills, indicating that higher match quality in larger cities can not be driven by higher skill levels in larger cities. Instead it seems like individuals with higher skill levels have a harder time finding a good match compared to individuals with lower skill levels.

Since match quality is predicted using only skills, this implies that two individuals with the same skills who work in the same occupation will have the same match quality, even if they live in different labor markets. Moreover, the only way for an individual to change match quality is by changing occupation.

4.2 Methodology

To estimate the difference in match quality depending on labor market size, I estimate equation (1) below, using OLS.

$$MQ_{ito} = \beta Log(Citysize)_{itL} + X_{it} + \varepsilon_{it} \ (1)$$

The dependent variable of interest is MQ_{it}, the match quality for individual i in time period t working in occupation o. The independent variable of interest is $Log(Citysize)_{itL}$, the log of the number of inhabitants in the local labor market. Appendix A.2 presents results with labor market size as categorical variables, by including dummy variables for living in medium or large labor markets compared to the omitted category living in a small labor market.

In the main specification, X_{it} includes age-fixed effects, year-fixed effects, and second-order polynomials in each of the eight skills to compare individuals with similar skill levels. Furthermore, alternative specifications are presented where X_{it} controls for additional individual characteristics such as education, and, in some specifications also individual fixed effects. It should be noted that, while controlling for education allows comparing more similar individuals, more education possibilities in larger labor markets could also be a mechanism for why individuals in larger labor markets can obtain higher match quality. Individual fixed effects control for all factors that are constant within the individual over time. However, when using individual fixed effects, the effect of local labor market size on match quality is identified from individuals who move between local labor markets of different sizes. If individuals dislike to move, we can expect that individuals who still choose to move to this for a reason, for example, to improve their occupation match. Thus, we can expect the estimate for the sub-population of movers to differ from the effect identified for all individuals.

5 Wage differences and labor market size

Research has shown that individuals living in larger labor markets earn more than those living in smaller labor markets. This pattern has been shown to hold in the United States (Papageorgiou, 2022), and in different European countries (see for example Rosenthal and Strange, 2008) including Sweden (see Eliasson and Westerlund, 2022). Table 2 provides evidence on wage premiums in larger labor markets, also in the setting studied here, by estimating regression 1 with log wage as the dependent variable. Conditional on only year and age fixed effects, the city-size wage elasticity is 0.053, indicating that a 1 percent larger city is associated with 0.05 percent higher wages. Adding controls for skills, the wage elasticity is still 0.039, which means that conditional on skills wages are 12 percent higher in large compared to small labor markets (see table A2 in appendix). Adding controls for education in addition to skills only reduces the estimate slightly, and the wage elasticity of city size is still 0.036.

The magnitude of the effect changes somewhat when individual fixed effects are included, in line with the effect now being identified from movers which might differ from the population. However, even when including individual fixed effects, wages are still higher in larger compared to smaller labor markets.

Table 2. *Wage and labor market size*

VARIABLES	(1) Ln Wage	(2) Ln Wage	(3) Ln Wage	(4) Ln Wage
Log population size	0.053***	0.039***	0.036***	0.015***
	(0.000)	(0.000)	(0.000)	(0.000)
Year Fe	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Control skills	No	Yes	Yes	No
Education control	No	No	Yes	No
Individual FE	No	No	No	Yes
Observations	6,005,629	6,005,629	6,002,946	6,005,629

Notes: This table provides results estimating equation 1, with log wage as the dependent variable, estimating the difference in log wages for individuals who live in labor markets of different sizes. The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013. In column 4, where individual fixed effects are included, the age dummies are normalized to be constant between 45 and 54 to avoid multicollinearity between age and year-fixed effects. Standard errors are adjusted for 659,206 clusters at the individual level.*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Figure 1 below shows a map of the distribution of labor market size in Sweden. As can be seen in the map, smaller and larger labor markets exist in all parts of Sweden.

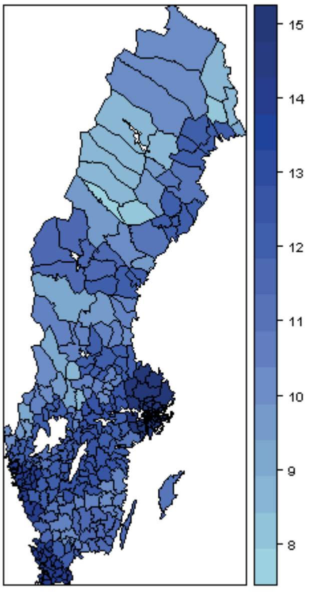


Figure 1. Labor market size Sweden

Notes: The figure plots the log size of local labor markets in Sweden. I rely on Statistics Sweden's definition of local labor markets based on commuting patterns. Darker colors represent larger labor markets.

6 Results

This section presents the key results on the city-size match quality gap. Section 6.1 presents the main results on how match quality differs depending on labor market size estimated with regression 1. It is possible that not all individuals gain equally in terms of match quality from living in a large labor market. Section 6.2, therefore presents how match quality differs with labor market size separately depending on an individual's skill level. Section 6.3 presents life-cycle dynamics on how match quality develops over the life-cycle for individuals who live in small, medium, and large labor markets.

6.1 Main results

Table 3 presents results for how match quality varies with local labor market size. In column 1, we see that individuals living in larger labor markets have higher match quality than individuals in smaller labor markets. Column 2 controls for second-order polynomials in each of the eight skills. Controlling for skills has a small and positive impact on the estimate, indicating that the difference in match quality depending on labor market size is not driven by different skill compositions of workers depending on local labor market size. When controlling for skills, the estimates suggest that an increase in city size by 10 percent is associated with 0.007 standard deviations higher match quality. Thus, the results in the table are in line with the theory of higher match quality in larger labor markets.

Column 3 in addition controls for education. The estimate falls somewhat, indicating that one mechanism for higher match quality in larger labor markets might be more education possibilities, which might increase the potential to realize the optimal match. However, even conditional on education match quality is higher in larger local labor markets. Column 4 includes individual fixed effects, and thus the estimated effect of local labor market size on match quality comes from individuals who move between local labor markets of different sizes. When controlling for individual fixed effects the effect decreases substantially. As discussed before, this could be because the individual fixed effect captures individual characteristics that affect match quality. On the other hand, it could also be because the effect is now identified by movers. If individuals dislike to move, they might, for example, only choose to move if they receive higher match quality. Nevertheless, when including individual fixed effects, match quality is higher in larger compared to smaller local labor markets.

Table A2 in Appendix B shows results with labor market size as a categorical variable, where match quality is divided into large, medium, and small labor markets. The conclusion from this specification is similar: larger and medium-sized labor markets have higher match quality than small labor markets. From Figure A4 it is clear that the relationship between match quality and city size is relatively linear, where match quality gradually increases with log city size.

The result in Table 3 speaks to the role of match quality as one explanation for why larger labor markets have higher wages. However, the question still remains if match quality is an important contribution to the city-size wage premium or not. In Table 4 I quantify how much of the wage premium in large local labor markets can be attributed to match quality. This is done by estimating the city-size wage premium regression and comparing the coefficient for local labor market size with and without controls for match quality. Thus, the coefficient in column 2 shows how much of the city-size wage premium remains when match quality is held constant, and the difference between the coefficients in columns 1 and 2 shows how much of the wage gap between large and small labor markets can be attributed to match quality. Note that controls for skills are included in both columns 1 and 2. Thus, the difference between the coefficient in columns 1 and 2 shows how much match quality

Table 3. *Main result*

VARIABLES	(1) Match quality	(2) Match quality	(3) Match quality	(4) Match quality
Log population size	0.064***	0.070***	0.059***	0.009***
	(0.001)	(0.001)	(0.001)	(0.001)
Year Fe	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Control skills	No	Yes	Yes	Yes
Control education	No	No	Yes	No
Individual FE	No	No	No	Yes
Observations	6,005,629	6,005,629	6,002,946	6,005,629

Notes: This table provides result estimating equation 1, estimating the difference in match quality for individuals who live in labor markets of different sizes. The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013. In column 4, where individual fixed effects are included, the age dummies are normalized to be constant between 45 and 54 to avoid multicollinearity between age and year-fixed effects. Standard errors are adjusted for 659,206 clusters at the individual level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

contributes to the conditional wage premium, where the fact that the population in small and large labor markets have different skill composition has already been taken into account.

Comparing the coefficients in columns 1 and 2 suggests that match quality explains around 30 percent of the city-size wage gap. Occupational match quality thus explains an important part of the wage difference between smaller and larger labor markets. The magnitude is in line with the result of Papageorgiou (2022), who uses a calibrated model and shows that occupational match quality explains around 35 percent of the city-size wage premium in the United States.

When using match quality as a control instead of as the dependent variable, it is important to get as accurate measure of match quality as possible. If the match quality measure is estimated with error, controlling for match quality might not capture the true match quality effect, and the contribution of match quality to the city-size wage gap might be underestimated. When using the linear version of match quality as a control instead of the non-parametric match quality measure estimated with the random forest, match quality is estimated to explain around 10 percent, instead of 30 percent of the city-size wage gap (see table A1 in appendix). Thus, even if the conclusion of higher match quality in larger labor markets holds with the linear match quality measure (see table 10), using the recent advancements in machine learning to estimate a non-parametric version of the match quality measure seems to allow for a more accurate quantification of the importance of match quality to explain the city-size wage gap.

Table 4. *Quantify the contribution of match quality to city-size wage premium*

	(1)	(2)	(3)	(4)
VARIABLES	Ln Wage	Ln Wage	Ln Wage	Ln Wage
Log population size	0.039***	0.028***	0.053***	0.043***
	(0.000)	(0.000)	(0.000)	(0.000)
Match quality		0.169***		0.163***
		(0.000)		(0.000)
Year Fe	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Control skills	Yes	Yes	No	No
Observations	6,005,629	6,005,629	6,005,629	6,005,629

Notes: This table provides results estimating equation 1, with log wage as the dependent variable adding match quality as a control comparing the coefficient for local labor market size with and without controls for match quality. The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013. Standard errors are adjusted for 659,206 clusters at the individual level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

6.2 Heterogeneity analysis

Earlier research has shown that the city-size wage premium is higher among high-skilled individuals (see for example Bacolod et al., 2009). For policy makers who may want to decrease the city-size match quality gap, understanding if the city-size match quality gap exists for everybody or also is concentrated to highly skilled individuals might be important to implement policy effectively. Table 5 therefore explores whether the city-size match quality gap is also higher among high-skilled individuals. Heterogeneity analysis is presented by the level of cognitive skills, non-cognitive skills and education.

There is a positive association between match quality and living in a larger labor market across the skill distribution. However, the correlation between labor market size and match quality is especially large for highly skilled individuals, a pattern that holds when measuring skills in all dimensions: cognitive skills, non-cognitive skills, and education. Table 6 quantifies how much the city-size wage premium can be explained by match quality separately for all skill groups. Match quality is one important explanation behind the city-size wage premiums for all groups and explains between 24% to 32% of the city-size wage premium. Match quality explains a slightly higher part of the wage premium for high-skilled workers, indicating that one potential reason for higher wage premiums for high-skilled individuals might be that high-skill individuals benefit more in terms of match quality from living in a large compared to a small local labor market. Nevertheless, it should be noted that both individuals with higher and lower skill levels seem to benefit in terms of higher match quality from living in larger labor markets.

Table 5. *Heterogeneity analysis*

	Dependent variable: Match quality					
	(1) Cognitive ability under median	(2) Cognitive ability over median	(3) Non-cognitive ability under median	(4) Non-cognitive ability over median	(5) High school or less	(6) University
Log population size	0.041***	0.099***	0.046***	0.094***	0.042***	0.085***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Year Fe	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Control skills	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,004,065	3,001,564	3,068,475	2,936,693	3,409,100	2,594,733

Notes: This table provides result estimating equation 1, estimating the difference in match quality for individuals who live in labor markets of different sizes, separately for different skill groups. The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013. Standard errors are clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1

Table 6. *Contribution to city-size wage premium by skill group*

	Dependent variable: Ln Wage					
	(1) Cognitive ability under median	(2) Cognitive ability over median	(3) Non-cognitive ability under median	(4) Non-cognitive ability over median	(5) High school or less	(6) University
Panel A.						
Log population size	0.026***	0.053***	0.028***	0.050***	0.025***	0.053***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Panel B.						
Log population size	0.019***	0.036***	0.021***	0.035***	0.019***	0.038***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Match quality	0.168***	0.168***	0.169***	0.167***	0.148***	0.171***
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Year Fe	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Control skills	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,004,065	3,001,564	3,068,475	2,936,693	3,409,100	2,594,733
Panel C. Share of city size wage premium explained by match quality						
Share explained	27%	32%	25%	30%	24%	28%

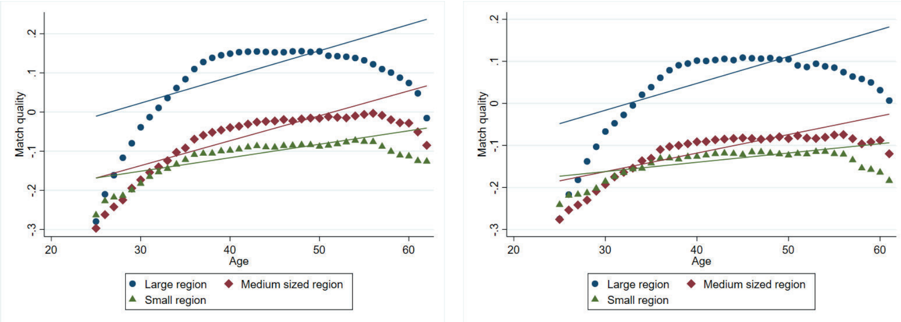
Notes: This table provides results estimating equation 1, separately by skill group, with log wage as the dependent variable adding match quality as a control comparing the coefficient for local labor market size with and without controls for match quality. The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013. Panel A shows results for the city-size wage premium without controls for match quality, Panel B shows results for the city-size wage premium with controls for match quality and Panel C shows how much match quality contributes to the city-size wage premium. Standard errors are clustered at the individual level.*** p<0.01, ** p<0.05, * p<0.1

6.3 Life-cycle pattern

The previous section showed that individuals living in large labor markets have higher match quality than individuals in smaller labor markets, and that occupational match quality explains a substantial part of the wage difference between labor markets of different sizes. This section explores the life-cycle dynamics of match quality depending on labor market size, by showing new evidence on how match quality evolves over the life cycles depending on labor market size.

Figure 2 plots match quality against age separately depending on labor market size, where the categorical definition of labor market sizes is used and la-

Figure 2. Match quality over the life-cycle



(a) Match quality *(b)* Match quality stayers

Notes: The figures plot match quality against age separately for individuals living in small, medium, and large labor markets. The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013. Figure (a) includes all individuals and figure (b) only includes individuals who always stay in the local labor market they were born in.

bor markets are divided into small, medium and large labor markets. Figure 2a plots this for all individuals after where they live at that age and figure 2b plots this for individuals who always stay in the local labor market they were born in. At age 25, match quality is similar in local labor markets of different sizes. Match quality then increases over age in all labor markets. However, the increase in match quality over the life cycle is, on average, larger in large compared to small labor markets. The increase in match quality is in line with the idea that individuals have imperfect information about their optimal match but learn about their match quality over time as they work (see Guvenen et al., 2020; Fredriksson et al., 2018).

However, the increase could also be in line with an occupation diversity explanation. If entry-level jobs are available in most cities but some high-skilled jobs are only available in larger cities, this could also explain the pattern observed in the data. At the beginning of the lice-cycle match quality is similar between different labor markets, and when individuals in larger labor markets continue to better matches in occupations that only exist in the larger labor markets this would result in a larger increase in match quality in larger labor markets.

Since Figure 2a shows match quality after where the individual lives at that age, the pattern could also be affected by a changed composition of individuals in large and small labor markets over the life cycle, where individuals move to local labor markets of different sizes over the life cycle. However, the pattern looks similar in figure 2b, which only includes individuals who always remain in the local labor market they were born in, indicating that the life-cycle dynamics in match quality are not driven by the changed composition of individuals in different labor markets.

Since the mean education level is higher in larger labor markets (see Table 1), individuals in larger labor markets might enter the labor market later, which could give different life-cycle patterns for individuals in large and small labor markets. To take this into account, Figure A5 in Appendix A.3 shows the results using potential experience instead of age.[7] The results look similar when using potential experience, indicating that the increase in match quality over the life cycle in large labor markets compared to small labor markets is not driven by the fact that individuals in large and small labor markets enter the labor markets at different ages.

To examine the dynamics behind the life-cycle pattern, I provide evidence for how the likelihood of changing occupation as well as moving up in the occupation match-quality distribution is related to labor market size. This is done by running equation (1) with occupation change and higher occupation match quality as the dependent variable. Changing occupation is defined as working in another occupation than the individual was observed in last. Results are presented for all occupation changes, and separately for changes within and between firms. An occupation change within the firm is defined as a change of occupation where the firm remains the same and an occupation change between firms is defined as a change of both occupation and firm. Higher occupation match quality is constructed as a dummy variable equal to one if the individual changes to an occupation with higher match quality than before and zero if the individual changes to an occupation with lower match quality than before. When estimating the result for higher occupation match quality, the sample is limited to individuals who change occupation.

Table 7 shows that individuals in larger labor markets are more likely to change occupations. The result of more occupation switches is in line with the theory of lower costs of changing occupation in larger labor markets (see Puga, 2010; Bleakley and Lin, 2012). The increased likelihood of changing occupation in large labor markets is equally driven by within and between firm changes. Thus, the lower cost of changing occupations in larger labor markets could both come from more job openings, making it easier to change firms and more career opportunities within the firm and thus a lower cost of changing occupations without having to change employers. For individuals in medium-sized labor markets, the higher likelihood of changing occupation compared to small labor markets is entirely driven by within-firm changes.

Furthermore, Table 7 shows that individuals in larger labor markets are more likely to move up in the occupation match quality distribution when they change occupation. For individuals in large compared to small labor markets, the move up the occupation match-quality distribution is mainly driven by between-firm occupation changes. However, for individuals in medium-sized local labor markets, the pattern looks different. The increase in match quality

[7] Potential experience is defined as age minus (years of education+ six), where six is the age the individual usually starts school.

Table 7. *Match quality dynamics*

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Occupation switch			Switch to higher match quality		
	All	Within firm	Between firm	All	Within firm	Between firm
Large labor markets	0.018***	0.009***	0.009***	0.017***	0.002	0.014***
	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Medium labor markets	0.002***	0.003***	-0.000	0.008***	0.009***	-0.001
	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Year Fe	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Control skills	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,346,423	5,346,423	5,346,423	790,525	790,525	790,525

Notes: This table provides results estimating regressions with changing occupation and good switch as the dependent variables, and dummy variables for living in a large and medium-sized labor market compared to the omitted category living in a small labor market as the independent variable of interest. The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013. Changing occupation is defined as working in another occupation than the individual did in the last observation. Switch to higher match quality is defined as a dummy variable equal to one for individuals who change occupation to an occupation with higher match quality and zero for individuals who change occupation to an occupation with worse match quality. When estimating the regression for the outcome switching to a higher match quality occupation, the sample is limited to individuals who switch occupations. Standard errors are clustered at the individual level.*** p<0.01, ** p<0.05, * p<0.1

for medium-sized compared to small local labor markets is entirely driven by within-firm changes.

Thus, the analysis shows that the improvement of match quality over the life cycle in larger compared to smaller labor markets comes from both more occupation switches and better occupation switches conditional on switching occupations.

7 Mechanisms

Section 6 showed that match quality is higher in larger labor markets. This section examines two different theoretical explanations for higher match quality in larger labor markets: more occupation diversity and more learning possibilities about match quality in larger labor markets.

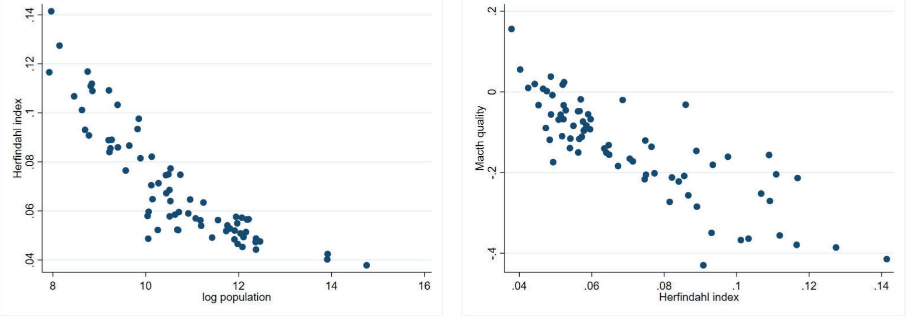
7.1 Occupation diversity

One theoretical mechanism behind the difference in match quality between large and small local labor markets is occupation diversity. If there are mobileity costs, and the occupation where the individual would have the best match quality is not available in the labor market, the individual might not be able to reach the best occupation match. Thus, a more diversified occupation structure gives individuals more occupations to choose from and increases the likelihood that individuals find an occupation that matches their skill set.

To study if occupation diversity is a mechanism behind higher match quality in larger labor markets, I construct a Herfindahl-Hirschman Index (HHI). The HHI index is a measure widely used to measure employment concentration (see for example Thoresson, 2024; Benmelech et al., 2022). Here I construct a HHI measure of occupation concentration in the labor market, defined as: $HHI_L = \sum_{o=1}^{o} (s_o)^2$, where L denotes the labor market, s_o is the employment share in the labor market of occupation o. Thus, the HHI index is calculated by squaring the employment share of each occupation in the labor market and summing the number over all occupations. The HHI index can take values between 0 and 1, where 1 means there is only one occupation in the labor market and zero means less employment concentration and, thus a more diversified occupation structure.

Furthermore, to study if the occupations that don't exist are relevant, I study if the occupation where the individual has the highest match quality exists in the labor market the individual lives in. Where the occupation is defined as existing if anybody in the occupation sample, in the relevant labor market, work in the occupation.

Figure 3. Occupation diversity



(a) Labor market size

(b) Match quality

Notes: The HHI index is a measure of occupation concentration in the labor market, defined as: $HHI_L = \sum_{o=1}^{o} (s_o)^2$, where L denote the labor market, s_o is the employment share in the labor market of occupation o. The HHI index can take values between 0 and 1, where 1 means it is only one occupation in the labor market and zero means less employment concentration and thus a more diversified occupation structure. Figure (a) plots the HHI index against labor market size and figure (b) plots the HHI index against match quality.

Figure 3a plots occupation diversity, defined with the HHI index, against labor market size. The figure shows a negative relationship between labor market size and the HHI index. Thus, larger labor markets seem to have less occupation concentration and more occupation diversity. The conclusions of higher occupation diversity in larger labor markets remain similar if occupation diversity is instead defined as the share of all occupations that exist in the local labor market following Papageorgiou (2022) (see Table A6b and A7b in Appendix A.5). The finding of more occupation diversity in larger labor markets is in line with the result of more occupation diversity in larger labor markets in the United States (see Papageorgiou, 2022) and more industry diversity in larger labor markets in Sweden (see Korpi, 2007).

Figure 3b shows that there is a positive association between occupation diversity and match quality. Thus, the descriptive pattern supports the theory of occupation diversity as one mechanism for why match quality is higher in larger local labor markets.

To study how much occupation diversity contributes to the higher match quality in larger labor markets, I run equation (1) and include a control for the HHI index. Column 1 in Table 8 shows the main result for how match quality is related to labor market size, and column 2 shows the same result but controls for the HHI index. From the table, it is clear that the HHI index is negatively correlated to match quality (and thus, occupation diversity is positively correlated with match quality). The coefficient on labor market size becomes smaller when including controls for occupation diversity, indicating that occupation diversity might explain part of the city-size match quality gap. The coefficient on labor market size is reduced by approximately 23 percent when controls for occupation diversity are included. Column 3 shows that the occupation in which the individual would have the highest match quality is more likely to exist in larger compared to smaller labor markets. Thus, the results in this section suggest that one reason for higher match quality in larger labor markets is more occupation diversity in larger labor markets.

Table 8. *Occupation diversity*

VARIABLES	(1) Match quality	(2) Match quality	(3) Best match exist in labor market
log population size	0.070***	0.054***	0.003***
	(0.001)	(0.002)	(0.000)
Herfindahl-Hirschman Index		-2.370***	
		(0.209)	
Year Fe	Yes	Yes	Yes
Age FE	Yes	Yes	Yes
Control skills	Yes	Yes	Yes
Observations	6,005,629	6,005,629	6,005,629

Notes: This table provides result estimating equation 1, estimating the difference in match quality for individuals who live in labor market of different sizes, column 2 includes controls for the HHI. The HHI is a measure of occupation concentration in the labor market, defined as: $HHI_L = \sum_{o=1}^{o}(s_o)^2$, where L denote the labor market, s_o is the employment share in the labor market of occupation o. The HHI can take values between 0 and 1, where 1 means it is only one occupation in the labor market and zero means less employment concentration and thus a more diversified occupation structure. Standard errors are clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1.

7.2 Match quality and learning

Learning and match quality have been discussed as different mechanisms behind the city-size wage premium (Puga, 2010), and research has shown that more learning possibilities in larger cities are one important explanation for the city-size wage premium (De La Roca and Puga, 2016; D'Costa and Overman, 2014). However, learning and match quality are not necessarily disconnected explanations. Since individuals seem to be unsure about their match quality from the beginning, one possible explanation for higher match quality in larger labor markets is more learning opportunities about which occupations are a good match in larger labor markets. As noted by Wheeler (2008) a lower cost of switching jobs might allow the worker to explore more different jobs, making it easier to find the job where they are the most productive.

To estimate if learning is one channel for higher match quality in larger labor markets, I follow De La Roca and Puga (2016) and estimate the return to experience in labor markets of different sizes but with match quality instead of wages as the dependent variable:

$$MQ_{it} = \alpha_L + \lambda_i + \sum_{k=1}^{j=3} \mu_{kL}\theta_{ikt} + \rho_{iLt} \quad (2)$$

In equation (2) α_L are separate dummy variables for living in a large or medium labor market, with the omitted category living in a small labor market, λ_i is individual fixed effects, θ_{ikt} is years of experience working in a labor market of size k for worker i and time period t. Years of experience are calculated as years of working in a labor market of size L since age 20.[8] The value of experience is allowed to vary depending on both in which labor market the experience was obtained (k) and where the individual lives now (L). The specification also includes second-order polynomial terms of experience. Since experience in labor markets of different sizes now are included, α_L captures the static match quality premium of moving to labor market size L.

The idea behind the exercise is that if learning is one important mechanism behind higher match quality in larger labor markets, not only the static premium should matter. Instead, match quality should increase gradually and years of experience in large labor markets should be important. Moreover, if learning is the primary mechanism driving the difference in match quality, the knowledge of what is a good match should also be portable. Thus, if learning is important, we could expect years of experience in a large city to also be beneficial for individuals who now live in a small city.

The learning mechanism about match quality could either be driven by labor market explanations or more education possibilities in larger cities. Thus, it is also possible that the learning possibilities in larger cities are connected to learning through a more direct education channel. By moving to a large city, the worker can get access to a university education, which could increase the knowledge of the individual and give access to more occupations. To examine if the learning channel about match quality seems to be driven by education possibilities or labor market factors, I estimate regression (2) separately for individuals with and without a university education. The idea is that for individuals without a university education, the remaining mechanisms are related to labor market factors. As stated above, labor market factors driving the effect could be initial uncertainty about match quality and a lower cost of switching occupations in larger labor markets, allowing workers to explore more occupations and learn more about what occupation is a good match. However, another possible labor market driver of the effect could be more learning possibilities on the job in larger cities (see Glaeser and Maré, 2001; De La Roca and Puga, 2016). According to this channel, experience in large cities gives valuable knowledge that might open up possibilities to enter other occupations.

Table 9, column 1 shows the results for all workers, column 2 shows the results for workers with high school or less, and column 3 shows the results for workers with university education. The regression includes experience in

[8] To be able to calculate the experience of the worker, I use data for where the worker lives and if they have earnings from 1990. To be able to observe the worker from the age 20 the sample is limited to workers born 1970-1976. Since occupation data is available from 1996-2013, years from 1990 are used to calculate experience, but only years after 1996 are used to calculate the outcomes. One year of experience in labor marker L is defined as a year of living in labor market L and having positive earnings. Table A4 in Appendix A.4 presents results when experience is instead defined as years of living in a labor market since age 20. The results remain similar whether years of experience are defined conditional on working or not.

large and medium-sized labor markets as well as overall experience. Thus, the term experience now captures having experience in a small labor market and experience in large and medium captures the extra benefit of having experience in a large and medium relative to a small labor market. When studying the result for all workers, it is clear that the static premium becomes insignificant. Experience in large and medium relative to a small labor market is positive and significant, indicating that learning is one important mechanism behind the higher match quality in large labor markets. The interaction term of having experience in a large labor market and living in a large labor market is negative, indicating that the experience from a large labor market is actually more valuable for individuals now living in a small labor market. Thus, the knowledge the individual has obtained about what occupations are a good match seems to be portable.

When studying the result separately for individuals with high school education and individuals with university education, it is clear that for both groups experience in large and medium is more valuable for match quality than experience in small labor markets. The point estimates are larger for those with a university education, which could indicate that part of the learning effect found in column 1 can be learning through university education. However, experience in large labor markets is also more valuable than experience in small labor markets for individuals with a high school education, indicating that parts of the learning effect also come through the labor market. Thus, the result suggests that one explanation for higher match quality in larger labor markets is less frictions and more learning possibilities of what occupations to work in to have high match quality in larger labor markets.

Table 9. *Learning*

	Dependent variable: Match quality		
	(1)	(2)	(3)
VARIABLES	All	High school or less	University
Large labor market	-0.001	0.049	-0.074**
	(0.026)	(0.037)	(0.035)
Medium labor market	-0.032	-0.001	-0.108***
	(0.023)	(0.032)	(0.033)
Experience large	0.071***	0.038***	0.060***
	(0.006)	(0.010)	(0.008)
(Experience *large*)^2	-0.002***	-0.001*	-0.002***
	(0.000)	(0.001)	(0.001)
Experience medium	0.038***	0.020***	0.029***
	(0.005)	(0.007)	(0.008)
(Experience *medium*)^2	-0.002***	-0.001**	-0.001**
	(0.000)	(0.000)	(0.001)
Experience	0.022***	0.010***	0.049***
	(0.002)	(0.002)	(0.004)
Experience^2	-0.000**	0.000	-0.001***
	(0.000)	(0.000)	(0.000)
Large labor market#experience large	-0.041***	-0.014	-0.048***
	(0.006)	(0.009)	(0.008)
Large labor market#(experience *large*)^2	0.002***	0.001	0.002***
	(0.000)	(0.001)	(0.001)
Medium labor market#experience large	-0.000	0.013	-0.011
	(0.006)	(0.010)	(0.008)
Medium labor market#(experience *large*)^2	-0.001	-0.001	0.000
	(0.000)	(0.001)	(0.001)
Large labor market#experience medium	-0.000	-0.001	-0.004
	(0.006)	(0.008)	(0.008)
Large labor market#(experience *medium*)^2	-0.000	0.000	-0.000
	(0.000)	(0.001)	(0.001)
Medium labor market#experience medium	-0.029***	-0.017**	-0.038***
	(0.005)	(0.007)	(0.007)
Medium labor market#(experience *medium*)^2	0.001***	0.001*	0.002***
	(0.000)	(0.000)	(0.001)
Large labor market#experience	0.017***	0.004	0.033***
	(0.004)	(0.005)	(0.006)
Large labor market#*experience*^{2}	-0.001***	-0.000**	-0.001***
	(0.000)	(0.000)	(0.000)
Medium labor market#experience	0.006*	0.003	0.026***
	(0.003)	(0.005)	(0.005)
Medium labor market#*expeirence*^{2}	0.000	-0.000	-0.001***
	(0.000)	(0.000)	(0.000)
Individual FE	Yes	Yes	Yes
Observations	1,351,753	679,803	671,950

Notes: This table provides result estimating equation 2. The sample includes males born between 1970 and 1976, with outcomes for the years 1996-2013. Standard errors are clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1

8 Robustness

Earlier sections have shown that match quality is higher in larger labor markets. This section examines the robustness of the results. First, results are presented for individuals who move between labor markets of different sizes, making it possible to control for individual fixed effects. It should be noted that even if the movers' design allows for control for individual characteristics that are constant over time, the decision to move is a choice, and individuals who move have overcome the mobility frictions and might move optimally. Thus, the result from this group might differ from the general population. Nevertheless, studying movers is a good complement to the main specification. Finally, the section also presents alternative specifications of the match quality measure.

8.1 Comparing movers and stayers

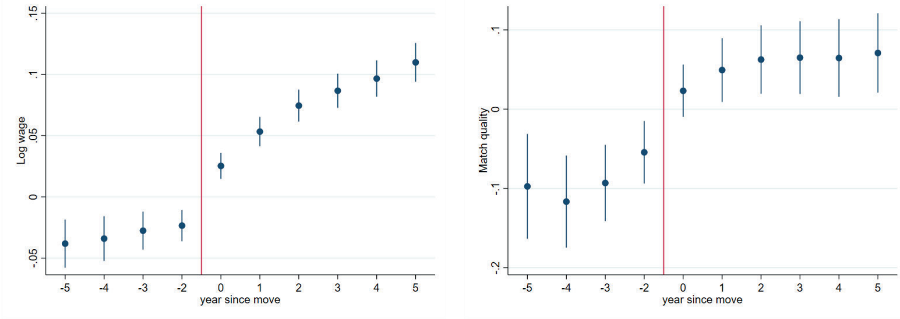
This section presents results comparing individuals who move from the small local labor market they were born in, to a large labor market compared to individuals who always stay in the small labor, by estimating:

$$y_{it} = \alpha_i + \lambda_t + \sum_l \mu_l D^l_{it} + X_{it} + \rho_{it} \quad (3)$$

where α_i is individual fixed effects, λ_t is year-fixed effects, and X_{it} is age dummies, and D^l_{it} is time since the move. Including both year and age-fixed effects together with the individual fixed effect would give perfect multicollinearity, so to overcome this problem the age dummies are normalized to be constant between 45 and 54. To avoid the problem with two-way fixed effects models and heterogeneous treatment effects, I use the procedure from Sun and Abraham (2021), and use never treated as the control, and estimate a weighted average of the cohort-time-specific treatment effects, where weights are set to the estimated cohort shares. The result is estimated for the first move the individual makes. First moves are defined as the first move in the data, observed between the years 1996 and 2013 and that the move is from the local labor market the individual is born in. The control group is individuals who always stay in the small local labor market they were born in.

Figure 4 shows the results for moving to a large labor market where year zero is the year of the move. Figure 4a shows the results for wages, and Figure 4b shows the results for match quality. From Figure 4 it is clear that both wages and match quality on average increase after an individual moves to a large local labor market. However, the figure also shows a pre-trend before moving, indicating that individuals who are on a positive trend in match quality are more likely to move to a large local labor market. Thus, it seems like match quality increases for individuals who choose to move from a small to a large labor market. However, if individuals dislike moving, they may only choose to move if match quality increases. Thus, if individuals only move

Figure 4. Move large from small



(a) Wage

(b) Match quality

Notes: The figures plot event graphs for individuals who move from a small labor market to a large labor market estimated with Sun and Abraham (2020) estimator, using never mover who always stays in the small local labor market they where born in as the control. The x-axis plot normalized time since the move, where period zero is the time of the move. Figure (a) plots the result for log wages and figure (b) plots the result for match quality. The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013.

to increase match quality the higher match quality might come from moving itself, and not from the larger labor market. To take this into account, Section 8.2 instead compares movers to labor markets of different sizes.

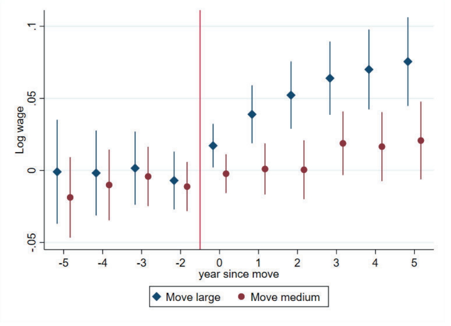
8.2 Comparing movers

Moving could in itself have a direct effect on match quality, and individuals may only choose to move if they currently have a bad match. Odio Zúñiga and Yuen (2020) have for example shown that individuals who have bad match quality are more likely to move. To take away the direct effect of the move, this section therefore estimates results using only movers and comparing individuals who move to local labor markets of different sizes. This is done by estimating the regression:

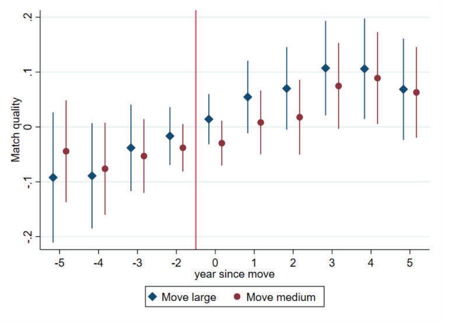
$$y_{it} = \sum_{l=j}^{l=k} \beta_l N_l + \beta_T treated_i * \sum_{l=j}^{l=k} N_l + treated_i + \lambda_t + X_{it} + e_{it} \quad (3)$$

where N is normalized time since the move, and treated are separate dummy variables for moving to a large or medium labor market with the omitted category being moving to a small local labor market. X_{it} includes age controls, and second-order polynomials in each of the eight skills, controls for the local labor market the individual is born in, and a fixed effect for the occupation the individual works in the year before the move. The parameter of interest β_T gives the difference between moving to a large and medium labor market compared to moving to a small local labor market.

Figure 5. Movers



(a) Wage



(b) Match quality

Notes: The figure plots the difference between moving to a large or medium-sized labor market compared to the omitted category moving to a small labor market. The x-axis plot normalized time since the move, where period zero is the time of the move. Figure (a) plots the result for log wages and figure (b) plots the result for match quality. The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013.

Figure 5 shows what happens to wages and match quality for individuals who move to a medium-sized and large labor market compared to the omitted category moving to a small labor market. Figure 5a shows that after the move, wages increase over time for individuals who move to a large compared to a small labor market. The estimates also suggest that over time, wages increase somewhat for individuals who move to a medium compared to a small labor market, even though the estimates are not statistically different from zero. Figure 5b shows a similar pattern for match quality, where match quality increases for individuals who move to large compared to small labor markets. The point estimates suggest that match quality increases somewhat in the first two years after the move and then is relatively constant over time. Overall, the results from the movers support the earlier result of higher match quality in large compared to small labor markets.

8.3 Robustness of the match quality measure

This section provides robustness tests to the match quality measure by presenting results with alternative match quality constructions.

One potential worry with the match quality measure is that the return in small occupations might be estimated with more uncertainty. Figure A3 in Appendix A.1 plots match quality against occupation size, and the figure indicates that there might be a relationship between match quality and occupation size. Thus, to study the robustness of the results Table A3 presents results following Guvenen et al. (2020), using the external O*Net data to proxy for skill requirements in each occupation. [9] Reassuringly the conclusion of higher match quality in larger labor markets remains when using O*Net data to measure skill requirements in each occupation.

Column 2 estimates the return to skills linearly instead of using a random forest, again the conclusions remain with this specification. One other potential concern with the construction of the match quality measure is that since more individuals live in large labor markets, the training data have more individuals from large labor markets. If returns to skills in occupations are different in large and small labor markets, the estimated returns might be more similar to the returns in large labor markets. To study if this drives the results, Table 10 presents results when the returns to skills in occupations are allowed to vary with labor market size by estimating separate random forest regression for small, medium, and large labor markets. Also, with this specification, the conclusions of higher match quality in larger labor markets remain.

Another potential concern with the match quality measure is that some occupations might be over-represented in large or small labor markets and that the return might be higher in large cities for other urban wage reasons, such as a lower cost of living in smaller labor markets. To take this into account column 5 presents result when only training the random forest in large labor markets, reassuringly the results remain very similar indicating that the result of higher match quality in larger labor markets are not driven by different return in labor market of different sizes.

Finally, one potential worry with the match quality measure is that individuals might be well-matched even if they are not correctly priced for their skills. To take this into account column 3 uses the random forest to predict the probability that a worker works in an occupation instead of estimating return to skills, again the conclusions remain similar. Thus, from Table 10 it is clear the conclusion of higher match quality in larger compared to smaller labor markets also holds with the alternative definitions of match quality.

[9] To construct the match quality measure I use five different occupation skill requirements from O*Net chosen to match the skills for the individuals from the military enlistment test; inductive skills, verbal skills, spatial skills, technical skills, and social skills. The mismatch measure is constructed as the sum of the absolute difference between the rank of the standardized skill from the military enlistment test and the rank of the occupation requirement for that skill for each of the five skills. Match quality is then defined as the negative of the mismatch measure.

Table 10. *Alternative match quality measures*

VARIABLES	(1) O*Net	(2) Linear return	(3) Random forest probability	(4) Return vary with labor market size	(5) Random forest trained on large labor market
Log population size	0.005***	0.015***	0.019***	0.168***	0.075***
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Year Fe	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
Control skills	Yes	Yes	Yes	Yes	Yes
Observations	5,820,642	6,005,629	6,005,629	6,005,629	6,005,629

Notes: This table provides results estimating equation 1, estimating the difference in match quality for individuals who live in labor markets of different sizes, using alternative measures of the match quality measure. The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013. Standard errors are clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1

9 Conclusion

While earlier research found that wages are higher in larger than smaller labor markets, the mechanism behind this has been less studied. One theoretical mechanism for the city-size wage premium is that workers in larger cities are better matched to their occupations. However, even though this mechanism has been discussed theoretically, the empirical evidence for how match quality differs in small and large labor markets has been limited. Building on the recent literature on multidimensional skill mismatch, this paper contributes to the literature by providing direct evidence on how occupational skill match quality differs depending on the labor market size in Sweden.

The key result from the paper is that match quality indeed is higher in larger compared to smaller labor markets. This finding holds conditional on skills, when including individual fixed effects for individuals who move from a small to a large labor market, and when using movers comparing individuals who move to a large compared to a small labor market. In the preferred specification, match quality explains around 30 percent of the city-size wage gap. The difference in match quality between individuals in large and small labor markets is especially large for individuals with high skills, measured as cognitive skills, non-cognitive ability, or education, in line with the fact that the city-size wage premium is higher for highly skilled individuals (see Bacolod et al., 2009). The large differences in match quality are consistent with high mobility frictions, in line with research showing that people are unlikely to move in response to economic shocks (see Yagan, 2019).

I examine two different mechanisms behind the higher match quality in larger labor markets: a more diversified occupation structure in larger labor markets, increasing the likelihood that individuals find an occupation that matches their skills and more learning possibilities about match quality in larger cities. When exploring the empirical support for these mechanisms, suggestive evidence emerges that higher match quality in larger labor markets comes from both a more diversified occupation structure and higher learning possibilities.

It should be noted that the finding that match quality is higher in larger labor markets does not necessarily mean individuals would be better off by moving to large labor markets since the utility of the individual is also affected by the cost of living and their preferences about where to live. Thus, while the result here highlights the limited ability to find a good match in smaller labor markets, research is needed on how to best solve this problem. Possible solutions could be increasing mobility or increasing the size of small labor markets. Policies for increasing the size of the local labor market could be lowering the commuting cost by investing in infrastructure or increasing the possibility of working from home.

Moreover, since this study has shown that match quality is an important factor behind the city-size wage premium, policies directly focused on increasing match quality in small labor markets could be useful in decreasing the city-size wage gap. Such policies could be increasing occupation diversity in small cities, for example by moving government agencies to smaller cities or increasing incentives for entrepreneurship in different occupations. Another policy to increase match quality in smaller labor markets is to decrease the cost of exploring different occupations in small labor markets by lowering search costs. All of these possible solutions probably have different pros and cons, and more research is needed to decide on the best solutions.

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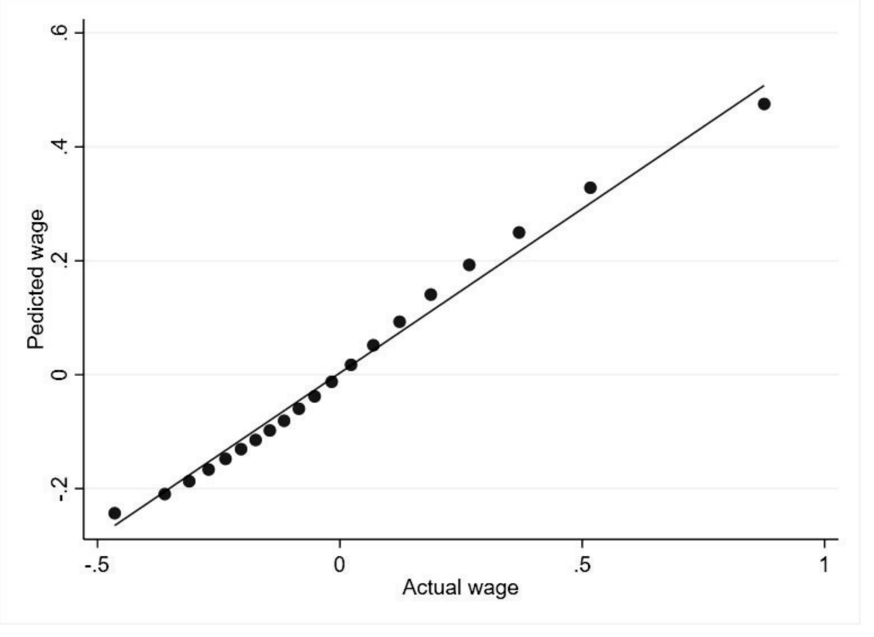
Appendix A: Additional figures and tables

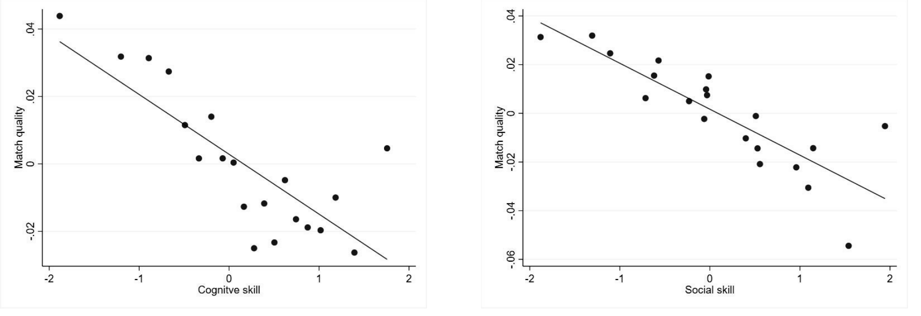
A.1 Match quality measure

Figure A1. Predicted wage with random forest vs actual wage



Notes: The figure plot predicted wages based on skills estimated with the random forest against actual wages. Wages are residualized on age and year-fixed effects. The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013. The individuals used for training the random forest are excluded from the sample.

Figure A2. Match quality and skills

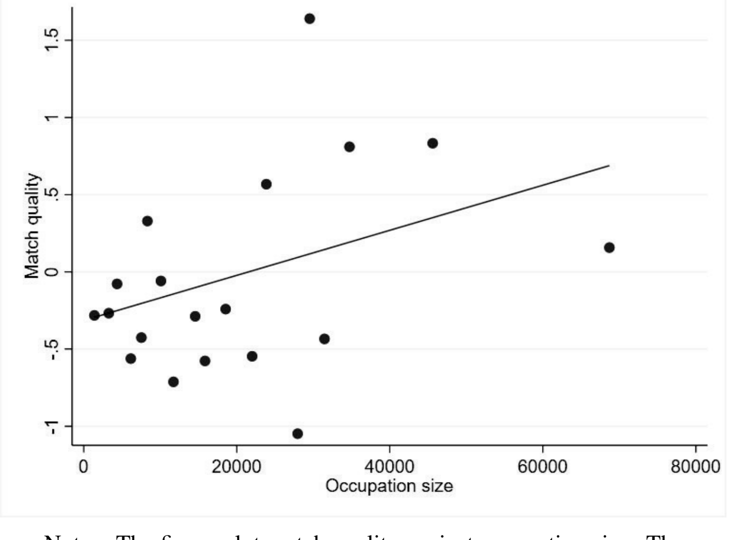


(a) Cognitive skills

(b) Social skills

Notes: The figure plot match quality against skills, figure a shows the result for cognitive skills and figure b shows the result for social skills. The match quality measure is standardized to have mean zero and standard deviation one. The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013.

Figure A3. Match quality and occupation size



Notes: The figure plot match quality against occupation size. The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013.

Table A1. *Different match quality measures*

	Dependent variable: Ln Wage					
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES		O*Net	Linear return	Random forest proba-bility	Return vary with labor market	Random forest trained on large labor market
Log population size	0.039***	0.039***	0.036***	0.039***	0.010***	0.027***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Match quality		0.010***	0.207***	0.028***	0.176***	0.167***
		(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Observations	6,005,629	5,820,642	6,005,629	6,005,629	6,005,629	6,005,629
Year Fe	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Control skills	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table provides results estimating equation 1, with log wage as the dependent variable adding match quality as a control comparing the coefficient for local labor market size with and without controls for match quality. Column 1 shows result without controls for match quality, and column 2-6 adds different match quality measures as controls. The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013. Standard errors are adjusted for clusters at the individual level. *** p<0.01, ** p<0.05, * p<0.1

A.2 Labor market definitions

Table A2. *Labor market size as categorical variables: Match quality*

VARIABLES	(1) Match quality	(2) Match quality	(3) Match quality	(4) Match quality
Large labor markets	0.208***	0.226***	0.184***	0.014**
	(0.004)	(0.004)	(0.004)	(0.006)
Medium labor markets	0.048***	0.056***	0.036***	-0.019***
	(0.004)	(0.004)	(0.003)	(0.006)
Year Fe	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Control skills	No	Yes	Yes	Yes
Control education	No	No	Yes	No
Individual FE		No	No	Yes
Observations	6,005,629	6,005,629	6,002,946	6,005,629

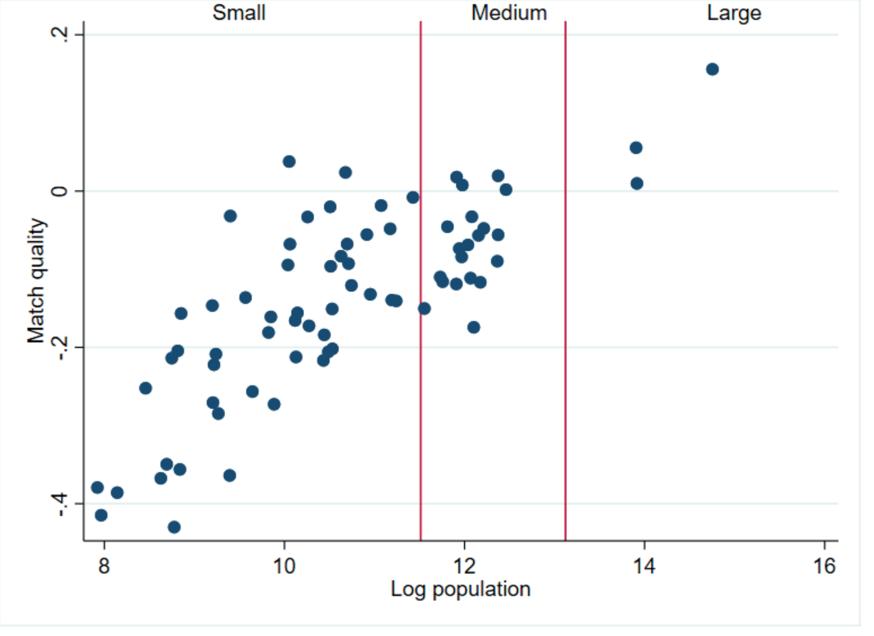
Notes: This table provides result estimating equation 1, estimating the difference in match quality for individuals who live in a large and medium-sized labor market compared to the omitted category living in a small labor market. The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013. In column 4, where individual fixed effects are included, the age dummies are normalized to be constant between 45 and 54 to avoid multicollinearity between age and year-fixed effects. Standard errors are adjusted for 659,206 clusters at the individual level. *** p<0.01, ** p<0.05, * p<0.1

Table A3. *Labor market size as categorical variables: Wage*

VARIABLES	(1) Ln Wage	(2) Ln Wage	(3) Ln Wage	(4) Ln Wage
Large labor markets	0.174***	0.123***	0.110***	0.045***
	(0.001)	(0.001)	(0.001)	(0.002)
Medium labor markets	0.040***	0.019***	0.013***	-0.001
	(0.001)	(0.001)	(0.001)	(0.002)
Year Fe	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Control skills	No	Yes	Yes	Yes
Education control	No	No	Yes	No
Individual FE	No	No	No	Yes
Observations	6,005,629	6,005,629	6,002,946	6,005,629

Notes: This table provides results estimating equation 1, with log wage as the dependent variable, estimating the difference in log wages for individuals who live in a large and medium-sized labor market compared to the omitted category living in a small labor market. The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013. In column 4, where individual fixed effects are included, the age dummies are normalized to be constant between 45 and 54 to avoid multicollinearity between age and year-fixed effects. Standard errors are adjusted for 659,206 clusters at the individual level. *** p<0.01, ** p<0.05, * p<0.1

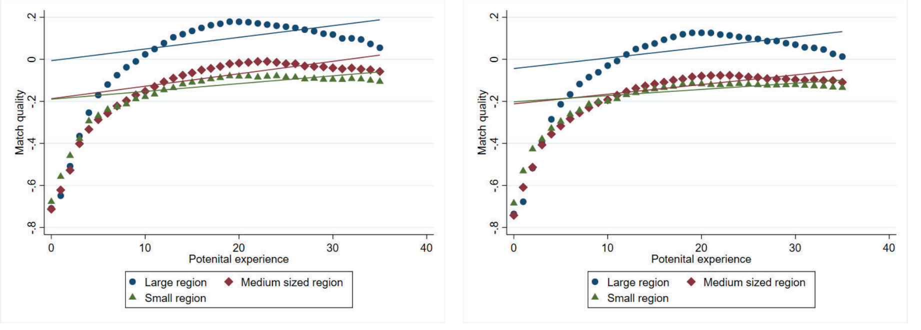
Figure A4. Match quality and labor market size



Notes: The figure shows match quality against population size in the labor market. The vertical line shows the limit for small, medium, and large labor markets. The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013

A.3 Life cycle pattern

Figure A5. Match quality over the life-cycle



(a) All individuals (b) Stayers

Notes: The figures plot match quality against potential experience separately for individuals living in small, medium, and large labor markets. Potential experience is constructed as age-(years of education + 6). The sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013. Figure (a) includes all individuals and figure (b) only includes individuals who always stay in the local labor market they were born in.

A.4 Learning

Table A4 shows estimates of regression 2, where experience is defined as years of living in a labor market since age 20.

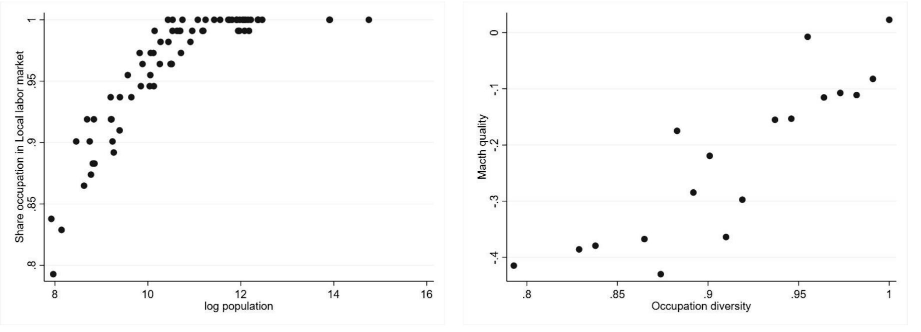
Table A4. *Learning*

	Dependent variable: Match quality		
VARIABLES	(1) All	(2) High school or less	(3) University
Large regions	-0.010	0.056	-0.107***
	(0.028)	(0.040)	(0.038)
Medium-sized regions	-0.026	0.010	-0.113***
	(0.025)	(0.034)	(0.036)
Experience large	0.068***	0.039***	0.053***
	(0.006)	(0.009)	(0.008)
(Experience *large*)^2	-0.002***	-0.001*	-0.001**
	(0.000)	(0.001)	(0.001)
Experience medium	0.035***	0.022***	0.022***
	(0.005)	(0.007)	(0.007)
(Experience *medium*)^2	-0.001***	-0.001**	-0.001*
	(0.000)	(0.000)	(0.000
Experience	0.028***	0.015***	0.064***
	(0.002)	(0.002)	(0.004)
Experience^2	-0.000***	-0.000	-0.001***
	(0.000)	(0.000)	(0.000)
Large region#experience large	-0.037***	-0.013	-0.042***
	(0.006)	(0.009)	(0.008)
Large region#(experience *large*)^2	0.002***	0.001	0.002***
	(0.000)	(0.001)	(0.001)
Medium region#experience large	0.003	0.015	-0.006
	(0.006)	(0.009)	(0.008)
Medium region#(experience *large*)^2	-0.001	-0.001*	-0.000
	(0.000)	(0.001)	(0.001)
Large region#experience medium	0.003	-0.002	0.002
	(0.005)	(0.008)	(0.007)
Large region#(experience *medium*)^2	-0.000	0.000	-0.001
	(0.000)	(0.001)	(0.001
Medium region#experience medium	-0.025***	-0.019***	-0.029**
	(0.005)	(0.007)	(0.007)
Medium region#(experience *medium*)^2	0.001***	0.001**	0.001**
	(0.000)	(0.000)	(0.000)
Large region#experience	0.017***	0.004	0.034***
	(0.004)	(0.005)	(0.006)
Large region#*experience*^{2}	-0.001***	-0.000***	-0.001***
	(0.000)	(0.000)	(0.000)
Medium region#experience	0.005	0.003	0.022***
	(0.003)	(0.004)	(0.005
Medium region#*expeirence*^{2}	0.000	-0.000	-0.000**
	(0.000)	(0.000)	(0.000)
Individual FE	Yes	Yes	Yes
Observations	1,351,753	679,803	671,950

Notes: This table provides result estimating equation 2. The sample includes males born between 1970 and 1976, with outcomes for the years 1996-2013. Standard errors are clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1

A.5 Occupation diversity

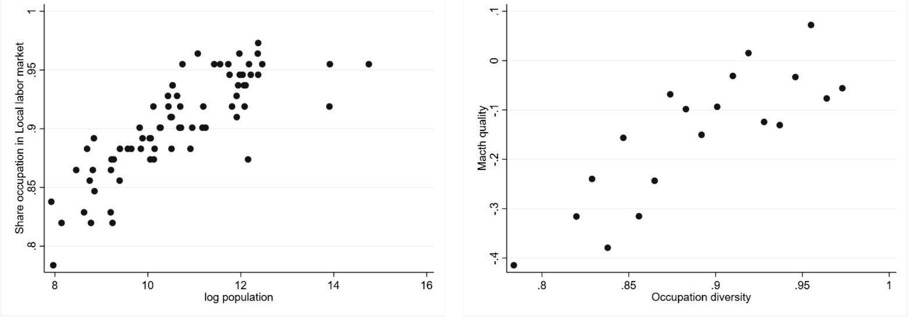
Figure A6. Occupation diversity



(a) Labor market size *(b)* Match quality

Notes: The figures plot occupation diversity, where occupation diversity is defined as the share of all possible occupations at the three-digit level that exists in the labor market. An occupation is defined as existing if somebody in the occupation sample works in the occupation. Both males and females are used to construct the occupation diversity variable. To measure the outcomes, the sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013. Figure (a) plots occupation diversity against labor market size, and figure (b) plots occupation diversity against match quality.

Figure A7. Occupation diversity



(a) Labor market size *(b)* Match quality

Notes: The figures plot occupation diversity, where occupation diversity is defined as the share of all possible occupations at the three-digit level that exists in the labor market. An occupation is defined as existing if somebody in the occupation sample works in the occupation. Both males and females are used to construct the occupation diversity variable. Occupation diversity is estimated by taking a random sample of 10,000 individuals from each labor market. To measure the outcomes, the sample includes males born between 1951 and 1976, with outcomes for the years 1996-2013. Figure (a) plots occupation diversity against labor market size, and figure (b) plots occupation diversity against match quality.

Essay 2: Labor-market Drivers of Intergenerational Earnings Persistence

Co-authored with Martin Nybom and Jan Stuhler

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Introduction

Children from high-income families earn substantially higher incomes than children from low-income families (Jäntti and Jenkins 2014; Deutscher and Mazumder, 2023). A key question in the social sciences is why this is the case. Seminal contributions in the economic literature emphasize human-capital mechanisms and differences in parental investment (Becker and Tomes, 1979; Solon, 2004), skill development across childhood (Heckman and Mosso, 2014), and the role of credit constraints in these processes (Lochner and Monge-Naranjo, 2012; Lee and Seshadri, 2019). A related strand of the literature studies the roles of nature and nurture by decomposing intergenerational transmission into pre- and post-birth factors (e.g. Björklund et al., 2006; Black et al., 2005). As such, much of the existing intergenerational mobility literature focuses on how various differences in human capital accumulation *in childhood* translate to pay differences in adulthood. In contrast, very few studies focus on the role of labor-market mechanisms in shaping intergenerational persistence in incomes. An earlier exception is Corak and Piraino (2010), who document that high-SES children are more likely to work for the same employer as their father.

In this paper, we study the contribution of firms, sorting, and career advancement on the labor market to intergenerational persistence. Employing Swedish population-wide earnings data with both employer-employee and parent-child links, we first decompose earnings into individual and firm components using the two-way fixed effects framework of Abowd et al. (1999) (similar to e.g. Dobbin and Zohar, 2023). We show that labor-market factors, broadly speaking, and socioeconomic status (SES) gradients in sorting across employers more specifically, explain 31% of overall income persistence measured at mid-age. The contribution of firms rises to 38% if we incorporate differences in returns to experience across firm types in our analysis. The importance of such labor-market mechanisms grows over age, suggesting that differences in labor-market behavior can partly explain why SES gradients in earnings increase with age. The SES gradient in firm pay (or the "firm pay gradient") can partly be attributed to confounding sorting on skills, but at least half of the gradient remains when conditioning on a rich set of skill measures. Finally, we analyze whether there is a similar gradient in terms of the overall attractiveness of employers, including both their pay and non-pay characteristics. We find little evidence that the purely monetary focus overstates intergenerational persistence in overall welfare.

We first present a set of empirical facts on the SES gradient in firm pay premia in Sweden. We here follow a similar approach as an earlier study by Dobbin and Zohar (2023), who study the role of firms in intergenerational persistence in Israel. While the individual component is generally more important in explaining earnings inequality and persistence, the "static" firm component does account for nearly one third (31%) of the intergenerational earnings elasticity (IGE), a slightly larger estimate than Engzell and Wilmers (2024), who focus on percentile ranks, find in similar data. Interestingly, the estimated contribution of firms to intergenerational persistence is substantially higher in our data as compared to Dobbin and Zohar (2023). We provide evidence suggesting that the comparatively high quality of our data, spanning over a 35 year period, allows us to estimate firm pay premia, and their contribution to intergenerational persistence, more precisely.

The sorting of workers across firms therefore explains a substantial part of the overall transmission of income advantages from one generation to the next. The selection into higher-paying industries (such as financial sector support services, telecommunications manufacturing, or IT services) explains some of the firm pay gap, while differences in firm pay by region or firm size are less important. Jointly, region, industry, and firm size explain more than half of the SES gradient in firm pay. Where one works plays a particularly important role for intergenerational persistence at the top of the parental income distribution. While both the worker and firm components increase with parental income, the firm component increases more strongly in relative terms. The mean firm premium is twice as large in the top percentile as compared to in the 90th percentile of the parental income distribution. As such, this steep rise in firm premia in the right tail of the parent-income distribution is a potentially important explanation to the elevated persistence at the top of Sweden's income distribution, as documented by Björklund et al. (2012).

In the second part of our paper, we study how the firm pay gradient evolves over the lifecycle. Do children from high-income parents sort into higher-paying firms right at the beginning of their career, as one would expect if parents (Kramarz and Skans, 2014; Staiger, 2022) or peers (Cornelissen et al., 2017; Zimmerman, 2019) provide access to higher-paying firms? Or does the firm pay gap expand gradually over age, as one might expect if children from high-income families make faster career progress, perhaps by exploiting social and informational networks and by navigating the job search process more proficiently? We find that much of the disparities in firm pay open up right at the beginning of the career, consistent with the interpretation that children from richer parents find more favorable "entry points" to the labor market. Still, the SES gradient in firm pay widens further in the early part of the career, before stabilizing in the mid-30s. Differences in early-career progress therefore explain part of the firm pay gap, coinciding with the career stage when many workers have high earnings growth and when differences in earnings magnify relatively fast.

Indeed, high-SES children not only more frequently switch firms in the early career, but also make larger gains in firm pay conditional on switching. These results can be interpreted using the family of models that illustrate how search frictions can lead to variation in firm pay and "job ladders" (Burdett and Mortensen, 1998; Manning, 2013), suggesting that high-SES children reach higher rungs of the firm job ladder faster. All these patterns hold within education groups, but are especially strong among college graduates. These differences in the frequency and quality of job switching also hold when conditioning on voluntary firm switches, which we define as firm-to-firm transitions without any intermediate unemployment spell. Moreover, firm pay gradients exist not only in static comparisons ("firm fixed effects"), but also in a dynamic sense: children from more privileged backgrounds sort into expanding firms that are characterized by higher wage *growth*.

To study the implication of such dynamic firm advantages, we follow Arellano-Bover and Saltiel (2021) and use a k-means clustering algorithm to estimate an extended two-way fixed effects model that allows for firm-class specific returns to experience. We show that children from affluent families sort into firms with substantially higher wage growth; by age 40, these dynamic firm advantages contribute 20% to their overall pay advantage. Incorporating these dynamic advantages, the total contribution of firms increases to 35% of the IGE (or 38% when netting out the AKM residuals and covariates). We therefore find that labor market sorting explains a large share of the intergenerational persistence in income.

In the third part we investigate whether high-SES children are overrepresented in better-paying firms simply because they are more skilled. It is well known that firm and individual components from the AKM framework correlate positively due to assortative matching, i.e. that more productive workers tend to sort into higher-paying firms (Card et al., 2013). Because high-SES children have on average higher individual earnings components, the SES gradient in firm pay could potentially reflect such skill sorting. In this scenario, the firm component would merely be a "mediator" of the effect of skill differences and inequities in childhood.

We first show that the SES gradient in firm pay weakens when conditioning on the worker fixed effects from the AKM model: a unit increase in log parental income increases the firm pay premium at age 40 by 3.7 rather than 5.4 log points in the unconditional case. Thus, about 30% of the SES gradient in firm pay can be attributed to skill sorting as captured by the worker fixed effects – a similar share as Dobbin and Zohar (2023) find for Israel, using the same approach. However, our main contribution here is that we can test for skill sorting more directly, as we have access to late-adolescence skill scores from the mandatory military enlistment tests. Conditioning on cognitive and non-cognitive skills measured at age 18, the residual firm pay gradient decreases to about 50% of the unconditional SES gradient, i.e. *half* of the SES gradient in firm pay at age 40 is due to skill sorting. The cognitive skill measure provides the most additional explanatory power to understand the pattern of sorting.

While this analysis confirms that an important part of the SES gradient in firm pay is due to skills, we find that family background plays an important role for labor-market outcomes even conditional on own skill. Family background effects unrelated to skill play a particularly important role in the early career: at age 25, nearly 70% of the firm pay gradient is due to such direct family effects, falling to just 50% at age 40. Parental background and networks thus provide particularly crucial advantages at labor market entry; this finding is in line with recent evidence by Kramarz and Skans (2014), San (2022), and Staiger (2022), who document the importance of parents' co-worker networks for their children's job finding. These studies focus on isolating the effects of parental networks on job finding and earnings among young workers, but do not quantify the overall contribution of parental co-worker networks to the long-run firm pay gradient. In comparison, we can quantify the overall contribution of family background over the career, but cannot isolate the contribution of parental networks from other family background effects.

In the final part we ask whether the remaining SES gradient in firm pay can be traced to differences in preferences, compensating differentials, and/or other non-wage attributes of firms. If intergenerational income transmission partly stems from inherited preferences – e.g. that some families value income and consumption compared to non-monetary attributes more than other families – then measures of income persistence would overstate the extent to which differences in welfare persist across generations. On the other hand, if children from more affluent families sort into jobs with more favorable non-monetary attributes, in addition to higher firm pay premia, then intergenerational mobility in underlying welfare would be even lower than income-based estimates suggest. To our knowledge, there exists very little evidence on this potential mechanism behind intergenerational income associations.

We test for the role of non-monetary attributes of firms, using different approaches. We first explore how firm premia and parental income relate to auxiliary measures of the attractiveness of firms, including quit rates and hiring rates from other firms (poaching). The results confirm that higher-paying firms are more desirable employers (in the sense of being able to poach workers from other firms), and that children from more affluent families sort into more desirable firms. To deepen this analysis, we employ a revealed-preferences based approach similar to Sorkin (2018). The approach exploits two-sided firm-to-firm transitions of workers over time, and infers the non-monetary values of firms from whether a firm on net gains or loses workers from firms of different values. Overall, we find little evidence on compensating differentials being systematically related to the family background of workers. Skills and other drivers of labor-market advantages seem thus more important for intergenerational transmission than correlated preferences for non-wage job attributes within families.

Our work contributes to a nascent literature on the importance of labor market factors to intergenerational persistence. An important motivation for our study is Dobbin and Zohar (2023), whose methodological approach we adopt in Section 2. Compared to their paper, we find a greater contribution of firms to intergenerational persistence; while the firm pay gradient contributes 22% to the IGE in their Israeli data, we find that firm advantages contribute 31% in our static and 38% in our dynamic model (at age 40, and net of AKM residuals and covariates). Differences in data quality and, in the dynamic model, the incorporation of firm-specific returns to experience explain most of this contrast. We further find that skill sorting explains half of the contribution of firm pay to the IGE. This estimate is in line with Dobbin and Zohar, even though we use a different approach to study skill sorting. We use our richer data to extend their analysis in various ways. We study how the SES gradient in firm pay develops over the lifecycle, study the dynamic implications of labor market sorting, and examine the role of non-wage attributes of firms.

Parts of out analysis overlaps with Engzell and Wilmers (2024), who also study the intergenerational transmission of labor market advantages in Sweden. In comparison, they bring a sociological perspective based on stratification theory and place more attention to the source of earnings in the parent generation.

A novel part of our paper is that we highlight the *dynamics* of firm pay gaps, uncovering and understanding their development over the lifecycle. Our finding of a large SES gradient in firm pay opening upright at career start is consistent with recent studies tracing the effect of social connections on worker-firm sorting in early career. Corak and Piraino (2010) document that the intergenerational transmission of employers in Canada is stronger in high-income families; Bingley et al. (2012) and Stinson and Wignall (2018) confirm these findings for Denmark and the U.S. Further, Kramarz and Skans (2014) and Staiger (2022) quantify the role of parental and family connections in the early career. While their studies provide evidence on a particular mechanism (social connections), we quantify the contribution of firms to intergenerational persistence overall, which depends also on other mechanisms, such as skill sorting. We further show that rather than skills, the firm pay gradient at career start is primarily explained by other family-related advantages (including social connections). However, skill-based sorting explains most of the *increase* in the firm pay gradient over age.

Our work further relates to recent studies on skill sorting on the labor market (Eeckhout 2018, Card et al. 2018). Consistent with Dobbin and Zohar (2023), we find that there is far more sorting by parental background than one might expect from skill sorting alone. We document firm sorting with respect to cognitive and non-cognitive skill measures (similar to Nybom, 2017, for college education), over and above the extent of sorting captured by worker effects from worker-firm two-way fixed effects models. Our results also relate to recent evidence showing that differences in firm pay premia contribute to earnings gaps along other dimensions, such as gender (Card et al., 2016) and race (Gerard et al., 2021). For example, Gerard et al. (2021) find that skill-based sorting contributes to about 55-65% of the firm pay gap between whites and non-whites in Brazil. While the approaches differ, our estimates of the role of skill sorting for differences in firm pay by family background in Sweden are thus rather similar. Several recent studies also highlight the the extent to which firm pay differences contribute to cross-sectional inequality overall (e.g. around 20% in Card et al., 2013 or 11-12% in our data). Our findings suggest that differences in firm pay contributes a considerably higher share to *intergenerational* persistence in earnings.

1 Data and specifications

1.1 Data

Our empirical analyses require data on earnings, employers, education, and other background characteristics of parents and their children covering multiple decades. For this purpose, we combine a set of linkable administrative registers made available by Statistics Sweden. We use tax registers for earnings records, full-count employer-employee data to identify the firms and establishments of workers, the education register for highest level and years of education, and the multigenerational register to link children to parents.

The earnings data cover the full working population for the years 1968-2018. Our main analyses use gross annual earnings from work including self-employment, bonuses and fringe benefits, and short-term (employer provided) sickness benefits.[1] Firm and establishments including their location and industry affiliation are available for the years 1985-2018. Official birth and family registers allow us to match nearly all Swedish-born children born 1932 or later, and foreign-born children born 1961 or later, to both their parents.

Main intergenerational sample Our main analysis is based on men and women born 1967-1977, who can be observed on the Swedish labor market between age 25-41, and have at least one observed firm fixed effects at those ages. We focus on these cohorts such that we can observe a long and important part of their own labor-market career, while still being able to observe the prime-age incomes of their fathers. We measure the father's long-run income between ages 45 and 55, and consider as long-run income measures either the mean log earnings or mean earnings rank across the 45-55 age range. Fathers annual earnings observations are net of year dummies and quadratic age effects and ranks are computed relative to fathers in the main sample. We drop individuals for whom we cannot observe their father's income, which only applies to a small fraction of individuals (typically for migration-related reasons). In parts of our analysis we focus on the child generation's peak career outcomes, measuring earnings and employers at age 39-41. About 10% of the main sample drops out due to missing firm or earnings at these ages.

AKM sample. To estimate firm premia using an AKM model we use a second matched employer-employee sample, covering the entire Swedish labor

[1] In all analyses we exclude very low annual earnings observations. For each year, we compute the median earnings of men aged 45, and set annual earnings observations corresponding to less than 20% of this median to missing. This ensures that our estimates are not overly influenced by variation in labor supply.

force age 20-64 in the period 1985-2018. For this sample we use information on annual earnings, firm, age, gender, and education. The estimated firm and individual fixed effects are then used as inputs in our analyses of the main intergenerational sample.

Descriptive statistics. Table 1 provides descriptive statistics of the AKM sample and our main intergenerational samples. Differences between these samples are due to differences in age composition and due to the AKM samples containing observations from earlier years. As individuals in the AKM sample are on average born 12 years earlier, the fraction with a college degree and their log earnings are slightly lower compared to the main samples. The average log firm size is 6.35, but our data also allows us to identify the worker's specific establishment within large firms.[2] Our full-population data contain 1,301,551 observations with positive earnings at age 39-41. Imposing non-missing father links (e.g. due to migration) and earnings of the father, this number drops to 1,076,969 and 1,059,546, respectively. The sample size further drops to 910,665 when excluding very low earnings observations (of the child or father). Lastly, requiring a valid firm connection with identified firm fixed effect (and demographic characteristics such as education) leaves us with an age 39-41 sample of 857,064 observations.

Table 1. *Descriptive statistics*

	AKM sample	Main sample (born 1967-77)	
Age	20-64	25-41	39-41
Earnings years	1985-2018	1992-2018	2006-2018
Log earnings	12.46	12.54	12.79
Share women	0.49	0.50	0.49
Share with college degree	0.38	0.47	0.49
Year of birth	1960	1972	1972
Log firm size	6.35	5.97	6.05
Log establishment size	4.21	4.16	4.18
Number individuals	7,668,377	967,417	857,064
Number firms	341,798	228,285	118,258

Notes: Descriptive statistics for different samples. Column 1 shows statistics for the AKM sample, covering individuals aged 20 to 64 born between 1922 and 1997. Columns 2 and 3 show statistics for the intergenerational sample born between 1967 and 1977, separately for the 25-41 and 39-41 age ranges.

[2] Average firm size is relatively large for a few reasons. First, Sweden has a large manufacturing sector with many big firms. Second, public sector workers are included in our samples, and all working for a certain municipality count as working for the same "firm". Third, small firms without sufficient firm-to-firm mobility are excluded from all samples (see below).

1.2 Estimation of Worker and Firm Fixed Effects

We use the widely applied two-way fixed effects framework of Abowd et al. (1999) to decompose earnings into firm and individual components, conditional on a set of time-varying controls. Our specification for the log earnings, y_{ijt}, of individual i in year t while employed in firm $j = J(i,t)$ is:

$$y_{ijt} = \alpha_i + \psi_j + X_{it}\delta + \varepsilon_{ijt} \tag{1}$$

where α_i is a worker fixed effect, ψ_j a firm fixed effect ("firm pay premium"), X_{it} a vector of time-varying controls with coefficient vector δ, and ε_{ijt} an error term. The time-varying covariates in X_{it} include year dummies and a restricted set of education-gender-specific age dummies. Due to the well-known collinearity between age, cohort, and time, unrestricted age dummies are not identified. Rather than imposing a particular functional form for lifecycle earnings profiles, we follow Engbom et al. (2023) and assume that the age effects are constant for ages when the earnings profile is roughly flat, where we impose the age effects to be constant between ages 45 to 54. For studying lifecycle firm sorting by parental income (Section 3) it is important to account for education-specific variation in earnings over age, given that own education and parental income tend to be positively correlated. We therefore allow for earnings profiles to vary freely by education groups.

The firm pay premia ψ_j in equation (1) are identified from conditional changes in earnings as workers switch firms. In particular, they are only identified relative to some baseline firm within a set of firms connected through such firm-to-firm transitions ("movers"). Firm fixed effects identified within non-overlapping connected sets of firms are not directly comparable to each other. We therefore follow standard procedures to compute the largest connected set for our time period and drop worker-year observations for which workers are employed in firms outside of this set (approximately 0.7% of all worker-year observations). Firm fixed effects tend to be noisily estimated for firms connected to other firms by only a small number of movers (e.g. Andrews et al., 2008; Bonhomme et al., 2023; Kline et al., 2020), which leads to an inflated variance of the firm component in a variance decomposition based on equation (1). We thus drop firms (and associated worker-year observations) that are connected to other firms through fewer than five movers (an additional 6% of worker-year observations). [3] This adjustment together with the fact that we use population-wide data for a long time period should diminish concerns about such limited-mobility bias.

We estimate equation (1) using our employer-employee sample for the years 1985-2018. We focus on full-time workers, approximated by excluding worker-year observations with annual earnings lower than 20% of the yearly median earnings of 45-year old males. We also provide robustness tests using data on

[3] However, table A4 in appendix A.2 shows that the results remain similar when we do not exclude firms with fewer than 5 movers.

actual wage rates instead of annual earnings for a large subsample covering the same time period (see Appendix A.2). In the subsequent sections, we use our estimates of α_i and ψ_j as inputs in our intergenerational analysis.

Using our estimates from equation (1), we can decompose the variance in earnings into its different components; Appendix A.1 reports such decompositions for both the AKM and the intergenerational samples. We find that worker effects explain 29-38% while firm effects explain 7-11% of the variance in log earnings. The covariance between worker and firm effects contributes another 7-11%, reflecting the sorting of more productive workers to better-paying firms (assortative matching). Overall, our decomposition results are similar to Engbom et al. (2023), who use similar data and specifications, and also largely in line with evidence from the US (e.g. Song et al., 2019).

2 The contribution of firms to intergenerational persistence

We start by providing some basic facts on how children from different socioeconomic backgrounds sort into firms with different pay premia, as well as how such firm sorting contributes to intergenerational income persistence. To this end, we estimate variations of the linear regression

$$y_{ijt} = \alpha + \beta y_{f(i)} + u_{ijt}, \tag{2}$$

where y_{ijt} is child log earnings, $y_{f(i)}$ the log earnings of the father of child i, and β is the intergenerational earnings elasticity (IGE). We measure the father's long-run income as the mean residual of log income for the ages 45 and 55, where log income has been residualized of year dummies and quadratic age effects. The childrens income are measured as mean of log income for the ages 39-41. As y_{ijt} can be decomposed according to equation (1), the slope coefficients from separate regressions of each of its components on $y_{f(i)}$ will together sum up to β. Our primary focus is on the slope coefficient from a regression of the child's estimated firm pay premium $\hat{\psi}_j$ on $y_{f(i)}$, which we denote β_{firm} and refer to as the *socioeconomic status gradient in firm pay* or, shorter, *SES gradient in firm pay*.

Table 2 shows estimates of the IGE and its components. The estimates are based on our main intergenerational sample, with earnings and firm premia measured as averages over age 39-41. As shown in column (1), the IGE is roughly 0.20 in our sample, which aligns with prior Swedish estimates of the IGE in *labor* income in pooled samples that include both sons and daughters (e.g. Brandén and Nybom, 2019; Engzell and Mood, 2023).[4] Columns (2) and

[4]The IGE tends to be slightly higher for total income, especially for sons (e.g. Nybom and Stuhler, 2016). In our sample, the IGEs estimated separately for sons and daughters are 0.23 and 0.17, respectively.

(3) of Table 2 decompose the IGE into individual and firm components. About 60% of the IGE is attributed to the individual effects, which capture all permanent determinants of earnings (e.g. time-constant abilities and skills). The firm effects contribute 27% of the IGE, rising to 31% if we first net out the AKM covariates and residuals from y_{ijt}.[5] The firm pay gradient explains therefore a sizable share of income persistence from one generation to the next.[6] Its magnitude for Sweden contrasts with earlier work by Dobbin and Zohar (2023), who find that firms contribute about 22% to the IGE (after having netted out the AKM residuals) in Israel – about two thirds of the share that we find in our setting.[7]

Why do we find stronger sorting of workers across firms by family background than previous work? One possibility is that firm sorting is really stronger in Sweden than in Israel. But another possible explanation is that differences in the quality of the underlying income data explain part of this gap. In particular, while Dobbin and Zohar observe all workers in the Israeli labor market in a six-year period (2010-2015), we observe the entire Swedish labor force over a 34-year period (1985-2018), allowing us to pin down firm pay premia more precisely. When we replicate their data scenario, with an earnings panel over the years 2010-2015, we find a substantially diminished SES gradient in firm pay (see Appendix Table A2), explaining only 16% of the IGE (18% when netting out the AKM covariates and residuals). A classical measurement noise should not effect the estimate since firm pay is the dependent variable. However, figure A1 shows that the noise it not classical, instead the firm premia estimated for the short period seems to systematically overestimate the firm premia at low values of the firm premia and underestimate the value of the firm premia for high values of the firm premia. Differences in the quality of the AKM estimates therefore seems to affect the estimated firm con-

[5]The correlation between the log of father's income and $X_{it}\beta$ and ε_{ijt} explains 14% of the IGE (see column 4). Abstracting from this component, firm effects explain 27%/ (59%+27%)=31% of the IGE.

[6]As shown in Appendix Table A6, the firm pay gradient is slightly stronger for women than men.

[7]The magnitude we find is also slightly larger than in Engzell and Wilmers (2024), who use similar Swedish data but focus on percentile ranks of earnings and firm pay premia

tribution to the IGE.[8][9] Section 2.3 provides more evidence on the sensitivity of our estimates to sampling and specification choices.

Table 2. *Decomposition of the intergenerational earnings elasticity*

	Dependent variable				
	y_{ijt}	$\hat{\alpha}_i$	$\hat{\psi}_{j=J(i,t)}$	$X_{it}\hat{\beta}+\hat{\varepsilon}_{ijt}$	$\hat{\psi}_{j=J(i,t)}$
	(1)	(2)	(3)	(4)	(5)
$y_{f(i)}$	0.201***	0.118***	0.054***	0.029***	0.037***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)
$\hat{\alpha}_i$					0.151***
					(0.001)
Share of IGE	100%	59%	27%	14%	18%
Worker obs.	857,064	857,064	857,064	857,064	857,064

Notes: Column (1) reports the estimated slope coefficient from regression (2) of mean of log child earnings at ages 39-41 on log father's earnings. Columns (2)-(4) report the slope coefficients from the corresponding regressions when decomposing child log earnings y_{ijt} according to equation (1) into individual fixed effects α_i, mean firm fixed effects ψ_j for the same ages earnings are estimated, and time-varying controls. Robust standard errors in parentheses.

Skill Sorting. While worker-firm sorting thus explains a substantial share of the IGE, such sorting might be only indirectly related to family background. Prior research has documented assortative matching between workers and firms, such that firm and individual fixed effects tend to correlate positively. This sorting of more productive workers to higher-paying firms contributes to income inequality in the cross-section, as shown in eq. (A1) and Appendix Table A1 (see Card et al., 2013; Song et al., 2019). In addition, worker-firm sorting increases intergenerational persistence, since individual fixed effects correlate positively with parental income (see column (2) of Table 2). The SES gradient in firm pay could therefore be a "mechanical" conse-

[8] While the results from the measurement error correlations are in line with the importance of high quality data to estimate the AKM, differences in firm pay for the specific periods could also potentially explain the results.

[9] The quality of the data differs also in other aspects. First, we measure parental earnings over a longer age span, between age 45-55. Second, we can retain 93% of all Swedish children born in 1967-1977 in our analysis, while Dobbin and Zohar (2023) drop about half of their sampled cohorts due to insufficient information on earnings or incomplete sets in the AKM estimation. We show in Appendix Table A3 that randomly dropping half of our sample has little effect on our estimates. Excess homogeneity in non-representative sample could bias estimates of the IGE downward (Solon, 1992), but it is less clear how it would impact the share of the IGE attributable to firms.

quence of the assortative matching between workers and firms as documented by prior research.

To abstract from the general degree of assortative matching between workers and firms, we follow Dobbin and Zohar (2023) and regress the firm fixed effect on the log of father's income *conditional* on the individual fixed effect (column (5) of Table 2). The coefficient on the individual fixed effect is large and statistically significant, reflecting assortative matching. The coefficient estimate for log of father's income now falls by about 30%, to 0.037. Part of the SES gradient in firm pay thus originates from gradients in (permanent) worker-level characteristics (e.g. skills), as captured by the individual fixed effects. Put differently, by amplifying the pay-off to skills, worker-firm sorting magnifies the well-known SES gradient in skills and human capital. According to the estimates here, this mechanism explains about a third of the firm contribution to intergenerational persistence; even after accounting for assortative matching, systematic differences in firm premia still explain roughly 18% of the IGE. The sorting of workers across firms by family background appears therefore substantially stronger than one would expect based on the observed degree of skill sorting in a worker-firm fixed effects model. However, skill sorting is difficult to measure, and we return to this question in Section 4.

2.1 Non-linear firm pay gradients

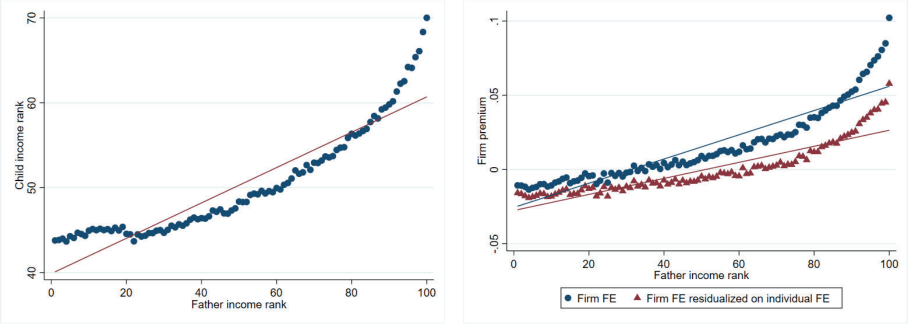
Does the sorting across firms matter more among low- or high-income families? Figure 1 shows how the expected child earnings (subfigure A) and firm premia (subfigure B) at age 40 vary across the distribution of parental income, with incomes now expressed in ranks (see the corresponding Figure A2 in the Appendix for log income). The two figures reproduce the positive relationships from Table 2, but with a gradient that is strongly increasing starting from about the 75th percentile of the parental-income distribution. The SES gradient in firm pay is qualitatively similar when conditioning on the individual fixed effect (red triangles in subfigure B), but smaller in magnitude. Moreover, the difference between the unconditional (blue circles) and conditional (red triangles) gradients increases along the distribution, implying that skill-based sorting is particularly important among richer families. Overall, the results show that sorting into high-paying firms is an especially important driver of intergenerational transmission among high-income families.

2.2 The roles of region, industry, and firm size

We next explore some potential determinants of the SES gradient in firm pay. Pay premia differ not only across firms, but also across regions and industries. For example, the industries with the highest mean firm pay premia are oil and

Figure 1. Child income and firm premia by father's income (ranks)

(a) Child vs father income rank *(b)* Firm premium vs father's income rank



Notes: Figure (a) shows binned scatter plots of child's income rank at age 40 by father's income rank. Figure (b) shows firm fixed effects ψ_j at age 40 estimated by equation 1 and firm premia residualized on individual fixed effect, by father income rank.

natural gas extraction, financial sector support services, telecommunications manufacturing, chemical manufacturing, IT services, and research and development. Moreover, large firms tend to pay higher premia than smaller firms. The positive relationship in Table 2 could therefore, among other things, be due to children entering firms in similar industries as their fathers, or located on the same local labor markets.

Table 3 extends our prior analysis by controlling for various sets of fixed effects. Controlling for region (21 counties, column 2) diminishes the SES gradient in firm pay by roughly 20%. Redoing the main analysis within 2-digit industries (59 in total, column 3) instead diminishes the SES gradient by about 36%. Controlling for the size of the workplace has a smaller, yet non-trivial effect on the estimated SES gradient (column 4). Controlling for region, industry and size jointly has a noticeable impact, with only about 42% of the unconditional SES gradient in firm pay being left unexplained. Thus, an important share of the SES gradient can be traced to observable firm characteristics, though a similarly important part of the gradient remains unaccounted for.

2.3 Sensitivity to specification and sampling choices

We perform a number of tests to probe the sensitivity of our estimates to specification. First, one might worry that the time period we use to estimate the AKM components is *too* long; while we capture a lot of worker flows between firms, the assumption that firm-specific pay is fixed over time is less likely to hold over long time spans. Thus, we consider a modified AKM specification in which firm pay is allowed to vary over time (e.g. Lachowska et al., 2020; Engbom et al., 2023). To estimate the time-varying AKM we divide the period

Table 3. *Decomposing the relationship between firm premia and parental income*

	Dependent variable: Estimated firm pay premium $\hat{\psi}_{j=J(i,t)}$				
	(1)	(2)	(3)	(4)	(5)
$y_{f(i)}$	0.053***	0.041***	0.034***	0.045***	0.022***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Region FEs		X			X
Industry FEs			X		X
Establishment size (log)				X	X
Share of β_{firm}	100%	77%	64%	85%	42%
Worker obs.	812,697	812,697	809,758	780,025	779,596

Notes: Column (1) reports estimates of the slope coefficient from regressing $\hat{\psi}_j$, as estimated from equation (1), on log fathers earnings. Columns (2)-(5) report coefficient estimates from the same regression with different sets of control variables. Column (2) controls for region fixed effects (21 counties), column (3) for industry fixed effects (2-digit level, 59 industries), column (4) for log establishment size, and column (5) controls for region, industry and log establishment size simultaneously. Robust standard errors in parentheses.

1985-2018 into 4 periods, and allow the firm fixed effects to vary between the periods. As shown in Appendix Table A4, Panel A, this modified specification only renders a very marginal increase in the role of firms; we therefore continue to use the traditional AKM specification as our baseline. Second, we consider sorting across *establishments* rather than firms. As shown in Panel B of Table A4, establishment fixed effects explain 35% of the IGE (39% if we first net out the AKM residuals), a substantially greater share than firm fixed effects (cf. Table 2). While there are arguments in favor of using establishment codes – for example, public sector employers constitute large and somewhat artificial "firms" – we align with the bulk of the related literature and stick to using firms in our main analyses. Third, we explore whether the results are sensitive to using wages rather than annual labor earnings as outcome. Wages are observed for a subsample of workers in an employer survey. While the role of firms is considerably diminished (see Appendix Table A5), part of this decrease appears to be due to the wage sample being smaller and more selective (the wage survey oversamples workers in large firms and the public sector). We thus proceed with using earnings, as much of the prior literature, but recognize that variation in labor supply might contribute to the estimated AKM components.

We further test how our estimates vary with sampling choices. One question is whether the firm pay gradient is driven by children choosing to work in the same firm as their parents. However, while the share of "firm followers" is indeed non-negligible in our sample (in line with evidence by Corak and Piraino 2010, Stinson and Wignall 2018), the estimated firm pay gradient remains nearly unchanged when we drop them from the estimation sample

(Appendix Table A6, Panel C). In contrast, dropping public sector workers raises the firm contribution by 10% (Table A6, Panel D). This might reflect that wages are more compressed in the public sector, but also that some public employers are very large, such that their firm fixed effects may not well reflect pay in different groups within the “firm”. Finally, restricting the sample to firms for which we observe at least 10 or 50 “movers” in the AKM estimation decreases the firm pay gradient (Table A6, Panels E and F). On the one hand, dropping small firms with few movers decreases the limited mobility bias in AKM estimates (Bonhomme et al., 2023). On the other hand, firm fixed effects might be less informative about worker-specific pay in large than small firms. We therefore stick with our baseline specification, which retains firms with at least five movers.

3 Firm pay gradients over the lifecycle

The previous section showed that children from high-income families end up working at higher-paying firms at age 40, that this pattern holds conditional on their own permanent skills as captured by the individual fixed effects, and that this firm-sorting explains a sizable part of the persistence of income inequality across generations. This section explores the career dynamics of firm pay, with the aim to further understand how and why children from more privileged family backgrounds end up at higher-paying firms.

3.1 Firm pay premia over the lifecycle

Figure 2 plots age profiles of the mean firm pay premium by quartile of parental income. Subfigure (a) shows a clear SES gradient in firm pay already at age 25, with children from higher parental-income quartiles working at firms with higher pay premia. Further, children originating from the top parent quartile (yellow squares) improve their firm premia at a substantially faster rate than other children in the early part of their careers: while the bottom three quartiles increase their firm premium by about 1 percentage point up until the early 30s, the increase among children from the top quartile is about 3 percentage points. The gap in firm pay stabilizes in the mid-30s. The standard deviation of the firm premium is 0.14-0.15, depending on the sample (see Table A1), which implies that children in the top quartile have a firm pay advantage compared to the next quartile corresponding to about 29% of a standard deviation.

However, some of these dynamics might be driven by education-specific differences in career profiles and systematically higher education levels and delayed labor-market entry among children from well-off backgrounds. Subfigures 2b and 2c thus reproduce the same plot separately for children with at most high school and some college or more, respectively. In both groups, a similar type of SES gradient in firm pay is apparent. However, among non-college children, firm premia generally grow less over age. Among college children, all quartiles grow their firm premia over age, but the top-quartile children do so at a faster rate than others. While the levels and early-career growth in firm premia are higher among college graduates, the qualitative patterns are surprisingly similar whether we consider all children or only children with similar levels of education.[10]

Given the stability of the patterns when comparing the full sample and education-based subgroups, we proceed to focus on the former. However, results by education are reported in Appendix A.5, and we discuss noteworthy education-related differences in the main text.[11] Finally, Figure A4 in the Appendix plots the difference in firm pay premia over the lifecycle conditional on individual fixed effects, to account for the general degree of assortative matching between workers and firms that is due to skill sorting (i.e., the lifecycle version of column (5) of Table 2). The gaps in firm pay premia as well as the large early-career increase in firm pay among children with high-income fathers remain largely similar.

In sum, gaps in firm pay open up already at the beginning of the career, with high-SES children being substantially more likely to work in firms with more generous pay policies. This gap increases further in the early career stages, and then stabilizes by the mid-30s. In Section 4 we show that the initial gap *cannot* be explained by differences in worker skills, while the subsequent widening of the gap is mainly explained by skill differences that correlate with parental background.

3.2 Climbing the job ladder

We showed that high-SES children not only start their career at higher-paying firms, but also that a large part of the long-term SES gradients in firm pay build up over the first 10-15 years on the labor market. This divergence can be due to either *more frequent* firm switches among high-income children (i.e., climbing the job ladder faster), or that they do *better* switches (i.e., the rungs of their ladder are further apart), or a combination of both.

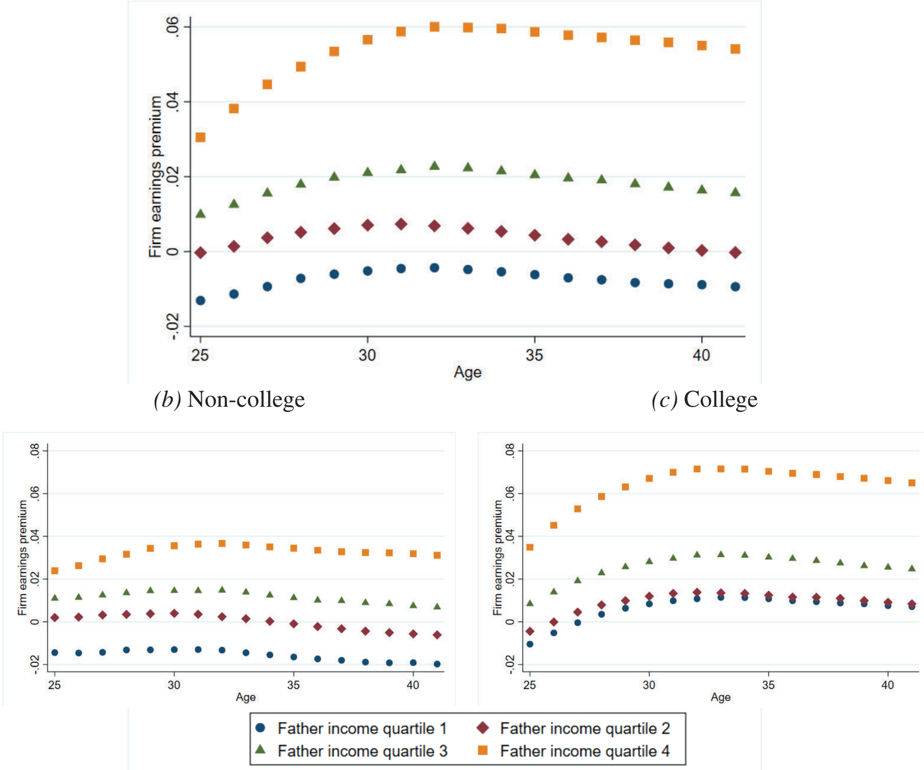
Figure 3a shows the annual firm switch rate, i.e. the probability of being observed in a new firm at age a compared to at age $a-1$, by age and parent-income quartile. As expected, all children are more likely to switch firms at early age, with roughly 20-25% of young workers annually switching firm

[10] This similarity reflects that time-constant differences in worker pay are captured by the individual fixed effects, while differences in lifecycle growth within firm are captured by the education-age interactions in equation (1).

[11] To further probe that our results on career dynamics are not driven by differences in age at labor-market entry we reproduce the analysis by potential experince, finding very similar results (see Appendix A.5).

Figure 2. Firm earnings premium over the lifecycle

(a) Full sample



(b) Non-college — *(c)* College

Notes: The figures plots the mean estimated firm premium $\hat{\psi}_j$ over the life cycle, by quartile of father's income. Sub-figure (a) shows the result for the full sample, sub-figure (b) shows the result for children without collage education and sub-figure (c) shows the result for children with college education. Father's income quartiles are defined in the full sample.

through ages 25-30, while only 11-12% switch at age 41. Up until the early 30s, there is also a small but clear SES gradient in the likelihood to switch firm, with high-SES children being more avid switchers. These differences in the probability of switching diminish over age, and become negligible after age 35. The patterns hold within education groups (Appendix Figure A6), though overall higher education is associated with slightly more firm switching and the elevated switching of high-SES children is more pronounced in the college group.

Simply moving to a new firm is not enough to improve your firm pay, unless such moves entail improvements in firm pay premia.[12] Figure 3b plots the proportion of firm switches that are indeed premium-improving, defined

[12]While there are other potential reasons why worker mobility may benefit human capital and wage growth, we focus here on the benefits in terms of firm pay premia.

as moving to a firm at age a with an estimated firm premium that is strictly larger than the one in the previous firm at age $a-1$, again computed separately by age and parent-income quartile. Children tend to switch to better firms at early age (i.e., they "climb the firm ladder"), as the chance of a "good" (premium-improving) firm switch is greater than 50% for all SES groups. But again there are distinct differences with respect to SES, with high-SES children being more likely to experience such premium-improving switches. The likelihood of good and bad switches tend to even out over age, as well as the SES gradients. Moreover, while low-SES children are more likely to switch firms than mid-SES (subfigure a), they are not more likely to gain in firm pay conditional on switching. This observation indicates that high- and low-SES children switch for different reasons, e.g. the latter might to a larger extent be involuntary switches or moves between various forms of temporary employment.

Figure 3c shows the average change in the firm premium in a similar fashion. The figure shows a clear gradient indicating that with every firm switch, high-SES children raise their earnings by about one additional percent compared to others through improved firm pay. Put differently, children from the top quartile of parental income enjoy gains in firm pay that are more than twice as large as the gains achieved by children in the lower quartiles. This difference shrinks over age, and after age 35 the effect of switches on firm pay premia is close to zero, independent of parental income. However, this closing of the gaps at later ages may be a mechanical consequence of gaps at earlier age: rapid improvements at early age should mechanically make it harder to improve your firm premium at later age, while those who remain working at low-pay firms at early age retain more scope for improvements in firm pay later on. In the extreme scenario of fully randomized switches, low-SES children with lower initial firm premia would experience much larger gains from switching.

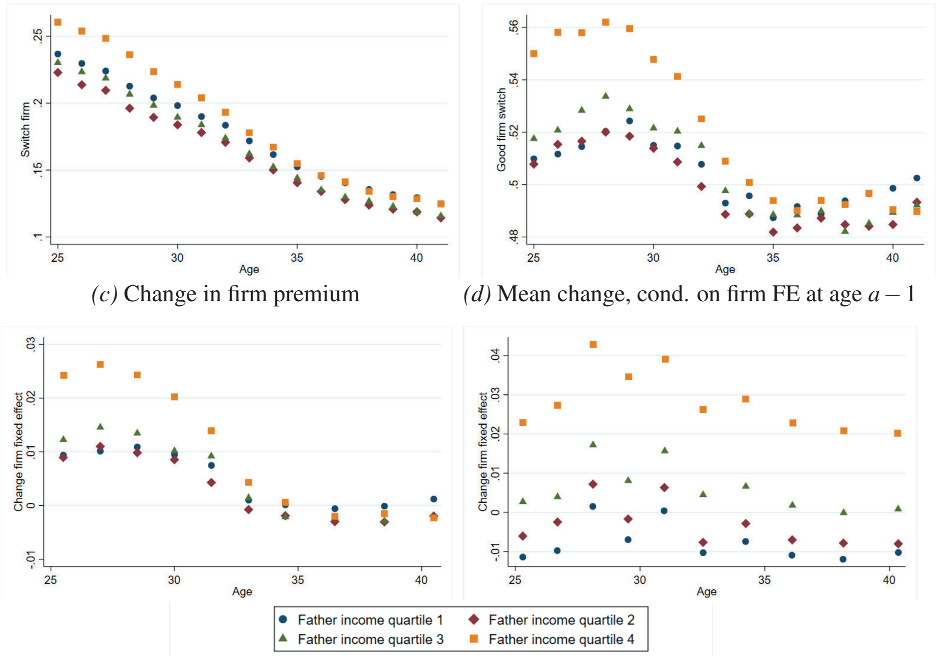
To account for this mechanical relation between the change and level of firm pay premia, Figure 3d reports the change in firm premia while conditioning on the level of the firm fixed effect in the previous year. Thus, instead of relating the new firm premium to your own premium in the year prior, we implicitly relate them to the mean premium of the cohort in the year prior. We find that over the entire age range, high-SES children switch to better firms than children in the lower quartiles. Thus, the estimates in Figure 3c are partly compressed by the fact that premium-improving switches are increasingly hard to find for those who have already climbed high up on the firm ladder. Accounting for this mechanical relation, the SES gradient is now essentially constant over the lifecycle.

Appendix Figures A7 and A8 show the proportion of "good" firm switches and the mean change in firm pay conditional on switching separately by education group. College-educated individuals are generally more likely to experience premium-improving switches and larger boosts in firm pay through-

Figure 3. Firm switching patterns over the lifecycle

(a) Probability to switch firms *(b)* Premium-improving firm switches

(c) Change in firm premium *(d)* Mean change, cond. on firm FE at age $a-1$



Notes: The figures show pattern for switching firms over the life-cyle, by quartile of father's income. Figure (a) shows the probability of working in a new firm compared to the year before. Figure (b) shows the probability of switching to a firm with a higher firm premium than the one before conditional on switching. Figure (c) shows the difference between the new firm premia and the firm premia before for individuals who switch firms. Figure (d) shows the same but adds a control for the firm premia in the firm before the switch.

out the career, but the SES gradients are otherwise similar across education groups. Moreover, we show that the larger gains in firm pay among high-SES children does not appear to be driven by differences in unemployment – even considering only voluntary switches without any intermediate unemployment there is an SES gradient in the quality of switches.[13]

3.3 Commuting up the job ladder?

One reason for why children from high-income families gain higher firm pay premia over the life-cycle is that they might be more likely to commute longer for work. In particular, they might have access to wider social networks and/or

[13] We can use data on UI benefit receipts to distinguish voluntary switches (without any UI receipt in between employment spells) and involuntary switches via spells of unemployment (as measured by UI benefit receipt). As shown in Figure A9, high-SES children are more likely to experience improvement in the firm premium following both voluntary and involuntary switches.

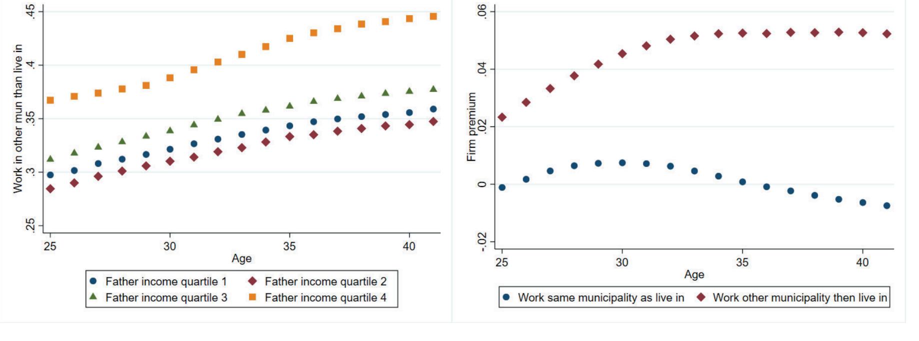
have access to support structures that make commuting easier, such as child care or different modes of transportation. By expanding the choice set of feasible jobs, commuting may increase wages and firm pay premia (Le Barbanchon et al., 2020; Agrawal et al., 2024). On the other hand, commuting is costly, both financially and in terms of time. Hence, if differences in commuting patterns can explain a sizable fraction of the SES gradient in firm pay, then gaps in firm pay premia might overestimate the implied gaps in overall welfare.

Figure 4a shows that children from high-income families are indeed more likely to commute (i.e., working and residing in different municipalities). At age 25, the share of commuters is 8 pp. (nearly 30%) higher among children in the top compared to bottom quartile of parental income at age 25, increasing to 10 pp. by age 40. Figure A11 in the Appendix shows that these findings also hold within education groups; while more educated workers are more likely to commute, differences in education do not explain the strong SES gradient in the commuter share.

Figure 4b shows that commuters earn higher firm premia, in particular at later ages. At age 40, the gap in firm premia between commuters and non-commuters reaches 6 pp., nearly half of a standard deviation. However, this gap also reflects selection effects, as workers who commute might differ in other important ways from those who do not (e.g., live in a bigger city, where it is more common to work and reside in different municipalities). To abstract from selection, Table A9 in the Appendix reports event-study type regression estimates conditional on individual fixed effects, showing how firm pay changes for individuals who begin to commute. We find on average commuting raises firm premia by about 1 pp., increasing to 1.7 pp. at age 40. Differences in commuting behavior therefore contribute to the SES gradient in firm pay, and the observed increase of that gradient over age (cf. Figure 2).

Figure 4. Commuting

(a) Commuting and parental income *(b)* Commuting and firm premia



Notes: Figure (a) shows the proportion of individuals who commute (i.e., work in another municipality than they live in) over the life-cycle, by quartile of father's income. Figure (b) plots the mean firm premia by age and commuting status.

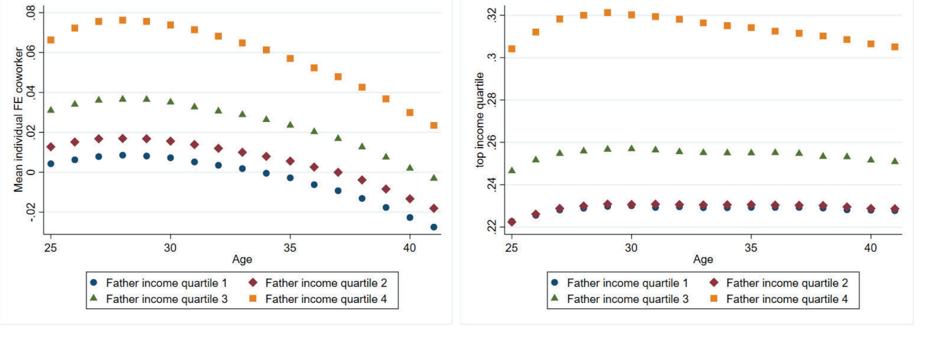
3.4 Beyond firm pay premia

Firms differ in other important dimensions apart from their pay premia. In this section, we study whether the type of firms in which children from high-income families work offer other advantages apart from higher pay. We begin by considering "static" firm characteristics, such as the composition of the workforce. Figure 5a shows that children from high-income families tend to have more productive co-workers, as captured by their estimated AKM worker fixed effects $\hat{\alpha}_i$. The mean co-worker effect is about 6 pp. higher for children from the top compared to bottom quartile of parental income. They are also exposed to a higher share of co-workers from high-income families, as shown in Figure 5b. This type of firm-level segregation is relatively stable over age – if anything, workers are more segregated by own productivity and SES at early than at later age. If working with more productive co-workers increases one's own productivity (as in Kremer, 1993), such segregation might also contribute to the firm pay advantage that we documented in section 3.1.

Figure 5. Static firm characteristics

(a) Individual fixed effects of co-workers

(b) Share of co-workers with father in top quartile



Notes: The figures shows firm characteristics by age, by quartile of father's income. Sub-figure (a) shows the mean individual fixed effect of the co-workers. Sub-figure (b) shows the share of co-workers with fathers in the top income quartile.

The fact that high-SES children end up in firms with more productive workforces (as measured by co-worker individual fixed effects) might have interesting implications regarding their career trajectories, as there might be positive spillovers on their own productivity due to learning or fostering of valuable social networks. Moreover, firms might offer different opportunities for career development, partly because different firms might themselves grow at different rates. As a consequence, firms may have different *dynamic* implications, apart from the static difference in firm pay as captured by the AKM approach.

To illustrate that firms also differ in a dynamic sense, Figure 6 shows the mean employment and earnings growth of co-workers over the following five years (i.e., between age a and age $a+5$). Figure 6a shows that children from high-income families are more likely to work in growing firms. Indeed, children from the top quartile of father's income work in firms that grow 50-100% faster than children in the lower quartiles.

High-SES children are also more likely to work in firms with high earnings growth, as shown in the next two sub-figures. Figure 6b plots the mean earnings growth between age a and age $a+5$ for co-workers who stay in the firm, while Figure 6c tracks the five-year earnings growth for co-workers irrespectively of whether they stay or leave the firm. Interestingly, the SES gradients are much more pronounced in Figure 6c, suggesting that much of the difference in co-worker earnings growth comes from mobility to new firms, rather than just differences in earnings growth for incumbent workers within firms. This finding is in line with the results shown above that children from high-SES families tend to work together, are more likely to switch firms, and make switches that render higher firm pay premia.[14]

3.5 Heterogeneity in firm-specific returns to experience

Individuals from high-income families are more likely to sort into high-paying firms, but firms differ in other dimensions, such as the average earnings growth of workers in those firms. Such differences in earnings growth could arise because of different learning or promotion opportunities; and while some firms might not pay that well, it could still be optimal for individuals to work in such firms if they offer higher returns to experience, increasing income later in the career.

To more explicitly estimate the role of firm-specific returns to experience, we follow Arellano-Bover and Saltiel (2021) and split our sample of workers into two random samples. Using one of the random samples we divide firms into ten classes using the distribution of stayers' yearly unexplained earnings growth using a k-means clustering algorithm. We then use the other random sample to estimate the firm-class specific returns to experience following the methodology in Arellano-Bover and Saltiel (2021). In particular, we estimate an extended two-way fixed effects framework according to:

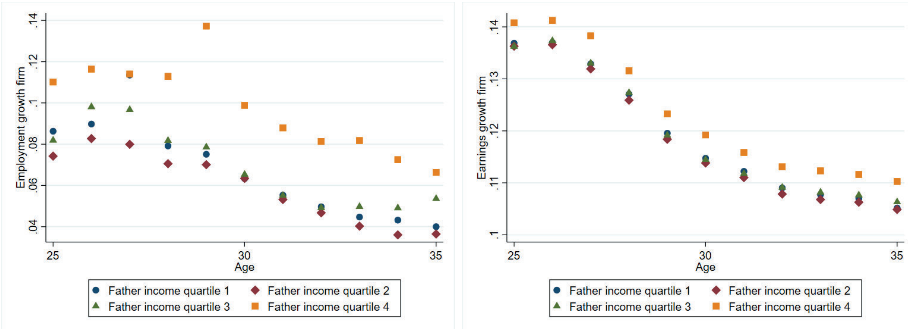
$$y_{ijt} = \alpha_i + \psi_j + \sum_{m=1}^{K} \gamma_m Exp(m)_{it} + X_{it}\beta + \varepsilon_{ijt}, \tag{3}$$

where $Exp(m)_{it}$ is years of experience in firm class m up until year (or age) t. As above, we include individual and firm fixed effects, such that γ_m is identified from workers who are employed in the same firm, but have earlier experience from different firm classes. $X\beta$ controls for age and year fixed

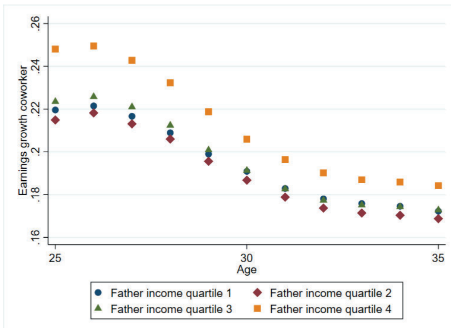
[14]Interestingly, the gaps between the top quartile of father's income and the rest in co-worker earnings growth and firm employment are relatively stable across age, while differences between those from the first, second, and third quartiles are generally much smaller.

Figure 6. Firm-level employment and earnings growth

(a) Employment growth of firm *(b)* Earnings growth of co-workers, stayers



(c) Earnings growth of co-workers, all



Notes: The figures show the difference in firm characteristics between year t and year $t+5$ for the individuals who work in the firm at that age by quartile of father's income. Figure (a) plots the mean employment growth in the firm, by quartile of father's income. Figure (b) plots the earnings growth for coworkers who stay in the firm between year t and $t+5$. Figure (c) shows the earnings growth for all coworkers, including co-workers who stay in the firm and co-workers who switch to another firm.

effects.[15] To be able to estimate equation (3), we limit the sample to workers who we can observe for their whole career, and thus focus on workers born between 1967 and 1977 observed over ages 20 to 41.

Figure 7 summarizes our results. In sub-figure (a) we show the estimated returns per year of experience by firm class m, where class 1 is the firm class consisting of firms with the highest returns to experience and class 10 consists of the firms with the lowest returns to experience. Largely similar to Arellano-Bover and Saltiel (2021), we find important differences in the returns to working in firms belonging to different classes as defined by their un-

[15] Since we only include observations up to age 41 we cannot use the assumption that ages is constant between ages 45-54 as we do in our main AKM specification to be able to include both age and year fixed effects. Instead we normalize age relative 40 and include second and third order polynomials of age interacted with education and gender.

explained earnings growth. However, the differences in firm-specific returns between the top and bottom classes are considerably smaller in our Swedish data compared to their data from Italy and (especially) Brazil: while workers in the top firms (firm class 1) experience an annual firm-specific boost to their earnings growth of 2.5 percentage points, the expected firm-related earnings growth in the bottom firms (type 10) is slightly negative. Note that these growth components are net of the general education-by-gender specific earnings growth component as captured by $X_{it}\beta$.

In Figures 7b and 7c we show how the probability to work in high-return firms (classes 1 or 2) and low-return firms (classes 9 or 10) differs by SES and over age. Children from high-income families are more likely to work in firms with very high returns to experience (belonging to the top-two classes) and less likely to work in firms with the lowest returns to experience (the bottom-two classes). The gap in the probability to work in firms with high returns is evident at all ages, and peaks around age 27-28, when about 16% of top-quartile children and about 10-11% of the non-top-quartile children work in high-return firms. Differences between those from quartiles 1-3 are generally smaller; if anything, it is those from the second quartile that have the lowest (highest) chance to work in firms with high (low) returns to experience.

Figure 7d shows the estimated average annual firm-specific return at the individual's current firm at different ages, separately by father's income quartile. We can see that top-quartile children are more likely to be employed in firms with higher returns to experience throughout the observed age range. There is also a clear tendency of an increasing SES gap in returns up until about age 28-29, while the gap shrinks after that age. In terms of magnitudes, those with parents in the top quartile enjoy firm-specific returns that are on average 20% higher between age 20 and age 30 than those with parents in the bottom quartile.

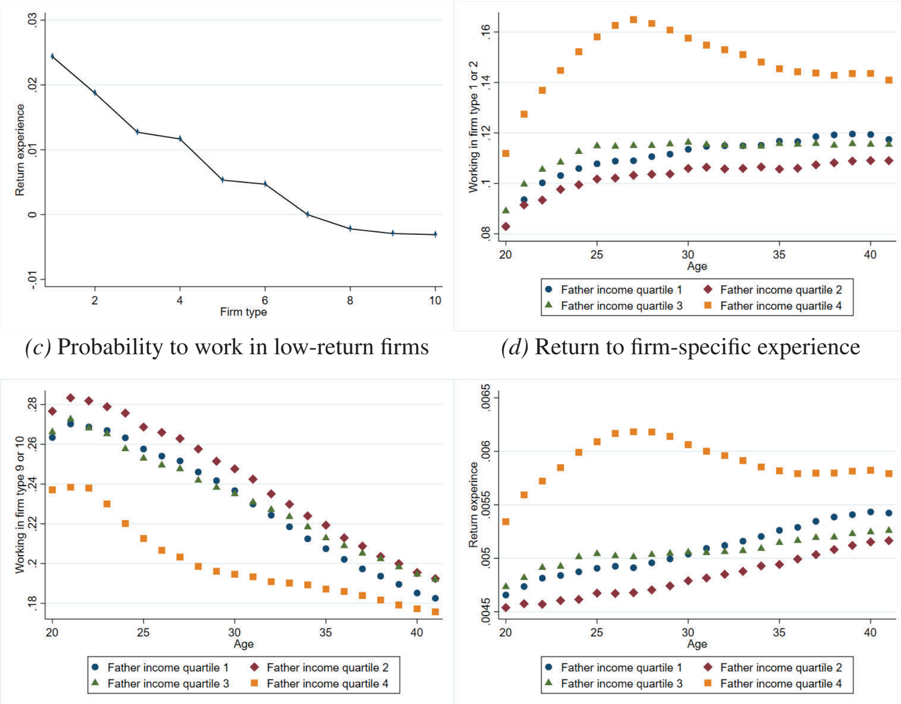
Table 3 quantifies how much of the intergernational earnings elasticity at age 40 (column 1) can be attributed to the different components in equation (3). In particular, we decompose the contribution to the firm pay premium at age 40 into the early-career firm premium at age 25 (column 3) and the *change* in firm effects between ages 25 and 40 (column 4). Almost half of the SES gap in firm pay at age 40 – and 12% of the IGE – can be attributed to changes in firm pay over age, i.e. SES gaps in firm-ladder climbing (see Section 3.2). Moreover, working in firms with higher *returns to experience* explains another 7% of the intergenerational elasticity (column 5). This estimate captures the cumulative impact of the SES gaps in returns shown in Figure 7d.

Since the identification of the return to the firm-specific experience comes from workers who currently are employed in the same firm but have earlier experience from different firm classes, the return component does not capture returns that may crystalize only from moving to other, better-paying firms. Thus, the contribution of returns to firm classes in column 5 of Table 3 can be seen as a lower bound of the contribution of firm-specific returns. Indeed, Fig-

Figure 7. Work in high- vs- low-return firms

(a) Returns in different firm classes *(b)* Probability to work in high-return firms

(c) Probability to work in low-return firms *(d)* Return to firm-specific experience



Notes: Sub-figure (a) shows estimates of the coefficients on firm-class experience from equation (3). Sub-figures (b) and (c) show the probability to work in the firm class with the highest returns (firm types 1 or 2) and lowest returns (firm types 9 or 10), by quartile of father's income. Sub-figure (d) shows the average firm-specific returns to experience, by quartile of father's income.

ure A12b in Appendix A.8 shows that there is indeed a positive relationship between the returns to firm experience γ_m (as captured by column 5) and the change in firm premia (column 4), indicating that firm-specific returns to experience could also explain why children from high-income parents climb the firm ladder faster. Taken together, this dynamic view of firm pay suggests that firms play an even larger role for prime-age intergenerational earnings persistence than the static decomposition in Table 2. Adding columns 3-5 suggests that firms can explain at least 35% of the IGE (or 38% net of column 6).

One worry is that some of these dynamics are driven by the fact that children from high-income families always have higher returns to experience and that they are sorted into particular types of firms. The differences in returns across firm classes as shown in Figure 7a might therefore reflect heterogeneity across individuals rather than firms. Arellano-Bover and Saltiel (2021) test for this concern by interacting firm-specific experience with worker-fixed effects. We

Table 4. *Decomposition of the intergenerational earnings elasticity at age 40*

	Dependent variable					
	y_{ijt}	$\hat{\alpha}_i$	$\hat{\psi}_{j=J(i,t)}$ at age 25	$\Delta\hat{\psi}_{j=J(i,t)}$ age 25-30	Returns to firm experi-ence	$X_{it}\hat{\beta}+\hat{\varepsilon}_{ijt}$
	(1)	(2)	(3)	(4)	(5)	(6)
$y_{f(i)}$	0.178***	0.099***	0.029***	0.021***	0.012***	0.015***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
Share of IGE	100%	56%	16%	12%	7%	8%
Worker obs.	284,318	284,318	284,318	284,318	284,318	284,318

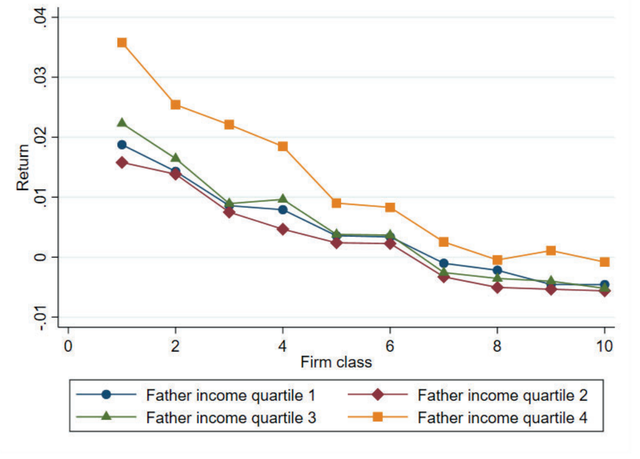
Notes: Column (1) reports the estimated slope coefficient from a regression of log child earnings at ages 40 on log father's earnings. Columns (2)-(6) report the slope coefficients from the corresponding regressions when decomposing child log earnings y_{ijt} according to equation (3) into individual fixed effects α_i, firm fixed effects, where the firm fixed effect is divided into firm fixed effect at age 25 and the change in firm fixed effects between age 25 and 40, return to firm-specific experience and time-varying controls. The sample differs from the main sample since (3) is estimated using workers born between 1967-1977, to be able to observe the workers' whole career, and half of the sample is used to cluster firms into firm classes and half of the sample us used to estimate (3). The sample in the table is restricted to individuals who have an observed firm at age 25. Robust standard errors in parentheses.

follow a similar approach and allow the returns to experience to vary with the father's earnings quartile to estimate the regression:

$$y_{ijt} = \alpha_i + \psi_j + \sum_{m=1}^{K} \gamma_m Exp(m)_{it} + \sum_{m=1}^{K} \delta_m Exp(m)_{it} * \theta_i + X_{it}\beta + \varepsilon_{ijt}, \quad (4)$$

where θ_i is a vector of dummy variables indicating the quartile of the income of the father. Thus, we allow the returns to experience to vary depending on the father's income. Indeed, Figure 8 shows that children from high-income families have higher returns to experience *within* all firm types. However, all children independent of family background benefit from working in firms with higher returns, and the variation in returns across firm classes is nearly as large as in our estimates based on equation (3) that did not allow for returns to vary with the father's earnings quartile (cf. Figure 7a). In fact, for children from the top quartile of father's earnings, returns vary *more* when allowing for variation by family background.

Figure 8. Returns in different firm classes by father's income



Notes: The figure shows estimates of the coefficients on firm-class experience from equation 4, by quartile of father's income.

4 The role of sorting across firms by skill

We found that some of the SES gradient in firm pay is due to assortative matching between firms and workers. Children from high-income families tend to be more educated and productive (according to the permanent worker component), and prior research shows that workers with higher permanent productivity (or skills) tend to sort into firms that pay higher premia (e.g., Card et al., 2013). But as we also showed, two thirds of the SES gradient in firm pay remains when controlling for the individual fixed effects from the AKM regression (see Table 3, column 4).[16] In the remainder, we refer to this specification as the "conditional firm pay gradient".

There are a few potential interpretations as to why much of the firm pay gradient remains, even conditional on this proxy for individual skills. First and foremost, parental income could indeed have a direct effect on firm sorting beyond what is mechanically driven by skill advantages among their children. Such direct effects could arise due to multiple sources, including informational advantages, social and co-worker networks, and credit or other constraints.[17] They could also arise if preferences for non-monetary amenities differ across families of different income levels, such that part of the gradient in firm pay

[16]Note that we condition on the *estimated* individual fixed effect from the main AKM regression. Thus, we do not include a new set of individual fixed effects in the the regression of estimated firm pay premia on (log) parental income, which would obviously be collinear with parental income.

[17]For example, credit constraints in early age could force poorer children into safe but low-paying jobs (see Staiger, 2022).

reflects compensating differentials or other non-pay attributes of firms (see Section 5).

However, a second possibility is that the estimated worker effects $\hat{\alpha}_i$ that we condition on are incomplete measures of skill, and therefore do not capture the full extent of skill sorting. First, the estimates $\hat{\alpha}_i$ are only a noisy measure of the α_i component in the AKM model, and such measurement error will bias the estimated contribution of assortative matching to the firm pay gradient (as shown formally by Dobbin and Zohar, 2023). Second, the individual fixed effects α_i capture *all* persistent within-firm differences in earnings, not just those that are due to differences in productivity. For example, persistent taste-based discrimination between ethnic groups would here be falsely interpreted as skill differentials, contributing to skill sorting.[18] A third but related possibility relates to the multidimensionality of skills. One may surmise that worker-firm sorting is on specific dimensions of skill (e.g., cognitive skills) rather than the entire bundle of skills that contributes to the individual fixed effects in log earnings – and parental background might be more or less strongly associated with those specific sorting dimensions than with other dimensions of skill.

To analyze skill sorting more thoroughly, we consider data on cognitive and non-cognitive skills from military enlistment tests, which we use in addition to our fixed effects-based measures.[19] Specifically, we use a decomposition similar to Gelbach (2016) and Hjorth-Trolle and Landersø (2023) to study sorting on (i) cognitive skills, (ii) social skills, (iii) education and (iv) the estimated individual fixed effect from the AKM model. As the enlistment test was only mandatory for males, we restrict the analyses in this section to males.[20] The

[18]Moreover, Dobbin and Zohar (2023) note that the AKM worker effects α_i may reflect "social capital", if parents help their children not only to secure a job in better-paying firms, but also to be promoted to better-paying jobs within those firms. While plausible, Stinson and Wignall (2018), San (2022) and Staiger (2022) find that most of the gains from parental networks come from working at a high-wage firm rather than from wage advantages within the firm. And in principle, parental connections might even have a negative effect on wages. For example, Bello and Morchio (2022) predict that "occupational followers" who choose their father's occupation earn lower wages, due to skill mismatch. To address these potential limitations in the interpretability of the AKM fixed effects α_i, we consider here more direct measures of skill.

[19]Dobbin and Zohar (2023) implement two alternative approaches to study the role of assortative matching. First, they develop an instrumental variable approach that uses the child's education as an instrument for their worker fixed effect α_i, which under plausible assumptions provides an upper bound for the contribution of assortative matching to the firm pay gradient. Second, they use education and demographic group as observable proxies for human and social capital, which under alternative assumptions also provides an upper bound for the contribution of assortative matching.

[20]The military tests are taken at around age 18 and were compulsory for all men in the cohorts that we study. The overall cognitive skill score represents an aggregated score from four subtests that measure verbal, logical, spatial and technical skills. The non-cognitive/social test score is based on a half-hour semi-structured interview with a certified psychologist who grades the enlistee along dimensions such as leadership, social maturity, and emotional stability, but also in an overall sense (for further details, see e.g. Lindqvist and Vestman, 2011). We standardize

skill measures from this test are highly informative about labor productivity, as demonstrated by their strong associations with wages and other long-term labor-market outcomes (Lindqvist and Vestman, 2011; Nybom, 2017). This allows us to directly test the hypothesis that the AKM fixed effects that are typically conditioned on are incomplete measures of skill, such that sorting is underestimated. Moreover, we can compare the relevance of different dimensions of skills (cognitive, social, etc).

The decomposition, summarized by the regression equations (5a)-(7), parses out how much of the relationship between children's firm premia and parents' log income can be attributed to various factors influencing child log income. Having estimated the firm pay gradient β_{firm} in (5a), we then augment this regression with our four mediators of interest: cognitive skill, social skill, education, and the AKM individual fixed effect.

$$\hat{\psi}_{j(it)} = \mu_{\psi} + \beta_{firm} y_{f(i)} + \omega_i \tag{5a}$$

$$\hat{\psi}_{j(it)} = \mu_{\psi,res} + \beta_{firm,res} y_{f(i)} + \beta_{cog} cog_i + \beta_{soc} social_i + \beta_{edu} edu_i + \beta_{akm} \hat{\alpha}_i + \upsilon_i \tag{5b}$$

The coefficient $\beta_{firm,res}$ in this augmented regression (5b) captures the "direct" effect of family background not mediated by skills, while $\beta_{firm} - \beta_{firm,res}$ captures the part explained by the mediators. We then run a set of auxiliary regressions (6a)-(6d) to pin down how closely related each of the mediators are to parental income,

$$cog_i = \mu_{cog} + \phi_{cog} y_{f(i)} + \varepsilon_{1i} \tag{6a}$$

$$social_i = \mu_{soc} + \phi_{soc} y_{f(i)} + \varepsilon_{2i} \tag{6b}$$

$$edu_i = \mu_{edu} + \phi_{edu} y_{f(i)} + \varepsilon_{3i} \tag{6c}$$

$$\hat{\alpha}_i = \mu_{akm} + \phi_{akm} y_{f(i)} + \varepsilon_{4i} \tag{6d}$$

The contribution of the respective mediators to β_{firm} then together sum up to $\beta_{firm} - \beta_{res}$,

$$\beta_{firm} - \beta_{firm,res} = \beta_{cog}\phi_{cog} + \beta_{soc}\phi_{soc} + \beta_{edu}\phi_{edu} + \beta_{akm}\phi_{akm} \tag{7}$$

Table 5 presents the results from this decomposition. We again focus on firm premia observed at age 40. Column (1) replicates the baseline estimate of the relationship between firm pay and (log) parental income, β_{firm}, for the sample of males with observed skill measures. The estimate for this sample of enlisted males (0.056) is very similar to the ones for all males (0.058, see Table A6) and the full population (0.054, see Table 3). Column (2) shows that nearly half of the SES gradient in column (1) is due to sorting on our various skill measures. On the flip side, about 52% of the estimated β_{firm} cannot

both the overall cognitive and non-cognitive scores to mean zero and standard deviation one, separately for each birth year.

Table 5. *Skill vs. SES gradient in firm pay*

	Firm pay premia					
	Overall	Unexp.	Cognitive	Social	Education	Indiv. FE
	β_{firm}	β_{res}	$\beta_{cog}\phi_{cog}$	$\beta_{soc}\phi_{soc}$	$\beta_{edu}\phi_{edu}$	$\beta_{iFE}\phi_{iFE}$
	0.056***	0.029***	0.011***	0.001***	0.004***	0.012***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Share	100%	51.8%	19.6%	1.8%	7.1%	21.4%
Obs.	371,411	371,411	371,411	371,411	371,411	371,411

Notes: The table reports estimates from the decomposition outlined by equations (5a)-(7) using the main sample, but excluding women and males with missing enlistment scores. Standard errors obtained by 250 bootstraps.

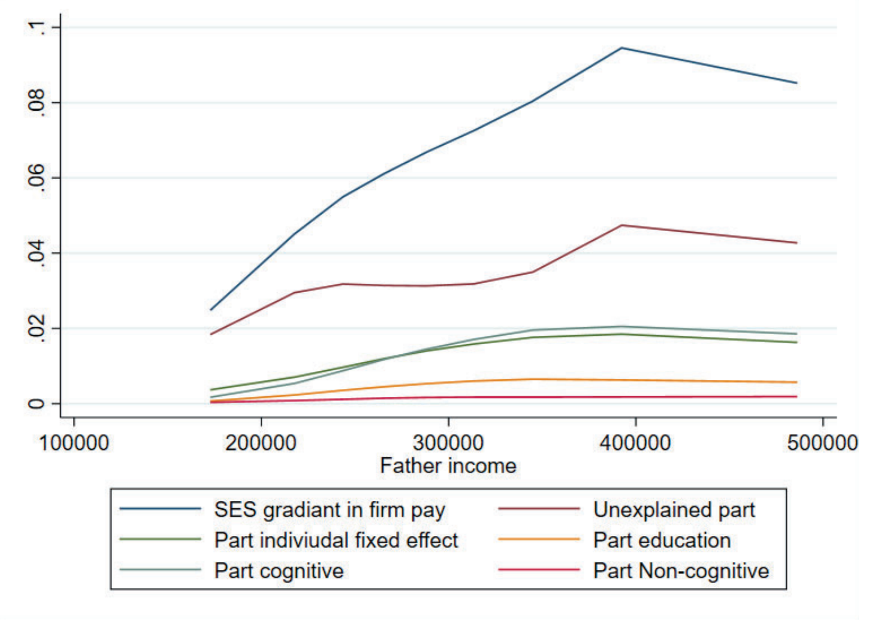
be explained by skills, suggesting that parental background has a substantial direct effect on firm sorting beyond what is mechanically driven by gaps in the skills of children. Further, columns (3)-(6) show the contribution of each of the respective skill measures to overall sorting (i.e. the components $\beta\phi$ from equation (7)). The individual fixed effects and cognitive skills are both key in explaining why children from richer families work at better-paying firms, with each contributing about 20% to the firm pay gradient β_{firm}. While the education and social/non-cognitive skill components are substantially smaller, they are not inconsequential and together contribute nearly 10%.[21]

Table 5 confirms that the role of skill sorting is underestimated when approximating skill solely by the estimated fixed effects from an AKM model (as conjectured by Dobbin and Zohar, 2023). In Section 2, we reported a conditional firm pay gradient of roughly 0.037, corresponding to an unexplained part of 68.5% (0.037/0.054), increasing to 70.7% when considering males (0.041/0.058, see Appendix Table A6). But when adding our full set of skill measures, the unexplained part drops to 51.8% (0.029/0.056). Cognitive skills are particularly important, explaining 20% of the SES gradient in firm pay, even conditional on estimated individual fixed effects and other controls. However, our analysis also indicate that despite a rich set of skill measures, half of the parental-income gradient in firm pay cannot be explained by skills, thus suggesting that parental income plays a key role for firm sorting beyond what is "mechanically" driven by child skills.

[21] That education contributes much less to the firm pay gradient is interesting, given that it correlates *more* strongly with parental income than the other mediators (see Appendix Table A7). One possible interpretation is that the worker fixed effects from the AKM model capture differences in formal education better than differences in cognitive skills, but that the latter are an important determinant of worker-firm sorting.

Next, Figure 9 and 10 show the same type of decomposition but across the distribution of parental income and over child age, respectively. To analyze how skill sorting varies with parent income, we follow Hjorth-Trolle and Landersø (2023) and run local-linear regressions of equations (5a)-(7) at each decile of parental income.[22] Figure 9 again demonstrate that the firm gradient generally increases in strength along the distribution, reaching almost 0.1 at high levels of parental income. Moreover, the unexplained part not attributable to sorting grows in absolute size along the distribution but decreases as a share of $\beta_{premium}$. The cognitive skill measure and the individual fixed effect grow steadily in importance, and higher up in the distribution they are important mediators for the raw relationship between firm premia and parental income.

Figure 9. Local linear decompositon of skills along the parent income distribution



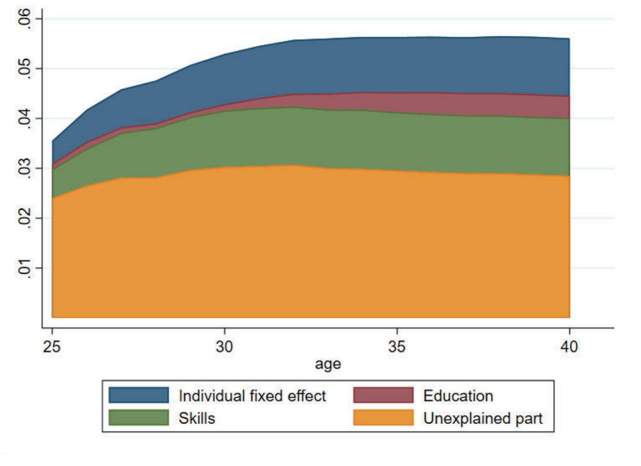
Notes: The figure shows local linear regressions, from the decomposition outlined by equations (5a)-(7) using the main sample, but excluding women and males with missing enlistment scores. The local linear regression estimates a linear regression around the income mean at each decile of parental income, using a bandwidth of 50 000 Swedish kronor and an epan kernel.

Figure 10 shows the decomposition separately for each age over the lifecycle. The part of the firm pay gradient explained by skill sorting, $\beta_{firm} - \beta_{firm,res}$, increases substantially over age. In contrast, the direct effect of family background not mediated by skills, $\beta_{firm,res}$, is already large at age 25 and

[22]The local linear regression estimates a linear regression around the income mean at each decile of parental income, using a bandwidth of 50 000 Swedish kronor and an epan kernel.

grows only slightly in size over age. Its relative contribution to the overall firm gradient β_{firm} decreases substantially: nearly 70% of the firm pay gradient at age 25 is due to direct family effects, falling to just 50% at age 40. The finding that family background effects unrelated to skill play a relatively more important role in the early career is intuitive, as children are then likely more closely linked to their parents and parental networks and contacts might be more useful. As children age, skills become more important for firm sorting, contributing further to the overall influence of family background on firm sorting.

Figure 10. Skill vs. SES gradient in firm pay by age



Notes: The figure shows age specific estimates from the decomposition outlined by equations (5a)-(7) using the main sample, but excluding women and males with missing enlistment scores.The different skill part sum up to the SES gradient in firm premia.

5 Do firm pay reflect compensating differentials?

Pay is not the only firm attribute that matters to workers, and other aspects of the firm – such as its location, average workloads or fringe benefits – also vary across firms. It is therefore not necessarily the case that high-paying firms are more desirable firms. As these other aspects may also co-vary with parental background, the SES gradient in firm pay may under- or overstate the role of firms in the intergenerational transmission of advantages. To fix ideas, decompose the firm pay premium as

$$\psi_j = r_j - \kappa a_j \tag{8}$$

where the pay premium of firm j is the sum of a firm-specific worker rent r_j and an amenity component, κa_j. We think of r_j as arising when a firm has some monetary rents to share with its workers, e.g. due to frictions and/or other forms of imperfect competition. The amenity a_j can be either positive, if the firm offers attractive non-pay attributes (e.g. temporal flexibility), or negative, if the firm offers bad non-pay attributes (e.g. poor work environment). Together with the non-negative amenity price $\kappa > 0$, which depends on the marginal worker's preferences, the amenity component, κa_j, thus constitutes a firm-specific compensating differential. We thus assume that a_j is perfectly priced and paid for on the market, and thus will be uncorrelated with worker utility. Consequently, if high-premium firms are overall more desirable it points to the importance of rents, while a weaker association between firm pay premia and the actual attractiveness of firms is evidence of compensating differentials.

Whether the SES gradient in firm pay is primarily due to rents or amenity compensation is crucial for its interpretation, and for the interpreation of intergenerational mobility estimates more generally. Perhaps the SES gradient arises because high-income families place a stronger value on consumption and/or are less averse to bad working conditions. In this scenario, children from high-income families end up in better-paying firms, but those firms are actually worse in other dimensions: measures of intergenerational income persistence would then overstate the extent to which levels of welfare persist across generations. Alternatively, high-income families are better equipped with contacts and networks, information, or other resources, and therefore end up in firms that pay their workers more, conditional on the level of amenities they offer. In this scenario, and if the SES gradient in amenity compensation is small, the gradient in firm pay would approximate the corresponding gradient in welfare. Or maybe firms with high pay tend to be *better* in other dimensions, too, and those non-monetary attributes of firms or jobs are generally better in high-income families. Indeed, given their more favorable financial position, children from high-income families might systematically select into firms that have worse pay but better non-pay attributes, all else equal. In this scenario, intergenerational mobility in underlying welfare would be even lower than income-based estimates suggest.

The decomposition above assumes that amenities are always fully priced into pay, which might not be an accurate description of the world. We can therefore consider an extended decomposition:

$$V_j = \psi_j + \kappa a_j + \kappa b_j = r_j + \kappa b_j \tag{9}$$

where we now focus on the overall value of a firm j, denoted V_j, which depends on the rent-part of the firm pay premium and a second component b_j capturing non-pay characteristics of the firm that are not priced into the worker's pay. Note that if a_j is correctly priced then this part of the pay premium has no influence on the value (or utility) of working for a firm. Thus, the overall value of the firm depends potentially on rents and amenities that are "free of charge" (or at least imperfectly priced).

With some way of inferring the overall value or attractiveness of firms (the V_j), we can address a couple of key questions. First, we can explore whether high-paying firms in general also are more desirable firms. The extent to which higher-paying firms are more desirable firms can then be seen as evidence of rents, while the extent to which this is not the case is evidence of compensating differentials (Sorkin, 2018). In the extreme case, if variation in firm pay is solely due to compensating differentials, there is no relation between the value of firms and their pay premia. Second, we can study whether there is an SES gradient in firm values in a similar fashion as we did for firm pay. By further conditioning on α_i we can infer to what extent the SES gradients in firm value arise from skill sorting or not. Finally, under assumptions of $Corr(a_j, b_j)$, we can estimate SES gradients in firm value conditional on the firm pay premium and infer whether b_j is systematically related to SES.

To explore the different sources to the SES gradient in firm pay, the overall value of firms, and the role of compensating differentials, we use various alternative analyses that all involve using some proxy measure of V_j. Many of the analyses exploit worker transitions between firms, and it thus becomes crucial to distinguish voluntary from involuntary employer-to-employer transitions. Our main strategy here is to focus on voluntary moves, which we define as transitions without any intermediate period of unemployment.[23] First, we explore how parental income relates to alternative measures of a firm's attractiveness that also capture non-pay characteristics, such as the firm's "poaching" and quit rates. We then employ a revealed-preferences based approach similar to Sorkin (2018), which infers the overall values of firms (V_j) from worker transitions between firms.

5.1 Alternative measures of the attractiveness of firms

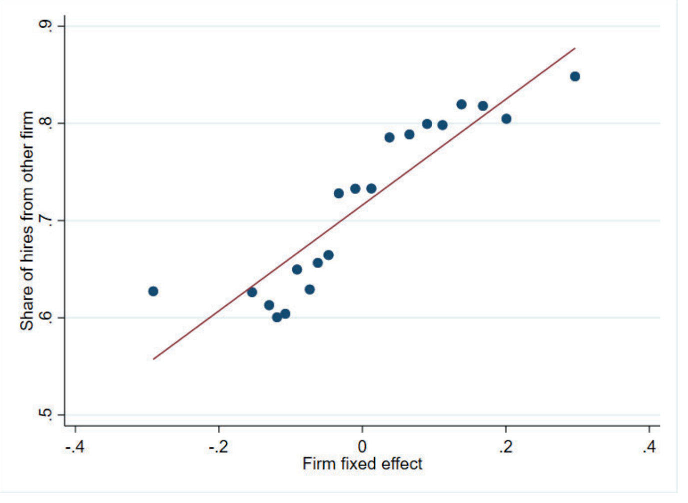
Are high-paying firms indeed more desirable firms? Figure 11 shows that new hires in high-paying firms mostly arrive from employment in other firms, i.e. the new employees have been "poached" from other firms. In contrast, low-paying firms often hire individuals from non-employment, who are less likely to have strong outside options at the time of their hire. This pattern is consistent with the "job ladder" from standard search models (Burdett and Mortensen, 1998), and indicates that high-paying firms are indeed more attractive from the perspective of workers.[24] Given our simple decomposition

[23] We identify unemployment periods using data on UI benefit receipts, and include in our analyses only employer-to-employer transitions associated with zero received benefits.

[24] However, while this relationship may hold on average, it does not necessarily follow that it also holds for the SES-gradient in firm pay (i.e., the way children from high-SES background select into firms may differ from the average relations observed in the labor market). In the next section, we quantify the attractiveness of each firm, to then study this question in more details.

above, the result would be inconsistent with that most or all of the firm pay premium reflects compensating differentials (priced non-pay characteristics).

Figure 11. Poaching rate from other firms



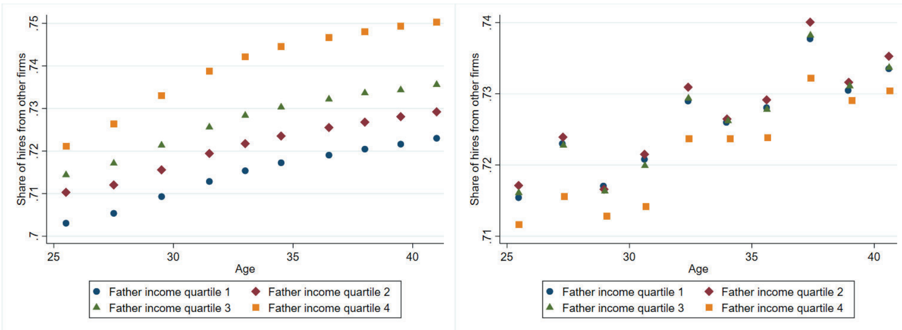
Notes: Binned scatterplot of the share of hires from employment ("poaching rate") on the firm fixed effects estimated based on the AKM model in equation (1).

In Figure 12a, we study whether the firms' poaching rate varies systematically by parental background. Indeed, we see a similar pattern by parental income in the poaching rate as we do in firm pay premia (see Figure 2a): the gaps open up already at early age, and widen further up to the mid 30s. However, Figure 12b shows that *conditional* on firm pay, high-SES children end up in firms with slightly *lower* poaching rates. This might indicate that those firms are not as desirable as they seem in terms of firm pay, although the gaps in the conditional poaching rate in Figure 12b are much smaller than the unconditional gaps in Figure 12a. Overall, we find that high-SES children do sort into more attractive firms (as proxied by firm poaching rates).

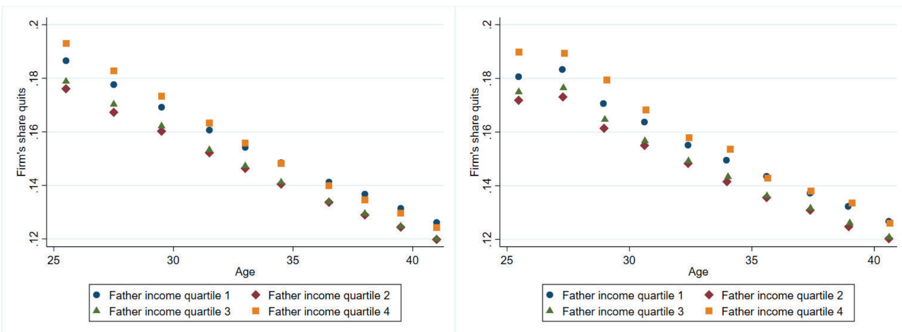
In Figures 12c and 12d we show the corresponding pattern in the *quit rates* of firms. Again, the idea is that firms that for monetary or non-monetary reasons are attractive employers will see fewer quits among their existing workforce, as it is harder for other firms to poach workers from these firms. The evidence here is more mixed: early in their careers, children from the bottom but also from the top SES quartile work in firms that have slightly *higher* quit rates. The quit rates generally fall with age, and so do the gaps between SES groups. The patterns are also less sensitive to the inclusion of firm fixed effects.

Figure 12. Poaching and quit rates

(a) Poaching rate from other firms *(b)* Poaching rate, controlling for firm FE



(c) Quit rate *(d)* Quit rate, controlling for firm FE



Notes: The figure shows "poaching" and quit rates over the lifecycle by father's income quartile. Figure (a) shows the mean poaching rate, defined as the share of hires from employment, figure (b) shows the same poaching rate but controlling for the estimated firm premium. Figure (c) shows the quit rate at the firm and figure (d) shows quit rates controlling for the estimated firm premium.

5.2 Inferring SES gradients in firm values using revealed preferences

While the previous analyses of poaching and quit rates provided insights, a concern is that these measures are imperfect proxies of the overall values of firms. We therefore employ a more comprehensive way of inferring firm values, or V_j from above, using the revealed-preference measure of firm values based on worker flows across firms suggested by Sorkin (2018). One can think of Sorkin's approach as the Google PageRank algorithm, but for firms. In short, the idea is that if workers voluntarily move from one firm to another it must imply that the value of the destination firm is higher. We define voluntary employer-to-employer transitions as transitions where workers receive no unemployment benefits or have a year of zero earnings, in between adjacent employment spells associated with different firms. We thus retrieve a value for each firm, which could consist of both rents and (non-priced) amenities. While showing that children from high-SES families work in firms with higher pay premia is not necessarily proof of them being better off in a welfare sense, studying the same gradients in terms of firm values allows us to draw inference about welfare differences. Table 6 shows results for how firm values relate to father's log income.

Table 6. *Firm values and compensating differentials*

	Dependent variable				
	$\hat{\psi}_{j=J(i,t)}$ (1)	$\hat{V}_j$ (2)	$\hat{V}_j$ (3)	$\hat{V}_j$ (4)	$\hat{V}_j$ (5)
$y_{f(i)}$	0.063***	0.034***	0.016***	0.016***	0.006
	(0.001)	(0.004)	(0.004)	(0.004)	(0.004)
$\hat{\psi}_{j=J(i,t)}$				0.286***	0.233***
				(0.012)	(0.013)
$\hat{\alpha}_i$			0.148***		0.110***
			(0.007)		(0.007)
Observations	356,774	356,774	356,774	356,774	356,774

Notes: Column (1) reports the slope coefficient from regressing $\hat{\psi}_j$ from equation (1) on father's log income for the subsample of firms that are included in the model to estimate the firm values. Column (2) reports the slope coefficient from regressing the estimated firm value, $\hat{V}_j$, following Sorkin (2018), on father log income. Columns (3)-(5) show slope coefficient estimates from regressing firm values on father log income including different controls. Column (3) controls for $\hat{\alpha}_i$, column (4) controls for $\hat{\psi}_j$, and column (5) controls for $\hat{\psi}_j$ and $\hat{\alpha}_i$ simultaneously. Robust standard errors in parentheses.

Because we cannot retrieve estimates of V_j for all firms – they need to be a part of a more restrictive *strongly* connected set in terms of voluntary firm-to-firm transitions (see Sorkin, 2018) – the sample size is reduced considerably. In column 1, we thus re-estimate the SES gradient in firm pay premia, which is slightly larger in this more restricted sample than in our baseline (.063 vs. .057). In column 2, we then document that the SES gradient extends to the estimated firm value, and thus the overall desirability of the firms. Thus, it seems like sorting across employers actually make children from high-SES families better off. When controlling for the individual fixed effect (column 3), the SES gradient is substantially weakened but remains positive and significant. Thus, one reason why high-SES children are able to enter more desirable firms is that they have higher skills, which enable them to sort into higher value firms. It is notable that skill sorting appears more important for firm values than for firm pay premia, which is intuitive if people use their skills to maximize their overall welfare (rather than just their pay check).[25] However, even conditional on skills, high-SES children are considerably more likely to end up in higher value firms.

The firm value consists of both rents and non-priced amenities, and without further assumptions we cannot distinguish the relative roles of these components. The results are also silent about whether high-SES children experience positive or negative amenities — it only says that the value of the firm premium is larger than any potential negative amenities. Thus, and together with the fact that firm pay premia and firm values are positively correlated, there is evidence that not all of the variation in pay premia is due to compensating differentials – there is some room for rents.

In column 4, we show the SES gradient in firm value conditional on the firm premium, thus comparing children from different backgrounds that work in firms with similar pay premia. In doing so, the SES gradient in terms of firm value decreases in size but remains positive. Thus, when working in firms with similar pay premia, people from high-SES families are able to enter firms with relatively higher rents and non-priced amenities than priced (bad) amenities compared to low-SES children. However, without further assumptions we cannot know if this is since high-SES children earn higher rents or higher non-priced amenities given a certain firm premium. Obviously, we might assume away non-priced amenities, and define the firm value as a proportional function of firm-specific rents. In that case, the estimates in column 4 would suggest an SES gradient in rents, even conditional on firm pay premia. However, when we in addition control for the individual fixed effect (column 5), the SES gradient becomes insignificant, indicating that the main reason that high-SES children are able to enter high-value firms, given the firm premium, is skill sorting.

6 Conclusions

This paper examined the extent to which the sorting of workers across firms contributes to intergenerational earnings persistence. We build on the large literature on the drivers of intergenerational persistence. While the literature has traditionally focused on childhood development and inequalities in parental investments in their children's human capital, we add by providing a labor-market perspective. In particular, we use Swedish administrative data and decompose earnings into permanent individual components (approximating productivity) and firm-specific pay premia a la Abowd et al. (1999) and many others in their footsteps. We then add data enabling us to link parents to children, and provide a multitude of evidence on the SES gradient in the firm

[25]Controlling for the individual fixed effect diminishes the SES gradient in firm value by 53% but the firm pay premium "only" by 31% (see Table 2).

portion of pay, how it evolves over the lifecycle, its underlying drivers, and how the gradient ought to be interpreted.

Our findings indicate that disparities in firm pay premia can account for a significant portion of the intergenerational elasticity of income in Sweden. This suggests that the advantages or disadvantages associated with people's family backgrounds can have lasting impacts on their career trajectories and long-run outcomes in life. The emergence of SES gaps in firm pay already at the outset of one's career implies that individuals from more privileged backgrounds have access to more favorable entry points into the labor market. These advantages are compounded by the fact that they are able to climb the firm pay ladder faster, frequently switching employers and securing higher pay gains conditional on such changes. While skill sorting – the fact that high-SES children tend to have higher skills, and highly skilled people sort into better firms – accounts for a sizable portion of the widening of these pay gaps, a large share of the SES gradient in firm pay remains also conditional on a very detailed set of controls for skill. Furthermore, our results remain robust even after accounting for compensating differentials and alternative measures of firm quality. Thus, high-SES children sort into firms that not only deliver larger pay checks, but ultimately also higher overall welfare.

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Appendix A: Additional figures and tables

A.1 Variance decomposition of the AKM model

Table A1. *Variance decomposition*

	AKM sample	Main sample (born 1967-77)	
Age	20-64	25-41	39-41
	(1)	(2)	(3)
Variance of log earnings	0.273	0.248	0.208
Components:			
Individual FEs	0.103 (37.7%)	0.073 (29.4%)	0.074(35.6%)
Firm FEs	0.020 (7.3%)	0.023 (9.3%)	0.023(11.1%)
Covariance (sorting)	0.019 (7.0%)	0.021 (8.5%)	0.023 (11.1)
Covariates and residual	0.131 (48.0%)	0.129 (52.0%)	0.088(42.3%)
Worker obs.	7,668,377	967,417	857,064
Number of firms	341,798	228,285	118,258
Worker-year obs.	126,475,937	13,550,074	2,437,567

Notes: The table shows a variance decomposition of log earnings into the of components of equation (1). Column (1) show the variance decomposition for the AKM sample, column (2) shows the result for the main lifecycle sample and column (3) shows the result for the main sample with mean earnings estimated for the ages 39-41.

Using our estimates from equation (1), we can decompose the variance in income as

$$\begin{aligned} Var(y_{ijt}) &= Var(\alpha_i) + Var(\psi_j) + 2Cov(\alpha_i, \psi_j) + Var(X_{it}\delta) \\ &\quad +2Cov(X_{it}\delta, \alpha_i + \psi_j) + Var(\varepsilon_{ijt}) \end{aligned} \tag{A1}$$

We report the results in Appendix Table A1, separately for three samples: our AKM sample, our main intergenerational sample across the entire age span (ages 25-41), and our main sample at age 39-41. The first two terms on the right-hand side in equation (A1) describe what fraction of the overall earnings variance is due to individual and firm components, respectively. The third component measures the contribution of worker-firm sorting; if this covariance is positive, there is positive assortative matching in the sense that workers with high (permanent) unobserved productivity sort into firms with high pay premia. The last three terms capture earnings variation due to covariates and the error term.

As found by others, the most important component for explaining the variance of log earnings is the (variance of) worker effects, here at 29-38% across the three samples. On the other hand, firm fixed effects and the covariance between firm and worker fixed effects together explain 14-22% of the total variance. When we compare the samples, we see that the variance decomposition is largely stable across samples. However, for the main sample observed over the lifecycle (column 2) we find a somewhat decreased importance of the individual component, compared to the full AKM sample (column 1). For the prime-age version of the main sample (column 3), which only includes incomes at ages 39-41, there is a slight uptick in the importance of firms and sorting (rows 2 and 3) compared to the baseline. Overall, the decomposition is very similar to Engbom et al. (2023) who use similar data and specifications, and also largely in line with evidence from the US (e.g. Song et al., 2019).

A.2 Decomposition of the IGE

Measurement error

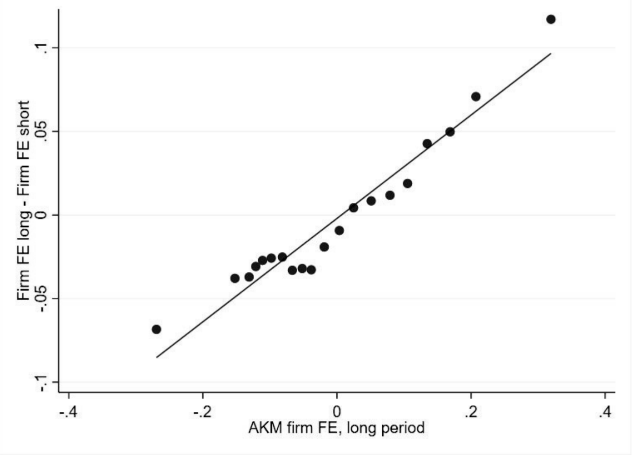
The table shows the IGE decomposition when the AKM model have been estimated for shorter time period, for the years 2010-2015.

Table A2. *Decomposition of the IGE, AKM estimated for the years 2010-2015*

	Dependent variable			
	y_{ijt}	$\hat{\alpha}_i$	$\hat{\psi}_{j=J(i,t)}$	$\hat{\psi}_{j=J(i,t)}$
	(1)	(2)	(3)	(4)
Panel A: AKM estimated for the years 2010-2015				
$y_{f(i)}$	0.197***	0.165***	0.031***	0.021***
	(0.001)	(0.001)	(0.000)	(0.000)
$\hat{\alpha}_i$				0.059***
				(0.000)
Share of IGE	1.0	0.84	0.16	0.11
Worker obs.	784,259	784,259	784,259	784,259
Panel B: AKM estimated for 1985-2018 (intergenerational sample same as in Panel A)				
$y_{f(i)}$	0.197***	0.117***	0.051***	0.033***
	(0.001)	(0.001)	(0.000)	(0.000)
$\hat{\alpha}_i$				0.154***
				(0.001)
Share of IGE	1.0	0.59	0.26	0.17
Worker obs.	784,259	784,259	784,259	784,259

Notes: Column (1) reports the estimated slope coefficient from regression (2) of mean of log child earnings over the ages 39-41 on log father's earnings. Columns (2)-(4) report the slope coefficients from the corresponding regressions when decomposing child log earnings y_{ijt} according to equation (1) into individual fixed effects α_i and mean firm fixed effects ψ_j over the ages 39-41. The columns for time-varying control are not included since we have imputed firm values for the ages 39-41, even if these ages are outside of the 2010-2015 window if the firm existed for years 2010-2015, and thus we miss data on the time-varying controls for those observations. In Panel A, equation (1) is estimated for the years 2010-2015. In Panel B, equation (1) is estimated for the full time-period 1985-2018, but the observations in the intergenerational regression are limited to the same as in Panel A. Robust standard errors in parentheses.

Figure A1. Difference AKM 2010-2015 and long AKM



Notes: The figure shows that difference in the estimated firm premia between the AKM estimated for the long period 1985-2018, and the AKM estimated for the short period 2010-2015 against the firm premia from the AKM estimated for the long period.

Table A3. *Decomposition of IGE, random sub-sample*

	Dependent variable			
	y_{ijt} (1)	$\hat{\alpha}_i$ (2)	$\hat{\psi}_{j=J(i,t)}$ (3)	$\hat{\psi}_{j=J(i,t)}$ (4)
A: Dropping 50% of individuals before AKM estimation				
$y_{f(i)}$	0.203***	0.120***	0.053***	0.036***
	(0.002)	(0.001)	(0.001)	(0.001)
$\hat{\alpha}_i$				0.145***
				(0.001)
Share of IGE	1.0	0.59	0.26	0.18
Worker obs.	418,980	418,980	418,980	418,980
B: AKM estimated for full sample (intergenerational sample same as in panel A)				
$y_{f(i)}$	0.203***	0.120***	0.053***	0.035***
	(0.002)	(0.001)	(0.001)	(0.000)
$\hat{\alpha}_i$				0.152***
				(0.001)
Share of IGE	1.0	0.59	0.26	0.17
Worker obs	418,980	418,980	418,980	418,980

Notes: Column (1) reports the estimated slope coefficient from regression (2) of mean of log child earnings over the ages 39-41 on log father's earnings. Columns (2)-(4) report the slope coefficients from the corresponding regressions when decomposing child log earnings y_{ijt} according to equation (1) into individual fixed effects α_i, mean firm fixed effects ψ_j over the ages 39-41, and time-varying controls. In panel A equation (1) is estimated after dropping a random subsample of 50% of all individuals. In panel B equation (1) is estimated for the full sample, but the observations in the intergenerational regression are limited to the same as in panel A. Robust standard errors in parentheses.

Alternative estimation of the AKM regression

Table A4. *Decomposition of the IGE: Alternative estimations of the AKM equation*

	Dependent variable				
	y_{ijt}	$\hat{\alpha}_i$	$\hat{\psi}_{j=J(i,t)}$	$X_{it}\hat{\beta}+\hat{\varepsilon}_{ijt}$	$\hat{\psi}_{j=J(i,t)}\|\hat{\alpha}_i$
	(1)	(2)	(3)	(4)	(5)
	A. AKM estimated with time-varying firm FE				
$y_{f(i)}$	0.200***	0.125***	0.057***	0.018***	0.040***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)
Share of IGE	1.0	0.63	0.29	0.09	0.20
Worker obs.	847,447	847,447	847,447	847,447	847,447
	B. AKM estimated using establishment codes				
$y_{f(i)}$	0.198***	0.105***	0.072***	0.022***	0.056***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)
Share of IGE	1.0	0.53	0.36	0.11	0.28
Worker obs.	831,927	831,927	831,927	831,927	831,927
C. AKM estimated using establishment codes for large firms					$y_{f(i)}$
0.198***	0.107***	0.069***	0.023***	0.052***	
	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)
Share of IGE	1.0	0.54	0.35	0.12	0.26
Worker obs.	836,554	836,554	836,554	836,554	836,554
D. AKM estimated without excluding firms with few movers					
$y_{f(i)}$	0.199***	0.115***	0.058***	0.027***	0.043***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)
Share of IGE	1.0	0.58	0.29	0.14	0.22
Worker obs.	904,384	904,384	904,384	904,384	904,384

Notes: Column (1) reports the estimated slope coefficient from regression (2) of mean of log child earnings at ages 39-41 on log father's earnings. Columns (2)-(4) report the slope coefficients from the corresponding regressions when decomposing child earnings y_{ijt} according to equation (1) into individual fixed effects α_i, mean of firm fixed effects ψ_j for ages 39-41, and time-varying controls. The different panel shows different variants of estimating equation (1). In panel A we estimate time-varying firm fixed effects by dividing the period 1985-2018 into 4 periods, and allow the firm-fixed effects to vary between the periods. In panel B we use establishment codes instead of firm codes to estimate equation (1). In Panel C establishment codes are used for large firms and firm codes for small firms with 1,000 or fewer unique workers during the analysis period. In panel D we estimate the AKM without excluding firms with few movers.

Table A5. *Decomposition of the IGE: AKM estimated with wages*

	Dependent variable				
	y_{ijt}	$\hat{\alpha}_i$	$\hat{\psi}_{j=J(i,t)}$	$X_{it}\hat{\beta}+\hat{\varepsilon}_{ijt}$	$\hat{\psi}_{j=J(i,t)}$
	(1)	(2)	(3)	(4)	(5)
	A: AKM estimated with wages				
$y_{f(i)}$	0.175***	0.132***	0.022***	0.021***	0.008***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
$\hat{\alpha}_i$					0.105***
					(0.001)
Share of IGE	1.0	0.75	0.13	0.12	0.05
Worker obs.	565,231	565,231	565,231	565,231	565,231
	B: AKM estimated with earnings (excluding individuals not in wage sample)				
$y_{f(i)}$	0.194***	0.120***	0.044***	0.030***	0.025***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)
$\hat{\alpha}_i$					0.156***
					(0.001)
Share of IGE	1.0	0.62	0.23	0.15	0.13
Worker obs.	565,231	565,231	565,231	565,231	565,231

Notes: Column (1) reports the estimated slope coefficient from regression (2) of mean of log child wages at ages 39-41 on log father's earnings. Columns (2)-(4) report the slope coefficients from the corresponding regressions when decomposing child wage y_{ijt} according to equation (1) into individual fixed effects α_i, mean of firm fixed effects ψ_j for ages 39-41, and time-varying controls. In Panel A we estimate equations (1) and (2) using the wage structure sample, which covers roughly a third of private sector employees (with those in larger firms oversampled) and all public sector employees, in total corresponding to about 50% of the workforce. In Panel B we estimate equations (1) and (2) using earnings, but limiting the observations to the same observations as in the wage sample. Robust standard errors in parentheses.

Decomposition of the IGE: Heterogeneity

Table A6 shows the decomposition of the intergenerational earnings elasticity for different subsamples.

Table A6. *Decomposition of the IGE: Heterogeneity*

	Dependent variable				
	y_{ijt}	$\hat{\alpha}_i$	$\hat{\psi}_{j=J(i,t)}$	$X_{it}\hat{\beta}+\hat{\varepsilon}_{ijt}$	$\hat{\psi}_{j=J(i,t)}\|\hat{\alpha}_i$
	(1)	(2)	(3)	(4)	(5)
A. Sample: Men					
$y_{f(i)}$	0.230***	0.135***	0.058***	0.036***	0.041***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Share of IGE	1.0	0.59	0.25	0.16	0.18
Worker obs.	436,709	436,709	436,709	436,709	436,709
B. Sample: Women					
$y_{f(i)}$	0.169***	0.097***	0.050***	0.022***	0.040***
	(0.002)	(0.001)	(0.000)	(0.001)	(0.000)
Share of IGE	1.0	0.57	0.30	0.13	0.24
Worker obs.	420,355	420,355	420,355	420,355	420,355
C. Excluding workers who work in same firm as father					
$y_{f(i)}$	0.191***	0.112***	0.051***	0.028***	0.033***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)
Share of IGE	1.0	0.59	0.27	0.15	0.17
Worker obs.	748,293	748,293	748,293	748,293	748,293
D. Excluding public sector firms					
$y_{f(i)}$	0.228***	0.124***	0.068***	0.035***	0.051***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)
Share of IGE	1.0	0.54	0.30	0.15	0.22
Worker obs.	549,635	549,635	549,635	549,635	549,635
E. Excluding firms with less then 10 movers					
$y_{f(i)}$	0.200***	0.118***	0.052***	0.030***	0.034***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)
Share of IGE	1.0	0.59	0.26	0.15	0.17
Worker obs.	836,199	836,199	836,199	836,199	836,199
F. Excluding firms with less then 50 movers					
$y_{f(i)}$	0.201***	0.121***	0.047***	0.032***	0.028***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)
Share of IGE	1.0	0.60	0.23	0.16	0.14
Worker obs.	745,473	745,473	745,473	745,473	745,473

Notes: Column (1) reports the estimated slope coefficient from regression (2) of mean of log child earnings at ages 39-41 on log father's earnings. Columns (2)-(4) report the slope coefficients from the corresponding regressions when decomposing child earnings y_{ijt} according to equation (1) into individual fixed effects α_i, mean of firm fixed effects ψ_j for ages 39-41, and time-varying controls. Panel A shows results for males, and panel B shows results for women. In Panel C, workers who work in the same firm as their fathers are excluded, where working in the same firms as the father is defined as having ever worked in the father's main firm (main firm is the firm the father works in for most years between 1985-2018). Panel D shows results where public sector firms are excluded, where public sector firms are defined as firms in industries with SNI92 codes 75, 80, 85, 90, 91, 92, 93 and 99. Panel E excludes firms with less than 10 movers and Panel F excludes firms with less than 50 movers (baseline: at least five movers).

A.3 Skill sorting

In this Appendix we provide additional evidence on the decomposition of the SES gradient in firm pay into skill-based sorting ("assortative matching") and residual sorting. Table A7 reports estimates from the auxiliary regressions (6a)-(6d) of each skill measure on parental income (i..e, estimates of $\phi_{cog}, \phi_{soc}, \phi_{edu}$ and ϕ_{akm}). Note that the regression coefficients are not directly comparable, as they also reflect differences in the scaling of each variable. We therefore focus on the correlation coefficient, which is equal to the square root of the R-squared reported in the table. We find that parental income correlates most strongly with child education, while the correlation with the child's social skills is lowest. Despite correlating strongly with parental income, child education contributes only a small share to the firm pay gradient (see Table (5)). The comparatively low correlation for our proxy of social skills is possibly explained by measurement error, as social skill measures tend to be more noisy than measures of cognitive skills (Grönqvist et al., 2017).

Table A8 reports estimates regression (7), showing that the coefficients change only marginally when excluding parental income from the regression.

Table A7. *Skills and father's income*

	Dependent variable			
	Cognitive skills	Social skills	Education	$\hat{\alpha}_i$
$y_{f(i)}$	1.022***	0.714***	1.477***	0.129***
	(0.007)	(0.006)	(0.009)	(0.001)
Observations	371,411	371,411	371,411	371,411
R-squared	0.060	0.038	0.070	0.052

Notes: The figure shows result from the Gelbach decomposition, equations (6a)-(7), regressing each of the skills on father's income.

Table A8. *Firm pay premia and skills*

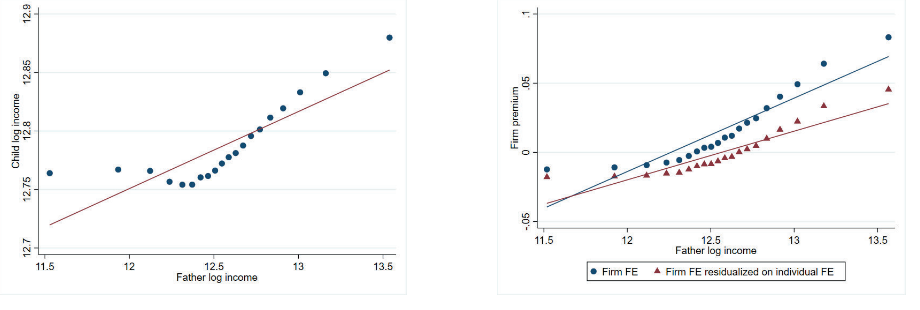
	Dependent variable	
	$\hat{\psi}_{j=J(i,t)}$	$\hat{\psi}_{j=J(i,t)}$
$y_{f(i)}$	0.029***	
	(0.001)	
Cognitive skills	0.010***	0.011***
	(0.000)	(0.000)
Social skills	0.002***	0.002***
	(0.000)	(0.000)
Education	0.003***	0.004***
	(0.000)	(0.000)
$\hat{\alpha}_i$	0.089***	0.096***
	(0.001)	(0.001)
Observations	371,411	371,411
R-squared	0.086	0.080

Notes: The figure shows result from the Gelbach decomposition, equation (5b), regressing the firm premia on father's income and each of the skills.

A.4 Non-linear firm pay gradients

Figure A2. Child income and firm premia by father's income (logs)

(a) Child vs father log income *(b)* Firm premium vs father's log income

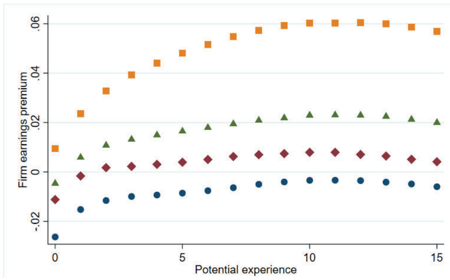


Notes: Figure (a) shows binned scatter plots of child's log income at age 40 by father's log income. Figure (b) shows firm fixed effects ψ_j at age 40 estimated by equation (1) and firm premia residualized on individual fixed effect by father log income.

A.5 Additional evidence on lifecycle dynamics

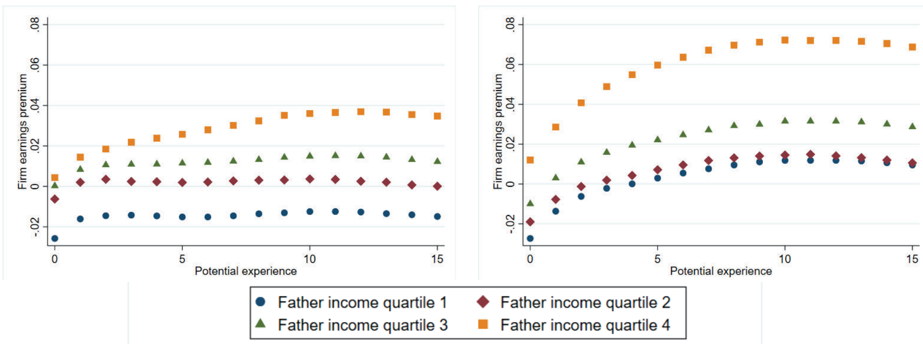
Figure A3. Firm earnings premium over the lifecycle using potential experience

(a) Full sample



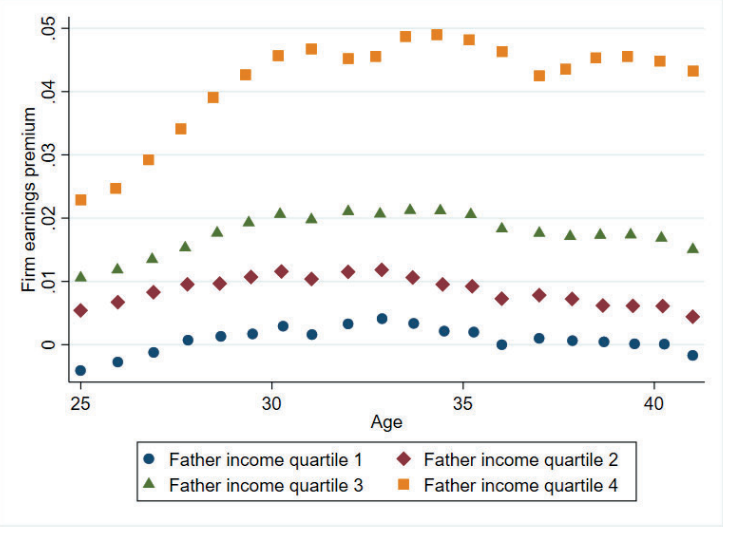
(b) Non-college

(c) College



Notes: The figures plots the mean estimated firm premium $\hat{\psi}_j$ against potential experience over the life cycle, by quartile of father's income. Potential experience is defined as the year minus the the year the individual enters the labor market. Entering the labor market is defined as the first year, after age 20 of having higher then low earnings (where low earning is defined as 20% of the median earnings of men aged 45), and after the age for potential finishing school (defined as age- (years of education age - education +6)). Sub-figure (a) shows the result for the full sample, sub-figure (b) shows the result for children without collage education and sub-figure (c) shows the result for children with college education. Father's income quartiles are defined in the full sample.

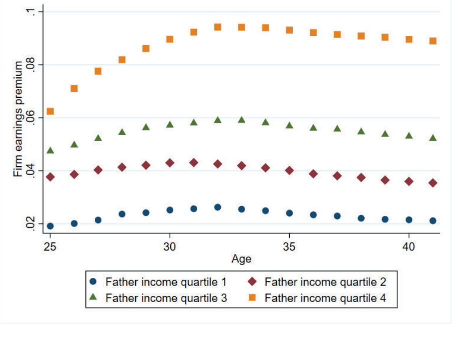
Figure A4. Firm pay premium over the lifecycle conditional on individual fixed effects



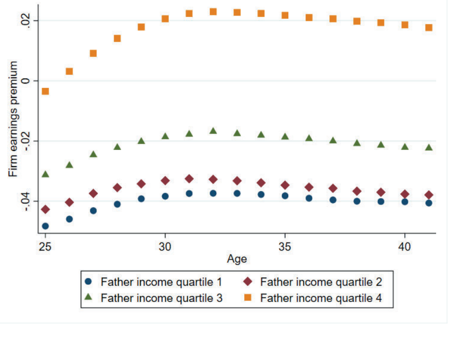
Notes: The figures show the estimated firm premium $\hat{\psi}_j$ from equation (1), residualized on the individual fixed effects, over the life cycle and by quartile of father's income.

Figure A5. Firm premia over the life-cycle by gender

(a) Men



(b) Women

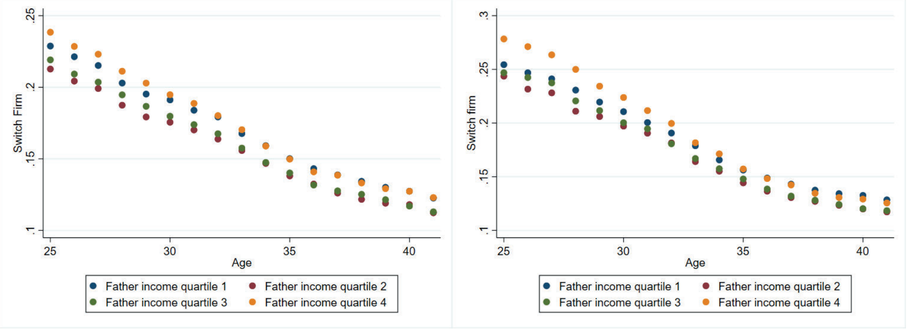


Notes: The figures show the estimated firm premium $\hat{\psi}_j$ from equation (1) over the life cycle, by quartile of father's income. Sub-figure (a) shows the results for men and (b) shows the result for women. Father's income quartiles are defined in the full sample

A.6 Firm switching

Figure A6. Firm switches by education

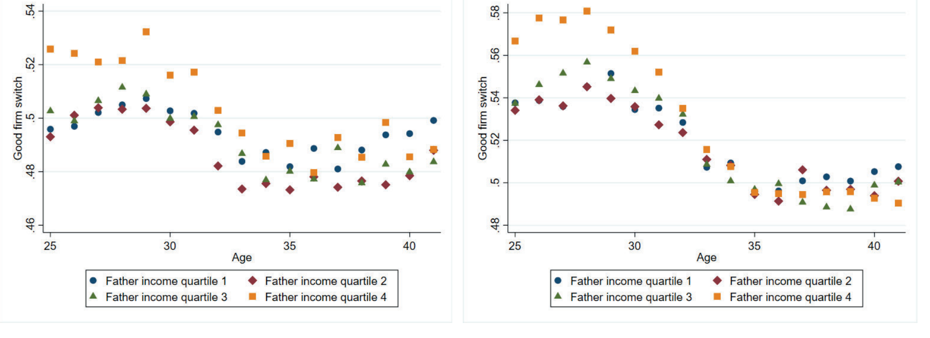
(a) Non-college *(b)* College



Notes: The figures show pattern for switching firms over the life-cyle by quartile of father's income. Sub-figure (a) shows the result for individuals who do not have a college education and sub-figure (b) shows the results for n individuals who have a college education.

Figure A7. Proportion of premium-improving firm switches

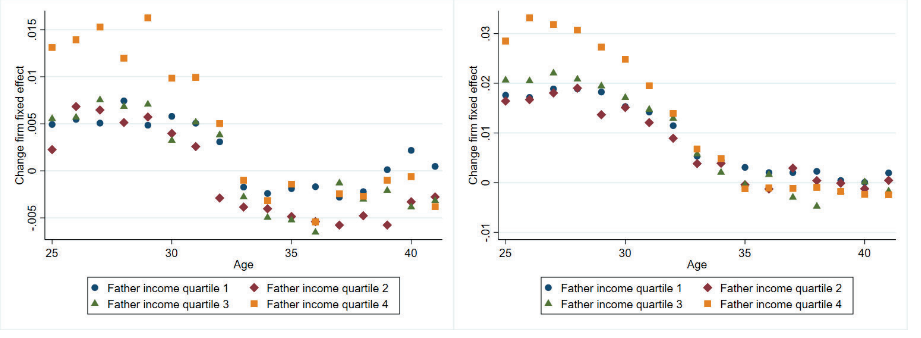
(a) Non-college *(b)* College



Notes: The figure shows the probability of switching to a firm with a higher firm premium than the one before conditional on switching, by father's income quartile. Sub-figure (a) shows the result for individuals who do not have a college education and sub-figure (b) shows the results for individuals who have a college education.

Figure A8. Mean change in firm premium among switchers

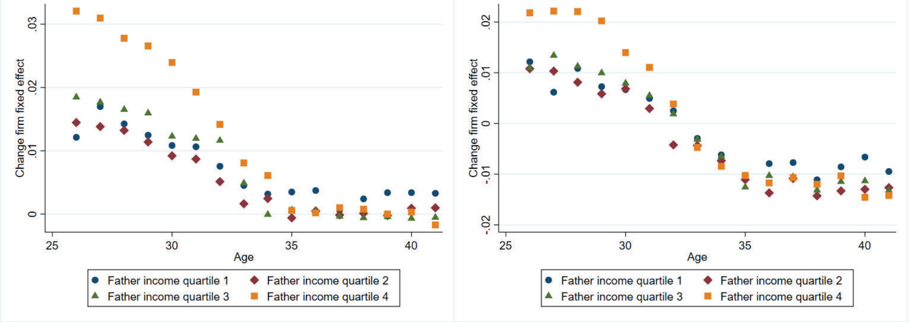
(a) Non-college *(b)* College



Notes: The figure shows the difference between the new firm premia and the firm premia before for individuals who switch firms. Sub-figure (a) shows the result for individuals who do not have a college education and sub-figure (b) shows the results for individuals who have a college education.

Figure A9. Change in firm FE for voluntary and involuntary switches

(a) Voluntary switches only *(b)* Involuntary switches only



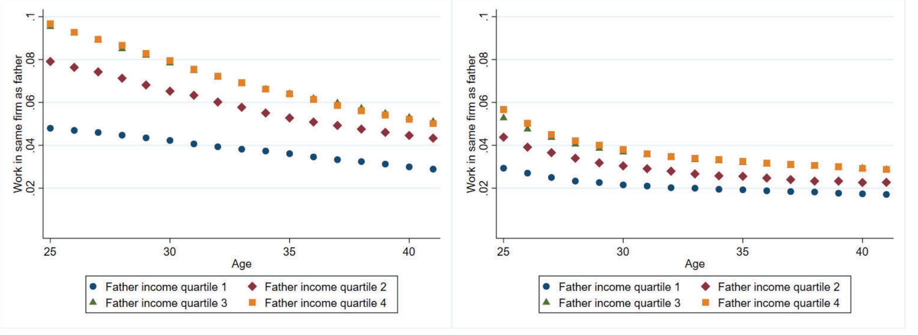
Notes: The figure shows the difference between the new firm premia and the firm premia before for individuals who switch firms, by father's income quartile. Sub-figure (a) shows the results for voluntary switches, defined as a switch without any unemployment insurance or without any year with zero income. Sub-figure (b) shows the result for involuntary switches, defined as a switch with either unemployment insurance or a year of zero income between working at the old firm and starting at the new firm.

A.7 Working patterns

In this section, we provide additional evidence on how working and commuting patterns vary by parental background. Figure A10 plots the share of individuals who work in the same firm as their father, separately for those

Figure A10. Working in the same firm as father

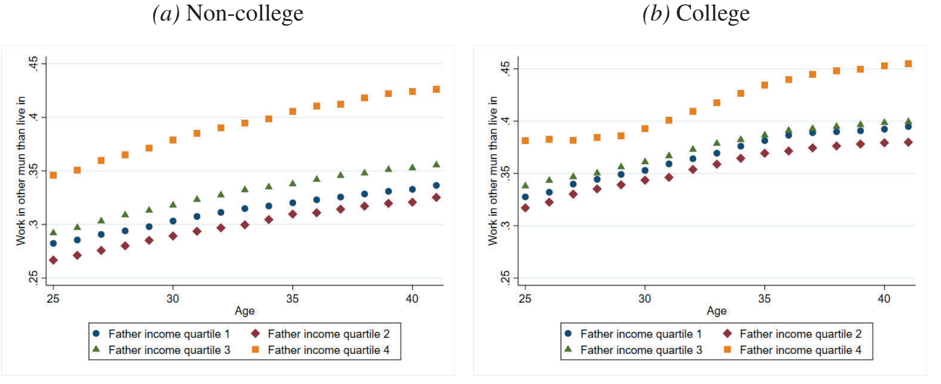
(a) Non-college *(b)* College



Notes: The figure shows the proportion of children who work in the father's main firm at different ages, separately by quartile of fathers' income. The father's main firm is defined as the firm the father works in for the most years between the years 1985-2018. Figure (a) include children without college education and figure (b) include children with college education.

with and without a college degree. Figure A11 plots the share of individuals who commute (i.e., work and reside in different municipalities), separately for those with and without a college degree. Children from high-income parents are more likely to commute, even within education group. Finally, Table A9 provides event-study type regression results conditional on individual fixed effects, showing how firm pay changes for individuals who begin to commute. Also when we consider such within-individual variation, the firm premium increases when individuals start to commute. As shown in column 1, commuting raises firm premia by about 1 pp. The pay benefit of commuting grows somewhat with age (column 2). Finally, column 3 shows that individuals from different parental backgrounds in terms of father's income benefit similarly from commuting. Thus, we conclude that firm pay is positively related to commuting and in a similar manner irrespective of parental background, but that high-SES children are more likely to commute.

Figure A11. Commuting



Notes: The figure shows the proportion of individuals who commute (i.e., work in another municipality than they live in) over the life-cycle, by quartile of father's income. Sub-figure (a) includes children without college education and sub-figure (b) children with college education.

Table A9. *Commuting*

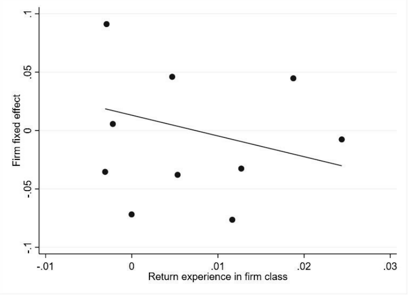
	Dependent variable: $\hat{\psi}_{j=J(i,t)}$		
	(1)	(2)	(3)
Commuting indicator	0.010***	0.017***	0.011***
	(0.000)	(0.000)	(0.000)
Commuting # 2nd father's income quartile			-0.001**
			(0.000)
Commuting # 3rd father's income quartile			0.000
			(0.000)
Commuting # 4th father's income quartile			-0.000
			(0.000)
Commuting # age normalized at 40		0.001***	
		(0.000)	
Age controls	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes

Notes: The table shows results with the firm premia as the dependent variable. The regressions include a dummy variable for working in a municipality other than living in and individual fixed effects. Columns 1 and 3 include flexible age controls (age, age squared, age interaction with father income quartile, and age squared interacted with father income quartile). Column (2) includes linear age dummies normalized at age 40 to ease interpretation and includes dummies for working in other municipalities than living in interacted with age. Column (3), includes dummies for working in other municipalities than living in interacted with the father income quartile.

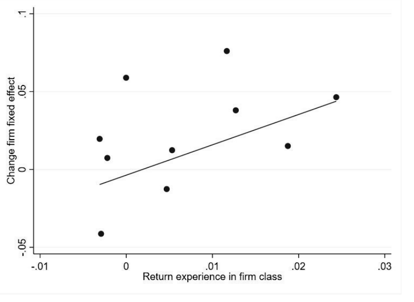
A.8 Return to experience in firm classes

Figure A12. Firm fixed effects and return to experience

(a) Firm fixed effects and return to experience



(b) Change in firm fixed effects and return to experience



Notes: The figures show firm fixed effects and experience estimated with regression (3). Figure (a) shows the relationship between firm fixed effects and returns to experience in the firm class. Figure (b) shows the relationship between the change in firm fixed effects for individuals who change firms, and the return to experience in the firm class the individuals switch from.

Essay 3: Do sibling correlations in skills, schooling, and earnings vary by socioeconomic background? Insights from Sweden

Co-authored with Akib Khan and Olof Rosenqvist

Acknowledgements: We thank Adrian Adermon, Georg Graetz, Raoul van Maarseveen, Martin Nybom, and seminar participants at IFAU and Uppsala University for valuable comments.

1 Introduction

Family background can shape an individual's life trajectory. There are strong correlations in socioeconomic outcomes between parents and children, and between siblings (e.g., Solon et al., 1991; Solon, 1999; Björklund and Jäntti, 2009; Björklund and Jäntti, 2020; Chetty et al., 2014; Behrman and Taubman, 1989; Vosters and Nybom, 2017; Adermon et al., 2021). But is life more formed by family background in certain groups than in others? Cross-country heterogeneity in the importance of family background is well documented. For example, sibling correlations – a comprehensive measure of family influence – in education is higher in developing countries (e.g., Dahan and Gaviria, 2001; Ahsan et al., 2022).[1] But there is also variation among industrialized countries. Nordic countries, for instance, are often documented to have relatively low sibling correlations in earnings and educational attainment (e.g., Björklund et al., 2009; Björklund and Jäntti, 2020). However, little is known about heterogeneities across the socioeconomic spectrum within a country.

Two societies where the importance of family background as a whole is similar can be different in terms of the distribution of this importance across social groups within a society.[2] In one society, sibling differences in socioeconomic outcomes may primarily arise in families with high socioeconomic status (SES), whereas the other society can display relatively constant sibling differences across groups with different SES. Although there might be disagreement over whether one society is preferred to the other, documenting the relationship between sibling similarity and parental SES arguably offers a richer picture of the equality of opportunity in a society.

An individual's (perceived) options in life, and thereby the possibilities of differentiating him/herself from their siblings, are potentially affected by the family's resources, preferences, and expectations, all of which may vary by SES. There is a theoretical discussion linking a potential socioeconomic gradient in sibling similarity to parental strategies for investing in their children. While several different mechanisms could give rise to differences in sibling similarity by parental SES,[3] parents' decisions on whether to reinforce, or compensate for, initial ability differences between siblings have been empha-

[1] Björklund et al. (2010) describe the sibling correlation as: "an omnibus measure of the importance of family background and community effects. It includes anything shared by siblings: parental income and parental influences such as aspirations and cultural inheritance, as well as things not directly experienced in the home, such as school, church and neighborhood effects" (p. 4).

[2] Just as the importance of family background can be different in two societies with the same cross-sectional inequality (see discussion in Solon, 1999)

[3] For instance, children of low SES parents might attend different schools than children of high SES parents and the schools might be different in terms of how they benefit children with different endowments. In addition, the degree of complementarity between child endowments and parental inputs could also matter for the relationship between parental SES and sibling similarity.

sized in the literature (Becker and Tomes, 1976; Griliches, 1979; Behrman et al., 1982). Griliches (1979), for instance, argues that parents want to compensate for initial ability differences and that high SES parents have more resources to make such investments.[4] Smaller sibling differences in high SES families would result in higher sibling correlations in these groups. Given this long-standing theoretical interest, the question has received surprisingly little empirical attention.

A key empirical challenge in answering this question is the availability of data on both generations and on a scale that would allow constructing granular groups of differing socioeconomic affluence. There is a small but growing set of studies on this question, mainly based on data from the U.S., Germany, and Sweden. Collectively, the evidence these papers offer on social gradients in sibling similarity is inconclusive.[5] Many of these papers use relatively small survey-based samples, while others construct broad SES groups. Both of these approaches, however, may limit the possibility to detect gradients and non-linearities in the relation between sibling similarity and parental SES.

In this paper, using population-wide Swedish register data spanning multiple generations, we provide one of the most comprehensive examinations yet on sibling similarity across the socioeconomic spectrum. In particular, we compare sibling correlations in skills, schooling, and earnings across fine-grained groups defined by parental education and earnings.[6] The register data contain information on mid-life earnings and years of schooling for the parent generation (born between 1940–1950) as well as the child generation (born between 1965–1982). For men in the child generation, the dataset also includes measures of cognitive and non-cognitive skills from the military conscription assessments made at age 18–19.[7]

In contrast to existing work, the register data allows us to construct fine-grained groups by parental education and earnings. It is important to use fine-grained groups rather than, e.g., divisions by the median, because the degree of homogeneity in the different groups matters for the comparison of the sibling correlations. For example, the variation in years of schooling and income is typically much higher above the median than below. Consequently, families in the group below the median will be more homogeneous than families above the median. Since the sibling correlation is the ratio of the between-family

[4]He writes, “Thus, I would expect that the within-family variance in socioeconomic achievement would decline at higher income levels” (Griliches, 1979, p. S62).

[5]Please see, for the US: Conley and Glauber (2007), Conley and Glauber (2008), and Conley et al. (2007); for Germany: Anger and Schnitzlein (2017), Grätz (2018), and Baier (2019); and for Sweden: Grätz et al. (2021), Grätz and Kolk (2022), and Hällsten and Thaning (2022).

[6]In the paper, we sometimes use the term income rather than earnings, but in both cases it is labor income we refer to.

[7]The ability measures from the military conscription have previously been used by, e.g., Lindqvist and Vestman (2011), Edin et al. (2022), and Grätz et al. (2021). Lindqvist and Vestman (2011) and Edin et al. (2022) have shown that these measures are strong predictors of labor market performance.

variation to the total variation, this will lead to a lower sibling correlation in the below-median group even if the within-family variation is the same across the two groups (Solon et al., 1991). Since we, in this paper, are not interested in the between-family variation per se, we want to avoid this. Lastly, a finer division is better equipped to detect gradients and non-linearities in the data.[8]

A more granular division tends to equalize the between-family variation in the different groups so that any differences in sibling correlations across groups are more likely to reflect differences in within-family variation, which arguably is of higher interest and more related to the parental investment theories discussed above. Hence, we also directly compare this within-family variation across groups as suggested by Breen and Ermisch (2021). An alternative approach would be to only focus on the within-family variation. However, the measure is then never put in relation to the overall variation, which could be misleading. Therefore, we generally show both the sibling correlation and the underlying variance components, paying particular attention to differences in sibling correlations across social groups that are driven by differences in the within-family variance.

Our results show a clear and consistent pattern. Sibling correlations generally decrease with parental SES. For years of schooling, mid-life earnings and cognitive skills, sibling correlations decline almost monotonically with both the education and earnings of the parents. In comparison, sibling correlations in non-cognitive skills only decrease with parental earnings.

The socioeconomic gradient we find is substantive. Moving from the 5th to the 15th ventile of parental income, for instance, sibling correlations in income and cognitive skills decline by over 20% and 7%, respectively. The corresponding relative decline for the sibling correlation in non-cognitive skills is 14%.

The decline in sibling correlations for education and income with parental SES is mainly driven by an increase in within-family variation, indicating that siblings are more similar to each other in families with low SES (i.e., contrary to the prediction by Griliches, 1979). In contrast, the decrease in sibling correlations in skills is driven by a relative decrease in between-family variation.

Our main results are based on male siblings as we lack data on skills for women. However, reassuringly, the pattern for sibling correlations in years of schooling and earnings remains very similar when women are included in the sample. Lastly, we show that these patterns are robust to measuring income at different ages, and are not driven by differences in family structure (number of siblings and age differences between them) across the socioeconomic spectrum.

[8]Figure A3–Figure A6 in the Appendix substantiate this point, presenting results in an ascending order of granularity. Furthermore, our replication of Hällsten and Thaning (2022) in Section 7.1 offers a concrete example.

We contribute to three strands of the literature. First, we add to a small set of papers documenting variations in sibling similarity across different demographic groups.[9] As mentioned before, these papers often rely on small samples or create broad groups that might not be sufficient to detect meaningful inter-group differences in sibling correlations. Using population-wide administrative data covering multiple generations in Sweden, which enables us to create granular subsets of the population with varying SES, we provide one of the first pieces of evidence of a robust negative relationship between parental SES and sibling correlations in skills, earnings and education.[10]

It is particularly interesting to discuss the findings in Grätz et al. (2021), Grätz and Kolk (2022), and Hällsten and Thaning (2022) since they also use Swedish register data. Grätz et al. (2021) focus on cognitive ability, school grades, and educational attainment, finding higher sibling correlations among low SES families when defined by parental occupation, while results are less consistent when defined by parental education. Grätz and Kolk (2022) find similar sibling correlations in total earnings over the ages 18–60 across three SES groups based on parental occupation. Lastly, Hällsten and Thaning (2022) use data on education, occupation, income and wealth for both the child and parent generation and estimate sibling correlations for each outcome in groups defined by parental SES quintiles. They generally find higher correlations in high SES families, particularly when defined by wealth. We replicate some of Hällsten and Thaning's findings and find suggestive evidence that these patterns are sensitive to variations in the granularity with which we measure SES.

The evidence we generate also relates to the theoretical literature on the role of parental inputs in inter-sibling differences in human capital formation and earnings. Parents might be averse to inequality and attempt to compen-

[9]The results in these papers are mixed with both positive, negative and non-existent associations between sibling similarity and parental SES being reported. To some extent, this can perhaps be explained by the fact that different outcomes and SES definitions are used by different studies. But even within countries, and for similar outcomes, it is hard to discern consistent patterns. In Germany, e.g., Baier (2019) finds that the sibling correlation in cognitive ability is higher in low SES families than in high SES families while Grätz (2018), who studies cognitive ability and educational attainment, finds no differences by parental SES. In addition, Anger and Schnitzlein (2017) find that high SES families display a higher sibling correlation in non-cognitive ability. There are also indications that the relation between sibling similarity and parental SES can be non-linear. Karhula et al. (2019), studying education and labor market outcomes in Finland, find a U-shaped pattern between sibling similarity and parental SES.

[10]Since papers estimating intergenerational parent-child correlations in socioeconomic outcomes typically find stronger associations among high SES families (e.g., Acciari et al., 2022; Bratberg et al., 2017), our finding of an opposite-signed relationship with respect to sibling correlations might seem somewhat surprising. However, sibling correlations and parent-child associations have different interpretations, with the latter being a narrower measure of family influence. In fact, Solon (1999) derives: Sibling Correlation = $(Child - ParentCorrelation)^2$ + other shared factors that are orthogonal to the parental variable. Using this relationship, Björklund and Jäntti (2020), for instance, show that these "other shared factors" are substantially more important in their relative contribution to the sibling correlation.

sate for ability differences across their children. Efficiency considerations, on the other hand, might lead them to reinforce these ability differences. The magnitude of such reinforcing investments, however, would depend on the resources a family has to begin with. Our results are therefore consistent with a scenario where parents generally reinforce ability differences across their children, with higher SES parents having greater capacity to do so more effectively. However, we acknowledge that the observed socioeconomic gradient in sibling similarity could arise even under compensating parental investments if there are sufficiently strong complementarities between parental investments and child ability. Still, this scenario arguably relies on stronger assumptions than the scenario with reinforcing investments.

Finally, we add to a large body of literature that uses the sibling correlation as a measure of the importance of family background in shaping children's socioeconomic outcomes. This strand of literature often uses this measure to compare different countries or broad groups within a country (e.g., males vs females; Björklund and Jäntti, 2020). Our paper undertakes an in-depth study of how sibling correlations vary across the socioeconomic spectrum within a country, thereby offering a more nuanced portrayal of the country's opportunity landscape.

The paper is organized as follows: In section 2, we discuss theories and previous empirical evidence concerned with parental investment strategies and child-rearing principles and discuss what they imply in terms of sibling differences across social groups. Section 3 describes the data whereas Section 4 outlines the empirical approach. Results are presented in Sections 5 and 6. Section 7 provides a discussion of the results and section 8 concludes.

2 Conceptual framework

The theoretical literature on a potential socioeconomic gradient in sibling similarity goes back to the 1970s. In the model of Becker and Tomes (1976), parents have incentives to reinforce initial ability differences between siblings for efficiency reasons. Siblings with poorer endowments are then compensated later in life with monetary transfers. Since parents with high SES have more resources they can make larger reinforcing investments. Thus, according to this theory, sibling differences in human capital and earnings would tend to be larger in more well-off families (and sibling correlations thereby lower).

On the other hand, Griliches (1979) and Behrman et al. (1982) suggest that parents have equity concerns, not only regarding consumption levels of the siblings, but also regarding the human capital levels. A key conclusion is that parents may make compensating investments even if it's more efficient to reinforce. Therefore, sibling differences would instead be smaller, and sibling correlations higher, in families with high SES if the influence from the equity concern dominates.[11]

Clearly, the direction of the models' predicted socioeconomic gradient in sibling similarity depends on whether parents make reinforcing or compensatory investments. This question has received substantial empirical attention (e.g., Fan and Porter, 2020; Savelyev et al., 2022; Yi et al., 2015; Frijters et al., 2013; Hsin, 2012; Restrepo, 2016; Grätz and Torche, 2016; Rosenzweig and Wolpin, 1988; Rosenzweig and Zhang, 2009; Behrman et al., 1994). The evidence from these studies is mixed, but reinforcing investments are slightly more common than compensating investments (see, e.g., the review and discussion in Almond and Mazumder, 2013).

Importantly, parental responses to endowment differences between siblings may vary by SES. Hsin (2012), Restrepo (2016), and Grätz and Torche (2016) study this question. Despite the fact that all studies analyze US data and use birth weight as an indicator of the child's endowment, they reach different conclusions. Hsin (2012) and Restrepo (2016) find that low-educated mothers spend more time with children with a higher birth weight (i.e., reinforce) whereas high-educated mothers spend more time with children with a lower birth weight (i.e., compensate).

In contrast, Grätz and Torche (2016) generally find small parental responses to differences in birth weight that do not vary by SES. In addition, Grätz and Torche (2016), using a measure of cognitive ability at age 4, find that parents with high SES provide more cognitive stimulation to higher-ability children, while parents with low SES do not react to ability differences. Thus, the socioeconomic gradient in parental responses to endowment differences between siblings appears to be complex with varying patterns across different types of child endowments. Consequently, this research gives little guidance on what to expect for the relationship between sibling differences in socioeconomic outcomes and family SES.

Baier (2019) introduces another perspective, suggesting that sibling outcome differences might be larger in high SES families due to child-rearing strategies. High SES parents are better equipped to supply more child-specific inputs, adjusting interactions based on individual talents and interests (e.g., play music with a child that displays musical talent/interest and do math with a child that displays mathematical talent/interest). In contrast, low SES par-

[11] It should also be noted that the nature of marginal returns to investments in children can play a role for the predictions. Arguably, parents with higher SES generally invest more in the children and so potential differential investments in the children occur at a higher baseline level compared to parents with lower SES. If the return to investments is decreasing, which is often assumed, a given differential investment will matter less for outcomes if the baseline investment level is higher. With this perspective, it could be argued that it actually is harder for high SES parents to compensate or reinforce. However, Becker et al. (2018) challenge the assumption of decreasing marginal returns to investments in children and argue that the returns might actually be increasing instead.

ents might provide more generic inputs to all their children. This hypothesis suggests that sibling outcome differences in high SES families are expected to be larger than in low SES families.

A couple of recent papers highlight a weaker association between potential educational attainment (measured by a polygenic score) and actual educational attainment for individuals from low SES families.[12] This has been shown in the US Papageorge and Thom (2020) as well as in the welfare state of Denmark (Ronda et al., 2022). Besides suggesting that there is wasted potential in society, these findings indicate that sibling differences in educational attainment can be lower in low SES families as endowment differences are carried through to a lesser extent to differences in actual educational attainment. These results align with Becker and Tomes (1976), suggesting low SES parents may lack resources for reinforcing investments.

Other papers explore parental responses to variations in public investments in the children (Das et al., 2013; Pop-Eleches and Urquiola, 2013; Fredriksson et al., 2016). Fredriksson et al. (2016) find that high-income parents of children in larger classes help more with homework whereas no corresponding pattern is found for low-income parents. Such substitutability between public and private investments in children from high SES families suggests that sibling outcome similarity may increase with family SES.

In conclusion, the relationship between sibling similarity in socioeconomic outcomes and family socioeconomic status (SES) is multifaceted. Early models suggest that parental reinforcement of initial ability differences could lead to larger disparities in well-off families, while others propose that equity concerns may drive compensatory investments, potentially reducing differences in high SES families. Empirical evidence presents a mixed picture, with varied parental responses to endowment differences by SES. Moreover, recent findings suggest that educational attainment differences may be lower in low SES families due to constrained resources, echoing earlier insights. Overall, the intricate interplay between parental investment strategies, socioeconomic context, and public policies underscores the complexity of this relationship.

3 Data

We combine multiple Swedish registers covering the population. These registers have been compiled by Statistics Sweden and include pseudonymized personal identifiers, making it possible to collate information from different registers. A key data source is the multigenerational register which links children with parents for children born in the period 1932–2019. This information lets us identify siblings in the data. In our dataset, a row corresponds to a child,

[12] Some specific genetic variants are highly associated with educational attainment. The polygenic score for educational attainment is obtained by combining these known associations with individual level DNA information.

and to this unit of observation we add data on skills, schooling, and earnings pertaining to the child and their parents.

For skills, we observe results on cognitive and non-cognitive ability tests taken during the military conscription at age 18–19. The test of cognitive ability, which is a written test, is similar to a standard intelligence test (Carlstedt, 2000; Carlsson et al., 2015), whereas non-cognitive ability is assessed by a trained psychologist on the basis of an interview with the conscript (Mood et al., 2012). This information is only available for men and covers cohorts born between 1950–1980. The test results are standardized within test cohorts.

Data on years of schooling come from the LISA register, which includes all individuals aged 16–65 residing in Sweden in a given year. This register is available since 1985 and contains annually updated information on the highest level of education. Annual income data is also available from 1985 and we use this data to average income over key mid-life years for both children and parents.

For children, we approximate life-time earnings with the average annual earnings for the ages 35 to 37 years.[13] We then perform a percentile rank of the income measure by birth year. Thus, the sibling correlations in income are measured as rank-rank correlations. Education is measured at age 30. If education data is missing at age 30, we use the next available observation after age 30.

To rank parents along the socioeconomic spectrum, we use two different measures of socioeconomic status: income and education. For parents, we define individual life-time income as the average income for the ages 45 to 50 years and calculate parental income as the total of maternal and paternal incomes. We then divide these parental incomes into ventiles by the average birth year of the two parents.[14] Similarly, for education, we take the average of the two parents' years of education and divide them into ventiles by the average birth year of the parents.

The sample is limited to parents born between 1940 and 1950, and children born between 1965 and 1982. The appendix includes results when only doing the cohort restrictions for children, extending the parent cohorts by using income from 1968[15] (Figure A12 and Figure A13).[16] Skills data are only available for men. The results in the main body of the paper are therefore limited to only include male siblings to keep the sample harmonized across outcomes. However, the appendix includes results where both men and women

[13] Our conclusions remain similar if we instead use average earnings between ages 25–30 (see Figure A11 in the appendix).

[14] A ventile represents 5% of the underlying population.

[15] We observe earnings data for the years 1968, 1970, 1971, 1973, 1975, 1976, 1979, 1980, 1982 and annually for 1985–2019.

[16] The results are not sensitive to these parent cohort variations.

are included (Figure A1 and Figure A2). Table 1 below describes our analysis samples and shows summary statistics for the outcomes. [17]

Table 1. *Summary Statistics of Sample*

	Male sample			Male and female sample		
Outcomes	Children (N)	Families (N)	Mean	Children (N)	Families (N)	Mean
Income	441,644	322,943	319,924	858,061	460,594	266,948
Education	410,782	304,204	12.7	798,383	438,422	12.9
Cognitive ability	357,435	273,334	0.061			
Noncognitive ability	357,435	273,334	0.068			

Notes: This table describes our main samples. Education represents years of schooling. Income represents average annual labor income during ages 35–37 in 2018 SEK. Ability measures are standardized by test year.

4 Estimation

We compare sibling differences in socioeconomic outcomes across groups by estimating sibling correlations separately by outcome and group. Following the literature (see, e.g., Björklund et al., 2010), we estimate mixed-effect models of the type specified in Equation (1) below.[18] The subscripts i and j denote family and child respectively, and the control vector includes gender (where applicable) and birth-year dummies.

$$y_{ij} = x_{ij}\beta + a_i + b_{ij} \tag{2}$$

From this model, we obtain estimates of the within-family variation (σ_b^2) and the between-family variation (σ_a^2). We can then estimate the sibling correlation as follows:

$$\rho = \frac{\sigma_a^2}{\sigma_a^2 + \sigma_b^2} \tag{3}$$

Thus, the sibling correlation amounts to the share of total variation that is due to between-family variation. Alternatively, the sibling correlation can be thought of as the correlation in the outcome between randomly drawn pairs of siblings. Note, however, that all families, no matter the number of siblings, are included in the analysis sample.[19] Since the sibling correlation depends on both the between-family variation and the within-family variation in the

[17] Almost 50% of the children in the male sample are the only child in their family within the sample. The main results are virtually unchanged when we drop these singletons from the estimation sample.

[18] We use restricted maximum likelihood estimation (reml). The analyses are performed using the mixed command in Stata.

[19] All observations are given the same weight, i.e., we don't let weights vary by family size.

studied outcome, it is crucial to understand the relative importance of the two components. Throughout the paper, we therefore present estimates of the variance components alongside estimates of the sibling correlations.

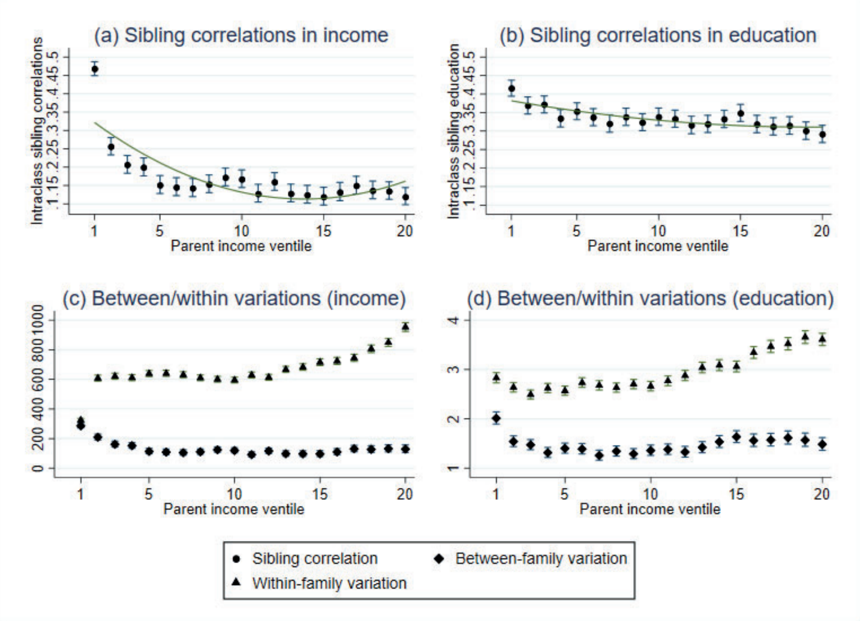
To investigate how the sibling correlations in skills, earnings and education vary by parental SES, we do separate estimations for each ventile of parents' SES, where parents' SES is defined by either education or income.

5 Main results: sibling correlations by parental earnings

This section presents estimates of sibling correlations, stratified by ventiles of parents' earnings, for the following outcomes: income, education, cognitive skills and non-cognitive skills.

5.1 Sibling correlations in income and education by parental income

Figure 1. Sibling correlations in income and education by parental income ventiles



Notes: The figure shows sibling correlations in earnings and years of schooling for male siblings by parental income ventiles. The green line shows a second-order polynomial fitted line for the sibling correlations.

We start by documenting sibling correlations in income and years of schooling by ventiles of parental income (Figure 1). The left and right panels show sibling correlations in income and education, respectively. Both figures show a decline in sibling correlation with parents' income.[20] In other words, siblings in families with low parental income are more similar to each other in terms of both income and education compared to siblings in families with parents who earn more. The decline is particularly salient for income in both absolute and relative terms. For instance, moving from the 1st to the 20th ventile of parental income, the sibling correlation in income declines by more than 0.3 (or by over 70%; please see Table 2).[21] The corresponding decline for education is 0.12 (30%). In both cases, the differences are statistically significant at the 1% level. Figure 1 includes male siblings only, but the pattern holds when we expand the sample to comprise both men and women (results are reported in Figure A1).

What drives this decline in sibling correlations with parents' socioeconomic status: within-family (σ_b^2) or between-family (σ_a^2) variation? Ceteris paribus, higher within-family variation leads to lower sibling correlation whereas the correlation increases monotonically with between-family variation. Both for earnings and education, Figure 1 shows an increase in within-family variation with parental income, especially on the right tail of the parental income distribution (i.e. contrary to the prediction by Griliches, 1979). This indicates that the decrease in sibling correlations by parental SES is mainly driven by an increase in within-family variation. The results look similar when the sample is limited to families with two children with an age gap of four years or less (see Figure A7), suggesting that the decline is not driven by differences in family structure by SES.

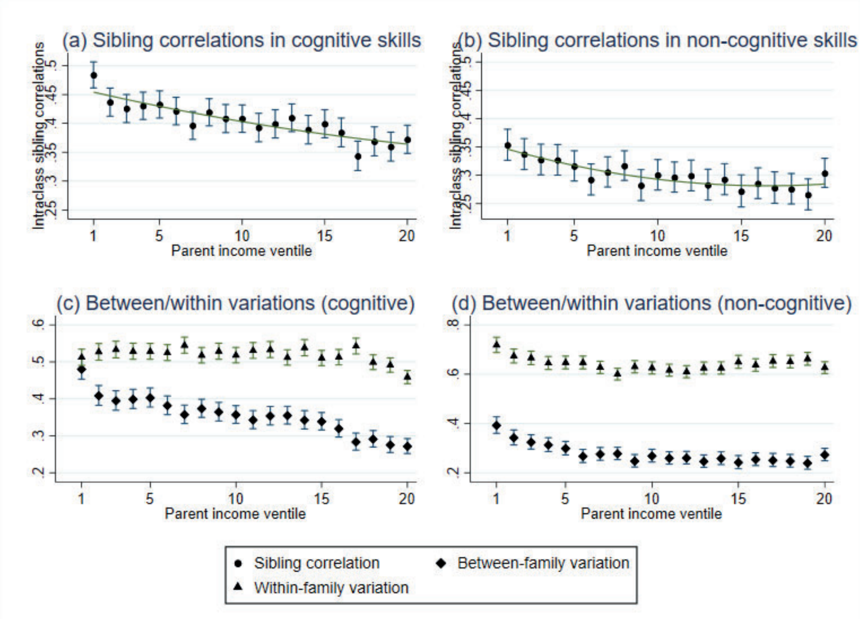
5.2 Sibling correlations in cognitive and non-cognitive skills by parental income

Figure 2 reports sibling correlations in cognitive and non-cognitive skills by parental income. These skills can complement years of schooling as a measure of human capital investment. In particular, while compulsory education policies might place a lower bound on completed years of schooling, this might not be the case for these measured skills. However, these are measured at an earlier age, namely when the individuals are 18–19 years old, whereas completed years of schooling is measured at age 30 or later.

[20]Figure A3 and Figure A4 show that the negative relation between sibling correlations and parental income is more pronounced with a more granular division of parental quantiles. This is particularly true for education.

[21]The results for the first ventile should be interpreted with some caution as some parents with very low (or zero) labor income might in practice be well-off individuals who receive income via capital investments rather than labor. The first ventile could therefore potentially be more diverse than the other ventiles. We do also see slightly higher between-family variation in this group (see Figures 1c and 1d). Still, the very high sibling correlation in income in ventile 1 (Figure 1a) is primarily driven by a markedly lower within-family variation (Figure 1c).

Figure 2. Sibling correlations in cognitive and non-cognitive skills by parental income



Notes: The figure shows sibling correlations in cognitive and non-cognitive ability for male siblings by parental income ventiles. The green line shows a second-order polynomial fitted line for the sibling correlations.

Consistent with the results for income and education, sibling similarity in both cognitive and non-cognitive skills tends to decline with parental income.[22] [23] However, in contrast to the result for education and income, this decline in sibling correlations seems to be driven by a decline in between-family variation rather than an increase in within-family variation.

6 Robustness: sibling correlations by parental education

This section replicates the analyses above by using parental education – instead of income – to define SES. Note, however, that, compared to parental income, education is measured more coarsely in our dataset. In addition, as mentioned before, compulsory education policies constrain the variation in this measure, especially on the left tail. For these reasons, we view parental

[22] Again, while the negative relation is always there, it is less clear when parents are grouped by the median income, or by quintiles or deciles (Figure A5 and Figure A6).

[23] The results look similar when the sample is limited to families with two children with an age gap of four years or less (see Figure A8 in the appendix).

Table 2. *Differences in sibling correlations between different parental income ventiles*

Outcome	Income	Education	Cognitive skills	Non-cognitive skills
Panel A. Comparison between parental income ventiles 5 and 15				
Ventile 5	0.151	0.353	0.433	0.316
Ventile 15	0.119	0.348	0.399	0.272
Difference	0.032*	0.005	0.034*	0.044**
Panel B. Comparison between parental income ventiles 1 and 20				
Ventile 1	0.468	0.416	0.484	0.353
Ventile 20	0.119	0.292	0.372	0.304
Difference	0.349***	0.124***	0.112***	0.049***

Notes: This table presents sibling correlations estimated for parental income ventiles 1, 5, 15, and 20, and the difference between these correlations for ventiles 5 and 15 and 1 and 20, respectively. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

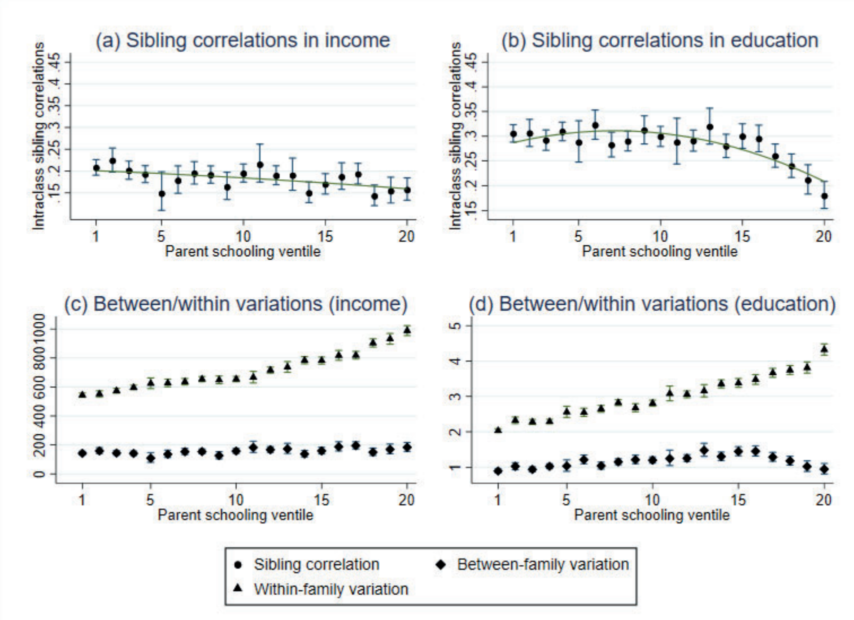
income as our main measure of parental SES, while parental education constitutes an alternative measure.

6.1 Sibling correlations in income and education by parental education

Figure 3 shows how sibling correlations in income and education vary with parental education. Consistent with previous results, we see a decline in sibling correlation in education at higher ventiles, driven by rising within-family variations. However, the corresponding pattern for income looks weaker. Similar results are found when female siblings are included (Figure A2).

Although the pattern of decreasing sibling correlations by parents' SES holds whether this status is measured in terms of parental education or income, some differences exist. When parents' SES is defined by education, the decline in sibling correlations is steeper at the end of the distribution (particularly for education). In contrast, when parents' SES is measured as income, the decrease in sibling correlations is more pronounced on the left tail of the parental income distribution (particularly for income).

Figure 3. Sibling correlation in income and education by parental education

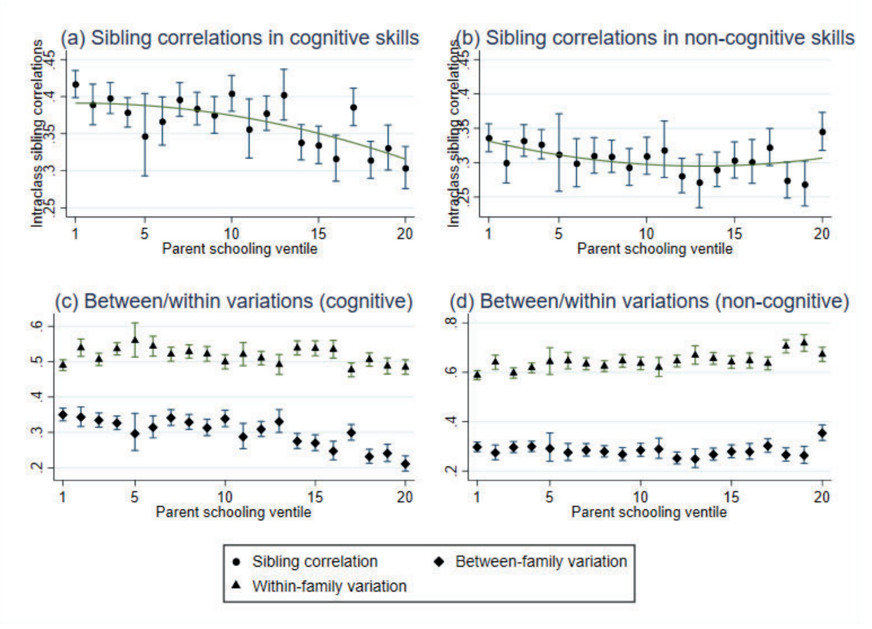


Notes: The figure shows sibling correlations in income and years of schooling for male siblings by parental education ventiles. The green line shows a second-order polynomial fitted line for the sibling correlations.

6.2 Sibling correlations in skills by parental education

Figure 4 replicates Figure 3 for sibling correlations in cognitive and non-cognitive skills. The negative gradient we found earlier for cognitive skills in Figure 2 appears to hold. However, the decline is less salient for the correlations in non-cognitive skills. One reason is that the correlation goes up sharply at ventile 20. Also note that the negative relation between the sibling correlation in cognitive skills and parental education is driven by a decrease in between-family variation at the right tail of the SES distribution rather than an increase in within-family variation. For non-cognitive skills, on the other hand, we find a somewhat positive relation between within-family variation and parental schooling ventile.

Figure 4. Sibling correlations in skills by parental education



Notes: The figure shows sibling correlations in cognitive and non-cognitive ability for men by parental education ventiles. The green line shows a second-order polynomial fitted line for the sibling correlations.

7 Discussion

7.1 Comparison with previous estimates

How do our findings compare to those in the existing studies, particularly those using similar data from Sweden? The closest paper of interest is Hällsten and Thaning (2022), especially their results on sibling correlations in education by parental education, which we can attempt to replicate with similar data.[24] Note, however, that, in the absence of a replication package, we cannot ensure that our analysis is technically identical to theirs.[25]

Figure A9 is an attempt to replicate their analysis of sibling similarities in education across parental education quintiles. We find a somewhat positive gradient similar to their estimates. As we increase the granularity of the parental education groups by creating twenty ventiles, this positive slope disappears (see Figure A10). Instead, consistent with our results (see panel b of

[24]This is what we study in panel b of Figure A3

[25]In similarity with Hällsten and Thaning (2022) we limit the sample to children born between the years 1945–1976 and parents born between the years 1930–1939, and measure children's education in ranks. To measure parents' socioeconomic status, we follow Hällsten and Thaning (2022) and define their status on the basis of their education, and divide education into five quintiles.

Figure 3), we see a marked decline in sibling correlations at the right tail of the parental education distribution. This exercise further strengthens the argument that creating granular SES groups is critical to discerning how sibling similarity might vary with family background.

7.2 Implications for theories about parental investments

Our results relate to the theoretical literature on the role of parental inputs in inter-sibling differences in human capital formation and earnings (Becker and Tomes, 1976; Griliches, 1979; Behrman et al., 1982). Parents might be averse to inequality and attempt to compensate for ability differences across their children. Efficiency considerations, on the other hand, might lead them to reinforce these ability differences. High SES parents have more resources to make compensating or reinforcing investments, and thus we expect smaller sibling differences (and larger sibling correlations) in high SES families if the inequality concern dominates and larger sibling differences (and smaller sibling correlations) in high SES families if the efficiency concern dominates.

Since we generally find smaller sibling correlations in high SES families, the results are consistent with the theory that parents reinforce endowment differences between siblings. But this is of course very indirect evidence and does not prove that parents generally reinforce. The pattern we observe can arise for other reasons. In fact, there could be smaller sibling correlations in high SES families even if parents generally make compensating investments if there are strong complementarities between parental inputs and child endowments.

Still, on balance, we believe that our results are more consistent with reinforcing parental inputs than compensating. Interestingly, that interpretation aligns well with the conclusion in Almond and Mazumder (2013) that reinforcing investments are slightly more common than compensating investments. It has proven difficult to directly document reinforcing or compensating behavior in a compelling way and the results in this literature are mixed. The type of indirect evidence that we contribute with here can therefore complement earlier studies.

8 Conclusion

A person's (perceived) possibilities in life, and consequently, the opportunities to distinguish themselves from siblings, can be influenced by the family's resources, preferences, and expectations. These aspects may vary depending on the family's socioeconomic status (SES). Hence, to understand the opportunity landscape in a society, it is critical to assess, for instance, if life is more formed by individual endowments and considerations in families with better access to resources. In other words, are outcome differences between siblings greater in high SES families than in low SES families?

In this paper, we have provided one of the most comprehensive examinations yet on sibling similarity in skills, schooling and earnings across granular groups defined by parental SES. Measuring sibling similarity in terms of sibling correlations, the results show a remarkably consistent pattern. Sibling correlations generally decrease in the SES of the parents. For years of schooling, mid-life earnings and cognitive ability, we see that sibling correlations decrease in both the education and income of the parents, while sibling correlations in non-cognitive ability only decrease in the income of parents.

The main results are based on male siblings since we lack data on skills for women, but the pattern for sibling correlations in years of schooling and earnings remains very similar when also women are included in the sample. Our results are to some extent different compared to earlier studies from Sweden, particularly Hällsten and Thaning (2022). We believe that differences in cohorts, exact outcome measures and SES granularity can explain at least parts of these disparities. Importantly, we are able to replicate their results on sibling correlations in education by education of the parents. We further show that the positive relation between the sibling correlation and parental SES documented in Hällsten and Thaning (2022) becomes more similar to our results (i.e. more negative) when the same granularity of SES groups as in our paper is used.

Since the sibling correlation depends on both the between-family variation and the within-family variation in the studied outcome, it is crucial to understand the relative importance of these two components for the observed social gradient in sibling outcome similarity. Ceteris paribus, higher within-family variation leads to lower sibling correlation whereas the correlation increases monotonically with between-family variation. We show that the decline in sibling correlations of income and education is driven by an increase in within-family variation by parental SES. In contrast, the decline in sibling correlations of skills is driven by a decline in between-family variation. Thus, the interpretation of the skills results is not straightforward as they do not consistently point toward lower sibling similarity in the upper part of the socioeconomic spectrum. It is interesting to note that within-family variation in education and earnings is increasing in parental SES while the relation is relatively flat for skills. This means that it is not larger within-family differences in skills that give rise to the larger differences in education and earnings among the high SES families. A potential explanation is that high-ability children from low SES families cannot reach their full potential in terms of earnings and educational attainment, but instead end up closer to their lower-ability siblings (as also suggested by Papageorge and Thom, 2020, and Ronda et al., 2022).

Finally, while more research is required to uncover the mechanisms behind the observed social gradient in sibling similarity, the results suggest that life is more formed by individual endowments and considerations for individuals from high SES backgrounds as compared to individuals from low SES backgrounds. In other words, children from low SES homes not only, have worse average socioeconomic outcomes than children from high SES homes, but they also appear to have worse possibilities to develop individually. We argue that this is an important aspect of inequality in a society that largely has been overlooked in the literature. Lastly, we document a robust negative relationship between parental SES and sibling correlations in Sweden, a welfare state in which sibling correlations for the country as a whole generally tend to be lower than in most other industrialized countries. Future studies from other countries, using similar levels of SES granularity as in the current study, is therefore of potential interest.

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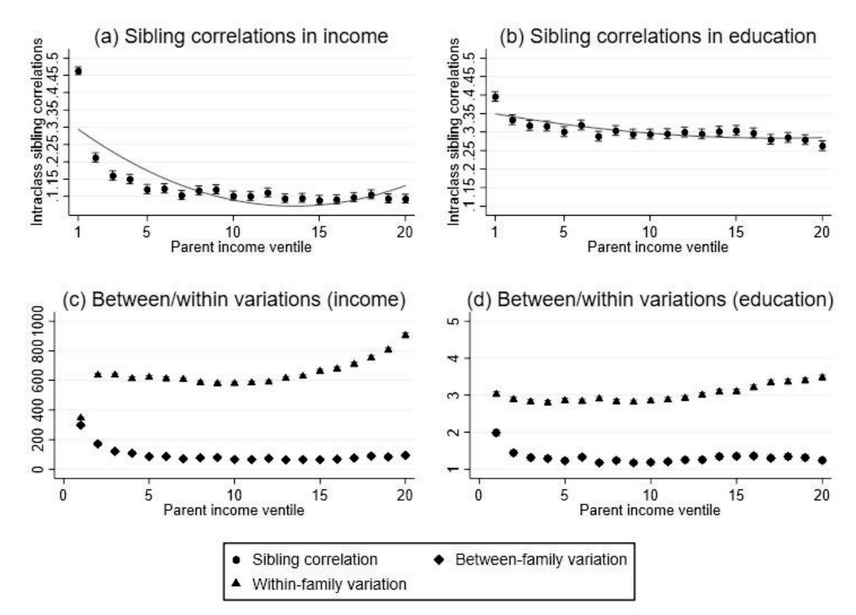
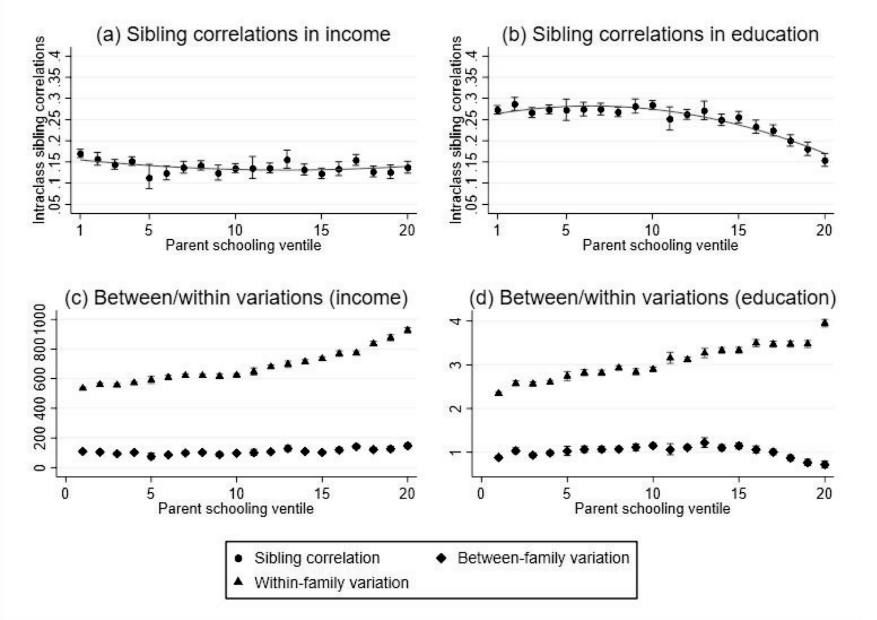
Appendix A: Additional figures and tables

Figure A1. Sibling correlations in income and education by ventiles of parents' income (men and women)



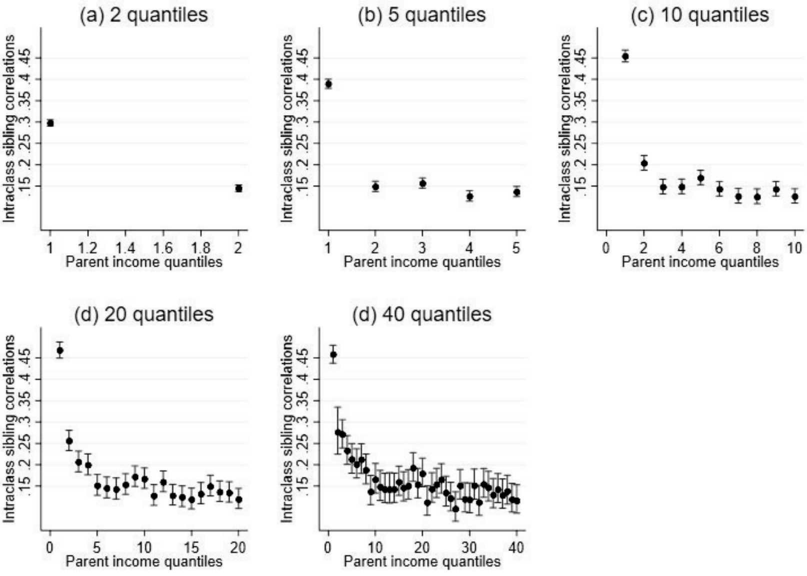
Notes: The figure shows sibling correlations in income and years of schooling by parental income ventiles in a sample in which both men and women are included. The line shows a second-order polynomial fitted line for the sibling correlations.

Figure A2. Sibling correlations in income and education by ventiles of parents' education (men and women)



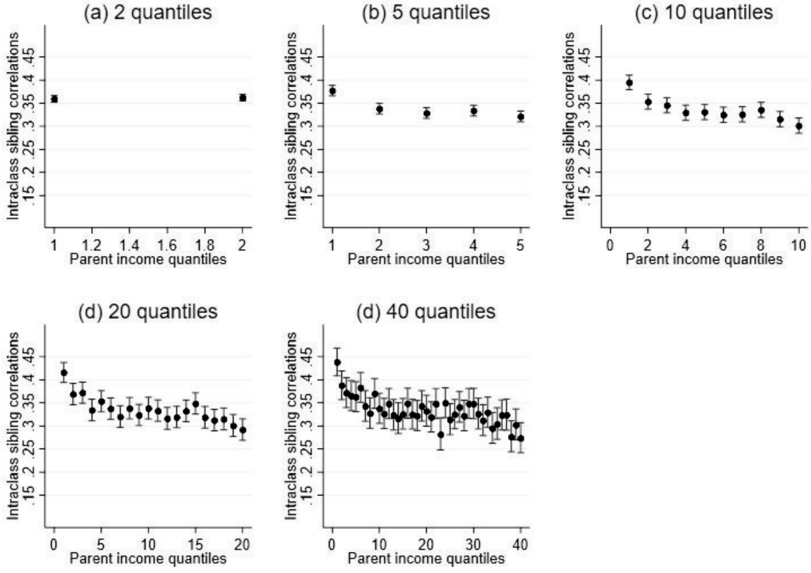
Notes: The figure shows sibling correlations in income and years of schooling by parental education ventiles in a sample in which both men and women are included. The line shows a second-order polynomial fitted line for the sibling correlations.

Figure A3. Sibling correlations in income by different quantiles of parents' income



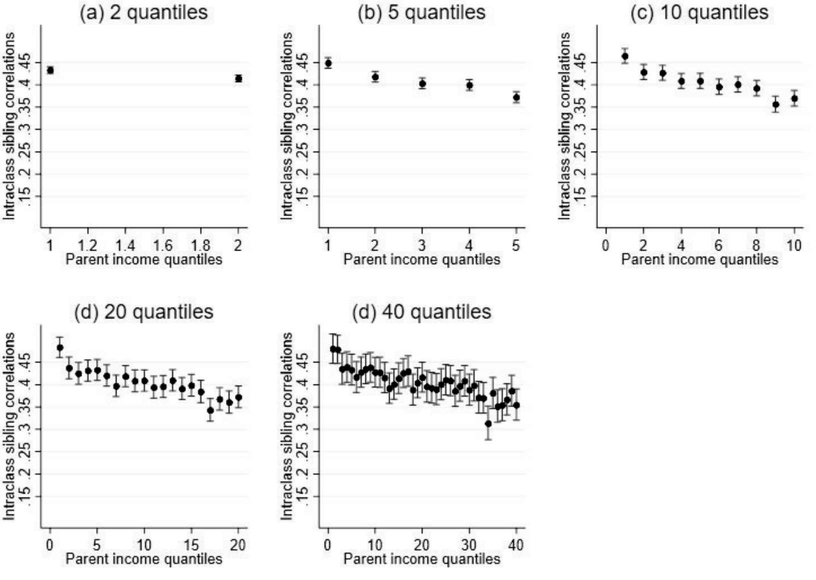
Notes: The figure shows sibling correlations in income for men by different divisions of parental income quantiles, where parents' income has been divided into 2, 5, 10, 20 or 40 quantiles.

Figure A4. Sibling correlations in education by different quantiles of parents' income



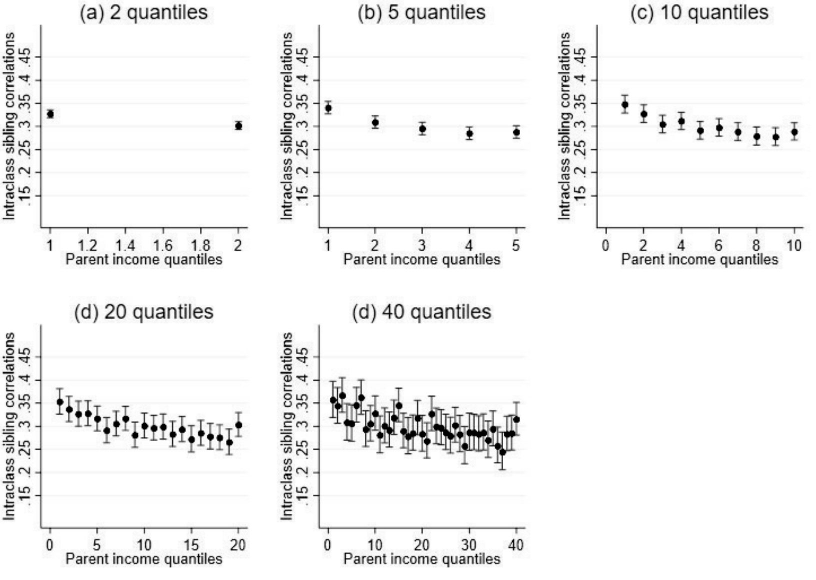
Notes: The figure shows sibling correlations in education for men by different divisions of parental income quantiles, where parents' income has been divided into 2, 5, 10, 20 or 40 quantiles.

Figure A5. Sibling correlations in cognitive skills by different quantiles of parents' income



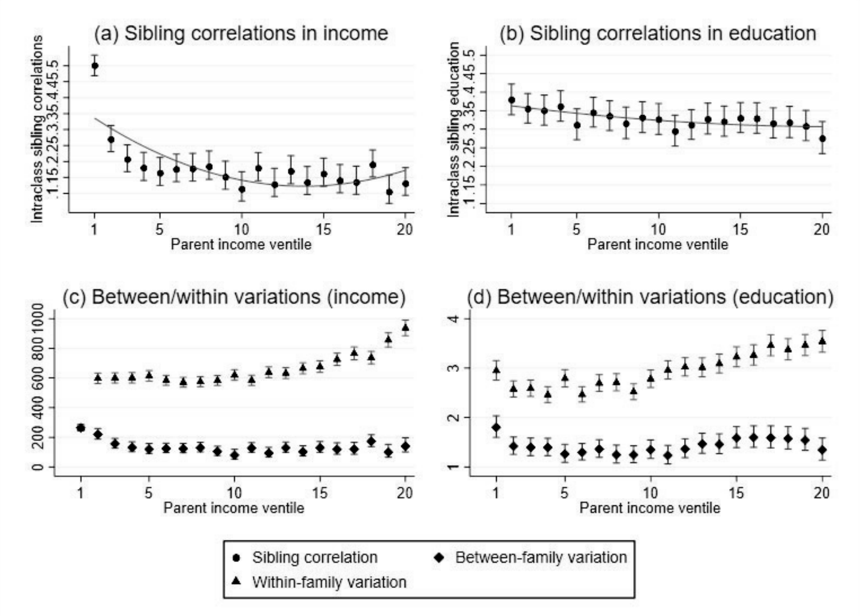
Notes: The figure shows sibling correlations in cognitive ability for men by different divisions of parental income quantiles, where parents' income has been divided into 2, 5, 10, 20 or 40 quantiles.

Figure A6. Sibling correlations in non-cognitive skills by different quantiles of parents' income



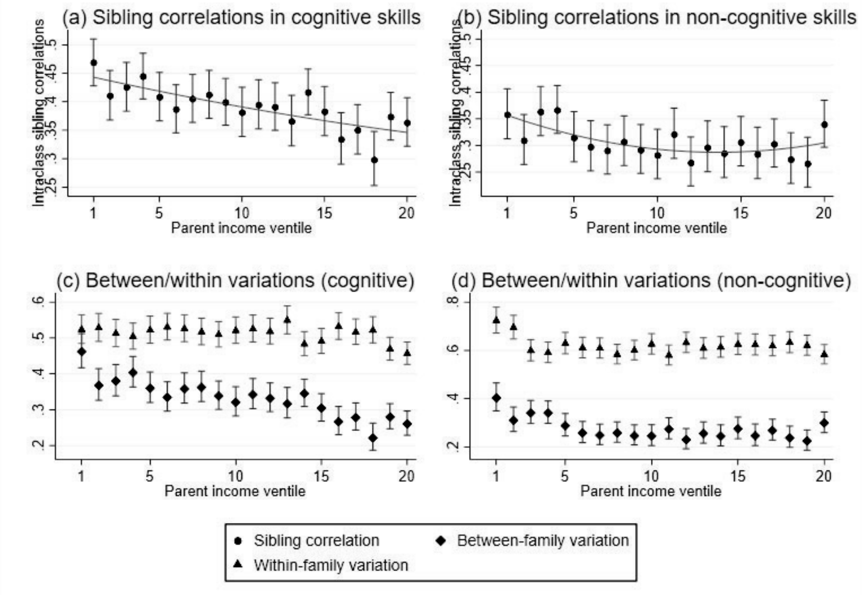
Notes: The figure shows sibling correlations in non-cognitive ability for men by different divisions of parental income quantiles, where parents' income has been divided into 2, 5, 10, 20 or 40 quantiles.

Figure A7. Sibling correlations in earnings and education by parental income ventiles (family structure held constant across parent income ventiles)



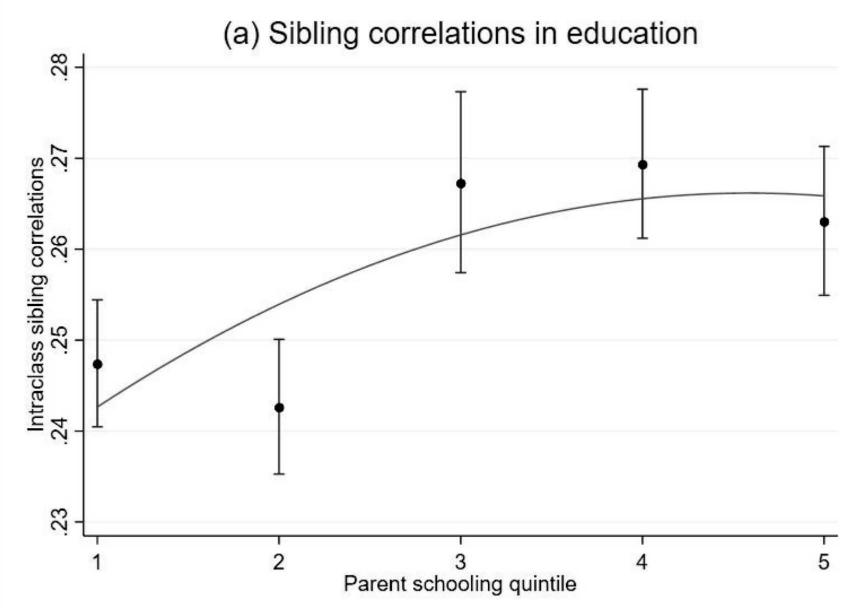
Notes: The figure shows sibling correlations in earnings and years of schooling for male siblings by parental income ventiles. The line shows a second-order polynomial fitted line for the sibling correlations. The sample is limited to families with two children with an age gap of four years or less.

Figure A8. Sibling correlations in cognitive and non-cognitive skills by parental income ventiles (family structure held constant across parent income ventiles)



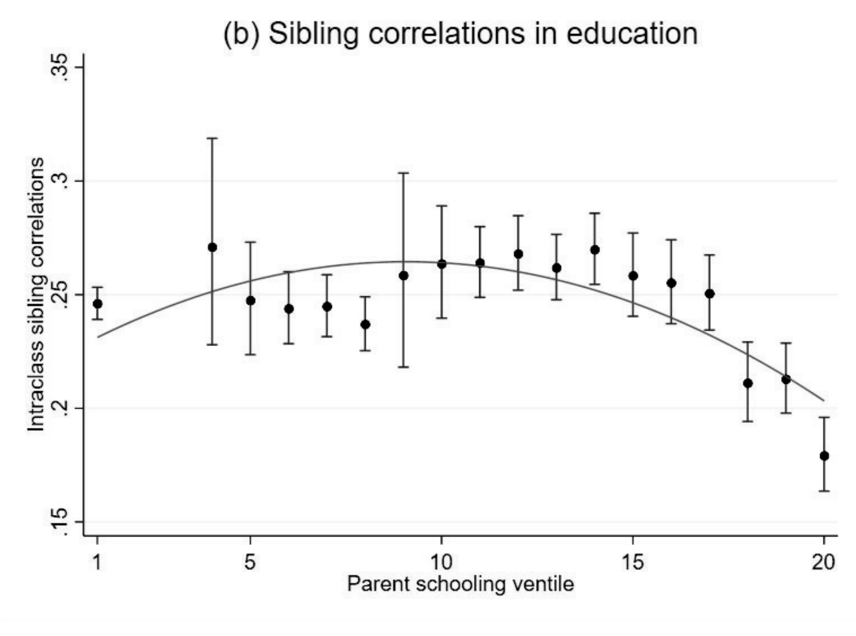
Notes: The figure shows sibling correlations in cognitive and non-cognitive ability for male siblings by parental income ventiles. The line shows a second-order polynomial fitted line for the sibling correlations. The sample is limited to families with two children with an age gap of four years or less.

Figure A9. Sibling correlations in education by parental education quintiles



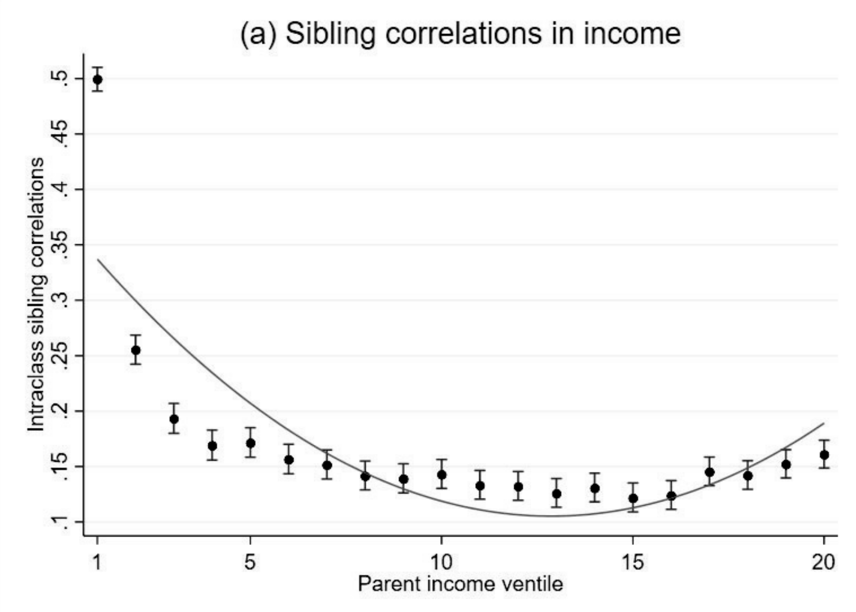
Notes: The figure shows sibling correlations in schooling measured in ranks by parental education, where education is divided into 5 quintiles. The line shows a second-order polynomial fitted line for the sibling correlations. The sample is limited to children born between the years 1945–1976 and parents born between 1930–1939.

Figure A10. Sibling correlations in education by parental education ventiles



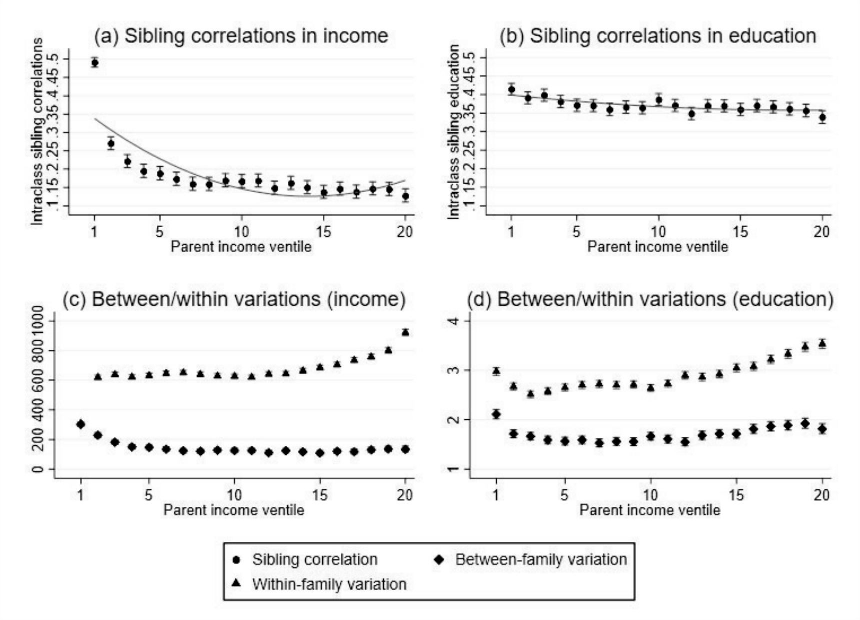
Notes: The figure shows sibling correlations in schooling measured in ranks by parental education quartile, where education is divided into 20 ventiles. The line shows a second-order polynomial fitted line for the sibling correlations. The sample is limited to children born between the years 1945–1976, and parents born between 1930–1939.

Figure A11. Sibling correlations in income by ventiles of parents' income (income measured at ages 25–30)



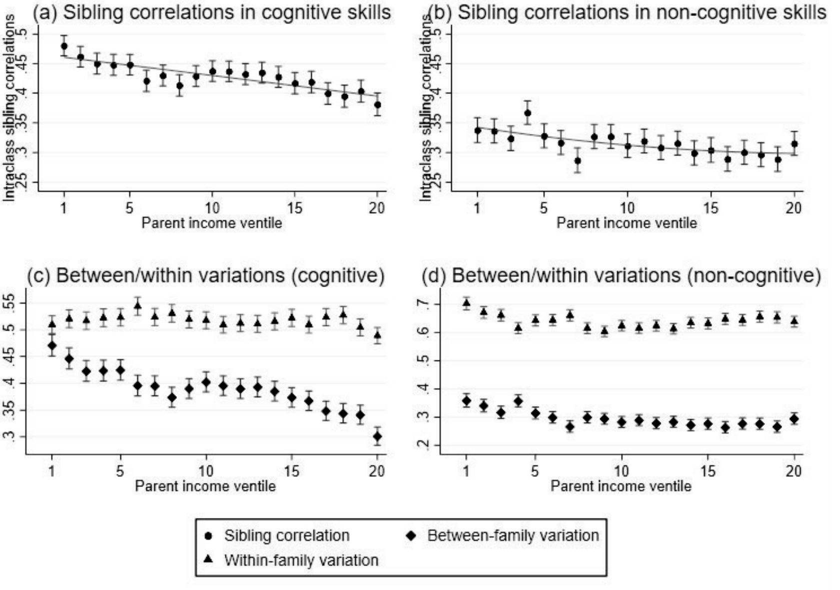
Notes: The figure shows sibling correlations in income ranks for men by parental income ventiles. The line shows a second-order polynomial fitted line for the sibling correlations. Income is measured as an average for the ages 25–30.

Figure A12. Sibling correlations in income and education by ventiles of parents' income (Extended parent sample)



Notes: The figure shows sibling correlations in income ranks for men by parental income ventiles. The line shows a second-order polynomial fitted line for the sibling correlations.The sample consist of males born between 1965 and 1982 and their parents, using income data 1968–2019.

Figure A13. Sibling correlations in skills by ventiles of parents' income (Extended parent sample)



Notes: The figure shows sibling correlations in cognitive and non-cognitive skills by parental income ventiles. The line shows a second-order polynomial fitted line for the sibling correlations. The sample consist of males born between 1965 and 1982 and their parents, using income data 1968–2019.

Essay 4: Making Background Salient: The Effects of Open Access Criminal Databases on Offender Behavior

Co-authored with Hans Grönqvist, Susan Niknami and Mårten Palme

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1 Introduction

Criminal background information is arguably more readily available to the general public today than ever before. Large online databases containing information on criminal offenders, accessible through state agencies or private background check firms, have substantially reduced the cost for citizens, employers, and landlords to access criminal records.[1] The effect of these databases on the outcomes of the offenders is heavily debated. On the one hand, it is possible that being included in criminal databases could improve the outcomes of offenders through specific deterrence (Anker et al., 2021; Chalfin and McCrary, 2017). On the other hand, the information could send a negative signal that harm rehabilitation efforts. The potential for this is supported by results in quasi-experimental studies showing that arrests and incarceration reduce formal sector employment (Mueller-Smith, 2015; Dobbie et al., 2018; Dobbie et al., 2018; Grenet et al., 2024). Because of this concern, many states in the US have implemented reforms to conceal criminal background information, e.g. through expungement (Prescott and Starr, 2020; Agan et al., 2024; Selbin et al., 2018), sealing (Mueller-Smith and Schnepel, 2021), or "Banning-the-Box" (Doleac and Hansen, 2020; Rose, 2021). While the literature has mostly focused on how employers respond to this type of information,[2] the behavioral adjustments of the offenders actually included in these databases are much less clear. Most empirical studies also struggle with the fact that having a criminal record is not random and therefore likely correlates with unobserved risk factors.

We address these issues by studying the effects of accessing criminal records within the context of the unexpected launch of Sweden's first online criminal database. The platform, introduced in January 2014, transformed the way criminal background checks were conducted by enabling the general public to anonymously search by name for individuals charged with a crime. The criminal background information is sourced from records mandated by law to be provided by courts. Unlike most criminal databases, which offer limited background information on specific crime types (e.g., sex crimes) only for some groups of offenders and target certain users (e.g., employers or landlords), this database covers all criminal charges in Swedish courts and is accessible to everyone without the need for a user profile or any costs. Search results map name to current age and place of residence and reveal whether an individual has faced a criminal charge. For a nominal fee (approximately USD 5), users can obtain the complete verdict, detailing the crime type, conviction year, sentence, and any additional circumstances of the crime. The platform rapidly

[1] This development has been particularly stark in the United States where it is estimated that the three largest background screening companies alone (out of approximately 2,000) in 2019 conducted 56 million searches (NCLC, 2019).

[2] See e.g. Pager (2003), Holzer et al. (2006), Agan and Starr (2018), Cullen et al. (2024), Agan et al. (2024), Doleac and Hansen (2020), Rose (2021), and Finlay (2008).

garnered widespread use. By 2019, when competing platforms were finally launched, searches for criminal records on the site had neared 100 million in total, averaging around 10 searches per person in Sweden.

To identify the causal effects of revealing criminal background information, we exploit a legislative rule which stipulates that courts should not disclose individual information on criminal charges dating back more than five years since the inquiry. Consequently, this legislation created a time window beyond which the company behind the platform was unable to access criminal records information. By analyzing data provided by the company we were able to determine the specific cutoff month for each court, which depend on the date when the company applied for and was later granted access to the records. We then utilize administrative data containing information on the complete criminal histories of individuals. These data enable us to distinguish between individuals charged after the specified cutoff date, resulting in their criminal records being exposed online, and non-exposed defendants who were charged prior to the cutoff date. The data also contains a wide array of labor market, educational, and demographic characteristics, which allows us to investigate behavioral responses of the defendants.

We start by verifying empirically that individuals on both sides of the cutoff date are similar in their baseline characteristics and that the pre-reform trends for exposed and non-exposed offenders are parallel. Moreover, we show that there is no evidence of bunching of cases or changes in offender characteristics near the cutoff. These findings confirm that neither courts nor offenders were able to foresee and respond to the future arrival of the platform.

We then proceed to the results from our difference-in-differences model which accounts for correlated unobservables by contrasting the change in outcomes for exposed versus non-exposed defendants. Since all individuals in our sample were charged with a crime and exposure is defined before the platform was introduced our empirical design also addresses potential concerns of general deterrence effects. The results suggest that exposure significantly decrease log earnings by 9.3 percent. This effect size corresponds to about one quarter of that from event study estimates of the effect of a criminal charge on earnings in the period before the platform was introduced. We find no significant effect on labor supply at the extensive margin or criminal recidivism. There are, however, significantly stronger detrimental effects on both labor market outcomes and recidivism in defendant subgroups: those with at least a high school degree, acquitted individuals, native born, and those either charged with a misdemeanor or living in places with a relatively low concentration of ex-criminals. These are groups of the population with arguably a relatively higher opportunity cost of engaging in crime. In many of these groups we also find adverse effects on educational performance among the children of the defendants.

When trying to identify the sources of the earnings losses we find small and mostly insignificant exposure effects on behaviors such as enrolling in labor market training programs, residential mobility, family formation, and sorting across industries based on the share of ex-offenders working in a given industry. However, we find a significant differential effect in defendant sorting with respect to the firm earnings premium estimated using the methods suggested by Abowd et al. (1999). Our results suggest that workers in the subgroups most strongly affected by the mark of a criminal record are more likely to work in firms that pay a lower earnings premia. This observation aligns with job ladder models, which suggest that workers continuously search for better job opportunities throughout their careers, gradually transitioning to higher-paying employers. In these models, pay disparities among firms reflect differences in how the surplus is distributed between workers and employers (Burdett and Mortensen, 1998; Haltiwanger et al., 2018; Lachowska et al., 2020). Within this framework, affected workers suffer earnings losses due to the impact of moving down the job ladder to employers that offer lower wages.

Our results are robust to a battery of specification checks, including changing the bandwidth for inclusion in our sample, and we also show that there is no significant effect when assigning court-specific placebo cutoff dates one year before and one year after the actual cutoff date.

Our study provides some of the very first pieces of evidence regarding the causal effect of open access to online criminal background information on the behavior of offenders. One of few exceptions is the study by Finlay (2008) who uses cross-state cross-time variation to study the labor market effects of employer access to state-provided online information on incarceration history for individuals with moderate to long prison sentences. The results show adverse employment consequences in states with open records policies.[3] Our study differs in that the database includes the universe of individuals charged with a crime regardless their sentence and type of crime committed. This is important since, for instance, the ciminal records of felony conviction may have different consequences for defendants, relative to criminal records of misdemeanor charges. We are also able to document the effects for a wide range of outcomes as well as probe underlying behavioral mechanisms.

Our results also add to the literature on the impact of criminal background checks from the employers' perspective.[4] This body of literature either studies the callback rate to fictitious job applications signaling criminal background or examines policies intended to influence the hiring practices of employers. A common finding in the former line of research is that employers are averse to calling back or hiring job applicants with criminal records (Pager, 2003;

[3]Finlay (2008) studies a sample of individuals included in the National Longitudinal Survey of Youth (NLSY), in which 369 offenders had received prison sentences for a minimum of one year.

[4]These studies are typically motivated by the fact that individuals with a criminal record are more likely to be unemployed. For instance, Bushway et al. (2022) show that 46 % of unemployed men in the United States have been convicted. Other studies include e.g. Western (2002) and Freeman (1992).

Holzer et al., 2006; Agan and Starr, 2018; Uggen et al., 2014; Cullen et al., 2024).[5] The latter line of research primarily focuses on Ban-the-Box (BTB) legislation, which restricts inquiries about criminal histories on job applications until later in the hiring process. It often relies on state-level variations in these laws to address correlated unobservables. Doleac and Hansen (2020) demonstrate that these policies decrease the probability of employment by 3.4 percentage points for young, low-skilled black men, suggesting that in the absence of information about an applicant's criminal history, employers statistically discriminate against demographic groups with higher proportions of ex-offenders. Rose (2021) finds that a 2013 Seattle BTB law had minimal effects on ex-offenders' labor market outcomes, indicating that employers may delay background checks until the final stages of the hiring process or that ex-offenders may gravitate towards jobs where clean records are not obligatory.[6] While the responses of employers to criminal background information represents a potentially important mechanism, the setting we consider allows us to investigate the effects of criminal background information that is much more widely available also to the general public.

A few recent studies examine policies that not necessarily only target employers. Agan et al. (2024) study the labor market impacts of retroactively reducing felonies to misdemeanors in San Joaquin County, CA where criminal justice agencies reclassified certain theft and drug offenses from felonies to misdemeanors, without requiring input or action from affected individuals. The results reveal no discernible benefits among individuals whose records were reduced proactively.[7] Mueller-Smith and Schnepel (2021) study diver-

[5]For instance, Pager (2003) demonstrates that disclosing a criminal record to employers who do not explicitly request such information has minimal impact on hiring decisions. Agan and Starr (2018) reveal a significant six-fold increase in callback rates for white-to-black applicants following the implementation of Ban-the-Box legislation in New Jersey and New York. Cullen et al. (2024) conduct experimental research to assess strategies for increasing the demand for workers with criminal records. Their study involves presenting hiring managers with hypothetical scenarios that influence whether workers with criminal records can accept job offers in the future. Results indicate that initially, 39% of employers are open to hiring individuals with criminal records. However, this figure rises to over 50% when managers are offered incentives such as crime insurance, a single performance review, or a limited background check covering only the past year.

[6]Furthermore, Shoagii and Veugeriii (2016) estimate the effects of BTB by considering impacts on residents of high-crime versus low-crime neighborhoods. The results show positive effects on employment in high-crime neighborhoods, which the authors rationalize by minority men may benefit from the law overall, despite negative impacts on some sub groups as suggested by Doleac and Hansen (2020). Jackson and Zhao (2017) use unemployment insurance records to study a 2010 BTB reform in Massachusetts in a difference-in-differences framework. Their results suggest that BTB lowered ex-offender's employment by 2.4 percentage points and quarterly earnings by USD 300.

[7]Agan et al. (2024) also conduct a field experiment notifying a subset of individuals about their proactive reduction also showing null results, implying that lack of awareness is unlikely to explain the findings.

sion programs in Harris County, Texas, providing first-time felony defendants with an opportunity to circumvent a criminal record either by reclassifying drug and property offenses or by diverting low-risk defendants. Their findings suggest that these programs significantly reduce recidivism rates and improve quarterly earnings by about 50 percent. We add to this literature by studying an intervention that not only affected certain groups of felony offenders but the universe of individuals charged with a crime. We also contribute by using data that allows us to shed light on the sources underlying the labor market penalty of having a criminal record.

Importantly, our results are connected to an extensive literature that examines the effects of criminal justice sanction strictness on specific deterrence, often finding mixed evidence.[8] However, while the formal sanctions studied in this literature (e.g. incarceration or community service) are the criminal justice system's primary tool to discourage deviant behavior, many scholars also argue that informal social sanctions attached to the stigma of being marked as a criminal may potentially also act as a crime deterrent (Chalfin and McCrary, 2017; Zimring et al., 1973; Nagin and Pogarsky, 2003; Weibull and Villa, 2005; Funk, 2004).[9] In this case, conveying socially valuable information on criminal background may actually be an efficient low-cost strategy for society to combat crime.[10] On the other hand, the criminal labeling theory, one of the most fundamental models in criminology, predicts that exposing criminal background will amplify offending behavior. According to this view, interactions with the criminal justice system labels the offender a criminal, which leads him to internalize stigmatizing attitudes and conform to a deviant identity (e.g. Becker, 1968; Lemert, 1967).[11] However, since a conviction almost always involves at least one sanction, it has proven almost impossible to separate informal sanction costs from the imposed formal ones (Chalfin and

[8]Specific studies include but are not limited to: Agan et al. (2023), Abrams (2012), Aizer and Doyle Jr (2015), Barbarino and Mastrobuoni (2014), Bhuller et al. (2020), Buonanno and Raphael (2013), Dobbie et al. (2018), Dobbie et al. (2018), Drago et al. (2009), Garin et al. (2023), Helland and Tabarrok (2007), Hjalmarsson and Lindquist (2022), Kessler and Levitt (1999), Kling (2006), Kuziemko (2013), Lee and McCrary (2017), Mueller-Smith (2015), Owens (2009), Grönqvist et al. (2022), Ouss et al. (2023), Rose and Yotam (2021), and Raphael (2014).

[9]In fact, in the Becker (1968) economic model of crime, informal sanction costs alone can fully predict criminal behavior if individuals are more sensitive to changes in the probability of apprehension, thus acquiring a criminal record, than to changes in the sanction regime (Chalfin and McCrary, 2017; Nagin, 2013).

[10]See e.g. Mungan (2016), Nagin (2013), Polinsky and Shavell (2007), Kahan and Posner (1999), and Rasmusen (1996).

[11]The concept of labeling is also evident in the economic model of crime. Once an individual is labeled a criminal, the opportunity cost of refraining from crime may decrease, as potential employers may limit their access to job opportunities (Agan and Starr, 2018; Becker, 1968; Grogger, 1995; Holzer et al., 2006).

McCrary, 2017).[12] This is important as it is fully possible that these mechanisms operate in different direction.

A few recent studies provide quasi-experimental evidence from contexts that arguably approaches distinguishing between these effects. Agan et al. (2023) estimate the causal effects of misdemeanor prosecution on defendants' subsequent criminal justice involvement. The analysis exploits the quasi-random assignment of nonviolent misdemeanor cases to District Attorneys in Suffolk County, MA, who vary in their leniency of prosecuting decisions. The results show that nonprosecution of a nonviolent misdemeanor offense leads to a 53 percent reduction in the likelihood of a new criminal complaint. These effects are largest for defendants without prior criminal records, suggesting that averting criminal record acquisition is an important mechanism. Kamat et al. (2024) use a random-judge design and show that misdemeanor convictions cause an increase in the number of new offenses committed over the following five years while incarceration on more serious felony charges has no effect after release. The study by Mueller-Smith and Schnepel (2021) on diversion programs arguably also approaches distinguishing between these effects. While these studies compellingly empirically rule out many alternative explanations the authors are also clear that there could also be other mechanisms aside from stigma that the data do not allow them to eliminate.[13] To our knowledge, no prior study has employed a quasi-experimental research strategy where the empirical approach by design is capable of identifying the impact of informal sanctions net of that of formal sanctions.

The rest of this paper is structured as follows. Section 2 describes the institutional background. Section 3 discusses the data and sample construction. We outline the empirical strategy in Section 4. The results are provided in Section 5 followed by conclusions in Section 6.

2 Background

In most countries, criminal background checks serve as a key tool utilized by various actors such as employers, landlords, and higher education institutions to address concerns surrounding public safety and the prevailing notion that individuals with criminal histories may be less inclined to fulfill their responsibilities. Additionally, these checks are often employed to mitigate potential liability, particularly in the realms of negligent hiring or negligent renting doc-

[12]Most empirical studies compare offenders either convicted or incarcerated with those not formally sanctioned e.g. Bernburg and Krohn (2003), Farrington et al. (1978), and Murray et al. (2017).

[13]For instance, selective out-migration, re-sentence deterrence connected to the original offense and changes in monitoring by law enforcement in response to defendants' observable criminal histories could in some cases not fully be discarded.

trines.[14] The criminal background information tend to be accessible through various channels, including direct provision by states and an increasing number of private companies, especially in the United States, offering background search services.

The databases used for the records checks compile data from diverse origins, often procured in bulk from public sources such as law enforcement agencies, state courts, or through web scraping technology from public websites (NLCS, 2019). The checks tend to reveal current criminal charges or past arrests, but may also encompass non-conviction data, including records of police interactions, mere allegations, withdrawn or stayed charges, as well as acquittals.[15] A background check typically contains limited details about listed offenses but generally includes the individual's name associated with the record, the jurisdiction of origin, the record's creation date, and a case number or law enforcement identifier (NLCS, 2019). While a brief description of the offense, such as "possession of marijuana", may be provided, details about the offense or related circumstances are usually omitted. Some public sources only display criminal convictions, whereas private firms may report arrests for misdemeanors that later result in dismissal (Bushway and Sweeten, 2007).

2.1 The Swedish setting

The regulations governing the request of criminal records in Sweden have traditionally been stringent, primarily for employment-related purposes (Backman, 2012). Employers are strictly limited in their ability to request a candidate's criminal record, typically permitted only at the concluding stages of the hiring process. Subsequently, the job applicant is required to formally request his criminal record from the police, who dispatches it in a sealed envelope. Upon receipt of the record, the applicant must then forward the envelope directly to the employer. This protocol often results in a significant time delay, with the entire process spanning several weeks. It is therefore not surprising that survey data from a large sample of employers collected in 2011 indicates a relatively low utilization of criminal background checks by employers in Sweden, with only approximately 14 percent of the 1,200 employers reported to have engaged in such practices (SOU, 2011). This conclusion is also supported by Figure A2 showing that only about two percent of the job ads posted on the Public Employment Office's in 2011 mentioned criminal background checks. Consistent with this, the same year the ratio of the number of official

[14] About 94 percent of employers in the United States conduct some form of criminal history check, and about 90 percent of landlords run background checks on prospective tenants (NLCS, 2019). In 2004, a survey from the United States showed that about 50 percent of the employers check the criminal background of job applicants (Holzer et al., 2006)

[15] Several US states have laws prohibiting the use of arrests that did not result in convictions in employment screenings, e.g. https://ccresourcecenter.org/state-restoration-profiles/50-state-comparisoncomparison-of-criminal-records-in-licensing-and-employment/

background checks to the number of firms was 17 percent. Since then, there has been a slowly growing trend towards increased use of official background checks.

The information provided in the official criminal background checks is expunged after 3-10 years. The exact amount of time depends on factors such as the age of the offender when committing the crime and the sentence.

2.2 The database

In January 2014, the online platform Lexbase completely transformed the landscape of criminal background checks in Sweden. Instead of targeting solely employers, the platform offered unrestricted access to criminal records for the general public. The database allowed anyone to anonymously search for charged individuals by name, leveraging data sourced from court records mandated by law to be provided upon request. The check maps the name of the individual to current age and place of residence and reveals whether that specific person has been charged with a crime. This means that even for common names it was typically possible to uniquely identify an individual.

Unlike many other online criminal databases, which often specialize in specific crime types and cater exclusively to certain actors like employers or landlords, the platform presents a comprehensive repository of every criminal charge in Swedish courts, without any fees or user profile. For a nominal fee, at the time approximately USD 5, registered users could also obtain full court proceedings, gaining access to information about the number of charges, conviction and crime date, type of crime, circumstances surrounding the offense, the sentence, and the address and personal id number (cf. social security number) of the individual. However, acquiring the complete court proceedings seems to have been a rare occurrence. Data from the database's company indicate that 0.16 % of all background searches actually resulted in a purchased court proceeding in the first year (see Appendix Figure 6). When it was introduced, there was no information on the website that indicated that the database also included acquittals, only that it contained the records of individuals convicted in court.[16]

[16] The following text is a translation of the official press release from the company sent on the day of the launch: "All criminal convictions from the last five years are now easily searchable - New unique site is launched today. Today, a beta version of a new site, Lexbase.se, is being launched, which makes all criminal convictions, five years or younger, easily searchable. Lexbase will be Sweden's largest database with legal information and a new ground-breaking service on the Swedish market." (https://www.lexbase.se/site/press/21?s=285b36b14745ae4aedb1d218ea9989c4). Reports in the media also highlighted that only criminal convictions were included. For instance, the following text is a translation of parts of a report in one of Sweden's largest newspapers called Expressen. "At Lexbase you could find out everything about the convictions of your friends, neighbors and acquaintances. Is the neighbor convicted of rape? Or maybe just for speeding?

Appendix Figure A4 show the homepage of the platform and Appendix Figure A5 shows the different stages in the search process.

The platform immediately received large media attention and gained immense popularity. Data provided by the company behind the platform show that 32 million background checks were conducted in just the first year. Appendix Figure 6 shows that by 2019 when the first competing platform was introduced, the cumulative number of searches had soared to 103 million. Appendix Figure A6 shows the sharp increase in the number of media articles written in printed pres mentioning the database. Appendix Figure A7 shows that from the week after its inception and up until 2019, Lexbase consistently outnumbered other popular Google search terms in Sweden, surpassing common queries such as "new car" or "best restaurants".

Since the platform did not have any competitors until 2019, the massive use of the database cannot be explained by a shift in the use from other similar platforms. It is also seems unlikely that employers substituted away from official background checks by the police to instead using the platform. First, the introduction of the platform did not coincide with any meaningful shift in the long-run trend towards increased use of official criminal background checks (see Appendix Figure A2). Secondly, the demand for regular background checks was relatively low among employers. As discussed earlier, a large survey of 1,200 firms just before the introduction of the platform revealed that 14 percent of employers reported that they check criminal records (SOU, 2011) and only two percent of the job ads posted on the Public Employment Office's mentioned criminal background checks. Third, the number of official background checks, 171,229 in 2011, is completely dwarfed by the about 20 million of searches conducted every year on the platform. Taken together, the large use of the platform is most likely explained by a high demand for the service in the general public.

The platform was closed down by the Internet service provider just two days after its launch, after the site came under heavy criticism from Swedish media and the general public for making criminal records too easily accessible. However, since April 2014 the platform has been fully operational. Several attempts by the Swedish government to shut town the platform have failed because the information is protected by the Swedish constitution.

The only thing you needed to find the convictions was their name or social security number. The convictions were exposed as red dots on a map and could then be downloaded for a small fee. The new site was launched at a high-profile press conference on Monday. Already on the same day, lots of curious people started logging in, while harsh criticism was immediately directed at Lexbase. And immediately the first reports from desperate people came in to the Chancellor of Justice (JK). "I was so sick I couldn't go to work. I'm in that database. I've done everything to get back to a normal life and have been holding a job for the past two years. If someone searches for me, I'll lose my job immediately. You have to do something about this. My neighbors can see my house on a map with a red dot. I'm totally stressed out now by this! I feel so damn bad I might kill myself," one man writes to JK ." (https://www.expressen.se/nyheter/deras-liv-forstors-av-lexbase/)

While the information provided by the database in theory was accessible through public records even before 2014, the process was laborious, necessitating visits to multiple courts to potentially find a matching name. Just as in the case of most other online criminal databases, the introduction of this particular platform thus represented a significant reduction in the costs of accessing criminal background information, especially for the general public.

2.3 Mapping to empirical design

The company behind the platform was officially established as a business entity in 2012. During the two years preceding the public launch of the database, the company began acquiring criminal background data through direct engagement with individual courts, requesting access to criminal charges stored in the digital administrative system known as Vera. A government decree mandated courts to grant access to Vera to anyone seeking information on criminal charges for a given individual.[17] However, after five years, courts should expunge the information.[18] The courts had never before processed this type of large scale request for the universe of criminal cases. Together with some uncertainties surrounding the exact details regarding the expungement procedures, this created some variation among courts in the way they were able to respond to the request. For instance, while some courts were not able to strictly adhere to the exact expungement period, other courts not only delivered criminal cases but also civil law suits.[19] In the end, the request resulted in distinct cutoff dates for each court, marking the point at which criminal charges were integrated into the database. The specific cutoff date was determined by both the timing of the company's request for information and the length of time it took for each court to process the request and then deliver the records.

To identify the cutoff dates, we obtained data from the company detailing the number of cases added to the database for each court and month. Utilizing our comprehensive micro data covering all criminal charges in Sweden since 1986, we calculated the share of cases included in the database. Our analysis comprises the 46 courts that operated during the period preceding the database's launch (i.e., 2006-2013). For each court j and month t, we computed the change in the share of cases by calculating the difference between the average share of cases in the preceding 12 months and that of the subsequent 12 months. The cutoff date was identified as the month exhibiting the

[17] See decree 2001:639)

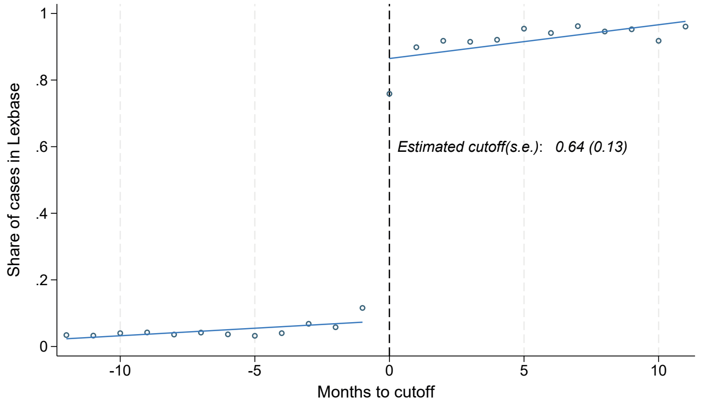
[18] The courts were still obliged to preserve cases in printed form. In order to search for a specific case a person had to visit the court to manually search for the case number in a volume of books that compiles all the cases that have been processed by the court.

[19] For instance, in 2013 the Swedish Authority for Privacy Protection criticized DC Västmanland for not following the stipulated timeline (DNR 1319-2013). A similar statement was made for DC Attunda (DNR 1316-2013). Report 2012:1 by the Swedish Authority for Privacy Protection provide a detailed discussion about these issues.

largest increase in the share of cases over the entire period. In the empirical analysis we focus on the 32 courts with observed cutoff dates before 2010 to ensure the representation of all courts in our sample within the database at its launch.[20] Additionally, we excluded four courts with no clear evidence of cutoff.[21]

While approximately 5-6 years passed between the initial charge and the release of the platform, the open access version of the database did not include the date of the charge. This omission meant that most employers, landlords, and ordinary citizens could not distinguish between charges based on their timing unless they actively registered on the platform and purchased the sentencing information. Consequently, from the perspective of most public agents, the launch of the platform provided new information about the individuals listed in the database.

Figure 1. Change in the weighted share of cases around the cutoff



Notes: The figure plots the weighted average share of Lexbase cases across all district courts in our sample within a 12 month window around the court-specific cutoff date

Figure 1 shows the average share of cases included in the database around the cutoff for the courts in our sample. Consistent with not all courts strictly adhering to the stipulated timeline for expungement, the cutoff is not sharp. However, there is a clear discontinuity in the share of cases with the an estimated size of the cutoff of 0.64 (0.13).[22] [23] When interpreting this figure it should be noted that the estimates are affected by measurement error. First, our register based micro data do not include the complete number of cases that

[20] It is unclear whether all courts were actually included in the database at the time it was launched. Thus, it is possible that some courts were added at a later point in time.

[21] These courts, which represent 7.5 percent of the total number of cases, are DC Umeå, DC Gällivare, DC Haparanda, DC Värmland.

[22] The estimated size of the cutoff is 0.68 (0.12) when not weighting by the size of the courts as measured by the number of cases.

[23] Figure A8 show the cutoff for each court in our sample.

exist in Lexbase since we miss information on individuals that are lacking a personal id number. Most of these individuals are asylum seekers. Second, in some cases, courts accidentally delivered not only criminal cases but also civil cases that were not made searchable on the platform.[24]

3 Data and sample construction

3.1 Administrative data

Our empirical analyses rely on micro data that originate from various administrative registers managed by Statistics Sweden. These registers contain information on the entire Swedish population aged 16 to 65, spanning the years from 1990 to 2016. The data holds demographic and socioeconomic information such as age, place at birth, gender, educational level, employment and income measures. To ensure comprehensive tracking of labor market outcomes for all individuals in our sample, we restrict the sample to individuals younger than 55 in the year of the initial charge.

These data have been linked to the Swedish Conviction Register maintained by the National Council for Crime Prevention (*Brottsförebyggande rådet* - BRÅ). Within these records, we have access to comprehensive details concerning criminal charges during this period. The data include information on the date of the conviction as well as the sentence. This information pertains to criminal charges in Swedish district courts, which is the court of first instance. A single criminal charge may encompass multiple crimes, and we observe all crimes within a given charge.[25] The data also include information on acquittals.

Appendix B describes how the variables in our analysis are constructed.

3.2 Classifying offenders

Utilizing the court-specific cutoff dates, we categorize defendants by exposure statuses employing a 12-month window. Defendants charged within 12 months after this date are classified as exposed, while those charged up to 12 months prior to this are categorized as non-exposed. We focus on first time defendants to ensure that each individual will only appear once in our estimation sample. In addition, this group is particularly important from a policy perspective as non-disclosure interventions are likely to have a greater chance to succeed for individuals before they have accumulated significant criminal

[24] Data provided by the company behind the database show that 20.81 percent of the total number of records delivered were civil cases.

[25] The data exclude minor offenses such as speeding tickets but include offenses such as driving without a license and DUI.

experience. This is also the reason why non-disclosure programs typically target first-time defendants.

Table A1 columns (1) and (2) shows descriptive statistics for our sample separately for exposed and non-exposed defendants. These baseline characteristics are measured in one year before the initial trial. As expected, the background characteristics of the individuals are very similar in the two groups. We can also see that there are striking similarities in terms of the case characteristics and the sentence. This does not only show that the covariates in the two groups balance but also that our research design should be able to identify the role of stigma net of that from formal sanctions.

4 Research design

In our main analysis, we estimate the following conventional difference-in-differences model where we compare the outcomes of exposed and non-exposed individuals before and after the database was introduced

$$Outcome_{it} = \alpha + \beta \text{exposed}_i * \text{post}_t + \text{exposed}_i + \gamma_t + \lambda_c + \delta X_{it} + \varepsilon_{it} \qquad (1)$$

where $Outcome_{igt}$ is the relevant outcome of defendant i in calendar year t. $exposed_i$ is an indicator variable set to one for individuals who were charged after the cutoff date and zero otherwise; $post_t$ is an indicator variable set to one for years when the database was in place and zero otherwise. $exposed_i$ and court (λ_c) fixed effects control for persistent unobserved heterogeneity between exposed and non-exposed individuals as well as between courts. X_{it} is a vector of controls for baseline characteristics such as age, gender and immigrant. It also controls for time since the charge and seasonality in criminal charges by including calendar month fixed effects.[26] Since the reform affected everyone at the same time and remained in effect, the concerns typically associated with staggered difference-in-differences research designs (Roth et al., 2023) do not apply to the current setting.

This strategy relies on the standard assumption of parallel trends: the outcomes should not have evolved differently between exposed and non-exposed offenders in the absence of the database. We assess this by inspecting the estimated β coefficients for the years leading up to the launch of the platform.

The other main assumption is that of no anticipatory effects, which is violated if, for instance, courts or offenders somehow were able to foresee the introduction of the database and the exact cutoff date in each court. In this case, we would expect to see a change in the frequency of court cases around the cutoff date. For instance, courts might choose to have the trial after the cutoff date to ensure that an offender gets exposed in the database. We might

[26] There are typically fewer cases processed by courts in the summer and around Christmas.

also expect to see changes in the characteristics of offenders around the cutoff if some offenders, for instance, more highly educated ones, are better able to predict the future arrival of the database. In this case, there should be an increase in the share of highly educated offenders just before the cutoff.

We already saw in Table A1 that the characteristics of exposed and non-exposed offenders are very similar. To examine this in more detail, we estimated the change in the frequency of court cases and in the characteristics of the offenders within a one-year window around the court-specific cutoff date. Figure A1 shows no evidence of such changes for the average number of cases in the courts and the predicted risk of recidivism.[27] These findings are, of course, as expected, as it is hard to think of a scenario where courts could predict the (timing of) arrival of a currently non-existing platform of this type, less which the cutoff dates would be.[28]

5 Results

This section presents the results from our empirical analysis. We start by providing the main results and robustness. We then show results for relevant subgroups of the population. We end by examining some of the underlying mechanisms.

5.1 Main results

Table 1 shows the difference-in-differences estimates of the effect of exposure on criminal behavior and labor supply.

We can see that exposure has no statistically significant exposure effect on the probability that the individual is (re-)convicted. We can also see that there is no significant effect on employment. The estimates are relatively precise, ruling out larger increases in the probability of conviction than 0.9 percent

[27]We calculate our measure of recidivism risk from the predicted dependent variables for each individual from the following OLS regression

$$\text{Convicted}_i = \alpha + \beta X_i + \varepsilon_i$$

where X_i is a vector of the following baseline defendant characteristics, age, immigrant, educational attainment, employment, log earnings. The risk score is then defined as

$$\hat{\alpha} + X_i\hat{\beta}$$

and captures a defendant's risk of recidivism over a five year period. Appendix Figure A10 shows similar results when estimating the model separately for each individual characteristic included in the prediction.

[28]Consistent with this, Appendix Figure A2 shows that there was no mentioning of Lexbase in Swedish media before Jan 2014.

Table 1. *Main Results*

	Convicted		Employed		Log earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed*Post	0.003	0.003	-0.007	-0.008	-0.093**	-0.093**
	(0.003)	(0.003)	(0.008)	(0.008)	(0.042)	(0.042)
Sample mean	0.088	0.088	0.666	0.666	5.835	5.835
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes
Time since charge FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	No	Yes	No	Yes	No	Yes
Observations	186,840	186,840	186,840	186,840	186,840	186,840

Notes: This table reports difference-in-differences estimates based on model (1) for crime and labor market outcomes. Exposed is an indicator variable set to one for defendants sentenced up to 12 months after the court-specific cutoff date; zero otherwise. Post is an indicator variable set to one for the years the database was in place (i.e. 2014 and onwards); zero otherwise. The regressions control for age, gender, immigrant and the main effect of being exposed. Numbers in bracket show the mean of the dependent variable. Standard errors in parenthesis are clustered at the individual and court level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

points and larger decreases in the probability of being employed by 2.4 percentage points with 95 percent confidence. This corresponds to about one third of the causal effect of incarceration on the probability of being employed.[29] We do, however, find a that exposure leads to a statistically significant decrease in log earnings by about 9.3 percent.[30] While we cannot rule out adjustments in terms of hours worked, the absence of an effect on labor supply at the extensive margin provide suggestive evidence that open access to online criminal databases may primarily hurt the outcomes of defendants by reducing wages.

Consistent with the strong similarity in baseline characteristics shown in Table A1 the estimates are almost completely invariant to adding baseline controls to the regressions. Figure A11 plots dynamic difference-in-differences

[29] Dobbie et al. (2018) estimate the causal effect of incarceration on the probability of being employed in Sweden to be -7.6 percentage points.

[30] We replace zero earnings with 1 before taking logs. The results are similar when instead estimating the model using the inverse hyperbolic sine.

estimates that mimics those in Table 1 by year relative to 2013.[31] We find no evidence of differential pre-trends.

To further explore how exposure impacts labor market outcomes, Appendix Figure A12 plots the difference-in-differences estimates based on model (1) and corresponding 90 percent confidence intervals of the effect of exposure on the probability of a defendant's earnings falling above various thresholds. The impact of on earnings is concentrated in the left tail of the earnings distribution, with little to no effect on the probability of earning above higher thresholds than the 80th percentile. These results suggest that exposure primarily affects earnings at the low to mid-end of the earnings distribution.

The exposure effects on earnings are smaller than those shown by Mueller-Smith and Schnepel (2021) who find that diverted felony defendants in Harris County, TX experiences increases in total earnings over the ten-year follow up period grow by 93 percent and improvements in quarterly employment rates by 49 percent. The effect is also smaller than those reported by Finlay (2008) who show that ex-prisoners in US states with open records to online criminal databases experience 18.7 percent reductions in earnings. On the other hand, Agan et al. (2024) find no significant benefits among individuals whose records were reduced proactively in San Joaquin County, CA.

To which extent can differences in the results be explained by the specific Swedish context? We examine this question by following Rose (2021) and estimate the following event study specification to examine the labor market impact of a criminal conviction

$$Y_{it} = \alpha_i + \gamma_t + \beta X_{it} + \sum_{\mathrm{s} \in [-5,5]} \delta_s \gamma D_{ist} + \varepsilon_{it}, \tag{2}$$

where Y_{it} is employment or log earnings for individual i and year t, α_i is an individual fixed effect, γ_t is year fixed effects, X_{it} is a vector of time-varying annual age dummies, an D_{ist} is 1 when individual i is s quarters from his first conviction. We use dummies for $s \in [-5,5]$ to estimate 5 years of dynamic effects focusing on individuals convicted for the first time between 1997 and 2010 so that the outcomes are observed for at least 5 years before and after conviction.[32] We take advantage of the fact that our data provides information on the full population of individuals charged with a crime. While the

[31] We estinate the following dynamic difference-in-differences model

$$Outcome_{i,g,s} = \alpha + \sum_{\substack{k=2011 \\ k \neq 2013}}^{2016} \beta_k \cdot 1\{exposed\} \cdot 1\{s=k\} + \gamma \cdot 1\{exposed\} + \theta_s + \beta X_{i,g,s} + \varepsilon_{i,l,s},$$

where the outcome $Outcome_{i,g,s}$ is observed for individual i, belonging to exposure group g in year s.

[32] We use +/- 7 years and bin the end points after +/- 5 years to be able to identify both the age and year fixed effects.

results should primarily be interpreted as a descriptive comparative exercise, the model controls for permanent unobserved heterogeneity using individual fixed effects. The results for the two-way fixed effects model are reported in Appendix Figure A13 and the results using the procedure by Sun and Abraham (2021) are reported in Appendix Figure A14.

The results show a sharp reduction in both employment and earnings after the initial charge. The drop in earnings is about 38 percent one year after the charge. This effect size is similar to that reported by Rose (2021) who finds earnings losses by 44 percent for individuals convicted of a felony and about 26 percent for individuals convicted for a misdemeanor.[33]

5.2 Further robustness checks

Table A2 provides results from further robustness checks. It is possible that unobserved seasonal breaks that potentially coincide with the timing of our cutoffs could confound with our results.[34] While we did not see any evidence of differences in the composition of crime types or sentences between exposed and non-exposed defendants in Table A1, Table A2 provides additional support for the absence of seasonal confounders. The table presents the estimated effects of a placebo experiment where we adjust the cutoff dates to one year earlier or one year later than the actual court-specific cutoff dates. Panels A and B show that there are no significant discontinuities in these placebo exercises.

Panels C and D show the sensitivity of our results for decreasing the bandwidth for individuals included in the treatment and control group to +/-9 months and +/-6 months. We can see that, while the sample size decreases considerably, the estimates for log earnings are similar to baseline albeit slightly larger. The other estimates are also similar and not significantly different from baseline.

There are at least two reasons to believe that our exposure effects represent a lower bound of the actual effect of being listed in the database. First, it is possible that some of the defendants that we classify as non-exposed re-offended in the period between the initial charge and the introduction of the platform and therefore also were exposed. Second, society may place less emphasis on offenses that occurred in the distant past, as is the case in our setting. However, given that the recidivism rate within our sample—comprised of first-time offenders—is relatively low, and that the date of the charge is not revealed unless the full verdict is purchased, we anticipate that such bias is limited.[35] We

[33] As another comparison, Grenet et al. (2024) show that avoiding a prison term by substituting from prison to electronic monitoring in Sweden increases log earnings by 22.1 percent.

[34] For instance, during holiday seasons courts may choose to process more urgent cases and postpone less serious ones.

[35] While the one-year recidivism rate is 8.8 percent for first-time defendants it is 26.1 percent among multiple-time defendants.

empirically assess the role of this “bias” by taking advantage of the fact that the database excluded confession cases in the first two years after its launch. Confession cases generally involve less severe crimes and entail a defendant’s admission of guilt, with the prosecutor, rather than a court, overseeing these cases. This institutional feature provides a way to examine whether the immediate exposure effects are different from the intermediate exposure effects. Panel E shows estimates from this alternative difference-in-differences model where the "exposed" group includes defendants whose cases where processed in court, and the "non-exposed" group comprises defendants with a confession case. In this analysis we measure the outcomes one-year post-conviction and the repeated cross-sectional dataset runs from 2010 to 2015. All regressions control for the same set of covariates as in model (1). While the results in Panel E are similar to our baseline specification, these results should, be interpreted with some caution as confession cases represent special types of crimes.[36] [37]

5.3 Heterogeneity

Having established that our results are not sensitive to changes in the specification or sample we next examine heterogeneity in the effects of exposure based on two different criteria: (i) defendant baseline socioeconomic and demographic characteristics, (ii) case characteristics. The first criterion is motivated by the observation that the effect of criminal justice interactions often vary based on labor market attachment and demographics; e.g. Dobbie et al. (2018), Garin et al. (2023), Jordan et al. (2023), Rose (2021), and Doleac and Hansen (2020). The second criterion is motivated by providing additional insights about the role of stigma.

Defendant characteristics

Table 2 presents results from a variation of model (1) where we interact exposed*post with baseline defendant characteristics measured in the year before the initial trial. In this exercise we use indicators for male, above age 25 (the median age in our sample), immigrant and whether the defendant holds a high school (HS) degree. Each panel presents the results for a different dependent variable. Column (1) recapitulates our baseline estimates from Table 1.

[36]This analysis is potentially also affected by general deterrence effects if the existence of the database reduced the likelihood of engaging in crime in the general population.

[37]As an alternative to model (1) we also estimated a regression discontinuity in time design (RDiT) where the running variable was time to the court-specific cutoff using the same dependent variables observed after the introduction of the platform. The estimates were, however, very imprecise to inform about the effect of interest. While a 95 percent confidence interval overlapped with our baseline estimates, the interval was not informative about the causal effect of interest. For instance, the exposure effects including 20 percent reductions in earnings could not be ruled out with 95 percent confidence.

In column (2), we find that the average exposure effect on convictions masks significant differences by gender. While the effect of exposure is close to zero for males, exposure increases the risk of conviction for females by 1.2 percentage points. We also find significant stronger increases in the probability of conviction for defendants above age 25, and for those with at least a HS degree. Turning to labor market outcomes in Panels B and C we find significantly stronger adverse effects on both employment and earnings for males, defendants above age 25 and those holding a HS degree. We also find that the effect on earnings is significantly weaker for migrants compared to natives -3.6 and -11.4 percent, respectively. In summary, we find stronger adverse exposure effects in groups where the opportunity cost of engaging in crime is higher.

Case characteristics

Table 3 presents estimates from regressions where we investigate potential heterogeneous exposure effects by case characteristics. Such effects are interesting in themselves as they may be informative about what type of information is relevant to include in criminal background checks. They potentially also contribute to shed light on the role of informal sanction costs. The labeling theory is one of the cornerstone models in criminology and posits that revealing one's criminal background can exacerbate criminal behavior. In this model, interactions with the criminal justice system labels the offender and cause him to adopt stigmatizing beliefs about himself and adopt a deviant identity (e.g. Becker, 1968; Lemert, 1967). Because of the severe difficulties involved in distinguishing informal sanction costs attached to the stigma of holding a criminal record from the imposed formal ones such as fines, community service or prison, we are not aware of any study where the empirical strategy by design is able to identify informal sanctions net of formal ones (Chalfin and McCrary, 2017).[38] Isolating the role of informal sanction costs is important as these mechanisms may operate in different direction and vary depending on the context. For instance, in places where the spatial concentration of criminals is higher, the role of stigma could be reduced.[39]

The fact that all individuals in our sample are charged with a crime but public exposure of criminal records is quasi-randomly determined presents a rare opportunity to identify the impact of informal sanctions independently of formal ones. While this setting is sufficient to identify the role of informal

[38] Empirical studies in criminology tend to compare offenders either convicted or incarcerated with those not formally sanctioned e.g. Bernburg and Krohn (2003), Farrington et al. (1978), and Murray et al. (2017).

[39] It can be useful to consider the (dis)utility of the offender, U, as a function of formal sanctions, F, and informal sanctions, I, so that $U = f(F,I)$. If informal sanctions lead to worse utility $\partial U/\partial I < 0$, while formal sanctions improve utility $\partial U/\partial F > 0$, for instance if prisons provide adequate rehabilitation programs, then the reduced form effect depends on the relative magnitudes of these partial derivatives and their interaction within the utility function.

Table 2. *Results by Demographic and Socioeconomic Characteristics*

Panel A: Convicted	(1)	(2)	(3)	(4)	(5)
Exposed x Post	0.003	0.012***	-0.000	0.003	-0.002
	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
x Male		-0.011***			
		(0.003)			
x Above age 25			0.007**		
			(0.003)		
x Immigrant				-0.001	
				(0.003)	
x HS degree					0.007**
					(0.003)
Sample mean	0.088	0.088	0.088	0.088	0.088
Panel B: Employed					
Exposed x Post	-0.008	0.005	0.016**	-0.010	0.006
	(0.008)	(0.010)	(0.007)	(0.008)	(0.007)
x Male		-0.017*			
		(0.009)			
x Above age 25			-0.047***		
			(0.006)		
x Immigrant				0.007	
				(0.007)	
x HS degree					-0.027***
					(0.008)
Sample mean	0.666	0.666	0.666	0.666	0.666
Panel C: Log Earnings					
Exposed x Post	-0.093**	-0.010	0.027	-0.114**	-0.010
	(0.042)	(0.064)	(0.046)	(0.047)	(0.042)
x Male		-0.107*			
		(0.060)			
x Above age 25			-0.237***		
			(0.048)		
x Immigrant				0.078*	
				(0.046)	
x HS degree					-0.167***
					(0.048)
Sample mean	5.835	5.835	5.835	5.835	5.835
Year FE	Yes	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes
Time Since Charge FE	Yes	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes
Observations	186,840	186,840	186,840	186,840	186,840

Note: This table reports difference-in-differences estimates based on model (1) of the effect of exposure on crime and labor market outcomes. The regressions control for age, gender, immigrant and the main effect of being exposed. Standard errors in parenthesis are clustered at the individual and court level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 3. *Case characteristics and labeling*

Panel A: Convicted	(1)	(2)	(3)	(4)
Exposed x Post	0.003	0.002	0.001	0.002
	(0.003)	(0.003)	(0.004)	(0.003)
x Acquitted		0.007**		
		(0.004)		
x Misdemeanor			0.003	
			(0.003)	
x Neighborhood offenders				0.001
< median				(0.003)
Sample mean	0.088	0.088	0.088	0.088
Panel B: Employed				
Exposed x Post	-0.008	-0.004	0.001	-0.001
	(0.008)	(0.008)	(0.007)	(0.008)
x Acquitted		-0.029**		
		(0.011)		
x Misdemeanor			-0.036***	
			(0.009)	
x Neighborhood offenders				-0.017***
< median				(0.006)
Sample mean	0.666	0.666	0.666	0.666
Panel C: Log Earnings				
Exposed x Post	-0.093**	-0.065	-0.051	-0.054
	(0.042)	(0.046)	(0.043)	(0.044)
x Acquitted		-0.194**		
		(0.076)		
x Misdemeanor			-0.141**	
			(0.061)	
x Neighborhood offenders				-0.098**
< median				(0.039)
Sample mean	5.835	5.835	5.835	5.835
Year FE	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes
Observations	186,840	186,840	186,840	186,840

Note: This table reports difference-in-differences estimates based on model (1) of the effect of exposure on crime and labor market outcomes. The regressions control for the same variables as in Table 2. Numbers in brackets show mean of the dependent variable. Standard errors in parenthesis are clustered at the individual and court level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

sanctions, an analysis of whether the exposure effect varies by case characteristics provides an additional way to investigate the importance of stigma.

The results in column (2) of Panel A show that the exposure effects on recidivism are significantly higher for defendants who were acquitted in court compared to defendants who were convicted. The estimates suggest that the probability of being convicted increases by almost 1 percentage point for those acquitted.

Panels B and C show that there is also a significantly larger labor market penalty for acquitted defendants compared to convicted defendants. We can also see that the labor market penalty of exposure is significantly higher for defendants who were convicted of a misdemeanor compared to those convicted with a non-misdemeanor. Many misdemeanor offenses do not involve violence or firearms; rather, they stem from the criminalization of everyday behaviors like possessing small amounts of banned substances for personal use, vandalism, disturbing the peace, minor theft, and trespassing.[40]

Recall that the open access version of the database did not differentiate between severe crimes and misdemeanors. Additionally, it included acquitted defendants without indicating their not-guilty verdicts. Therefore, one possible interpretation for the stronger treatment effects is that the platform's information may have signaled that an individual was a more prolific criminal than they actually were.

Several theoretical models predict that, all else equal, the degree of stigma in a given area is declining in the number of individuals with criminal records (e.g. Chalfin and McCrary, 2017; Weibull and Villa, 2005; Funk, 2004; Rose, 2021). To examine this we used our data to calculate the number of individuals convicted of a crime in the past five years living in each municipality. We then interacted *exposed* x *post* in model (1) with an indicator for the defendant living in an area with less than median criminals. The results show that the adverse labor market effects indeed are concentrated in the subsample of individuals who lives in neighborhoods with a low share of the population having a criminal conviction. For instance, while the decrease in earnings is 15.2 percent for those living in places with relatively few ex-convicts, it is 5.4 percent for defendants in neighborhoods with a higher concentration of ex-convicts.

5.4 Interpretation and additional results

Our main findings so far is that the massive use of the database is connected to significant adverse exposure effects on earnings but not on employment or criminal recidivism. However, there are significantly stronger detrimental

[40]Misdemeanors are minor offenses, usually resulting in incarceration of one year or less. Felonies are more serious, such as robbery, theft over USD 1,000, and assault with a deadly weapon, carrying harsher penalties. While the grouping of offenses into misdemeanor and felonies is not used in Sweden, we classify misdemeanors as crimes for which no individual was sentenced to prison in our data.

effects on both labor market outcomes and recidivism in specific defendant subgroups such as those with at least a high school degree, acquitted individuals, and those living in areas with few criminals. These results are consistent with stigma attached to being marked as a criminal playing a key role in explaining the effects of interactions with the criminal justice system. In this section we provide results from additional analyses that aims to assess various explanations for our findings.

Dynamic scarring effects

Several studies show that criminal conviction is associated with a large drop in labor market outcomes (Rose, 2021). In Section 5.1 we showed that this is also the case in Sweden. If a conviction causes individuals to quickly accumulate labor market scars, such as lengthy employment gaps, employers may remain hesitant to hire these individuals, potentially making it more difficult to identify exposure effects (Agan et al., 2024). However, we do find a significant labor market penalty even net of this dynamic scaring effect. We also estimated our event study model (model (2)) focusing on the effect on labor market outcomes around the time of acquittal and misdemeanor charges. While imprecise, the point estimates are much closer to zero compared to that of convictions.[41] Since we find significantly stronger exposure effects for acquittal and misdemeanor cases, it means that for those results, any dynamic scaring effects are likely to be small. The results in Panel E in Appendix Table A2 where we examine the immediate effect of the introduction of the platform using confession cases as an alternative control group finding similar results as in baseline also seem to rule out that dynamic scarring effects matters, at least in the context of this paper.

Behavioral responses

To what extent can our results be explained by behavioral adjustments taken by the defendants listed in the database? We examine this by estimating exposure effects for several potentially important behavioral markers. Previous studies have shown that criminal justice interactions has meaningful effects on earnings through changes in family structure (Charles and Luoh, 2010; Chetty et al., 2020; Svaver, 2011) and human capital investments (Finlay et al., 2023). Table A3 shows the results for exposure effects on the probability of enrolling in college and the likelihood of starting a labor market training program. These outcomes could proxy for human capital investments that defendant may potentially take in order to avoid the potential harmful effects of having ones criminal records exposed. We also examine the probability of moving to a different neighborhood. Finally, we present estimates for family formation outcomes. With the exception for the probability of enrolling in college, we find no significant exposure effects. The results for college en-

[41] The results are available on request.

rollment should also be interpreted with some caution since the effect size is small (0.4 percentage points) and only significant at the 10 percent level. One potential reason for why individuals not seem to respond to exposure may be that lack of knowledge or perceived low rewards. It also provide suggestive evidence that the reaction of employers to exposure is what explains the labor market penalty.

Industry sorting

Individuals could also respond to exposure by sorting into jobs that are more lenient towards hiring individuals with criminal records. To examine this we follow Mueller-Smith and Schnepel, 2021 and classify each two digit SNI (NAICS) industrial sector as having low or high employment penetration among convicted individuals using the regression adjusted ratios of the sector-specific employment rate for those individuals with convictions versus those without in our analysis samples (see Table A4). Sectors with ratios of 1.5 or less are high penetration industries relatively more accessible to those with criminal histories and the opposite is true for low penetration industries with ratios greater than 1.5. The results in Table A5 show small and imprecise estimates but no strong evidence in favor of differential sorting across industries for any of the groups. For sectors with employment penetration ratios of 1.2 or higher we find some evidence that individuals are sorting away from high penetration industries but the estimates are small in magnitude and imprecise.

AKM sorting

Job ladder models propose that workers engage in on-the-job search throughout their careers, gradually moving to higher-paying employers. In these models, pay disparities among firms reflect differences in how the surplus is divided between workers and employers (Burdett and Mortensen, 1998; Haltiwanger et al., 2018). Within this framework, exposed workers may experience earnings losses due to employer effects as they move down the job ladder to employers that offer lower pay. We examine to what extent the exposure effects can be explained by differential sorting of workers to firms that vary in their estimated earnings premia by following (Lachowska et al., 2020). We estimate the earnings premia in different firms using the methods suggested by Abowd et al. (1999) (AKM). In this exercise we define a firm as an establishment.

The AKM model can be written

$$logY_{itj} = \alpha_i + \beta_{j(i,t)} + \phi_t + X_{itj} + \varepsilon_{itj} \tag{3}$$

where $logY_{itj}$ denotes earnings of worker i with employer j in year t; α_i is a worker-specific fixed effect; $\beta_{j(i,t)}$ is an employer-specific fixed effect; ϕ_t is a vector of calendar year indicators; X_{itj} consist of education-gender-specific age dummies (following Engbom et al., 2023), and ε_{itj} is the error component. The function $j(i,t)$ indexes the employer j effect for worker i in year t. Due to the problem of multicollinearity between age and year-fixed effects, we assume that the age effects are constant between ages 45 and 54.

This model controls for sorting of workers based on fixed differences in their underlying characteristics. The firm fixed effects capture firm characteristics that result in above- or below-average earnings for all workers at employer j and can be interpreted as representing the advantages derived from being employed by a given employer. We estimate the AKM model for firms in the connected set over the years 2005-2016, including workers aged 18-65. We exclude establishments with fewer than 5 movers when estimating the model but note that our results are robust to also including all establishments in the connected set.

We use the estimated $\hat{\beta_{j(i,t)}}$ as outcomes when we estimate exposure effects using model (1). The results are presented in Appendix Table A6. In general, we find significantly stronger adverse exposure effects on the earnings premia in the subgroups of defendants where the labor market responses were concentrated. This result is consistent with the predictions in job ladder models that exposed workers will experience earnings losses due to employer effects, as they moves down the job ladder to a lower-paying employer.

In order to better understand whether a move down the job ladder is due to workers changing employer or workers staying in the same firm we estimated model (1) using as dependent variable an indicator set to one if the individual has changed firm from one year to another and zero if (s)he stayed in the firm. Individuals who are non-employed are missing in the data. Overall, the results shown in Appendix Table A7 shows that workers at least age 25 are significantly more likely to change firm compared to younger workers, that acquitted workers are significantly more likely to change firm relative to convicted workers, and that workers charged with a misdemeanor are significantly more likely to move to a different firm relative to those charged with a more severe crime.

Taken together, it seems as if at least part of the reduction in the AKM earnings premium is due to workers transiting to lower-paying employers. While we cannot distinguish between voluntary and involuntary job losses, it is possible that exposed workers are fired or do not get their temporary contracts renewed. These workers may be forced to adjust their reservation wage downwards and in the process moving down the job ladder to a lower-paying employer.

Effects on children

In this section we investigate whether the introduction of the database affected the outcomes of the children of exposed defendants. The potential for significant yet previously overlooked spillover effects of parental criminal justice involvement is suggested by recent studies estimating the causal effect of parental incarceration (Arteaga, 2022; Bhuller et al., 2020; Dobbie et al., 2018; Norris et al., 2021), but it is unclear how children are affected by increased public access to criminal records of their parents.[42] In this analysis, we focus on children who finishes grade 9 (typically at age 16) in the period 2010-2016 and link them to their parents. We also add information on court convictions for these children. We start by verifying in Appendix Table A8 that the effects in the parental subsample are relatively similar to that in the full population, although the precision of the point estimates is lower for log earnings and higher for employment. Table A9 shows the difference-in-differences estimates from model (1) of the exposure effects on children. We find significantly stronger adverse exposure effects on compulsory school GPA for children belonging to the subgroups of defendants with the largest adverse exposure effects on the defendants themselves. It should be noted that the adverse exposure effect on children could be driven by both a direct effect of the children experiencing the stigma from having a parent in the database or by a reduction in the family's resources.

6 Concluding Remarks

We study the unexpected launch of Sweden's first online criminal database in 2014 that provided unrestricted, anonymous, and cost-free public access to the criminal records of every individual charged with a crime. To estimate the causal effect of public access to this database, we leverage rich register data and the fact that administrative rules only allowed the company behind the database to identify offenders sentenced before certain dates in the past.

We find significant adverse effects of exposure on earnings but not on employment or criminal recidivism. However, there are significantly stronger detrimental effects on both labor market outcomes and recidivism in defendant subgroups with a higher opportunity cost of engaging in criminal activity, acquitted individuals, those charged with a misdemeanor and those living in areas with fewer criminals. Our results highlight that the stigma associated with being marked as a criminal is a potentially important but previously unappreciated mechanism explaining behavioral responses to criminal justice interactions.

[42] While the findings are mixed, many of the estimates lack precision, resulting in overlapping confidence intervals for comparable outcomes. Using a random-judge design, Dobbie et al. (2018) report that parental incarceration leads to increased teen crime, lower school performance, and negative impacts on employment and earnings. Consistent with this, Grenet et al. (2024) show quasi-experimental evidence that a reform were parents were able to substitute a prison sentence to instead serve the full sentence using electronic monitoring significantly improved child outcomes. Conversely, Norris et al. (2021) find that in the United States, parental incarceration reduces teen crime, does not affect teen parenthood, and increases the likelihood of children living in affluent neighborhoods as adults. Arteaga (2022) observes positive effects on children's educational attainment in Colombia. Bhuller et al. (2020) find no significant effects on school performance or children's propensity for criminal behavior.

While we are unable to directly measure the long-run impact of open access criminal databases on defendants using our data, we place the magnitude of our estimates in perspective by considering a partial back-of-the-envelope calculation that accounts for the direct economic impacts on the defendant himself. To calculate the implied effect we make the conservative assumption that the treatment effect on log earnings is constant for a period of three years then completely fades away. We multiply the treatment effect with baseline average earnings (SEK 148,134) and estimate the three year earnings loss for the defendants in our sample to be SEK 44,436. Since 2014, about 47,000 first time defendants are added to the database every year. For instance, in 2018 48,183 first time defendants were convicted in court and consequently were listed in the database.[43] The combined earnings losses this year thus amounts to SEK 2.14 billion, or 23 % of total government spending on prison and rehabilitation that year.[44] It is, however, important to note that we are unable to determine the full welfare consequences of open access to online criminal records using our research design. It is possible that these databases have a general deterrence effect in the full population. Our analysis will, therefore, overstate the social costs of these databases if the threat of being included in the database decreases crime rates. The existing literature has struggled to provide definitive explanations for why the impact of stricter sentencing policies on offenders' outcomes appears to vary across different study contexts. Our findings potentially offer a partial explanation. In environments where stigma carries less weight, such as places with a high spatial concentration of criminals, informal sanctions may play a diminished role in explaining the reduced form effect of sanction policy relative to that of formal sanctions. On the other hand, in contexts where informal sanction costs matter more, the beneficial effects of more prison staff or rehabilitation programs could be offset. Our findings directly speak to the debate over what type of information on cases should be disclosed in criminal background checks. Our results suggest that reforms that increase leniency in the conviction dimension (e.g. higher evidentiary standards or not prosecuting marginal cases) potentially has social benefits. In many countries, the information documented in background checks has vastly expanded along with their public availability so that many record checks now include non-criminal details that presumptively mark individuals as risky (Zedner and Ashworth, 2019). However, hardly any research has investigated the consequences of shifts in the content and governance of these records.

[43] https://bra.se/statistik/kriminalstatistik/personer-lagforda-for-brott.html

[44] Total government spending on prison and rehabilitation was SEK 9.3 billion in 2018. https://www.kriminalvarden.se/globalassets/publikationer/ekonomi/kriminalvardens-arsredovisning-2018.pdf

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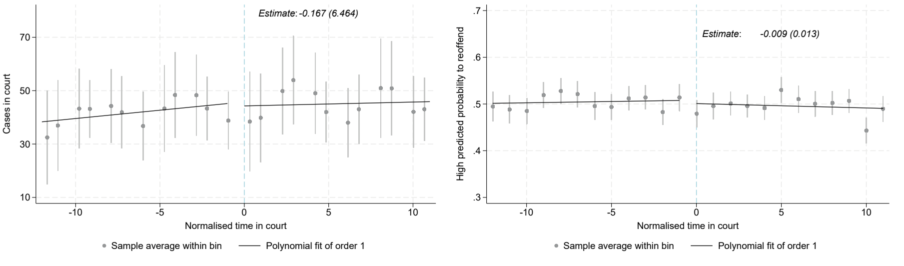
Appendix A: Additional figures and tables

Figure A1. Bunching around the cutoff

(a) Cases *(b)* Predicted risk of recidivism



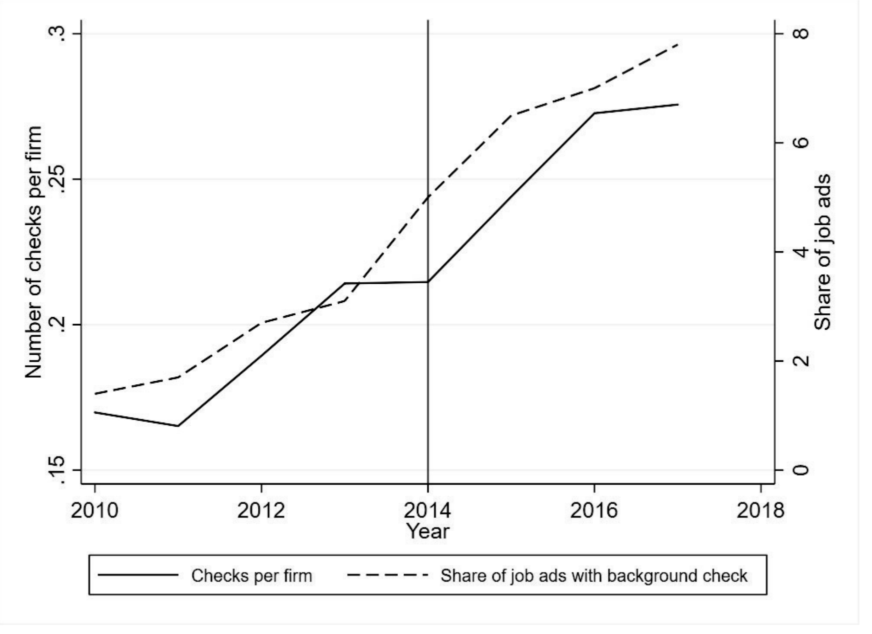
Notes: Figure (a) shows the average number cases for the courts in our sample by month relative to the cutoff. Figure (b) shows the average predicted risk of recidivism of defendants by month relative to cutoff. The prediction is done using the baseline individual characteristics of the defendant listed in Table 1.

Table A1. *Descriptive Statistics*

Panel A: Baseline Characteristics	Exposed defendants	Non-exposed defendants
Male	0.779	0.777
	(0.415)	(0.416)
Age	29.627	29.133
	(11.966)	(12.031)
Immigrant	0.295	0.282
	(0.456)	(0.450)
High school degree	0.618	0.607
	(0.486)	(0.488)
Log earnings	5.068	4.968
	(3.375)	(3.363)
Employed	0.544	0.528
	(0.498)	(0.499)
Panel B: Case characteristics		
Acquitted	0.144	0.144
	(0.351)	(0.351)
Misdemeanor	0.171	0.157
	(0.377)	(0.363)
Felony	0.829	0.843
	(0.377)	(0.363)
DUI	0.086	0.081
	(0.280)	(0.273)
Drug offense	0.057	0.052
	(0.231)	(0.222)
Property offense	0.148	0.148
	(0.355)	(0.355)
Violent offense	0.173	0.182
	(0.378)	(0.385)
Traffic offense	0.157	0.147
	(0.363)	(0.354)
Other offense	0.381	0.390
	(0.486)	(0.488)
Panel C: Sentence		
Fines	0.499	0.488
	(0.500)	(0.500)
Probation	0.176	0.172
	(0.381)	(0.378)
Community service	0.214	0.217
	(0.410)	(0.412)
Prison	0.074	0.075
	(0.262)	(0.263)
Other	0.037	0.048
	(0.189)	(0.214)
Observations	97,374	89,466

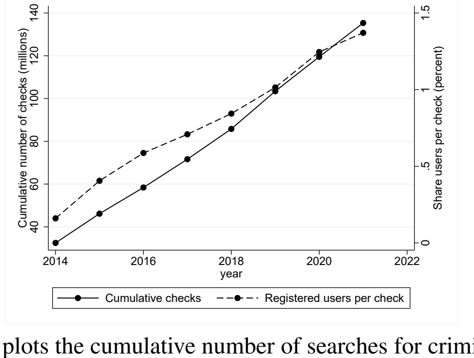
Notes: This table provides summary statistics for baseline characteristics of the estimation sample for exposed and non-exposed defendants. Exposed defendants were sentenced up to 12 months after the court-specific cutoff date, making them visible in the database, and non-exposed defendants were sentenced in a 12 month window before the cutoff. Baseline characteristics are measured one year before the initial trial.

Figure A2. Criminal background checks



Notes: This figure plots the number of official criminal background check inquires for personal use (likely to be submitted to a potential employer) per registered firm submitted to the Swedish police over the period 2010-2017 using data from the Swedish police and Statistics Sweden. The figure also plots the share of ads posted at the Public Employment Office that mentions criminal background checks using data from the Public Employment Office.

Figure A3. Background checks using the platform



Notes: This figure plots the cumulative number of searches for criminal background using the platform. The figure also plots the number of registered users per background check conducted. The figure uses data provided by the company behind the platform

.

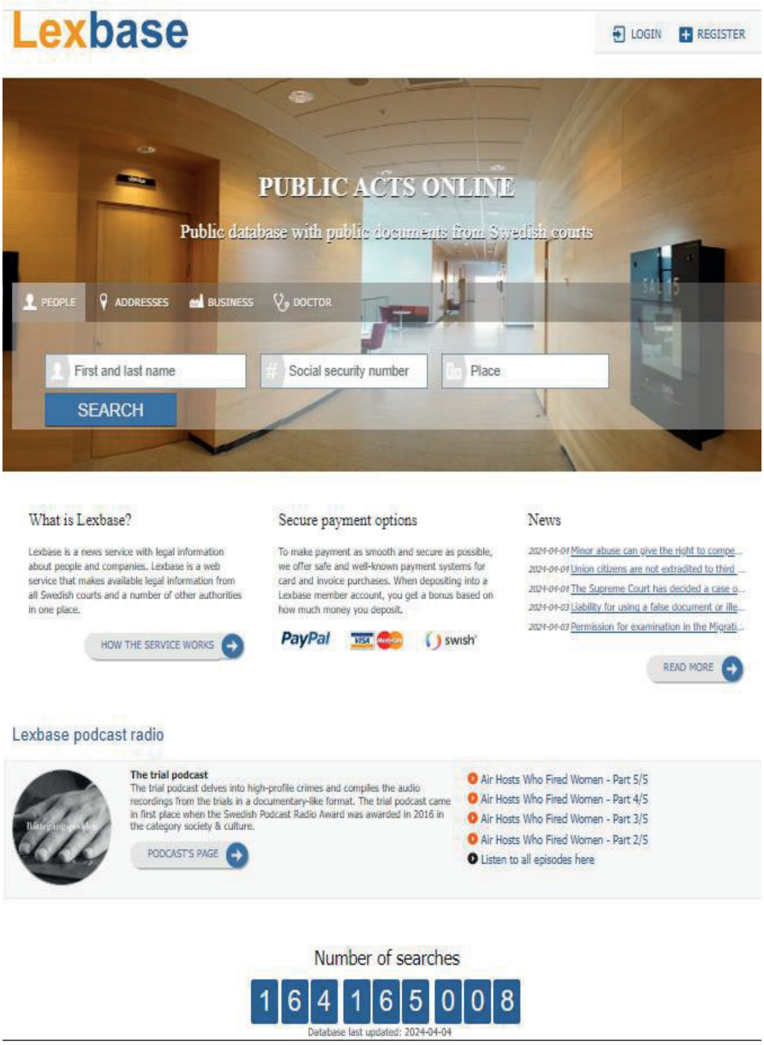


Figure A4. Homepage Lexbase

Notes: This image shows the homepage of Lexbase and was retrieved on April 4, 2024.

How to use Lexbase

With a few single button presses, you can obtain useful decision-making information. You start by choosing a search method. The guide below explains how the service works.

1. Select search method

A) Search person
In this search function Search person you can specify your search using four fields; First name, last name, social security number and city. If you get a hit on a person, it means that he or she has been subject to a court hearing. If you do not get a hit, it is probably because the person does not appear in any legal process.

Example: Shows results for Anders Andersson in Stockholm

In the search fields, you can use asterisk (*) in your search text to replace part of a search term. You also don't need to search for an exact name of a person or company. Parts of the name in question are sufficient.

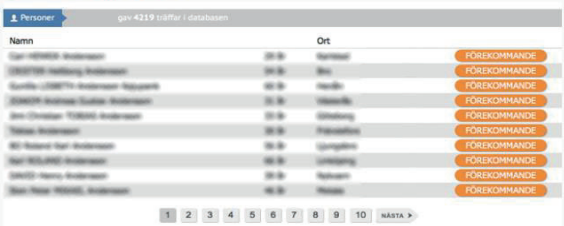
If you do not get a hit on a certain person, it is probably because they are not present in any legal process.

B) Search address
This search function helps you who want information about people living in a certain area or in your immediate area. The search tool sorts by Address, Postcode and City. The more fields you fill in, the narrower the hit picture you get.

Example: Showing results on Storgatan 21 in Gothenburg.

2. Search results are displayed

When you have made a search, the search result is presented in a list. To read a report, click on the button on the right which looks like this and is called FÖREKOMMANDE



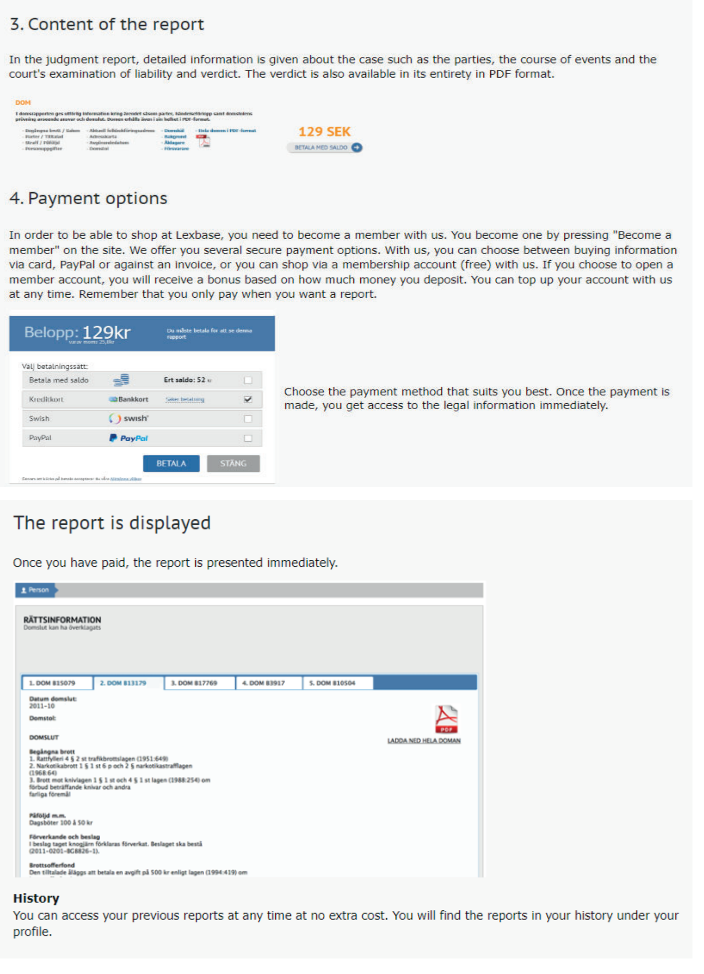
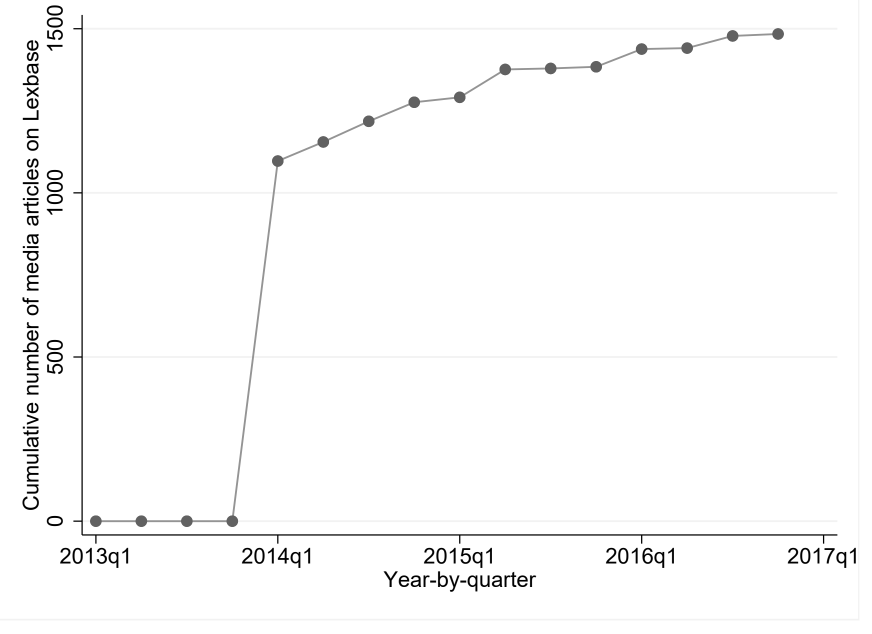


Figure A5. How the Search-Engine at Lexbase Works

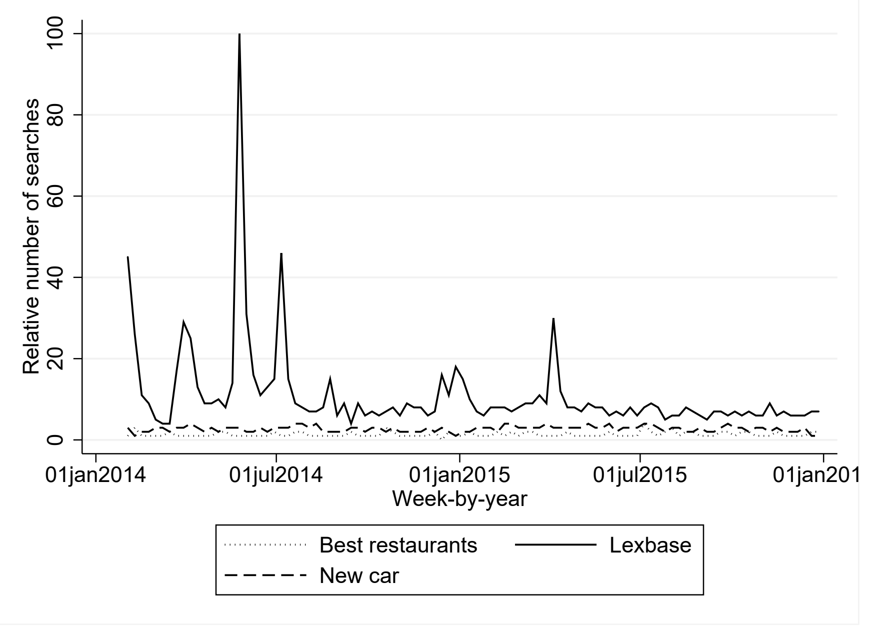
Notes: This image is from the homepage of Lexbase and provides the guide from the platform on how to use the search engine. The image was retrieved on April 4, 2024.

Figure A6. Cumulative number of media articles covering Lexbase 2013-2016



Notes: This figure plots the cumulative number of media articles mentioning Lexbase in 2013-2016. Data on media coverage is from the Retriever/Mediearkivet database, which is a Nordic news archive with material from mainly major Swedish newspapers. The information was collected on Jan 20th, 2024.

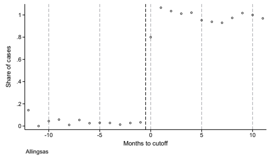
Figure A7. Relative number of searches on Google



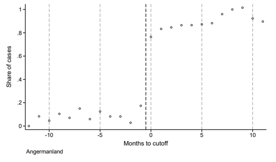
Notes: This figure plots the relative number of searches on Google for the terms "Lexbase", "Best restaurants" and "New car". The data from Google Trends was collected on Jan 21st, 2024. Google Trends normalizes search data to make comparisons between terms easier. Search results are normalized to the time and location of a query by dividing each data point by the total searches of the geography and time range it represents. The resulting numbers are then scaled on a range of 0 to 100 based on a topic's proportion to all searches on all topics. To make the figure more easy to read we drop the days immediately following the launch of the database when the searches skyrocketed and focus on the period starting one week after the introduction and up until the last week of 2015. The information was collected on Jan 20th, 2024.

Figure A8. Cases around the cutoff

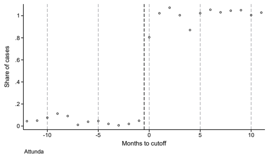
(a) DC Allingsås



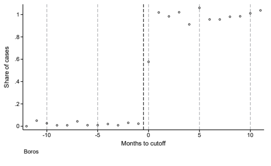
(b) DC Ångermanland



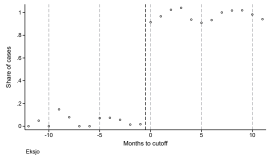
(c) DC Attunda



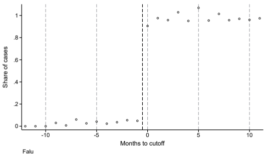
(d) DC Borås



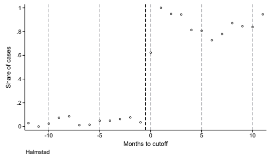
(e) DC Eksjö



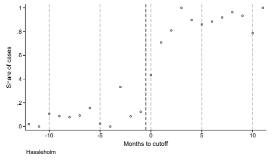
(f) DC Falun



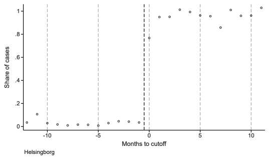
(g) DC Halmstad



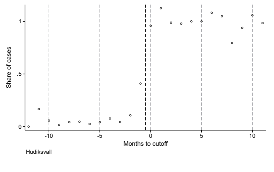
(h) DC Hässleholm



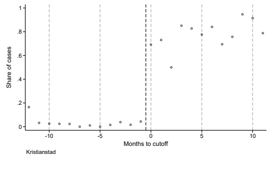
(i) DC Helsingborg



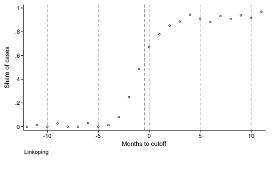
(j) DC Hudiksvall



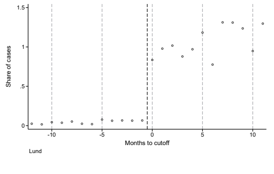
(k) DC Kristianstad



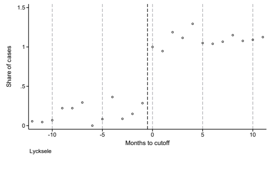
(l) DC Linköping



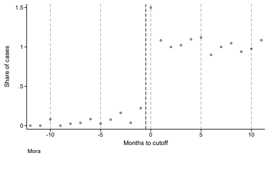
(m) DC Lund



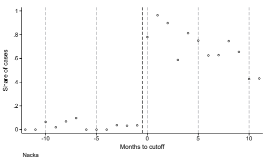
(n) DC Lycksele



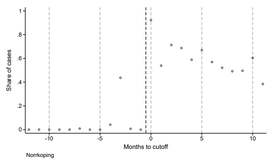
(o) DC Mora



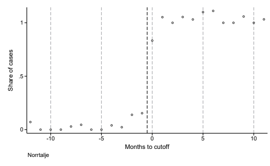
(a) *DC Nacka*



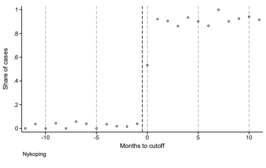
(b) *DC Norrköping*



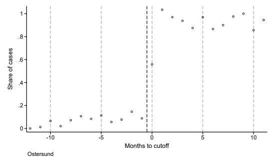
(c) *DC Norrtälje*



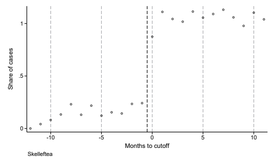
(d) *DC Nyköping*



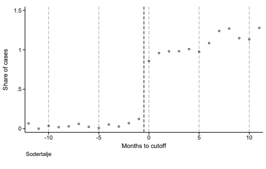
(e) *DC Östersund*



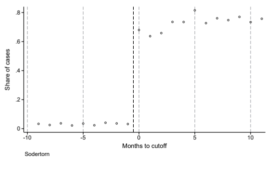
(f) *DC Skellefteå*



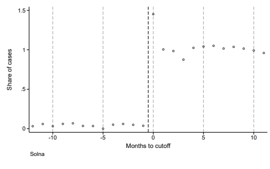
(g) *DC Södertälje*



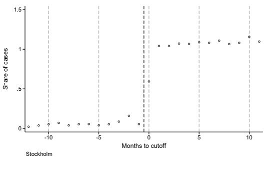
(h) *DC Södertörn*



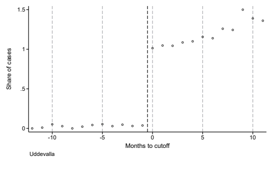
(i) *DC Solna*



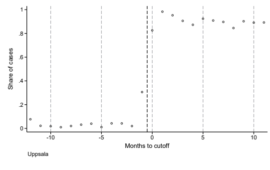
(j) *DC Stockholm*



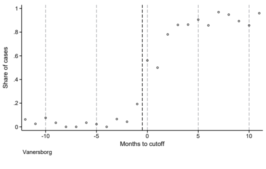
(k) *DC Uddevalla*



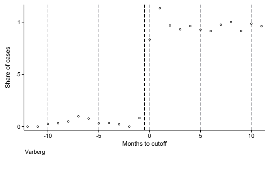
(l) *DC Uppsala*



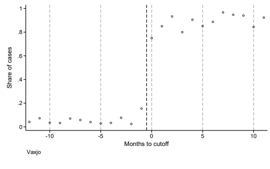
(m) *DC Vänersborg*



(n) *DC Varberg*



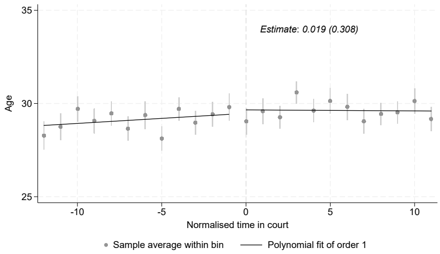
(o) *DC Växjö*



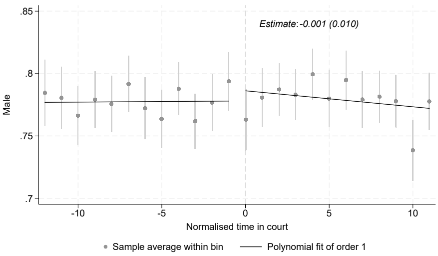
Notes: The figure plots the share of Lexbase cases in our register data within a 12 month window around the cutoff separately for the district courts (DC) in our sample.

Figure A10. Case characteristics around the cutoff

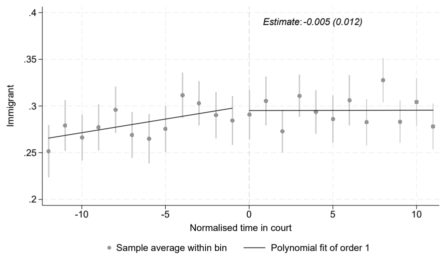
(a) Age



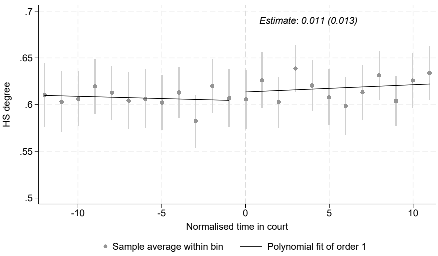
(b) Males



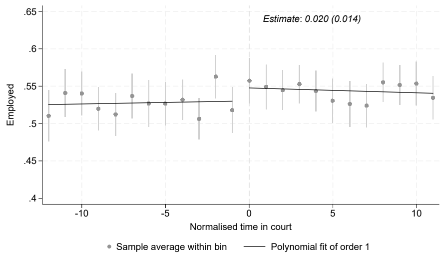
(c) Immigrants



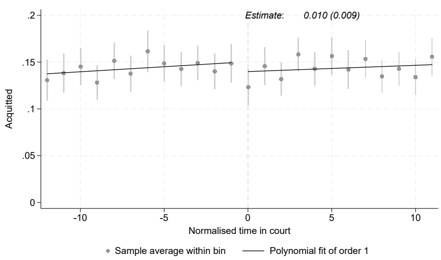
(d) High school degree



(e) Employment



(f) Acquitted

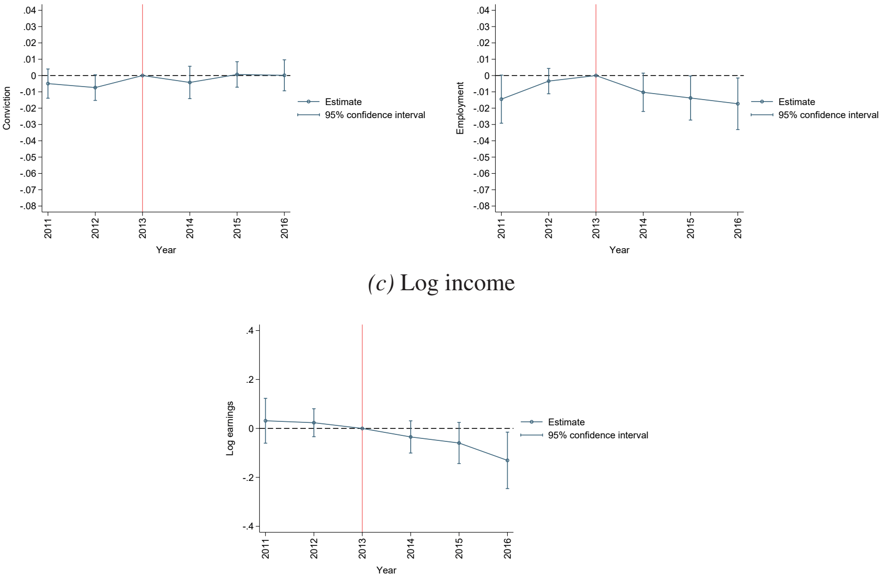


Notes: The figure shows the average case and defendant characteristics by time relative to cutoff.

Figure A11. Annual DD estimates of the exposure effects

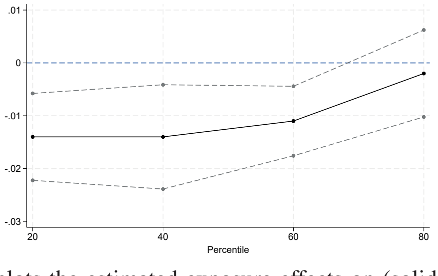
(a) Conviction *(b)* Employment

(c) Log income



Notes: This figure plots DD-estimates from model (1) by year relative to 2013. All regressions control for year, court, time since charge, calendar month, age, gender, immigrant and the main effect of being exposed. The bars show the 95 percent confidence interval based on standard errors clustered at the individual and court level. Panel (a) shows conviction between 2011-2016, Panels (b)-(c) shows labor market outcomes in 2011-2016.

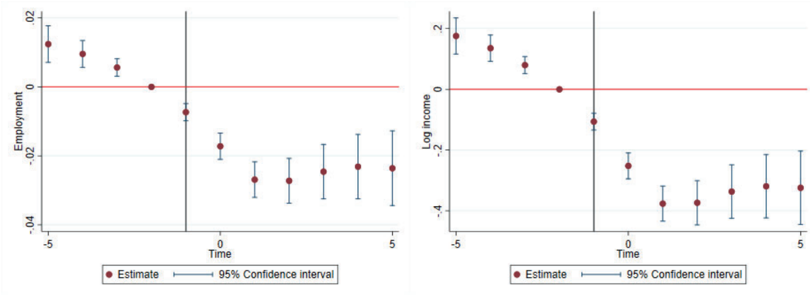
Figure A12. Effect of exposure on position in the earnings distribution



Notes: This figure plots the estimated exposure effects on (solid line) along with the 90 percent confidence interval (dashed line) based on model (1) where the dependent variable is an indicator for the individual earning more than the i:th percentile in the earnings distribution. The regressions control for age, gender, immigrant and the main effect of being exposed. Numbers in brackets show mean of the dependent variable. Standard errors in parenthesis are clustered at the individual and court level.

Figure A13. Event study estimates of the effect of conviction.

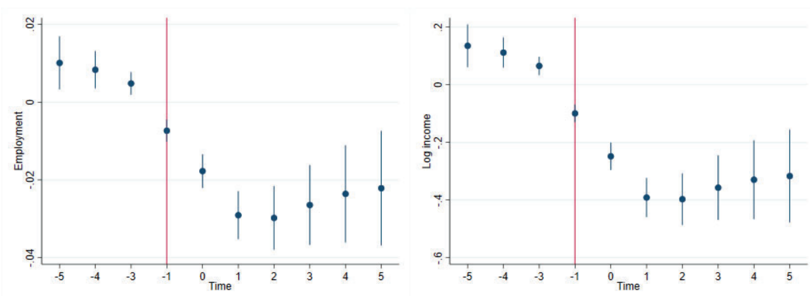
(a) Employment *(b)* Earnings



Notes: This figure estimates event study coefficients +/- 7 years around the charge, where the end points after +/- 5 year are binned. Panel (a) gives the coefficients for employment, and Panel (b) gives the coefficients for log earnings. The sample is restricted to first-time offenders between 1997-2010 for those aged 25-57 at the time of the charge. All regressions control for age, year and individual fixed effects. The bars show the 95 percent confidence interval based on standard errors clustered at the individual level.

Figure A14. Event study of conviction following Sun and Abraham

(a) Employment *(b)* Earnings



Notes: This figure estimates event study coefficients +/- 7 years around the charge, where the end points after +/- 5 year are binned. The procedure from Sun and Abraham (2021) is employed, using the last treated unit as control, excluding time periods after the last unit received treatment. Panel (a) gives the coefficients for employment, and Panel (b) gives the coefficients for log earnings. The sample is restricted to first-time offenders between 1997-2010 for those aged 25-57 at the time of the charge. All regressions control for age, year and individual fixed effects. The bars show the 95 percent confidence interval based on standard errors clustered at the individual level.

Table A2. *Robustness*

	Convicted	Employed	Log Earnings
Panel A: Cutoff -1 (N=172,548)	(1)	(2)	(3)
Exposed*Post	-0.000	0.003	0.023
	(0.003)	(0.006)	(0.045)
Outcome mean	0.084	0.674	5.875
Panel B: Cutoff +1 (N=206,364)			
Exposed*Post	-0.004	-0.003	-0.048
	(0.003)	(0.005)	(0.030)
Outcome mean	0.101	0.648	5.728
Panel C: Bandwidth 9 months (N=142,764)			
Exposed*Post	0.002	-0.011*	-0.120***
	(0.003)	(0.006)	(0.031)
Outcome mean	0.088	0.665	5.825
Panel D: Bandwidth 6 months (N=95,724)			
Exposed*Post	0.001	-0.010*	-0.135***
	(0.004)	(0.006)	(0.030)
Outcome mean	0.087	0.667	5.847
Panel E: Short-term effects (N=136,656)			
Exposed*Post	0.004	-0.007	-0.098***
	(0.004)	(0.006)	(0.035)
Outcome mean	0.101	0.532	5.118
Year FE	Yes	Yes	Yes
Court FE	Yes	Yes	Yes
Time since charge FE	Yes	Yes	Yes
Calendar month FE	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes

Note: This table reports difference-in-differences estimates based on model (1) of the effect of exposure on crime and labor market outcomes. Panels A and B show results from placebo cutoff dates one year before and one year after the actual cutoff date in a given court, respectively. Panels C and D show results using alternative bandwidths around the court-specific cutoff date. Panel E shows estimates from an alternative difference-in-differences model where the "exposed" group includes defendants who underwent a trial, while the "non-exposed" group comprises defendants who were directly convicted by a prosecutor for confession cases. These confession cases were not included in the database in 2014 and 2015. Outcomes are measured one year post-conviction and the repeated cross-sectional dataset runs from 2010 to 2015. All regressions control for age, gender, immigrant and the main effect of being exposed. Numbers in brackets show mean of the dependent variable. Standard errors in parenthesis are clustered at the individual and court level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A3. *Additional outcomes*

	All
	(1)
In college	0.004*
	(0.002)
	[0.029]
Labor market training	0.001
	(0.002)
	[0.011]
Moving	0.004
	(0.006)
	[0.227]
Single household	0.001
	(0.005)
	[0.703]
Get pregnant	-0.001
	(0.003)
	[0.052]
Year FE	Yes
Court FE	Yes
Time since charge FE	Yes
Calendar month FE	Yes
Baseline Controls	Yes
Observations	186,840

Note: This table reports difference-in-differences estimates based on model (1) for alternative outcomes. The regressions control for age, gender, immigrant and the main effect of being exposed. Numbers in brackets show the mean of the dependent variables. Standard errors are clustered at the individual and court level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A4. *Employment penetration by industry*

	Ratio	Penetration classification
	(1)	(2)
Banking	2.2	Low
Construction	0.4	High
Culture	1.2	High
Education	2.7	Low
Electricity	1.9	Low
Farming	1.5	High
Healthcare	1.6	Low
Hotel and Restaurants	0.6	High
Information	1.5	High
Law and Economics	1.3	High
Letting	0.6	High
Manufacturing	1.0	High
Mining	0.9	High
Other services	1.4	High
Public Administration	2.6	Low
Real estate	1.2	High
Retail	0.9	High
Transport	0.5	High
Water & Sanitation	0.6	High

Note: This table presents the ratio of employment in a particular industry for individuals without a court conviction relative to those with a court conviction in year 2013. No adjustments for baseline characteristics are done. Equal employment penetration among these two groups is expressed as a ratio equal to 1. We classify industries as low penetration if the ratio exceeds 1.5.

Table A5. *Industry effects*

	All
	(1)
Empl. in high penetration industries 1.5	-0.010*
	(0.006)
	[0.584]
Empl. in low penetration industries 1.5	-0.003
	(0.003)
	[0.150]
Empl. in high penetration industries 1.2	-0.008*
	(0.004)
	[0.489]
Empl. in low penetration industries 1.2	-0.004
	(0.005)
	[0.245]
Empl. in high penetration industries 1	-0.005
	(0.005)
	[0.398]
Empl. in low penetration industries 1	-0.00
	(0.005)
	[0.336]
Year FE	Yes
Court FE	Yes
Time since charge FE	Yes
Calendar month FE	Yes
Baseline Controls	Yes
Observations	186,840

Note: This table reports difference-in-differences estimates based on model (1) of the effect of exposure on sorting across industries. The regressions control for age, gender, immigrant and the main effect of being exposed. Numbers in brackets show mean of the dependent variable. Standard errors are clustered at the individual level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A6. *AKM sorting*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposed x Post	0.002	0.016	0.014*	-0.005	0.021*	0.006	0.011	0.016**
	(0.007)	(0.012)	(0.007)	(0.006)	(0.009)	(0.006)	(0.008)	(0.008)
x Male		-0.017*						
		(0.009)						
x At least age 25			-0.022**					
			(0.006)					
x Immigrant				0.033***				
				(0.009)				
x HS degree					-0.031***			
					(0.008)			
x Acquitted						-0.019**		
						(0.009)		
x Misdemaneor							-0.036***	
							(0.009)	
x Nbh offenders < med.								-0.032***
								(0.007)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	100,152	100,152	100,152	100,152	100,152	100,152	100,152	100,152

Note: This table reports difference-in-differences estimates based on model (1) of the effect of exposure on the sorting of workers to firms with different earnings premia. The dependent variable is the earnings premia estimated using the methods suggested by Abowd et al. (1999). We define a firm as an establishment. To estimate the AKM model, we include firm fixed effects, individual fixed effects, and age and year fixed effects. The age dummies are education-gender-specific (following Engbom et al. (2023)). Due to the problem of multicollinearity between age and year-fixed effects, we assume that the age effects are constant between ages 45 and 54. We estimate the AKM model for firms in the connected set over the years 2005-2016, including workers aged 18-65. We exclude establishments with fewer than 5 movers but note that the results are very similar also when including all establishments in the connected set. The regressions control for the same variables as in Table 2. Standard errors are clustered at the individual and court level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A7. *Firm switching*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposed x Post		-0.002	-0.029***	-0.008	-0.023**	-0.010	-0.013*	-0.011
		(0.013)	(0.009)	(0.010)	(0.010)	(0.007)	(0.008)	(0.008)
x Male		-0.007						
		(0.011)						
x At least age 25			0.040***					
			(0.006)					
x Immigrant				0.003				
				(0.007)				
x HS degree					0.024***			
					(0.006)			
x Acquitted						0.017**		
						(0.007)		
x Misdemaneor							0.025***	
							(0.008)	
x Nbh offenders < med.								0.007
								(0.006)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	103,908	103,908	103,908	103,908	103,908	103,908	103,908	103,908

Note: This table reports difference-in-differences estimates based on model (1). The dependent variable is an indicator set to one if the individual has changed firm from one year to another and zero if (s)he stayed in the firm. Individuals who are non-employed are missing in the data. The regressions control for the same variables as in Table 2. Standard errors are clustered at the individual and court level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A8. *Results for parental subsample*

	Convicted		Employed		Log earnings		Family log disp. inc.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposed*Post	0.004	0.004	-0.023*	-0.023*	-0.104	-0.104	-0.036	-0.036
	(0.006)	(0.006)	(0.013)	(0.013)	(0.074)	(0.074)	(0.035)	(0.034)
Sample mean	0.049	0.049	0.735	0.735	6.221	6.221	8.084	8.084
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	23,388	23,388	23,388	23,388	23,388	23,388	23,388	23,388

Note: This table reports difference-in-differences estimates based on model (1) for crime and labor market outcomes restricted to the subsample of individuals in our main sample that were parents of children belonging to the grade 9 graduation cohorts 2010-2016. Exposed is an indicator variable set to one for defendants sentenced up to 12 months after the court-specific cutoff date; zero otherwise. Post is an indicator variable set to one for the years the database was in place (i.e. 2014 and onwards); zero otherwise. The regressions control for the same variables as in Table 2. Numbers in bracket show the mean of the dependent variable. Standard errors in parenthesis are clustered at the individual and court level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A9. *Effects on children's outcomes*

Panel A: GPA (pct. rank)	(1)	(2)	(3)	(4)	(5)	(6)
Exposed x Post	-2.203	0.190	-1.043	-2.170	-0.799	-0.709
	(2.192)	(2.394)	(2.638)	(2.241)	(2.234)	(2.025)
x Defendant is the father		-3.368**				
		(1.671)				
x Parent HS degree			-1.828			
			(1.763)			
x Parent aqcuitted				-0.736		
				(1.493)		
x Parent misdemaneor					-3.827*	
					(1.972)	
x Nbh offenders < med						-3.706**
						(1.572)
Sample mean	42.572	42.572	42.572	42.572	42.572	42.572
Panel B: Court conviction						
Exposed x Post	0.009	-0.013	-0.000	0.009	-0.000	0.008
	(0.010)	(0.009)	(0.011)	(0.010)	(0.016)	(0.010)
x Defendant is the father		0.032***				
		(0.009)				
x Parent HS degree			0.012			
			(0.011)			
x Parent aqcuitted				0.0053		
				(0.013)		
x Parent misdemaneor					0.003	
					(0.011)	
x Nbh offenders < med						0.003
						(0.010)
Sample mean	0.023	0.023	0.023	0.023	0.023 2	0.023
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,119	5,119	5,119	5,119	5,119	5,119

Note: This table reports difference-in-differences estimates based on model (1) of the effect of exposure on compulsory school GPA (percentile rank) in grade 9. The sample consist of children to defendants in our main sample belonging to the cohorts who graduated from compulsory school 2010-2016. The regressions control for the same characteristics of the parents as in Table 2. Numbers in brackets show mean of the dependent variable. Standard errors in parenthesis are clustered at the individual and court level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix B: Data

Exposed: An indicator equal to one if the individual was sentenced within 12 months after the court-specific cutoff month.

Non-Exposed: An indicator equal to one if the individual was sentenced within 12 months before the court-specific cutoff month.

Male: An indicator equal to one if the individual is a male.

Immigrant: An indicator equal to one if the individual was born outside of Sweden.

Age at Initial Trial: We calculate age at initial trial as the year of the trial minus the calendar year when the individual is born.

High School Degree: An indicator for whether an individual has a high school degree or a higher education degree (e.g., college, university).

Convicted: An indicator set to one if an individual was charged and convicted in the initial trial.

Acquitted: An indicator set to one if an individual was charged and acquitted in the initial trial.

Misdemeanor: An indicator set to one if the the offender committed a crime that no defendant has ever been sentenced to prison for in our data.

Convictions per 10,000 inhabitants: The number of convictions per 10,000 inhabitants in the municipality of residence.

Criminal Conviction: An indicator for whether the charged individual is convicted of new crime in a given year.

Employment: Measures the employment status of the charged individual in a given year. A binary employment indicator set equal to one if the charged individual is registered as employed on Nov 1st.

Earnings ($1,000s)): Annual labor earnings. Earnings are deflated to 2015 and represented in U.S. dollars using the exchange rate SEK/$ = 9.25.

Mobility: An indicator set to one if the individual has changed place of residence in a given year.

In college: An indicator set to one if the individual was enrolled in college education in a given year.

Labor market training: An indicator set to one if the individual was enrolled in labor market training in a given year.

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