Workplaces and Intergenerational Mobility in Germany*

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Abstract

We study the role of workplaces in intergenerational mobility using a novel dataset that enables the identification of household relations in German administrative data for more than 2 million children. First, we provide new empirical evidence on intergenerational mobility in Germany and its spatial distribution. We show that the local labor market structure and the average firm pay premia strongly predict the regional variation in upward mobility. Exploiting variation in children's ages at the time their fathers move across regions, we demonstrate that a significant share of exposure effects of better places comes from access to higher-paying firms. Second, we construct establishment-level mobility statistics using detailed matched employer-employee data. High-opportunity firms are high-paying firms that are strongly segregated by parental income background, but substantially improve the subsequent earnings of low-income children hired. Third, leveraging variation in the composition of labor demand that labor market entrants face, we find that contractions in hiring by high-opportunity firms reduce upward mobility for high-skilled low-income children. Fourth, we simulate how much intergenerational mobility would change if the allocation of workers to firms were parental-income-blind, holding fixed workers' skills and occupations. We estimate that inducing parental-income-blind hiring reduces the rich-poor top-quintile outcome gap by 12%.

*preliminary draft, please do not circulate

1 Introduction

Economic disadvantage is inherited: children who grow up in lower-income households tend to have lower incomes as adults themselves (Black and Devereux, 2011; Deutscher and Mazumder, 2023). The chances of breaking this cycle and achieving upward mobility vary sharply across countries (Manduca et al., 2024; Boustan et al., 2025) and even across regions within countries (Chetty et al., 2014; Chyn and Katz, 2021). The intergenerational mobility literature has traditionally explained such differences by emphasizing human capital as an important cause of children's adult outcomes (Becker and Tomes, 1979).

By contrast, the literature on labor market inequality shows that workplaces are another central source of earnings differences among otherwise similar workers (Abowd et al., 1999; Card et al., 2013; Kline, 2024). Yet workplaces have remained largely absent from the study of intergenerational mobility (Dobbin and Zohar, 2025). Understanding the role of workplaces is key to understanding why economic advantage persists across generations. Firms differ systematically not only in pay, but also in career progression and job stability, and these differences are especially consequential at the beginning of workers' careers (von Wachter and Bender, 2006).

Although recent work has studied how person and establishment premiums contribute to the intergenerational earnings elasticity , much less is known about the causal effect of firms on the upward mobility of young workers (Card et al., 2026). This is because decompositions of estimated worker and firm components need not identify the causal role of workplaces in upward mobility if young workers sort nonrandomly across firms or if firm effects differ across the parental income distribution. Moreover, the gains from working at certain firms may emerge only gradually through learning and career progression (Rosen, 1972; Gibbons and Waldman, 2006; Arellano-Bover and Saltiel, 2026); so decompositions based on current employer firm effects may understate the role of firms in upward mobility.

This paper documents substantial positive causal effects on upward mobility of exposure to high-paying workplaces and of starting one's career at an employer with a high mobility rate among children from low-income families. We further find that a large share of these gains is driven by faster growth in the worker component of two-way fixed-effects wage decompositions (Abowd et al., 1999), consistent with an important role for human capital accumulation within firms.

Using rich matched employer–employee data, we identify intergenerational relationships in German administrative data for the first time. Germany's geographic size and the long panel dimension of our labor market data allow us to estimate area-exposure effects from childhood moves (Chetty and Hendren, 2018) and to trace how these effects map into worker and firm components at ages 29 to 33. We next construct upward-mobility rates for individual firms, defined as the product of the access they provide to children from low-income households and those children's conditional probability of reaching the top tercile of the earnings distribution. We exploit the timing of idiosyncratic firm-specific labor demand shocks affecting labor market entrants (Arellano-Bover, 2024; Rose and Shem-Tov, 2025), which shift the composition of available jobs across high- and low-opportunity workplaces in young workers' predicted regions of entry, to identify the long-run consequences of early-career workplaces for children from low-income backgrounds.

Our analysis has five different parts. First, we present new empirical evidence on intergenerational income mobility in Germany, the world's third-largest economy, for which large-scale administrative data with intergenerational links have so far not been available.[1] Exploiting address and surname information, we link 2.2 million children born between 1985 and 1990 to their fathers and construct measures of permanent income for both generations. We find that the association between mean child income ranks and father income ranks is almost linear. On average, a 10 percentile-point increase in the father's income rank is associated with a 2.9 percentile-point increase in the child's income rank in Germany.[2]

We document substantial geographic variation in both relative and absolute upward mobility. Intergenerational mobility is lowest in eastern Germany and highest in the south, where labor markets are stronger and income levels are higher. We show that local labor market conditions—and especially the average AKM firm pay premium—account for more than 46% of the cross-regional variation in absolute upward mobility and 32% of the corresponding variation in relative mobility. By contrast, the correlations between our mobility measures and the sociodemographic factors emphasized in prior work—such as segregation, income inequality, and school quality (Chetty et al., 2014)—become much weaker once we control for local labor market conditions, pointing to an outsized role for labor markets.

In the second part of the paper, using the empirical framework introduced in Chetty and Hendren (2018), we identify the causal effect of place exposure by exploiting variation in the age at which children move within Germany. We find that, on average, spending an additional year during childhood in a local labor market where the mean income rank of children of permanent residents is 1 percentile higher, within a given birth cohort and parental income group, raises a child's own income rank at age 30 by about 0.021 percentiles.[3]

We then turn to the labor market channels through which place matters, focusing on exposure to better firms and to peers with skills that are more highly valued in the labor market. We focus on firms for two reasons. First, a large labor economics literature shows that firms are a central source of earnings differences (Abowd et al., 1999; Card et al., 2013). Second, recent work points to firm and human capital channels as distinct drivers of intergenerational mobility (Forsberg et al., 2024; Dobbin and Zohar, 2025; Card et al., 2026). Unlike this second strand of the literature, however, our approach allows us to directly estimate the causal contributions of firm and human capital channels to mobility. Our estimates imply that about 75% of these earnings gains are accounted for by improvements in worker components by age 30, with the remaining 25% accounted for by employment at higher-paying firms.

Third, we leverage our detailed linked employer-employee data to construct establishment-level mobility statistics.[4] We focus on two channels through which establishments could contribute

[1] By contrast, German administrative data have been used extensively to study labor market inequality in a large and influential body of work; see, for example, Card et al. (2013); Dustmann et al. (2026).

[2] In Section 3, we discuss the representativeness of these estimates and compare them to survey-based estimates from the German Socio-Economic Panel (GSOEP). Previous work using the GSOEP relies on an effective sample size of roughly 1,000 child–father pairs (Baarck et al., 2025).

[3] This annual convergence rate of 2.1% is about half as large as in the United States and Australia (Chetty and Hendren, 2018; Deutscher, 2020), but very similar to recent European evidence: Beekers (2024) estimates an exposure effect of 0.0211 for the Netherlands.

[4] Throughout this paper, we use the terms establishments and firms interchangeably.

to intergenerational mobility: (i) hiring patterns, and (ii) conditional on hiring, pay policies and opportunities for promotion, that determine career progression. We calculate these statistics as the fraction of children from low-income fathers (in the bottom tercile of the income distribution) and the fraction of such children who reach the top tercile of the income distribution in adulthood at each establishment. The product of these two statistics is the establishment's upward mobility rate, which is conceptually similar to *college scorecards* introduced in Chetty et al. (2020). We find that firms that contribute the most to upward mobility are also the most selective in hiring from low parental income background, and that segregation of workers across firms by parental background can be partially but not completely accounted for by skills, with an estimated contribution of around 35%.

Fourth, we use an instrumental-variables design to estimate the returns to working at a higher mobility firm early in one's career for children from low-income families. Our instrument exploits variation over time and across regions in the composition of regional labor demand for young workers to predict the mobility rate of the first employer. The instrument captures the relative demand of high-mobility versus low-mobility firms—while holding constant the overall level of labor demand- a child faces in the predicted year and region of labor market entry. Predictions are based on fathers location and standard German school completion times. We find that starting one's career at a higher mobility firm is highly beneficial for children with fathers in the bottom tercile of the national income distribution. Our 2SLS estimates imply, that a one standard deviation increase in the predicted mobility rate of the first employer increases average yearly income at age 29-33 by 12.5%. Around 25% of the total effect of starting at a higher mobility firm at age 16-24 on income at age 29-33 operates through the firm channel, as higher mobility firms are on average more productive. Dynamic and improved skill accumulation at higher mobility firms, measured by the individual AKM component, accounts for a large share of the increase in earnings.

Fifth, we quantify how intergenerational mobility would change if residual segregation by parental background —holding skills fixed—were eliminated. We achieve this by randomly assigning each slot in a given establishment to a randomly drawn individual from the same 2-digit occupation $\times$ skill decile cell as the original employee. As we abstract from changes in individual skills and changes in the firm pay premia due to different resulting allocations, and thus not take dynamic skill accumulation that arises due to longer exposure at higher mobility firms into account, our simulation results should be interpreted as lower bounds. Or in other words, we simulate how much mobility measures would change if the current worker-firm allocation at age 30 would be parental income blind, holding constant both the skill $\times$ occupation composition in each firm and worker's skills. We find that inducing parental-income-blind hiring reduces the rich-poor top-quintile outcome gap -measured by difference in the probability to reach the top income quintile for children from the bottom versus top father income quintile- by 12%.

Our paper contributes to the literature on the role of place in shaping economic opportunity (Chetty et al., 2014; Nakamura et al., 2022; Chyn et al., 2025; Chetty et al., 2026). We extend this literature by bringing firms into causal exposure-mover designs, focusing on outcomes between ages 29 and 33, when workers have typically progressed further up the firm ladder. We also contribute to the literature on labor-market segregation by parental income, including Haeck and Laliberté (2025) on occupational

segregation and Lo Bello and Morchio (2022) and Long and Ferrie (2013) on occupational persistence.[5] This literature has mainly focused on occupations as proxies for expected labor-market trajectories, whereas we focus on firms as an important determinant of careers, influenced by a large literature in labor economics that has shown that pay-setting and wages differ systematically across employers for identical workers (Abowd et al., 1999; Card et al., 2013).

Dobbin and Zohar (2025) quantify the contribution of firms to intergenerational earnings persistence in Israel by developing an AKM-based decomposition of the IGE into worker and firm components. They show that sectoral sorting accounts for a significant share of the relationship between parental background and access to high-paying firms. Forsberg et al. (2024), using a related approach with Swedish data, add firm-specific returns to experience, which increases the importance of firm components relative to static decompositions. For the United States, Card et al. (2026) also apply AKM-style decompositions and further decompose parental earnings into worker and firm components, while incorporating neighborhood effects in a parsimonious way. Their results largely support a block-diagonal structure: children's individual wage components are primarily related to the individual wage components of their parents and neighbors, whereas children's firm-related wage components are primarily related to the firm components of parents and neighbors. In contrast to these studies, our paper first investigates the causal effect of neighborhood exposure on firm components using mover designs and, second, provides IV estimates that, under our identification assumptions, account for sorting on unobservables. We then use this second approach to estimate the effects of starting one's career at a high-opportunity firm for children from low-income households, documenting substantial causal effects. Moreover, we simulate how equalizing access to firms across parental-income backgrounds, conditional on skills at labor-market entry, affects intergenerational mobility, thereby moving beyond effects on conditional mean outcomes.

Finally, the paper provides new insights into intergenerational mobility in Germany at a new level of precision and granularity by leveraging administrative data, which was not feasible in earlier work based on German survey data (Eisenhauer and Pfeiffer, 2008; Heineck and Riphahn, 2009; Baarck et al., 2025).[6]

The next section of the paper provides an overview of our data source and the linkage algorithm. Section 3 reports mobility estimates at the national and sub-national level, and causal neighborhood exposure estimates. In Section 4 we present our firm-level mobility statistics. Section 5 reports causal estimates of starting one's career in firms offering different levels of economic opportunity. In Section 6 we simulate how mobility estimates would change if residual segregation by parental background across firms—holding skills fixed—were eliminated.

[5] Haeck and Laliberté (2025) document substantial segregation into occupations by parental income in Canada. However, they find that occupational persistence plays only a limited role for income mobility, explaining just 10 percent of the income rank-rank relationship.

[6] Dodin et al. (2024), link half a million children using the German Microcensus and study geographic variation in the association between parental income and children's educational opportunities in Germany but are limited to educational outcomes due to data constraints.

2 Data

We use matched employer–employee data derived from the universe of German social security records spanning the period 1975–2022. This data has been compiled by the Institute for Employment Research into the Integrated Employment Biographies (IEB).[7] The IEB covers all individuals in employment subject to social security, those in marginal employment, recipients of unemployment insurance benefits, registered job seekers, and participants in active labor market or training programs. Overall, the IEB covers approximately 80 percent of the German labor force. Civil servants and self-employed individuals are excluded, as their employment is not subject to social insurance provided through the Social Security system. The data consists of day-specific spells and includes information on employees' earnings (reported up to the contribution assessment ceiling), educational attainment, occupation, and core sociodemographic characteristics such as gender, date of birth, and citizenship. For the empirical analysis, we convert the spell-based information into an annual panel. Specifically, for each individual and calendar year, we retain only the spell that overlaps June 30 and aggregate daily earnings within the corresponding calendar year to obtain a measure of annual earnings.

2.1 Household Identification in German Administrative Data

In this section, we explain how we identify family relationships in the German administrative data, as these are not directly observed in the admin data.

1. Linkage Algorithm. Our algorithm for identifying households in the IEB leverages information on exact residential locations and surnames to probabilistically link family members. Methodologically, it closely follows the existing approach described in Goldschmidt et al. (2017) and Bächmann et al. (2021) for the identification of married couples in the IEB and consists of three steps: (i) identification of candidate households based on address information, (ii) differentiation of households within each address using surname information, and (iii) assignment of household roles to individuals within these inferred households.

We begin by augmenting the IEB records for the period 2001–2015 with geocoded data detailing the precise residential locations of all individuals included in our dataset.[8] We exclude all observations associated with geocodes for which (i) fewer than two individuals are observed at the same address, or (ii) no individual younger than 26 years is observed in a given year, since such geocodes cannot host a household with children.

Second, following Goldschmidt et al. (2017), we assume that members of the same household share a common surname.[9] We link the person-level data, including geocodes, to individual names. For each

[7] *IAB Integrierte Erwerbsbiografien (IEB) V17.00.00-202212, Nürnberg 2023*

[8] *Geocodierte Adresshistoriken von IEB-Personen, -Betrieben und -Bedarfsgemeinschaften (IEB GEO) V02.01.00-201804, Nürnberg 2021).* The IEB GEO contains the exact residential locations of all IEB individuals for the years 2000-2017. Persons' addresses originate from three sources: the social security records (DEÜV-Meldungen) according to the social code IV, the base data of the federal employment agency, and the base data of authorized municipal authorities according to the social code II (zugelassene kommunale Träger). These addresses are processed by a geo-information system (GIS) with reference date March 2015.

[9] Surnames in our data originate from the following three sources: the social security records (DEÜV-Meldungen messages, social code IV), the base data of the federal employment agency, and the base data of authorized municipal authorities (zugelassene kommunale Träger, social code II)

year between 2001 and 2015 and for each geocode, we construct a unique household identifier that assigns the same household identifier to all individuals who, in a given year, reside at the same address and share the same (or sufficiently similar) surname.[10]

Finally, for each potential child (defined as individuals younger than 26 years), we identify the most probable father and mother across all years from 2001 to 2015 within each household identifier. This is accomplished by leveraging information on gender and date of birth for all individuals within a household identifier. In cases where a child is associated with multiple conflicting potential mothers or fathers, we assign the most frequently observed candidate parent.

Note that to successfully identify parent-child links, parents and children have to live at the same address, share the same surname, and have an IEB entry covering June 30th in the same year at least once between the years 2001-2015, while the child is below 26 years old. Once such a connection is successfully identified, we can observe the full employment history of parents and their offspring between 1975 and 2022 in the de-identified IEB described at the beginning of this section, irrespective of the place of residence or the surname.

2. Match Results. The linkage procedure enables the identification of parent–child relationships for over 12 million children born between 1975 and 2000 in the IEB dataset.[11] We identify 6.5 million mothers and 5.2 million fathers in the data. For 3.8 million children only the mother can be identified, and for 2.1 million children only the father can be identified. For the remaining 6 million children, both parents can be identified. The mean number of children per parent is 1.5. The sample of children comprises 55% males and 45% females. This gender composition remains stable across household types, irrespective of whether only the mother, only the father, or both parents are identified.

Furthermore, for 96% (98%) of children, the linkage to the mother (father) is established via an exact match of surnames. In more than 98% of all child–parent linkages, the identified match is unique, implying that for 98% of all children there existed only a single candidate mother/father over all years and across all records observed before the child reached the age of 26.

2.2 Sample and Variable Definitions

Our base data set of children consists of all individuals who (i) were born between 1985 and 1990, (ii) have an identified father for whom we can construct reliable income measures, and (iii) are German citizens by the age of 33. We impose the birth cohort restriction because the surname and address information required to link children and their parents is only available after 2000. Including earlier cohorts requires for household identification that children remain living at home longer. Analogously,

[10] We closely follow Goldschmidt et al. (2017) in correcting name-related errors and typographical mistakes, in handling German special characters (e.g., ä, ö, ß, etc.), and in dealing with hyphenated names. In addition, we allow for partial matches of surnames within the same geocode using the Jaro–Winkler distance measure (Winkler, 1990). Approximately 98% of all matches are exact matches.

[11] Mechanically, we are unable to identify children born before 1975 because information on surnames and addresses is only available from 2001 onward, and we operationalize "children" as individuals younger than 26 years. For earlier birth cohorts, record linkage is therefore highly selective, as successful linkage requires that individuals still reside with their parents in their mid- to late twenties. Similarly, as we use only data on surnames and address up to 2015, children born after 2000 cannot be identified: their linkage would require labor market entry in 2015 at an age younger than 16. In practice, the majority of children can be linked between ages 16 and 21, when they typically enter the labor market while still co-residing with their parents.

since we use surname and address information up to 2015, including later birth cohorts only works if children participate in the labor market at young ages. Thus, the share of identifiable children decreases for earlier and later birth cohorts. Including children for whom only the mother can be identified introduces measurement error at the bottom of the parental income distribution, as we cannot differentiate between lower income single-mothers, and families with a high income father working as a civil servant or being self-employed (as these fathers are not observed in the IEB). Thus, we restrict the base data set to children with identified fathers. Relaxing this restriction does not change our baseline results. The core sample contains more than 2.2 million children, covering approximately 50% of all individuals from these birth cohorts.

We measure all monetary variables in 2015 euros, adjusting for inflation using the consumer price index provided by the research data center of the IAB.[12]

Father Income. Our income measure is derived directly from the IEB and is defined as gross daily wages aggregated to annual gross wage income. Specifically, we compute gross annual earnings from the fixed-period wages reported by employers and the duration of the (unsplit) original notification period in calendar days. The annual income measure captures wage earnings from all employment spells subject to social security contributions within a given calendar year. Earnings that exceed the upper earnings limit for statutory pension insurance are censored at this threshold in the administrative data. For these cases, we use imputed wages as described in Drechsler and Ludsteck (2025). For calendar years in which fathers are not observed in the IEB on June 30th, we define income as missing. To construct a proxy for long-term parental income that is less affected by transitory income shocks, we average the annual labor earnings of fathers over the years in which the child is between the ages of 15 and 19. Finally, we define father's income percentile ranks based on their positions in the cross-sectional distribution of father incomes among all fathers with children born in the same birth cohort.

Father Location. In each year, fathers are assigned municipality codes based on the municipality in which their employer is located. We aggregate fathers' municipality-level information to 141 local labor markets (LLMs), as defined in Kosfeld and Werner (2012).[13]

Child Income. We define child income analogously to father income. In our baseline specification, we measure child income as the average of annual labor earnings over the years in which the child is between 29 and 33 years old. We define children's percentile ranks based on their position in the distribution of child income within their respective birth cohorts.

Workplaces. The linked employer–employee data enable us to identify the employers of individuals at specific ages, as well as to construct experience-weighted measures of employer affiliations within our core dataset. We will elaborate on these measures and the related methodological approaches in Section 4 and subsequent sections.

AKM measures. We use pre-estimated worker and establishment fixed effects obtained from Abowd–Kramarz–Margolis (AKM)–type regressions (Abowd et al., 1999). These AKM specifications are estimated based on 7 year windows in the full administrative data (for a detailed description of the

[12] Website of the FDZ

[13] Information on place of residence is only available from 2001 onward. Consequently, we use information on place of work as a proxy for place of residence. In years for which both place-of-residence and place-of-work information is available, approximately 80% of fathers in our core sample both work and reside in the same LLM.

estimation procedure and data construction, see Lochner and Wolter (2025)). For some specifications, we compute our own AKM measures for different observation windows, to have a better overlap between the AKM estimation window and the age at which we measure child outcomes.

3 Intergenerational Mobility in Germany

3.1 Baseline Estimates

In our baseline analysis, we use the core sample (1985-1990 birth cohorts) and measure parent income as mean father income when the child is between 15 and 19 years old, and child income as mean individual income between age 29 to 33. Our measure of relative mobility is the rank-rank slope or Spearman correlation, which is obtained from the following regression:

$$y_{i,s} = \alpha + \beta p_{i,s} + \epsilon_i, \tag{1}$$

where $y_{i,c}$ is the child's income rank, relative to all other children in cohort s, and $p_{i,s}$ is the father's income rank, relative to all other fathers with children in cohort s. Unless otherwise noted, we hold the definition of these ranks fixed based on positions in the aggregate distribution, even when analyzing subgroups.

Figure 1 presents a binned scatter plot of the mean percentile rank of children $E[y_i|p_i = p]$ versus their fathers' percentile rank p. The conditional expectation of a child's rank given his fathers' rank is almost perfectly linear. Using an OLS regression, we estimate that a 1 percentage point (pp) increase in father rank is associated with a 0.28 pp increase in the child's mean rank.

Our baseline estimates are robust to different income and family definitions: including children of single mothers, using parental household income, or restricting the sample to children for whom both mothers and fathers can be identified, does not change our results. Potential concerns regarding the representativeness of our estimates and sample may arise from biases introduced by the household linkage algorithm. To address these concerns, we present two sets of evidence: (i) reweighted estimates and (ii) a comparison with intergenerational mobility estimates reported in prior studies on Germany.

We construct a full sample of children born between 1985 and 1990 who satisfy the sampling criteria discussed in Section 2.2, with the exception of the requirement that a father is observed. This sample comprises approximately 4.5 million individuals from the IEB. Imposing the requirement that the father be observed reduces the sample size to 2.2 million observations from the core sample. We apply entropy balancing (Hainmueller, 2012; Doudchenko and Imbens, 2016) to compute weights for the 2.2 million individuals in the core sample, thereby reweighting the core sample to be representative of the full sample of potential children.[14] The reweighted OLS estimates of equation 1 are very similar to the slope of 0.28 depicted in Figure 1.

Additionally, we compare our slope estimate of 0.28 with existing evidence for Germany to assess the representativeness of our core sample. Prior work in Germany typically relies on the German Socio-Economic Panel (GSOEP)—a nationally representative household panel survey—which usually yields an effective sample size of approximately 1,000 children for intergenerational mobility analysis.

[14] We weight based on gender, birth years, school degrees, observed labor market outcomes at age 29-33 (days in employment, observed wages, industry), and regional information

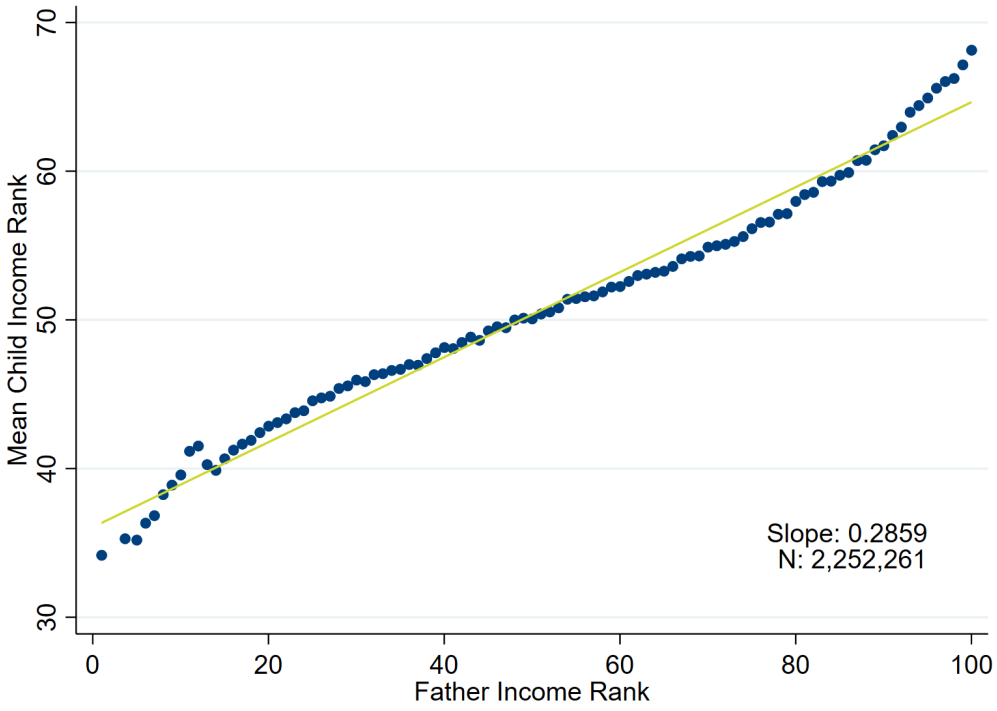


Figure 1. Association between Children's and Fathers' Percentile Ranks

Notes: The Figure shows a nonparametric binned scatter plot illustrating the association between children's and fathers' percentile income ranks in the core sample (birth cohorts 1985–1990). Children's income is measured as the average labor earnings between ages 29 and 33, while fathers' income is defined as the average labor earnings during the years when the child is 15 to 19 years old. Children are ranked relative to all other children in their own birth cohort, and fathers are ranked relative to all other fathers in the core sample who have children in that same cohort. The slope and best-fit line are estimated using an OLS regression on the micro data.

Reported rank–rank slope estimates based on GSOEP data range from 0.25 (Bratberg et al., 2017) to 0.35 (Dodin et al., 2024), depending on the underlying sampling criteria, income definitions, and child cohorts considered (Schnitzlein, 2016). In a recent paper, Baarck et al. (2025) provide cohort-specific estimates and document a rank–rank slope of 0.279 for birth cohorts 1978–1987 (see Table 1 in Baarck et al. (2025)), which is very close to our baseline estimate.

3.2 Regional Variation

We now exploit the extensive scale of our newly constructed dataset to document heterogeneity in intergenerational income mobility across areas within Germany. With the exception of Dodin et al. (2024), who analyze regional variation in social mobility based on educational outcomes in Germany, previous research has not been able to provide sub-national estimates of income mobility due to the limited sample size of available survey data. We first describe our definition of location and then present estimates of relative and absolute intergenerational income mobility by area. Finally, we examine the association between our measures of intergenerational mobility and a set of characteristics of the local area.

We partition Germany into a set of geographical areas in which children grow up based on an approach introduced in Kosfeld and Werner (2012). Specifically, they delineate 141 local labor markets (LLMs), which are constructed as aggregations of German districts based on commuting patterns. We

classify our sample into "movers" and "permanent residents" using the location history of fathers. Permanent residents are defined as children whose fathers did not change LLMs during childhood. Among the 2.2 million father–child pairs in our core sample, approximately 1.3 million can be classified as permanent residents. The analysis in this section focuses on this subsample. Each child in this group is permanently assigned to the single LLM in which the father continuously was observed, which we interpret as the child's area of upbringing. This LLM does not necessarily correspond to the LLM in which the child resides in adulthood, when we observe labor income at ages 29 to 33. In our data, 33% of children are located in a different LLM at age 30 than the one in which they grew up.

To obtain estimates of relative mobility at the LLM level, we estimate equation 1 separately for each LLM. Importantly, we continue to rank both children and fathers based on their positions in the national income distribution in the core sample, rather than in the income distribution of permanent residents within their respective LLM. Panel A of Figure 2 presents the resulting slope estimates. We document substantial heterogeneity in relative mobility across LLMs. On average, a 10 percentile rank increase in father income is associated with a 2.9 percentile rank increase in the income rank of children in regions in the bottom decile (in terms of relative mobility). In contrast, in a region in the top decile, a 10 percentile rank increase is associated with a 1.9 percentile rank increase in the incomes of children.

Additionally, we define *absolute mobility* at percentile p in LLM l as the expected income rank of a child who grew up in LLM l whose father has a national income rank of p:

$$\bar{r}_{pl} = \hat{\alpha}_l + \hat{\beta}_l p \tag{2}$$

We focus on average absolute mobility for children from families with below-median father income in the national distribution ($E[y_{i,s}|p_{i,s} < 50]$). Because the rank–rank relationship is approximately linear, the average rank of children with below-median father income is equal to the average rank of children whose fathers are at the 25th percentile of the national income distribution, i.e. $\bar{r}_{25,l} = \hat{\alpha}_l + 25\hat{\beta}_l$. Panel B of Figure 2 plots $\bar{r}_{25,l}$, the expected income rank of children whose fathers are in the lower half of the national father income distribution. Regional disparities are even more pronounced when focusing on absolute mobility: children with fathers in the bottom half of the national income distribution who grow up in a bottom-decile region (in terms of absolute mobility) end up 12 percentile ranks lower in the income distribution, in expectation, than their low-income peers growing up in a top decile region.

The geographical patterns in relative mobility and absolute mobility across LLMs in Figure 2 are very similar. The unweighted correlation across LLMs between the two measures is -0.66; the population-weighted correlation is -0.67. This suggests that places with higher levels of relative mobility generally produce better absolute outcomes for children growing up in low-income families. A more systematic investigation of the relationship between the two measures reveals that places with higher relative mobility (lower β_l) produce not only better outcomes for children from low-income fathers, but for children from all father-income backgrounds (see Figure A2).

Why do some regions display substantially higher levels of upward mobility than others? As a first step toward addressing this question, we relate our measures of intergenerational mobility to a set

(A) Relative Mobility: Rank-Rank Slopes by LLM

(B) Absolute Upward Mobility: $\bar{r}_{25}$

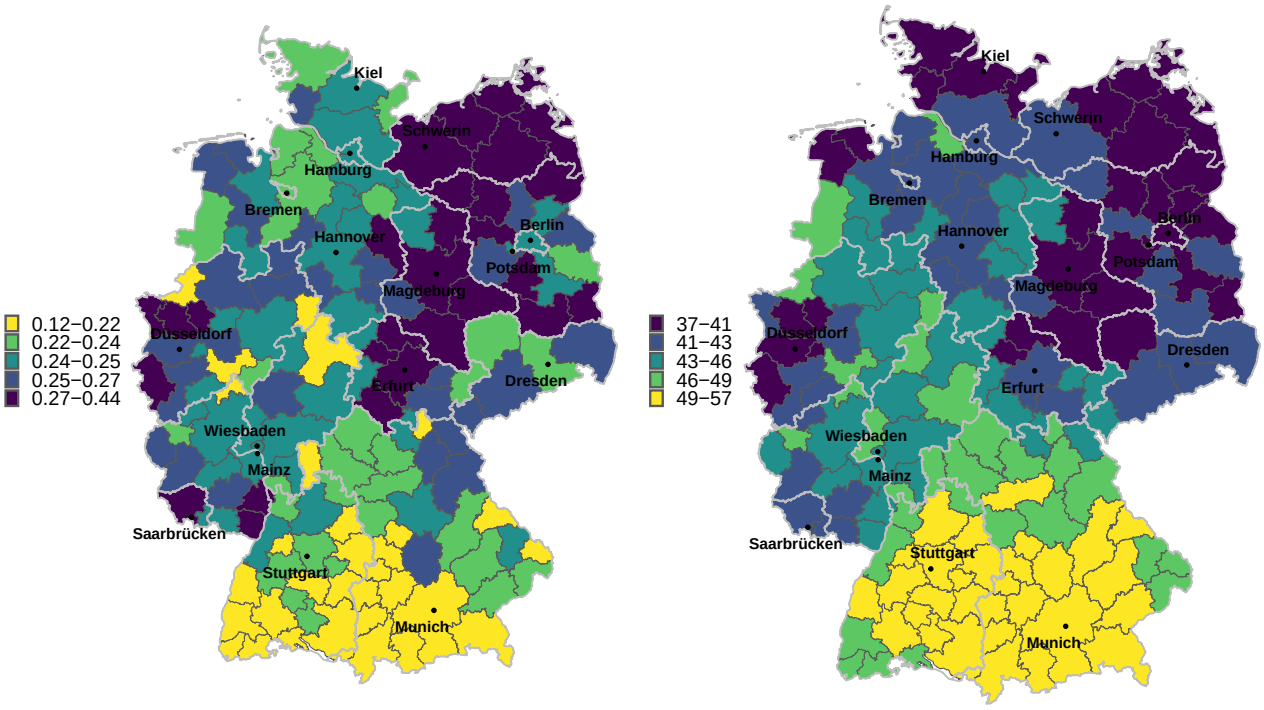


FIGURE 2. The Geography of Intergenerational Mobility in Germany

Notes: The figure shows heat maps of our two intergenerational mobility measures across local labor markets (LLMs). The maps are constructed using permanent residents in our core sample. We classify children as permanent residents according to their fathers' location histories: a permanent resident child is one whose father never moves across LLMs while the child is between ages 0 and 30. We assign children to this constant LLM, irrespective of where they live as adults. In each LLM, we regress the child's income rank on a constant and the father's income rank. Panel A displays these relative mobility estimates (rank-rank slopes). Panel B presents absolute mobility estimates, which we calculate as the intercept + 25 x (rank-rank slope). This corresponds to the predicted average rank of children with below-median father income. The maps are constructed by grouping LLMs into 5 quintiles. Darker shading indicates lower relative mobility (higher slopes) and lower absolute mobility (lower expected income ranks).

of local-area characteristics. Naturally, such correlations cannot be interpreted as causal relationships. Our objective is solely to motivate the subsequent focus on workplaces by showing that, in the German context, firm- and labor-market-related factors explain a larger share of the cross-LLM variation in mobility indicators than do socio-demographic characteristics of regions.

We start by regressing our mobility estimates on a set of local labor market characteristics that have been shown to be able to explain a large share of the variation in mobility estimates across places in the US (Chetty et al., 2014). Namely we focus on: Commuting distances within LLMs, local Gini, High-school dropout rates, and the share of foreigners. We standardize both our measures of mobility, and all regressors to have mean 0 and standard deviation 1. Jointly, these local characteristics can explain 40% of the variation in absolute mobility and 21% of the variation in relative mobility across regions, compared to 75% and 48% in the US (compare columns (1) and (5) of Table 1 with Table VI in Chetty et al. (2014)). In contrast the firm quality structure, measured by the average firm AKM pay premium can alone explain 46% of the variation in absolute mobility and 32% of the variation in relative mobility (see columns (2) and (6) of Table 1). Dobbin and Zohar (2025) and Forsberg et al. (2024), while employing a different decomposition method on the national level in Israel and Sweden,

Table 1. Correlates of Intergenerational Mobility

	Absolute upward Mobility				Relative Mobility			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Avg. Commuting Distance	-0.049			-0.0011	0.19*			0.16
	(0.088)			(0.056)	(0.085)			(0.086)
Gini	-0.48***			-0.077	0.20*			-0.15
	(0.10)			(0.056)	(0.095)			(0.12)
School Dropouts	-0.059			-0.066	0.057			0.046
	(0.061)			(0.039)	(0.11)			(0.098)
Fraction Foreigners	0.29***			-0.10	-0.31***			0.27*
	(0.074)			(0.095)	(0.087)			(0.12)
Avg. Firm Quality		0.69***	0.39***	0.47***		-0.57***	-0.46***	-0.77***
		(0.053)	(0.047)	(0.092)		(0.082)	(0.092)	(0.17)
Employment-to-Pop. Ratio			0.31***	0.35***			0.031	-0.016
			(0.065)	(0.069)			(0.092)	(0.11)
Share Manufacturing			0.31***	0.19			-0.24	-0.082
			(0.084)	(0.10)			(0.12)	(0.18)
N	141	141	141	141	141	141	141	141
adj. R^2	0.392	0.467	0.719	0.725	0.213	0.323	0.347	0.371

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Each column presents coefficients from an OLS regression estimated at the LLM level. In columns (1)–(4), the dependent variable is our baseline measure of absolute upward mobility, defined as the expected rank of children whose fathers are at the 25th percentile of the national income distribution. In columns (5) to (8), the dependent variable is relative mobility, measured by the rank–rank slope within each LLM. All dependent and independent variables are normalized (within the relevant estimation sample) to have mean 0 and standard deviation 1. The average commuting distance is computed for fathers in our core sample in 2005. The Gini coefficient is calculated from fathers' annual labor earnings recorded in the IEB for the core sample in 2005. The school dropout rate is defined as the residual from a regression of the share of children in the LLM who do not complete high school—estimated using data from INKAR in 2005, on mean household income (also from INKAR, 2005). The fraction of foreigners is measured as the average share of foreigners relative to the total population in 2005, using INKAR data. Average firm quality is defined as the mean firm AKM pay premium in 2005 (AKM estimation window 2002-2009). The employment-to-population ratio and the share of workers employed in the manufacturing sector are calculated based on INKAR in 2005. All INKAR-based variables are aggregated to the 141 LLMs by weighting district-level values by the total number of inhabitants.

respectively, find that the firm AKM component on the national level accounts for 23 percent and 31 percent of relative mobility. When we include additional characteristics of the labor market such as the employment-to-population ratio and the share of employees working in the manufacturing sector, the adjusted R2 increases to 0.72 and 0.35, respectively.

Taken together, these patterns indicate that firm-level characteristics and workers' differential access to higher-paying, more productive employers are likely key factors behind regional variation in mobility measures in Germany, compared to the US, where no systematic correlation between mobility and local labor market conditions has been documented (Chetty et al., 2014). Before analyzing workplace characteristics in detail, we first assess whether these patterns also hold in a causal framework.

3.3 Exposure Effects of Place

We follow the estimation strategy introduced in Chetty and Hendren (2018) to identify causal place effects, with the goal of quantifying how much a child's potential outcomes would improve if they grew

up in an area where the outcomes of permanent residents are higher. This strategy consists of two steps. In the first step, we use data on permanent residents to construct predicted outcomes for children who grow up in a given area. In the second step, we examine children who move exactly once across areas to estimate childhood exposure effects. These exposure effects capture how much moving one year earlier shifts expected outcomes from those predicted for the child in their origin toward those predicted in their destination. We provide a brief description of these two steps below. A more detailed and formal exposition of the identification strategy can be found in Chetty and Hendren (2018).

Step I: Predicted Outcomes. We use the sample of permanent residents—defined as children in our core sample whose fathers do not move across LLMs before the child reaches age 30—to predict outcomes for individuals who grow up in a given area. To do this, we estimate equation 1 separately for each LLM. The fitted model then generates two predicted ranks for every child in the subsample of one-time movers: $\bar{y}_{pos}$ and $\bar{y}_{pds}$. These reflect the expected income ranks of children growing up in the origin (destination) region o (d) born in the same birth cohort s, and have fathers in the same income percentile p, serving as counterfactuals, that they were permanent residents of their origin and destination, respectively.[15]

Step II: Expected Outcomes of Movers. In the second step, we use the subsample of children whose fathers moved exactly once before the child reached 30 years of age, in order to estimate the causal effect of place. We use the same specification as Chetty and Hendren (2018) to estimate a child's outcome in adulthood as a function of their predicted origin outcome ($\bar{y}_{pos}$) and the "shock" to this coming from their destination ($\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$), interacted with their age at move m. Formally, we estimate the following model:

$$\begin{aligned} y_i = &\sum_{s=1985}^{1990} I(s_i = s)\left(\alpha_s^1 + \alpha_s^2 \bar{y}_{pos}\right) + \sum_{m=1}^{29} I(m_i = m)\left(\varsigma_m^1 + \varsigma_m^2 p_i\right) \\ &+ \sum_{m=1}^{29} b_m I(m_i = m)\Delta_{odps} + \sum_{s=1985}^{1989} \kappa_s^d I(s_i = s)\Delta_{odps} + \varepsilon_i, \end{aligned} \tag{3}$$

where y_i denotes the child's income rank and $I(x_i = x)$ is an indicator function that is 1 when $x_i = x$ and 0 otherwise. The first two terms of this specification control for origin quality and disruption effects. The third term represents the exposure effects of interest and the fourth consists of cohort interactions with Δ_{odps} to control for differential measurement error across cohorts.

The b_m coefficients capture both the causal impact of moving at age m (β_m) and selection effects (δ_m). The selection component δ_m captures the possibility that parents who choose to relocate to higher-opportunity areas may have latent ability or higher wealth—that independently foster improved child outcomes. To identify causal effects, we follow Chetty and Hendren (2018) and make an additional assumption that *selection effects are invariant with respect to the child's age at move* ($\delta_m = \delta \; \forall \; m$).[16] This

[15] For non-income outcomes, we analogously compute $\bar{y}_{qos}$ and $\bar{y}_{qds}$ as the average outcome of permanent residents in o and d who are born in the same cohort s and whose fathers are in the same income quartile q.

[16] Unfortunately, we cannot test the validity of this assumption directly, as this would require data on child outcomes before the move takes place. Other studies implementing the *mover-exposure design* have pre-move childhood data and can thus directly test the key assumption (e.g. Beekers (2024)). However, as in Chetty and Hendren (2018), this is not feasible

assumption, while strong is less restrictive than it might initially seem. In particular, it does *not* require that where people move (is the place better or not) or when people move (is it a good time to move or not) is orthogonal to a child's potential outcomes. Rather, we assume away mechanisms that relate the two of these; for instance, systematic patterns whereby parents with better (worse) unobservables move earlier to places with better (worse) expected outcomes (Deutscher, 2020). This more involved selection mechanism is unlikely for long-distance relocations, which should be less motivated by idiosyncratic household preferences, neighborhood quality considerations, or similar family-level motives, and instead are more likely to be induced by work opportunities. Consequently, we focus on one-time movers with moves larger than 50km (Deutscher, 2020).

Assuming that selection effects are constant, the causal effects of place can be identified in two steps. First, the selection effect δ can be identified from b_m, where m is greater than the age at which the outcome is measured, since any move occurring after the outcome is measured cannot have a causal impact, and thus the causal effect is zero. Second, the causal effect β_m of moving at age m can be identified by subtracting the selection effect δ from b_m. The causal effect of one additional year of exposure at age m is then identified as $\gamma_m = b_m - b_{m+1}$.

Results. We start by presenting graphical evidence for the validity of the assumption of constant selection effects. We do so by measuring income ranks of children not at age 29-33, but at age 24. This allows us to separate b_m into causal exposure effects for $m < 24$ and selection effects $m > 24$.[17] Figure 3 displays the estimated exposure effects (b_m) for each age at move m using the model specification described in equation 3. Two key patterns emerge.

First, we find evidence of positive but age-invariant selection effects. All coefficient estimates obtained for ages $m > 24$ can only represent a selection effect, since there cannot be a causal effect of place in the case where the move occurs after the child's income is measured. Following Chetty and Hendren (2018), we regress $\vec{b}_m$ on $m > 24$. We interpret the resulting insignificant slope of 0.0063 as evidence of the validity of Assumption A1 ($\delta_m = \delta \ \forall m$). We determine selection effects by the average level of $\vec{b}_m$ for $m > 24$, finding that the average selection effect is equal to 0.25. This is slightly higher than the selection effect of 0.226 for the United States (Chetty and Hendren, 2018) and the 0.228 for the Netherlands (Beekers, 2024), but lower than the 0.308 for Australia (Deutscher, 2020).

Second, we find significant causal effects of place. Regressing $\vec{b}_m$ on $m < 24$ produces a slope of -0.0204, suggesting that the outcomes of children converge to the outcomes of permanent residents of the destination region at a rate of 2.04% per year of exposure to the destination region. Our results are very similar to previous studies that focus on exposure effects in the Netherlands (Beekers, 2024), but much lower than the effect documented for the US and Australia (Chetty and Hendren, 2018; Deutscher, 2020), where estimates are as high as 4.4% per year of exposure. In total, a child whose parents move at

when working with administrative employment data. Thus, in the spirit of Chetty and Hendren (2018), we provide graphical suggestive evidence for the validity of the main identification assumption in the form of constant selection effects (see Figure 3).

[17] It is important to note that measuring the incomes of the child generation at an earlier stage in life compared to the previous section does not introduce life-cycle bias in the estimation of exposure effects in equation 3, as our estimates of b_m are identified by correlating the incomes of children who move with the incomes of permanent residents in the destination at the same age in adulthood. This allows us to measure the convergence of movers' outcomes to permanent residents' outcomes at any age, irrespective of whether these outcomes at that age reflect the permanent outcomes (Chetty and Hendren, 2018).

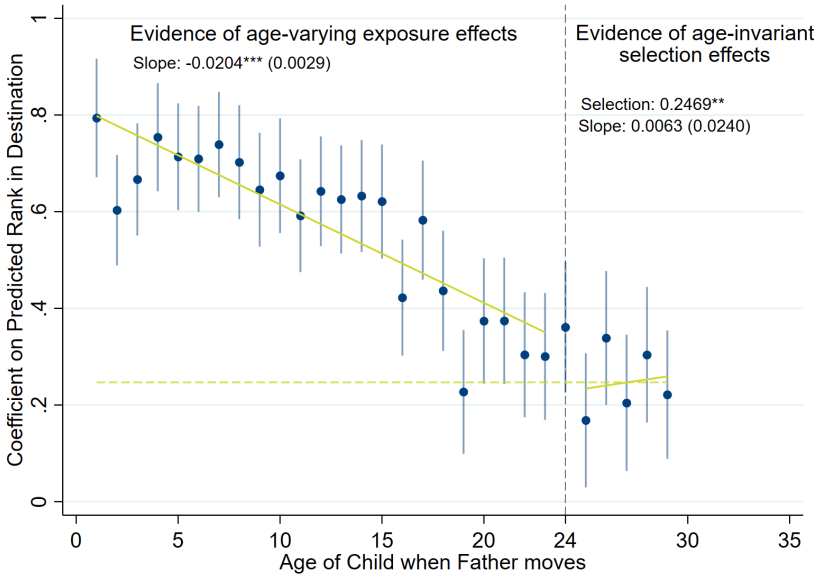


FIGURE 3. Childhood Exposure Effects on Income Ranks in Adulthood

Notes: The figure plots the coefficients b_m from equation 3, along with 95 percent confidence intervals. The b_m capture the expected gain in an individual's income rank at age 24 associated with moving at age m to a place with 1 percentile rank higher expected outcomes for permanent residents. The dashed vertical lines divide the data into two groups: age at move $m < 24$ and $m > 24$. Best-fit lines are estimated using unweighted OLS regressions of the b_m coefficients on m separately for $m < 24$ and $m > 24$. For $m < 24$, the slope magnitudes provide estimates of the annual effects of childhood exposure effects. The parameter δ is defined as the mean value of the b_m estimates for $m > 24$. Cohort and origin effects are captured by indicators for cohort and their interactions with predicted outcomes for permanent residents of the origin. Disruption effects are modeled using indicators for age at move and their interactions with parental rank, while indicators for cohort interacted with Δ_{odps} account for potential mismeasurement of origin. This reproduces Panel B of Figure IV in Chetty and Hendren (2018).

birth would be expected to pick up about 50 percent of the difference in predicted outcomes between their origin and destination, purely due to the causal effect of the region.

In Table 2, we estimate exposure effects directly using the micro data, measuring outcomes at ages 29–33 as in previous section. We do so by parameterizing the exposure effect shown in Figure 3 linearly, replacing the nonparametric $\sum_{m=1}^{29} b_m I(m_i = m)\Delta_{odps}$ term in equation 3 with $I(m_i \leq 23)(b_0 + (23 - m_i)\gamma)\Delta_{odps}$. Estimating this directly in the micro-data and restricting the data to moves that occur before the child turns 19, we estimate that for each additional year of exposure, movers' income ranks converge toward those of permanent residents at a rate of 2.14%, as shown in column 1 of Table 2. Focusing on firm and individual AKM ranks in adulthood, we find that each year of exposure to a region where permanent residents work for 1 percentile AKM rank higher employers at age 30 (have 1 percentile rank higher individual AKM ranks), increases the firm (individual) AKM ranks of movers by 2.73 (1.84) percentile ranks, as shown in columns 2 and 3 of Table 2. Moving from a labor market at the 25th to the 75th percentile 10 years earlier than counterfactually would imply a 16% increase in annual labor income in adulthood for a child at the 50th percentile of the income distribution. Around 40% of which (6 p.p.) is attributable to the workplace premium. Although convergence for individual AKM components (skills) is considerably slower at a rate of 1.84% per years - as opposed to 2.7% per year of exposure in firm AKM component-, the relative higher importance of the individual AKM component for total earnings compared to the firm AKM component, implies that the same

Table 2. Childhood Exposure Estimates

	Moves before age 18			Moves before age 18 and father in p33		
	(1) Income Rank	(2) Firm AKM Rank	(3) Person AKM Rank	(4) Income Rank	(5) Firm AKM Rank	(6) Person AKM Rank
Exposure effect (γ)	0.0214***	0.0273***	0.0184***	0.0327***	0.0296***	0.0357***
	(0.0034)	(0.0026)	(0.0040)	(0.0059)	(0.0042)	(0.0073)
N	117,760	117,760	117,760	33,254	33,254	33,254

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table presents estimates of annual childhood exposure effects on children's outcomes at ages 29–33 (γ). These coefficients can be interpreted as the effect of spending one additional year of childhood in an LLM where the children of permanent residents have outcome ranks that are 1 percentile point higher at ages 29–33 than in the origin region. Each column shows coefficients from a regression of a child's rank at ages 29–33 on the difference between permanent residents' predicted ranks in the destination and origin LLMs, interacted with the child's age at the time of the move (m). Column (1) reports exposure effects for income ranks. Columns (2) and (3) report exposure effects for the firm and individual AKM components (ranks) estimated when children are between 29-33 years old. Columns (4)-(6) repeat columns (1)-(3), but for the subset of children moving before the age of 18, with parents in the bottom tercile of the parental income distribution.

counterfactual move would increase a child income by approximately 8 p.p, which is 50% of the total effect.

For low-income children of parents from the bottom tercile of the income distribution, exposure effects are much more pronounced. For low-income children, we estimate that for each additional year of exposure, their income ranks converge toward those of permanent residents at a rate of 3.27%, as shown in column 4 of Table 2. Analogously, each year of exposure increases the child's future employer AKM rank by 2.96%, and their personal AKM component rank by 3.6%. Counterfactually, for the typical child of permanent residents in the bottom tercile of the income distribution, this implies that a 10 year earlier move from a labor market at the 25th to the 75th percentile, increases income by 24%, of which 26% (6.3 p.p)is attributable to the firm pay premium, whereas 74% (17.6 p.p) is due to higher skill accumulation.

4 Workplaces and Segregation by Parental Income

We now turn to our main objective of constructing workplace-level mobility statistics for Germany. We begin by describing our main mobility measure. We then examine how children's earnings outcomes and rates of intergenerational upward mobility differ across establishments.

4.1 Workplace-level mobility measures

We focus on two channels through which establishments could contribute to intergenerational mobility: (i) hiring patterns and segregation, and (ii) conditional on hiring, pay policies, opportunities for promotion, etc., determine career progression. Thus, any workplace-level mobility measure should reflect these two channels. We do so by focusing on two statistics: the fraction of children from low-income fathers (in the bottom tercile of the income distribution) and the fraction of such children who reach the top tercile of the income distribution in adulthood at each establishment (similar to

College Scorecards, see Chetty et al. (2020)). The product of these two statistics is the establishment's upward mobility rate:

$$\underbrace{P(y_i > 66 \;\&\; p_i < 33)}_{\text{Mobility Rate}} = \underbrace{P(p_i < 33)}_{\text{Access Rate}} \times \underbrace{P(y_i > 66 \mid p_i < 33)}_{\text{Success Rate}}. \tag{4}$$

To be more precise, as children can have multiple employers over their labor market career, we calculate employment weighted mobility scorecards over two different employment periods: age 16-24 and age 29-33, separately. We always measure outcomes at age 29-33. Formally, we define $S^{\bar{p},\bar{y}}_{j,\underline{a},\overline{a}}$ as the mobility rate of establishment j (= the fraction of its children who come from the bottom tercile of the father income distribution and end up in the top tercile of the child earnings distribution), computed as:

$$S^{\bar{p},\bar{y}}_{j,\underline{a},\overline{a}} = \underbrace{\frac{\sum_{i=1}^{N} w_{ij} * I(p_i < \bar{p})}{\sum_{i=1}^{N} w_{ij}}}_{\text{Access rate = Fraction low income}} \times \underbrace{\frac{\sum_{i=1}^{N} w_{ij} * I(p_i < \bar{p}, y_i > \bar{y})}{\sum_{i=1}^{N} w_{ij} * I(p_i < \bar{p})}}_{\text{Success = P(child in } \bar{y} \text{ | father in } \bar{p})}, \tag{5}$$

where p_i is the father income rank, and y_i is the child's income rank at age 29-33 (both p, y ranked within child birth cohorts) and N total number of children working at least once for j at age $a \in [\underline{a}, \overline{a}]$ and $I(.)$ is an indicator function equal to one if (.) is true. w_{ij} is the share of $i's$ employment in $[\underline{a}, \overline{a}]$ spent working at establishment j. We compute $w_{ij} = \frac{1}{\bar{A}} * \sum_{a=\underline{a}}^{\overline{a}} I(i_a(j) = j)$, where $I(.)$ is an indicator function equal to one if i worked at firm j at age a. $\bar{A}$ is the total number of years observed working between age $\underline{a}$ and $\overline{a}$. The weight captures how much of i's success at age 29-33 can be attributed to establishment j's impact.

Importantly, mobility rates reflect a combination of selection effects (the types of children employed) and causal effects (the value-added of establishments). In this section, we simply document how mobility rates vary across establishments without distinguishing between these two factors; we aim to identify the causal effect of working in a high mobility workplace in the next section.

To obtain reliable workplace mobility statistics, we compute $S^{p<33,y>66}_{j,\underline{a},\overline{a}}$ only for establishments that employed at least 5 children at ages $a \in [\underline{a}, \overline{a}]$. We focus on all children in the core sample (birth cohorts 1985–1990). In addition, we include children from earlier cohorts born between 1980 and 1984. In total, this allows us to construct mobility measures for almost 200,000 establishments, which together employ around 60% of the entire German workforce in 2015.

4.2 Descriptive Results

Figure 4 plots the fraction of low-income children who reach the top tercile of the income distribution (P(child in p66 | parents < p33)) versus the fraction of low income children (P(p<33)) at the establishment level. We measure access at the age of 16-24, and outcomes at age 29-33. We find that workplaces with higher fractions of low income children tend to have fewer employees who reach the top earnings tercile on average. Or in other words, the most segregated workplaces, where only few children from the bottom tercile of the income distribution are employed, are most efficient in providing upward mobility prospects, conditional on access. To investigate this pattern in more detail, we plot both components (access and success) separately against the firm AKM pay premium (estimated in 2004-2011). Panel A

of Figure 5 shows that higher paying workplaces on average hire fewer low-income children, but those that gain access are very likely to reach the top of the income distribution (Panel B of Figure 5).

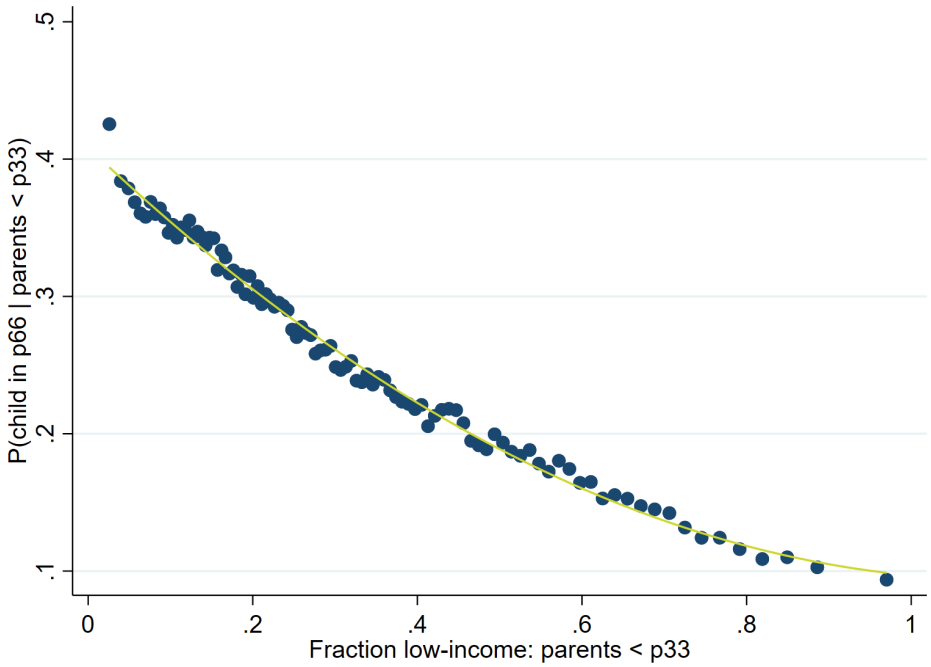


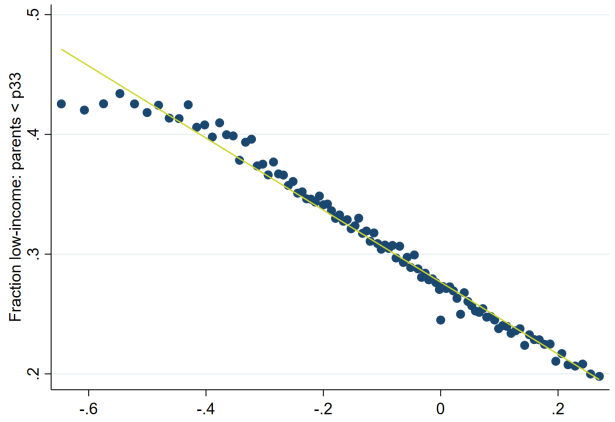
Figure 4. Children's Income Rank in Adulthood versus Fraction of Low-Income Children, by Workplace

Notes: The figure presents a binned scatter plot at the establishment level, plotting the share of children who reach the top tercile of the earnings distribution at ages 29–33, conditional on having parents in the bottom income tercile versus the share of children with parents in the bottom income tercile (the "fraction low-income"). Children are assigned to employers relative to the proportion they spend working for each employer during ages 16–24. All income ranks are constructed within child birth cohorts. Intergenerational mobility statistics are computed for establishments that employ at least 5 unique children from the 1980–1990 birth cohorts during ages 16–24. The best-fit slope is estimated on the underlying micro data.

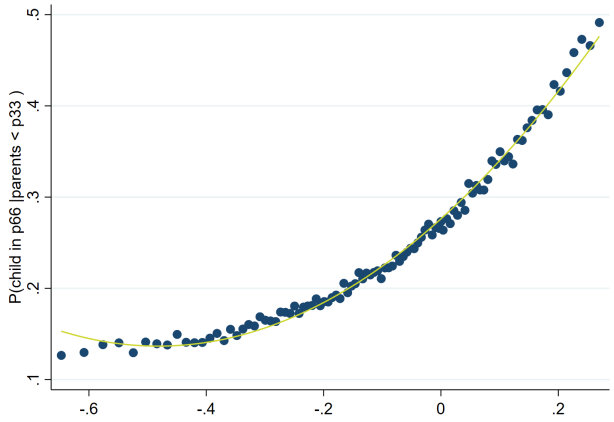
Table 3 reports correlations of workplace characteristics with the fraction of children from the bottom tercile of the father income distribution (column 1), the probability to reach the top income tercile for low income children (column 2), and with the mobility rates (column 3). All dependent and independent variables are standardized to have mean 0 and standard deviation 1. We weight all bivariate regressions by the number of children employed in each firm.

We document that larger firms, firms with higher shares of highly qualified workers, firms in the manufacturing sector, and firms with higher average wages or wage premia employ fewer children originating from the bottom tercile of the parental income distribution. At the same time, these firm characteristics are positively associated with the probability that children from low-income backgrounds reach the top tercile of the income distribution (column 2), which results in a positive correlation of these workplace characteristics and the overall mobility rate (column 3). On the other hand, higher shares of low-qualified workers and higher shares of foreign employees are negatively correlated with the success rate (column 2), but uncorrelated with the share of low-income children among hires.

(A) Fraction with Fathers from Bottom Tercile vs. Firm Pay Premium



(B) Fraction who Reach Top Income Tercile vs. Firm Pay Premium



(C) Workplace Mobility Rates vs. Firm Pay Premium

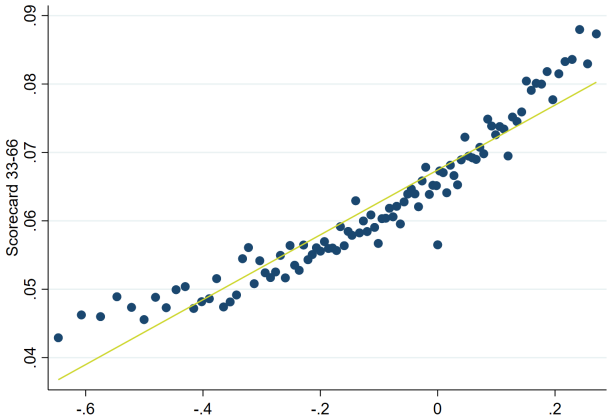


FIGURE 5. Workplace Mobility Components and Workplace Characteristics

Notes: The figure presents a binned scatter plot at the establishment level. Panel A plots the average share of children with fathers in the bottom income tercile versus the Firm AKM pay premium in 2004-2011. Panel B plots the share of children who reach the top tercile of the earnings distribution at ages 29–33, conditional on having parents in the bottom income tercile versus the firm pay premium. In Panel C we plot the mobility rate, which is the product of the two components from Panel A and B, versus the firm pay premium. Children are assigned to employers relative to the proportion they spend working for each employer during ages 16–24. All income ranks are constructed within child birth cohorts. Intergenerational mobility statistics are computed for establishments that employ at least 5 unique children from the 1980–1990 birth cohorts during ages 16–24. The best-fit slopes are estimated on the underlying micro data.

This yields an overall negative correlation with the mobility rate (column 3). Consistent with the patterns shown in Figures 4 and 5, we generally observe opposite signs for the correlations between workplace characteristics and, respectively, the share of low-income children and the top-outcome rates. Characteristics that are associated with higher shares of low-income children, at the same time correlate negatively with outcome rates, and vice versa. The only exception is the average age of the workforce: firms with an older workforce both recruit more young workers from low-income backgrounds and simultaneously offer higher prospects of upward mobility for these workers.

Table 3. Correlations of Workplace Characteristics with Mobility Statistics

	(1) Fraction low-income	(2) Top outcome rate	(3) Mobility rate
Firm size	-0.011***	0.023***	0.0034**
	(0.0025)	(0.0048)	(0.0013)
Share apprentices	0.036***	-0.031***	-0.021***
	(0.0064)	(0.0047)	(0.0029)
Share low-qualified	-0.0017	-0.10***	-0.061***
	(0.0075)	(0.0060)	(0.0031)
Share medium-qualified	0.023**	-0.050***	-0.0043
	(0.0081)	(0.0068)	(0.0040)
Share high-qualified	-0.035**	0.17***	0.066***
	(0.011)	(0.010)	(0.0049)
Share foreigners	0.011*	-0.052***	-0.018***
	(0.0048)	(0.0037)	(0.0024)
Avg. age	0.065***	0.024***	0.019***
	(0.012)	(0.0072)	(0.0038)
Manufacturing	-0.11***	0.23***	0.082***
	(0.0038)	(0.0060)	(0.0027)
Avg. daily wage	-0.29***	0.44***	0.14***
	(0.0074)	(0.0067)	(0.0038)
Firm AKM effect	-0.37***	0.37***	0.13***
	(0.0050)	(0.0048)	(0.0024)

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The table presents bivariate workplace-level regression coefficients, regressing the fraction of low-income children (fathers in the bottom tercile of the income distribution), the top-outcome rate ($P(y_i > 66 \mid p_i < 33)$), and the mobility rate (=low-income × outcome rate) on various workplace characteristics, respectively. All independent and dependent variables are normalized to have mean 0 and standard deviation 1. We weight all regressions by the number of children from cohorts 1980-1990 that worked at the establishment at age 16-24. All firm characteristics, except the firm pay premium, refer to year 2015. Firm pay premium is calculated over the period 2004-2011. Firm size is the number of total employees. All shares are calculated relative to the firm size.

5 Children's Outcomes Across High- and Low-Opportunity Workplaces

Our goal is to estimate the effect of working for a high mobility firm at labor market entry on children's income (ranks) in adulthood for children from low-income fathers. Formally, we estimate the following model:

$$y_i = \beta S_{j(i)} + f(d_{l,t}) + \delta_l + \delta_s + \epsilon_i, \tag{6}$$

where y_i denotes the income rank of child i at ages 29–33, measured relative to all other children from the same birth cohort within the core sample. $S_{j(i)}$ represents the mobility rate associated with child i's first stable employer. We define the first stable employer as the first employer for whom individual i is continuously employed for a minimum duration of two consecutive years between ages 16 and 24. The δs represent region of the first stable job and birth cohort fixed effects, while $f(d_{l,t})$ is a quartic function of regional job entries of young workers (ages 15–24), capturing business cycle variation.

OLS estimates of equation 6 are likely biased for two main reasons: (i) the employment history of individual i is mechanically linked to the mobility rate of the first stable employer, and (ii) unobserved

determinants of outcomes in adulthood that are plausibly correlated with the mobility rate of the first employer.

To address the first source of bias, we recompute the establishment scorecards as described in equation 5 using only the birth cohorts 1980–1984, excluding our core-sample cohorts born between 1985 and 1990.[18] We address the second issue by employing an instrumental variable approach, which we outline in the subsequent section.

5.1 Empirical approach

Our instrumental variables strategy closely follows the approach introduced in Arellano-Bover (2024), exploiting variation over time and across regions in the composition of regional labor demand for young workers to predict the mobility rate of the first employer.[19] The objective is to construct an index that summarizes the composition of labor demand faced by a young worker at labor market entry—specifically, the relative demand from high-mobility versus low-mobility firms—while holding constant the overall level of labor demand.

Although the timing of idiosyncratic demand shocks affecting high-mobility firms can plausibly be regarded as exogenous from the perspective of a child entering the labor market, the decision regarding when and where to enter is not. Consequently, we do not link the variation in the demand composition to workers on the basis of their realized year and region of entry; instead, we rely on the predicted region and year of entry. We predict the region of the first stable employer using information on the father's location when the child is 15 years old. Predicting the year of labor market entry is slightly more involved. To this end, we use information on the highest educational degree obtained before age 24 and infer the expected year of labor market entry based on standard German program-specific completion times.

For each individual i, or instrument $\bar{S}^{-i}_{l_0,t_0}$ is the predicted mobility rate of the first stable employer based on the labor demand composition in the predict region and predicted year of entry. We operationalize this by calculating a weighted average scorecard in the predicted region l_0 and year of entry t_0 using new hires of young workers (age 15-24) as weights in the administrative data:

$$\bar{S}^{-i}_{l_0,t_0} = \frac{\sum_{k \neq j(i)} (w_{k,t_0} S_k) + (w_{j(i),t_0} - I(t = t_0, l = l_0)) S_{j(i)}}{\sum_{k \neq j(i)} w_{k,t_0} + w_{j(i),t_0} (1 - I(t = t_0, l = l_0))}, \tag{7}$$

where w_{x,t_0} denotes the number of young workers who enter the establishment in the same calendar year as individual i's predicted year of labor market entry. The term $I(t = t_0, l = l_0)$ is an indicator function that equals one if the individual's first stable job was found in the predicted region l_0 and in the predicted year of labor market entry t_0, and equals zero otherwise. Equation 7 illustrates that we implement a leave-one-out approach. Specifically, if child i finds his first stable job in the predicted region and in the predicted year of entry, we exclude individual i from the measure of the inflow of young workers into firm $j(i)$, that is, from the construction of the corresponding weights.

[18] We focus on employment at ages $a \in [16, 24]$ and define low-income children as those in the bottom tercile of the father income distribution. Success is defined as reaching the top tercile of the child income distribution in adulthood. For notational convenience, we omit the $\bar{a}$, $\underline{a}$, $\bar{p}$, and $\bar{y}$ subscripts in this section.

[19] Arellano-Bover (2024) exploits variation in labor demand composition to predict the size of the first employer.

5.2 Results

Table 4 reports the OLS, first-stage, and IV (2SLS) estimates of β from equation 6. Standard errors are clustered at the level of the predicted local labor market at entry. The instrument does a good job of predicting the mobility rate of the first employer, with a first-stage F-statistic of 90 (column 3). Columns 1 and 2 present the OLS estimates in ranks and levels/logs respectively, and columns 4 and 5 the corresponding 2SLS estimates. In panel A the dependent variable is the child's average income at age 29-33. The OLS estimate for cohort-specific income ranks is 0.509, and 0.009 for log income, whereas the IV estimates are significantly larger and equal to 3.14 and 0.059, respectively. The IV estimates implies that a 1 p.p. increase in the mobility rate of the first employer increases the income rank of children whose fathers are in the bottom tercile of the income distribution by 3.14 percentile ranks, or log incomes by 0.059 log points. An alternative way to assess the magnitude of the IV coefficients in columns 4 and 5 is to scale these by the standard deviation of the first employer's mobility rate, which is 1.98. Accordingly, obtaining a stable job at labor market entry with an employer whose mobility rate is one standard deviation higher increases the income percentile rank at ages 29–33 of children with fathers in the bottom income tercile by approximately 6.2 percentile ranks or analogously by 12.44 percent.

Panel B of Table 4 presents estimates for the AKM person component at age 29-33, both in ranks (cohort specific and ranked within the full core sample) and in levels. The 2SLS estimates in column 4 (5) imply that a 1 p.p increase in the mobility rate of the first employer increases the individual AKM component of children whose fathers are in the bottom tercile of the income distribution by 4.3 percentile ranks (0.048 log points). Estimates for the AKM firm component are displayed in Panel C of Table 4. The 2SLS estimates in column 4 (5) imply an increase in the firm component of 2.1 percentile ranks (0.014 log points). The 2SLS estimates in column 5 across panels A-C imply, that almost 25% of the total effect of starting at a higher mobility firm at age 16-24 on income at age 29-33 operates through the firm channel. Or in other words, 25% of the income premium from starting at a higher mobility firm is due to these children working at higher-paying firms in adulthood. However, the majority of the effect on income materializes through an substantive increase in the individual AKM component, which indicates dynamic and improved learning/skill accumulation in higher mobility firms.

Table 4. Earnings in Adulthood and First-Employer Mobility Rates for Low Income Children: OLS and IV-TSLS Estimates

	A. Dep. variable: Income at age 29-33				
	OLS		First-Stage	IV-TSLS	
	(1)	(2)	(3)	(4)	(5)
	Income Rank	Log Income	First-Employer Mobility Rate	Income Rank	Log Income
First-employer mobility rate	0.5093***	0.0094***		3.1465***	0.0592***
	(0.0202)	(0.0004)		(0.1459)	(0.0029)
Labor demand instrument			1.5538***		
			(0.1636)		
N	145,660	145,660	145,660	145,660	145,660
First Stage F			90.25		
	B. Dep. variable: Individual AKM effect at age 29-33				
	OLS		First-Stage	IV-TSLS	
	(1)	(2)	(3)	(4)	(5)
	Pers. AKM Rank	Pers. AKM	First-Employer Mobility Rate	Pers. AKM Rank	Pers. AKM
First-employer mobility rate	0.4916***	0.0054***		4.2773***	0.0487***
	(0.0230)	(0.0003)		(0.2067)	(0.0024)
Labor demand instrument			1.5538***		
			(0.1636)		
N	145,660	145,660	145,660	145,660	145,660
First Stage F			90.25		
	C. Dep. variable: Firm AKM effect at age 29-33				
	OLS		First-Stage	IV-TSLS	
	(1)	(2)	(3)	(4)	(5)
	Firm AKM Rank	Firm AKM	First-Employer Mobility Rate	Firm. AKM Rank	Firm. AKM
First-employer mobility rate	0.4603***	0.0029***		2.1668***	0.0139***
	(0.0176)	(0.0001)		(0.1423)	(0.0011)
Labor demand instrument			1.5538***		
			(0.1636)		
N	145,660	145,660	145,660	145,660	145,660
First Stage F			90.25		

Notes: The table reports OLS and IV-TSLS estimates of the effect of starting in a higher-mobility firm on various outcomes at ages 29–33 for children whose fathers are located in the bottom tercile of the income distribution in our core sample (birth cohorts 1985–1990). Columns 1 and 2 show OLS estimates. Columns 4 and 5 show 2SLS estimates. In columns 1 and 4 we rank the resperctive outcomes relative to all other children from the same birth cohort in the core sample. In columns 2 and 5, outcomes are measured in logs. Panel **A** presents estimates for income at age 29-33, estimates for the individual (firm) AKM component are shown in Panel **B** (**C**). AKM measures are estimated in 3 observation windows, when children are between 29-33 years old: 2014-2019 (cohorts 1985/86); 2016-2021 (cohorts 1987/88), and 2018-2022 (cohorts 1989/90). Firm-level mobility rates are calculated using cohorts 1980–1984. We instrument the mobility rate of the first stable employer with the expected mobility rate at the predicted region and predicted year of labor market entry, constructed using an index that summarizes variation in the local labor demand composition for young workers. The region of entry is predicted from the father's location when the child is 15 years old. The year of entry is predicted based on standard German completion times for schooling and, where applicable, higher education. All regressions include birth cohort fixed effects and expected region-of-entry fixed effects. Additionally, we include a quartic polynomial in the level of regional demand for young workers to capture business cycle fluctuations. Standard errors are clustered at the predicted region of entry. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

6 Quantifying Changes in Intergenerational Mobility from Reducing Parental Income Segregation Across Firms

In the previous sections we have documented and analyzed how mobility prospects in terms of access to better paying firms for low-income children and outcomes across establishments (probability to reach the top income tercile) vary across workplaces in Germany. Additionally, we have showed in Section 5 that access to higher-mobility firms early in the labor market career is highly beneficial for low-income children. While the previous sections mainly focused on access to higher mobility firms at the start of the career, we aim to quantify in this section by how much mobility measures in Germany would change if establishments were to hire individuals in an parental-income-neutral way. We simulate how changing the allocation of individuals across workplaces after the initial hiring phase, that is at age 29-33 -when we measure outcomes-, would affect mobility measures.

We motivate this shift of attention to a later phase of the labor market career by documenting substantial income segregation across workplaces in Germany at the age of 30 in Figure 6. In the figure, we plot for each father income rank, the mean firm AKM rank (Panel A) and the average firm pay rank (Panel B) of the employers at which the corresponding children are employed at age 30. We find a strong positive relationship between the father's income rank and the average firm pay premium. A 10 percentile rank increase in the fathers' income ranks' is associated with on average a 1.84 percentile rank increase in AKM rank of the employer at age 30. This association flattens at the tails of the father income distribution and the estimate slope drops by around 35% once we include individual AKM percentile fixed effects (proxies for human capital/skill) and control for 5-digit occupations. The slope decreases by almost 60% considering the average firm pay rank, once we control for individual AKM percentiles and 5-digit occupations. This indicates, that around 35%-60% of the segregation can be explained by sorting due to (unobserved) skills and occupation. The remaining segregation -what we term residual segregation- might reflect discrimination, potentially correlated with ethnicity (Kaas and Manger, 2012; Kline et al., 2022), the influence of parental networks (Kramarz and Skans, 2014; Staiger, 2025) or sorting on preferences or amenities (Mas, 2025) . The mechanisms behind this, however, are beyond the scope of this paper.

We now aim to simulate how changes in the allocation of individuals across workplaces change mobility measures. We do so, by randomly filling each slot at each establishment with a random child with the same 2 digit occupation and the same skill-decile. Thus, we aim to shut down residual sorting, while keeping the number of jobs at each establishment fixed. We calculate counterfactual earnings of each individual as the sum of the individual AKM fixed effect (constant in all simulated allocations) and the AKM fixed effect of the counterfactually assigned employer. We construct cohort specific income ranks and repeat the procedure 1000 times.

Importantly, this simulation exercise abstracts from changes in individual skills and changes in the firm pay premia due to different resulting allocations. This means that the baseline simulation specification does not take dynamic skill accumulation that might arise due to longer exposure at higher mobility firms into account.[20] Analogously, we assume that firm pay premia remain unchanged if we

[20] A more sophisticated simulation algorithm would take the dynamic skill accumulation, resulting from earlier access to higher-mobility firms (Panel B of Table 4), into account. The baseline simulation carried out in this section can be regarded as

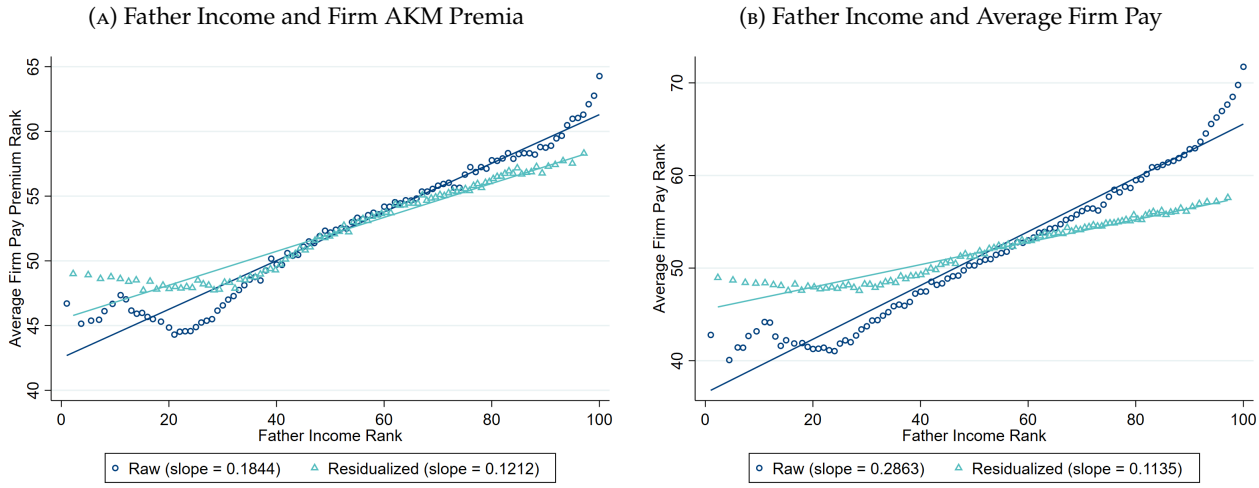


FIGURE 6. Father Income Segregation across Workplaces

Notes: The figure shows a binned scatter plot of the relation between father income rank and the AKM firm pay premia percentile rank of employers (Panel A), or the average firm pay rank (Panel B), where children in our core sample work at age 30. We report the raw relationship ("raw") and the residualized version, controlling for individual AKM fixed effect percentile and 5-digit occupations ("residualized"). The slopes and best-fit lines are estimated on the underlying individual-level micro data.

reassign each position within the firm to a child from our core sample with the same 2-digit occupation and skill decile as the actual employee.

Table 5 shows how the intergenerational transition matrix for children in our core sample (cohorts 1985-1990) would change under this counterfactual. Panel A shows the actual transition matrix. For example, the chance of reaching the top earnings quintile ranges from 11.29% for children with fathers in the bottom quintile to 37.97% for children with fathers in the top quintile, as shown in the fifth column of Panel A. This is a difference of 26 percentage points. In the counterfactual economy, where employers hire father-income-neutral (but from the same 2-digit occupation and skill decile cells), the chance of reaching the top quintile now ranges from 12.31% to 35.56% across father income quintiles (Table 5, Panel B), a gap that is 12% smaller than the empirically observed gap. This reduction is sizable, and similar to a reduction of around 14% in the rich-poor top-quintile outcome gap that would result from an income-neutral, but test score preserving college allocation in the US (Chetty et al., 2020).

keeping the initial hiring practices of firms intact and simply reshuffling workers at age 30 across establishments. Results on intergenerational mobility could therefore be interpreted as a lower bound. We plan to implement a more sophisticated version of the simulation - taking skill accumulation into account - in the upcoming weeks.

Table 5. Actual versus Counterfactual Intergenerational Transition Matrices

	A. Actual Outcomes				
	Fraction of Children with Earnings in each Group				
	Bottom 20%	Quintile 2	Quintile 3	Quintile 4	Top 20%
	1	2	3	4	5
.. for children with parents from					
Bottom 20%	31.38	23.73	18.73	14.86	11.29
Quintile 2	25.72	24.07	20.89	17.16	12.16
Quintile 3	18.82	21.69	22.55	20.96	15.98
Quintile 4	14.64	17.98	21.14	23.65	22.60
Top 20%	9.44	12.53	16.68	23.37	37.97

	B. Income Neutral Allocation				
	Fraction of Children with Earnings in each Group				
	Bottom 20%	Quintile 2	Quintile 3	Quintile 4	Top 20%
	1	2	3	4	5
.. for children with parents from					
Bottom 20%	28.64	23.65	19.46	15.94	12.31
Quintile 2	24.30	23.25	20.96	18.01	13.49
Quintile 3	20.00	21.33	21.58	20.46	16.63
Quintile 4	16.22	18.54	20.79	22.44	22.01
Top 20%	10.84	13.23	17.20	23.16	35.56

Notes: Panel A shows the actual intergenerational income transition matrix for children in our analysis sample (1985–90 birth cohorts). Each cell of Panel A reports the percentage of children with earnings outcomes in the quintile given by the column conditional on having fathers with income in the quintile given by the row for the analysis sample. Panel B repeats Panel A under the income-neutral (but skill and occupation preserving) worker allocation at age 30. We compute earnings in the counterfactual economy as the sum of the individual AKM fixed effect and the firm pay premia of the newly assigned workplace. Statistics in this table are constructed directly from the individual-level micro data.

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A Additional Figures and Tables

Table A1. Entropy Balance Table

	Raw				weighted			
	In Sample	Out Sample	Diff	STMD	In Sample	Out Sample	Diff	STMD
Demographics								
Male	0.542	0.499	-0.043	0.086	0.502	0.502	-0.000	0.000
Year of Birth	1987.520	1987.514	-0.006	0.004	1987.515	1987.515	-0.000	0.000
Age	30.906	30.916	0.011	0.024	30.917	30.917	-0.000	0.000
Missing School Degree Information	0.169	0.183	0.014	0.037	0.159	0.159	0.000	0.000
less than Mittlere Reife	0.172	0.138	-0.034	0.094	0.140	0.140	-0.000	0.000
Mittlere Reife	0.322	0.247	-0.075	0.166	0.255	0.255	-0.000	0.000
Abitur/Fachhochschulreife	0.338	0.432	0.095	0.196	0.446	0.446	0.000	0.000
No Vocational Training	0.066	0.080	0.013	0.052	0.083	0.083	0.000	0.000
Vocational Training	0.673	0.530	-0.143	0.295	0.548	0.548	-0.000	0.000
University Degree	0.211	0.309	0.097	0.224	0.319	0.319	0.000	0.000
Labor Market Outcomes age 29-33								
Log yearly labor earnings	10.117	10.028	-0.089	0.093	10.028	10.028	-0.000	0.000
Average days in empl. / year	324.527	313.948	-10.580	0.122	324.638	324.638	-0.000	0.000
Share part-time work	0.276	0.327	0.051	0.125	0.327	0.327	0.000	0.000
Primary Sector	0.007	0.008	0.001	0.015	0.008	0.008	0.000	0.000
Secondary Sector	0.264	0.189	-0.075	0.180	0.195	0.195	-0.000	0.000
Tertiary Sector	0.687	0.742	0.056	0.123	0.767	0.767	0.000	0.000
Place of residence age 29-33								
Schleswig-Holstein	0.029	0.033	0.005	0.026	0.033	0.033	0.000	0.000
Hamburg	0.024	0.034	0.010	0.059	0.034	0.034	0.000	0.000
Lower Saxony	0.095	0.087	-0.008	0.027	0.087	0.087	-0.000	0.000
Bremen	0.008	0.009	0.002	0.018	0.009	0.009	0.000	0.000
North Rhine-Westphalia	0.218	0.205	-0.013	0.032	0.202	0.202	-0.000	0.000
Hesse	0.069	0.070	0.001	0.003	0.070	0.070	0.000	0.000
Rhineland-Palatinate	0.046	0.043	-0.003	0.014	0.044	0.044	-0.000	0.000
Baden-Württemberg	0.130	0.119	-0.011	0.035	0.121	0.121	-0.000	0.000
Bavaria	0.156	0.154	-0.003	0.007	0.157	0.157	0.000	0.000
Saarland	0.012	0.011	-0.001	0.009	0.011	0.011	-0.000	0.000
Berlin	0.041	0.068	0.027	0.118	0.067	0.067	0.000	0.000
Brandenburg	0.030	0.032	0.002	0.009	0.032	0.032	0.000	0.000
Mecklenburg-Vorpommern	0.022	0.022	0.001	0.005	0.022	0.022	0.000	0.000
Saxony	0.059	0.058	-0.001	0.006	0.058	0.058	-0.000	0.000
Saxony-Anhalt	0.031	0.028	-0.003	0.017	0.027	0.027	-0.000	0.000
Thuringia	0.030	0.026	-0.004	0.024	0.026	0.026	-0.000	0.000

Notes: The Table presents summary statistics for children born in 1985-1990 forming our analysis sample (In Sample), and individuals fulfilling the same sampling criteria as for our analysis sample, but are not part of the sample due to missing parental information (Out Sample). Differences and Standardized Mean Differences are reported for the raw sample (columns 3 and 4), and the reweighted sample (columns 7 and 8). Weights are constructed using entropy balancing.

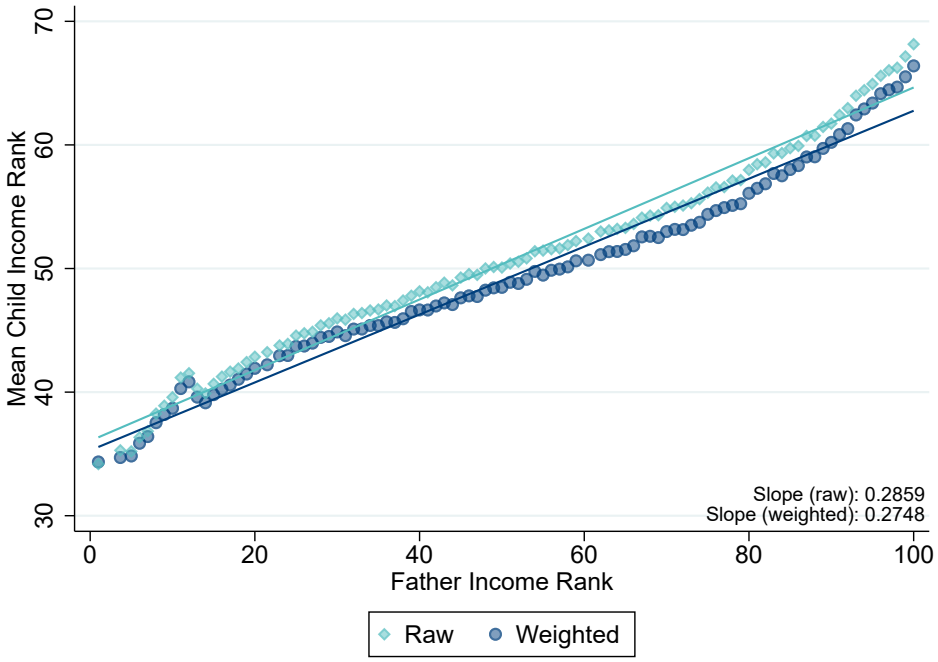


Figure A1. Association between Children's and Fathers' Percentile Ranks, Reweighted

Notes: The Figure shows a nonparametric binned scatter plot illustrating the association between children's and fathers'percentile income ranks in the core sample (birth cohorts 1985–1990). Children's income is measured as the average labor earnings between ages 29 and 33, while fathers' income is defined as the average labor earnings during the years when the child is 15 to 19 years old. Children are ranked relative to all other children in their own birth cohort, and fathers are ranked relative to all other fathers in the core sample who have children in that same cohort. The slope and best-fit line are estimated using an OLS regression on the micro data. Diamond show raw associations, ad circles the reweighted correlation (based on entropy balancing displayed in Table A1).

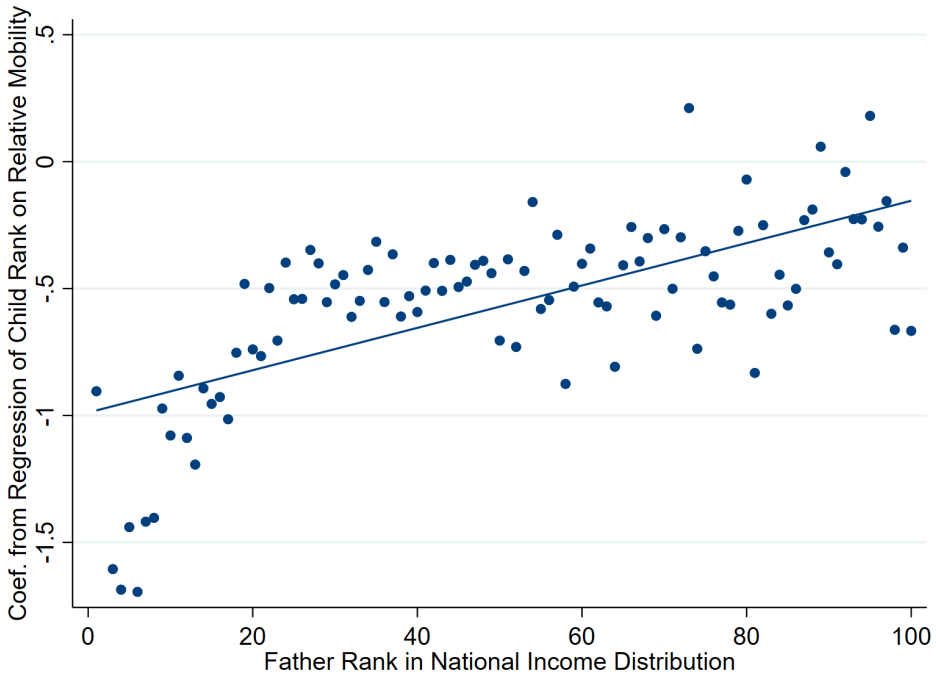


FIGURE A2. Association between Absolute and Relative Mobility

Notes: The figure shows the correlation between relative mobility and absolute mobility at various percentiles of the income distribution. We calculate the mean income rank of children in LLM l with parents in (national) percentile p. We then run a LLM-level regression of the mean income rank on relative mobility at each percentile p separately. The figure plots the resulting regression coefficients versus the percentile p. The coefficient can be interpreted as the mean impact of a 1 unit increase in relative mobility on the absolute outcomes of children whose parents are at percentile p. We also plot the best linear fit across the 100 coefficients.

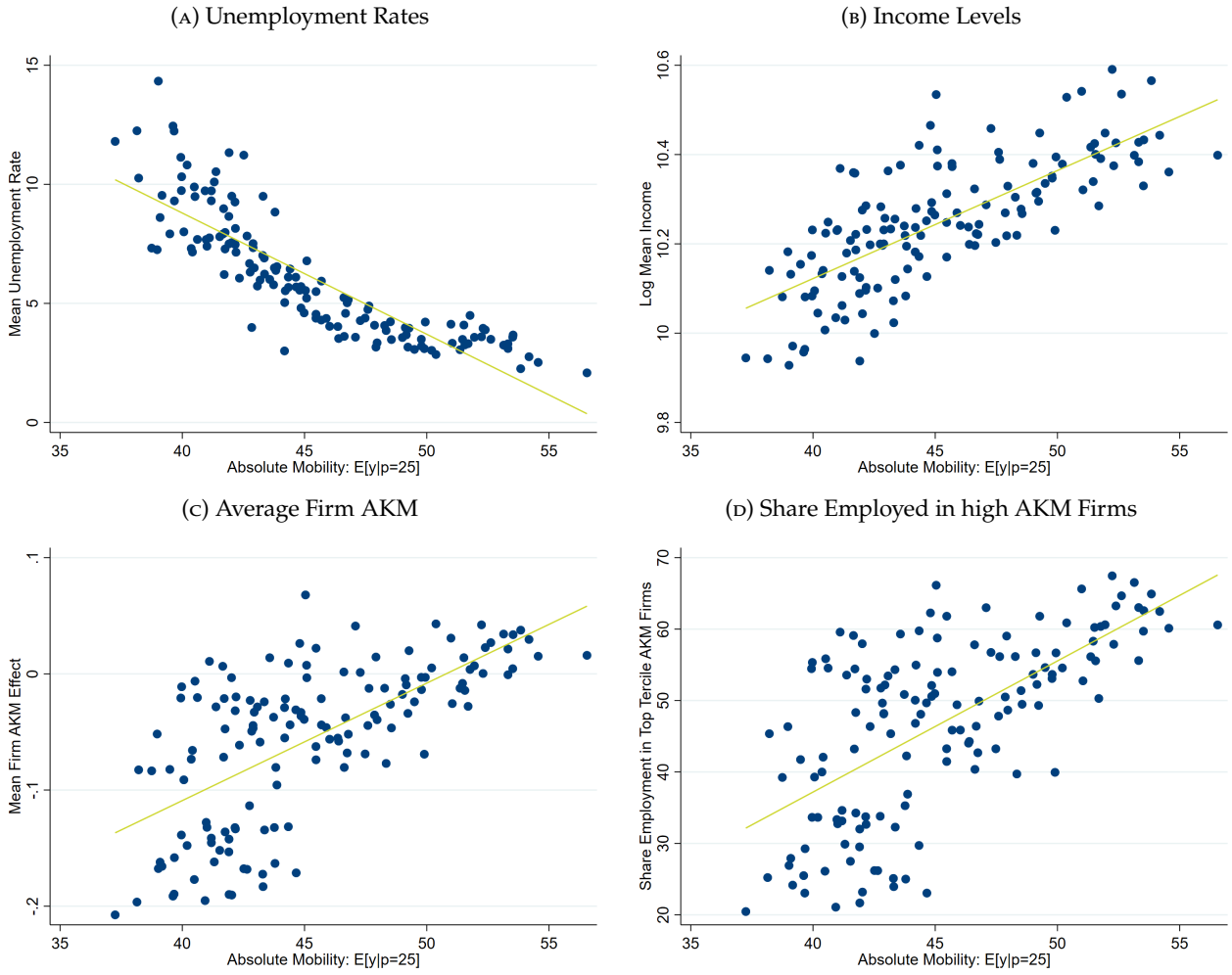


FIGURE A3. Absolute Upward Mobility and Local Labor Market Statistics

Notes: The figure plots the correlation between absolute upward mobility (the expected income rank of children with parents in the bottom half of the income distribution) vs. various local labor market characteristics, at the level of 141 LLMs.

Table A2. Childhood Exposure Estimates, Outcomes measured at Age 24

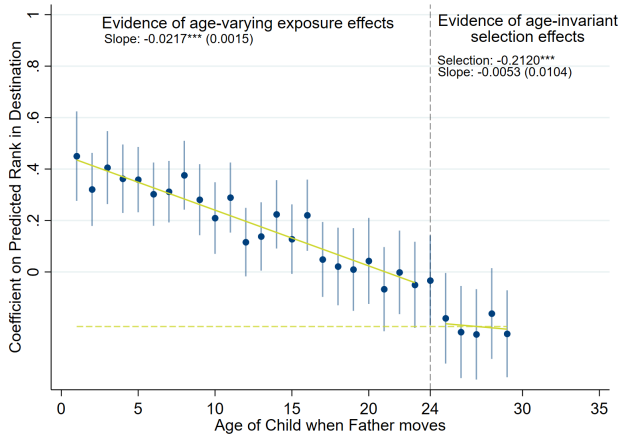
	Child's income rank at age 24		
	pooled	Age ≤23	Age ≤18
Exposure effect (γ)	0.0204***	0.0205***	0.0136***
	(0.0018)	(0.0018)	(0.0025)
N	182,523	151,764	125,543
	AKM rank of child's employer at age 24, own AKM Measures		
Exposure effect (γ)	0.0218***	0.0219***	0.0208***
	(0.0022)	(0.0022)	(0.0030)
N	135,051	112,770	93,756
	Firm pay rank of child's employer at age 24		
Exposure effect (γ)	0.0183***	0.0184***	0.0172***
	(0.0023)	(0.0023)	(0.0032)
N	150,319	125,302	104,087
	AKM rank of child at age 24, own AKM Measures		
Exposure effect (γ)	0.0227***	0.0228***	0.0230***
	(0.0020)	(0.0020)	(0.0027)
N	143,268	119,195	98,889
	Child's occupation rank at age 24		
Exposure effect (γ)	0.0130**	0.0129**	0.0107
	(0.0041)	(0.0041)	(0.0058)
N	155,678	129,697	107,714

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The Table presents exposure estimates directly estimated on the microdata for children moving before the indicated age for various outcomes. Columns 1 uses all moves before the age of 29, column 2 restricts the sample of movers to movers before the age of 23, and column 3 to movers before the age of 18. These coefficients can be interpreted as the effect of spending one additional year of childhood in an LLM where the children of permanent residents have outcome ranks that are 1 percentile point higher at ages 24 than in the origin region.

(A) Firm AKM Rank



(B) Individual AKM Rank

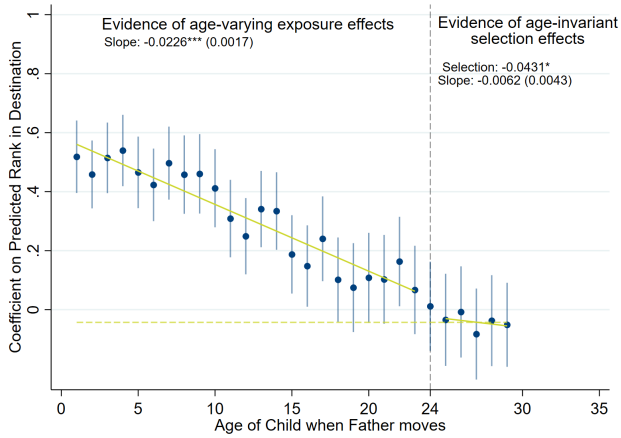


FIGURE A4. Childhood Exposure Effects on AKM Component Ranks in Adulthood

Notes: See Notes of Figure 3. We change outcomes, to reflect the AKM rank of the employer the individuals works for at age 24, and the individual AKM component based on cohort-specific AKM estimation windows when children are between 16 and 24 years old.

TABLE A3. Childhood Exposure Estimates, Outcomes measured at Age 29-33

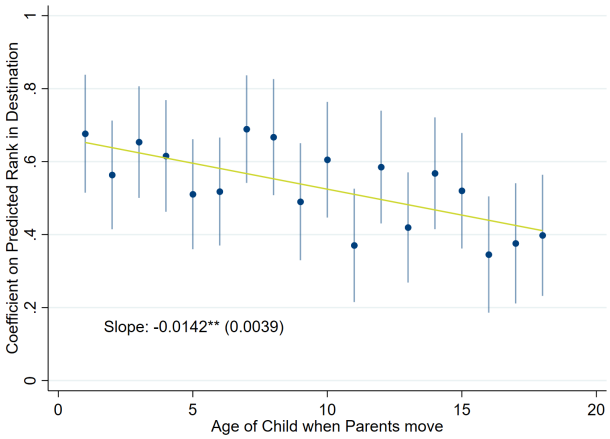
	Child's income rank at age 29-33		
	pooled	Age ≤23	Age ≤18
Exposure effect (γ)	0.0152***	0.0152***	0.0141***
	(0.0024)	(0.0024)	(0.0033)
N	227,199	189,006	156,297
	AKM rank of child's employer at age 29-33, own AKM Measures		
Exposure effect (γ)	0.0243***	0.0244***	0.0258***
	(0.0017)	(0.0017)	(0.0023)
N	214,218	178,286	147,640
	Firm pay rank of child's employer at age 29-33		
Exposure effect (γ)	0.0200***	0.0201***	0.0235***
	(0.0018)	(0.0019)	(0.0025)
N	213,998	178,135	147,529
	AKM rank of child at age 29-33, own AKM Measures		
Exposure effect (γ)	0.0144***	0.0143***	0.0179***
	(0.0029)	(0.0029)	(0.0040)
N	171,796	143,000	118,529
	Child's occupation rank at age 29-33		
Exposure effect (γ)	0.0137***	0.0138***	0.0114*
	(0.0033)	(0.0033)	(0.0046)
N	219,793	182,924	151,461

Standard errors in parentheses

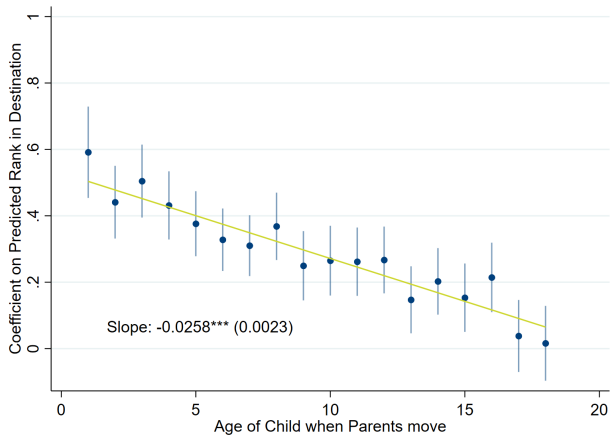
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: See notes of Table A2. However, outcomes are measured as averages across the age 29-33 instead of at age 24.

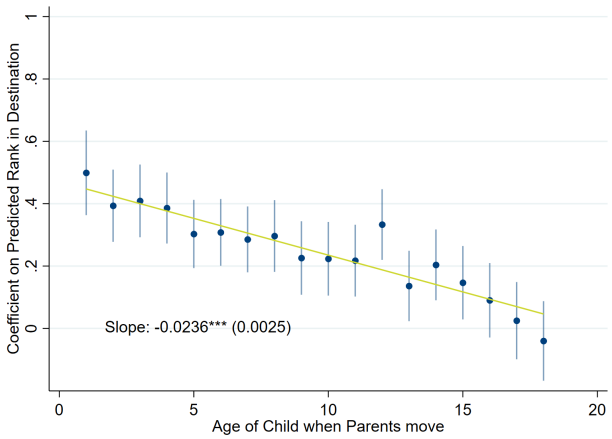
(A) Income Rank



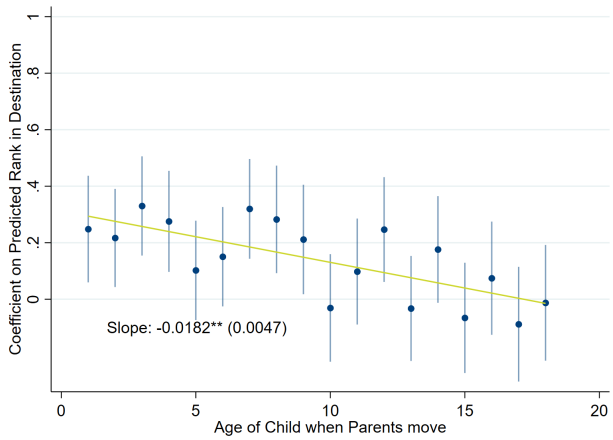
(B) Firm AKM Rank



(C) Firm Pay Rank



(D) Individual AKM Rank



(E) Occupation Pay Rank

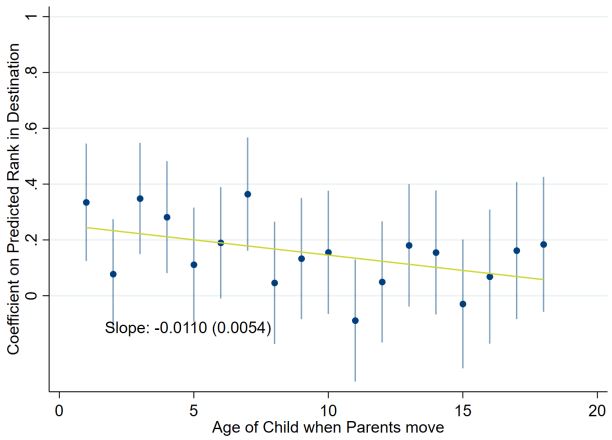


FIGURE A5. Childhood Exposure Estimates at Age 29-33, moving before Age 18

Notes: The Figure shows coefficients of equation 3 for movers before the age of 18 and outcomes measured at age 29-33.