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ABSTRACT

This dissertation investigates several drivers of intergenerational mobility. The first chapter examines how returns to colleges and majors, admission policies, and student sorting affect intergenerational mobility. I study this question in Denmark, where tuition-free colleges and generous living grants provide an ideal setting to investigate the sources of inequality that remain in the absence of financial constraints. Leveraging GPA admission cutoffs from a centralized matching algorithm, I apply a regression discontinuity design to examine whether students from low socioeconomic status (SES) benefit from selective degrees (college-major pairs). Scoring just above the cutoff for admission to a more selective degree raises low-SES students' income by 9% at age 30, partially closing the SES wage gap. I develop a joint model of degree selection and labor market outcomes, to disentangle the contribution of degree-specific returns from that of students' degree choices as sources of inequality. The model accounts for the impact of degree peer composition on outcomes. Estimates from the model reveal that returns to degree enrollment are higher for low-SES students than for high-SES peers across the distribution of degree selectivity. Furthermore, I find that the current GPA-based meritocratic admissions system is inefficient in terms of maximizing long-term earnings. Implementing need-affirmative admissions could reduce the SES wage gap by 15% through upward mobility of disadvantaged students, without adversely impacting displaced high-SES students. Notably, the SES wage gap could be eliminated if students selected degrees based on expected earnings returns.

The second chapter studies households' willingness to pay for neighborhood and school attributes. In Denmark, education at all levels is tuition-free. Teacher salaries and expenditures on schools are mandated to be equal. Yet there is substantial variation in teacher quality and student performance across neighborhoods around schools for which parents pay in terms of housing prices. Homophily—sorting by parental education and income into neighborhoods with different peer, teacher and housing attributes explains this phenomenon

and not the sorting by preferences for school quality and tax expenditure emphasized in many models of local public goods. Parents buy packages of attributes and not just school quality, which is the emphasis in much of the literature. We develop and apply multiple approaches to identifying parental valuation of measured school quality in the presence of strong neighborhood sorting.

Finally, the last chapter examines whether growing up in a better neighborhood has positive consequences on long-term labor market outcomes and educational attainment. We exploit a unique spatial dispersal policy that randomly resettled refugees across neighborhoods in Denmark from 1986-1998. We find that a higher quality neighborhood—as measured by the wage outcomes of children already living there—is significantly associated with increased market income in adulthood. Our mediation analysis reveals that the effect of neighborhood quality on child adult income is fully mediated through the impact of assigned neighborhood on parental income.

CHAPTER 1

UNIVERSAL HIGHER EDUCATION AND UPWARD MOBILITY: DEGREE RETURNS, ADMISSION POLICIES AND STUDENT SORTING

This paper examines how returns to colleges and majors, admission policies, and student sorting affect intergenerational mobility. I study this question in Denmark, where tuition-free colleges and generous living grants provide an ideal setting to investigate the sources of inequality that remain in the absence of financial constraints. Leveraging GPA admission cutoffs from a centralized matching algorithm, I apply a regression discontinuity design to examine whether students from low socioeconomic status (SES) benefit from selective degrees (college-major pairs). Scoring just above the cutoff for admission to a more selective degree raises low-SES students' income by 9% at age 30, partially closing the SES wage gap. I develop a joint model of degree selection and labor market outcomes, to disentangle the contribution of degree-specific returns from that of students' degree choices as sources of inequality. The model accounts for the impact of degree peer composition on outcomes. Estimates from the model reveal that returns to degree enrollment are higher for low-SES students than for high-SES peers across the distribution of degree selectivity. Furthermore, I find that the current GPA-based meritocratic admissions system is inefficient in terms of maximizing long-term earnings. Implementing need-affirmative admissions could reduce the SES wage gap by 15% through upward mobility of disadvantaged students, without adversely impacting displaced high-SES students. Notably, the SES wage gap could be eliminated if students selected degrees based on expected earnings returns.[1]

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1.1 Introduction and Motivation

Across OECD countries, college enrollment rates and major choices differ significantly between children from high- and low-income families [OECD, 2018, Bailey and Dynarski, 2011, Patnaik et al., 2021]. Recognizing these trends, policies have focused on broadening college access and improving the match quality between students and majors, including through tuition-free colleges and stipends for living expenses.[2] These policies can improve long-term outcomes for students who might otherwise be unable to borrow against future earnings and have been implemented in several countries—including Denmark, the setting of this paper.[3]

In an environment without financial barriers, higher education may still uphold existing inequalities rather than foster mobility. First, the returns to colleges and majors (degrees, henceforth) may vary by SES background, depending on the nature of the education production function [Hanushek, 1971, Durlauf, 2008]. If complementarities exist between degree selectivity and academic preparation, less-prepared students may experience lower returns in selective degrees, though the evidence remains mixed.[4] Second, students from low-income backgrounds may systematically sort into degrees that produce low labor market returns.[5] Finally, admission policies that rely on high school GPA or entrance exams can disproportionately disadvantage low-SES students, as these metrics may provide downward-biased measures of their underlying cognitive and non-cognitive skills [Heckman et al., 2021, Sethi and Somanathan, 2022].

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2. Henceforth, I define a tuition-free system with government-funded living stipends as universal higher education.

3. For a recent review of the impact of financial aid on outcomes, see Dynarski et al. [2022].

4. See, e.g., Arcidiacono and Lovenheim [2016], Arcidiacono et al. [2016], Dillon and Smith [2020], Chetty et al. [2023].

5. Several paper study sorting patterns in the U.S., e.g., Hoxby and Avery [2012], Chetty et al. [2020], Sloane et al. [2021].

This paper examines how causal returns to colleges and majors, admission policies, and student sorting affect intergenerational mobility. Despite egalitarian policies from an early age, I show that substantial inequalities persist—not only in students' pre-college achievements but also in their degree choices. I find that admission policies and student sorting, rather than limited returns to selective degrees, are the primary drivers of the SES wage gap. These findings have implications beyond Denmark, as many countries already offer tuition-free higher education systems and means-tested financial aid.[6] More broadly, these results shed light on some of the fundamental sources of inequality within higher education markets, beyond financial constraints.

This analysis leverages several features of the Danish system. First, the higher education system's centralized admission process, based on GPA cutoffs and student-ranked degree preferences, provides an ideal setting to address challenges in estimating returns to degree selectivity. These challenges arise from multiple selection margins—such as application, admission, and matriculation—that typically complicate the comparison of students across degrees of varying selectivity.[7] I tackle these issues in two ways: first, by leveraging the GPA cutoffs that dictate admissions, and second, by using students' elicited preferences for degrees to recover selection-corrected estimates of degree-specific returns.

A second key feature is that Denmark offers all students a tuition-free higher education system with generous stipends and subsidized loans that can fully cover living expenses. This setting provides an ideal backdrop to understand the sources of the inequalities that remain. Immediate financial constraints cannot account for the SES gap in degree choice. Despite this generous financial support, there is growing evidence that such egalitarian policies enshrined

6. Austria, Belgium, Estonia, Finland, France, Germany, Norway, Sweden, Turkey are among OECD countries with tuition-free public institutions [OECD, 2022].

7. Selection is also present at each school-grade transitions (see e.g., Heckman et al. [2018]).

in the law are often undone in practice.[8,9]

Finally, the availability of rich administrative data allows me to paint a comprehensive picture of the long-term distributional consequences of higher education degrees. To that end, I use data from the higher education system, which provides students' submitted rank-ordered lists of degrees as well as admission decisions. I combine this data with several administrative registers, to study a number of long-term outcomes of interest, including labor market outcomes, occupation, family formation, fertility and crime, by SES background. Moreover, I link each student to their parents, which enables me to characterize individual's socioeconomic backgrounds and study heterogeneity along this margin. To this end, my primary measure of students' socioeconomic status is based on parental income at the time of college application.

I begin by examining whether enrollment in more selective degrees benefits disadvantaged students, providing evidence on whether the current system can serve as a lever to reduce inequalities. I do so by applying a regression discontinuity design (RDD) in the context of the Danish centralized college admission system to recover the marginal returns to admission and enrollment into more selective degrees across SES backgrounds. Students who apply to higher education submit rank-ordered lists of their preference for degrees. A deferred acceptance algorithm matches students to capacity-constrained degrees, where priority is determined by students' GPAs. In equilibrium, GPA-based admission cutoffs determine admission to more selective degrees. I consider students whose GPA is marginally above and below that cutoff, stacking together all cutoffs. Examining the returns to selectivity for students at

8. For instance, Eshaghnia et al. [2023b] examines how egalitarian policies at the level of primary schooling—tuition-free, equalized per-pupil expenditures, and standardized teacher salaries—is undermined in practice by the sorting of both students and teachers across neighborhoods, leading to unequal educational opportunities.

9. Heckman and Landersø [2022] provide evidence of the SES gradient in college attendance in Denmark, showing strong similarities with the U.S. For instance, for the 1987 birth cohort whose parents are at the bottom quartile of the wage income distribution, college attendance stands at around 30% in both countries. At the top quartile of the income distribution, this number rises to around 50% and 60%, in Denmark and the US, respectively.

this margin is highly policy-relevant, as it directly informs whether marginally increasing the representation of low-SES students in more selective degrees enhances their long-term outcomes.

My findings reveal substantial heterogeneity in the labor market impact of cutoff-crossing by SES background. Students from low-SES backgrounds reap a benefit of around 9% in pre-tax annual income, 12 years post-application. In contrast, there is no significant effect for students from high-SES backgrounds. Notably, crossing the GPA cutoff closes the gap in entering the top 1% of income earners. Both students from low- and high-SES backgrounds who marginally cross the GPA cutoff experience sharp changes in degree peer composition. Degree selectivity, as measured by averaging incoming students' high school GPA, increases by about $.3\sigma$ for low-SES students and by $.4\sigma$ for high-SES students. On the other hand, college completion, years of education or learning, as captured by college grades, remain unaffected by cutoff-crossing. University and broad field of study also remain unchanged at the cutoff. Extending the analysis beyond economic outcomes, I show that degree selectivity increases fertility for both low- and high-SES students, while having only limited effects on household structure and criminal behavior.

I also provide suggestive evidence that these results are in part driven by students from low-SES backgrounds disproportionately benefiting from exposure to better peers. I employ two strategies to estimate the effect of attending a degree with better quality peers on long-term outcomes. First, I build on an approach developed by Kirkeboen et al. [2016] in the context of instrumental variables with unordered treatments, which exploits variation in the quality of peers in the fallback degree. Second, I rely on within-degree, cross-cohort variation in the composition of peers, which allows me to hold constant student sorting into degrees. Both strategies provide consistent evidence on the differential role of social interactions by socioeconomic background.

These findings show substantial returns to degree selectivity for marginal low-SES students, indicating that admission policies may serve as levers to enhance social mobility. However, the impact of such policies must account for equilibrium effects that arise from student being displaced across the system. Admission policies also alter the peer composition within each degree, which can independently influence outcomes.

Results from the RDD further reveal a persistent wage gap, even for low-SES students above the GPA cutoffs. Notably, these estimates bundle student degree choices with degree-specific returns, as the returns experienced by low-SES students may potentially reflect the poor fallback options they select. Unpacking these effects requires adding structure to separate degree-specific causal returns from student choice patterns. Estimating causal returns for specific degrees poses significant challenges, as students may self-select into degrees based on unobserved attributes.

To address these questions, I develop a model of the Danish higher education system that allows me to recover degree-specific returns, disentangling these from the effect of student degree sorting. This framework further enables the evaluation of admission policies by accounting for general equilibrium effects and the influence of degree peer composition on outcomes. To that end, I leverage features of the centralized admission system in Denmark. First, I summarize student preferences by fitting discrete choice models to students' rank-ordered lists of degrees. Due to the strategy-proof nature of the deferred acceptance algorithm used to allocate students to degrees, truthfully ranking degrees is a weakly dominant strategy [Dubins and Freedman, 1981, Abdulkadiroğlu and Sönmez, 2003]. This feature enables me to recover students' true preferences for degrees. In a second step, I specify a model of earnings outcomes, which innovates upon most of the previous literature by allowing for peer effects, which accounts for the impact of peer composition at the degree-cohort level on outcomes [Abdulkadiroğlu et al., 2020, Dur et al., 2020, Barahona et al., 2024]. To identify the parameters of the model requires dealing with students self-selecting into degrees. To that end, I jointly model treatment selection and labor market outcomes using rich information on preferences elicited from students' rank-ordered choice lists. The model is identified from variation in students' ranking conditional on enrolling in the same degree.

Findings from the model reveal that the labor market returns to degree enrollment is higher for low-SES compared to high-SES students across the distribution of degree selectivity. Importantly, the model recovers estimates of the causal effect of degrees on long-run outcomes, accounting for selection on unobservables. The RDD deals with selection by holding constant the distribution of ability and preferences between students above and below the cutoff. The local average treatment effects estimated pertains to instrument compliers situated at the cutoff.[10] In contrast, estimates from the model go beyond this margin and pertain also to students away from the discontinuity.[11]

The model provides a framework to evaluate whether alternative admission policies could enhance outcomes for low-SES students by expanding their access to more selective degrees. I consider budget-neutral policies that hold constant degree capacities. Importantly, when simulating alternative admission policies, I compute counterfactual outcomes while accounting for peer composition changes and the general equilibrium effects inherent in such policy changes. These effects are critical, as policy interventions that shift one student into a different degree generate spillover effects, displacing other students in the process and thereby altering the overall composition of students across degrees, as shown by Gandil [2023].

Simulations from the model show that departures from *ex-ante* meritocratic admissions—based on students' GPA—improves allocative efficiency, by increasing the earnings of low-SES students without harming high-SES students. Enhancing the representation of low-SES students in more selective degree reduces the SES wage gap by approximately 15%. These findings provide evidence that GPA may not be an accurate measure of the latent skills

10. General equilibrium effects are also unaccounted for.

11. Importantly, low-SES students near the discontinuities are high-achievers relative to the average low-SES student. Therefore, the returns to degree selectivity observed at these discontinuities may not generalize beyond this group.

of low-SES students, since a departure from *ex-ante* meritocratic admissions increases their earnings substantially. Finally, my analysis reveals that if disadvantaged students prioritized degrees based on expected returns, the wage gap could be entirely eliminated. This highlights the role of student degree choices and admission policies, rather than degree returns, as fundamental sources in explaining the inequalities that remain.

Related Literature and Contributions. This paper contributes to four strands of the literature. First, I provide novel evidence that students from low-income households disproportionately benefit from enrollment to selective colleges and majors, relative to high-income peers. These findings build on estimates from an RDD and a model of the higher education system, contributing to our understanding of the heterogeneous nature of the returns to colleges and major selectivity. Moreover, this paper is the first, to my knowledge, to extend the analysis well beyond labor market outcomes to examine effects on asset accumulation, occupational choice, family structure, fertility, and criminal behavior, by parental income.[12] I find limited effects across these dimensions, with the exception of fertility, which increases across SES groups. These results contribute to the literature on long-term returns to college selectivity, which has provided mixed evidence.[13] On the one hand, a number of papers find large effects employing regression discontinuity designs [Anelli, 2020, Jia and Li, 2021], in the context of elite institutions. Others find more limited effects [Dale and Krueger, 2002, 2014, Heinesen, 2018, Mountjoy and Hickman, 2021].[14] In addition to choosing a college, students make the consequential decision of selecting a major. The evidence points to large heterogeneity in returns across majors (Kirkeboen et al. [2016], Hastings et al.

12. Black et al. [2005], Kaufmann et al. [2013], Humlum et al. [2017], Kirkebøen et al. [2021], Ge et al. [2022] also study the impact of college quality of family formation and spousal quality, without investigating the effects by parental income.

13. A long and established literature has studied the labor market returns to college for those at the margin of attendance (see e.g., Carneiro et al. [2011], Oreopoulos and Salvanes [2011], Zimmerman [2014], Heckman et al. [2018]).

14. Focusing on elite institutions, the literature further suggests that returns are concentrated at the tails [Chetty et al., 2023] or for high-SES students [Zimmerman, 2019].

[2013]; Altonji et al. [2016] provides a review).[15] In this paper, I show that these returns to majors differ across SES backgrounds, a relationship previously understudied [Arcidiacono and Lovenheim, 2016].

Second, I provide novel evidence on the role of higher education admission policies in equalizing opportunities and fostering social mobility. To that end, I provide an empirical framework to assess the allocative efficiency of admission policies in a universal higher education systems in terms of long-term outcomes, while accounting for peer composition and supply-side capacity constraints. I show that departing from current *ex-ante* meritocratic admission policies—based solely on past performance as captured by high school GPA—could improve social mobility. Low-SES students are under-matched and reap larger benefits from being admitted to more selective degrees, even after accounting for changes in peer composition on outcomes. Displaced high-SES students are unharmed by need-affirmative admissions in terms of income. These findings underscore the limitations of high school GPA as a predictor of long-term outcomes, especially for low-SES students.[16] This contribution builds on a rich body of literature examining the range of skills that influence later-life outcomes, demonstrating that achievement relies on a broader set of skills beyond those captured by grades [Heckman et al., 2006a, Borghans et al., 2008, 2016, Heckman et al., 2021].[17] Findings from this paper further contribute to assessing the so-called "mismatch" hypothesis, suggesting that students admitted to academically rigorous institutions through affirmative action may struggle to succeed because their academic preparation is lower than

15. In Norway, Kirkeboen et al. [2016] find that relative to a Teaching degree, taking up an Engineering degree results in a gain of about $75,000 in annual income. In the U.S., the premium to majors in Science, Technology, Engineering, and Math (STEM) has been increasing [Gemici and Wiswall, 2014].

16. Almost half of OECD countries use GPA or national exams as criteria for degree admission [OECD, 2019].

17. This is also consistent with the theoretical model by Sethi and Somanathan [2019], who argue that past accomplishments may provide only a noisy signal of the skills relevant to future performance. Group identity can then offer additional information into potential, suggesting that greater representation of low-SES students may enhance long-term outcomes. In a similar vein, Riehl [2024] shows empirically that for admission to more selective degrees to benefit disadvantaged students, entrance exams must accurately reflect their underlying ability.

that of their peers, leading to poor outcomes [Sander et al., 2012, Arcidiacono et al., 2016]. A recent and growing literature has re-assessed this hypothesis, finding contrasting evidence, in the context of race-based affirmative action policy [Bleemer, 2022, Barahona et al., 2024]. Relative to previous papers, my analysis takes into account changes in the composition of peers on realized long-term outcomes, under counterfactual admission policies.[18]

Third, I contribute to the literature on student decision-making regarding college and major choice, by relating students' observed sorting behavior to their long-term causal effects. The context of this paper provides the ideal laboratory to study these patterns given the absence of financial constraints, removing one potential candidate for differential sorting by socioeconomic background. The current research base indicates that disadvantaged students tend to enroll in colleges that do not fully align with their abilities [Griffith and Rothstein, 2009, Black et al., 2015, Hoxby and Avery, 2012, Dillon and Smith, 2017].[19] Few papers directly connect college and major choices to the causal labor market returns for the specific students involved.[20] This paper advances the literature by linking student choices to their expected returns, thereby assessing whether observed undermatching has a detrimental impact on long-term outcomes. I find that students from low-SES background are less likely to apply to more selective degrees. This is the case despite the potentially large returns to more selective degrees for these students. By selecting degrees based on expected returns, disadvantaged students could substantially enhance their long-term labor market outcomes and effectively close the SES income gap.

Finally, this paper provides novel evidence on the effect of peers on long-term outcomes in higher education markets. The role of peers in shaping the choices and outcomes of individuals has produced mixed evidence in education [Barrios-Fernandez, 2023]. For instance,

18. For instance, Machado et al. [2024] show how peer composition is particularly important both for students benefiting from admission policies, and those being displaced.

19. Dynarski et al. [2023] provides a review of non-financial constraints to higher education.

20. Arcidiacono et al. [2016], Campbell et al. [2022] also study mismatch on the basis of college majors, without studying the long-term causal effects on earnings.

Abdulkadiroğlu et al. [2014] find that students attending exam schools in New York and Boston with significantly better peers as compared to their fallback options, provides no returns in terms of their performance. Identifying the effect of peer quality on outcomes presents significant challenges, particularly given the number of educational inputs that may be correlated with better peers. Both strategies I develop in this paper provide suggestive evidence that the heterogeneous returns I uncover at the cutoff are driven by differential returns to peer exposure. [21] These findings have implications for the impact of admission policies on outcomes through changes in degree composition, which I incorporate into my counterfactual analyses.

This paper proceeds as follows. In Section 1.2, I present the context, before moving on to describe three stylized facts about the Danish universal higher education system, in Section 1.3. Section 3.3 describes the data, while Section 1.5 turns to describing the regression discontinuity framework employed to study the returns to degree selectivity. I then outline a model of student degree choice and earnings outcomes in Section 1.6, presenting parameter estimates in Section 1.7, before considering the impact of need-affirmative admissions and student degree choices in Section 1.8.

1.2 Danish Context

Denmark implements a number of educational policies aimed at providing equal opportunities to all. This translates into generous childcare, universal high-quality pre-K, as well as free primary and secondary schools where per-pupil expenditure are equalized across schools. Below, I describe in greater detail policies pertaining to the higher education system and the Danish centralized admission system.

21. These results align with recent findings from Brazil, which highlight the role of social networks in mediating the long-term effects of peers on students benefiting from affirmative action policies [Machado et al., 2024].

1.2.1 Tuition Free Colleges and Living Grants

Tuition at Danish higher education institutions is free for Danish and EU/EEA students. The Danish government supports students in covering living costs across a wide range of courses and studies through the State Educational Grant and Loan Scheme (SU). In 2020, over 474,000 students benefited from these forms of educational support, with an annual budget amounting to $4.2 billion, which is about 1.2% of the GNP.

As of 2024, Danish college students living away from their parents receive a monthly government stipend of DKK 6,820 (approximately $1,000). Additionally, students can apply for a supplementary loan of DKK 3,489 (approximately $500) per month. These amounts cover basic living expenses, even in more expensive cities such as Copenhagen [University of Copenhagen].[22] To apply for SU, students must be enrolled in an eligible educational program and submit their application online before the end of the month for which they want to receive SU.

When a student enrolls in a university or other higher education institution in Denmark, they are entitled to SU for up to 72 months. Since most degrees (excluding medicine) typically last five years, this allows students an additional year to complete their studies or to change their major without financial penalty.

Implemented during the 1960s and 1970s, these post-secondary policies have been associated with an increase in college attainment. For cohorts born in 1955, average years of education amounted to 11.5 years. Thirty years later, this number increased to 14 years, consequently closing the gap with the U.S., where educational attainment had stayed almost constant over this period [Karlson and Landerso, 2024]. Underlining the importance of these reforms, a large literature points to the high pecuniary and non-pecuniary returns to higher education (see for instance Carneiro et al. [2011], Oreopoulos and Salvanes [2011],

22. In addition to parents, Danish students with disabilities and those facing special circumstances such as sickness or childbirth also receive additional support from the government.

Zimmerman [2014], Heckman et al. [2018]).

Despite the implementation of these policies, substantial inequalities can be observed in Denmark [Landersø and Heckman, 2017]. Not only did educational mobility decrease in past decades [Karlson and Landerso, 2024], recent research also points to social immobility across generations in terms of life-time well-being measures [Eshaghnia et al., 2021]. Moreover, strong sorting is at play, with parents purposefully choosing neighborhoods, which through social interactions further amplifies the role of the family and generates inequalities in access to schools, as well as outcomes later in life [Eshaghnia et al., 2023a,b]. I provide descriptive evidence of significant inequalities in the context of higher education in Section 1.3.

1.2.2 Centralized College Admission System

To apply for a higher education degree in Denmark, candidates apply online though a national admission portal and submit a rank-ordered list of degrees of up to eight degrees (college-major pairs). Two application tracks can be chosen by students. Quota 1 admissions are based on a student's GPA, calculated as a weighted average of grades from nationwide-standardized oral and written exams in specific subjects taken during the final year of high school, as well as from continuous assessments throughout the year.[23,24] Students typically apply in July, after their high school grades are realized. Quota 2 considers additional criteria, such as work experience. Students typically apply in the Spring. Over the period studied, between 2001 and 2009, over 80% of applicants choose Quota 1.

The allocation of students to degrees is implemented through a centralized mechanism. A student-proposing deferred acceptance (DA) algorithm matches students to capacity-constrained degrees where degree capacities are set by the government. Under DA, students

23. Nationwide-standardized written exams are graded by two external examiners, while oral exams are graded by the teacher alongside one external examiner. These exams carry more weight than continuous assessments in calculating GPA, with weighting further adjusted based on the difficulty level of the subject.

24. Additionally, degree-specific grade requirements in specific high school exams may be imposed by degrees.

produce rank-ordered lists of degrees based on their preferences and submit applications to degrees. In each round, degrees tentatively accept applicants, ranked by their GPA, up to available capacity. Unassigned students then proceed to apply to their next-preferred option. This iterative process continues until either all students are assigned to a degree or their preference lists are exhausted.

Since DA is strategy-proof, truthfully ranking schools is a weakly dominant strategy for students [Dubins and Freedman, 1981].[25] Once the match is realized, students are admitted to one degree at a maximum.[26] Switching degrees requires in most cases that students reapply through the centralized admission mechanism. Heinesen [2018] provides an in-depth description of the centralized admission system in Denmark and Gandil [2023] of the matching mechanism.

1.3 Stylized Facts: Inequalities in a Universal Higher Education System

1.3.1 Differences in High School Grades at Time of Application

In Denmark, grades differences across SES backgrounds are present from a very young age, despite the free and equal provision of a host of educational programs from an early age: heavily subsidized childcare, free primary and high school education [Heckman and Landersø, 2022]. These differences may reflect differences in accumulated human capital. To showcase these inequalities in the context of the higher education system in Denmark, Figure 1.1a depicts the relationship between high school GPA and parental gross income pre-tax income

25. Strategy-proofness requires the rank-ordered list to be unrestricted in length. In practice, only about 3% of students list eight degrees. This motivates my modeling approach in Section 1.6, which recovers student preferences by fitting a rank-ordered choice model to these preference lists.

26. Students who are not allocated may apply for admission to some non-capacity constrained degrees or re-apply through the centralized mechanism the following year.

at time of application.[27] There is a striking SES gradient in terms of grades. Across the parental income distribution, there is a $.6\sigma$ difference in grades. Figures 1.8a and 1.8b of Appendix 1.10.1 further illustrates these inequalities, both in terms of skills acquired in Mathematics and in Danish during high school.[28] Due to the grade-based meritocratic admission system in Denmark, these disparities restrict the feasible degree choice set for students from disadvantaged backgrounds.

1.3.2 Degree Portfolio Composition and Under-Matching

Beyond differences in grades, I find a pronounced SES gradient in application patterns. The barriers to degree choice are multi-faceted (as shown by Hoxby and Avery [2012], for instance). In part, low-SES students may find it more costly to select an adequate degree portfolio ensuring adequate labor market returns from their most preferred to least preferred option.

First, I show there is a strong positive relationship between parental income and measures of degree quality. I focus here on two measures of degree quality: peer average grades and average future income. The former, henceforth degree selectivity, averages the high school GPA of all incoming students, excluding an individual's own grade. The latter is a measure of average degree income for all individuals who completed a given degree and are between 30 and 50 years old, from 2010 to 2020. Figure 1.1b shows that students from higher SES backgrounds tend to apply to degrees that are better in terms of peer quality, as measured by average high school grades. Differences in high school grades at time of application do not fully drive these findings. Figure 1.11c of Appendix 1.10.1 shows the relationship between degree-level peer GPA and parental gross income at time of application, by quartile of high school grades. Students from low-SES backgrounds are less likely to apply to more selective

27. This measure of income excludes transfers.

28. Similar gradients exist between high school grades and parental years of education, as shown in Figures 1.8c and 1.8d of Appendix 1.10.1.

degrees than high-SES students within the same quartile of high school GPA. Figure 1.12c of Appendix 1.10.1 shows similar patterns using our second measure of degree quality, which considers average earnings at degree level.

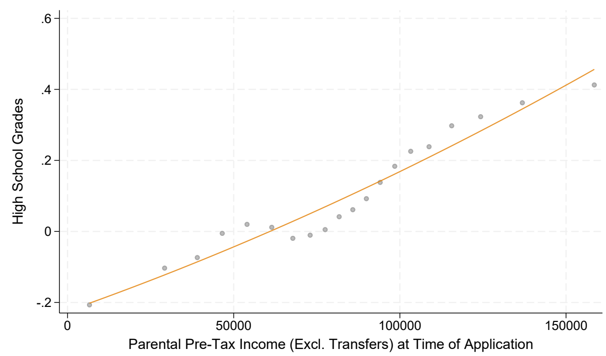
Second, low-SES students apply to different broad fields of study. Figure 1.10a of Appendix 1.10.1 outlines student's top-ranked degree and associated field of study, by SES background. high-SES students are almost twice as likely to rank Sciences or Business as a first-best, as compared to low-SES students, degrees with potentially higher returns. A similar pattern arises when focusing on the second-best alternative that students rank, as shown in Figure 1.10b of Appendix 1.10.1. In fact, students' fallback options are often within the same broad field of study, as shown in Figures 1.13a-1.13f and 1.14, which shows the transition matrix between the top-ranked and second-best alternatives in students rank-ordered lists.

These findings echo the literature in the U.S., which studies the factors that drive student sorting into colleges of varying quality [Hoxby and Avery, 2012, Dillon and Smith, 2017]. I find that students from disadvantaged background systematically choose lower quality college degrees, even after controlling for grades. These patterns of undermatching by SES background subsist, even in the absence of financial constraints.

1.3.3 Inequalities in Later-Life Outcomes

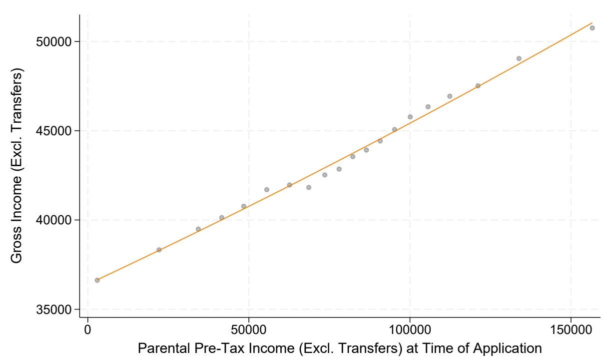
Inequalities in terms of grades as well as portfolio composition, are potential key determinants to long-term labor market outcomes of these individuals. Figure 1.1c presents the correlation between wage income 12 years post-application and parental gross income at time of application. Again, we see a striking dependence between parental SES and children's outcomes, despite the strongly egalitarian education system. Figures 1.15 and 1.16 of Appendix 1.10.1 further depict these inequalities in terms of a host of later life outcomes, by SES background. It showcases differences in economic and educational outcomes, and beyond, including family formation and fertility decisions, as measured 12 years after application to college. The following section aims to understand the extent to which admission to a more selective degree could indeed reduce these inequalities. I then turn to study the potential role of admission policies and student sorting patterns across degrees in fostering social mobility.

Figure 1.1: Three Facts



(a) High School GPA and Parental Income (USD)

(b) College Degree Quality (Measured by Average GPA of Peers) and Parental Income (USD)



(c) Student Pre-Tax Income and Parental Income (USD)

Notes: Panel (a) shows the relationship between high school grades and parental pre-tax income (excl. transfers) at time of application. Panel (b) shows the relationship between peer quality of the top-ranked degree in students' rank-ordered list and parental pre-tax income at time of application. Degree peer quality is measured by averaging over the GPA of students enrolled in that degree. Panel (c) shows the relationship between students' pre-tax income 12 years after applying and parental pre-tax income at time of application. The associated intergenerational elasticity stands at .47. The sample comprises of all Danes who apply to colleges between 2001 and 2009. Incomes are converted from DKK to 2010 USD. An exchange rate of 6.7 DKK per US dollar is used to obtain the dollar values.

1.4 Administrative Data and Key Variables

This research combines several administrative datasets covering the full Danish population. I focus on first-time college applicants between 2001 and 2009. Data from the centralized system is gathered by the Danish Ministry of Higher Education. The version I was granted access to provides information on the full rank-ordered list as well as their degree assignment. However, I do not directly observe the GPA processed by the mechanism (before 2009), nor the cutoffs. To construct the running variable and cutoffs, I build on Statistics Denmark's administrative registers, which include students' subject specific grades in oral and written exams, as well as overall GPA. Together with data on degree assignment, I recover degree-specific cutoffs, which I define as the GPA for the last admitted student. The running variable and recovered cutoffs are noisy, given that the GPA I observe may in practice differ from the GPA processed by the allocation mechanism.[29] As I describe in the next section, I account for noise in the running variable by employing a doughnut RD approach, as suggested by Dong and Kolesár [2023]. Appendix 1.10.8 shows results remain robust in specifications that do not rely on a doughnut approach.

I merge in data from several administrative registers covering the life cycle of individuals from birth up to around age 30, including information on parental attributes. These individual-level characteristics are used to test the validity of the regression discontinuity design, to capture individual heterogeneity in my model as well as to study long-term outcomes.[30]

Variables observed include income, education and origins of the parents. Parental income is measured as total pre-tax income at time of application for higher education. Parental education is captured by averaging over both parents' years of education.

29. This can arise through different channels. For instance, students who apply directly after graduation from high school get points added to their GPA.

30. These variables are added as controls for ability and family background in both the choice and treatment effects model.

Parental income, at time of application, is employed to define SES. Students are categorized into quartiles according to their parents' pre-tax income, with low-SES students identified as those in the lowest quartile and high-SES students as those in the highest quartile of the parental income distribution.

Alongside background characteristics, I use the administrative registers to link students to their socioeconomic outcomes 12 years after their first higher education application. These variables are used to explore the distributional consequences of higher education. I turn to describe these in greater detail below.

Higher Education Pipeline The data allows me to capture several aspects of the higher education pipeline, from application to completion. Beyond the application portfolio and admission recorded in the centralized admission data, I observe education spells in the education administrative register. This allows to study enrollment, time to completion, degree completion as well as years of education. College completion is defined as having undertaken over 15 years of education and graduating with at least a bachelor's degree.

Economic Outcomes The income registers provide the opportunity to examine a range of economic outcomes for individuals as they transition into adulthood. I focus on total gross income, excluding taxes and transfers, to assess earnings potential, and compare this with annual income net of taxes and transfers to evaluate the impact of redistribution in Denmark. The data also enables me to examine the impact of higher education on the extensive margin of work. I further explore home ownership as an outcome. An individual is defined as homeowner if reporting a housing value greater than \$20,000.[31] Lastly, I consider both government transfers received by individuals and taxes paid as key outcomes in the analysis.

31. If house is co-owned by partner, this implies a gross property value of \$40,000.

Family Formation I further explore the effect of higher education on a several dimensions of family formation, including marital or cohabitation status, childbearing and the timing of childbearing.

Criminal Behavior Finally, I create indicators to capture criminal participation, assessing whether an individual has ever committed a crime, and specifically distinguishing severe crimes that result in incarceration.

1.5 Marginal Effects of Admission to More Selective Degrees

This section presents evidence on the long-term consequences of admission to and enrollment in a more selective degree compared to a next-best alternative, with a focus on differences by SES background. The objective is to provide quasi-experimental evidence on whether selective degree enrollment benefits students from low-SES backgrounds and, consequently, whether increasing their representation in selective degrees could enhance their outcomes.

I build on features of the Danish centralized college admission system and apply a regression discontinuity design. Given student priorities, preference lists and capacity constraints, the equilibrium allocation arising from the deferred acceptance mechanism, gives rise to GPA cutoffs which determine degree admissions [Azevedo and Leshno, 2016]. Thus, students just above the GPA cutoff are significantly more likely to be admitted to a more selective degree than those below the GPA cutoff (as will be described in section 1.5.2 below).

1.5.1 Regression Discontinuity Design: Identification and Estimation

To identify the effect of being admitted to a more selective degree on later life outcomes, I rely on a fuzzy regression discontinuity design and use crossing the GPA cutoff as an instrument for admission. Crossing the GPA cutoff does not guarantee admission for two main reasons. First, degree-specific requirements beyond the GPA can be needed to be guaranteed admission. Second, students may gain admission to their target degree without meeting the GPA requirement by applying through 'Quota 2,' which considers criteria beyond GPA.

I specify the reduced-form relationship as follows:

$$Y_{ij} = f_j^s + \tau^s Z_{ij} + \beta^s X_{ij} + \gamma^s X_{ij} \times Z_{ij} + W_{ij} + \varepsilon_{ij} \tag{1.1}$$

where Z_{ij} is a dummy representing whether individual i is above or below the GPA cutoff for degree j. Y_{ij} is the outcome of interest. X_{ij} is the normalized GPA. f_j are degree-specific fixed effects.[32] Finally, W_{ij} represent controls, including parental income, gender, order of birth and origins. In practice, these control variables have very little effect on our estimates, and mainly serve to improve precision, as shown in Appendix 1.10.8.[33] τ^s represents the returns to cutoff-crossing by SES background, indicated by superscript s.

I consider the following first-stage equation:

$$T_{ij} = g_j^s + \rho^s Z_{ij} + \psi^s X_{ij} + \phi^s X_{ij} \times Z_{ij} + W_{ij} + \upsilon_{ij} \tag{1.2}$$

where T_{ij} is a dummy for individual i is admitted in degree j. g_j are degree-specific fixed effects.

Each student may be facing one or several cutoffs, depending on the length of their rank-ordered preference list. Formally, my main specification follows Pop-Eleches and Urquiola [2013] and stacks each of these cutoffs across individuals.[34] τ^s and ρ^s, capture the effect of

32. Fort et al. [2022] show that in multi-cutoff RDD induced by centralized allocation mechanisms, incorporating degree fixed effects allows to estimate the pooled average treatment effect at the cutoff—akin to what is done in the case of a stratified randomized trial.

33. For readability, I abstract from cohort subscripts. In practice, the specification includes degree-cohort fixed effects.

34. For example, a student applying to two capacity-constrained degrees—Economics at the University of Copenhagen and Business at Copenhagen Business School—would face two distinct cutoffs, which I stack in the analysis. Additionally, the inclusion of degree fixed effects ensures that the estimated effect leverages variation among students applying to the same degree who fall on different sides of the cutoff.

crossing the GPA cutoff to a more selective degree, for marginal applicants, relative to their next-choice option.[35] It should be interpreted as a weighted average of treatment effects around each cutoff [Fort et al., 2022].

In this setting, the local average treatment effect (LATE) corresponds to the causal impact of admission or enrollment on outcomes for instrument compliers at the cutoff, τ^s/ρ^s. It is identified under the following four assumptions. First, crossing the cutoff provides a strong instrument for admission and enrollment, as I show in the next section. Second, the conditional expectations of unobservables must be continuous around the cutoff. Though untestable, this is supported by the smooth density of applicants and their observable characteristics across the cutoff, for which I also provide evidence below. Additionally, the institutional context supports the exclusion restriction, where crossing the cutoff influences outcomes solely through its effect on admission or enrollment. Finally, the likely weakly positive direction of that effect for marginal applicants justifies the assumption of instrument monotonicity. Importantly, while the RDD identifies the LATE separately for low- and high-SES students, it does not identify the difference in the treatment effects between low- and high-SES students.

Focusing on students local to the cutoff is key to understanding the impact of marginal changes arising from policies that constrain or expand the supply of degree slots on outcomes. The LATE identified by the RDD allows to evaluate the impact of increasing the representation of low-SES in their target degree of choice. In other words, this analysis provides evidence on the allocative efficiency of *ex-ante* meritocratic admission policies, based on past performance, with respect to long-run outcomes. An efficient higher education system would ideally allocate students to degrees so as to maximize their potential gains, such as earnings. This will depend on properties of the education production function, which I study in greater detail in section 1.6.

35. Note that these more selective degrees are also more preferred. The model I build in Section 1.6 allows to disentangle degree-specific returns from preference.

The RDD specification allows for local linear approximations that differ arbitrarily on both sides of the discontinuity, focusing on a narrow bandwidth of one point around the cutoff.[36] Students' GPA are rounded at the first decimal in Denmark. Given the discrete nature of our running variable, I do not rely on methods that assume continuous density functions of the running variable to calculate the optimal bandwidth. I test for the sensitivity of my estimates to different bandwidth sizes and show robustness of the results. Inference is conducted by clustering the standard errors at the individual level, as suggested by Kolesár and Rothe [2018].

Finally, as explained in Section 3.3, I measure the running variable with noise, as I observe students' GPA from the administrative registers and not as processed by the centralized algorithm. As suggested by Dong and Kolesár [2023], I account for such noise in the running variable by employing a doughnut RD approach and drop observations around the cutoff over the interval $[-.1, .1]$, where the running variable may incorrectly classify the treatment assignment. Under this approach, the RDD recovers the effect of cutoff-crossing for individuals at the noisily measured cutoff, assuming that the conditional means of the potential outcomes are smooth at that cutoff. These specifications and the chosen bandwidth size do not drive the results I find, as shown in Appendix 1.10.8.

Before presenting results of the regression discontinuity design, I conduct two sets of validation exercises. First, I show in Figure 1.17 of Appendix 1.10.2 that there is no manipulation of the running variable. The histogram shows that the mass on either side of the discontinuity is smooth. The slight jump at the discontinuity arises from the construction of the cutoff and the pooling across multiple cutoffs. Since each degree-specific cutoff is set at the GPA of the last admitted student, there is necessarily at least an individual at that cutoff for each degree.[37] Importantly, there is no *a priori* reasons that students would sort

36. The Danish grading scale ranges from 0 to 13 until 2005. It was then replace by a scale ranging from -3 to 12.

37. Fort et al. [2022] shows this can arise in RDDs in centralized admission settings. It is reminiscent to

around the cutoff. Cutoffs are unobserved to students and vary year-on-year based on the supply and demand for each degree. I formally test the null of no manipulation around the discontinuity by conducting a version of the McCrary [2008] which allows for a discrete running variable, as developed by Frandsen [2017]. This approach requires to set a parameter $k \in [0, 1]$ which determines the maximum nonlinearity in the probability mass function allowed.[38] For low-SES students, I cannot reject the null hypothesis of a smooth histogram for any values of k (p-value = .144 for $k = 0$). For high-SES students, I cannot reject the null of no manipulation at conventional levels, for values of $k > 0.06$ and after dropping individuals right at the cutoff (p-value of .15 for $k = 0.07$).

Second, I run specification 1.1 with pre-determined variables as dependent variables, including parental income, college attendance, gender and origins. These tests of continuity are presented in Figure 1.18, which plots pre-determined variable against the running variable, and reports the coefficients of interest in the top-right corner. There is no statistically significant discontinuity in these variables at the cutoff.

Table 1.1 presents summary statistics on the sample used in this analysis, separately for low- and high-SES. There are large differences in parental gross income at time of application between these two groups, with parents from high-SES students having more than three times the parental income of low-SES students. Across SES background, there are also differences in parental education, with an average of two extra years of education for high-SES students. Importantly, students' performance in school, as captured by their GPA, differs across parental income, as do the quality of the primary school attended. Furthermore, low-SES students are also much likely to be of immigrant origins. In both groups, female are more represented than males. As seen in the last row of Table 1.1, mean difference in wage

issues arising in settings with randomized waitlists, where the last unit offered treatment is by construction also always taking up the treatment [de Chaisemartin and Behaghel, 2020].

38. A larger k allows for more deviation from linearity at the cutoff before the test rejects. A smaller k results in the test rejecting small deviations from linearity with high probability. Thus, selecting a higher k reduces the test's power.

income stands at almost 20% across both groups, 12 years after first applying to college.

Table 1.1: Summary Statistics

	Bottom Income Quartile	Top Income Quartile	Total
Parental Gross Income	39,699 (41,298)	163,890 (122,649)	128,233 (119,876)
Parental Gross Income (Excl. Transfers)	22,050 (40,611)	162,310 (122,551)	122,040 (123,316)
Parent College Completion	0.3 (0.5)	0.6 (0.5)	0.5 (0.5)
Parent Years of Education	12.4 (2.7)	14.7 (2.2)	14.2 (2.5)
Order of Birth	1.9 (1.1)	1.7 (0.8)	1.7 (0.9)
High School GPA	8.0 (1.2)	8.4 (1.1)	8.3 (1.1)
Peer Grades in Primary School	6.0 (0.7)	6.5 (0.6)	6.4 (0.7)
Gender			
Female	5,656	12,550	18,206
Male	2,365	7,317	9,682
Origin			
Natives	6,038	20,006	26,044
Immigrants	1,251	61	1,312
Descendants	830	92	922
Wage Income	40,502 (23,579)	47,749 (25,576)	45,669 (25,233)
Number of Observations	8,119	20,159	28,278

Notes: This table reports summary statistics of our RD sample. The sample comprises of all Danes who complete apply to college between 2001 and 2009. Incomes are converted from DKK to 2010 USD. An exchange rate of 6.7 DKK per US dollar is used to obtain the dollar values. Standard deviation in parentheses.

1.5.2 Degree Admission, Enrollment and Characteristics

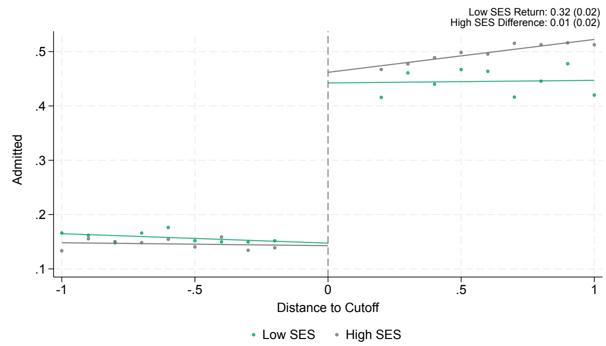
To begin the analysis, I assess the effect of crossing the GPA cutoff on admission and enrollment patterns. This provides evidence on the first-stage relationship, presented in Equation 1.2. I also provide evidence on the effect of cutoff-crossing on degree characteristics to better understand the nature of the effect of scoring above the GPA cutoff for both low- and high-SES students.

These effects are depicted in Figure 1.2a. This figure is constructed in the following way. First, I residualize the outcome Y_{ij}, removing degree fixed effects, f_j, and covariates X_{ij}, and adding back the outcome means, for readability. I then plot the residualized mean outcomes, for each SES group, binning at the level of each discrete value of the running variable. Each figure plots in the top-right corner, the estimated effect of cutoff-crossing for low-SES students, as well as the differential effect, for high-SES students.

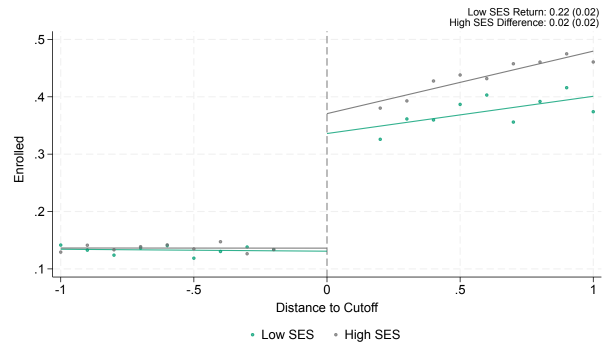
Cutoff-crossing leads to similar consequences in terms of admission across SES background. I find that admission to a more selective degree increases the probability of getting an offer by 32 percentage points.[39] Moving forward along the college admission pipeline, I find that cutoff-crossing leads to a 22 percentage point increase in degree enrollment. The effects are homogeneous across SES groups.

39. There are several reasons for imperfect compliance. First, degree-specific requirements beyond the GPA can be needed to be guaranteed admission. Second, the strategy aggregates all cutoffs, implying that some students who exceed a particular cutoff may not be admitted, as they could have already secured admission to a higher-preference degree. Third, the running variable is measured with noise, as discussed in Section 3.3. Finally, students may gain admission to their target degree without meeting the GPA requirement by applying through 'Quota 2,' which considers criteria beyond GPA.

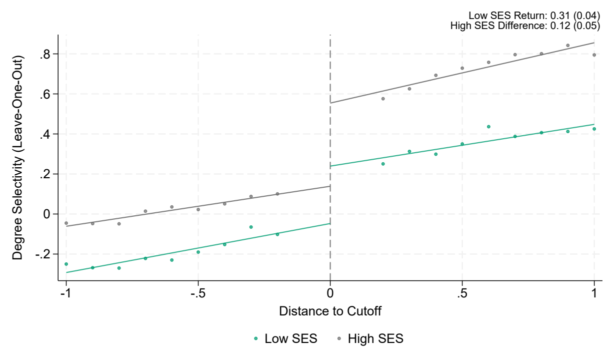
Figure 1.2: Degree Offer, Enrollment and Characteristics



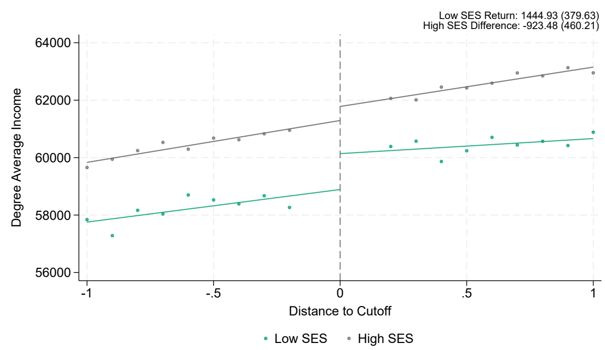
(a) Degree Offer



(b) Degree Enrollment



(c) Degree Peer Grades



(d) Degree Average Income

Notes: These figures present evidence on the first-stage effect of cutoff-crossing, as specified by each sub-figure title. Results are depicted separately for low-SES students (in green) and high-SES students (in gray). The top-right corner of each panel displays the estimated coefficients along with their standard errors. The term 'Low-SES return' denotes the effect for low-SES students, while 'High-SES difference' represents the estimated difference in effects between high- and low-SES students ($\tau^1 - \tau^0$). Standard errors clustered at the individual level are in parentheses. Panel (a), (b), (c) and (d) respectively outline the effect on degree offer, enrollment and degree peer characteristics, as measured by average grades and prior graduates' average income by degree.

Finally, I show how scoring above the cutoff impacts peer composition in the admitted degree, using the two measures described in Section 1.3. Figure 1.2c shows that students marginally above the cutoff, experience a degree selectivity that is about .3 of a standard deviation higher, than students marginally below the cutoff. This effect is larger by about .1 of a standard deviation for high-SES individuals. Another approach to understand the nature of the portfolio choice is to build on income-based measures of degree quality, to capture the extent to which students from different backgrounds choose degrees with different expected labor market income. Figure 1.2d shows that there is a discontinuity for both low- and high-SES students in terms of degree average income, as they cross the cutoff, although the effect is more pronounced for low-SES students.

To better interpret the estimates recovered by the stacked regression discontinuity design, I describe the degree portfolio composition of students at the margin. Crossing the cutoff may lead both to degree changes within and across fields of study and university. Figures 1.24-1.25 of Appendix 1.10.5 show that this is not the case. There are no discontinuous changes in both field of study as well as university choice. Thus, the effects estimated in the next section should be interpreted as the impact of crossing the GPA cutoff into a more selective degree within the same broad field of study and university.

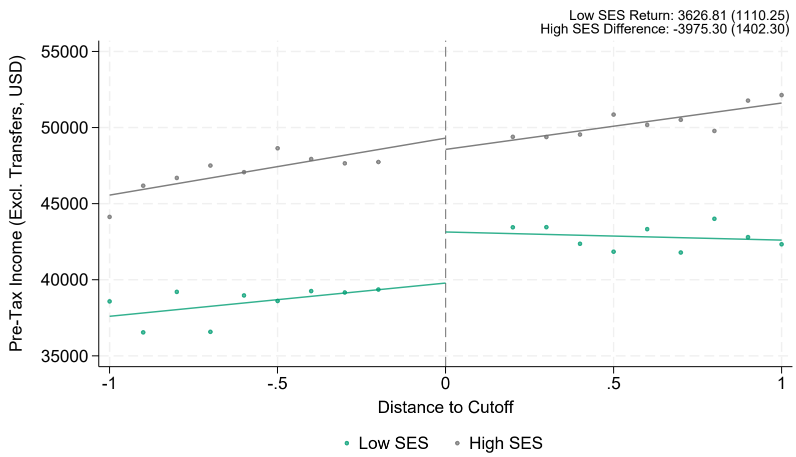
1.5.3 Long-Term Outcomes

In this section, I present evidence on the long-term socioeconomic impacts of admission and enrollment in a more selective degree for instrument compliers at the margin of attendance. I proceed by reporting estimates from the reduced-form specification set out in Equation 1.1, corresponding to the effect of cutoff-crossing on outcomes, by SES background. To recover the LATE, these estimates can be weighted by the inverse of the first-stage estimates, as presented in the previous section. In particular, reduced-form estimates can be multiplied by a factor of about 3.1 (1/.32) to get the effect of admission and by a factor of 4.5 (1/.22) to recover the effect of enrollment in a more selective degree.

Figure 1.3 provides graphical evidence of the effect of cutoff-crossing on pre-tax income of individuals 12 years after college application. As students from low-SES backgrounds cross the GPA cutoff, they experience a significant increase in their income, of around $3,600 annually. This represents a jump of around 9% as compared to students just below the cutoff. Strikingly, this increase in income at the cutoff is statistically insignificant for high-

SES students. Translating these effects into returns from admission and enrollment to more selective degrees, low-SES students experience an increase of around $11,000 increase in annual income from being admitted to a more selective degree, and of around $16,000 from enrolling in such degree.

Figure 1.3: Pre-Tax Income (Excl. Transfers), 12 years post-application



Notes: Reduced-form effect of cutoff-crossing on pre-tax income (excluding transfers), 12 years post-application. Results are depicted separately for low-SES students (in green) and high-SES students (in gray). The top-right corner of each panel displays the estimated coefficients along with their standard errors. The term 'Low-SES return' denotes the effect for low-SES students, while 'High-SES difference' represents the estimated difference in effects between high- and low-SES students ($\tau^1 - \tau^0$). Standard errors clustered at the individual level are in parentheses.

To explore the impact of the Danish welfare state and its redistribution through a comprehensive tax and transfer system, I consider the effect of cutoff-crossing on taxes and net income. The equalizing forces of the welfare state are at play, which reduce the gross returns by about $1,500 for low-SES students. Net of tax returns amount to around $2,100 as seen in Figure 1.19b of Appendix 1.10.3. The rise in tax contributions from low-SES students above the cutoff indicates that expanding access for disadvantaged students in selective degrees could be self-financing.

A growing literature in economics focuses on understanding the role of colleges in reaching top positions in the economy [Zimmerman, 2019, Michelman et al., 2021, Chetty et al., 2023]. I investigate this question by studying the impact of cutoff-crossing on probability of becoming a top income earner. Figure 1.19d presents a contrasting result to Zimmerman [2019], showing that the SES gap in the likelihood of reaching the top 1% closes as low-SES students cross the GPA cutoff.

Figure 1.20b further documents the differential effect on unemployment status, across SES background. The patterns presented point to larger returns for low-SES students, with a 2 percentage point reduction in unemployment. Interestingly, there no evidence that occupational sector is impacted by crossing the GPA cutoff, as shown in Figure 1.23.

I also consider the impact on capital accumulation, focusing in particular on home ownership. Unlike gains in terms of income, there does not seem to be any differential returns in terms of home ownership 12 years after application, as shown in Figure 1.20a of Appendix 1.10.3. This result may reflect the fact that individuals are observed early in their career, at around age 30.

Finally, degree selectivity may have consequences beyond economic returns, including effects on family formation. For instance, Kirkebøen et al. [2021] demonstrates that colleges can act as marriage markets where students meet future partners. I examine patterns of family formation by SES background to assess whether these effects vary. Figure 1.21 shows that cutoff-crossing increases fertility by approximately 0.08 children for both low- and high-SES students, while having no significant effect on family structure, such as cohabitation or marriage rates.

I also extend the analysis to explore the effect of degree selectivity on criminal activity, where the opportunity cost of crime may rise with increased income, particularly for low-SES students. Figure1.22 presents these findings, indicating that cutoff-crossing has no significant impact on criminal activity.

1.5.4 What Drives the Large Returns for Low-SES Students?

The findings thus far shed light on large differential labor market returns from attending and enrolling in a more selective college degree, for compliers at the margin of the GPA cutoff. While high-SES students obtain no gains, low-SES students benefit greatly.

This section examines several channels driving the substantial returns observed for low-SES students:

1. Educational trajectory and learning outcomes

2. Correlates of low-SES

3. Returns to exposure to higher-achieving peers

Educational Trajectory and Learning

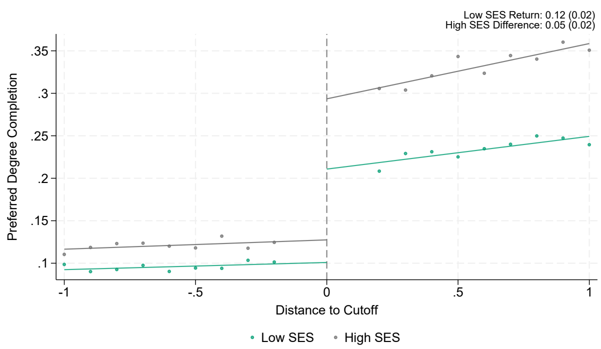
This analysis starts by assessing whether differences in educational trajectories can account for the heterogeneity in returns. I evaluate this hypothesis along three dimensions—preferred college degree completion, educational attainment, and time to completion—finding no evidence of differential effects between low- and high-SES students driving the patterns uncovered, as illustrated in Figure 1.4.

Figure 1.4a illustrates the effect of crossing the GPA cutoff on the probability of completing one's preferred degree twelve years after application. High-SES students exhibit a higher likelihood of graduating from their preferred degrees, with an increase of approximately 17 percentage points, compared to 12 percentage points for low-SES students.

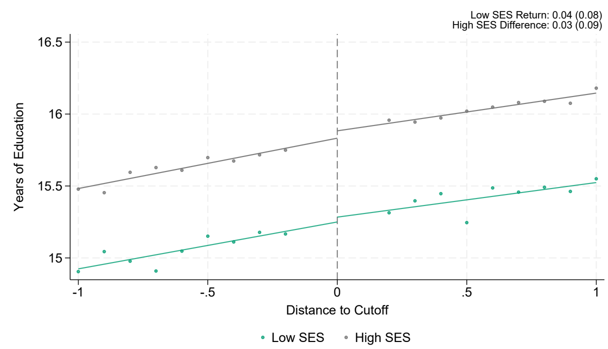
Figure 1.4b demonstrates that although high-SES students complete more years of education overall, crossing the cutoff has no significant effect on the number of years of education completed across SES groups. A similar pattern is observed for time to completion, defined as the number of years between application and the completion of the final degree, with no differential effect of cutoff-crossing across SES groups, as depicted in Figure 1.4c.

Finally, I consider the impact of cutoff-crossing on college GPA, as a means to uncover any potential heterogeneity in learning and human capital accumulation, which may drive the differential returns on income. As presented in Figure 1.4d, I do not find any significant effect across groups. This suggests that differences in learning, as measured by GPA, do not explain the patterns I uncover. However, it is important to note that GPA may be inversely correlated with degree selectivity, which could make it a noisy measure of learning outcomes.

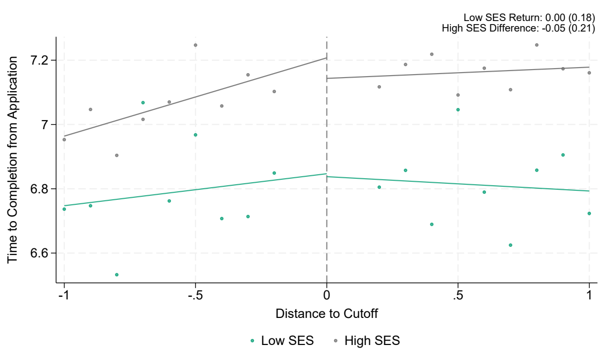
Figure 1.4: Potential Drivers of Heterogeneous Returns



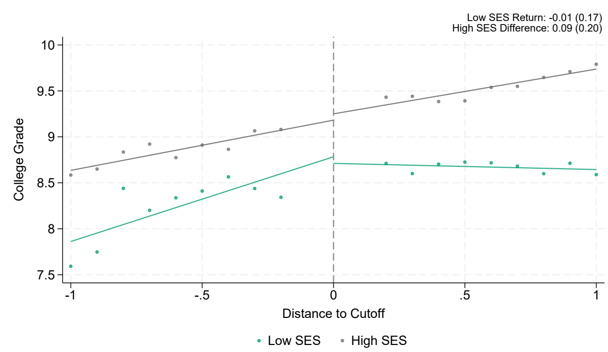
(a) Preferred Degree Completion



(b) Years of Education



(c) Time to Degree Completion



(d) Learning: College GPA

Notes: Potential mediators of the reduced-form effects of cutoff-crossing on income. Results are depicted separately for low-SES students (in green) and high-SES students (in gray). The top-right corner of each panel displays the estimated coefficients along with their standard errors. The term 'Low-SES return' denotes the effect for low-SES students, while 'High-SES difference' represents the estimated difference in effects between high- and low-SES students ($\tau^1 - \tau^0$). Standard errors clustered at the individual level are in parentheses. Panel (a), (b), (c) and (d) respectively show the effect of cutoff-crossing on degree completion, years of education, time to degree completion and college grade.

Correlates of Low-SES and Students' Ability

Moreover, observable differences in characteristics between low- and high-SES students near the discontinuity suggest that the estimated differential effects may be disproportionately concentrated among students exhibiting these specific traits. In particular, low-SES students have significantly lower high school grades than their high-SES counterparts with almost a half-standard deviation difference in high school grades between low- and high-SES students.

To understand whether the effects I find are driven by effects experienced by low GPA students, irrespective of SES background, I re-estimate my main specification for students with below-median grades. I find that the differential effects across SES background remains. Low-SES students with below-median grades experience significantly greater gains compared to their high-SES counterparts with similar academic performance from cutoff-crossing, as shown in Appendix 1.10.6. In contrast, the effects of cutoff-crossing for above-median students are close to zero across SES background.

Importantly, a unidimensional GPA provides only a noisy measure of the multidimensional nature of students' skills (see, for instance Borghans et al. [2016]). Differences in motivation, aspirations, or other skill dimensions may further contribute to the differential effects observed. This point raises questions about interpreting the pooled RDD, as students with varying latent abilities across choice margins are aggregated. Consequently, the average effect may obscure substantial heterogeneity in returns, as demonstrated in this paper. In ongoing work, I utilize subject-specific grades and test scores to construct a more comprehensive measure of student skills, with the goal of identifying the factors driving the observed gap in returns.

Finally, I show in Appendix 1.10.6 and 1.10.6, that the effect of cutoff-crossing on pre-tax earnings is more pronounced for males and students of non-Danish origins. These findings indicate that degree selectivity may offer significant economic mobility benefits for students of foreign origin.

Peer Effects

This section explores peer effects as a potential explanation for the differential returns. Earlier results suggest that neither educational trajectories nor ability differences are likely contributors. Moreover, portfolio composition fails to explain the observed returns, as students across the cutoff, regardless of SES, have a fallback option at the same university and in the same field as their preferred degree.

A plausible explanation is that peer effects vary by SES background. Low-SES students may benefit more from selective degrees by enhancing their social capital and building networks that could offer significant advantages in the labor market Machado et al. [2024]. Furthermore, a high-quality peer environment may influence aspirations, potentially having a stronger impact on low-SES students.

To test this hypothesis, I employ two empirical strategies. The first leverages methods akin to those developed by Kirkeboen et al. [2016]. The availability of students' rank-ordered preferences, as submitted to the centralized allocation mechanism, enables the construction of counterfactual degree admissions.

Specifically, for each individual, I can observe both their preferred degree and their fallback option. This allows for the estimation of counterfactual degree admission outcomes. I then link variations in peer quality across the GPA cutoff to later-life outcomes, focusing on clear margins of choice.

I define peer quality as the average parental income of students enrolled in a given degree, which serves as a proxy for the socioeconomic background of peers and the quality of its social network. To capture the impact of peer quality differences between the preferred degree and the fallback option, ΔQ_i, I categorize these differences into tertiles. I then estimate the following model separately for high and low-SES students:

$$Y_{ij} = \left[\mu_j^s + \nu_j^s + \tau^s Z_{ij} + \beta^s X_{ij} + \gamma^s X_{ij} \times Z_{ij}\right] \times \delta^s Q_i + \varepsilon_{ij} \tag{1.3}$$

where μ_j and ν_j denote fixed effects for the preferred and fallback degree, respectively. The remaining variables are specified as in Equation 1.1.

This model is akin to running the reduced-form specification in 1.1, while interacting it with the difference in peer quality between the preferred and fallback option.

As described in Table 1.4 of Appendix 1.10.7, peer quality improvements in the middle tertile correspond to an increase in average peer parental income of approximately $4,200. For students in this group, the long-term impact of cutoff-crossing results in an annual pre-tax income gain of about $8,200 (SE=$3,600). The implied local average treatment effect (LATE) estimate is roughly $12,600, given that the first-stage relationship between cutoff-crossing and admission stands at 65 percentage points.

The findings reveal that low-SES students disproportionately benefit from admission to degrees with higher peer quality relative to their fallback option, compared to high-SES students. These benefits are concentrated among students experiencing peer quality improvements in the middle tertile of the peer difference distribution, underscoring the non-linear nature of peer effects Brock and Durlauf [2001]. In contrast, high-SES students do not experience significant income gains across any of the peer quality tertiles following cutoff-crossing.

The second identification strategy exploits within-degree variation in peer quality across cohorts. This approach relies on cohort-level differences in peer composition, controlling for individual sorting into degrees through the inclusion of fixed effects.[40] The estimated model is as follows:

$$Y_{ijc} = \alpha + \beta S_{ijc} + \gamma F_{ic} + \rho_j + \tau_c + \varepsilon_{ijc} \tag{1.4}$$

where Y_{ijc} denotes later life income for individual i in degree j for a given cohort c. S_{ijc}

40. This identification strategy is in the spirit of Hoxby [2000] and more recently Cattan et al. [2023], exploiting idiosyncratic variation in peer composition in schools.

denotes average peer parental income, while F_{ic} denotes the vector of parent-supplied inputs.

Unbiased and consistent estimation of the effect of degree peer quality rests on the assumption that changes in peer quality across cohorts within the same degree are orthogonal to unobserved factors influencing outcomes, after conditioning on the vector of covariates:

$$\mathrm{Cov}(S_{ijc}, \varepsilon_{ijc} \mid F_{ic}, \rho_k, \tau_c) = 0 \tag{1.5}$$

Under this specification, variation in degree peer quality comes from changes in peers in adjacent cohorts within the same degree. Assumption (1.5) implies that students experiencing idiosyncratic shocks to their degree peer composition, do not endogenously sort as a response, for instance by delaying their enrollment decisions. This assumption may be plausible given information frictions regarding the exact composition of degree peers in any given cohort.

Estimates from Equation 1.4, reported in Table 1.5 of Appendix 1.10.7, provide additional evidence on the differential peer effects by SES background. A one-standard deviation increase in peer quality—reflecting a \$18,500 rise in average parental income—leads to a 6.2% increase in annual pre-tax income (excluding transfers) for low-SES students. In contrast, high-SES students do not experience any significant gains from improvements in peer quality.

Overall, the findings from both empirical strategies suggest that peer composition significantly influences labor market returns for low-SES students, whereas high-SES students appear unaffected. However, isolating the causal effect of peers remains particularly challenging given the existence of unobserved attributes correlated with peer quality. While I find that students' fallback options are within the same field of study and institution, the quality of peers may still be correlated with unobserved characteristics of the degree, such as the quality of instruction or the rigor of coursework. As a result, the findings in this section should be interpreted as suggestive rather than definitive evidence of differential peer effects.

The findings thus far offer quasi-experimental evidence on the returns to degree selectivity for low-SES students. Specifically, the results highlight that low-SES students derive substantial benefits from admission to more selective degrees, particularly through exposure to higher-quality peers. In contrast, the lack of significant effects for high-SES students suggests that the value of degree selectivity may be less relevant for those who already possess greater social capital and access to high-quality networks.

1.6 A Model of the Centralized Higher Education System in Denmark

In this section, I develop and estimate a model of the Danish higher education system to investigate the sources of labor market inequalities observed among college applicants, despite the existence of a universal higher education system. As illustrated in Figure 1.1b, significant socioeconomic disparities remain in Denmark, as measured by the wages of students from different SES backgrounds. To study the determinants of this gap, I build a model of the centralized higher education system, which serves two main objectives. First, to evaluate whether GPA-based admission policies limit intergenerational mobility by restricting access to selective degrees for disadvantaged students who stand to benefit. Second, to explore whether improving student portfolio choices of colleges and majors could eliminate this gap. Building on prior models of student choices and outcomes under centralized admission systems [Abdulkadiroğlu et al., 2020, Dur et al., 2020, Barahona et al., 2024], I extend this framework by incorporating the influence of average peer characteristics on students' long-term outcomes.

To achieve these objectives, the model enables me to recover student preferences and selection-corrected degree-specific returns by modeling students' degree choice behavior. This modeling approach allows to go beyond variation at the discontinuity studied in the quasi-experimental setting. Notably, these estimates bundle student degree choices with degree-specific returns, as the returns experienced by low-SES students may potentially reflect the poor fallback options they select. Unpacking these effects requires adding structure to separate degree-specific causal returns from student choice patterns. Moreover, estimating causal returns for specific degrees poses significant challenges, as students may self-select into degrees based on unobserved attributes.

Using the model, I then simulate alternative admission policies to evaluate counterfactual outcomes while accounting for the general equilibrium effects inherent in such policy changes. These effects are essential, as policy interventions that shift one student into a different degree can create a spillover effects, displacing other students in the process and thereby altering the overall composition of students across degrees, as shown by Gandil [2023]. For each reallocation, the model accounts for changes in peer composition of degrees and their impact on outcomes.

I proceed in the following way. After describing the environment, I capitalize on the centralized college assignment mechanism in Denmark, I summarize student preferences by fitting discrete choice models to students' rank-ordered lists of degrees (cf. Section 1.6.2). In a second step, I estimate degree-specific treatment effects on long-term outcomes through a joint model of potential outcomes and treatment selection (cf. Section 1.6.2 and 1.6.2). The model will allow for parameters to vary with students' socioeconomic backgrounds, so as to understand the heterogeneity both in sorting patterns as well as returns.

1.6.1 Setting

Consider a set of individuals indexed by $i \in \mathcal{I} = \{1, \ldots, N\}$. Let $\mathcal{J} = \{0, 1, \ldots, J\}$ be the set of all degrees (college-major pairs) to which students can apply. The outside option, $j = 0$, represents the decision to either defer college enrollment and reapply later or forgo college attendance altogether.

Student preferences can be summarized by a strict ordering, $\succ_i$, that represents the student's preference over degrees.[41] Further consider two groups within the student population, defined by their SES, $s_i \in \mathcal{S}$. Let $\mathcal{S} = \{0, 1\}$, such that $s_i = 0$ represents low-SES and $s_i = 1$ represents high-SES students. The status s_i of each student is observable to colleges. In the model, students are classified as low-SES if their parental income falls below the median, while those with parental income above the median are categorized as high-SES.

Degrees have preference over students solely based on their GPAs a_{ij}.[42] Each degree $j \in \mathcal{J}$ has a capacity $q_j > 0$. Assume $q_0 = \infty$ for the outside option, as not attending college is available to all. A student is fully characterized by their type $\theta_i = \left(\succ_i, s_i, a_{ij}\right)$, the combination of an applicant's preferences, type and GPA. I denote the set of all student types by $\Theta = \bigcup_{i \in \mathcal{I}} \theta_i$.

A deferred acceptance algorithm takes into account of student type Θ and degree capacity constraint q_j to match students to degrees [Gandil, 2023]. Let $\mu : \Theta \rightarrow \mathcal{J}$ be the matching function that matches a student of a given type to a particular degree. The DA algorithm used, ensures that the assignment between students and colleges are unique and stable [Gale and Shapley, 1962].[43]

The algorithm used is key to our analysis. Under DA, truthful reporting of preferences is a weakly dominant strategy [Dubins and Freedman, 1981]. This property motivates the approach presented in 1.6.2, which summarizes student preferences by fitting discrete choice models to the rank-ordered lists submitted by students.[44]

The matching, μ, can be uniquely characterized by a vector of market-clearing admission

41. Student's preferences are assumed to be complete, transitive, and strict.

42. As described in Section 1.2, college admission system in Denmark also considers a set of applicants based on other characteristics besides their GPA. In the model, I abstract from these students.

43. A stable matching is one in which there does not exist any student-degree pair (i, j), where both prefer each other to their current match under the matching.

44. An alternative approach to recovering preferences would relax the strategy-proofness assumption and instead leverage the stability property of the DA algorithm, as shown by Fack et al. [2019]. However, this approach relies on variation in observed choices, whereas the strategy I propose to identify the outcome equation requires variation in students' rank-ordered lists.

cutoffs, $c_j(\mu)$, for each degree j, as showed by Azevedo and Leshno [2016]. An admission cutoff, $c_j(\mu)$, represents the minimum score a_{ij} required for admission to degree j for students with SES status s_i. Because the outside option is always available, it is assigned an admission cutoff of $-\infty$.

In line with any strict-priority mechanism, the availability of degree options varies based on students' priority scores. Let $\Omega_i(\mu) = \{j \in \mathcal{J} \mid a_{ij} \geq c_j(\mu)\}$ represent the set of feasible degree for individual i, defined as those degrees for which the individual meets the required cutoff score based on their score and SES status under the matching μ. Let $D_i(\mu) = \{j \in \Omega_i \mid j \succeq k, \text{for all}, k \in \Omega_i(\mu)\}$ denote the most preferred option in the feasible choice set $\Omega_i(\mu)$. In other words, $D_i(\mu)$ represents the degree ranked highest by individual i among the set of degrees they are eligible for. According to the stability condition, $D_i(\mu) = \mu(\theta_i)$.

The realized outcome for student i is given by $Y_i(\mu) = \sum_j \mathbb{1}\{D_i(\mu) = j\} \times Y_{ij}$ where $D_i(\mu)$ denotes the degree attended, Y_{ij} the potential outcome of interest for student i attending degree j.

1.6.2 Empirical Models

This section outlines the strategy used to estimate two key components of the model: student preferences and degree-specific returns. The model then enables an analysis of the effects of reallocating students across degrees according to their preferences, under varying specifications of the matching function dictated by admission policies.

Degree Choice Model

The degree choice model presented in this section serves two purposes. First, to construct selection-corrected estimates of degree treatment effects, as described in Section 1.6.2. Second, in studying optimal allocations, these preference parameters will enable students to match with their best available degree choice, through the DA algorithm, under alternative policies.

Consider a selection model where individual i makes a degree choice j. I model student preferences using a random utility model. The utility function from enrolling in degree j, U_{ij}, is specified as follows:

$$U_{ij} = \delta_{c(Z_i)j} - \gamma_{c(Z_i)} d_{ij} + \eta_{ij} \tag{1.6}$$

where η_{ij} captures unobserved tastes.

The rank-ordered logit model allows for a degree specific mean utility $\delta_{j,c(Z_i)}$ and is fit separately to each cell $c(Z_i)$, where Z_i denotes a vector of individual characteristics. The following variables are dichotomized at the median, and included in Z_i: parental income and education as well as high school math and danish grades. Z_i further includes gender and tertiles of average degree income. In total, c includes 96 cells. Finally, d_{ij} denotes distance to college.

I model the unobserved taste η_{ij} as independent and identically distributed, following an extreme value type I distribution. With this assumption, Equation 1.6 is a rank-ordered multinomial logit model, which is estimated via maximum likelihood.[45] In particular, estimating this model recovers estimates of degree-specific mean utilities, $\delta_{j,c(Z_i)}$, which capture student preferences for a degree in each cell c.

The logit model implies the conditional likelihood of the rank list R_i is

$$\mathcal{L}\left(R_i \mid Z_i, d_i\right) = \prod_{k=1}^{\ell(i)} \frac{\exp\left(\delta_{c(Z_i)R_{ik}} - \gamma_{c(Z_i)} d_{iR_{ik}}\right)}{\sum_{j \in \mathcal{J} \setminus \{R_{im}: m<k\}} \exp\left(\delta_{c(Z_i)j} - \gamma_{c(Z_i)} d_{ij}\right)} \tag{1.7}$$

45. The rank-ordered logit model is also referred to as the exploded logit model because it treats each position in the rank-ordered list as a sequential decision point, effectively decomposing the overall ranking into a series of conditional logit choices. For each rank, the model estimates the probability of selecting a specific alternative, given that higher-ranked options have already been chosen, and continues to "explode" the ranking into logit choice probabilities until all ranked alternatives are considered [Chapman and Staelin, 1982].

where $\ell(i)$ denotes the length of the list.

Earnings Outcomes Model

This section specifies the earnings outcomes model, whereby a given allocation between students and colleges produce an output Y_{ij}. This model will be used to recover degree-specific treatment effects, by SES, and is specified as follows:

$$Y_{ij} = \alpha_j^s + X_i^{'}\beta_j^s + \bar{X}_i^{'}\gamma_j^s + \varepsilon_{ij} \tag{1.8}$$

Let Y_{ij} denote the potential outcome for student i if she attends degree j. Potential outcomes are a function of a degree specific intercept, α_j. Moreover, I allow each degree j to have a distinct effect based on the students' characteristics X_i, which I refer to as match effects. A match effect arises from the interaction between observed and unobserved characteristics and the returns to a given degree. For instance, students may benefit more or less from a high return degree such as engineering, based on their previously acquired skills. Covariates in X_i include parental income, education, origins, mathematics and danish grades and gender.[46]

Departing from much of the existing literature, I introduce heterogeneity in degree effectiveness based on peer characteristics, denoted $\bar{X}$. This extension is critical for assessing the role of peer compositional changes on student outcomes. Indeed, as shown in Section 1.5.4, peer composition can significantly impact labor market outcomes. In this context, peer attributes are captured by the average high school grades of students, X_i, aggregated at the degree-year level, $\bar{X}_i$.[47]

46. For the sake of readability, cohort fixed effects, while included in the model, are omitted from the discussion.

47. The identification of peer effects on student outcomes relies on variation in peer composition across cohorts within the same degree, in the spirit of Hoxby [2000]. Furthermore, using the average of past student performance to measure peer characteristics addresses simultaneity concerns inherent to the reflection problem [Manski, 1993].

I normalize X_i and $\bar{X}_i$ to have mean zero, so that α_j is the average outcome for students with average characteristics. Each of these parameters are SES-specific, denoted by a superscript s.

The mean outcome Y_i observed in the data for a given matching $\hat{\mu}$ is given by:

$$\mathbb{E}\left[Y_{ij} \mid X_i, \bar{X}_i, D_i = j\right] = \alpha_j^s + X_i'\beta_j^s + \bar{X}_i{}'\gamma_j^s + \mathbb{E}\left[\varepsilon_{ij} \mid X_i, \bar{X}_i, D_i = j\right]$$

A clear difficulty in identifying the population parameters α_j and β_j set out in Equation 2.5a, is that individuals may select into degrees, based on unobserved attributes. Estimating this specification with OLS would only recover unbiased estimates under the assumption that $\mathbb{E}[\varepsilon_{ij} \mid X_i, \bar{X}_i, D_i = j] = 0$. In the next section, I outline how leveraging information from students' rank-ordered lists can be used to relax this assumption.

Selection-Correction Approach: Identification and Estimation

To recover unbiased estimates of degree-specific treatment effects, I leverage the elicited preferences for degrees, from students' rank-ordered lists.

In particular, I account for unobserved heterogeneity by conditioning Equation 2.5a on the vector of unobserved tastes, $\eta_i = (\eta_{i0}, \eta_{i1}, ..., \eta_{iJ})$, thereby linking the outcome equation to the degree choice model.

Assumption 1. *The conditional expectation of Y_{ij} is specified as follows:*

$$\mathbb{E}[Y_{ij} \mid X_i, \bar{X}_j, \eta_{ij}] = \alpha_j^s + X'\beta_j^s + \bar{X}_i{}'\gamma_j^s + \mathbb{E}[\epsilon_{ij} \mid X_i, \bar{X}_i, \eta_{ij}] \tag{1.9}$$

This assumption is in the spirit of Dale and Krueger's "self-revelation" model [2002, 2014], recently revisited by Mountjoy and Hickman [2021]. Under this assumption, comparing sets of students with identical covariates and rank-ordered choice lists allows us to recover the causal effects of degree enrollment. By eliciting their preferences, students reveal their unobserved "types" through the degrees they apply to, implying that degree enrollment is conditionally random among students applying to the same degrees.

Although this assumption is conceptually appealing, fully nonparametric matching based on rank-ordered lists is impractical, as very few students share identical rankings. To address this limitation, I use the structure of the logit choice model to develop a parametric approximation to the matching procedure.[48] Specifically, I adopt the following parametrization:

$$
\begin{aligned}
&\mathbb{E}[Y_{ij} \mid X_i, \bar{X}_i, \eta_{i1}, \ldots, \eta_{iJ}] \\
&\quad = \alpha_j^s + X_i' \beta_j^s + \bar{X}_i{}' \gamma_j^s + \textstyle\sum_{k=0}^{J} \psi_k^s \times (\eta_{ik} - \mu_\eta) + \varphi^s \times (\eta_{ij} - \mu_\eta),\ j = 1, \ldots, J,
\end{aligned}
\tag{1.10}
$$

where $\mu_\eta = \mathbb{E}[\mu_{ij}]$ denotes Euler's constant.

As Abdulkadiroğlu et al. [2020] highlight, this model allows for essential heterogeneity (Heckman et al. [2006b]). The coefficient ψ_k^s reflects the effect of preference for degree k across all potential outcomes, allowing students with strong preferences for specific degrees to exhibit higher or lower ability. φ^s denotes a match effect of preference for degree j on the potential outcome for that particular degree.

By iterated expectations, Equation 1.10 implies that mean observed outcomes at degree j are

$$
\begin{aligned}
&\mathbb{E}\left[Y_i \mid X_i, \bar{X}_i, R_i, D_i = j\right] \\
&\quad = \alpha_j^s + X_i' \beta_j^s + \bar{X}_i{}' \gamma^s + \textstyle\sum_{k=0}^{J} \psi_k^s \lambda_k \left(X_i, \bar{X}_i, R_i\right) + \varphi^s \lambda_j \left(X_i, \bar{X}_i, R_i\right)
\end{aligned}
\tag{1.11}
$$

where $\lambda_k \left(X_i, \bar{X}_i, R_i\right) = \mathbb{E}\left[\eta_{ik} - \mu_\eta \mid X_i, \bar{X}_i, R_i\right]$ gives the mean preference for degree k conditional on a student's and peer's characteristics, and preference list. The $\lambda_k(\cdot)$ func-

48. Alternative approaches to controlling for selection in multinomial choice settings include Lee [1983a], Dahl [2002], Dubin and McFadden [1984].

tions act as control functions, addressing selection on unobservables [Heckman and Robb, 1985]. These functions generalize the formulas derived by Dubin and McFadden [1984] (see Appendix 1.10.9), extending them to account for the fact that I observe a ranked list of multiple alternatives rather than only the most preferred option.

The identification of the selection parameters, ψ_k^s and φ^s, hinges on the variation in preference rankings among students enrolling in the same degree, conditional on covariates. A positive ψ_k^s is inferred if students ranking degree k higher perform beyond expectation based on their observed characteristics in all degrees. A positive φ^s is inferred if these students only perform beyond expectation in degree k.

An additional source of identifying variation comes from an exclusion restriction for distance to colleges—which is included in the choice equation and not in the outcomes equation. This strategy is commonly employed in the literature on the returns to schooling [Card, 1995, Walters, 2018].

Estimation proceeds in two steps. First, I recover parameters of the choice model through maximum likelihood. These are used to construct estimates of the control functions. In a second step, I regress the outcomes of interest on dummies for degrees, interactions with student and peer characteristics, as well as the estimated control functions. This model recovers key parameters of interest, i.e. the treatment effect of each degree on outcomes, by SES.

1.7 Parameter Estimates

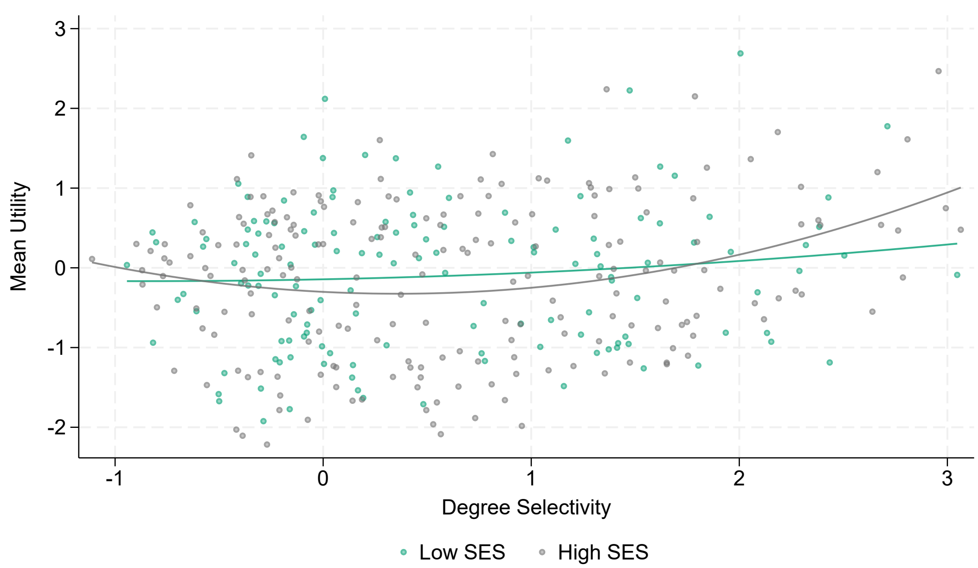
This section presents estimates from the model described above. The sample includes 13 cohorts of applicants who first applied to higher education from 1997 to 2009, and submitted their rank-ordered degree preferences through the centralized application system.[49] This

49. To maximize the size of the estimation sample, I add four additional cohorts to the RDD estimation sample.

amounts to 139,778 individuals who applied to 348 degrees.[50] 49,991 students are from low-SES background (about 35%), which is here defined as individuals with parental income below the median, at time of application. The outcome of interest, Y_i, throughout this analysis is wage income, 12 years post-application.

I begin by presenting the results from the degree choice model. Figure 1.5 plots the school-by-covariate-cell mean utility estimates $\delta_{c(Z_i)j}$ against degree selectivity. These estimates are averaged over covariate cells and residualized with respect to cell indicators. This Figure shows that students from disadvantaged backgrounds have mean utility that is only slightly positively associated with degree selectivity. In contrast, students from high-SES backgrounds seem to exhibit a steeper gradient, particularly at the upper end of the degree selectivity distribution.

Figure 1.5: Mean Degree Utility, by SES



Notes: This figure plots the estimates of degree-specific mean utility against degree selectivity, separately for low- and high-SES students.

I now report and analyze estimates from the outcomes model. The estimates I recover,

50. I restrict the analysis sample to degrees with more than 100 enrolled students.

include the degree-specific effects, α_j, and degree-specific interactions with covariates, β_j. I report the returns relative to the outside option of not enrolling in higher education in the year of application, respectively $\alpha_j - \alpha_0$ and β_j, where α_0 are estimates for the outside option. These are estimated separately for low- and high-SES students.

Table 1.2 reports the mean and standard deviation of these estimates across all degrees j. I find there is substantial variation in causal effects across degrees, in line with previous literature on returns to majors [Hastings et al., 2013, Kirkeboen et al., 2016]. The larger variation in estimates among low-SES students, relative to high-SES students, underscores the significant impact that these choices have on future earnings for disadvantaged students. Moreover, there is important heterogeneity in match effects based on students' characteristics. For instance, holding average treatment effect fixed, a one standard deviation increase in degree match quality for students with higher math grades leads to about $3,000 increase in annual wage income, while this stands at around $2,400 for high-SES students. Finally, estimated parameters from the model reveal evidence of selection on gains, particularly for low-SES students.

Table 1.2: Parameter Estimates

	low-SES		high-SES	
	Mean	SD	Mean	SD
Average Treatment Effect	3037.71	11797.12	-1629.97	7951.95
Male Match Effect	7328.72	6679.70	8033.01	5268.03
Math Grade Match Effect	1504.26	2972.32	1679.34	2345.39
Danish Grade Match Effect	949.61	2861.38	811.66	2382.50
Foreigner Match Effect	-97.66	7389.19	-61.38	4613.88
Parental Income Match Effect	429.62	1115.79	76.21	394.21
Parental Education Match Effect	67.81	969.75	-80.72	799.28
Peer Grades Match Effect	-546.41	7611.90	895.55	5780.67
Preference (ψ_k)	-367.52	1378.66	-65.49	1538.33
Selection (φ)	232.30	-	115.97	-

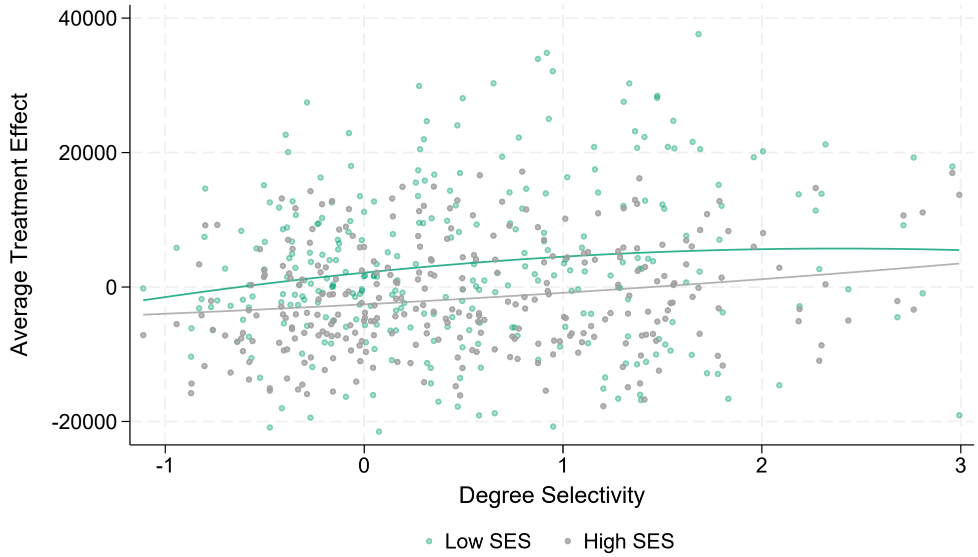
Notes: Estimated means and standard deviations of treatment effects parameters. I report the returns relative to the outside option of not enrolling in higher education in the year of application, respectively $\alpha_j - \alpha_0$ and β_j, where α_0 are estimates for the outside option

To visually illustrate the high dimensional vector of degree-specific ATEs, I plot them against degree selectivity in Figure 1.6. Low-SES returns are depicted in green, while high-SES returns are in gray. Interestingly, for students from disadvantaged backgrounds, the returns are positive across the distribution of degree selectivity. On the other hand, at the lower end of the degree selectivity distribution, degree returns high-SES students are in fact negative. This, in part, reflects the relatively better fallback options available to high-SES students as opposed to low-SES students.

Appendix 1.10.10 provides graphical evidence of the correlation between degree match effects across SES background, showing weak correlations. Figures 1.36f and 1.39f do so for estimates arising from the model and compares them with estimates of Equation 2.5a with OLS, without controlling for selection. Appendix 1.10.10 further studies the relationship between match effects against degree selectivity, separately for high and low-SES students. These figures illustrate the substantial heterogeneity in match effects across the degree selectivity.

To assess the impact of adopting a control function approach to addressing selection, Appendix 1.10.10 provides graphical evidence on the relationship between the estimates from the model compared to estimates recovered without addressing selection. These figures show estimates that do not control for selection overestimate the returns.

Figure 1.6: Average Treatment Effects, by SES



Notes: Relationship between degree-specific average treatment effects and degree selectivity. The latter is measured by averaging over enrolled students' high school GPA and standardized to have mean 0 and standard deviation 1. Low-SES students are depicted in green, while high-SES students are depicted in gray. The treatment effect is relative to the outside option of not enrolling in higher education in the year of application.

1.8 Admission Policies and Degree Sorting

1.8.1 Ex-ante Meritocratic Admission Policies

The model provides a framework to assess the role of current admission practices in driving the large SES wage gap that remains amongst college applicants, despite egalitarian higher education policies. To do so, I consider a set of need-affirmative admission policies to understand the extent to which long-term labor market outcomes of low-SES students can be improved, by departing from current *ex-ante* meritocratic admission policies.[51] The choice of such policy is driven by evidence from the regression discontinuity design that low-SES

51. By *ex-ante* meritocratic, I refer to admission criteria based on past performance, as opposed to *ex-post* meritocratic, which would be based on future returns, coinciding with an efficient allocation in terms of maximizing earnings.

students may benefit more than high-SES students from being admitted into their first-best college degree. Importantly, this model allows to estimate any potential losses from high-SES students who may be displaced from their preferred degrees, through the introduction of the policy.

The objective is to simulate student assignments under any given affirmative action (AA) schedule, utilizing the centralized matching mechanism.[52] This process begins by recovering the key inputs for the matching function $\vartheta(\Theta, q, \pi) = \mu$, where Θ denotes student types, q represents degree capacities and π the AA policy.

First, student types, $\Theta = \bigcup_{i \in \mathcal{I}} \theta_i$, where $\theta_i = (\succ_i, a_{ij}, s_i)$, are recovered. Preferences, $\succ_i$, are obtained by using estimates from the rank-ordered logit model. The remaining components of Θ, such as students' GPA, a_{ij}, and SES status, s_i, are assumed to be invariant to changes in the admission policies. This assumption—namely, the absence of behavioral responses to policy changes—enables me to recover these directly from the data. Degree capacities, $q = \{q_0, \ldots, q_J\}$, are also recovered from the data. Degree capacities are set by the government and are held constant in the simulation. After obtaining $\hat{\Theta}$ and q, I simulate the matching function as $\vartheta(\hat{\Theta}, q, \pi) = \hat{\mu}$.

With the simulated student assignments, I compute the new vector of degree peer characteristics $\bar{X}_i$ and the realized outcomes. For an individual of type θ_i, the expected outcome is given by $\mathbb{E}[\hat{Y}_i(\hat{\mu}(\theta_i)) \mid \theta_i]$, where $\hat{\mu}$ is the estimated matching function.[53] It is specified as follows, by plugging in Equation 1.10:

52. For the counterfactual exercises, I focus on the 2009 application cohort.

53. For each simulation, I draw an η_{ij} from an extreme value type I distribution, repeating this process 100 times and averaging over these iterations.

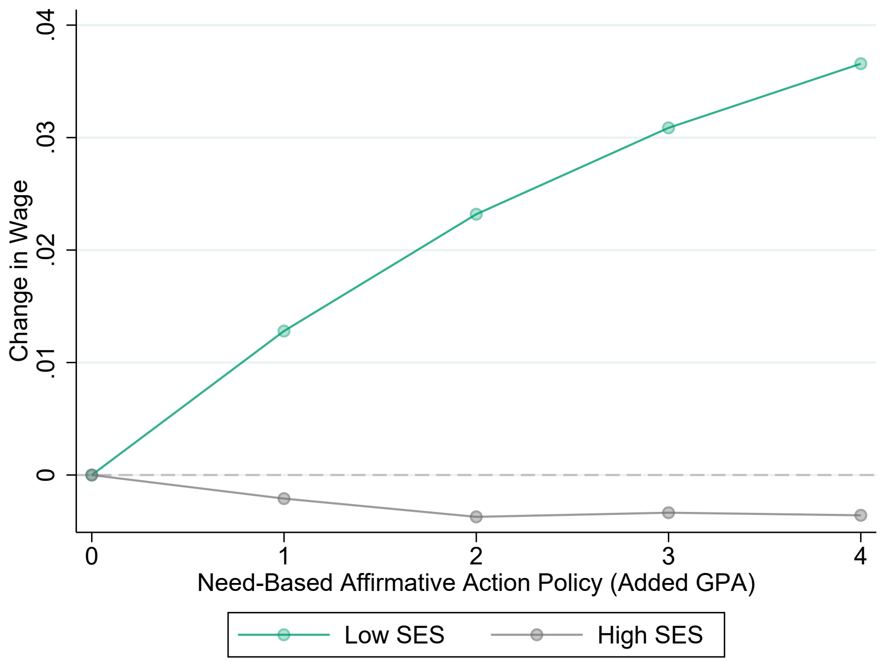
$$
\begin{aligned}
\mathbb{E}\left[\hat{Y}_i\left(\hat{\mu}\left(\theta_i\right)\right) \mid \theta_i\right] &= \sum_j \mathbb{1}\left\{\hat{\mu}\left(\theta_i\right)=j\right\} \times \mathbb{E}\left[\hat{Y}_{ij} \mid \theta_i\right] \\
&= \sum_j \mathbb{1}\left\{\hat{\mu}\left(\theta_i\right)=j\right\} \\
&\times \left(\alpha_j^s + X_i^{\prime}\beta_j^s + \bar{X}_i{}^{\prime}\gamma_j^s + \sum_{k=0}^{J} \psi_k^s \times \left(\eta_{ik}-\mu_\eta\right) + \varphi^s \times \left(\eta_{ij}-\mu_\eta\right)\right)
\end{aligned}
$$

This parametrization relies on the assumption that potential outcomes are unaffected by changes in the admission practices. Importantly, it accounts for the role of peer composition in the production function of degrees, as students are being reallocated to different degrees.

I consider policies that increase the probability of admission to the preferred degrees of low-SES students. I do so by gradually increasing the GPA of low-SES, a_{ij}, from 1 to 4 additional points. This is equivalent to reducing the GPA cutoff for low-SES students.[54]

54. Recall that in this period, the Danish grading scale ranges from -3 to 12.

Figure 1.7: Long-Term Returns of Need-Affirmative Admission Policy on “Winners" and “Losers"



Notes: Effect of need-affirmative admissions on low-SES students (depicted in green) and high-SES students (depicted in gray). The policy under consideration reduces the GPA requirement gradually between 1 and 4 points. Each dot represent the average change in wage as a result of implementing the policy.

Theoretically, while low-SES students benefit by being placed into better degrees, high-SES students may be harmed by being placed into lower return degrees, due to capacity constraints in their preferred choices. Moreover, low-SES students may themselves be harmed, particularly those away from discontinuities, for whom the effects may be different than the LATEs recovered by the RDD. Finally, changes in degree peer composition may also affect outcomes. The model and simulations I conduct provide a framework to account for these three aspects.

Figure 1.7 presents estimates of changes in returns as need-affirmative admissions become more generous toward low-SES students. Students from disadvantaged backgrounds experience increases in their wage, 12 years after application, of around 4%, while students from high-SES backgrounds experience only slightly wage declines. These estimates build directly on the model developed earlier, which links student preferences, degree-specific returns, and the equilibrium effects of affirmative action policies. By simulating counterfactual outcomes under different policy scenarios, the model allows for an analysis of how need-affirmative admissions impacts the distribution of students across degrees and affects long-term outcomes. Figures in Appendix 1.10.11 shows the distributional consequences of this policy, by SES status.

A key takeaway from this section is that *ex-ante* meritocratic student assignments, based solely on past academic performance, may result in inefficiencies, in terms of maximizing later life earnings in Denmark. Consistent with the quasi-experimental evidence discussed earlier, the model shows that low-SES students derive significant benefits from more generous affirmative action policies. This result highlights the potential for low-SES students to succeed in more selective degree. The findings suggest that academic metrics like GPA may not fully capture the potential of these students in selective educational environments. These findings underscore the limitations of high school GPA as a predictor of long-term outcomes, especially for low-SES students. This contribution builds on a rich body of literature examining the range of skills that influence later-life outcomes, demonstrating that achievement relies on a broader set of skills beyond those captured by GPA [Heckman et al., 2006a, Borghans et al., 2008, 2016, Heckman et al., 2021].

This insight aligns with the theoretical framework developed by Sethi and Somanathan [2019, 2022], which posits that academic grades, particularly for low-SES students, may serve as a noisy signal of true ability. The framework suggests that not only may the variance in grades be higher for low-SES students, but these signals could also be systematically biased, underestimating the true capability of disadvantaged students. As a result, admission policies that rely heavily on these signals can lead to misallocations, where high-potential low-SES students are undermatched in less selective degrees.

By reallocating low-SES students into more selective degrees, the policy significantly boosts their future earnings potential and promotes social mobility. Notably, the model also highlights that high-SES students displaced by these policies do not experience any negative impact on their long-term returns.

1.8.2 Degree Choice and First-Best Allocation

Changing admission practices could reduce the SES wage gap by about 15%, as shown in Figure 1.41 of Appendix 1.10.11. Still, significant wage inequality remain. In this section, I study whether disparities between low- and high-SES students could be addressed by students ranking degrees by expected returns. This allocation would coincide with the first-best allocation with respect to labor market earnings, 12 years after application.

To that end, I decompose the outcome into three components, by adding and subtracting Y_{i0} from Equation 2.5a to get:

$$Y_{ij} = \underbrace{\alpha_0^s + X_i^{'}\beta_0^s + \bar{X}^{'}\gamma_0^s + \varepsilon_{i0}^s}_{Y_{i0}^s} + \underbrace{(\alpha_j^s - \alpha_0^s)}_{\text{ATE}_j^s} + \underbrace{X_i^{'}(\beta_j^s - \beta_0^s) + \bar{X}^{'}(\gamma_j^s - \gamma_0^s) + (\varepsilon_{ij}^s - \varepsilon_{i0}^s)}_{M_{ij}^s} \tag{1.12}$$

ATE_j^s denotes average treatment effect for each degree, averaged over the whole population, while M_{ij}^s denote match effects relative to the outside option.

I consider a counterfactual scenario where students submit rank-ordered list, ordered by the returns offered by each degree, based on their own characteristics. Returns here are computed as the sum of the degree average treatment effect, ATE_j and the individual-specific match effect, M_{ij}. This would correspond to an *ex-post meritocratic* allocation of students to degrees, one that is based on predictors of success, rather than past achievements.

Table 1.3 presents the results, disaggregated by SES background, revealing significant differences in labor market returns based on degree choices. Both low- and high-SES students

Table 1.3: First-best Allocation

	low-SES		high-SES	
	ATE (1)	Match (2)	ATE (3)	Match (4)
Choice 1	36,660	17,522	25,963	13,486
Choice 2	36,079	14,463	25,709	10,816
Choice 3	35,855	12,747	25,256	9,498
Choice 4	35,772	11,233	24,990	8,479
Choice 5	35,403	10,186	24,838	7,533
Choice 6	35,150	9,240	24,608	6,243
Choice 7	34,930	8,422	24,319	6,243
Choice 8	34,678	7,726	23,986	5,888

Notes: The table reports rankings of degrees based on ATE and match effects, and associated returns relative to the outside option. All values are in USD.

experience considerable earnings gains by enrolling in their first-choice degree, but the effect is notably larger for low-SES students. Specifically, the average treatment effect of attending a first-choice degree is approximately $26,000 for high-SES students, compared to nearly $37,000 for low-SES students. Similarly, match effects—capturing the alignment between student characteristics and degree effects—are substantial, amounting to $17,500 for low-SES students and $13,500 for high-SES students. These figures suggest that, with a ranking of degrees based on labor market returns, the SES wage gap could be fully eliminated. This is demonstrated by the fact that the $14,500 difference in returns exceeds the SES wage gap of around $10,000.[55]

These findings raise important questions about the factors influencing students' degree choices. Despite the large labor market returns associated with certain degrees, many students—particularly from low-SES backgrounds—leave significant labor market gains on the table by not prioritizing degrees with the highest returns. Importantly, this analysis does not imply that students are choosing degrees irrationally. It could well be that students value other attributes of degrees, including field of study, specific universities and location,

55. This does not account for general equilibrium effects arising from changes in the supply of skills.

as well as other life outcomes, including non-pecuniary aspects. Information constraints or heterogeneity in preferences could further explain these patterns.[56] Recent evidence also suggests that neighborhood and school peers, along with siblings, play a significant role in influencing both major selection and college attendance decisions [Brenøe and Zölitz, 2020, Altmejd et al., 2021, Cattan et al., 2023, Eshaghnia et al., 2023a].

1.9 Conclusion

This paper develops a framework to evaluate the potential of higher education as a driver of upward mobility. The findings reveal that, although higher education can promote substantial mobility, disadvantaged students do not fully harness its potential. Degree sorting and admission policies restrict mobility more than the causal returns to selective degrees. The current GPA-based meritocratic admissions system is inefficient in terms of maximizing long-term earnings. By conducting this analysis in Denmark—where financial barriers are largely absent—this study isolates these sources of inequality from the effects of financial constraints.

Using rich administrative data from Denmark and the features of the centralized admissions system, I begin by estimating the labor market returns to degree selectivity for students at admission cutoffs. The findings indicate that low-SES students benefit substantially from admission and enrollment to more selective degrees, with cutoff-crossing students experiencing a 9% increase in earnings twelve years after college application. Additional analyses on various socioeconomic outcomes—including educational attainment, family structure, and criminal behavior—reveal limited impacts by SES backgrounds, except for increased fertility among both low- and high-SES students. To probe the mechanisms behind these heterogeneous returns, I employ two quasi-experimental methods to provide suggestive evidence of

56. A growing literature studies determinants of major choices, including Zafar [2013], Stinebrickner and Stinebrickner [2014], Wiswall and Zafar [2014, 2015], Patnaik et al. [2022]. See Patnaik et al. [2021] for a recent review.

differential peer effects.

In addition to documenting sizable returns to degree selectivity at the margin, I develop a joint model of degree choice and earnings, incorporating peer effects, to examine the drivers of Denmark's SES wage gap. This model enables the estimation of selection-corrected, degree-specific returns and assesses the roles of admission policies and student degree sorting in shaping labor market inequalities.

Evidence from the model reveals that degree returns are larger for low-SES students relative to high-SES peers, across the distribution of degree selectivity. The results further indicate that need-affirmation admission policies could improve labor market outcomes for low-SES students without adversely impacting displaced high-SES students. Expanding access to selective degrees for low-SES students narrows the SES wage gap by approximately 15%. These findings highlight the limitations of high school GPA as a predictor of long-term outcomes, particularly for low-SES students—long-term outcomes depend on a broader range of skills beyond those measured by GPA. Crucially, if disadvantaged students prioritized degree choices based on expected returns, the wage gap could be entirely closed. Overall, this evidence reveals that the observed disparity in outcomes is driven by differences in degree sorting and admission policies, rather than limited degree returns. Future research into the underlying determinants of students' degree choices may offer further insights into observed sorting patterns and addressing persistent inequality.

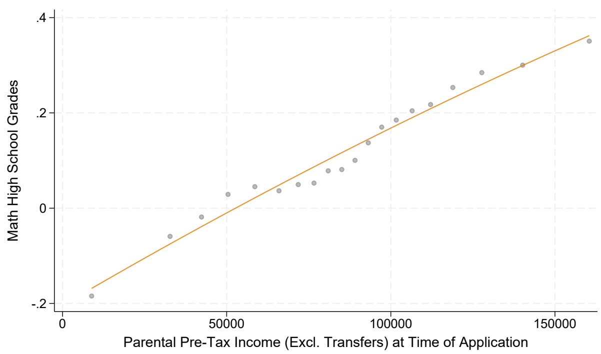
1.10 Appendix

1.10.1 Inequalities in Universal Higher Education System

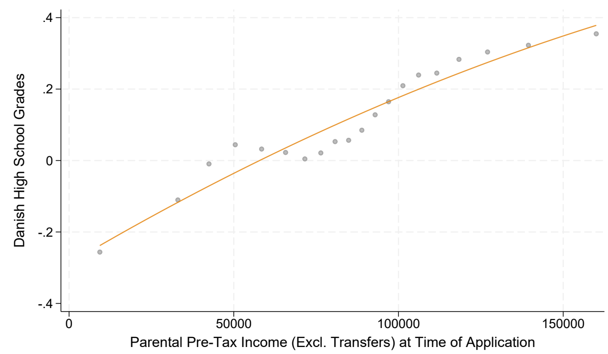
This Appendix provides further evidence on inequalities in Denmark, by parental income and education. Section 1.10.1 focuses on inequalities in skills as measured by high school grades, with a particular focus on grades in mathematics and danish.

Skill Differences

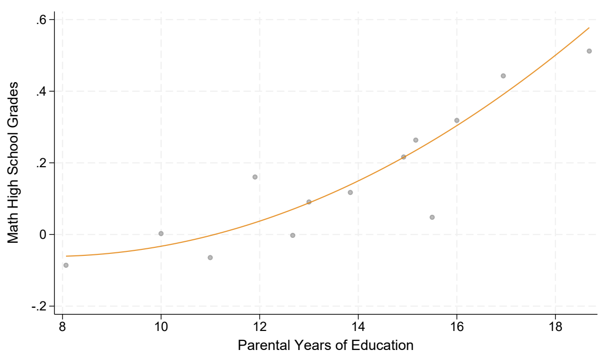
Figure 1.8: High School Grades and Parental Background



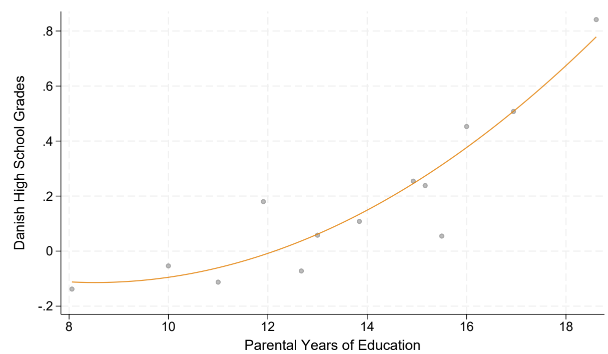
(a) Mathematics High School Grades and Parental Income



(b) Danish High School Grades and Parental Income



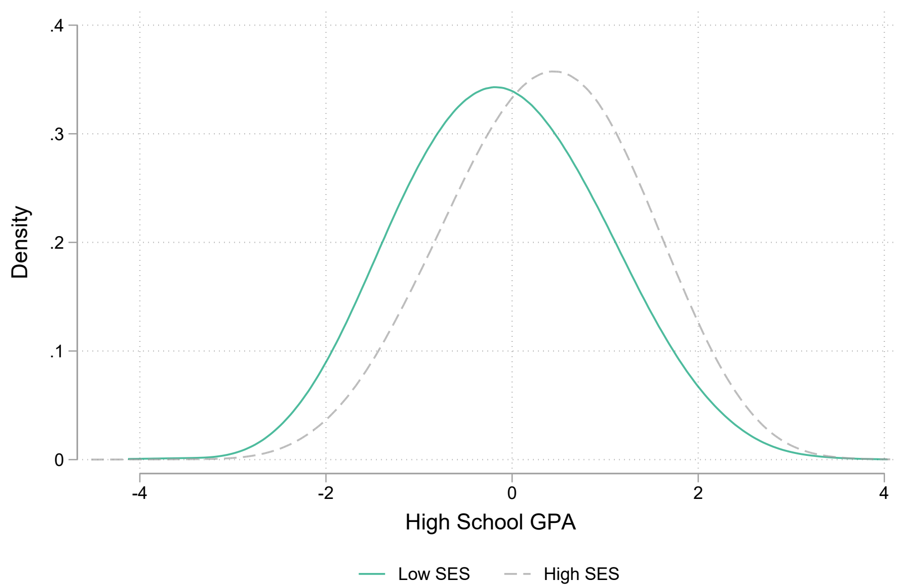
(c) Mathematics High School Grades and Parental Education



(d) Danish High School Grades and Parental Education

Notes: Relationship between student skills at time of application, proxied by mathematics and danish high school grades and parental characteristics. Panels (a) and (b) focus on the relationship between mathematics and danish high school grades, respectively, and parental pre-tax income (excluding transfers) at time of application. Panels (c) and (d) focus on the relationship between mathematics and danish high school grades, and parental education.

Figure 1.9: Distribution of High School Grades



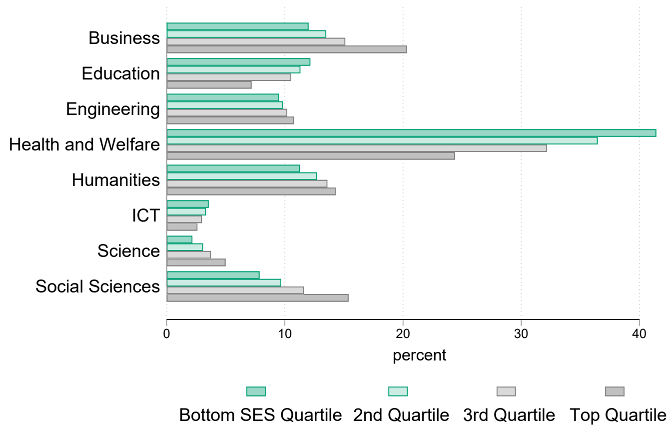
Notes: Densities of high school grades by SES background.

Degree Portfolio Composition

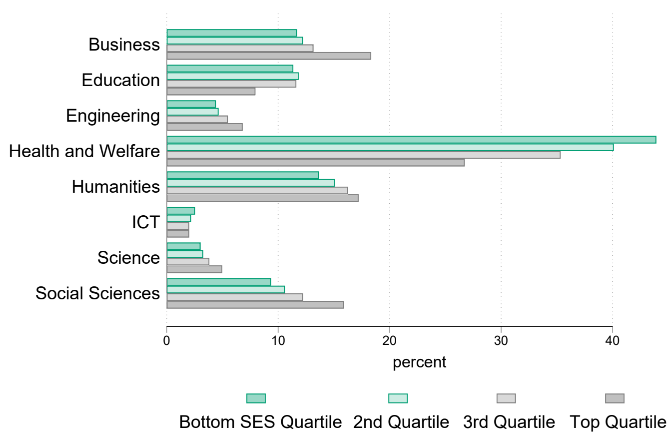
This Appendix provides evidence on differences in degree portfolio composition of students.

Field of Study: Top-Ranked and Second-Best

Figure 1.10: Choice of Field of Study, by SES



(a) Field of Study in Top-Ranked Choice, by SES



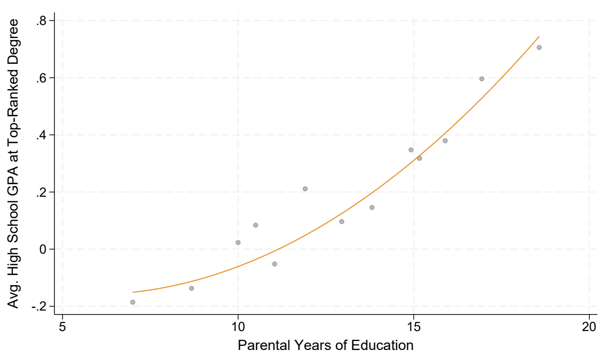
(b) Field of Study in Second-Best Choice, by SES

Notes: Panel (a) and (b) describe the application patterns by SES background, respectively for the top-ranked alternative in students rank-ordered list and the second-best alternative. The figures report the shares of students applying to a given field of study, by SES background.

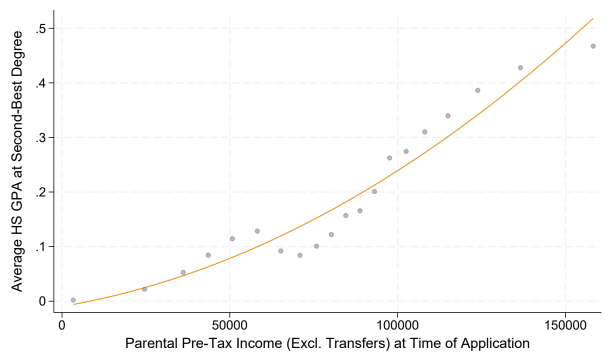
Degree Quality: Top-Ranked and Second-Best

This Section provides additional facts about the quality of degrees that students are ranking, by parental education and income. Degree quality is measured by the average high school grades of incoming students. I distinguish between the quality of the top-ranked alternative and the second-best. In particular, Figure 1.11c and Figure 1.11d show that across the distribution of high school grades, the positive gradient between degree quality and parental SES background remains.

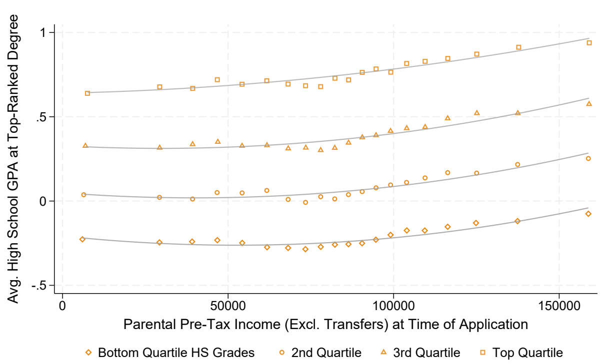
Figure 1.11: Top-Ranked and Second-Best Degree Quality



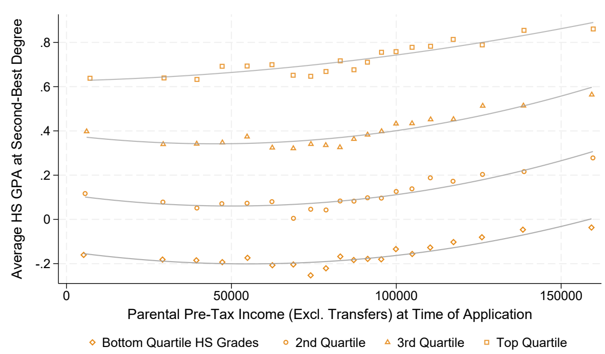
(a) Degree Average Grades and Parental Education



(b) Degree Average Income and Parental Income



(c) Degree Average Grades and Parental Education, by Grades Quartile



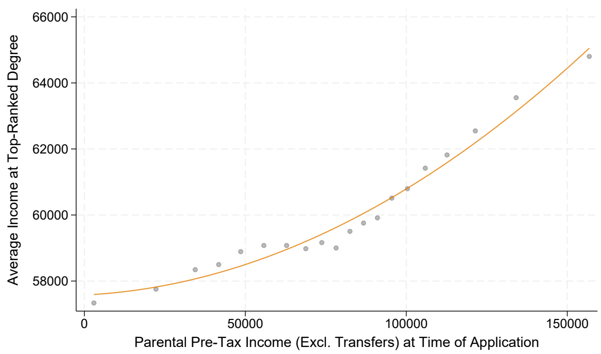
(d) Degree Average Income and Parental Income, by Grades Quartile

Notes: Panel (a) plots the average high school GPA of incoming students at the *top-ranked* degree chosen, against mother's years of education. Similarly, panel (b) describes the relationship between average high school grades of incoming students at the *second-best* alternative ranked by students, against parental pre-tax income. Panel (c) and (d) plot these measures of degree quality against parental pre-tax income, by quartiles of high school grades.

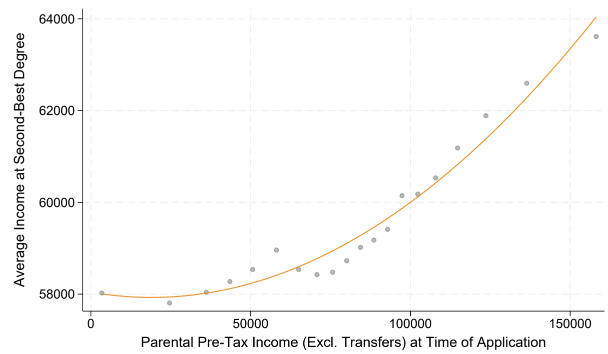
Degree Quality: Top-Ranked and Second-Best

This Section reproduces the analysis of the previous Section, measuring degree quality by the average income of individuals having completed a given degree. The income for these individuals is averaged over individuals aged between 30 to 50 years old. The positive gradient between degree quality and parental background remains (see Figures 1.12a and 1.12b), even after controlling for students' grades, as seen in Figures 1.12c and 1.12d.

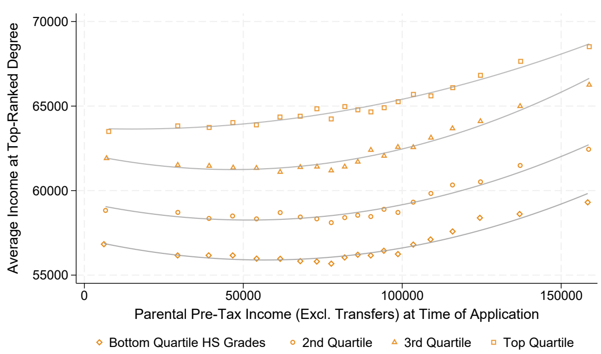
Figure 1.12: Top-Ranked and Second-Best Degree Quality, Income



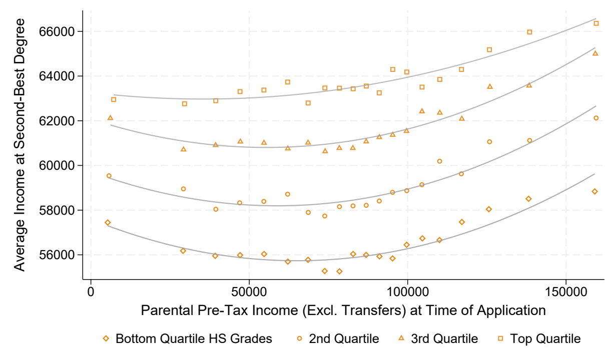
(a) Degree Average Income and Parental Income (Top-Ranked)



(b) Degree Average Income and Parental Income (Second-Best)



(c) Degree Average Income and Parental Income, by HS Grades Quartile (Top-Ranked)



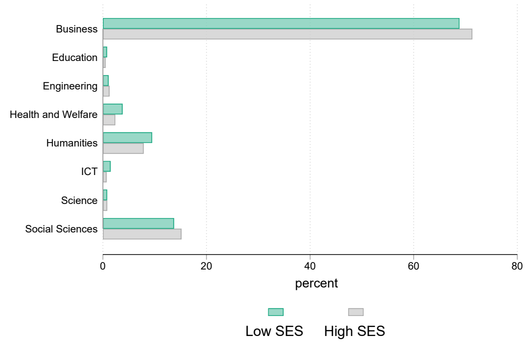
(d) Degree Average Income and Parental Income, by HS Grades Quartile (Second-Best)

Notes: Panel (a) plots the average income of students having completed the *top-ranked* degree chosen, against mother's years of education. Similarly, panel (b) describes the relationship between average income of students having completed the *second-best* alternative ranked by students, against parental pre-tax income. Panel (c) and (d) plot these measures of degree quality against parental pre-tax income, by quartiles of high school grades.

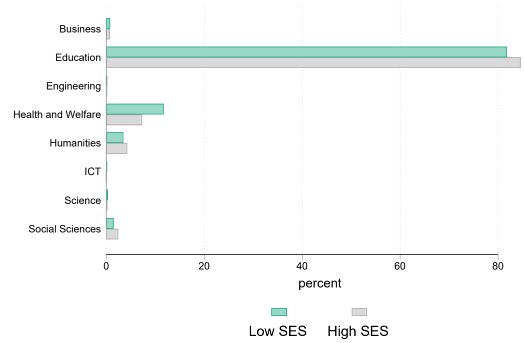
Transitions Across Fields: Top-Ranked and Second-Best

In this Appendix, I describe the relationship between the top-ranked alternative and second-best, in terms of the field of study chosen. The series of Figures presented showcase that across SES background, the field of study chosen remains largely constant between the first and second-best alternative.

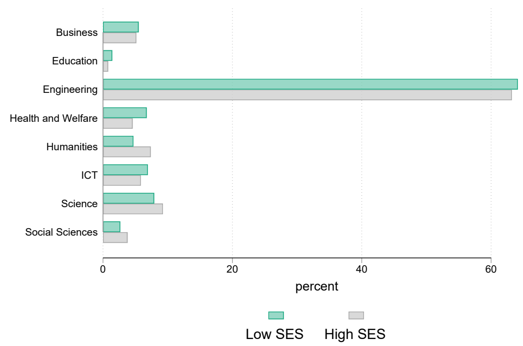
Figure 1.13: Choice of Field of Study as Second-Best



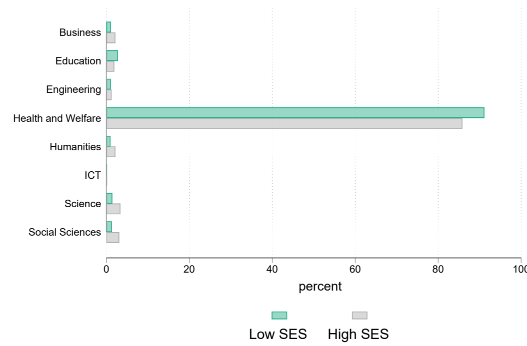
(a) Second-Best Field, Conditional on Business as Top-Ranked



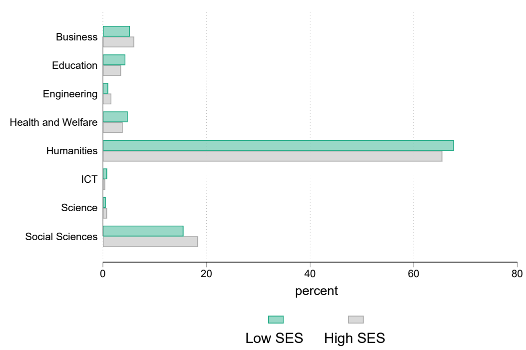
(b) Second-Best Field, Conditional on Education as Top-Ranked



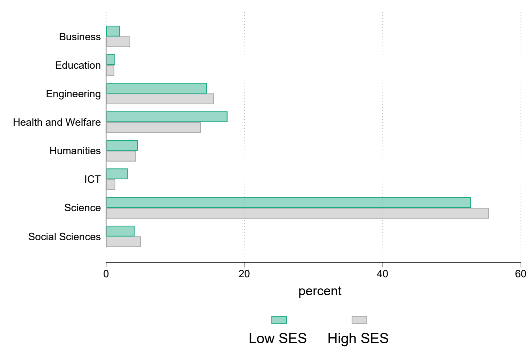
(c) Second-Best Field, Conditional on Engineering as Top-Ranked



(d) Second-Best Field, Conditional on Health as Top-Ranked



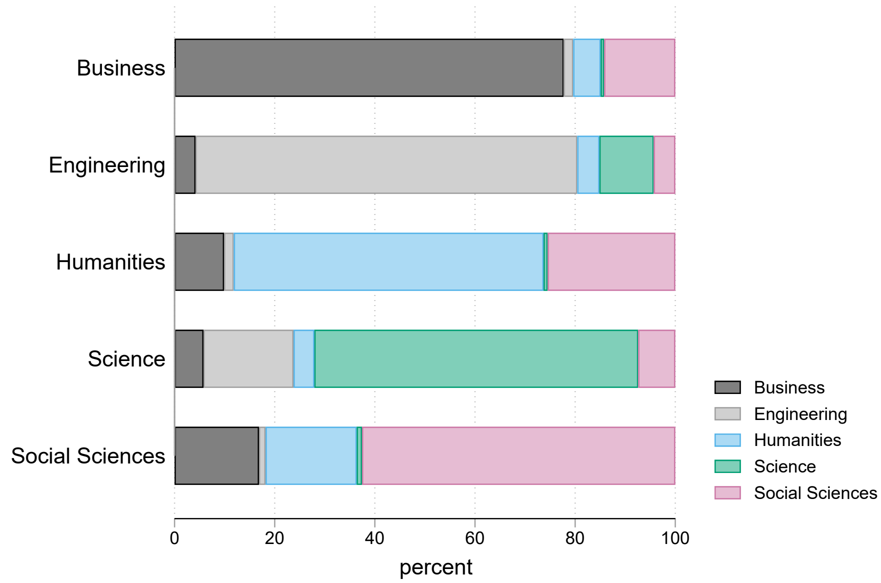
(e) Second-Best Field, Conditional on Humanities as Top-Ranked



(f) Second-Best Field, Conditional on Science as Top-Ranked

Notes: Each panel describes the percent of students choosing a field of study as second-best, conditional on a choice of first-best field, as specified in the sub-figure title.

Figure 1.14: Transition Matrix: Top-Ranked and Second-Best

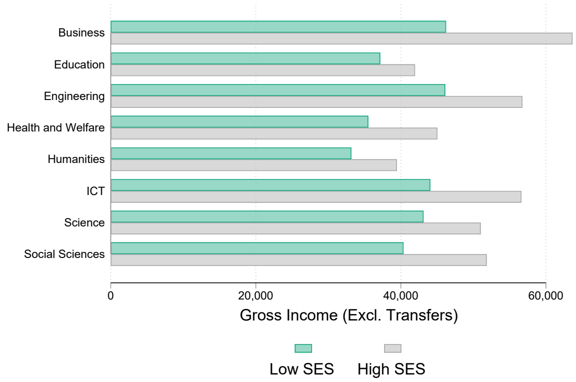


Notes: The figure depicts the transition between top-ranked and second-best alternatives in terms of field of study.

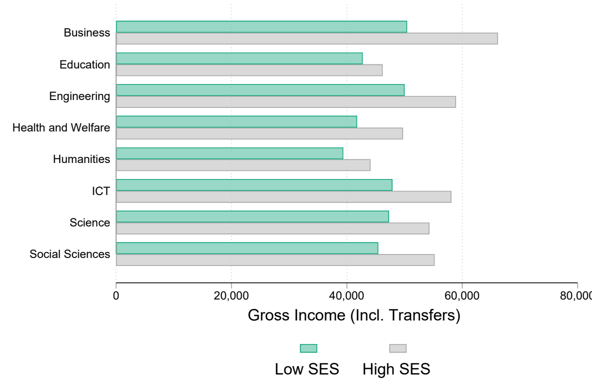
Later Life Outcomes Differences

To further study raw differences in outcomes, this Appendix depicts a number of later life outcomes, by SES background.

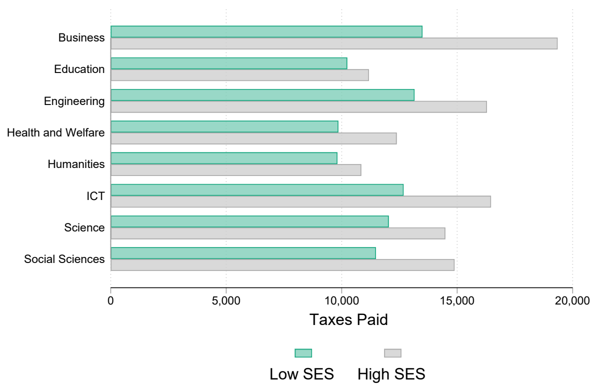
Figure 1.15: Outcomes Differences



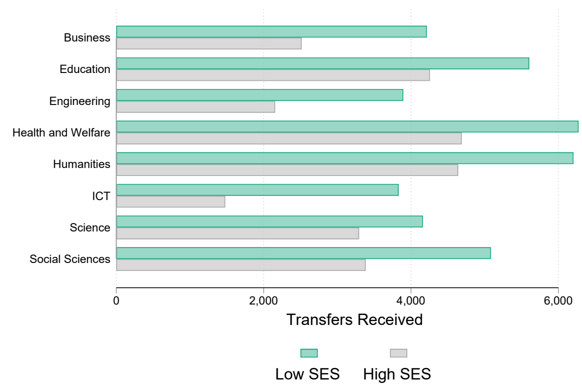
(a) Pre-Tax Income (Excluding Transfers)



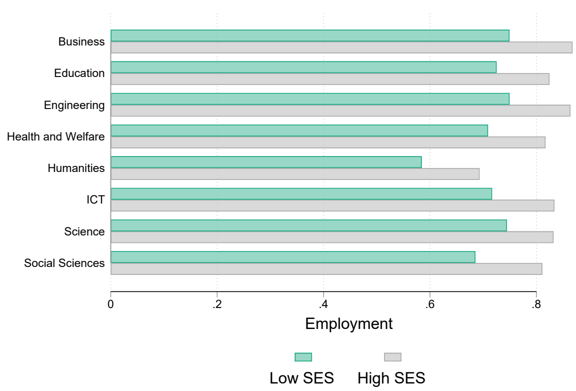
(b) Pre-Tax Income



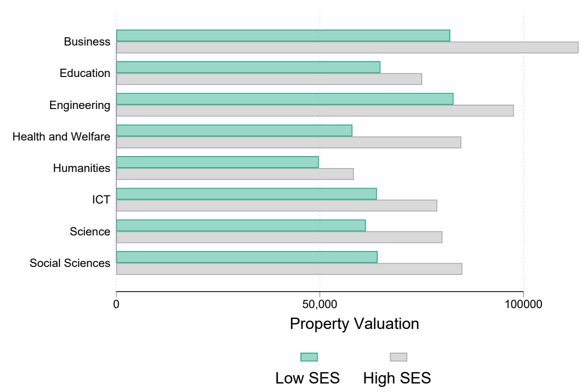
(c) Taxes



(d) Transfers



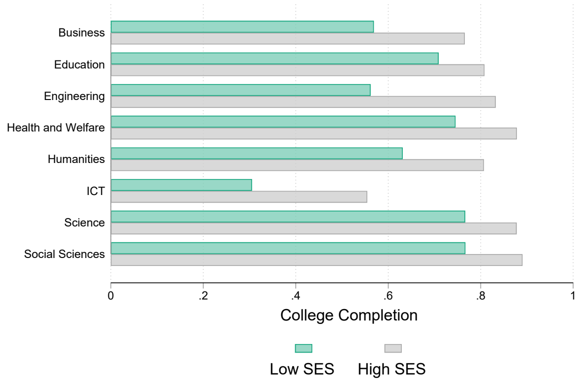
(e) Employed



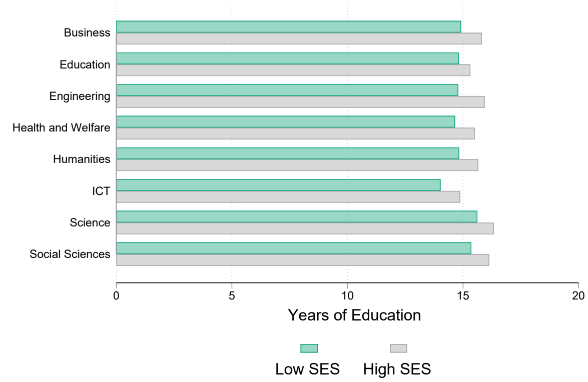
(f) Property Value

Notes: Each panel of this figure provides raw means of a given later life economic outcome by SES, as described in the title of the sub-figure.

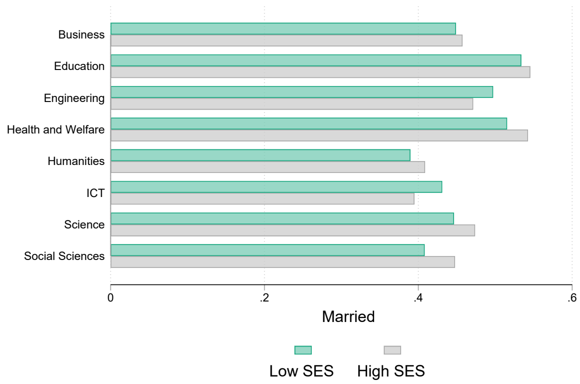
Figure 1.16: Outcomes Differences



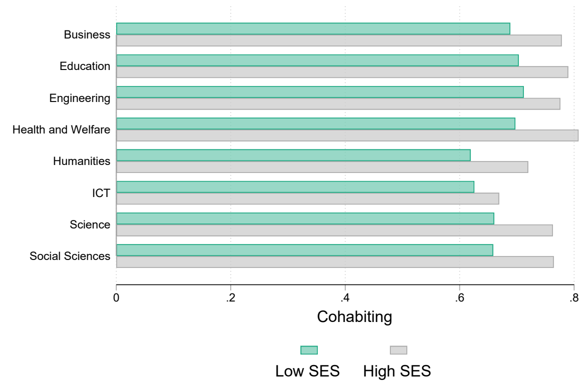
(a) College Completion



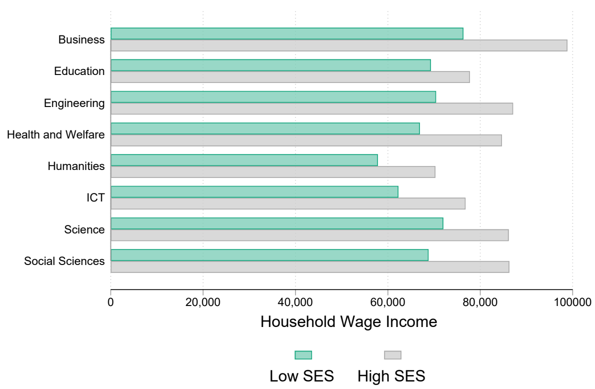
(b) Years of Education



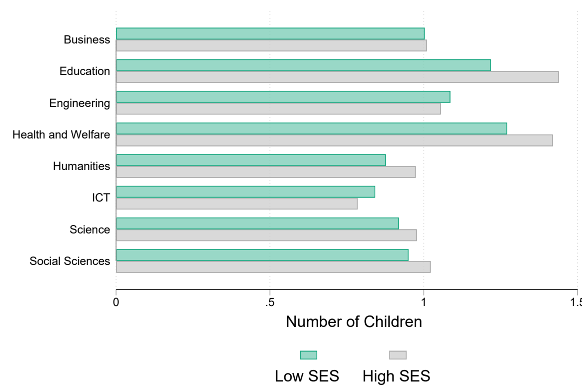
(c) Marital Status



(d) Cohabitation



(e) Household Income



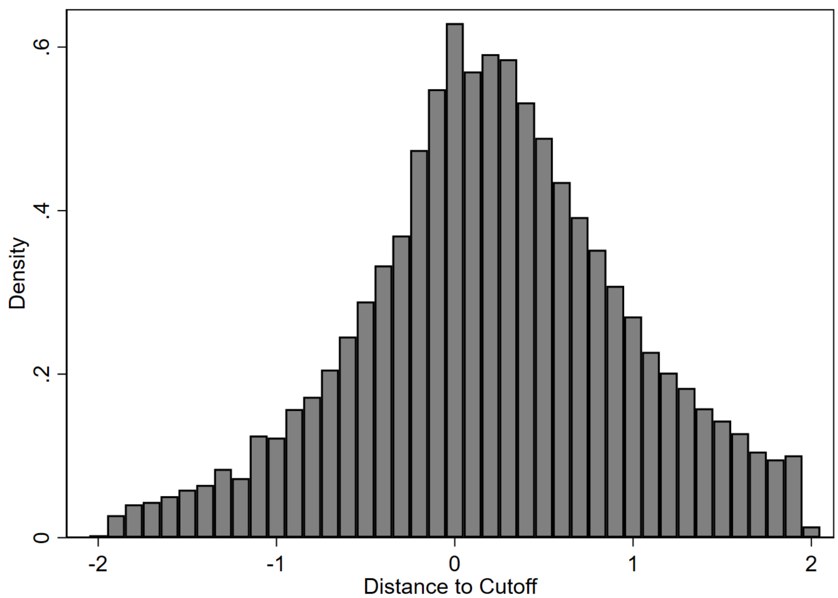
(f) Number of Children

Notes: Each panel of this figure provides raw means of a given education and household formation outcome by SES, as described in the title of the sub-figure.

1.10.2 Validation of RDD

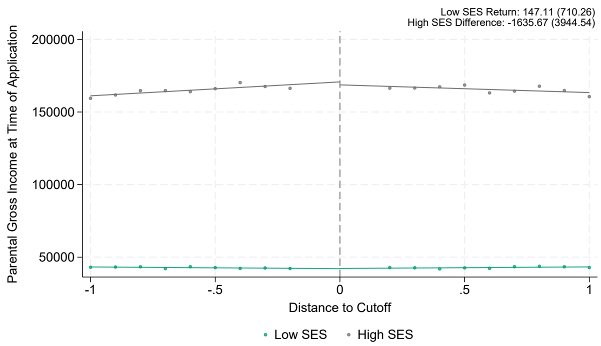
This appendix provides evidence on the validity of the RDD.

Figure 1.17: Histogram of the Running Variable

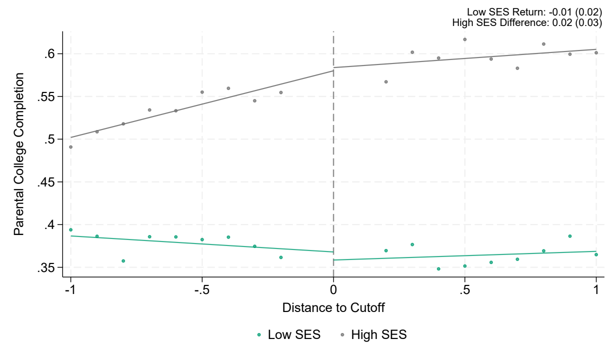


Notes: This figure provides a histogram of the discrete running variable.

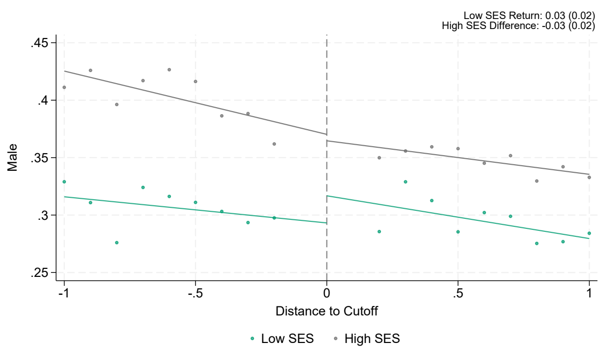
Figure 1.18: Continuity of Pre-Treatment Variables



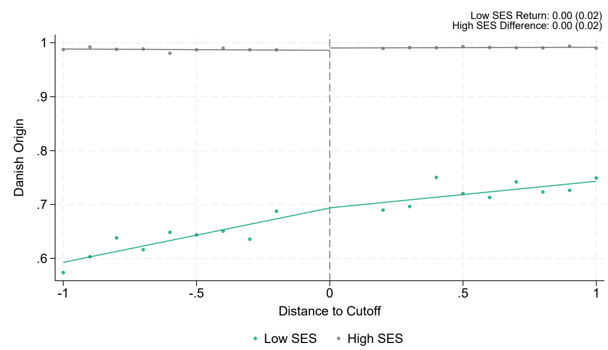
(a) Parental Income



(b) Parental College Attainment



(c) Gender



(d) Origins

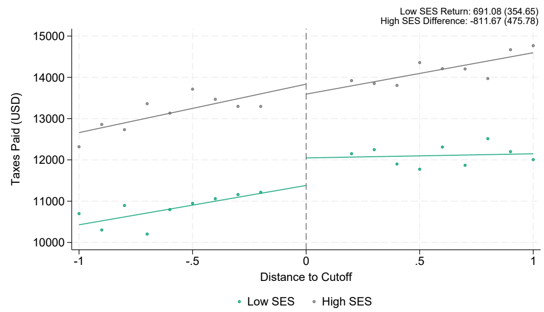
Notes: This figure provides a test of the continuity of pre-treatment variables across the cutoff. The figure depicts the effect for low-SES students (depicted in green) and high-SES students (depicted in gray). Coefficients and standard errors are reported in the top-right corner. "low-SES return" refer to the effect for low-SES students, while "high-SES difference" reports the difference in the returns between high- and low-SES students ($\tau^1 - \tau^0$).

1.10.3 Other Outcomes

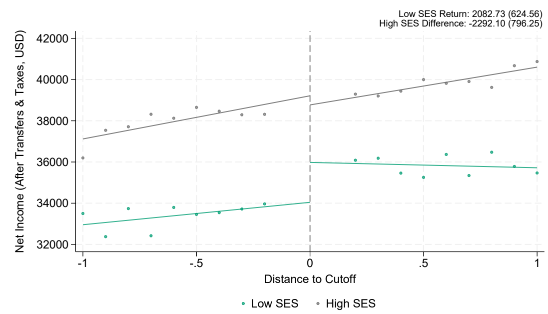
This Appendix provides additional results on the returns of cutoff-crossing on economic outcomes, household structure and criminal behavior.

Economic Outcomes

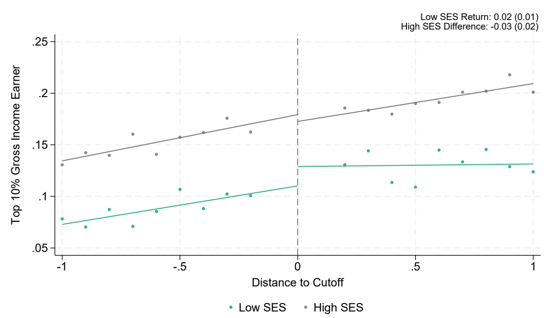
Figure 1.19: Other Economic Outcomes



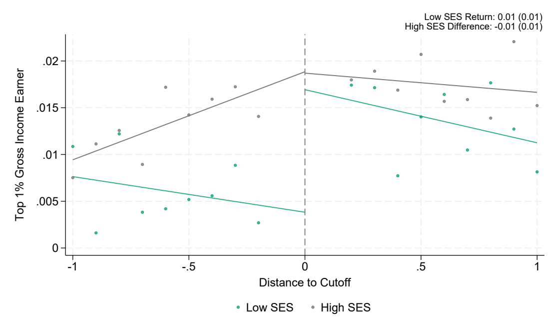
(a) Taxes Paid, 12 years post-application



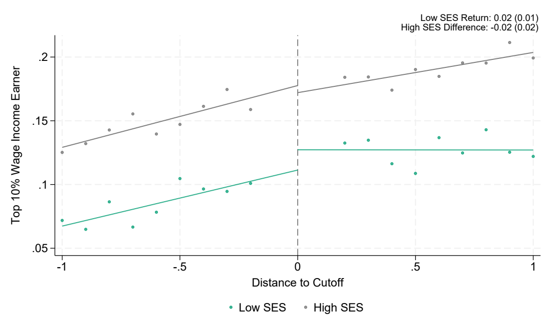
(b) Income, Incl. Taxes and Transfers, 12 years post-application



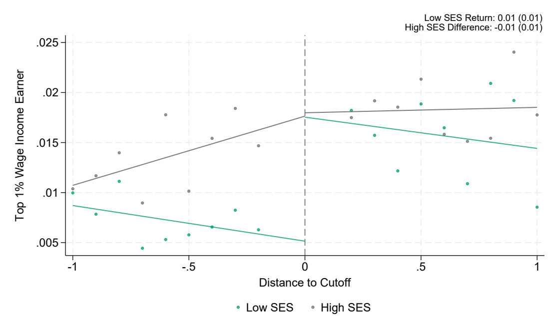
(c) Probability of Joining Top 10% Gross Income Earner, 12 years post-application



(d) Probability of Joining Top 1% Gross Income Earner, 12 years post-application



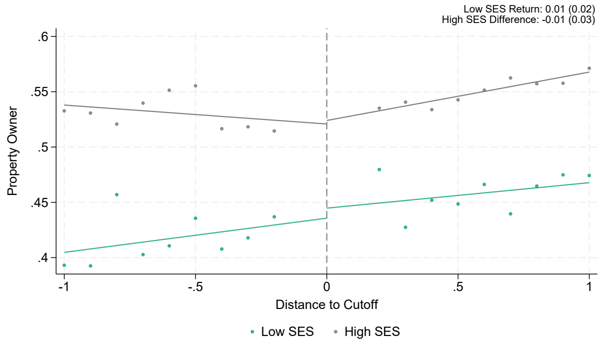
(e) Probability of Joining Top 10% Wage Income Earner, 12 years post-application



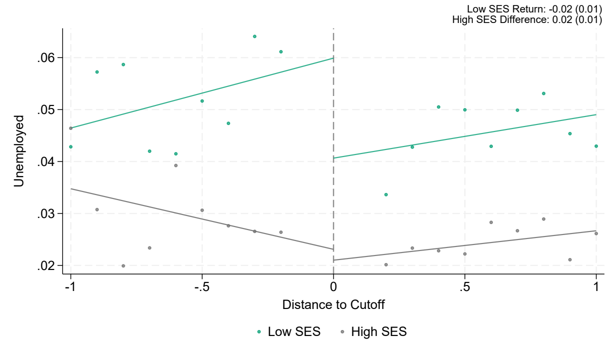
(f) Probability of Joining Top 1% Wage Income Earner, 12 years post-application

Notes: This figure presents evidence on the impact of cutoff-crossing on economic outcomes, as specified by each sub-figure title. Results are depicted separately for low-SES students (in green) and high-SES students (in gray). The top-right corner of each panel displays the estimated coefficients along with their standard errors. The term 'Low-SES return' denotes the effect for low-SES students, while 'High-SES difference' represents the estimated difference in effects between high- and low-SES students ($\tau^1 - \tau^0$). Standard errors clustered at the individual level are in parentheses.

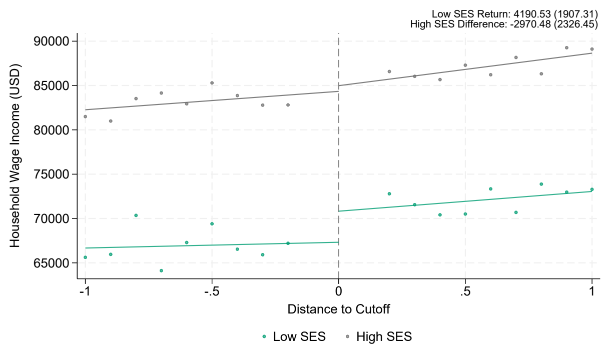
Figure 1.20: Other Economic Outcomes



(a) Homeowner, 12 years post-application



(b) Unemployed, 12 years post-application

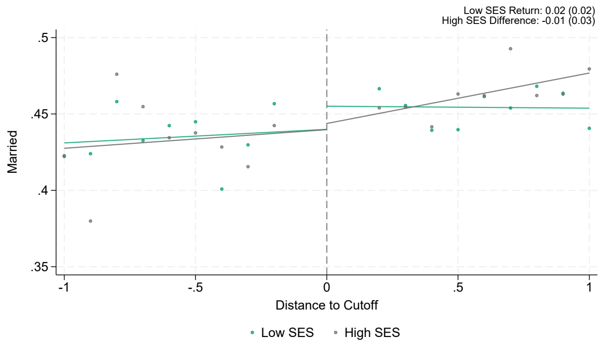


(c) Household Income, 12 years post-application

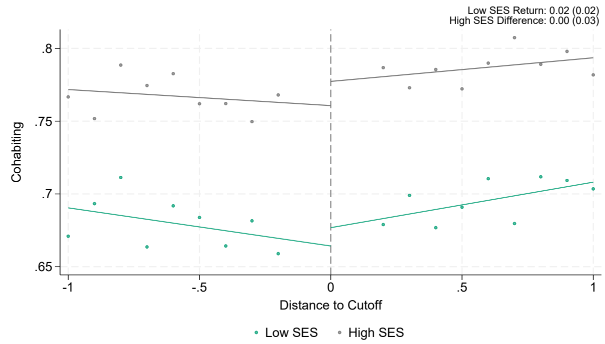
Notes: This figure presents further evidence on the impact of cutoff-crossing on economic outcomes, as specified by each sub-figure title. Results are depicted separately for low-SES students (in green) and high-SES students (in gray). The top-right corner of each panel displays the estimated coefficients along with their standard errors. The term ‘Low-SES return’ denotes the effect for low-SES students, while ‘High-SES difference’ represents the estimated difference in effects between high- and low-SES students ($\tau^1 - \tau^0$). Standard errors clustered at the individual level are in parentheses.

Household Structure and Family Formation

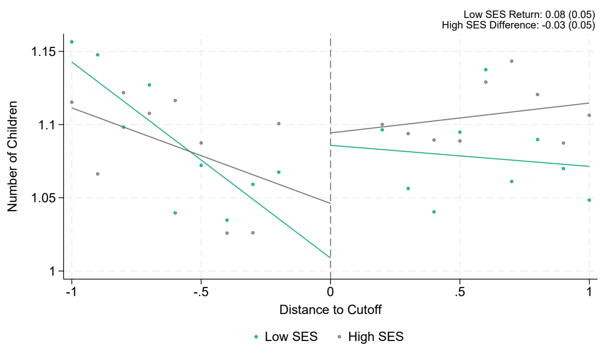
Figure 1.21: Household Outcomes



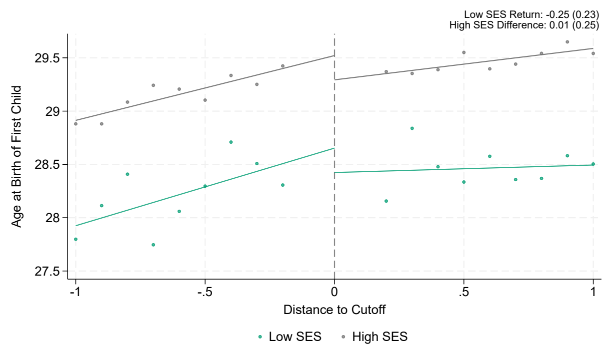
(a) Marital Status, 12 years post-application



(b) Cohabitation, 12 years post-application



(c) Number of Children, 12 years post-application

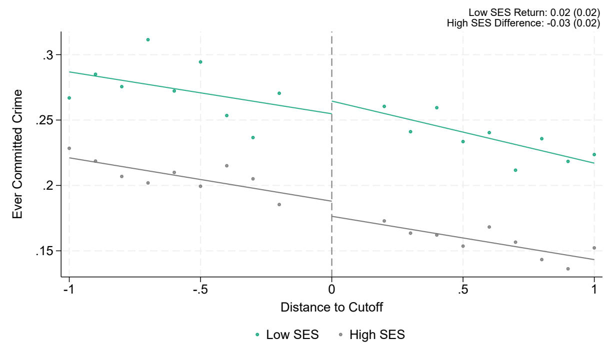


(d) Age at Birth of First Child, 12 years post-application

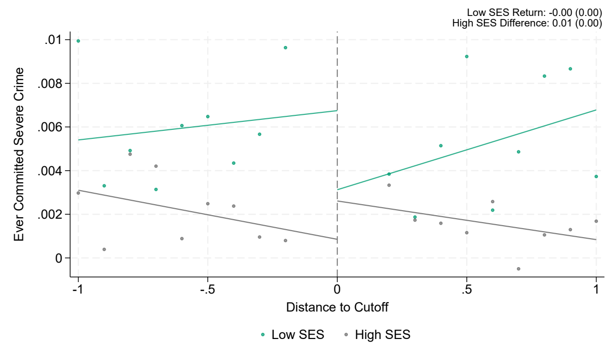
Notes: This figure presents evidence on the impact of cutoff-crossing on household structure and family formation, as specified by each sub-figure title. Results are depicted separately for low-SES students (in green) and high-SES students (in gray). The top-right corner of each panel displays the estimated coefficients along with their standard errors. The term 'Low-SES return' denotes the effect for low-SES students, while 'High-SES difference' represents the estimated difference in effects between high- and low-SES students ($\tau^1 - \tau^0$). Standard errors clustered at the individual level are in parentheses.

Criminal Behavior

Figure 1.22: Crime Outcomes



(a) Ever Committed Crime, 12 years post-application



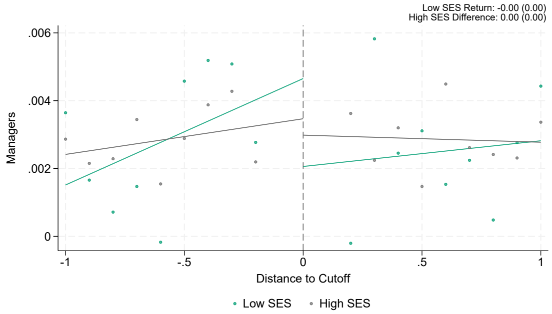
(b) Severe Crime, Incl. Taxes and Transfers, 12 years post-application

Notes: This figure presents evidence on the impact of cutoff-crossing on criminal behavior, as specified by each sub-figure title. Results are depicted separately for low-SES students (in green) and high-SES students (in gray). The top-right corner of each panel displays the estimated coefficients along with their standard errors. The term 'Low-SES return' denotes the effect for low-SES students, while 'High-SES difference' represents the estimated difference in effects between high- and low-SES students ($\tau^1 - \tau^0$). Standard errors clustered at the individual level are in parentheses.

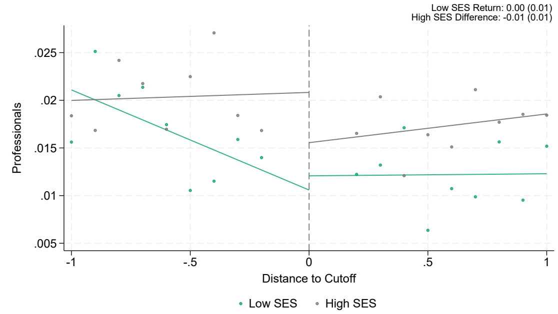
1.10.4 Occupational Sectors

This Appendix provides additional results on the returns of cutoff-crossing on occupational sector.

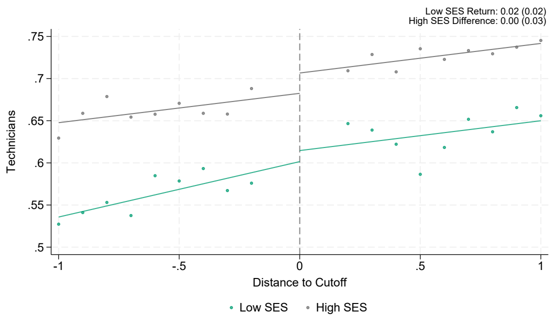
Figure 1.23: Occupations



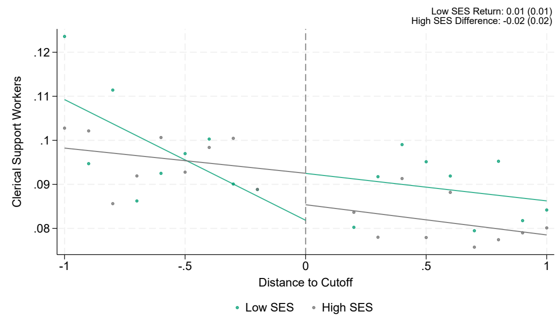
(a) Managers



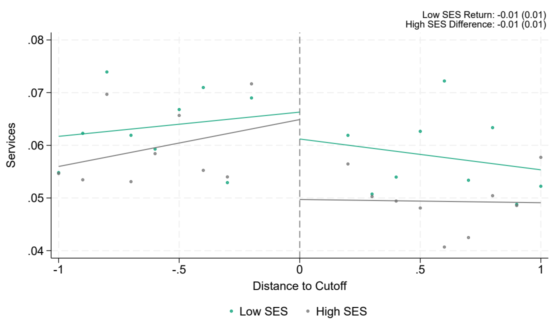
(b) Professionals



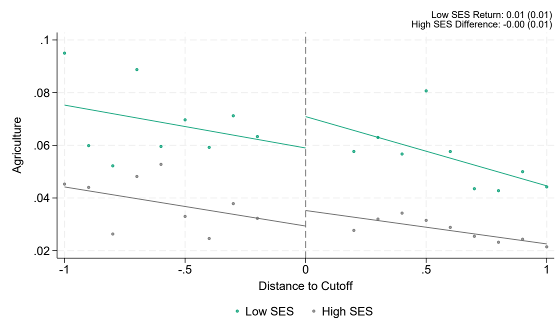
(c) Technicians



(d) Clerical Support Workers



(e) Services



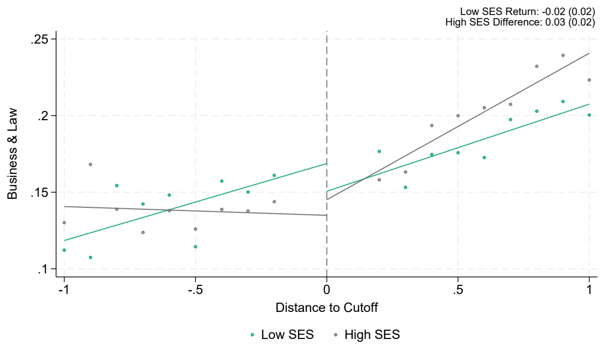
(f) Agriculture

Notes: This figure presents evidence on the impact of cutoff-crossing on occupation choice, as specified by each sub-figure title. Results are depicted separately for low-SES students (in green) and high-SES students (in gray). The top-right corner of each panel displays the estimated coefficients along with their standard errors. The term 'Low-SES return' denotes the effect for low-SES students, while 'High-SES difference' represents the estimated difference in effects between high- and low-SES students ($\tau^1 - \tau^0$). Standard errors clustered at the individual level are in parentheses.

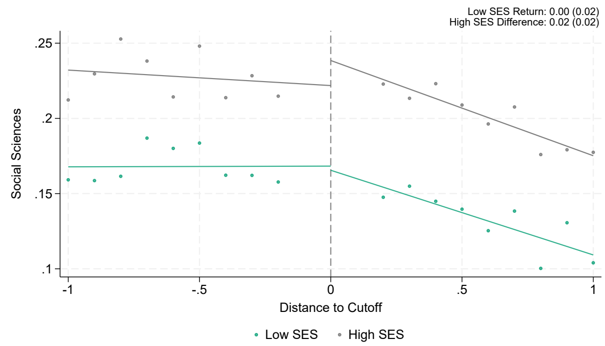
1.10.5 *Choice of Field of Study and Institutions*

This Appendix provides additional results on the returns of cutoff-crossing on field of study and college admission.

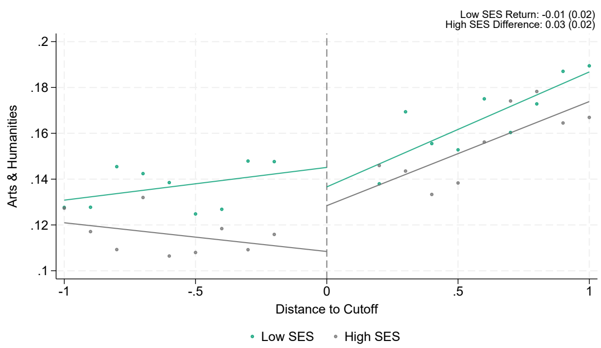
Figure 1.24: Choice of Field of Study and University



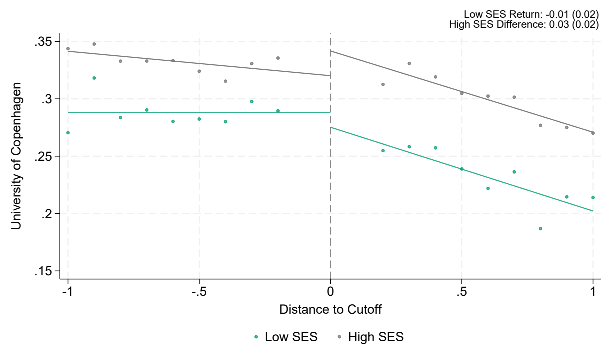
(a) Field of Study: Business & Law



(b) Field of Study: Social Sciences



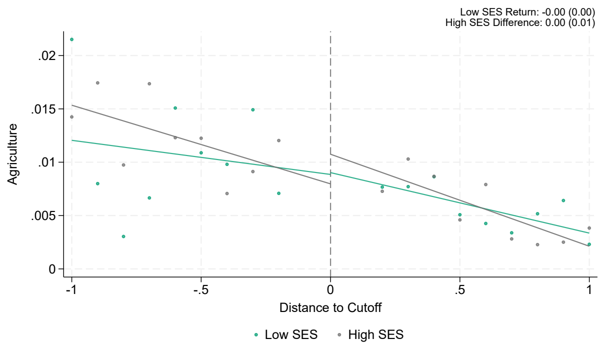
(c) Field of Study: Arts & Humanities



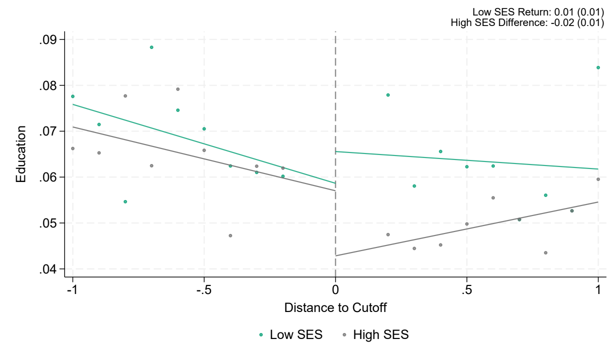
(d) Institution: University of Copenhagen

Notes: This figure presents further evidence on the impact of cutoff-crossing on field of study and college admitted, as specified by each sub-figure title. Results are depicted separately for low-SES students (in green) and high-SES students (in gray). The top-right corner of each panel displays the estimated coefficients along with their standard errors. The term 'Low-SES return' denotes the effect for low-SES students, while 'High-SES difference' represents the estimated difference in effects between high- and low-SES students ($\tau^1 - \tau^0$). Standard errors clustered at the individual level are in parentheses.

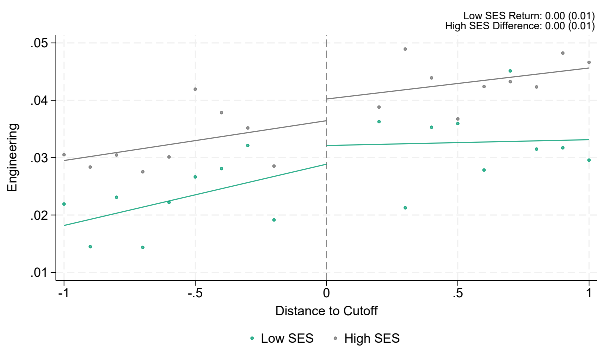
Figure 1.25: Choice of Field of Study



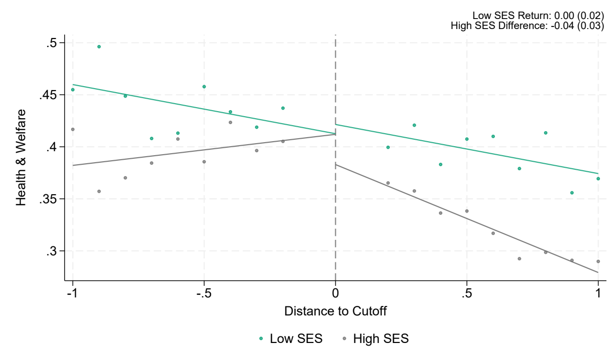
(a) Field of Study: Agriculture



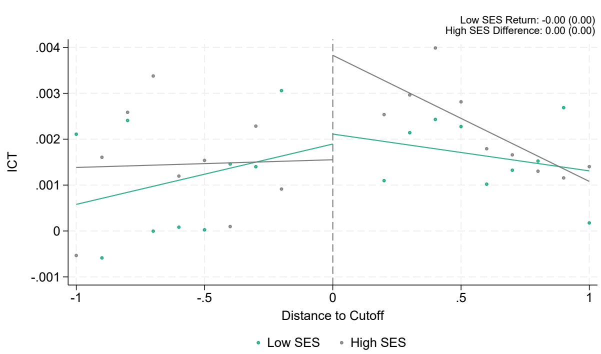
(b) Field of Study: Education



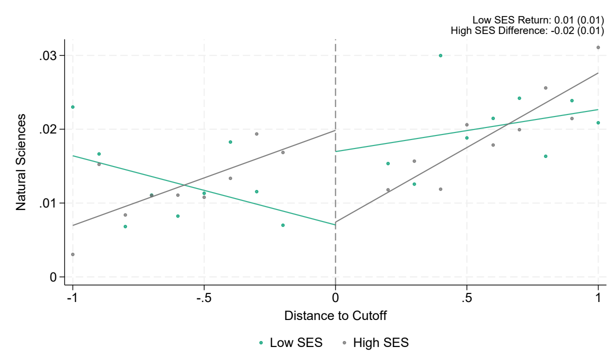
(c) Field of Study: Engineering



(d) Institution: Health & Welfare



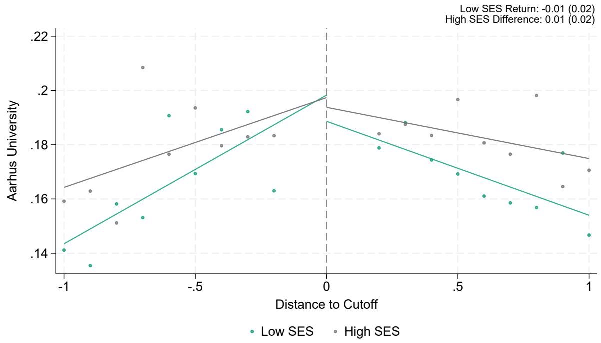
(e) Field of Study: ICT



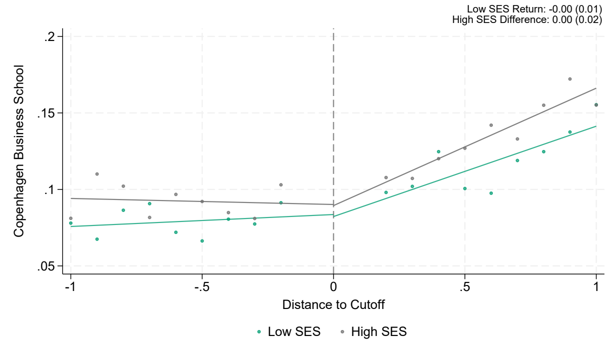
(f) Field of Study: Natural Sciences

Notes: This figure presents further evidence on the impact of cutoff-crossing on field of study, as specified by each sub-figure title. Results are depicted separately for low-SES students (in green) and high-SES students (in gray). The top-right corner of each panel displays the estimated coefficients along with their standard errors. The term 'Low-SES return' denotes the effect for low-SES students, while 'High-SES difference' represents the estimated difference in effects between high- and low-SES students ($\tau^1 - \tau^0$). Standard errors clustered at the individual level are in parentheses.

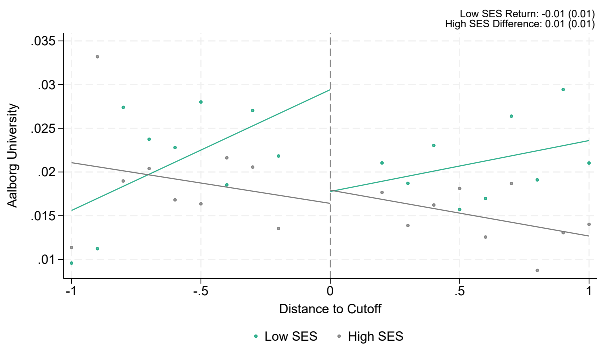
Figure 1.26: Choice of University



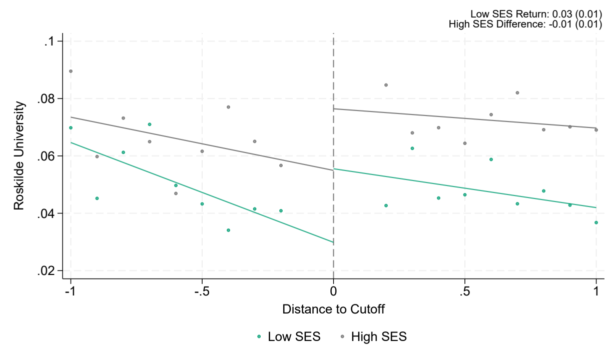
(a) Aarhus University



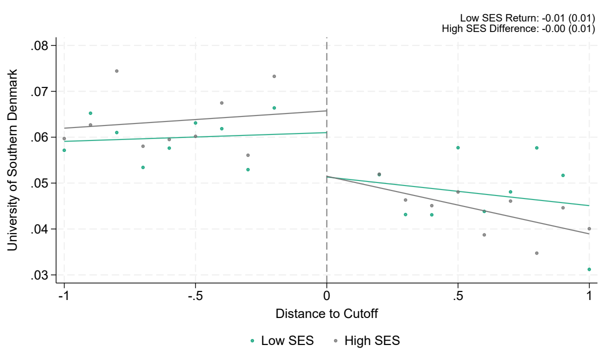
(b) Copenhagen Business School



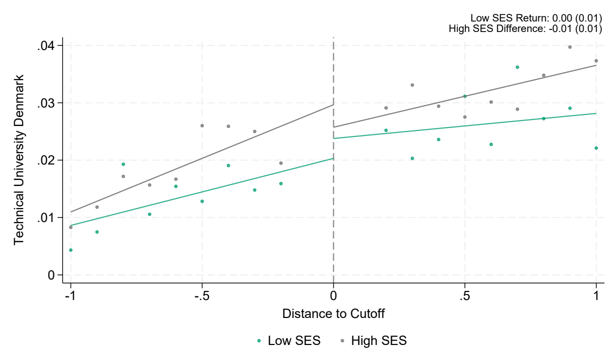
(c) Aalborg University



(d) Roskilde University



(e) University of Southern Denmark



(f) Technical University Denmark

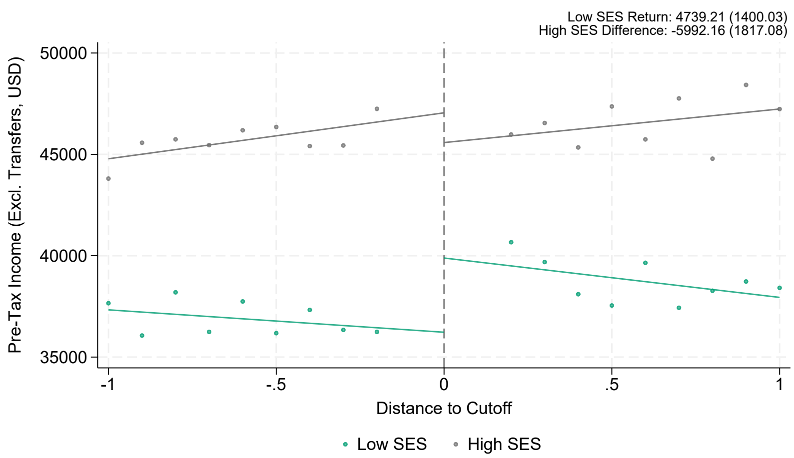
Notes: This figure presents further evidence on the impact of cutoff-crossing on the college admitted, as specified by each sub-figure title. Results are depicted separately for low-SES students (in green) and high-SES students (in gray). The top-right corner of each panel displays the estimated coefficients along with their standard errors. The term 'Low-SES return' denotes the effect for low-SES students, while 'High-SES difference' represents the estimated difference in effects between high- and low-SES students ($\tau^1 - \tau^0$). Standard errors clustered at the individual level are in parentheses.

1.10.6 Heterogeneity

This Appendix provides an analysis of the heterogeneous nature of the results, based on students' GPA, gender and origins.

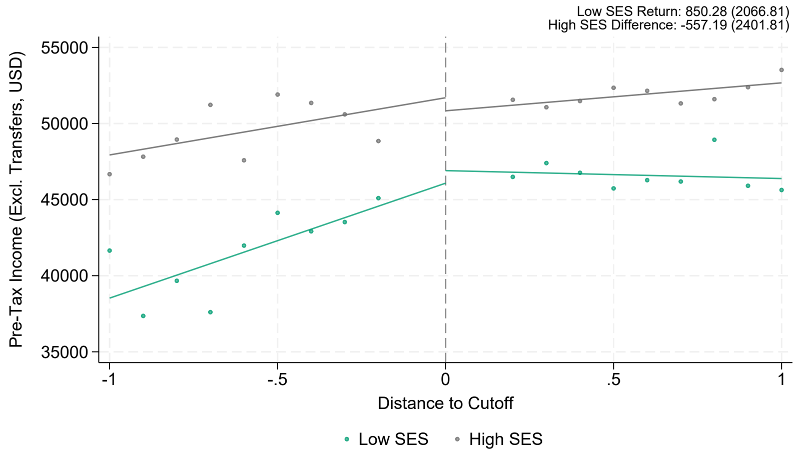
GPA

Figure 1.27: Cutoff-Crossing Effect, GPA < median, by SES



Notes: Reduced-form effect of cutoff-crossing on pre-tax income (excluding transfers), 12 years post-application, focusing on individuals with below median grades. The figure depicts the effect for low-SES students (depicted in green) and high-SES students (depicted in gray). Coefficients and standard errors are reported in the top-right corner. "low-SES return" refer to the effect for low-SES students, while "high-SES difference" reports the difference in the returns between high and low-SES students ($\tau^1 - \tau^0$).

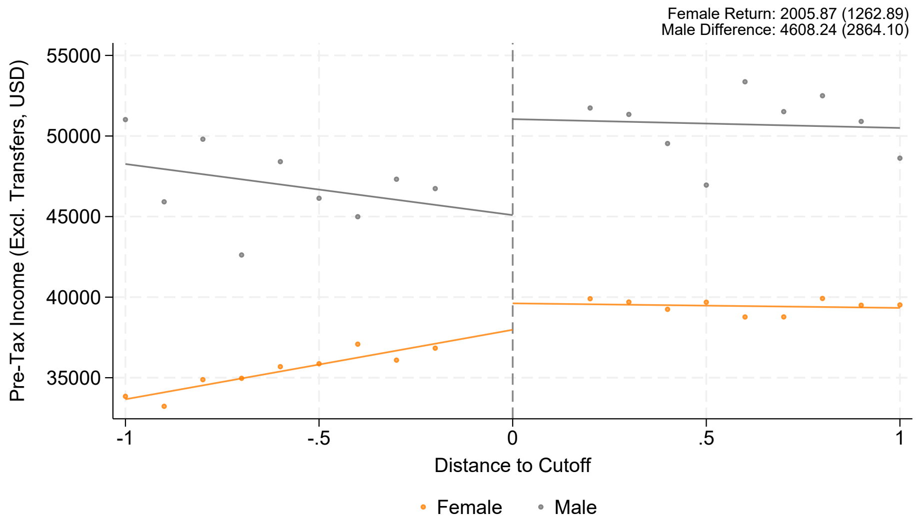
Figure 1.28: Cutoff-Crossing Effect, GPA > median, by SES



Notes: Reduced-form effect of cutoff-crossing on pre-tax income (excluding transfers), 12 years post-application, focusing on individuals with above median grades. The figure depicts the effect for low-SES students (depicted in green) and high-SES students (depicted in gray). Coefficients and standard errors are reported in the top-right corner. "low-SES return" refer to the effect for low-SES students, while "high-SES difference" reports the difference in the returns between high- and low-SES students ($\tau^1 - \tau^0$).

Gender

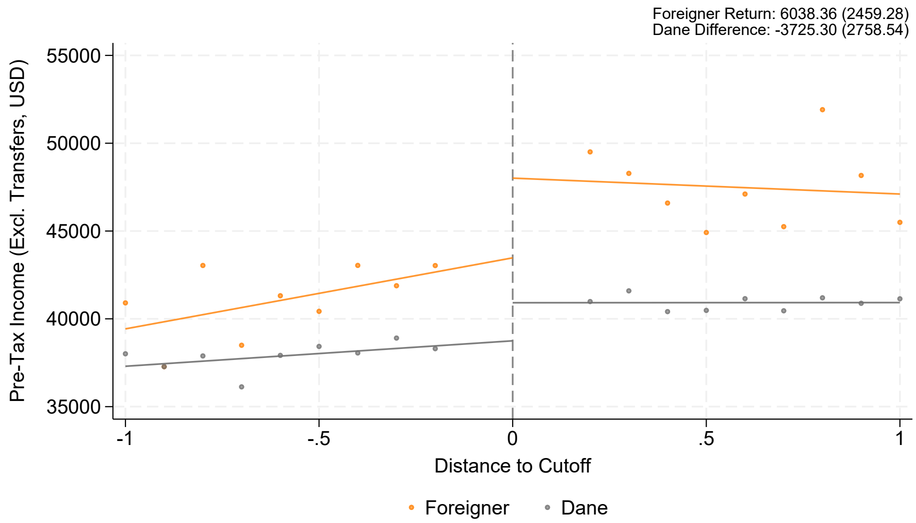
Figure 1.29: Cutoff-Crossing Effect for low-SES, by Gender



Notes: Reduced-form effect of cutoff-crossing on pre-tax income (excluding transfers), 12 years post-application, focusing on low-SES students. The figure depicts the effect for female students (depicted in orange) and male students (depicted in gray). Coefficients and standard errors are reported in the top-right corner. “Female return" refer to the effect for female students, while “male difference" reports the difference in the returns between male and female students.

Origins

Figure 1.30: Cutoff-Crossing Effect for low-SES, by Origin



Notes: Reduced-form effect of cutoff-crossing on pre-tax income (excluding transfers), 12 years post-application, focusing on low-SES students. The figure depicts the effect for students from foreign origins (depicted in orange) and students of danish origins (depicted in gray). Coefficients and standard errors are reported in the top-right corner. “Foreigner return" refer to the effect for foreigners, while “Dane difference" reports the difference in the returns between students of Danish and foreign origins.

1.10.7 Peer Effects

This Appendix reports the regression results for the analysis on the effect of peers on outcomes.

By Tertile of Peer Quality Difference

Table 1.4: Tertile-Specific Coefficients

	Low SES	High SES	Tertile Mean
Bottom Tertile	2474.77	4547.86	-11512.89
	(3728.18)	(2796.70)	
Middle Tertile	6861.91	-1685.39	4217.97
	(3422.35)	(2476.59)	
Top Tertile	3127.63	98.30	21411.59
	(3969.34)	(2359.42)	

Notes: The table reports estimates of the effect of threshold crossing, by peer quality difference between preferred and fallback option. The last column shows the average difference in peer quality in each of the three quartiles I consider. Robust standard errors are in parentheses.

Within-Degree Across-Cohort Variation

Table 1.5: Regression Results: Peer Effects

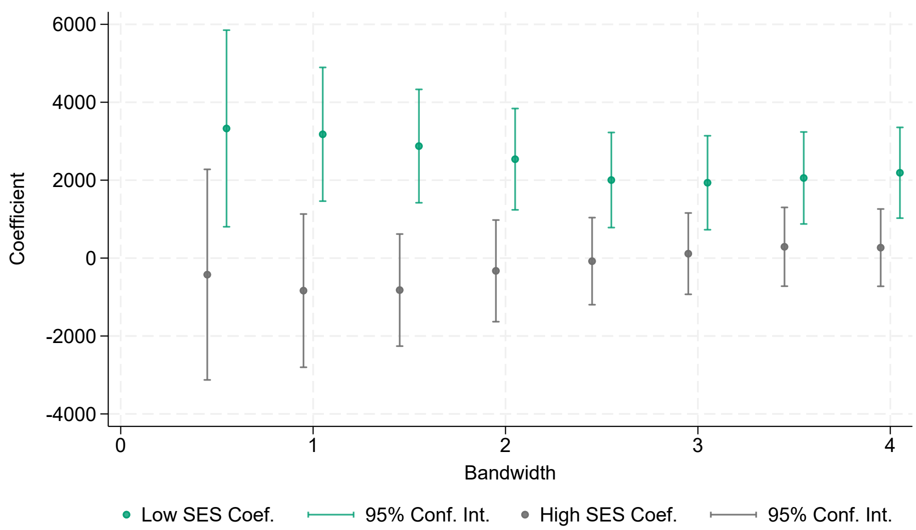
	OLS		Degree FE	
	Low SES	High SES	Low SES	High SES
Peer Quality (Parent Income)	0.201	0.220	0.062	0.016
	(0.023)	(0.047)	(0.028)	(0.028)
High School GPA	2363.2	2235.3	1466.8	2394.1
	(413.9)	(578.7)	(383.9)	(641.9)
Parental Gross Income	-0.007	0.019	0.001	0.018
	(0.023)	(0.007)	(0.018)	(0.006)
Age	-1406.9	-2437.5	-955.6	-1243.3
	(228.6)	(195.8)	(163.6)	(162.0)
Male	9584.7	13250.7	8784.7	10414.6
	(866.4)	(378.7)	(765.4)	(784.8)
Immigrants	3825.0	-4322.1	-190.8	-6685.4
	(892.7)	(2592.2)	(987.1)	(2772.9)
Descendants	3030.5	-5743.1	10.3	-5938.8
	(699.5)	(1796.2)	(685.6)	(1935.6)
Order of Birth	70.2	-621.2	-120.5	-298.5
	(185.7)	(212.5)	(151.8)	(187.5)
Parent College Completion	-1933.5	-1324.7	-1615.3	-492.0
	(654.7)	(485.7)	(355.1)	(560.4)
Constant	30429.4	53149.8	42835.6	49194.2
	(7987.6)	(9917.4)	(5993.3)	(10322.1)
Observations	7757	20201	7737	20194

Notes: The table reports estimates of the effect of peer quality on outcomes, separately for low and high SES students. The first two columns report results from OLS specifications, while the following two report parameters from specifications that include degree and cohort fixed effects. Peer quality is standardized. Robust standard errors are in parentheses.

1.10.8 Robustness

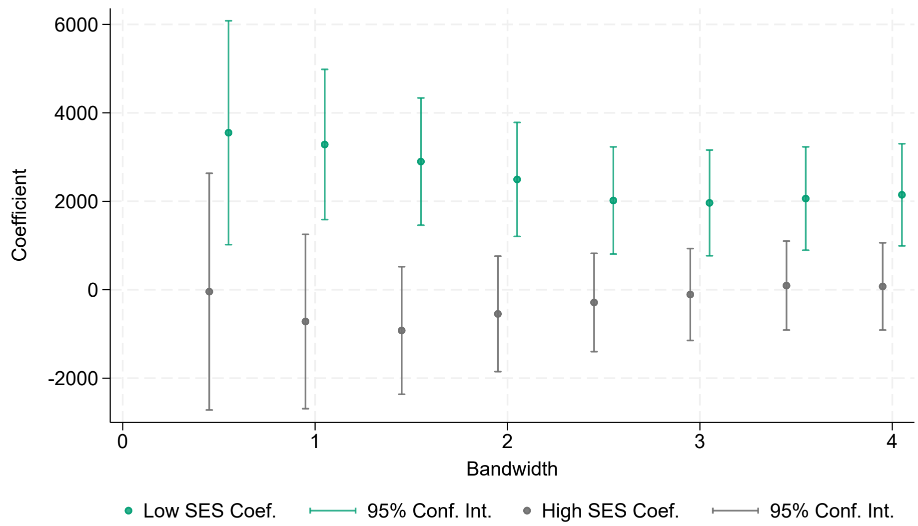
This Appendix assesses the robustness of the results by examining the role of (1) different bandwidth sizes, (2) controls, and (3) the doughnut strategy. The reported coefficients denote the effect of cutoff-crossing on pre-tax income (excluding transfers), twelve years post-application.

Figure 1.31: Specifications Without Controls and Without Doughnut



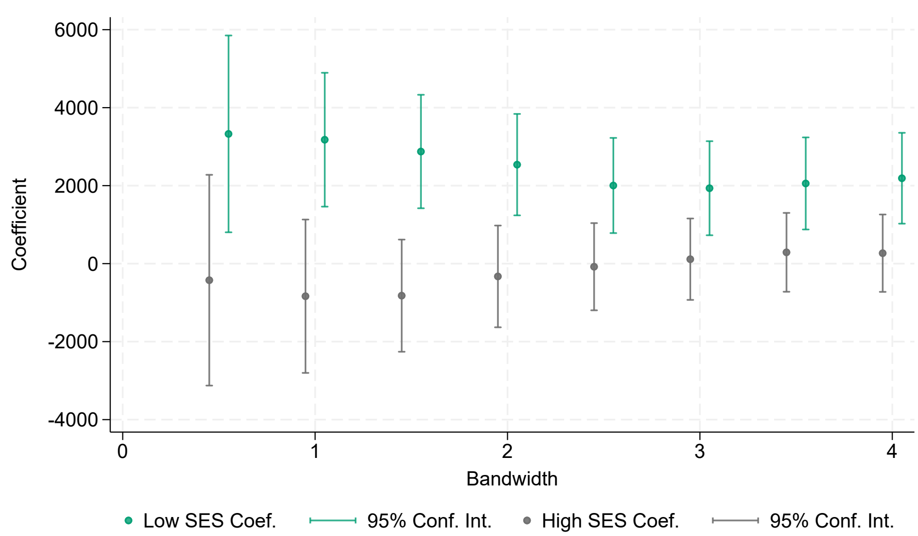
Notes: Effect of cutoff-crossing on pre-tax income, estimated with different bandwidth sizes, separately for low- and high-SES. The estimates reported come from a specification without controls and without dropping observations around the cutoff over the interval $[-.1, .1]$.

Figure 1.32: Specifications Without Controls and With Doughnut



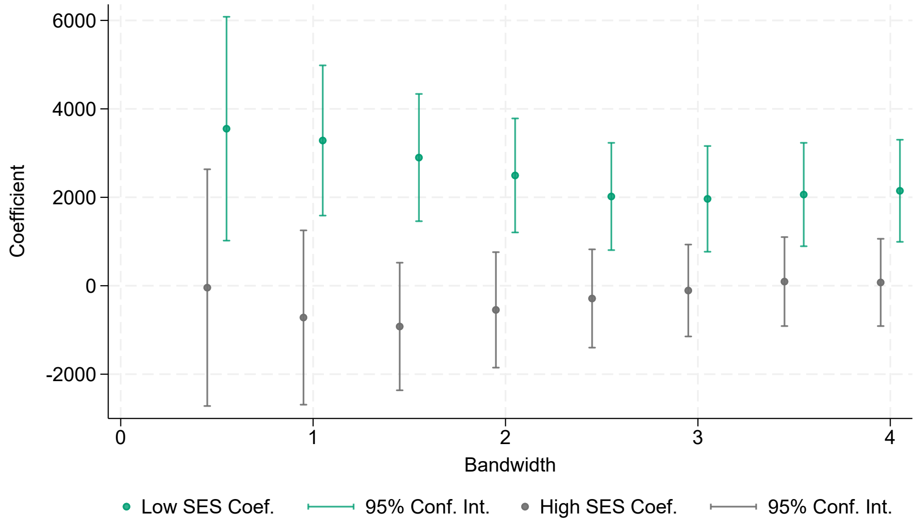
Notes: Effect of cutoff-crossing on pre-tax income, estimated with different bandwidth sizes, separately for low- and high-SES. The estimates reported come from a specification without controls and dropping observations around the cutoff over the interval $[-.1, .1]$.

Figure 1.33: Specifications With Controls and Without Doughnut



Notes: Effect of cutoff-crossing on pre-tax income, estimated with different bandwidth sizes, separately for low- and high-SES. The estimates reported come from a specification with controls and without dropping observations around the cutoff over the interval $[-.1, .1]$.

Figure 1.34: Specifications With Controls and With Doughnut



Notes: Effect of cutoff-crossing on pre-tax income, estimated with different bandwidth sizes, separately for low- and high-SES. The estimates reported come from a specification with controls and dropping observations around the cutoff over the interval $[-.1, .1]$.

1.10.9 Control Function Derivation

This section derives the control function approach from Dubin and McFadden [1984] to provide intuition on the framework used in this paper, which follows the approach of Abdulkadiroğlu et al. [2020]. Abdulkadiroğlu et al. [2020] extend the control function approach from Dubin and McFadden [1984] by allowing for rank-ordered preferences, whereas the original model by Dubin and McFadden [1984] is based on observing the preferred choice in polychotomous settings.

The unobserved factors η_j in the choice equation are assumed to have independent extreme value distributions. The density of the error term in the outcome equation, ε_1, conditional on $(\eta_1, ..., \eta_M)$ is assumed to have mean $(\sigma\frac{\sqrt{6}}{\pi})\sum_{j=1}^{M} r_j(\eta_j - \mathbb{E}(\eta_j))$ where $\sum_{j=1}^{M} r_j = 0$. r_j is the correlation of η_j and ε_1.[57] Formally, Dubin and McFadden [1984] rely on the following linearity assumption expressed in terms of the η_j:

A1- Dubin and McFadden's linearity assumption:

$\mathbb{E}(\varepsilon_1 \mid \eta_1, ..., \eta_M) = \sigma\frac{\sqrt{6}}{\pi}\sum_{j=1...M} r_j(\eta_j - \mathbb{E}(\eta_j))$,

where r_j is a correlation coefficient between ε_1 and η_j.

A1'- Dubin and McFadden's restriction:

$\sum_{j=1...M} r_j = 0$

The expected value of η_j given that alternative 1 is chosen is as follows:

$$\mathbb{E}(\eta_1 - \mathbb{E}(\eta_1) \mid V_1 > \max_{s\neq 1}(V_s), \Gamma) = -\ln(P_1), \tag{1.13}$$

$$\mathbb{E}(\eta_j - \mathbb{E}(\eta_j) \mid V_1 > \max_{s\neq 1}(V_s), \Gamma) = \frac{P_j}{1-P_j}\ln(P_j), \tag{1.14}$$

where P_j is the probability of choosing alternative j and $\Gamma = \{z'\gamma_1, z'\gamma_2, \ldots, z'\gamma_M\}$.[58] Then,

57. Note that η_j has unconditional variance $\pi^2/6$ and ε_1 has unconditional mean zero and unconditional variance σ^2.

58. With the multinomial logit model, $P_j = Prob(V_j > \max_{s\neq j}(V_s)) = e^{z_j\gamma_j}/\sum_s e^{z_s\gamma_s}$.

imposing the assumptions from Dubin and McFadden [1984]:

$$\begin{aligned}
\mathbb{E}\left(\varepsilon_1 \mid u_1 < 0, \Gamma\right) &= \mathbb{E}\big[\sigma\frac{\sqrt{6}}{\pi}\sum_{j=1...M} r_j(\eta_j - \mathbb{E}(\eta_j)) \mid u_1 < 0, \Gamma\big] \\
&= \sigma\frac{\sqrt{6}}{\pi}\big[-r_1\ln(P_1) + \sum_{j=2...M} r_j\big(\frac{P_j}{1-P_j}\ln(P_j)\big)\big] \\
&= \sigma\frac{\sqrt{6}}{\pi}\sum_{j=2...M} r_j\big(\frac{P_j}{1-P_j}\ln(P_j) + \ln(P_1)\big)
\end{aligned} \tag{1.15}$$

where $u_1 = \max\{V_{ik} - V_{il}\}$. The last equation holds because $\sum_{j=1}^{M} r_j = 0$, which means $(\sum_{j=2}^{M} r_j = -r_1)$. Selection correction in model 2.5a can be addressed by running least squares of the equation below:

$$Y_1 = \alpha_1 + X'\beta_1 + \bar{X}\gamma_1 + \sigma\frac{\sqrt{6}}{\pi}\sum_{j=2...M} r_j\big(\frac{P_j\ln(P_j)}{1-P_j} + \ln(P_1)\big) + w_1$$

Density of the Disturbance Terms of the Latent Models Conditionally on Alternative 1 Being Observed

The conditional density of η_1 is

$$f(\eta_1 \mid V_1 > \max_{s\neq 1}(V_s)) = f\big(\eta_1 \mid \eta_1 > \max_{s\neq 1}(z_s\gamma_s + \eta_s - z_1\eta_1)\big) \tag{1.16}$$

$$= \frac{g(\eta_1)}{Prob\big(V_1 > \max_{s\neq 1}(V_s)\big)}\int ... \int 1_{[\eta_1 > (z_2\gamma_2 + \eta_2) - z_1\gamma_1]} g(\eta_2)$$

$$...1_{[\eta_1 > (z_M\gamma_M + \eta_M) - z_1\gamma_1]} g(\eta_M) d\eta_2 ... d\eta_M \tag{1.17}$$

$$= \frac{g(\eta_1)}{Prob\big(V_1 > \max_{s\neq 1}(V_s)\big)}\prod_{s>1} G(\eta_1 + z_1\gamma_1 - z_s\gamma_s) \tag{1.18}$$

where G is the cumulative of the Gumbel distribution, that is, $G(x) = exp(-e^{-x})$, with the unconditional density of $g(x) = exp(-x - e^{-x})$. Now, note that

$$\prod_{s>1} G(\eta_1 + z_1\gamma_1 - z_s\gamma_s) = exp\big(- e^{-\eta_1} \frac{\sum_{s>1} e^{z_s\gamma_s}}{e^{z_1\gamma_1}}\big)$$

After a simple transformation, we get

$$f(\eta_1 \mid V_1 > \max_{s\neq 1}(V_s)) = \frac{e^{-\eta_1}}{P_1}.exp\big(- \frac{e^{-\eta_1}}{P_1}\big) = g(\eta_1 + \log P_1)$$

where $P_1 = Prob\big(V_1 > \max_{s\neq 1}(V_s)\big) = e^{z_1\gamma_1}/\sum_s e^{z_s\gamma_s}$.

The conditional density of η_j $(j \neq 1)$ is

$$\begin{aligned}
&f(\eta_j \mid V_1 > \max_{s\neq 1}(V_s)) = f\big(\eta_j \mid \eta_1 > \max_{s\neq 1}(z_s\gamma_s + \eta_s - z_1\eta_1)\big)\\
&= \frac{g(\eta_j)}{P_1}\int_{\eta_1 > (z_j\gamma_j + \eta_j) - z_1\gamma_1} \prod_{s>1, s\neq j} G(\eta_1 + z_1\gamma_1 - z_s\gamma_s) g(\eta_1) d\eta_1\\
&= \frac{g(\eta_j)}{P_1}\int_{\eta_1 > (z_j\gamma_j + \eta_j) - z_1\gamma_1} \left[exp\left(-e^{-\eta_1} - \sum_{s>1, s\neq j} exp(-\eta_1 - z_1\gamma_1 + z_s\gamma_s)\right)\right] exp(-\eta_1) d\eta_1\\
&= \frac{g(\eta_j)}{P_1}\int_{\eta_1 > (z_j\gamma_j + \eta_j) - z_1\gamma_1} \left[exp(-\frac{1}{\tilde{P}_1^j} exp(-\eta_1))\right] exp(-\eta_1) d\eta_1
\end{aligned}$$

where $\tilde{P}_1^j = Prob(V_1 > \max_{s\neq 1, s\neq j}(V_s)) = e^{z_1\gamma_1}/\sum_{s\neq j} e^{z_s\gamma_s}$. Integrating with respect to

η_1:

$$
\begin{aligned}
& f(\eta_j \mid V_1 > \max_{s \neq 1}(V_s)) \\
& = \frac{\tilde{P}_1^j}{P_1} g(\eta_j) \left[1 - exp \left(- \frac{1}{\tilde{P}_1^j} exp(-(z_j\gamma_j + \eta_j) + z_1\gamma_1) \right) \right] \\
& = \frac{1}{1-P_j} g(\eta_j) \left[1 - exp \left(- \frac{1-P_j}{P_j} e^{-\eta_j} \right) \right] \\
& = \frac{1}{1-P_j} \left[g(\eta_j) - g(\eta_j) exp(e^{-\eta_j}) exp \left(- \frac{e^{-\eta_j}}{P_j} \right) \right] \\
& = \frac{1}{1-P_j} \left[g(\eta_j) - e^{-\eta_j} exp \left(- \frac{e^{-\eta_j}}{P_j} \right) \right] \\
& = \frac{1}{1-P_j} [g(\eta_j) - P_j g(\eta_j + \log P_j)],
\end{aligned}
$$

where as before, $P_j = Prob(V_j > \max_{s \neq j}(V_s)) = e^{z_j\gamma_j} / \sum_s e^{z_s\gamma_s}$.

Now, the conditional expectation

$$
\mathbb{E}(\eta_j | V_1 > \max_{s \neq 1}(V_s)) = \int \eta_j f(\eta_j \mid V_1 > \max_{s \neq 1}(V_s), \Gamma) d\eta_j
$$

can be computed as follows. For $j = 1$

$$
\begin{aligned}
\mathbb{E}(\eta_1 | V_1 > \max_{s \neq 1}(V_s)) &= \int \eta_1 f(\eta_1 \mid V_1 > \max_{s \neq 1}(V_s)) d\eta_1 \\
&= \int \eta_1 g(\eta_1 + \log P_1) d\eta_1 \\
&= \int (\omega - \log P_1) g(\omega) d\omega
\end{aligned}
$$

where we use the change of variable $\omega = \eta_1 + \log P_1$ to obtain the last equation. Then,

$$\begin{aligned}
\mathbb{E}(\eta_1|V_1 > \max_{s\neq 1}(V_s)) &= \int(\omega - \log P_1)g(\omega)d\omega \\
&= -\log(P_1)
\end{aligned}$$

For $j \neq 1$.

$$\begin{aligned}
\mathbb{E}(\eta_j|V_1 > \max_{s\neq 1}(V_s)) &= \frac{1}{1-P_j}\int \eta_j[g(\eta_j) - P_j g(\eta_j + \log P_j)]d\eta_j \\
&= \frac{1}{1-P_j}\int \eta_j g(\eta_j)d\eta_j - \frac{P_j}{1-P_j}\int \eta_j g(\eta_j + \log P_j)d\eta_j
\end{aligned}$$

Make the change of variable $\omega = \eta_j + \log P_j$. Also, note that $\int \eta_j g(\eta_j)d\eta_j = \mathbb{E}(\eta_j) = 0$. Then

$$\begin{aligned}
\mathbb{E}(\eta_j|V_1 > \max_{s\neq 1}(V_s)) &= -\frac{P_j}{1-P_j}\int(\omega - \log P_j)g(\omega)d\omega \\
&= \frac{P_j}{1-P_j}\log P_j
\end{aligned}$$

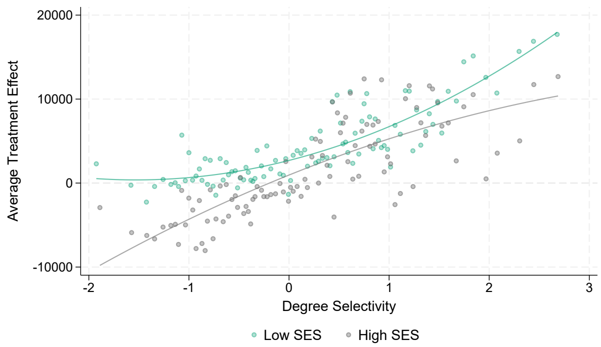
1.10.10 Estimates

This Appendix provides further evidence on the relationship between degrees-specific returns, match effects and degree selectivity. It distinguishes between estimates derived from the model and those that do not account for selection, referred to as 'OLS estimates.'

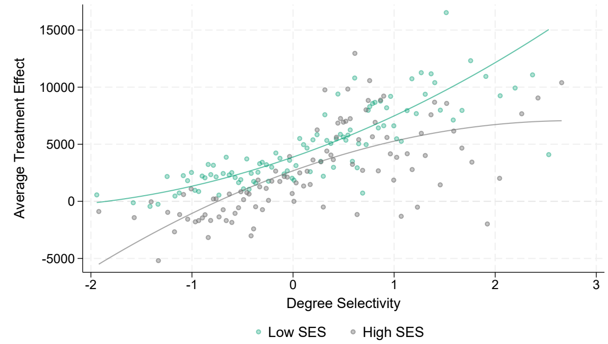
Section 1.10.10 presents binned scatter plots on the relationship between degree-specific returns and degree selectivity. Section 1.10.10 looks at the correlation between match effects by SES background. In Section 1.10.10, I turn to presenting the relationship between match effects estimates and degree selectivity. Finally, Section 1.10.10 provides a direct comparison between estimates that account for selection and those that do not.

Degree Treatment Effects

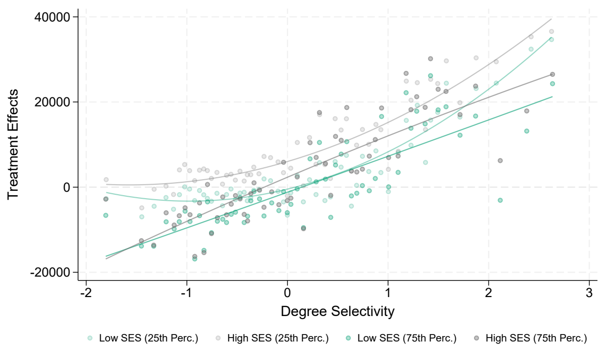
Figure 1.35: Model and OLS Estimates



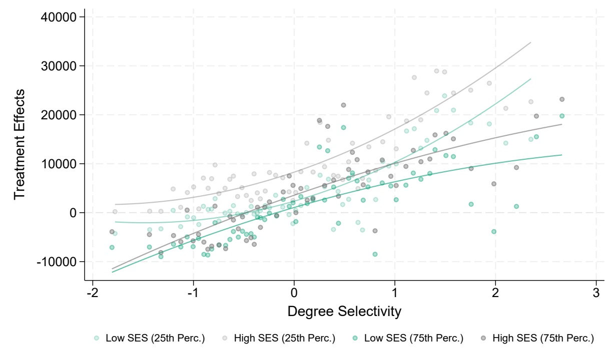
(a) Degree Treatment Effects, Model Estimates



(b) Degree Treatment Effects, OLS Estimates



(c) Degree Treatment Effects, Bottom Quartile and Top Quartile of Characteristics, Model Estimates



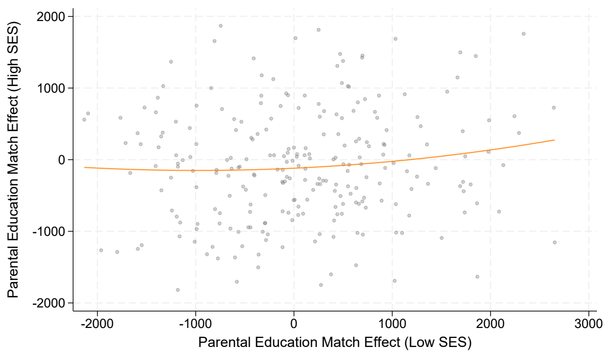
(d) Degree Treatment Effects, Bottom Quartile and Top Quartile of Characteristics, OLS Estimates

Notes: Binned scatter plots of degree-specific estimates against degree selectivity. Panels (a) and (b) present the effects at the mean levels of individual characteristics, relative to an outside option of not enrolling in a degree in the year of application. Panel (c) and (d) present the relationship between treatment effects at different levels of characteristics. Lighter shades of green and grey are used to depict students with characteristics at the bottom of their distribution, while darker shades correspond to estimates where individual characteristics stand at the top of the distribution. Low-SES students are depicted in green while high-SES students are depicted in gray.

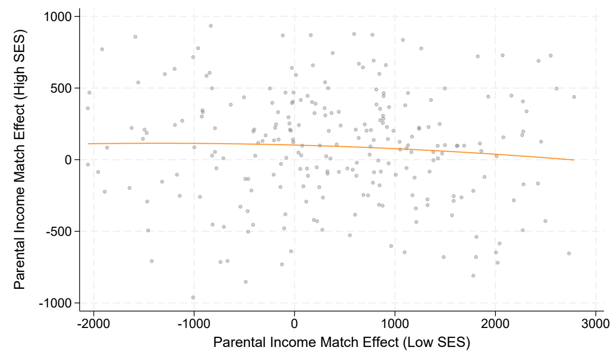
Match Effects Correlations

Model Estimates

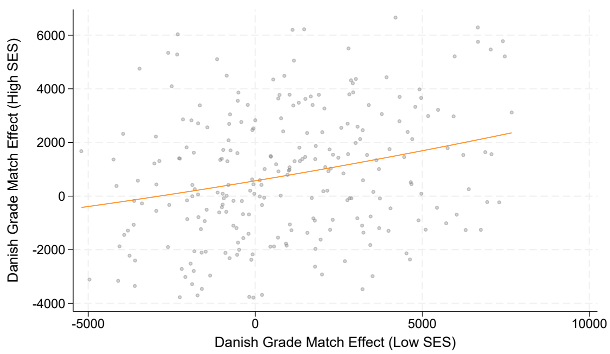
Figure 1.36: Correlation Between Degree Match Effects by SES



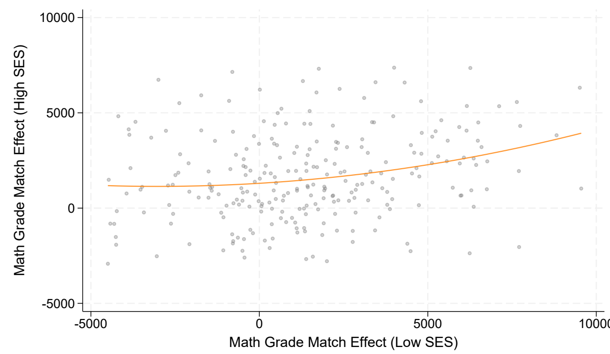
(a) Education



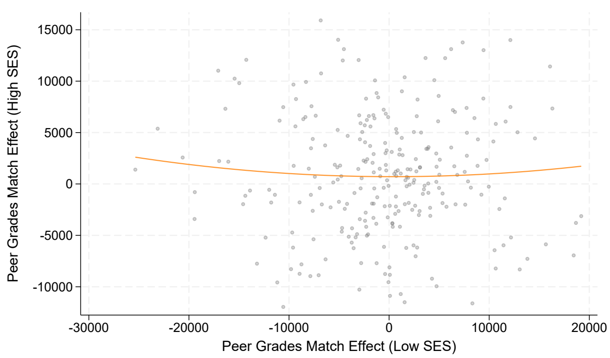
(b) Parental Income



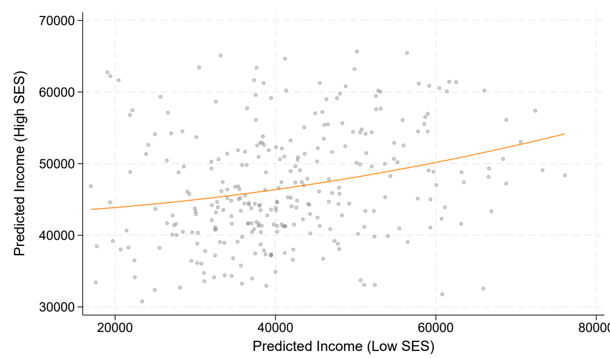
(c) Danish Grade



(d) Mathematics Grade



(e) Peer Grades

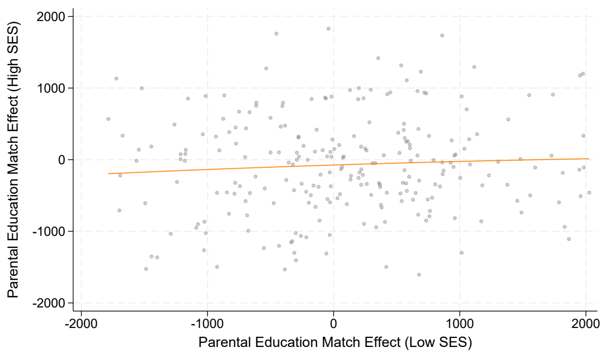


(f) Predicted Income

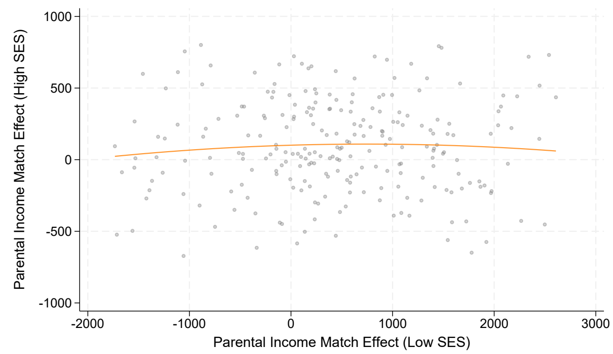
Notes: Correlations between match effects estimates from the model, across SES backgrounds.

OLS Estimates

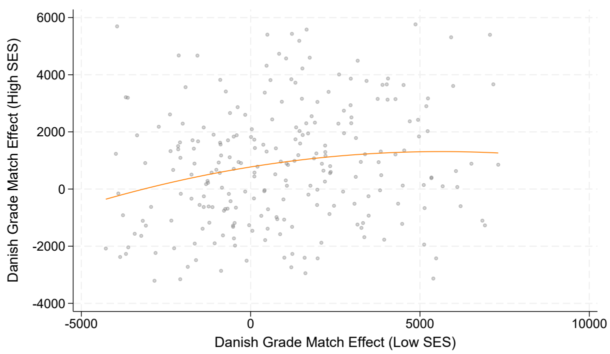
Figure 1.37: Correlation Between Degree Match Effects



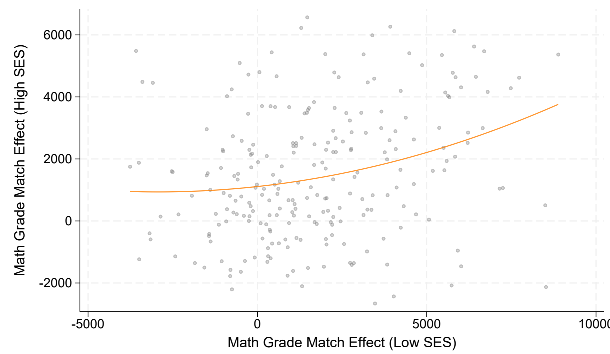
(a) Education



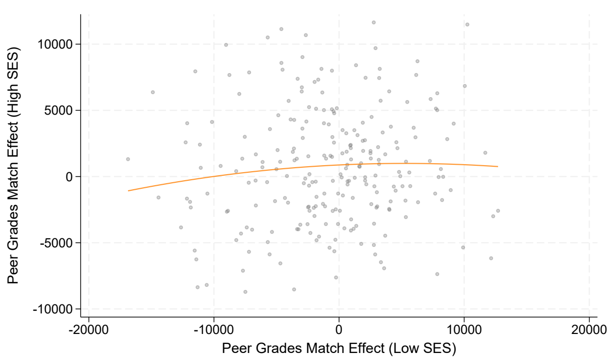
(b) Parental Income



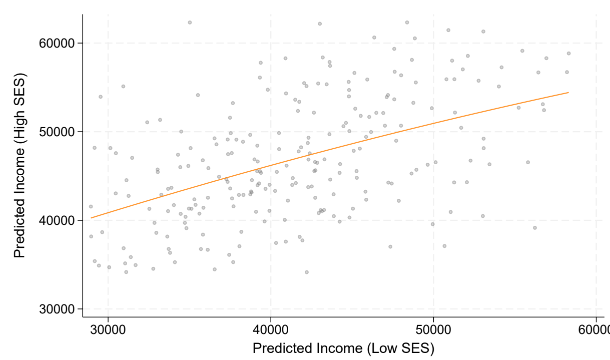
(c) Danish Grade



(d) Mathematics Grade



(e) Peer Grades



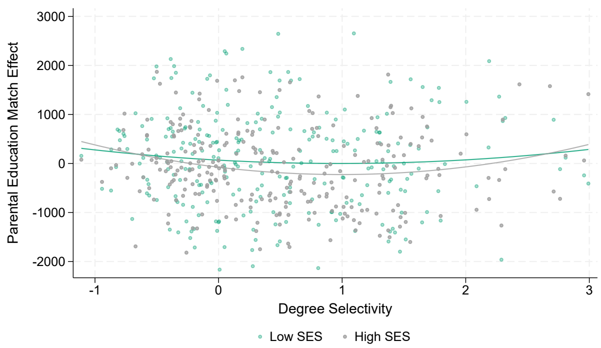
(f) Predicted Income

Notes: Correlations between match effects estimates from OLS, across SES backgrounds.

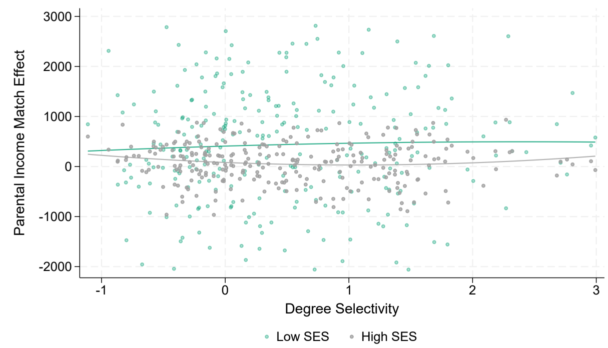
Match Effects And Degree Selectivity

Model Estimates

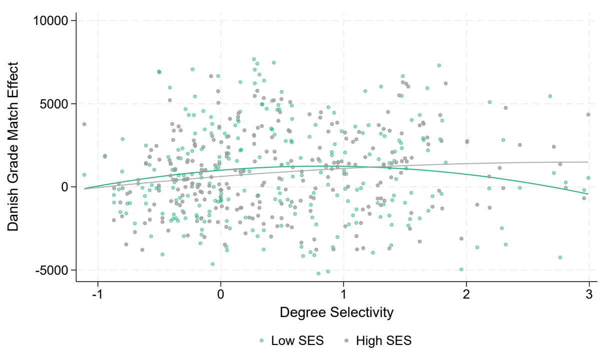
Figure 1.38: Match Effects And Degree Selectivity



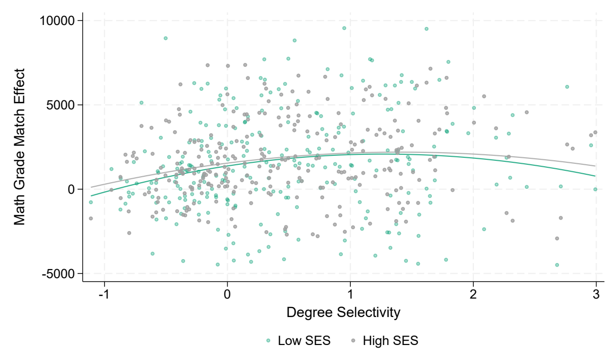
(a) Education



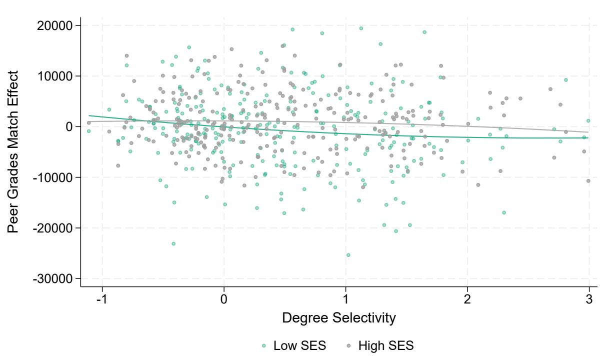
(b) Parental Income



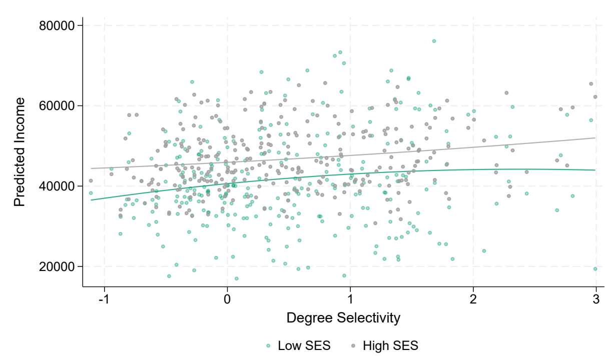
(c) Danish Grade



(d) Mathematics Grade



(e) Peer Grades

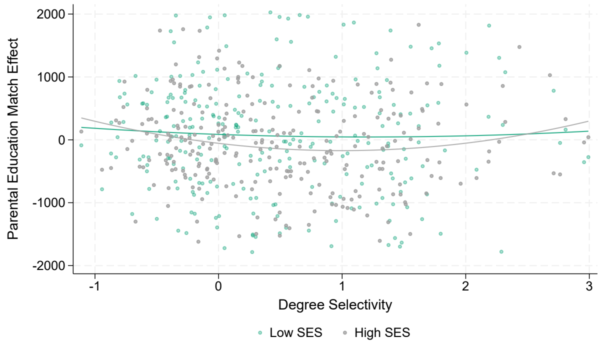


(f) Predicted Income

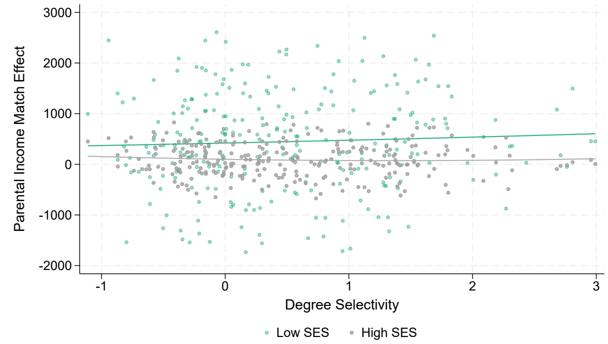
Notes: Correlations between match effects estimated from the model and degree selectivity, as measured by average high school grades of enrolled students.

OLS Estimates

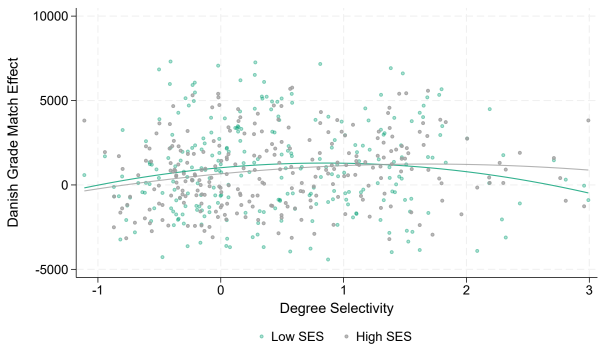
Figure 1.39: Match Effects and Degree Selectivity



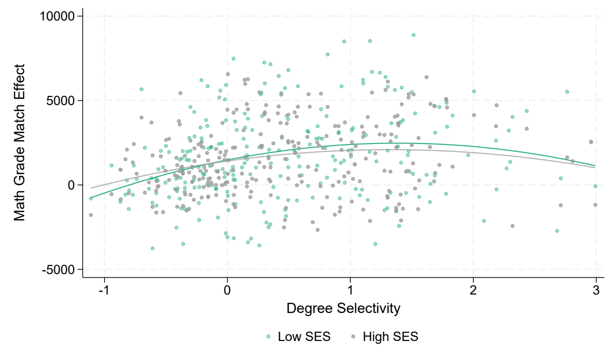
(a) Education



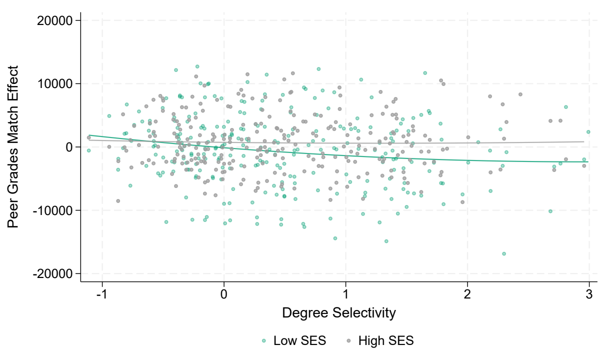
(b) Parental Income



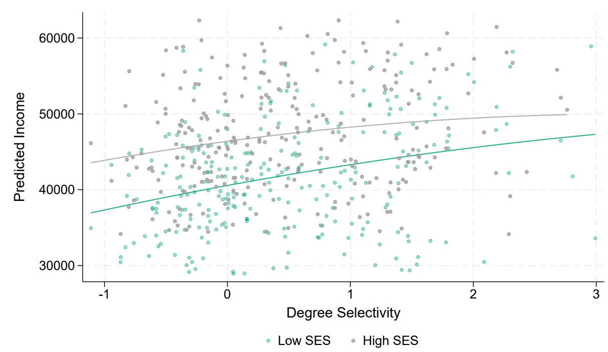
(c) Danish Grade



(d) Mathematics Grade



(e) Peer Grades

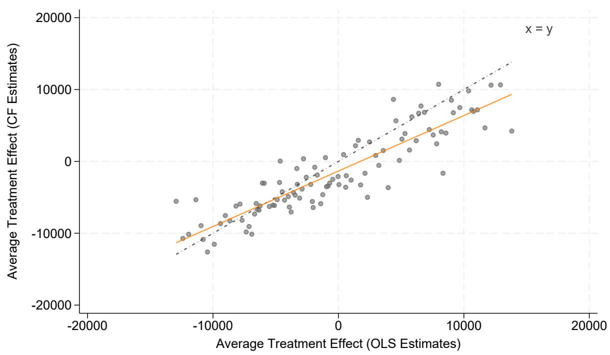


(f) Predicted Income

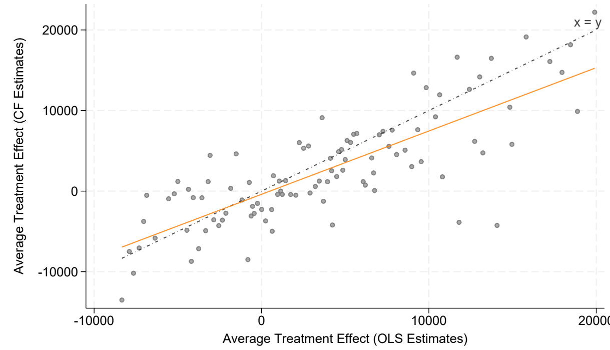
Notes: Correlations between match effects estimated by OLS and degree selectivity, as measured by average high school grades of enrolled students.

OLS vs. Model Estimates

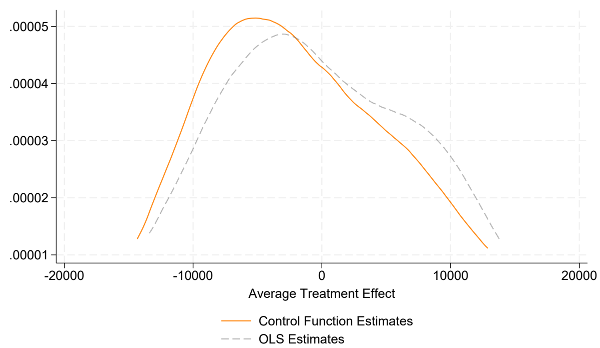
Figure 1.40: Comparison of Estimates



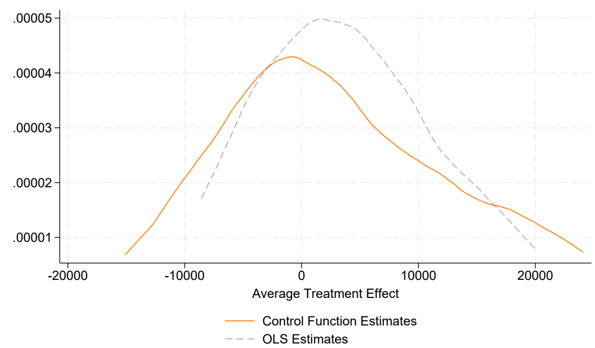
(a) Scatter Plot of Model Estimates Against OLS Estimates for high-SES Students



(b) Scatter Plot of Model Estimates Against OLS Estimates for low-SES Students



(c) Densities of Estimates for high-SES Students



(d) Densities of Estimates for low-SES Students

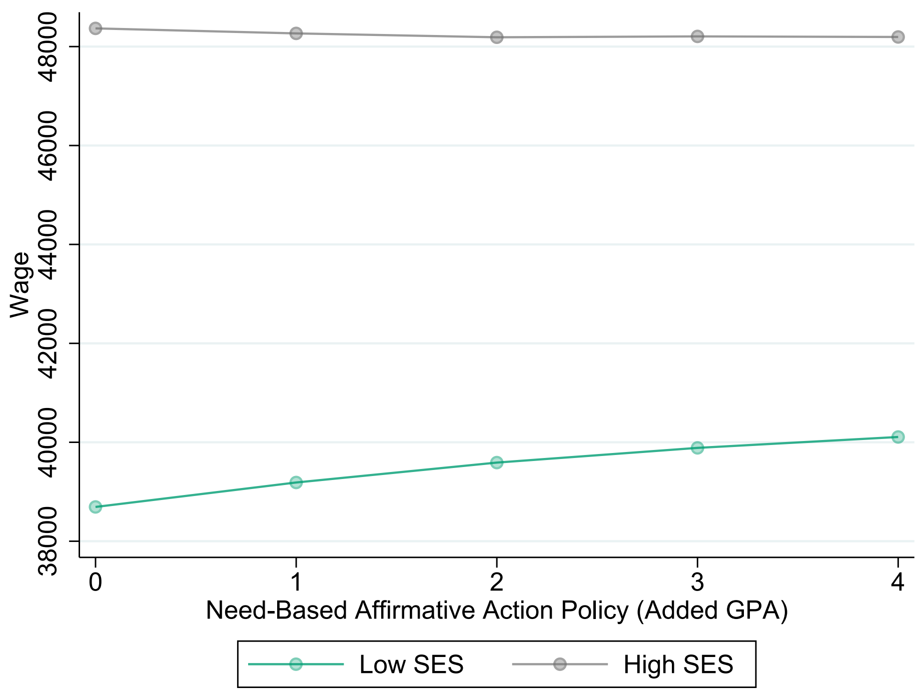
Notes: These figures compare OLS estimates with the model estimates. Panel (a) and (b) plot the degree-specific treatment effects estimates against each other, respectively for low- and high-SES students. Panel (c) and (d) depicts the distributions of estimates, respectively for low- and high-SES. The orange densities depict the Model estimates, while the dashed grey densities depict the OLS estimates.

1.10.11 Counterfactuals

This Appendix shows the impact of changing admission policies on the long-term outcomes of individuals from low- and high-SES backgrounds.

Wage Income

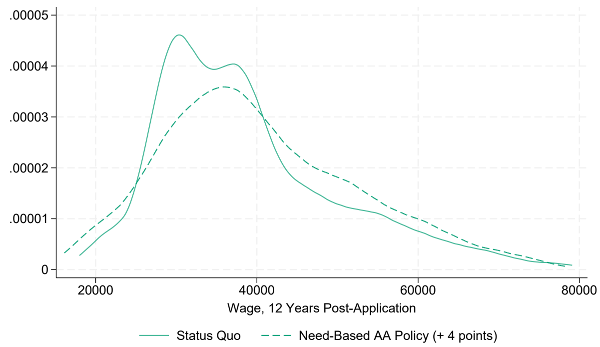
Figure 1.41: Annual Wage Income (USD), Under Need-Based AA Policy



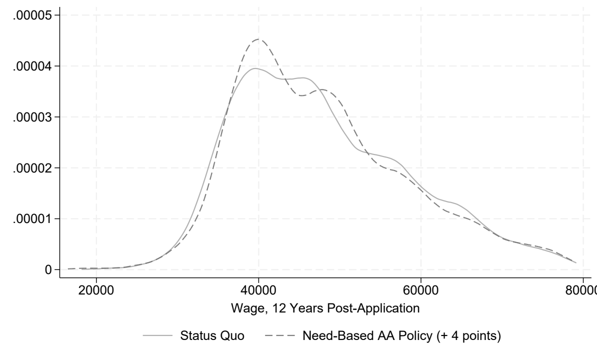
Notes: Effect of need-affirmative admissions on low-SES students (depicted in green) and high-SES students (depicted in gray). The policy under consideration reduces the GPA requirement gradually from 1 to up to 4 points. Each dot represents the average wage as a result of implementing the policy.

Wage Distribution: Before and After Policy

Figure 1.42: Distributional Consequences of Need-Based Affirmative Action



(a) Wage Income Density: Before and After Policy for low-SES



(b) Wage Income Density: Before and After Policy for high-SES

Notes: These figures showcase the distribution of wages under the current status quo and affirmative action policy (dashed lined), which adds 4 additional points to low-SES students. Panel (a) focuses on low-SES students, while panel (b) presents the distributions for high-SES students.

CHAPTER 2

PRICING SCHOOLS AND NEIGHBORHOOD ATTRIBUTES WITH SADEGH ESHAGHNIA AND JAMES HECKMAN

Danish education at all levels is tuition-free. Teacher salaries and expenditures on schools are mandated to be equal. Yet there is substantial variation in teacher quality and student performance across neighborhoods around schools for which parents pay in terms of housing prices. Homophily - sorting by parental education and income into neighborhoods with different peer, teacher and housing attributes explains this phenomenon and not the sorting by preferences for school quality and tax expenditure emphasized in many models of local public goods. Parents buy packages of attributes and not just school quality, which is the emphasis in much of the literature. We develop and apply multiple approaches to identifying parental valuation of measured school quality in the presence of strong neighborhood sorting.

2.1 Introduction

The Scandinavian welfare state is widely regarded as an exemplary system that reduces inequality by providing an education system that is free for all across the schooling spectrum and that equalizes per-pupil expenditures across schools. This equality, while enshrined in law, is undone in practice through the sorting of families and teachers across neighborhoods and not through differences in taxes and spending across districts as emphasized in many models of local public goods (see Arnold and Sieg [2022]).

Because of the placement of schools in neighborhoods, school choice is only one motivation for the sorting that takes place based on multiple attributes. Housing quality, teachers, and adult and child peers are equally important. More than local public goods drives this sorting. Families purchase packages of attributes and not just schooling quality. This feature of neighborhood choice is particularly clear in equal Denmark. Transactions in housing markets and not agreements to tax and spend on public schools explain neighborhood sorting patterns.

To establish these claims, we develop and apply a variety of empirical approaches to estimate how much parents value neighborhood, peer, and teacher quality in terms of their payment for housing with identical attributes. By estimating parental demand for neighborhood quality, we isolate a mechanism producing inequality in intergenerational social mobility in Denmark (Eshaghnia et al., 2022, Landersø and Heckman, 2017), despite strongly egalitarian state policy regarding expenditure. Sorting undoes mandated equality of expenditure.

We reach these conclusions by analyzing rich administrative data on small geographic units of neighborhoods that encompass school enrollment zones. We show sorting of parents, teachers, and students within narrowly defined clusters. We use a variety of approaches to reach our conclusions. (1) Using variation within narrow geographic clusters, we study parental willingness to pay for school quality, produced by peer, teacher quality, and neighborhood by controlling for characteristics that predict housing prices. Cluster indicators are used to control for unobserved neighborhood attributes in a fashion similar to that of Bayer et al. [2007]. (2) To address the potential bias in our estimates arising from selection into neighborhoods and schools on the basis of unobserved variables, we develop and estimate a model of residential choice. (3) We exploit variation in school boundaries over time and the resulting variation in school quality. (4) We apply methods initially developed by Altonji et al. [2005] to bound the impact of omitted variable bias arising from sorting on unobserved housing and neighborhood attributes within these clusters. Specifically we apply extensions of that method by Diegert et al. [2022] that allow for endogenous regressors. In addition, we conduct a number of sensitivity analyses to show that our main estimates remain.

The strength of the evidence is bolstered by the concordance of the estimates from very different approaches in the tradition of Kuznets (see, e.g., Fogel, 1987). No single assumption or methodology drives our empirical conclusions.

Controlling for housing and other neighborhood characteristics, households pay between 2% to 4.3% more for neighborhoods for a one-standard deviation increase in measured school quality. Our preferred estimate of 3.2% implies that households are willing to pay about \$8,000 for a one-standard deviation increase in average test scores for a house with the same attributes.[1] We further decompose parental willingness to pay for school quality into valuation for peers, teachers, and school effectiveness. In contrast with recent literature in the US (e.g. Abdulkadiroğlu et al. [2020]), we uncover positive estimates for these attributes. Households pay more for the entire neighborhood package.

We make several contributions to the literature. Sorting on the basis of preferences and income to produce schools and other local public goods has been studied since Tiebout [1956]. Bénabou [1993, 1996] and Durlauf and Seshadri [2018] analyze how such neighborhood sorting affects child development through provision of school quality. Epple et al. [2020] and Sieg [2020] summarize the literature on the provision of schooling. It focuses on the tax and spending decisions of local governments and their impacts on school expenditure.

Unequal spending by districts is frequently cited as a source of inequality in child outcomes across districts.[2] In Denmark, per-pupil expenditures and teacher salaries are mandated to be equal across public schools except for students with special needs and for cost-of-living adjustments. Thus, Tiebout sorting to raise revenue for financing school quality and local public goods is absent, although strong sorting still occurs.

Our findings shed light on the crucial role of neighborhood sorting driven by preferences for neighborhood quality rather than solely for determining the level of taxes to fund schools.[3]

1. Our estimates of the demand for neighborhood quality are broadly in line with estimates reported for public middle schools in Paris (see Fack and Grenet, 2010) which also has free schooling and mandated equality in expenditure across schools, albeit with more discretion for school and district-specific funding. Like Denmark, Paris has a competitive housing market and residence-based school assignment mechanisms.

2. See, for instance, Jackson et al. [2016], Hyman [2017], Lafortune et al. [2018], Baron et al. [2022], Jackson and Mackevicius [2024].

3. General equilibrium models studying the role of sorting based on preference for peer characteristics include Epple and Romano [2003], Nechyba [2000], Ferreyra [2007], Calabrese et al. [2006].

Sorting is the sole driver of differences in school quality. As a consequence, some households are priced out from good schools. A companion paper, Eshaghnia et al. [2023c], shows the strong effects of this sorting and neighborhood choice on a number of later life outcomes, including child income and educational attainment. Together, these findings add to a large empirical literature which demonstrate that parental choices of peers and neighborhoods are important determinants of student outcomes (e.g. see, Bursztyn and Jensen, 2015, Carrell et al., 2018 and Barrios-Fernandez, 2023 for a recent review).

High-quality teachers in Denmark are attracted to schools with high-quality students and parents and not because of higher salaries.[4] This non-monetary allocation mechanism operates in place of competition among districts to determine salaries or their need to finance local public goods.

Our findings on the importance of parental preferences for peers echoes the literature studying parental preferences and school choice policies in the US, e.g. Rothstein [2006], Abdulkadiroğlu et al. [2020], Agostinelli et al. [2021]. These studies emphasize the central role played by parental preferences for peers, which may limit demand-side pressure for improving school productivity. Contrasting with the results from Abdulkadiroğlu et al. [2020], however, we uncover parental preference for teacher quality and neighborhood peers, beyond just school peers. One byproduct of our analysis is the demonstration that multiple measures of school quality – average test scores, student peer quality, teacher quality, and value-added and parental peer quality – are strongly inter-correlated. A literature that focuses on average test scores misses these other dimensions of parental demand for quality.

Finally, we contribute to the literature by providing new methodological tools to estimate the willingness to pay for school quality in the context of neighborhood sorting. To that end, we develop and estimate a model of residential choice and use distance to grandparents' neighborhood of residence as an exclusion restriction. The underlying assumption is that

4. Flyer and Rosen [1997] discuss a similar mechanism.

household location decision is influenced by grandparents' neighborhood of residence, but does not impact equilibrium house prices. Our model builds on the work of Heckman [1979], Dubin and McFadden [1984], Dahl [2002] and provides a tractable approach in the context of neighborhood selection, where the choice set is potentially large and unobserved. We address the issue of dimensionality arising in polychotomous choice models by proposing and implementing a dimension reduction approach.

2.2 The Public Schooling System in Denmark

Similar to other Nordic countries, the social welfare model in Denmark is based on the principle of free and equal access to public services, regardless of income or economic need (Frelle-Petersen et al., 2020). The Danish system is strongly universal in terms of coverage and manifests some of the highest quality standards in the world (Esping-Andersen et al., 2012). Among OECD countries, Denmark ranks exceptionally well in residents' satisfaction with public services (Thibaut, 2023).[5] With respect to the education system, Danish legislation [2022] provides the national framework through which primary and lower secondary schools operate. It sets common objectives for all municipal schools, alongside requirements regarding the curriculum and the organization of schools.

The Danish schooling system is based on the principle of equal public expenditure for all. Education is provided at no charge in public schools through the university level. Per-pupil expenditure is equalized in Denmark by a system of redistribution across municipalities.[6]

Figure 2.1 displays the distribution of per-pupil expenditure across municipalities. Dan-

5. Around 85% of Danes are satisfied with the judiciary system, health care, and education system.

6. Redistribution is needed since the primary source of municipality revenue (about 70%) is collected from local taxes. Tax bases vary, motivating a redistributive system across municipalities. Residual differences in funding across municipalities are minor after controlling for several municipality features (share of private school students, the share of immigrants (to proxy special needs and language expenses), distance to school (rural areas have less possibility of economies of scale), specific housing types, population size (to approximate returns to scale), trends in the numbers of school-age children (budgets tend to respond to lagged changes in the number of students)). The residual is not correlated with factors such as voting behavior.

ish distributions are much more concentrated than those in the US. Dalsgaard and Andersen [2016] show that more than 50% of the heterogeneity in schooling expenditures across municipalities is due to compensatory finance that accounts for factors such as the fraction of non-westerners across municipalities and special education expenditure.

Despite near equalization in school expenditures and teacher salaries, teachers still sort based on the quality of the students they teach. Differences in school quality are not driven by differences in schooling expenditure. We provide extensive evidence of this sorting in Appendix 2.8.2. There are strong correlations among various school inputs, school value-added, and school average test scores.

Access to public schools in Denmark is residence-based. Each housing unit is part of a school catchment area assigned to a single school. Parents can defy school catchment area rules in certain cases, although the possibility of doing so is contingent on available capacity in alternative schools. We check the sensitivity of our estimates to noncompliance with initial assignments and find that they are robust.

Figure 2.13 of Appendix 2.8.1 shows the variation in our measure of school quality (average test scores) in various specifications reported in Table 2.19. Despite equal per-pupil school expenditures and teacher salaries, there is a great deal of variation in the performance of students across schools, which is due to the spatial sorting of families and teachers along the residence-based assignment of students to public schools.

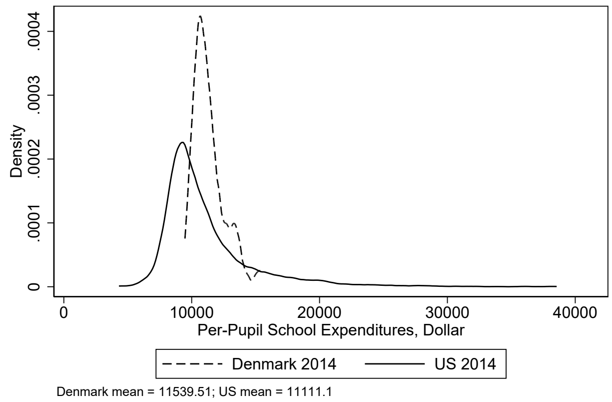


Figure 2.1: Comparing Per-Pupil School Expenditure across Municipalities in Denmark and the US (2014 US Dollar)
Notes: This figure shows the average per-pupil school expenditures in public schools in 2014 for the US and Denmark.

Schools are embedded in neighborhoods through their catchment zones. Expenditures on many local public goods such as police and health, are equalized. Equal schooling is just one manifestation of a strongly equalizing welfare state.

2.3 Our Analytical Framework and Data Base

We use hedonic models to analyze prices paid for neighborhood attributes, including schooling quality, teachers, and peers. An equilibrium is obtained when supply equals demand at each traded quality. The hedonic price function $P(\boldsymbol{z})$ is defined for $\boldsymbol{z} = (z_1, z_2, \ldots, z_n)$, vector of attributes of housing, peers, neighborhood qualities and school characteristics. Specifically, $\boldsymbol{z}$ includes neighborhood public services such as local school quality. The gradient of the hedonic price function with respect to school quality is the equilibrium differential that allocates individuals across locations. Locations with lower-quality amenities, such as low school and peer quality, are predicted to have (*ceteris paribus*) lower housing prices. In this framework, at each point on a hedonic price function, the marginal prices of housing characteristics are individual consumer's marginal willingness to pay for that bundle of characteristics *for those at the point of evaluation*. A long line of research using hedonic demand models builds on Tinbergen [1956] and Rosen [1974] and includes Epple [1987], Ekeland, Heckman, and Nesheim [2004], Bajari and Benkard [2005], and Heckman, Matzkin, and Nesheim [2010]. See Sieg [2020] for a recent definitive survey.

2.3.1 Data

Before turning to specific econometric models used to estimate neighborhood hedonic functions it is useful to describe the rich data we apply to them. We use administrative population-wide data from Statistics Denmark to fit our hedonic models. We know individual residential street addresses. For each housing unit, we observe numerous attributes including assigned school catchment areas, types of buildings, the number of floors, the number of units per building, the number of bedrooms, toilets, and bathrooms, the size of the living area, and the age of the property.

Our analyses of the Willingness To Pay (WTP) use two measures of property prices based on governmental valuations[7] and transaction prices. In both cases, it is defined as the price of the property owned by the biological parents. It is measured at the start of the school year which ends when the final exam is passed, which is typically taken at age 16. We drop all outliers of housing values below the first or above the 99th percentile (keeping observations with house prices above \$44,000 and below \$2.5 million 2010 USD.[8]

We find a pronounced pattern of parental neighborhood choice which we discuss at length in Section 2.6. Parents locate in neighborhoods containing desired schools shortly before, or just after, children are born. The average time spent living in a house is 11 years. About 40% of our sample never move after the first child is born. About 30% move only once. Families

7. Government valuation is computed based on sales of other housing units in the relevant market and adjusted for specific characteristics of the property (such as its square footage).

8. An exchange rate of 6.7 DKK per US dollar is used to obtain the dollar values.

end up living 8 years on average in the house.[9] Thus, most parents with children spend their school-age exposure years (10 years) living in the same house.[10,11] We do not have the full history of schools attended by children. Based on the limited history we observe, we estimate mobility to be around 5%–10% a year between grades 2 and 9.

2.3.2 School Quality Measures

Different measures of school quality are used in the literature, including output-based, input-based, and value-added measures. Our data allow us to consider all three sets of measures, a unique feature of this paper. Value-added measures require tracking student performances over time. Brasington [1999], Downes and Zabel [2002], and Brasington and Haurin [2006] find little evidence that such measures are capitalized into house prices. Input-based measures, such as per-pupil spending, also have weak effects on housing prices [Hanushek, 1986, 1997]. These results have led to the use of student performance measures.

Following the received literature, our main measure of school quality is average student grades at the school level.[12] These are national tests taken by all children in public schools in Denmark. In particular, test scores (broken down by subject) at the school level are available to all in Denmark and advertised on home buying websites.[13] We average student grades on exams taken in their last year of compulsory education. The students' test scores in ninth grade are important in their progression to upper secondary education. After completing the compulsory schooling (ninth grade), students decide whether to leave school or continue to upper secondary education, which consists of either an academic high-school track or

9. See Section 2.6.

10. See Section 2.6 for the distribution of the number of years spent in the home conditional on the number of times moved for this sample.

11. Eshaghnia [2021] shows that the vast majority of neighborhood location decisions are made long before children enter school.

12. See, e.g., Black [1999b], Bayer et al. [2007], Epple and Romano [2003], Avery and Pathak [2021].

13. See for instance www.dingeo.dk.

a vocational apprenticeship-based track. We focus on five cohorts of ninth graders, who attended ninth grade between 2002 and 2006 and whose biological parents are homeowners.

We also analyze school aggregates of teacher quality as measured by their years of experience, GPA rank in teacher's college, tenure at school and an index of quality.[14] We complement these with value-added estimates of schools, following the methodology of Chetty et al. [2014b,a] applied to schools. We control for 8th grade test scores, and a vector of household characteristics, including parental income, wealth, education, criminal record and marital status, as well as child order and gender.[15]

Appendix 2.8.2 discusses a number of measures that capture peer characteristics in schools, including average school student characteristics, such as parental income or education. Appendix 2.8.2 shows the strong correlations between all of the measures mentioned and our main measure of school quality, namely average test scores. This should not come as a surprise, given the strong household sorting we uncover in this paper. We attempt to separately estimate the effects of individual characteristics on house prices.

2.3.3 Defining Neighborhoods

This paper uses various definitions of neighborhoods. We can define neighborhoods narrowly to attain homogeneity and control for omitted characteristics. Table 2.1 provides a brief description of these neighborhoods as well as the numbers of each type of sample.

14. See Appendix Section 2.8.2. This index is based on Gensowski et al. [2021]. Using administrative records, all employees in teaching positions in schools between 2009 and 2016 are matched to their (a) academic records from high school (grades in Danish and Mathematics exams) and university as well as (b) employment records to identify unemployment spells. Children's GPA are then regressed on these teacher's characteristics. A national rank of school quality is then generated using linear regression.

15. School value-added is standardized to have mean zero and a standard deviation of one.

Table 2.1: Alternative Neighborhood Definitions

Neighborhood Definition	#	Description
Region	5	Below federal level. Supersedes municipalities.
Municipality	270	Each municipality lies within a specific region.
School Catchment Area	1475	Each school catchment area contains a single school.
Parish	1561	A neighborhood formed around a specific church.
Cluster	2215	See below for details.
Small Cluster	8008	See below for details.

Notes: Number and definition of different neighborhood concepts in Denmark.

Figure 2.2 depicts the neighborhood partitions used to recover estimates of the WTP for measured school quality. Our main specification uses variations in housing prices and school quality within clusters.[16] Variation in school quality arises from the boundaries of school catchment areas within different clusters. Figure 2.2 depicts, schematically, a large cluster that contains four small clusters. The two shades denote different school catchment areas which are bordered by the dashed line. The median cluster spans an area of 0.34 square miles and comprises 985 households, while the median small cluster spans 0.08 square miles and comprises 245 households in 2004.[17]

16. In the remainder of this paper, we sometimes refer to these clusters as "neighborhoods".

17. Further summary statistics and details on the construction of these clusters and expansion to different years are discussed in Appendix 2.8.1.

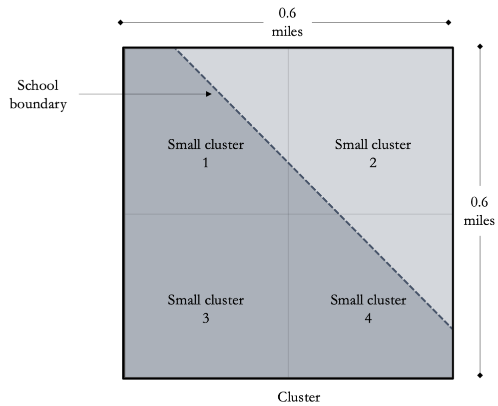


Figure 2.2: School Catchment Areas and Clusters

Notes: This figure depicts one cluster, with boundaries of the four small clusters represented with solid lines. The dashed line represents the boundary of two school catchment areas, which are illustrated in two different shades. The variation in school quality we utilize arises from variations in school catchment area boundaries crossing within clusters.

Our approach to constructing clusters builds on the research of Damm and Schultz-Nielsen [2008]. We use data from 1985 to 2004, using the following criteria: (1) clusters correspond to geographical areas within which an individual has social contact [18], and (2) they remain the same over time.[19] Damm and Schultz-Nielsen [2008] construct clusters on the basis of 431,233-hectare cells (100m x 100m) which exhaust Denmark's surface area. They aggregate these cells to meet confidentiality requirements in terms of the number of households per cluster.[20] Clustering is defined based on housing type and ownership information.[21]

18. In practice, this implies that two neighbors separated by physical barriers such as water, large roads, or forests, would not be included in the same cluster.

19. These clusters define geographic areas which do not vary over time. Their composition varies as individuals move in and out of the cluster.

20. Cluster sizes need to have at least 150 households for analysis of residential segregation and a minimum of 600 households for descriptive purposes, as required by Statistics Denmark.

21. Housing type in the register data is divided into four categories: farmhouse or detached house; town-house or small block of flats; large block of flats; second home or other house. Ownership information is also broken down into four categories, namely private ownership, privately owned rental, publicly owned rental, and private cooperative housing. In the calculation of which hectare cell is most similar, the latter is given a weight of 70%, while the former is given a weight of 30% in forming homogeneous clusters.

Moreover, visible features and geographic barriers such as lakes, forests, or major roads were used in defining the different boundaries between clusters (which is not always possible in less dense areas). This ensures that within-cluster differences in house prices are not driven by these barriers.

2.3.4 Control variables

We supplement our data on school quality and property values by using Denmark's rich administrative data to capture a wide range of characteristics at a small neighborhood level.[22] We use variables pertaining to the household, including income,[23] education level,[24] crime[25], family structure[26] and ethnicity[27]. We aggregate these measures both at the small cluster level and at the small-cluster-by-school-catchment-area level, for our analyses.[28] Moreover, we also include a host of housing characteristics.[29]

2.4 Neighborhoods

Our data are unusually rich and enable us to disaggregate to a granular level.

22. Summary statistics are given in Appendix 2.8.1.

23. We use net household after tax and transfers income.

24. We use years of completed education. When computing the small neighborhood education level, we first compute the maximum number of years of education at the household level. We then aggregate at the small cluster level.

25. We use an indicator for whether an individual ever committed a crime or not. We exclude traffic-related crimes.

26. We include a measure of marital status and non-intact household structure. For the latter, a family structure is considered intact if, during the first 18 years of a child being born, both parents are present.

27. We use the fraction of non-western foreigners in a neighborhood. Non-western foreigners are individuals who have at least one parent from a non-western country.

28. We report the correlation between the different neighborhood attributes in Appendix 2.8.2.

29. For example, type of building, the number of floors, the number of units per building, the number of bedrooms, toilets, and bathrooms, the size of the living area, and the age of the property.

2.4.1 Neighborhood Homogeneity

We now discuss the homogeneity of the clusters selected. Attributes of houses on both sides of school catchment boundaries within clusters are very similar. We analyze the spatial decomposition of inequality in housing types and characteristics across neighborhoods in Denmark using Theil's T Index and variance measures. We focus on two characteristics of buildings across neighborhoods: the number of apartments in the building, and the number of floors.

Figure 2.3 shows the decomposition across neighborhoods by different units of neighborhood, i.e., municipality, parish, cluster, and small cluster levels. Panel (a) analyzes the number of apartments in each property. At the municipality level, only about 20% of inequality can be attributed to the between-neighborhood component. The share of between-neighborhood component increases to about 90% when we analyze the inequality in small clusters.[30] Panel (b) considers the number of floors of each property and shows a similar pattern. The share of within-neighborhood variation in housing attributes decreases from about 60% to less than 20% when focusing on small neighborhoods rather than the municipalities. Variation in the housing types in the narrowly-defined neighborhood units is small. Variation in house prices inside these areas is not driven by differences in the housing structures. We reach similar conclusions when we use Theil's T Index decompositions.[31] We use a rich set of housing characteristics in our analysis of housing prices.[32]

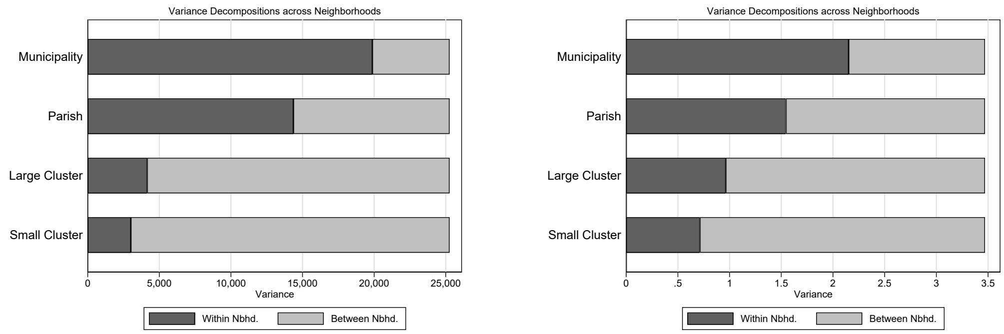
30. Appendix 2.8.4 discusses how we use Theil's T Index to compute within- and between-neighborhood inequality.

31. See Figure 2.32 of Appendix 2.8.4.

32. We also analyze the spatial decomposition of income inequality across neighborhoods in Denmark using the Theil's T Index in Appendix 2.8.4.

Figure 2.3: Variance Decomposition of Housing Characteristics across Neighborhoods

(a) Number of Apartments (b) Number of Floors



Notes: This figure presents the variance decomposition of building characteristics across different neighborhood units in Denmark. Panel (a) focuses on the number of apartments and Panel (b) shows the statistics for the number of floors. See Appendix 2.8.4 for details.

2.4.2 Neighborhood Sorting

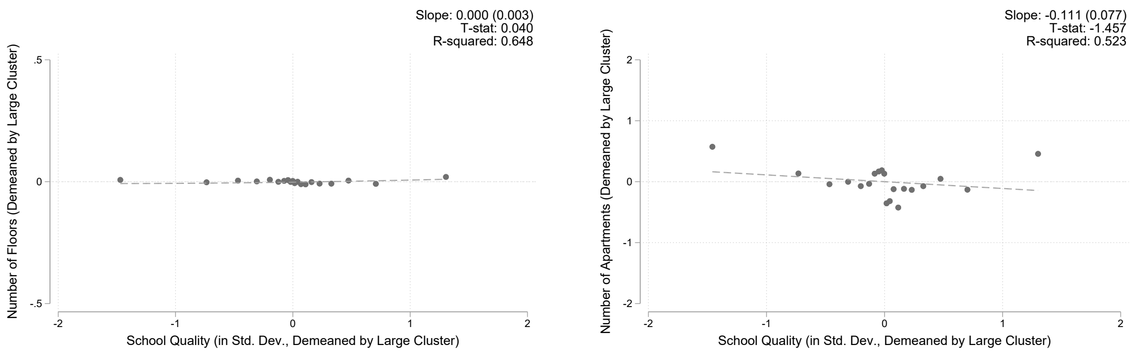
Housing characteristics are relatively homogeneous within clusters. This comes as no surprise, given that the algorithm generating these clusters minimizes the within-variance in housing types. Nonetheless, there is strong sorting of individuals based on their characteristics, as well as the characteristics of their neighbors. This is in line with our analysis in Appendix 2.8.4, which shows that although clusters are relatively homogeneous, there is still a significant variation in other attributes within clusters. For example there is sorting across school boundaries, which occurs within clusters. This enables us to price the value of the attributes like schooling that do vary holding housing attributes fixed.

To assess the level of sorting within clusters in Denmark, we plot the relationship between measured school quality and different attributes, both at the individual and household level after controlling for neighborhood-by-cohort fixed effects. This analysis examines whether unobserved neighborhood attributes vary within clusters.

Figure 2.4 shows that conditional on small neighborhood characteristics, and neighborhood-by-cohort fixed effects, housing characteristics are largely uncorrelated with differences in school quality across schools.[33] The neighborhoods we study are relatively homogeneous, at least regarding their housing characteristics.

Figure 2.4: Sorting within Clusters (Housing Characteristics)

(a) Number of Floors (b) Number of Apartments



Notes: This figure presents a binned scatter plot (with 20 equal-sized groups) depicting the relationship between demeaned housing characteristics and demeaned school quality within clusters. Each panel is constructed by regressing various housing characteristics on school quality, controlling for neighborhood-by-cohort fixed effects and small neighborhood attributes—average income, years of education, fraction married, non-westerners, crime, and non-intact households. Standard errors corrected for clustering at the cluster-cohort level are reported in the top right corner. The slope of the linear relationship and corresponding T-statistic are further reported in the top right corner. For additional plots of this nature, see Appendix 2.8.2.

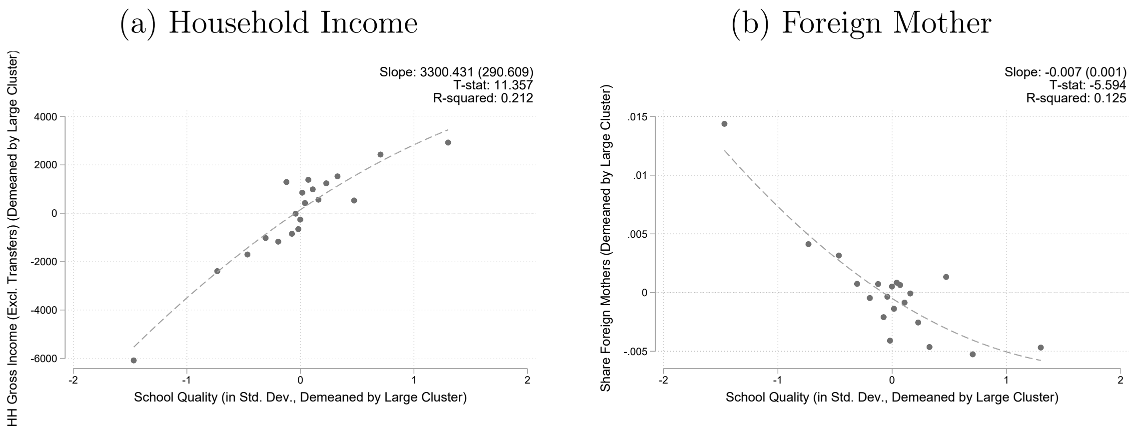
Still, households sort across neighborhoods in Denmark based on their preferences for vectors of neighborhood attributes. This is evidenced by the relationship between individual characteristics, as well as small neighborhood characteristics with school quality, within clusters. On average, households living in higher test score school catchment areas (within clusters), have higher gross income, as seen in Figure 2.19.[34] Moreover, within clusters, small neighborhoods associated with a better school catchment area, are on average more educated, have higher income and more stable family structures, while having less criminality and a

33. See also Figure 2.16, Figure 2.18, and Figure 2.20 of Appendix 2.8.2. Using per capita measures barely changes the figures shown here.

34. See also Figure 2.18 of Appendix 2.8.2.

smaller fraction of non-western foreigners.[35] Overall, this evidence showcases the importance of controlling for unobserved neighborhood characteristics through our cluster fixed effects strategy as well as small neighborhood attributes.[36] We exploit this variation to identify the hedonic model.

Figure 2.5: Sorting within Clusters (Individual Characteristics)



Notes: This figure presents a binned scatter plot (with 20 equal-sized groups) depicting the relationship between demeaned individual characteristics and demeaned school quality within clusters. Each panel is constructed by regressing various individual characteristics on school quality, controlling for neighborhood-by-cohort fixed effects, small neighborhood as well as housing attributes—the type of building, the number of floors, the number of units per building, the number of bedrooms, toilets and bathrooms, the size of the living area, and the age of the property. Standard errors corrected for clustering at the cluster-cohort level are reported in the top right corner. The slope of the linear relationship and corresponding T-statistic are further reported in the top right corner. For additional plots of this nature, see Appendix 2.8.2.

2.5 Empirical Methodology

Our main estimating equation relates house prices to a vector of housing and neighborhood characteristics, including measured school quality, student and parental peer attributes and

35. See Figures 2.20 and 2.21 of Appendix 2.8.2.

36. Clusters with single schools are dropped from the analysis since there is no variation in school quality remaining.

housing attributes.[37] Including a set of cluster fixed effects controls for unobserved neighborhood heterogeneity and allows us estimate the following hedonic price regression, which is assumed to be linear in its arguments:[38]

$$\ln(p_{imkt}) = \alpha + \beta S_{mkt} + \boldsymbol{\gamma_1'} \boldsymbol{H}_{imkt} + \boldsymbol{\gamma_2'} \boldsymbol{X}_{imkt} + \rho_{kt} + \varepsilon_{imkt}, \tag{2.1}$$

where $\ln(p_{imkt})$ denotes log property values of family i whose child attends school m in cluster k in cohort t. S_{mkt} denotes our measure of quality for school m in cluster k. Housing and neighborhood characteristics are denoted by $\boldsymbol{H}_{imkt}$ and $\boldsymbol{X}_{imkt}$.[39] Neighborhood-by-cohort fixed effects are denoted by ρ_{kt}. Finally, ε_{imkt} represents unobserved neighborhood and housing attributes assumed to be independent and identically distributed for a given t and uncorrelated with the regressors. We henceforth use a compact notation and define $\boldsymbol{\mathcal{Z}} = [S, \boldsymbol{H}, \boldsymbol{X}, \rho]$, so $\ln(p_{imkt}) = \boldsymbol{\omega'} \boldsymbol{\mathcal{Z}} + \varepsilon$.

Identification of hedonic models possess a great challenge to empirical researchers. See Ekeland et al. [2004] for a discussion. A general analysis remains to be developed. Special cases abound with the most commonly used assuming that people are alike despite the mountains of evidence against it.

This paper tackles these issues in multiple ways exploiting the richness of our data. All of our strategies exploit the granular nature of our data on housing and neighborhoods. Conditioning narrowly on neighborhood traits eliminates endogeneity concerns if (a) conditioning eliminates any remaining heterogeneity on unobserved preference and attribute variables as in the proximity theorem of Fisher [1966b] or (b) failing that, the variables remaining after

37. Figure 2.14 of Appendix 2.8.2 presents the relationship between property values at the school level and school quality.

38. We also run a local linear regression of this specification in Appendix 2.8.5. We show that the log-linear specification used in the main text accurately represents the price function.

39. Housing characteristics include the type of building, the number of floors, the number of units per building, the number of bedrooms, toilets, and bathrooms, the size of the living area, and the age of the property. Neighborhood characteristics include household net income and education, as well as fraction married, non-intact households, crime, and non-western foreigners.

conditioning are uncorrelated with measured attribute variables. Strategy (b) is pursued in Bayer et al. [2007].

Another approach that is based on the work of Black [1999b] conditions finely on housing attributes using data at boundaries of school catchment zones with different policies about school quality. The hope is that location at the boundary is random although that assumption is sometimes successfully challenged in empirical studies.We also use a bounding strategy developed by Altonji et al. [2005], refined by Oster [2019] and fundamentally extended by Diegert et al. [2022] to allow for endogeneity of the regressors in (1). This approach tackles endogeneity due to sorting by generating plausible intervals for the estimated parameters.

Fixed effect strategies, while commonly used, ignore the consequences of the choice of neighborhood for estimates. As a supplement, we jointly model the choice of neighborhoods along with the pricing of neighborhood attributes. This controls for selection at the granular level even within narrowly defined neighborhoods. We obtain a broad consensus on the willingness to pay for schools across these alternative methods with the selection-corrected estimates being the smallest. We decompose our estimated willingness-to-pay for quality schooling into interpretable aspects of schooling.

2.5.1 The Consequences of Analyzing More Granular Neighborhood Sorting

This section motivates a strategy of using smaller neighborhood districts to enforce homogeneity in house quality. This gives credibility to an estimation strategy for identifying the WTP parameter, using ever-contracting and evermore homogenous neighborhoods. Table 2.2 analyzes the R-squared of a set of regressions of house prices on measured school quality, with fixed effects at different neighborhood-by-cohort levels. To define the neighborhood unit, we use five alternatives, namely regions, municipality, parish, cluster, and small cluster (by diminishing order of size).

The table shows that the unexplained variation in house prices decreases when we move

Table 2.2: R-Squared for Regressions of Log Housing Price on School Quality with Fixed Effects (FE) at Various Neighborhood Levels

	Region FE	Munic. FE	Parish FE	Cluster FE	Small Cl. FE
	(1)	(2)	(3)	(4)	(5)
Without controls					
Full Sample	.19	.28	.35	.37	.52
Copenhagen Area	.19	.27	.37	.54	.71
With controls					
Full Sample	.45	.49	.53	.54	.64
Copenhagen Area	.48	.51	.55	.65	.77
# FEs (full sample)	25	1,341	7,521	9,982	31,619

Notes: Column (1) presents the R-squared of the regression of house prices on school quality using a region-by-cohort fixed effect model. Column (2) presents the R-squared of the regression of house prices on school quality using a municipality-by-cohort fixed effect model. Column (3) reports the R-squared of the regression of house prices on school quality using a parish-by-cohort fixed effect model. Column (4) shows the R-squared of the regression of house prices on school quality using a cluster-by-cohort fixed effect model. Column (5) presents the R-squared of the regression of house prices on school quality using a small cluster-by-cohort fixed effect model. We present two sets of results – with and without neighborhood and housing controls. For each of these specifications, we provide the corresponding number of area-by-cohort fixed effects for the full sample. We also provide a breakdown of the R-squared for the full sample as well as for a subset of our data focusing on the Copenhagen metropolitan area.

towards more narrowly-defined geographic units. For example, the R-squared almost doubles when we shift from using regional fixed effects to using cluster fixed effects. As Table 2.2 shows, the R-squared of the regression of house prices on measured school quality using the micro-level data increases from 0.19 using region fixed-effects to 0.28 using municipality fixed effects, to 0.35 for parishes, to 0.37 for clusters, and to 0.52 for small cluster fixed-effects.

Inducing homogeneity in constructed neighborhoods works best in more densely populated areas where sizes of clusters are smaller and sorting is likely to be finer. When we use cluster fixed effects we capture 54% of the variation in house prices, which increases to 71% for small cluster specifications, more than three times bigger than the variation explained by a broad neighborhood unit (i.e., region) fixed effect. This is more pronounced in urban areas, such as the Copenhagen metropolitan area.[40] In particular, adding covariates to the hedonic regression in the Copenhagen metropolitan area increases the R-squared to 77%.[41]

Note that even inclusion of all unobserved factors in the regression does not necessarily result in an R-squared of 1. In the presence of measurement errors in house prices and/or some idiosyncratic variation in house prices, the variance of house prices would not be fully explained even if one included the full set of economically relevant unobservables in the regression. Using the two available measures of property prices – governmental valuations and transaction prices – we can assess the role of measurement error in house prices on the R-squared of our hedonic regressions.[42] Measurement errors in property prices might fully account for the unexplained variation in the hedonic equation in Table 2.2.[43] We obtain very similar estimates at other levels of disaggregation of neighborhoods.

The reduction in the variance of the residuals when we choose neighborhoods more finely suggests application of the proximity theorem of Fisher [1966b] may be appropriate especially to the small clusters in Copenhagen. The theorem states that as the residual variance of equation (1) becomes small relative to the variance of the explanatory variables, asymptotic bias becomes small as well. We apply this result to estimate neighborhood and school attributes in this paper but with two cautions. First, the residual variance is still strongly bounded away from zero even at the small cluster level. Second, the remaining variance of

40. About 32% of the population live in the Copenhagen metropolitan area.

41. To test whether using government valuations reduces variability in the data, we compare the R-squared of this specification to one using transaction data. These R-squared are similar.

42. Let y^*, y^g, and y^t denote, respectively, the log property price without measurement error, the log government price, and the log transaction price, respectively. Write $y^g = y^* + \epsilon^g$ where ϵ^g denotes the measurement error in government price, and $y^t = y^* + \epsilon^t$ where ϵ^t indicates the measurement error in transaction prices. Under independence of the measurement errors and the true (latent) values, we can identify the variance of each source of measurement error. Note that, $\text{Var}(y^g) = \text{Var}(y^*) + \text{Var}(\epsilon^g)$. Similarly, $\text{Var}(y^t) = \text{Var}(y^*) + \text{Var}(\epsilon^t)$. Finally, $\text{Cov}(y^g, y^t) = \text{Var}(y^*)$. We find $\text{Var}(y^g) = .40$, $\text{Var}(y^t) = .44$, and $\text{Cov}(y^g, y^t) = .18$, respectively. This implies $\text{Var}(\epsilon^g) = .22$ and $\text{Var}(\epsilon^t) = .26$, respectively, which are bigger than the unexplained variation in house prices (0.13 and 0.15, respectively).

43. See Kotlarski [1967] who show that under mild conditions, it is possible to nonparametrically identify the distributions of all three components of error.

unobservables can still lead to selection bias in the choice of neighborhoods - an ever present danger of fixed effects approaches. We apply several methods to address this bias.

2.5.2 WTP for Measured School Quality

The remaining variability in narrow neighborhoods motivates us to try a variety of estimate strategies based on different identifying assumptions which we execute. These strategies take various approaches to the endogeneity of the regressors in (2.1) and control for dispersion within clusters. Some approaches focus on only a subset of the coefficients. The remarkable conclusion of this analysis is that once fine conditioning on neighborhood characteristics is made, estimates and bounds across procedures based on different assumptions are in close agreement.

Cluster Fixed Effects Strategy

Table 2.3 reports a set of estimates for various econometric specifications. Results from OLS without controls are reported in column (1). A one–standard deviation increase in student test scores is associated with an increase in house prices of 17.1%. Our second OLS specification builds the full hedonic model and adds a vector $\boldsymbol{X}$ of small neighborhood characteristics, such as average income and education, crime, as well as housing characteristics $\boldsymbol{H}$. The set of housing characteristics includes the type of building, the number of floors, the number of units per building, the number of bedrooms, toilets, and bathrooms, the size of the living area, and the age of the property. The coefficient on student test scores substantially decreases from 0.17 to 0.02, emphasizing that persons buy bundles of neighborhood and housing characteristics.

Table 2.3: Comparing OLS and Fixed Effect (FE) Estimates
Dependent Variable: Log House Price

	OLS	Nbhd Controls	Large Cl. FE	Controls and FE
	(1)	(2)	(3)	(4)
School's Average Test Scores	0.171	0.022	0.047	0.032
	(0.006)	(0.003)	(0.003)	(0.003)
Living Area		0.004		0.005
		(0.000)		(0.000)
Number of Floors		-0.055		-0.036
		(0.006)		(0.006)
Number of Rooms		0.002		0.004
		(0.002)		(0.002)
Number of Apartments		-0.001		-0.001
		(0.000)		(0.000)
Age of Building		-0.003		-0.004
		(0.000)		(0.000)
Number of Toilets		0.024		0.012
		(0.004)		(0.003)
Number of Bathrooms		0.051		0.040
		(0.004)		(0.004)
Log HH Net Income (at Nbhd Level)		0.462		0.088
		(0.027)		(0.026)
HH Years of Educ. (at Nbhd Level)		0.299		0.083
		(0.005)		(0.006)
Share Criminals (at Nbhd Level)		0.000		-0.000
		(0.000)		(0.000)
Share Married HH (at Nbhd Level)		-1.122		-0.379
		(0.034)		(0.035)
Share non-Westerners (at Nbhd Level)		-1.233		-0.175
		(0.267)		(0.193)
Share of Non-Intact HH (at Nbhd Level)		0.395		-0.006
		(0.026)		(0.027)
Large Cluster-Year FE	No	No	Yes	Yes
Year FE	Yes	Yes	No	No
Observations	126301	124598	126301	124598
R^2	0.078	0.404	0.366	0.540

Notes: Columns (1) and (2) show an OLS specification as a benchmark, while columns (3) and (4) show two different specifications with cluster-by-cohort as fixed effects. The sample includes all homeowners with children attending ninth grade between 2002 and 2006. Property values are in logs and school quality (average test scores) is standardized such that the coefficients can be interpreted as the WTP, in percentage terms, for a one–standard deviation increase in school quality. Standard errors corrected for clustering at the school-cohort level are reported in parentheses. Neighborhood characteristics include household net income, and education as well as fraction married, non-intact households, crime, and non-western foreigners. Housing characteristics include the type of building, the number of floors, the number of units per building, the number of bedrooms, toilets, and bathrooms, the size of the living area, and the age of the property. Singleton groups (groups where clusters include only one school) were kept, but results are robust to dropping them, as their number is small. In model (4), about half of the explained variation is due to the fixed effects, while the remaining is due to the controls.

We also use a cluster fixed effects strategy to assess the role of unobservables, in the third and fourth columns of Table 2.3. Compared to the OLS regression with no covariates, we see that accounting for neighborhood characteristics and housing significantly reduces the estimated effect of the attribute of school quality on house prices.

The final column adds both neighborhood fixed effects, (ρ), as well as the small neighborhood and housing characteristics, $\boldsymbol{X}$ and $\boldsymbol{H}$, to recover the price of school quality. For the average house between 2002 to 2006, our estimate of 3.2% implies that a one–standard deviation increase in average test score increases house prices by about $8,000,[44] holding housing and neighborhood characteristics constant.[45]

For U.S. data, the fact that this estimate is substantially lower compared to the estimate obtained from controlling only for neighborhood fixed effects suggests that households not only care about the characteristics of their neighborhood at large[46] but also their local neighborhood attributes and sort on that basis, consistent with that of Bayer et al. [2007].

School Boundary Changes as Exogenous Variation

As an additional source of variation, we exploit changes in schooling boundaries that occur in Denmark. Using within-old-school-boundaries-by-small-neighborhood variation in school quality, following Billings et al. [2014], we exploit the fact that reassigned students live in the same neighborhood thereby reducing potential biases arising from parents' neighborhood selection based on school quality. School boundaries change in ways that parents may not be able to predict. Variation in school quality arises from changes in school boundaries

44. This amounts to about $10,000 at mean house price in 2015, the most recent year of data availability.

45. We also estimate a specification where we add 8th grade average score (for available years – post 2010). The coefficient on 8th grade average score stands at 0.008 and is statistically significant, akin to a precise estimate of 0. In this regression, the coefficient on 9th grade average test score does not vary much compared to one without 8th grade average test score as a regressor (0.026 vs. 0.031 holding the sample identical). This is not a surprise, as 9th grade test scores are much more salient: they determine entrance into upper secondary schools.

46. Recall that the median size of these neighborhoods is around 0.34 square miles.

which cause some students within the old school boundary (and within a small cluster) to attend different schools. Baseline school boundaries are determined for the earliest time we observe them, i.e., in 2006.[47] We then focus on the variation experienced by cohorts between 2007 and 2015. Holding housing attributes and neighborhood fixed, the estimated impact of school quality on house prices is 2.2% (statistically significant, p-value < 0.01).

2.5.3 WTP for Neighborhood Quality

This section provides estimates of the WTP for the entire neighborhood package. Let $D \in \{0, 1\}$ be a variable that is equal to 1 if a household sorts on the high quality side of the school catchment area or 0 if the household sorts on the low quality side, within a cluster.[48] We estimate the following specification:

$$\ln(p_{imkt}) = \alpha + \beta D_{mkt} + \rho_{kt} + \varepsilon_{imkt}, \tag{2.2}$$

where ρ_{kt} denotes a cluster-by-cohort fixed effect.

Here, β is interpreted as the payment made by households for living on the high quality side of the cluster, which is the payment for the neighborhood, including school quality. In this case, our estimate stands at 5.2%. This higher estimate is consistent with parental choices of neighborhoods and investment in children. We measure the average difference in house prices across the school districts within the clusters.

47. We note that the sample we use here deviates from the sample used for the main estimating equation, given data availability. We describe how the sample is constructed beyond 2006 and up to 2015 in Appendix 2.8.1.

48. In this analysis, we keep only clusters that contain two school catchment areas. We note that on average, a change from a low to high quality school catchment, within a cluster, induces a 1 standard deviation increase in measured school quality.

2.5.4 Decomposing the Role of Peers, Teachers Quality and Value-Added

This section decomposes the overall effect of school quality on house prices, into attributes of peers, teachers and school value-added.

Understanding parental preferences for these different schooling attributes is key to assessing the effect of school choice policies on school quality improvements. In the US, recent education reforms have increase the choice sets of schools available to parents, driven in part by the idea that competitive pressure is likely to generate increases in school productivity and therefore students' outcomes [Friedman, 1962]. However, in the absence of parental preferences for effective schools, policies that increase parents' power to choose schools would not necessarily generate these improvement in school quality [Rothstein, 2006, Abdulkadiroğlu et al., 2020].

To investigate parental preferences for school attributes, we include measured school quality, alongside peer and teacher characteristics, as well as school value-added. To measure the quality of the student body, we average over students' birth weight at school.[49] To consider neighborhood peers, we construct a similar measure, averaged at the cluster level. With regards to teacher quality, we focus on teacher GPA rank at teacher's college, teacher years of experience, and teacher tenure at school.

Finally, we construct a measure of school value-added, following the methodology of Chetty et al. [2014b,a] applied to schools. We control for 8th grade test scores, and a vector of household characteristics, including parental income, wealth, education, criminal record and marital status, as well as child order and gender.[50]

We build on several specifications to show the stability of the estimated coefficients. In Table 2.5, the first two columns present a specification where we do not control for cluster-by-year fixed effects. The first column includes only housing attributes, while the second

49. Previous papers have shown that birth weight is significantly correlated with later outcomes in adulthood (see, e.g., Black et al., 2007).

50. School value-added is standardized to have mean zero and a standard deviation of one.

further adds neighborhood attributes.

Table 2.4: Regression Results

	OLS	Nbhd Controls	Large Cl. FE	Controls and FE
	(1)	(2)	(3)	(4)
School's Average Test Scores	0.263	0.070	0.054	0.035
	(0.008)	(0.005)	(0.005)	(0.004)
School Value-Added	-0.066	-0.047	0.171	0.090
	(0.063)	(0.036)	(0.036)	(0.033)
Teacher Years of Experience	-0.033	-0.012	0.005	0.004
	(0.002)	(0.002)	(0.001)	(0.001)
Teacher Rank in Teacher's College	0.193	0.016	0.046	-0.002
	(0.054)	(0.032)	(0.031)	(0.027)
Teacher Tenure at School	0.031	0.002	-0.005	0.003
	(0.023)	(0.015)	(0.013)	(0.012)
Birth Weight (at School Level)	0.017	0.020	0.013	0.027
	(0.025)	(0.020)	(0.014)	(0.018)
Living Area		0.005		0.006
		(0.000)		(0.000)
Number of Floors		-0.065		-0.031
		(0.008)		(0.008)
Number of Rooms		0.006		0.004
		(0.002)		(0.002)
Number of Apartments		-0.001		-0.001
		(0.000)		(0.000)

A second set of specifications considers cluster-by-year fixed effects, to alleviate concerns that our estimates are picking up parental preferences for local neighborhood attributes that are unobserved, as in our main specification. Column (3) includes housing characteristics, while column (4) adds small-cluster-level attributes. As previously, we reocgnize the importance to control for unobserved heterogeneity through our cluster fixed effects approach.

Our findings show that parents value school peers teacher attributes and school value-added. In particular, we find that average school birth weight is capitalized into house prices.[51] This is also the case for school value-added, where a one SD increase in school

51. This is not precisely estimated.

value-added increases house prices by 9%. Finally, parents also value teacher experience with an additional year of experience increasing house prices by 0.4%.

Table 2.5: Regression Results, Cont'd

	OLS	Nbhd Controls	Large Cl. FE	Controls and FE
	(1)	(2)	(3)	(4)
Age of Building		-0.004		-0.004
		(0.000)		(0.000)
Log Net Household Income (at Nbhd Level)		0.551		0.195
		(0.034)		(0.032)
HH Years of Educ. (at Nbhd Level)		0.265		0.070
		(0.007)		(0.008)
Share Criminals (at Nbhd Level)		0.000		0.000
		(0.000)		(0.000)
Share Married HH (at Nbhd Level)		-1.506		-0.535
		(0.042)		(0.048)
Share non-Westerners (at Nbhd Level)		0.725		-0.026
		(0.196)		(0.210)
Share of Non-Intact HH (at Nbhd x School Level)		0.321		-0.047
		(0.034)		(0.036)
Birth Weight (at Nbhd Level)		0.005		0.004
		(0.007)		(0.007)
Large Cluster-Year FE	No	No	Yes	Yes
Year FE	Yes	Yes	No	No
Observations	83780	75405	83780	75405
R^2	0.157	0.420	0.408	0.539

Notes: Columns (1) and (2) show an OLS specification as a benchmark, while columns (3) and (4) show two different specifications with cluster-by-cohort as fixed effects. The sample includes all homeowners with children attending ninth grade between 2002 and 2006. Property values are in logs and school quality (average test scores) is standardized such that the coefficients can be interpreted as the WTP, in percentage terms, for a one–standard deviation increase in school quality. Standard errors corrected for clustering at the school-cohort level are reported in parentheses. Neighborhood characteristics include household net income, and education as well as fraction married, non-intact households, crime, and non-western foreigners. Housing characteristics include the type of building, the number of floors, the number of units per building, the number of bedrooms, toilets, and bathrooms, the size of the living area, and the age of the property. Singleton groups (groups where clusters include only one school) were kept, but results are robust to dropping them, as their number is small. In model (4), about half of the explained variation is due to the fixed effects, while the remaining is due to the controls.

2.5.5 Extensions and Sensitivity Analyses

This section conducts a sensitivity analysis to show that our main estimates of the WTP for measured school quality and associated neighborhood quality remain robust.

School Catchment Area Boundaries and Immediate Neighbors

The first set of sensitivity analyses we present addresses the potential concern that school boundaries are not necessarily adjacent within our clusters, given the small geographical area they span. Data on the assigned school catchment area available from 2006 to 2015 allows us to verify that 80% of the clusters in that period are composed of at least two distinct school catchment areas. Column (1) of Table 2.6 reports estimates based on this sample using the same specification as in (2.1).

About 40% of small clusters have at least one set of adjacent schools. Using these small clusters, we can address the concern that our estimates are potentially biased due to individuals sorting based on characteristics that are even more local than what we control for in the previously defined small neighborhood (median area of 0.08 square miles) covariates. To investigate this, we replace cluster fixed effects with small cluster fixed effects in Equation (2.1). Moreover, we replace the small cluster-level characteristics with attributes computed at the small-cluster-by-school-catchment-area level. Figure 2.6 visually depicts this strategy. Our estimate of price of measured school quality for such specification is reported in column (2) of Table 2.6. These include a set of controls for housing attributes, as in our main specification.

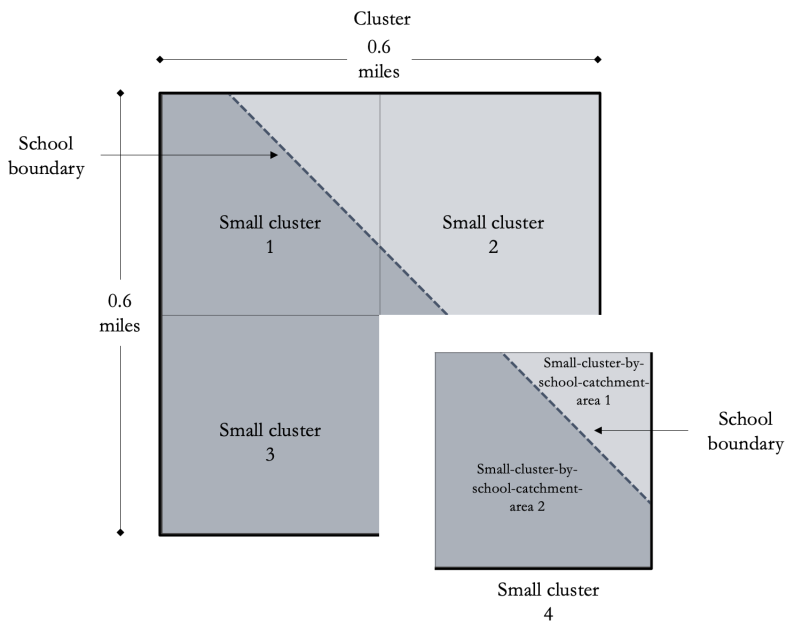


Figure 2.6: School Catchment Areas and Small Clusters

Notes: This figure depicts one cluster, with boundaries of the four small clusters represented with solid lines. The dashed line represents the boundary of two school catchment areas, which are illustrated in two different shades. In this robustness check, we utilize variation in school quality within small clusters and add small-cluster-by-school-catchment-area level characteristics, as depicted in the bottom right corner of the figure.

These two latter specifications are closely related to the idea of Boundary Discontinuity Design (BDD) of Black [1999b] since we use only variation in very close proximity to school boundaries. In fact, for the latter specification, which uses small cluster fixed effects, we use the variation particularly close to the boundary—retaining variation in school quality and house prices that are no further apart than within a 0.08-square-mile cluster. This ensures that houses are not only close to the boundary but also that they are in close proximity to each other. This is an important benefit of our data. The first two columns of Table 2.6 report our estimates of these two specifications. We find estimates of 3.9% and 3.4%, respectively, in line with our main specification.

Defiers

We address concerns regarding the presence of school catchment area defiers. Defiers are households that live in a specific school catchment area, but send their children to a school in a different catchment area. Given our data on school catchment between 2006 and 2015, we are able to examine the importance of defiers in Denmark, as well as their potential impact on our estimate.

We analyze defiers in two distinct ways.[52] First, we analyze a sample that drops all individuals who do not attend the most frequently attended school in a given large-cluster-by-school-catchment-area (reported in column (3) of Table 2.6) or in a given small-cluster-by-school-catchment-area (reported in column (4) of Table 2.6).[53] Second, we drop all cluster-by-school-catchment-areas that have *any* defiers. This leads us to drop about 50% of the sample when using clusters with any defiers, while dropping 25% of the sample when dropping small clusters with any defiers. Results are reported in columns (5) and (6). In all cases, we see that our estimate is robust to these sensitivity checks.

52. Our data allow us to capture schools attended by students. However, we cannot tell whether this school is the one assigned. This arises because we do not have a mapping between school catchment areas and schools. We, therefore, devise methods based on the most attended schools in narrowly defined geographical areas, as described in the main text.

53. These specifications drop approximately 15% of individuals.

Table 2.6: Robustness Checks: School Catchment Areas, Small Cluster Fixed Effects and Treating Defiers

	(1)	(2)	(3)	(4)	(5)	(6)
School's Average Test Scores	0.039	0.034	0.039	0.043	0.041	0.043
	(0.002)	(0.002)	(0.005)	(0.009)	(0.003)	(0.008)
Living Area	0.006	0.006	0.006	0.006	0.006	0.006
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of Floors	-0.031	-0.032	-0.018	-0.007	-0.032	-0.031
	(0.005)	(0.006)	(0.010)	(0.018)	(0.006)	(0.016)
Number of Rooms	0.005	0.004	0.003	0.003	0.006	0.007
	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)	(0.003)
Number of Apartments	-0.001	-0.002	-0.002	-0.002	-0.001	-0.002
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age of Building	-0.005	-0.004	-0.004	-0.005	-0.005	-0.005
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log Net HH Income (at Nbhd Level)	0.138				0.127	0.105
	(0.019)				(0.021)	(0.050)
HH Years of Educ. (at Nbhd Level)	0.07				0.076	0.055
	(0.005)				(0.005)	(0.013)
Share Criminals (at Nbhd Level)	-0.000				-0.000	-0.000
	(0.000)				(0.000)	(0.000)
Share Married HH (at Nbhd Level)	-0.468				-0.444	-0.308
	(0.028)				(0.031)	(0.077)
Share non-Westerners (at Nbhd Level)	0.207				0.257	0.657
	(0.130)				(0.141)	(0.335)
Share of Non-Intact HH (at Nbhd x School Level)	0.030				0.041	0.017
	(0.021)				(0.023)	(0.055)
Net HH Income (at Nbhd x School Level)		0.057	0.045	0.043		
		(0.023)	(0.023)	(0.036)		
HH Years of Educ. (at Nbhd x School Level)		0.038	0.036	0.023		
		(0.006)	(0.006)	(0.010)		
Share of non-Western Foreigners (at Nbhd x School Level)		0.009	0.009	-0.057		
		(0.140)	(0.139)	(0.185)		
Share Criminals (at Nbhd x School Level)		-0.228	-0.226	-0.388		
		(0.177)	(0.178)	(0.281)		
Share of Non-Intact HH (at Nbhd x School Level)		-0.085	-0.080	-0.076		
		(0.022)	(0.022)	(0.036)		
Share of Married HH (at Nbhd x School Level)		-0.083	-0.070	-0.078		
		(0.035)	(0.035)	(0.056)		
Observations	203753	252830	84619	46331	171094	49956
R^2	0.543	0.651	0.634	0.674	0.556	0.577

Notes: This Table shows estimates from various robustness checks. The sample includes all homeowners with children attending ninth grade in public schools between 2006 and 2015. Column (1) shows results from our main specification, using only clusters where school boundaries are crossing. Column (2) presents the results of a specification using small cluster fixed effects. Controls include small-cluster-by-school-catchment-area attributes measuring average household net income, and education as well as fraction married, non-intact households, crime, and non-western foreigners. Housing characteristics include the type of building, the number of floors, the number of units per building, the number of bedrooms, toilets, and bathrooms, the size of the living area, and the age of the property. Columns (3)–(6) show our estimates from conducting a subsample analysis aimed at removing defiers, as explained in the text. Property values are logged and school quality (average test scores) is standardized such that the coefficients can be interpreted as the WTP, in percentage terms, for a one–standard deviation increase in school quality. Neighborhood and housing characteristics are as above. Cluster-by-cohort fixed effects are included. Standard errors corrected for clustering at the school-cohort level are reported in parentheses. $p < 0.01$, $p < 0.05$, $p < 0.1$

Using Transaction Data

Thus far we have used governmental valuations from house prices. We examine whether using house prices directly from sales data impacts our estimates. We show that they are robust to this different measure of our dependent variable (column (1) of Table 2.7). We note that since only a fraction of houses are sold on the market every year, the number of observations is smaller for this analysis.

Copenhagen Metropolitan Area

We look at the sensitivity of our estimates to the choice of using data from different geographic areas. Column (2) of Table 2.7 shows estimates for the Copenhagen Metropolitan Area. The estimated coefficient is 2.9%, while the R-squared increases to .65. Our fixed effects strategy works better in more urban and dense areas, as we are able to better characterize heterogeneity. Given the high estimated measurement error, our model explains most of the relevant variation.

Urban and Rural Areas

Turning to columns (3) and (4), we find that our estimates are robust to using on urban or rural data.[54] Interestingly, a specification as in column (2) but without cluster fixed effects produces an estimate of the price for school quality of 5.8% , whereas in columns (3) and (4), the OLS estimate is lower compared to a fixed effects model (1). This provides evidence of the differential nature of unobserved attributes and their effect on prices and school quality, across places, in Denmark.

54. The definition used by the United Nations is used here, where urban (as opposed to rural) denotes a built-up area with at least 200 inhabitants, where the distance between the buildings is not more than 200 meters unless interrupted by public facilities, such as parks.

Table 2.7: Robustness Checks: Sales Data and Distinct Housing Markets

	Sales	Copenhagen Area	Urban	Rural
	(1)	(2)	(3)	(4)
School's Average Test Scores	0.037	0.029	0.033	0.037
	(0.009)	(0.005)	(0.003)	(0.009)
Living Area	0.005	0.006	0.005	0.007
	(0.000)	(0.000)	(0.000)	(0.000)
Number of Floors	0.092	-0.036	-0.034	-0.059
	(0.030)	(0.010)	(0.006)	(0.052)
Number of Rooms	0.002	-0.006	-0.000	0.010
	(0.006)	(0.004)	(0.002)	(0.004)
Number of Apartments	0.002	-0.001	-0.001	-0.001
	(0.001)	(0.000)	(0.000)	(0.004)
Age of Building	-0.002	-0.001	-0.004	-0.005
	(0.000)	(0.001)	(0.000)	(0.000)
Number of Toilets	0.018	0.019	0.013	-0.009
	(0.013)	(0.007)	(0.004)	(0.014)
Number of Bathrooms	0.001	0.023	0.043	0.032
	(0.014)	(0.009)	(0.003)	(0.017)
Log Net HH Income (at Nbhd Level)	0.146	0.076	0.117	-0.044
	(0.093)	(0.057)	(0.028)	(0.113)
HH Years of Educ. (at Nbhd Level)	0.039	0.094	0.080	-0.003
	(0.025)	(0.019)	(0.007)	(0.026)
Share Criminals (at Nbhd Level)	0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Share Married HH (at Nbhd Level)	-0.355	-0.332	-0.415	0.127
	(0.139)	(0.097)	(0.037)	(0.183)
Share non-Westerners (at Nbhd Level)	-0.428	0.084	-0.314	1.235
	(0.815)	(0.420)	(0.200)	(0.966)
Share of Non-Intact HH (at Nbhd x School Level)	-0.271	-0.175	-0.034	0.100
	(0.111)	(0.077)	(0.028)	(0.117)
Observations	17692	14245	105082	19516
R^2	0.652	0.641	0.585	0.513

Notes: This table shows estimates from various robustness checks. The sample includes all homeowners with children attending ninth grade in public schools between 2002 and 2006. Column (1) shows our estimate from using data on property transactions. Columns (2), (3), and (4) look at housing markets, respectively focusing on the Copenhagen Metropolitan Area, urban and rural areas. Property values are logged and school quality (average test scores) is standardized such that the coefficients can be interpreted as the WTP, in percentage terms, for a one–standard deviation increase in school quality. Neighborhood and housing characteristics are as above. Cluster-by-cohort fixed effects are included. Standard errors corrected for clustering at the school-cohort level are reported in parentheses.

Selection on Unobservables

We conclude this section on robustness by applying a recently proposed method by Diegert et al. [2022] to bound our estimates in the presence of selection on unobservables. This method generalizes the adaptation of the Altonji et al. [2005] method by Oster [2019], since it allows for endogenous included variables and for reverse causality. We summarize this approach in Appendix 2.8.6. We implement this methodology on the model presented in 2.5.5, where our estimate of the effect of our measure of school and neighborhood quality on house prices is least likely to be confounded by unobservables since we include fixed effects at the most granular level possible (small cluster level fixed effects). The method is based on the assumption that the variable of interest (school test scores) is less strongly correlated with omitted variables than with observed variables. It applies the standard omitted variable bias formula to determine the strength of the correlation between the unobservables and the variables of interest that is required to make a population OLS estimate of the parameter of interest (the impact of school quality on house value) zero.

Our estimates are robust in the sense that the strength of the correlation between the variables of interest and unobservables has to be greater than 61% of that of the correlation with observables for us to dismiss the resulting estimates. Given the wealth of our data and the high values of R-squared for the fitted values, this magnitude seems implausibly high. Importantly, the Diegert et al. [2022] breakdown point cannot be larger than 100%.[55]

2.6 Controlling for Selection into Both Neighborhoods and Schools

Thus far, following the literature, we have been casual in our treatment of neighborhood selection by parents. The literature on schooling quality usually treats the choice of neighborhood as externally specified. Yet the evidence is overwhelming that parents purposefully choose neighborhoods in advance of their enrollment of children in school and stay in their initial choices or improve them as the children age. This motivates our formal analysis of

55. See Appendix 2.8.6 for more details. We derive another breakdown point of using the framework of Oster [2019] and reach the same conclusion. Appendix 2.8.6 reports the results.

neighborhood choice. We begin by providing evidence that, on average, children who complete 9th grade in Denmark live for a significant number of years in the same residence. In subsection 2.6.2 and 2.6.3, we then turn to assessing mobility patterns over ages of the child, as well as the neighborhood quality achieved.

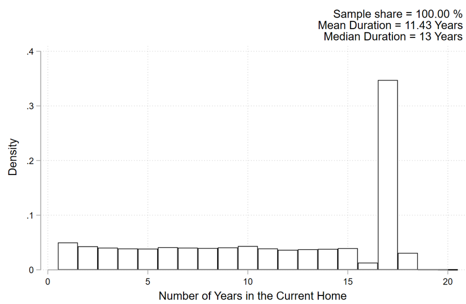
2.6.1 Number of Years Spent in Current House

In the series of figures below, we show the number of years spent by these student in their last recorded home at grade 9, conditional on the number of moves they have experienced in their life. We show how a significant share of these students never moved (close to 40%) while a similar fraction (around 30%) moved only once.

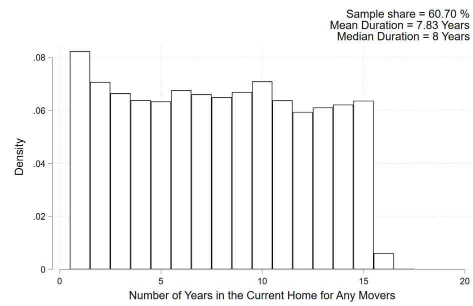
The pattern that emerges from our analysis is that prospective parents pick neighborhoods in advance of or in conjunction with the birth of a child, or shortly after. This initial choice largely persists over the life of the child. Any "fixed effect" for neighborhood conditions on the unobservables that help generate the choice. We account for this conditioning in this section.

Figure 2.7: Number of Years Spent in Current Home

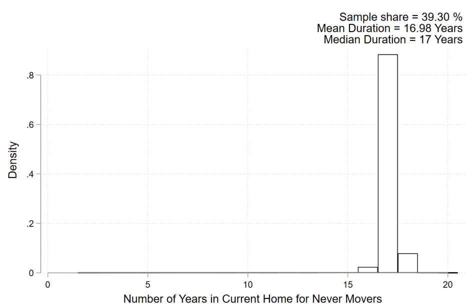
(a) Whole Sample



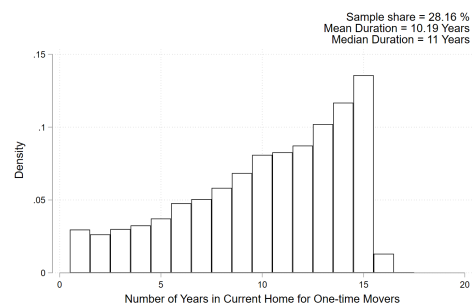
(b) Movers



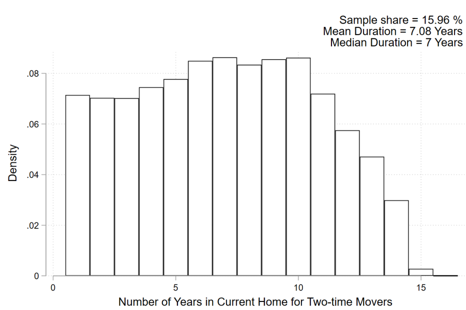
(c) Never Movers



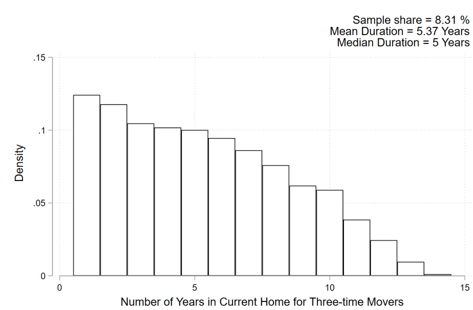
(d) One-time Movers



(e) Two-time Movers



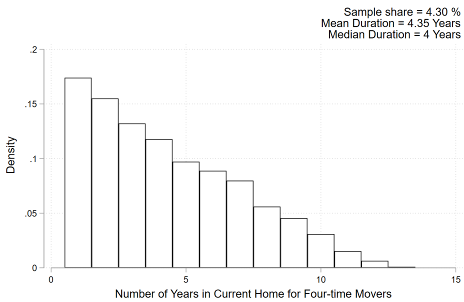
(f) Three-time Movers



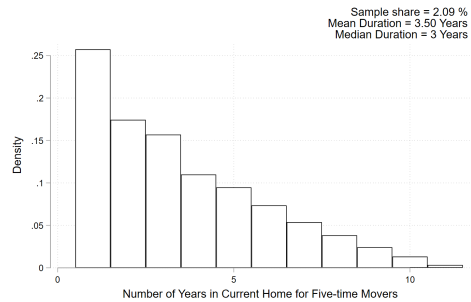
Note: Figures show densities for the number of years children spend in the same home as the one observed during ninth grade, conditional on a given number of moves since age 0. The sample shares, mean and median duration are provided in the upper right corner of each subfigures.

Figure 2.8: Number of Years Spent in Home

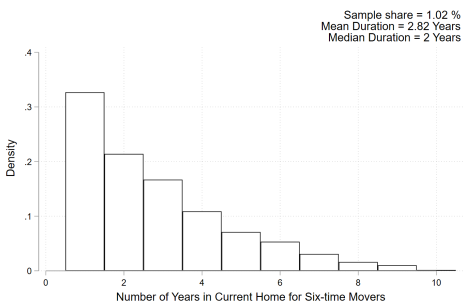
(a) Four-time Movers



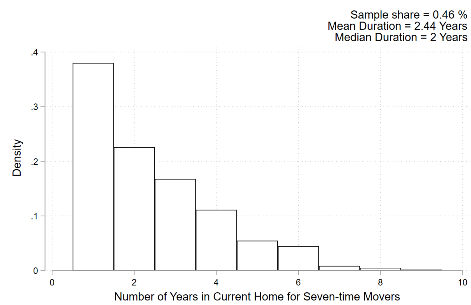
(b) Five-time Movers



(c) Six-time Movers



(d) Seven-time Movers



Note: Figures show densities for the number of years children spend in the same home as the one observed during ninth grade, conditional on a given number of moves since age 0. The sample shares, mean and median duration are provided in the upper right corner of each subfigures

2.6.2 Fraction of Movers over the Life Cycle

This section looks at the fraction of movers along the life cycle. For each year around the birth of the first child, we look at the fraction of individuals who are moving.

It is apparent from Figure 2.9 that the fraction of movers is particularly high before and immediately after the birth of the first child, while continuously decreasing in the following years. Furthermore, there is a socio-economic gradient in these mobility patterns, with students from more educated families experiencing less mobility, as the child ages. Purposeful choices characterize neighborhood choice.

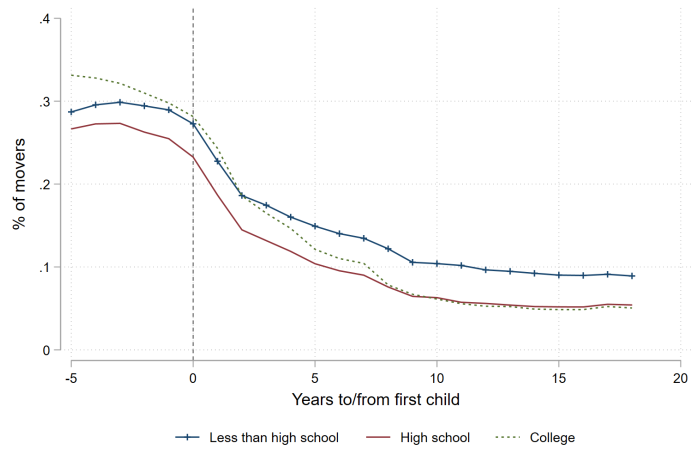


Figure 2.9: Fraction of moves to/from first child, by highest education level achieved

2.6.3 Neighborhood Quality along the Life Cycle

This section considers neighborhood quality around the birth of the first child, across education levels of the mother. Our definition of neighborhood quality is in terms of average test scores of students at the small cluster level. We standardize this variable to have mean zero and standard deviation of one.

As depicted in Figure 2.10, neighborhood quality stabilizes across socio-economic background of students, at around age 6, when students start school up until age 15, when primary schooling ends in Denmark. A large gap across socioeconomic statuses is present. Students whose mothers have not completed high school enter schools with average GPAs 0.25 standard deviation below students whose mothers have completed college. Upward mobility of college students and residential upgrading accounts for the growth in the quality of schooling over the life of the child.

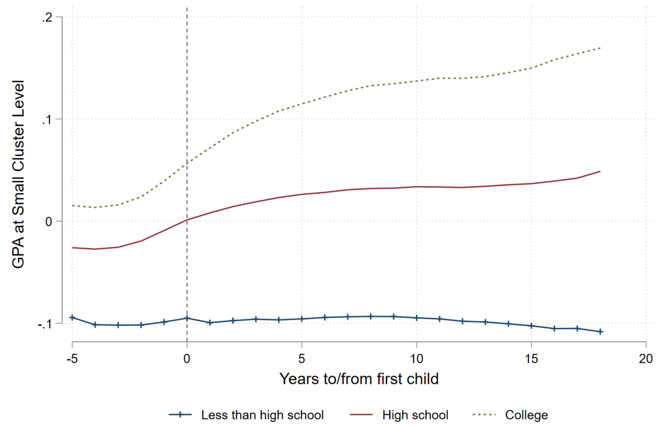


Figure 2.10: Neighborhood quality to/from first child, by highest education level achieved

2.6.4 Modeling Neighborhood Selection Bias

We tackle the issue of selection into both neighborhoods and schools, jointly, which requires dealing with the polychotomous nature of the choice. We estimate a selection model where individual i makes a choice, k, among K different clusters. We observe individual i's property value only for the chosen cluster k.[56] By controlling for selection by cluster, we avoid the selection bias that arises in using fixed effects.

The hedonic price regression for individual i in cluster k, is[57]

$$\ln(p_{imk}) = \alpha_k + \beta_k S_{mk} + \boldsymbol{\delta'_{1k}} \boldsymbol{H}_{imk} + \boldsymbol{\delta'_{2k}} \boldsymbol{X}_{imk} + \varepsilon_{imk},$$

where p_{imk} is house prices in chosen cluster k and school m, α_k is a cluster level specific constant, S_{mk} is measured school quality, $\boldsymbol{H_{ik}}$ and $\boldsymbol{X_{ik}}$ are vectors of housing and neighborhood characteristics, respectively, and u_{ik} is an error term.Individual utilities, V_{ik}, are specified as follows:

56. Domencich and McFadden [1975].

57. To simplify notation, we abstract from school and cohort subscripts, respectively m and t.

$$V_{ik} = \boldsymbol{\gamma_k'} \boldsymbol{Z_{ik}} + \eta_{ik}, \qquad k = 1, \ldots, K \tag{2.3}$$

where η_{ik} is an error term. (V_{ik}, η_{ik}) are mean zero variables with general dependence.

The vector $\boldsymbol{Z_{ik}}$ is comprised of neighborhood and schooling characteristics, including school average test score, household net income and education as well as fraction married, non-intact households, crime (excluding traffic incidents), and non-western foreigners. The model is non-parametrically identified from the exclusion of some of the variables in $\boldsymbol{Z_{ik}}$ from the variables in $\boldsymbol{H_{ik}}$ and $\boldsymbol{X_{ik}}$; We let $\boldsymbol{Z_{ik}}$ include a dummy capturing whether each child lives in the same cluster as their grandparents. The exclusion restriction for the identification purpose is that an individual's house price does not depend on whether their parents live in the same cluster. This specification allows individuals to have preferences for living close to their parents as well as the cluster characteristics but restricts the mean house price to be a function only of cluster characteristics in which the house is located.[58]

Without loss of generality, the outcome variable $\ln(p_{ik})$ is observed if and only if cluster k is chosen, which happens when $V_{ik} > \max_{l \neq k}\{V_{il}\}$.

Now, define:

$$\varepsilon_{ik} = \max_{k \neq l}\{V_{il} - V_{ik}\} = \max_{k \neq l}\{\boldsymbol{\gamma_l'} \boldsymbol{Z_{il}} + \eta_{il} - \boldsymbol{\gamma_k'} \boldsymbol{Z_{ik}} - \eta_{ik}\} \tag{2.4}$$

which is equivalent to $\varepsilon_{ik} < 0$.

Assuming that the η_{ik}'s are independent and identically Gumbel distributed, we obtain the multinomial logit model of McFadden [1974]:

$$P_{ik} \equiv P(\varepsilon_{ik} < 0 \mid \boldsymbol{Z_{ik}}) = \frac{\exp(\boldsymbol{\gamma_k'} \boldsymbol{Z_{ik}})}{\sum_l \exp(\boldsymbol{\gamma_l'} \boldsymbol{Z_{il}})}$$

where P_{ik} the probability that individual i chooses neighborhood k. Consistent estimates

58. We also consider an alternative exclusion restriction, where we fix the location of the grandparents at the time the child is born. We find very similar estimates.

of $\boldsymbol{\gamma_l}$ can be recovered by maximum likelihood. However, least squares estimates of β_k, our parameter of interest, may not be consistent given that the disturbance term in the hedonic regression, u_{ik}, may not be independent of all η_{il}'s.

Define $\mathbf{\Gamma}$ as follows: $\mathbf{\Gamma} = \left\{\boldsymbol{\gamma_1' Z_{i1}}, \boldsymbol{\gamma_2' Z_{i2}}, \ldots, \boldsymbol{\gamma_K' Z_{iK}}\right\}$. Generalizing the model from Heckman [1979], bias correction can be based on the conditional mean of u_{ik} :

$$\mathbb{E}\left(u_{ik} \mid \varepsilon_{ik} < 0, \mathbf{\Gamma}\right) = \iint_{-\infty}^{0} \frac{u_{ik} f\left(u_{ik}, \varepsilon_{ik} \mid \mathbf{\Gamma}\right)}{P\left(\varepsilon_{ik} < 0 \mid \mathbf{\Gamma}\right)} d\varepsilon_{ik} du_{ik} = \lambda(\mathbf{\Gamma})$$

where $f\left(u_{ik}, \varepsilon_{ik} \mid \mathbf{\Gamma}\right)$ is the conditional joint density of u_{ik} and ε_{ik}.

Given that the relation between the K components of $\mathbf{\Gamma}$ and the K probabilities, P_{ik}, is invertible, there is a unique function μ, such that:

$$\mathbb{E}\left(u_{ik} \mid \varepsilon_{ik} < 0, \mathbf{\Gamma}\right) = \mu\left(P_{i1}, \ldots, P_{iK}\right)$$

Therefore, consistent estimation of β_k can be based on one of two regressions:

$$\ln(p_{imk}) = \alpha_k + \beta_k S_{mk} + \boldsymbol{\delta_{1k}' H_{imk}} + \boldsymbol{\delta_{2k}' X_{imk}} + \mu\left(P_{i1}, \ldots, P_{iK}\right) + w_{ik} \tag{2.5a}$$

$$= \alpha_k + \beta_k S_{mk} + \boldsymbol{\delta_{1k}' H_{imk}} + \boldsymbol{\delta_{2k}' X_{imk}} + \lambda(\mathbf{\Gamma}) + w_{ik} \tag{2.5b}$$

where w_{ik} is a residual that is mean-independent of the regressors.

Semi-parametric estimation of this model confronts the curse of dimensionality. Whenever the number of alternatives is large it implies the estimation of a large number of parameters, making it intractable for practical implementation.[59] Thus, restrictions over $\mu(P_{i1}, \ldots, P_{iK})$, are required. Different papers in the literature [Lee, 1983b, Dubin and

59. To make this methodology tractable, we need to impose a restriction on the neighborhood choice set. We do so by considering two different choice sets, which both lead to similar results. First, we bin clusters into 50 equal groups based on average neighborhood education – an individual's choice set is any neighborhood that is in the same group as their current neighborhood quality. Second, we consider all neighborhoods within the same municipality. Below, we report results using the former restriction on the choice set.

McFadden, 1984, Dahl, 2002] impose different assumptions for the bias correction. In this paper, we follow Dahl [2002] and one based on index sufficiency as in Equation (2.5b).

2.6.5 Results

This section presents estimates of the effect of school, house quality and associated neighborhood quality on house prices, controlling for the selection of households to clusters in various ways.

Below we implement several methodologies, based on Equation (2.6):

$$\ln(p_{imk}) = \alpha_k + \beta_k S_{mk} + \boldsymbol{\delta'_{1k} H_{imk}} + \boldsymbol{\delta'_{2k} X_{imk}} + \mu(\Lambda) + w_{ik} \tag{2.6}$$

where, as above, p_{imk} and S respectively denote house prices and school quality, measured by average test scores.

We estimate three models:

1. An OLS specification of log house prices on school quality and housing and neighborhood characteristics, for each neighborhood, where we set $\mu(\Lambda) = 0$. This ignores selection.

2. A cluster selection approach, where we let $\mu(\Lambda) = \mu(P_{i1})$, as proposed by Dahl [2002].[60]

3. A cluster selection approach, where we let $\mu(\Lambda) = g(P_{i1} \ldots, P_{ik})$. We reduce the dimensionality of all probabilities, using PCA, and retaining the first 3 principal components. In this specification, as opposed to the previous one, we use the whole vector of estimated probabilities to select a given cluster, relaxing the scalar index sufficiency assumption of Dahl [2002]. To reduce the dimension of this vector of K probabilities,[61]

60. To estimate $\mu()$, we use polynomials of degree 3.

61. We have about 2,200 clusters, which are broken into 50 equal groups. Thus, K is about 44.

we conduct a principal component analysis (PCA) and retain the first three principal components.

The latter model further provides a test of the adequacy of the index sufficiency assumption of Dahl [2002] in our setting.

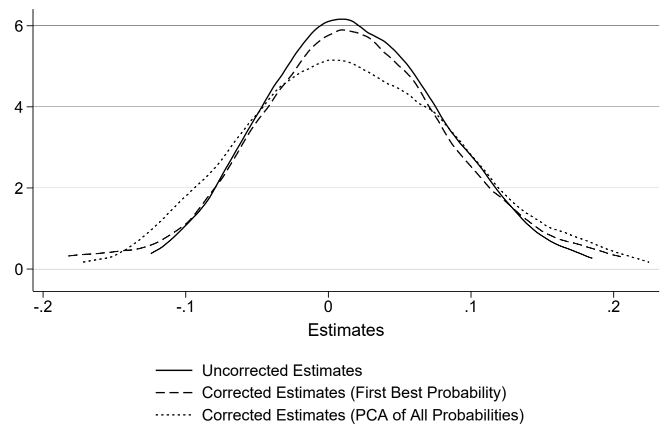


Figure 2.11: Distribution of neighborhood-level corrected vs. uncorrected estimates of the WTP for measured school quality β_k

Notes: Distribution of neighborhood-level corrected vs. uncorrected estimates. The sample includes all homeowners with children attending 9th grade between 2002 and 2015.

The average estimated value of β_k from each of the three approaches is respectively 1.9%, 1.6%, 1.7%.[62] The distributions are shown in Figure 2.11. These are the percentage increases in house prices for a one standard deviation increase in measured school quality. These estimates are consistent with our previous estimates.

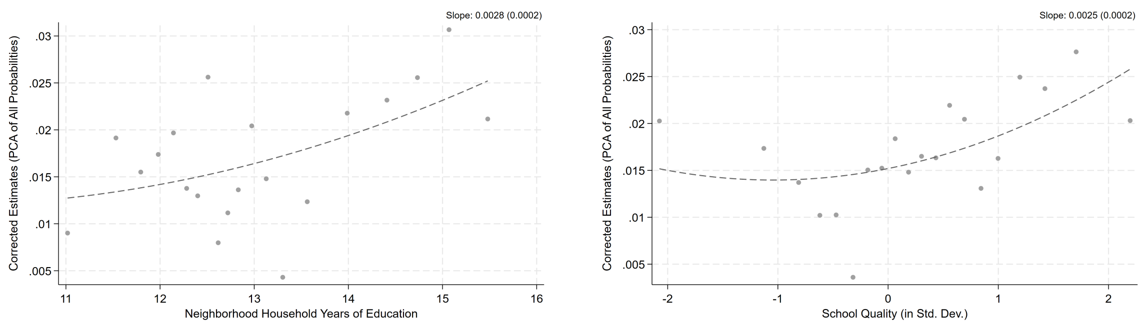
Recovering these selection-corrected neighborhood-specific estimates of WTP for school quality allows us to consider patterns of heterogeneity across neighborhoods. Figure 2.12 depicts the positive correlation between neighborhood average years of education and our selection-corrected estimates. This positive relationship provides evidence for the “Matthew Effect” that declares "to those who have, more is given". More educated households sort into neighborhoods with better amenities, including school quality, as shown in Section 2.4.2.

62. These estimates are averages of β_k over all neighborhoods, k.

Moreover, such households are willing to pay more for school quality. In light of our results in our companion paper on the long-run consequences of these choices [Eshaghnia et al., 2023c], such patterns provide a key explanation for labor-market inequalities present in Denmark.

Figure 2.12: Relationship Between Selection-Corrected WTP Estimates and Neighborhood Attributes

(a) Neighborhood Years of Education (b) School Quality



Notes: This figure presents a binned scatter plot (with 20 equal-sized groups) depicting the relationship between our selection-corrected neighborhood-specific estimates of the WTP for measured school quality (as captured by school average test scores) and neighborhood attributes. The slope of the linear relationship and the standard errors are reported in the top right corner.

2.7 Conclusion

The Scandinavian welfare state is often regarded as an exemplary system for reducing inequality and equalizing opportunities by providing uniform high quality social services and an education system of uniform high quality that is free for all. Yet, despite equal per capita school expenditure and teacher salaries, there exist substantial differences between schools in terms of quality of teachers and the skill levels of the student and adult peers. These differences are, in part, due to residence-based assignment of students to public schools along with the sorting of families and teachers across neighborhoods. More advantaged families sort into neighborhoods where school quality is higher. Equalizing per-pupil expenditure does not equalize school quality. School quality is, effectively, also a stand-in for neighborhood, student and peer quality.

We provide evidence that access to better schools and neighborhoods through residential choices is capitalized into house prices. Using rich longitudinal administrative data from Denmark, we apply a variety of empirical strategies to estimate the marginal WTP for access to schools in Denmark, where public schools are free. Our estimates show that households are willing to pay around 3% of house prices for a one–standard deviation increase in measured school test scores, which proxy school and neighborhood characteristics. Our results are robust to a variety of specifications and robustness checks. In a companion paper (Eshaghnia et al., 2023c), we show that our estimate of the willingness to pay for quality is strongly predictive of beneficial child outcomes. Per-pupil expenditures are not the driving force in explaining test scores in Denmark. Parents, peers, and neighbors are the sources of variation in schools. Disentangling the demands for each of these components is difficult given the high level of inter-correlation among them and is a task left for the future.

A central conclusion of this paper is that Tiebout-type models of schooling quality in which agents sort by income into neighborhoods to pay for local public goods such as school quality, do not apply to Denmark. Our estimates of school quality effects are comparable in magnitude to those from the US and other countries. It is likely that the estimates for other countries also capture teacher quality and peer effects in neighborhoods and schools and not school finance effects, stressed in the recent literature. More basic social forces are at work.

2.8 Appendix

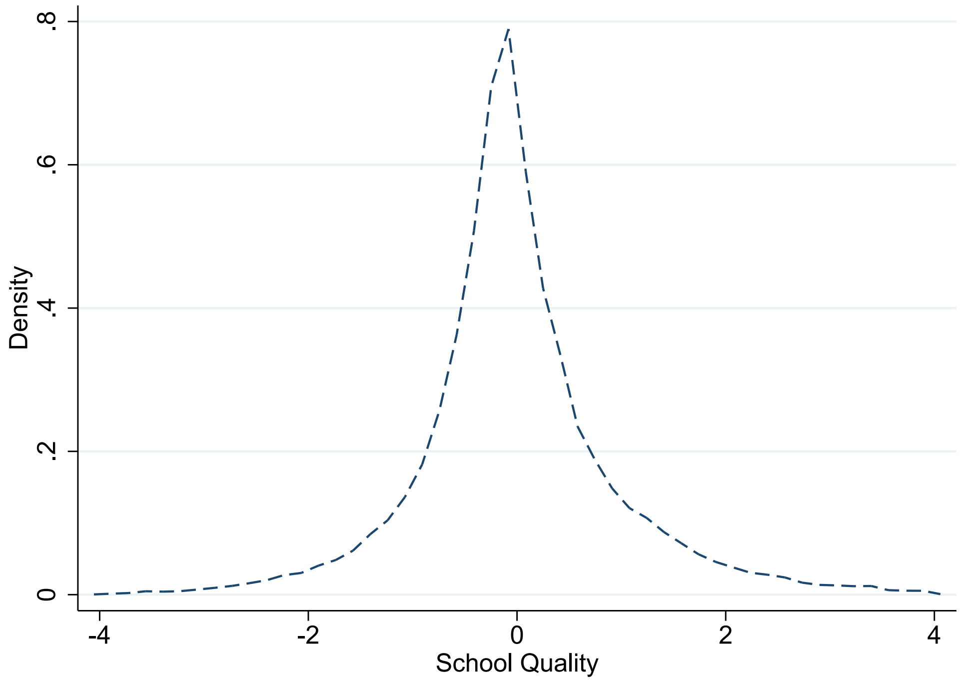
2.8.1 Data Description

This appendix presents summary statistics of the data used for empirical analysis. Section 2.8.1 provides summary statistics on small clusters and details how we expand cluster assignment to the years 2005–2015. Section 2.8.1 provides summary statistics for the control variables.

Variation in School Quality

This section presents evidence on the variation in our measure of school quality (average test scores). Figure 2.13 presents the density of our measure of school quality across schools in Denmark.

Figure 2.13: Variation in School Quality



Notes: The figure shows the distribution of school quality (measured as average test scores) in Denmark in 2006.

Characteristics of Clusters

Table 2.8 provides summary statistics on clusters.

Table 2.8: Summary Statistics of Clusters

	Median	Mean	Std.Dev.
Small Neighborhoods			
Number of Households 2004	245	272.7	115.0
Number of Persons 2004	526	592.2	285.5
Size (in hectares)	22	47.5	64.46
Neighborhoods			
Number of Households 2004	985	1079.7	396.2
Number of Persons 2004	2090	2344.5	1039.0
Size (in hectares)	88	187.9	236.3
# of small neighborhoods	4	4.0	1.3

Note: Summary statistics of the composition of clusters for both neighborhood units. Source: Damm and Schultz-Nielsen [2008].

Since a given address always holds the same cluster ID, we are able to expand the cluster IDs for all individuals (including children), beyond 2004 for addresses that existed before 2004. We do so for 2005 and 2006, achieving a matching rate of 94.4% and 92.5% respectively. However, unique identifiers of addresses change after 2006, making it more difficult to expand cluster IDs further. We exploit data on a number of housing attributes[63] to conduct probabilistic matching on housing units to recover a unique mapping between housing identifiers across 2006 and 2007. This mapping then allows us to expand our dataset of clusters further from 2007 to 2015, which we use in Sections 2.5.5 and 2.6.

The construction of these clusters ensures that their size remains small, such that we are able to control for unobserved neighborhood-level attributes. However, the requirement that each cluster consists of a minimum number of individuals (150 for small and 600 for clusters), implies that in more rural areas the size of these clusters can remain relatively large. This

63. These attributes include the type of building, the number of floors, the number of units per building, the number of bedrooms, toilets and bathrooms, the size of the living area, the age of the property as well as parish code.

has two impacts on our empirical strategy. First, the presence of larger clusters reduces our ability to control for unobserved neighborhood level characteristics. Second, we are less likely to have different school catchment areas within each cluster. To avoid such issues, we compute the density (inhabitants per square kilometers) of parishes in which clusters lie as a proxy for clusters' size. We then remove from our sample all observations which lie within the bottom 25th percentile of parishes in terms of density.[64] This reduces our sample by less than 1%, while allowing us to focus on denser areas, where our cluster fixed effects strategy is likely to capture more of the unobserved attributes of the space common to all houses.

Summary Statistics of Control Variables

This Appendix reports summary statistics on control variables used throughout our analysis.

64. An alternative strategy would be to remove clusters based on their geographical size. However, as we do not observe the size of clusters in our data, we base our measure on the size of the parishes in which they lie.

Table 2.9: Summary Statistics

	Mean	Standard Deviation	Coefficient of Variation
Neighborhood (Cluster) Level – % of Population			
Average Household Gross Income (Excluding Transfers)	52,745	20,200	0.38
Average Household Max. Years of Schooling	12.5	.886	0.07
Fraction of Married Households	.769	.209	0.27
Fraction of Not Intact Households	.506	.232	0.45
Fraction of Non-Western Foreigners	.028	.062	2.21
Fraction of Criminals	.024	.010	2.4
School Level			
Average Household Gross Income (Excluding Transfers)	95,721	23,423	0.25
Average Household Max. Years of Schooling	13.0	.827	0.06
Fraction of Married Households	.768	.097	0.13
Fraction of Not Intact Households	.514	.109	0.21
Fraction of Non-Western Foreigners	.030	.049	.1.63
Housing Attributes (Pooled Sample)			
Age of Building (years)	51.0	37.5	0.74
Living Area (square meters)	149.8	47.7	0.32
Number of Floors	1.14	.56	.44
Number of Apartments	4.19	14.2	3.39
Number of Rooms	5.08	1.45	0.29
Number of Toilets	1.60	.59	0.37
Number of Bathrooms	1.31	.49	0.37
House Price	255,253	218,115	0.86

Notes: This table reports summary statistics on the key variables used in the analyses. Our sample comprises of all Danes who complete ninth grade in years 2002–2006. We focus on homeowners, since we can observe their housing prices. Incomes and house prices are converted from DKK to 2010 USD. An exchange rate of 6.7 DKK per US dollar is used to obtain the dollar values. we consider a family as intact if the mother and the father were living together over the whole childhood stage (from age zero to 18 of the child).

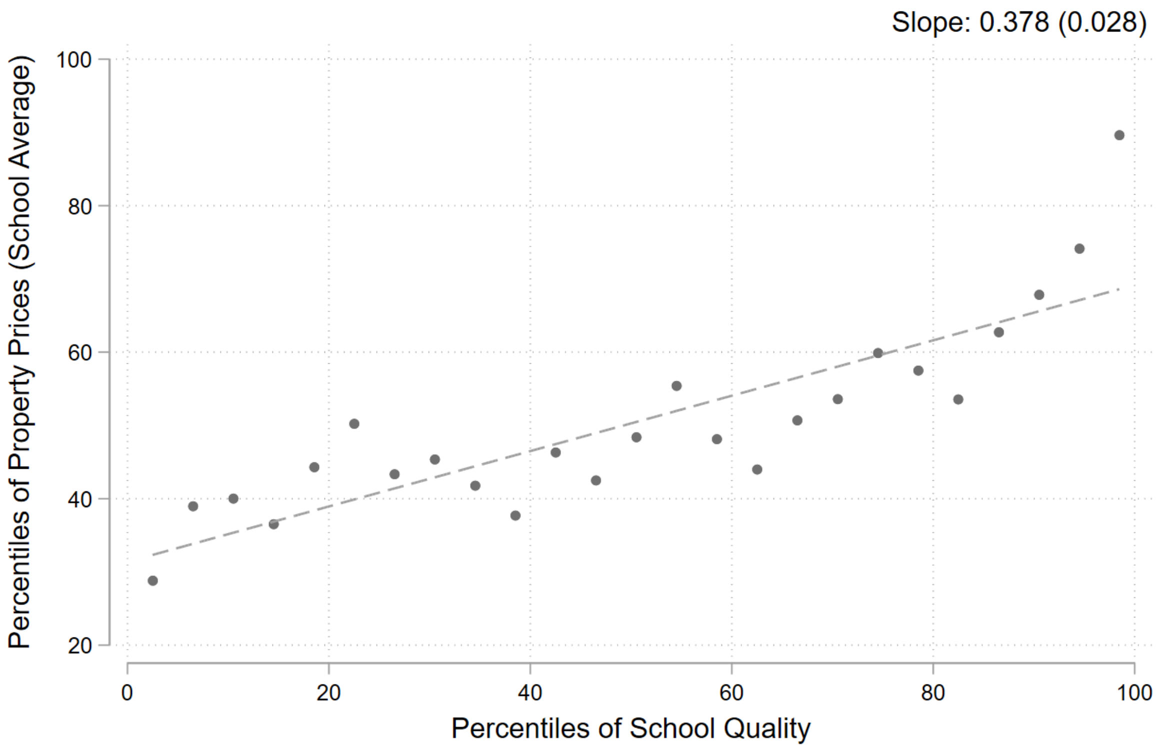
2.8.2 Sorting

This Appendix presents evidence of the importance of sorting in Denmark, focusing on house prices, individual and neighborhood characteristics, and various measures of school characteristics in different neighborhood units. Section 2.8.2 shows the relationship between house prices and various measures of school characteristics in Denmark as a whole. Section 2.8.2 presents evidence for sorting within clusters. Section 2.8.2 studies the relationship between school attributes, in particular peers, teacher quality, school value-added, and average test score. Section 2.8.2 conducts a principal component analysis of teacher, school peers and neighborhood attributes. Section 2.8.2 analyzes the remaining variation in school attributes at difference geographical levels. Finally, Section 2.8.2 presents the correlation between neighborhood and school attributes.

House Prices and Measured School Quality

Figure 2.14 graphs the relationship between house prices at the school level and school quality.

Figure 2.14: House Prices and School Quality



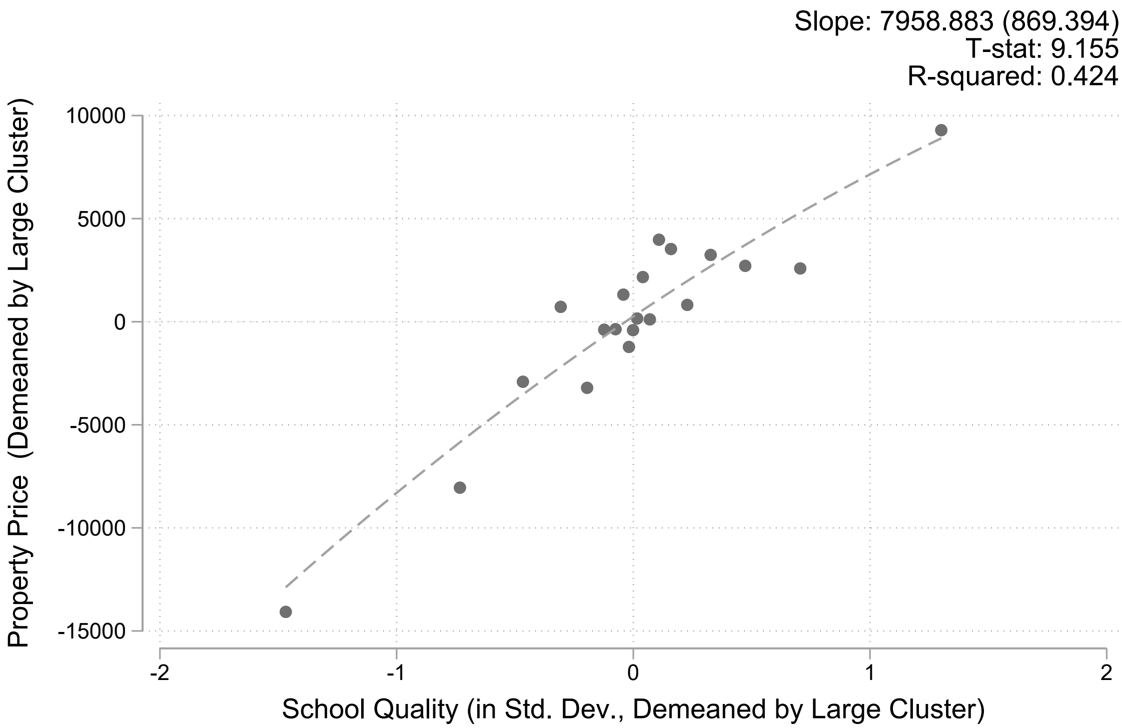
Notes: The figure shows a binned scatter plot (with 20 equal-sized groups), with a linear fit, of percentiles of parental property values averaged at the school level on percentiles of school quality. School quality is measured by taking the average test scores of students attending a given school. The sample contains property values for parents who have a child attending ninth grade in a public school between 2002 and 2006.

Sorting within Clusters

In this section, we present evidence for the sorting of households into schools within clusters. Figure 2.15 presents the relationship between house prices averaged at the school level and school quality within clusters. Figure 2.16 presents the relationship between a set of housing characteristics and school quality within clusters. Figure 2.18 presents the relationship between different individual characteristics and school quality within clusters. Finally, Figures 2.20 and 2.21 presents the relationship between various neighborhood characteristics and school quality within clusters.

Figure 2.15: Sorting Within Clusters - House Price

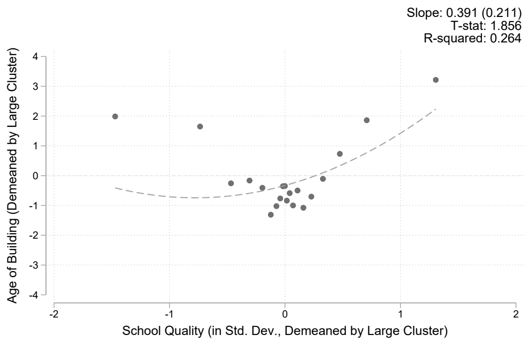
(a) Property Price



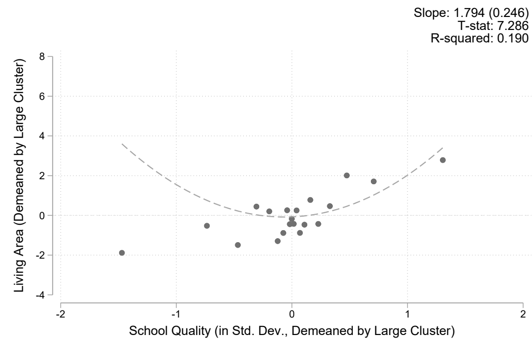
Notes: This figure presents a binned scatter plot (with 20 equal-sized groups) depicting the relationship between house prices and school quality measured by test scores within clusters. This is constructed by regressing property prices on school quality, controlling for neighborhood-by-cohort fixed effects and small neighborhood attributes—average income, years of education, fraction married, non-westerners, foreigners, crime, and non-intact households. Standard errors corrected for clustering at the cluster-cohort level are reported in the top right corner. The sample contains property values for parents who have a child attending ninth grade in a public school between 2002 and 2006.

Figure 2.16: Sorting within Clusters (Housing Characteristics)

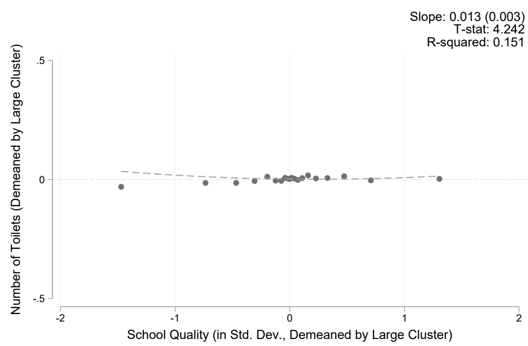
(a) Building Age



(b) Living Area



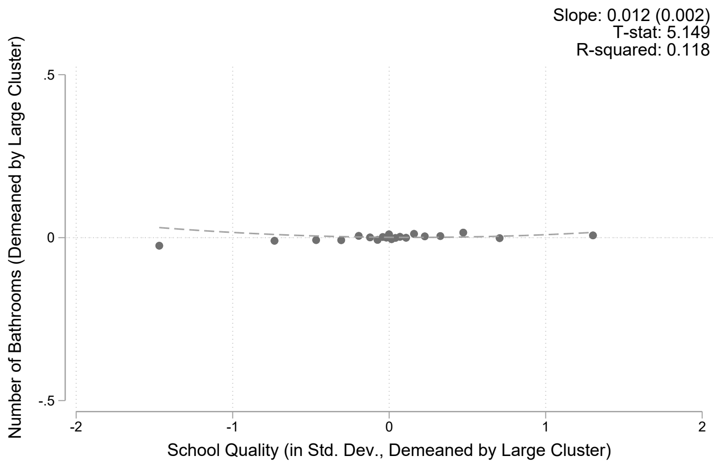
(c) Number of Toilets



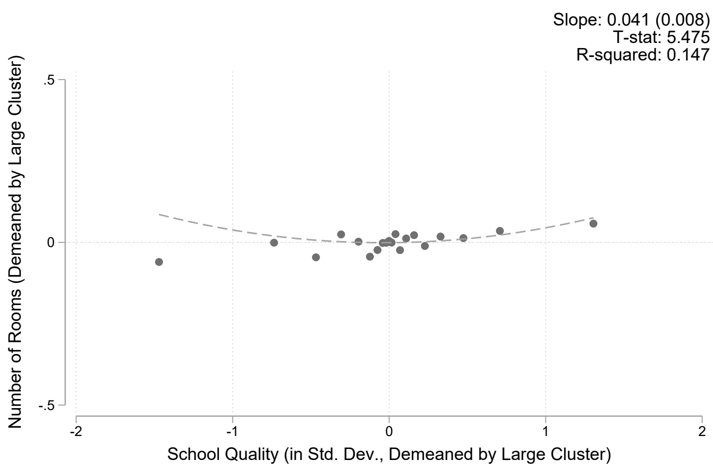
Notes: This figure presents a binned scatter plot (with 20 equal-sized groups) depicting the relationship between housing characteristics and school quality measured by test scores within clusters. This panel is constructed by regressing various housing attributes on school quality, controlling for neighborhood-by-cohort fixed effects and small neighborhood attributes—average income, years of education, fraction married, non-westerners, foreigners, crime and non-intact households. Standard errors corrected for clustering at the cluster-cohort level are reported in the top right corner. The sample contains property values for parents who have a child attending ninth grade in a public school between 2002 and 2006.

Figure 2.17: Sorting within Clusters (Housing Characteristics), cont'd

(a) Number of Bathrooms

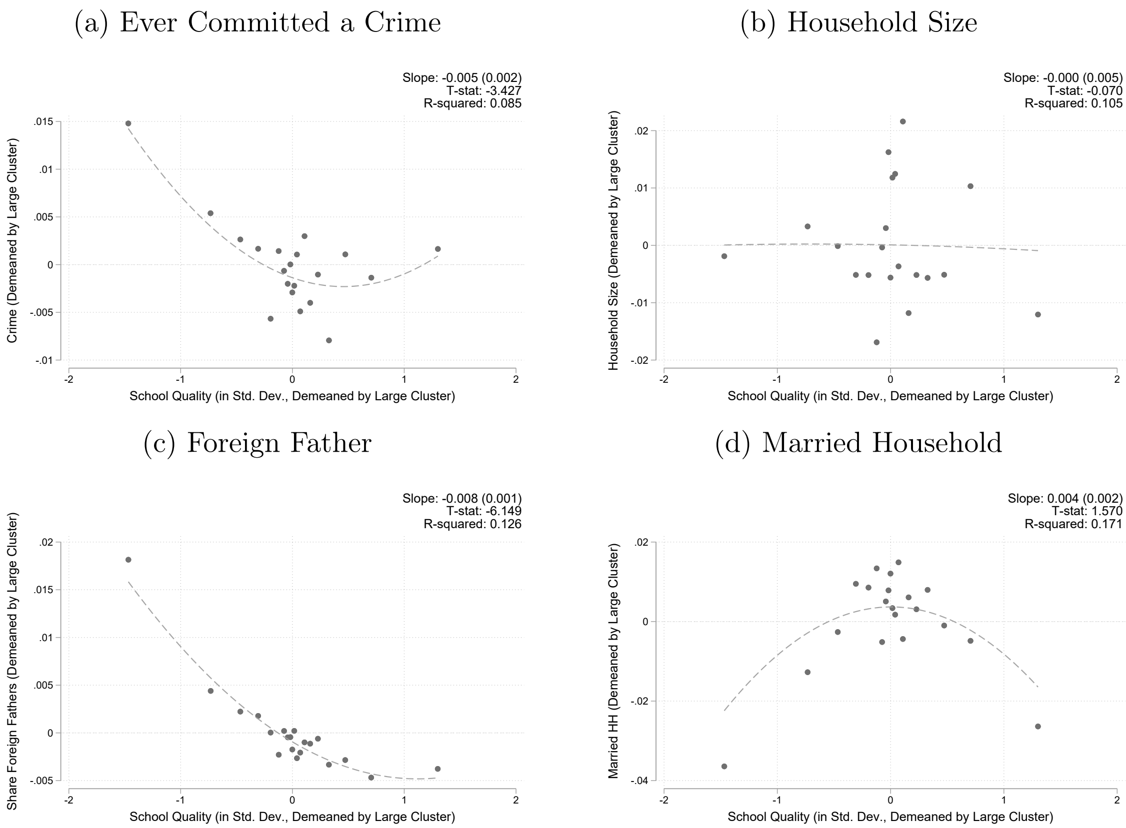


(b) Number of Rooms



Notes: This figure presents a binned scatter plot (with 20 equal-sized groups) depicting the relationship between demeaned housing characteristics and demeaned school quality within clusters. Each panel is constructed by regressing various housing characteristics on school quality, controlling for neighborhood-by-cohort fixed effects and small neighborhood attributes—average income, years of education, fraction married, non-westerners, foreigners, crime, and non-intact households. Standard errors corrected for clustering at the cluster-cohort level are reported in the top right corner.

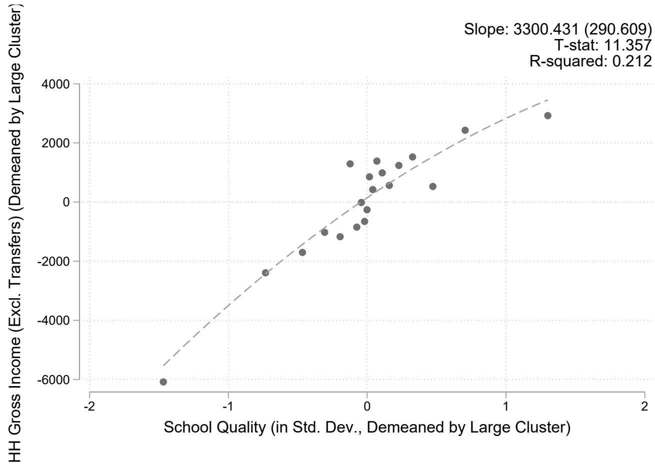
Figure 2.18: Sorting within Clusters (Individual Characteristics)



Notes: This figure presents a binned scatter plot (with 20 equal-sized groups) depicting the relationship between individual characteristics and school quality measured by test scores within clusters. Each panel is constructed by regressing various individual characteristics on school quality, controlling for neighborhood-by-cohort fixed effects, small neighborhood as well as housing attributes—the type of building, the number of floors, the number of units per building, the number of bedrooms, toilets and bathrooms, the size of the living area, and the age of the property. To measure crime, we use an indicator for whether parents ever committed a crime or not. We exclude traffic-related crimes. Standard errors corrected for clustering at the cluster-cohort level are reported in the top right corner. The sample contains property values for parents who have a child attending ninth grade in a public school between 2002 and 2006.

Figure 2.19: Sorting within Clusters (Individual Characteristics), cont'd

(a) Gross Income excluding Transfers



(b) Years of Education

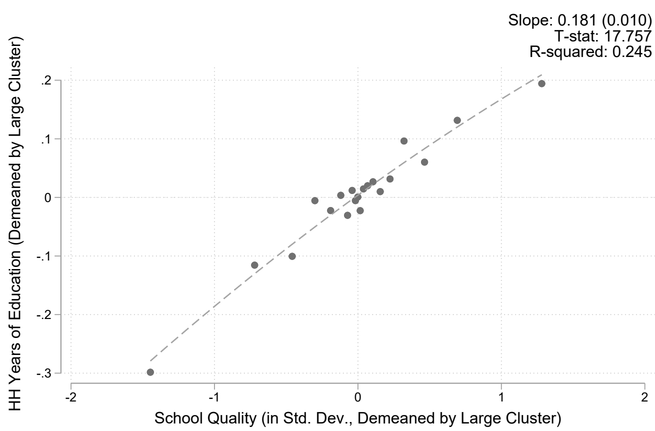
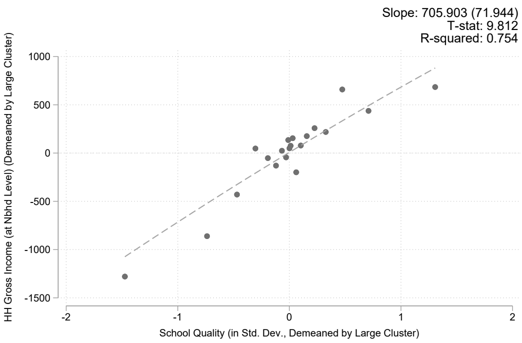
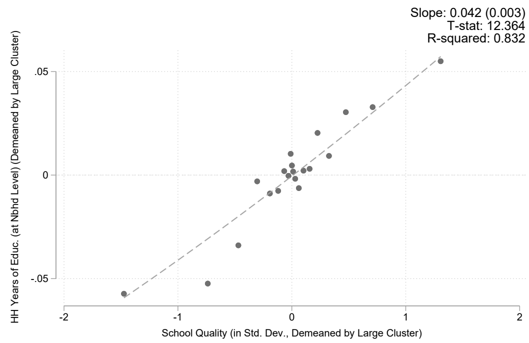


Figure 2.20: Sorting within Clusters (Neighborhood Characteristics)

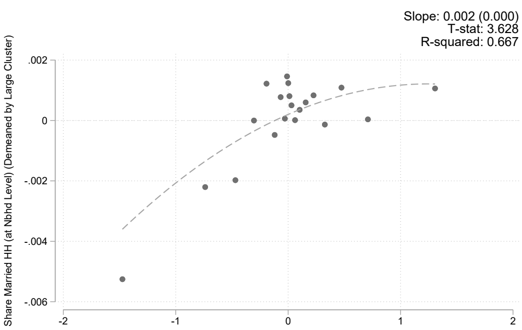
(a) Neighborhood Average Income



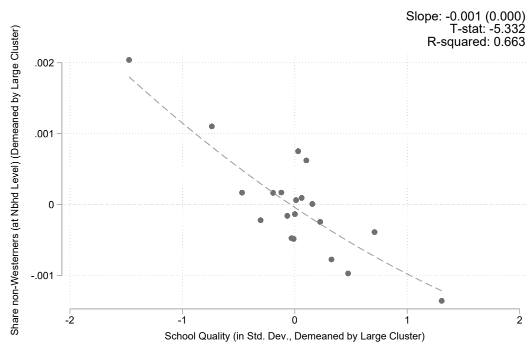
(b) Neighborhood Average Education



(c) Neighborhood Share of Married Households



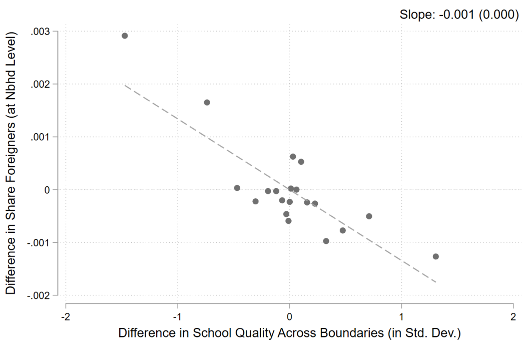
(d) Neighborhood Share of Non-Western Foreigners



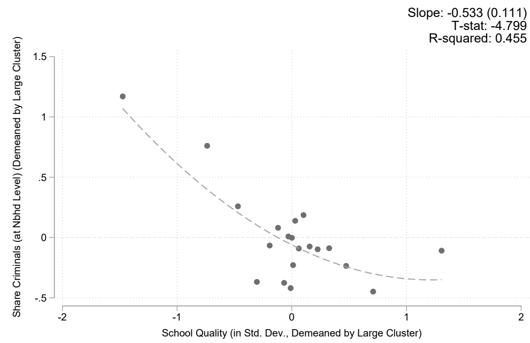
Notes: This figure presents a binned scatter plot (with 20 equal-sized groups) depicting the relationship between neighborhood characteristics and school quality measured by test scores within clusters. Each panel is constructed by regressing various small cluster characteristics on school quality, controlling for neighborhood-by-cohort fixed effects and housing attributes—the type of building, the number of floors, the number of units per building, the number of bedrooms, toilets and bathrooms, the size of the living area, and the age of the property. Standard errors corrected for clustering at the cluster-cohort level are reported in the top right corner. The sample contains property values for parents who have a child attending ninth grade in a public school between 2002 and 2006.

Figure 2.21: Sorting within Clusters (Neighborhood Characteristics): Origin, Crime, and Family Structure

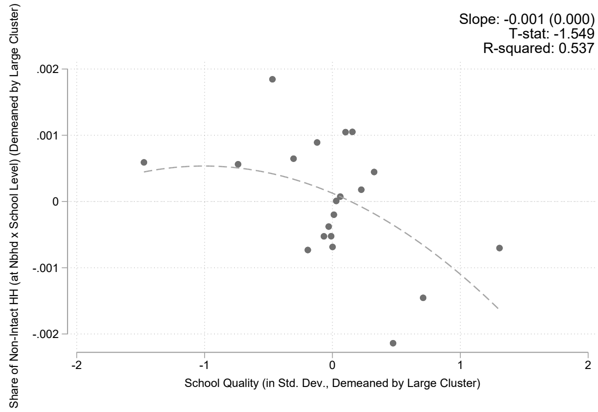
(a) Neighborhood Share of Foreigners



(b) Neighborhood Share of Criminals



(c) Neighborhood Share of Non-Intact Households



Notes: This figure presents a binned scatter plot (with 20 equal-sized groups) depicting the relationship between neighborhood characteristics and school quality measured by test scores within clusters. Each panel is constructed by regressing various small cluster characteristics on school quality, controlling for neighborhood-by-cohort fixed effects and housing attributes—the type of building, the number of floors, the number of units per building, the number of bedrooms, toilets and bathrooms, the size of the living area, and the age of the property. Standard errors corrected for clustering at the cluster-cohort level are reported in the top right corner. The sample contains property values for parents who have a child attending ninth grade in a public school between 2002 and 2006.

Relationship among Different Measures of Quality

This appendix clarifies our use of school average test scores as measures of school quality. The evidence below points to strong correlations among various school inputs, school value-added and school average test score. In light of such strong correlations it is not possible to precisely estimate the separate effects of different school inputs in samples at our disposal.

In Section 2.8.2, we look at two sets of inputs entering the school production function, namely teacher quality and peer quality and relate them to school average test score. The construction of these variables is briefly discussed in Section 2.8.2. We show how these variables are strongly correlated. These figures also provide further evidence on the sorting process in Denmark within neighborhoods.

Data The main sample used in this paper consists of cohorts in school between 2002 and 2006 who complete 9th grade. The analysis in this Appendix uses cohorts between 2002 and 2015, due to the following considerations. First, data on teacher quality is available from 2008 onward. Second, 8th grade test scores, which are used to construct school value-added estimates are available from 2011 onward.

We note that the literature has studied the role of observed teacher quality characteristics on outcomes (see for instance Rivkin et al. [2005]), while stressing that unobserved variables tend to play a much more significant role in explaining the impact of teachers in influencing children's later life outcomes.

In the sections below, we look at the following peer quality variables, which we average over all students attending a given school:

- Average household gross income excluding transfers;
- Maximum household education;
- Non-western origin of the child.

In terms of teacher quality, we focus on the following observed characteristics:

- Teacher GPA rank at teacher's college;
- Teacher age when graduating from teacher's college;
- Teacher tenure (only observed up to 3 years in a given school);
- Teacher quality index.[65]

Finally, we construct a measure of school value-added, following the methodology of Chetty et al. [2014b] applied to schools. We control for 8th grade test scores, and a vector of household characteristics, including parental income, wealth, education, criminal record and marital status, as well as child order and gender.

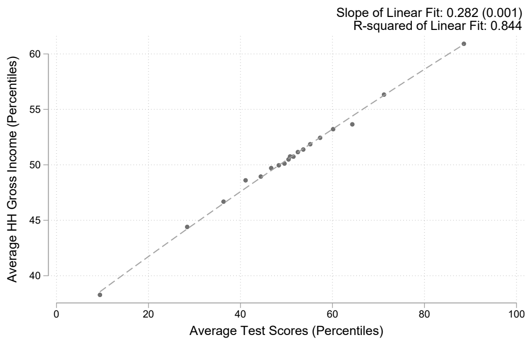
This section provides evidence on the relationship between various school attributes, as well as an output of the school production function, namely school average test score. All the variables used in analysis below in Section 2.8.2 are ranked between 0 and 100.

Figures 2.22-2.25 provide suggestive evidence of the strong correlation between these different inputs which enter the school quality production function and the output, namely test scores. Given such correlations, estimating the separate effects of each of these components is difficult.

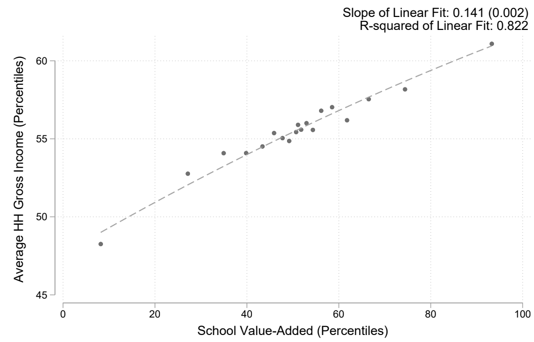
65. As described in Section 3.3, the teacher quality index is constructed as follows. Using administrative records, all employees in teaching positions in schools between 2009 and 2016 are matched to their (a) academic records from high school (grades in Danish and Mathematics exams) and university as well as (b) employment records to identify unemployment spells. Children's GPA are then regressed on these teacher's characteristics. A national rank of school quality is then generated using linear regression. This index is based on Gensowski et al. [2021].

Figure 2.22: Correlations Between Percentile of Household Average Gross Income at School Level and School Characteristics (As Specified in Sub-figure Title)

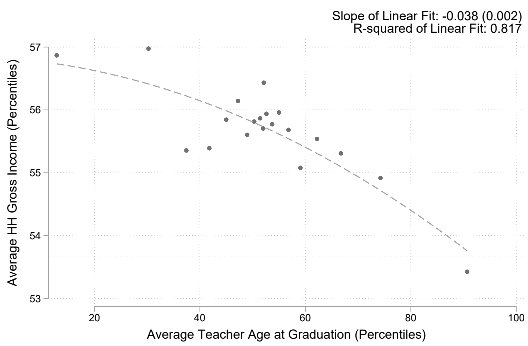
(a) School Average Test Scores



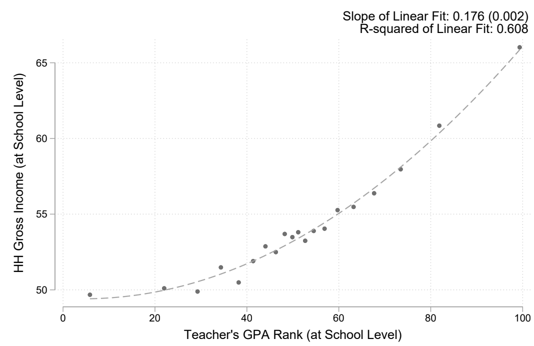
(b) School Value-Added



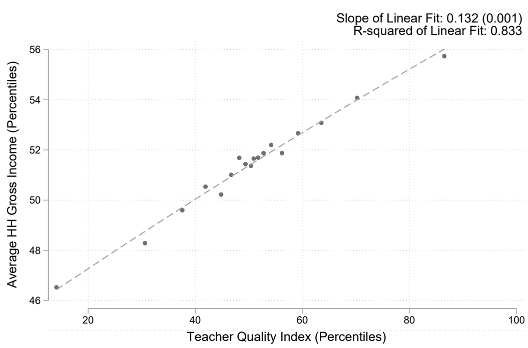
(c) Average Age at Graduation of Teachers



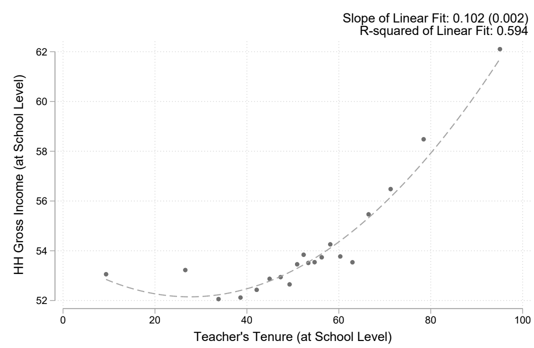
(d) Average Teacher GPA Rank at Teacher's College



(e) Teacher Quality Index



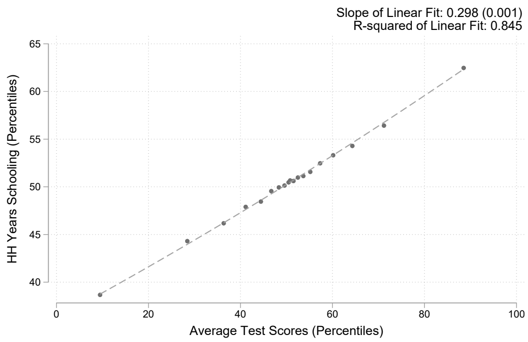
(f) Average Teacher Tenure



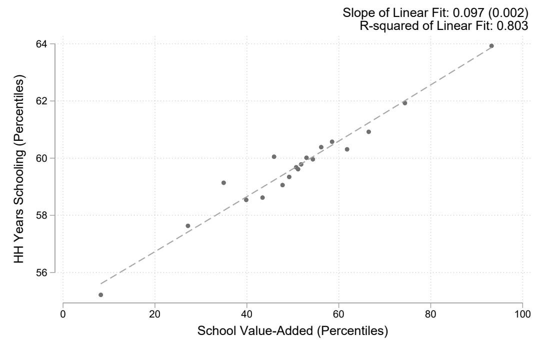
Notes: This figure presents a binned scatter plot (with 20 equal-sized groups) depicting the relationship between school-level average household gross income, school inputs and our measure of school quality (measured by average test scores). These figures control for neighborhood-by-cohort fixed effects. All variables are transformed into percentiles.

Figure 2.23: Correlations Between Percentile of Household Years of Education at School Level and School Characteristics (As Specified in Sub-figure Title)

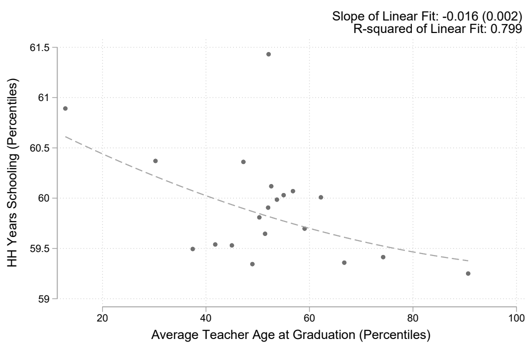
(a) School Average Test Scores



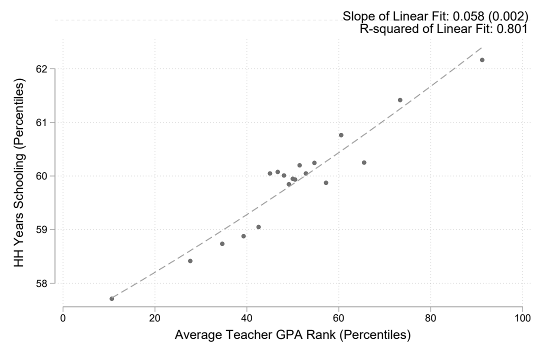
(b) School Value-Added



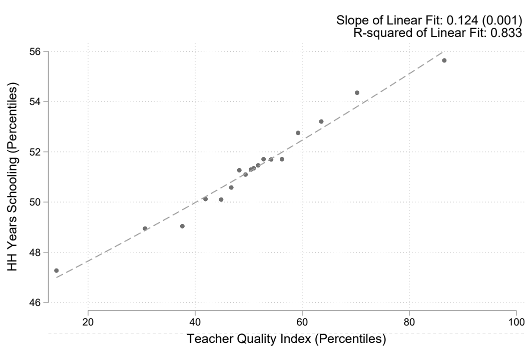
(c) Average Age at Graduation of Teachers



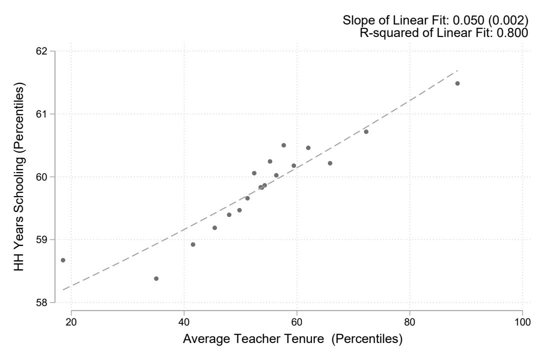
(d) Average Teacher GPA Rank at Teacher's College



(e) Teacher Quality Index



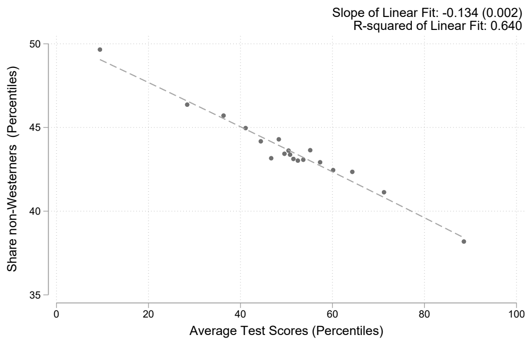
(f) Average Teacher Tenure



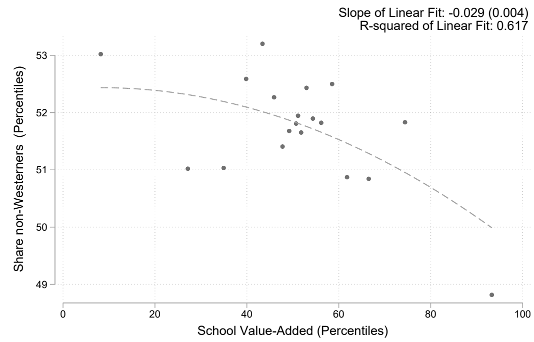
Notes: This figure presents a binned scatter plot (with 20 equal-sized groups) depicting relationship between school-level maximum parental years education, school inputs and our measure of school quality (measured by average test scores). These figures control for neighborhood-by-cohort fixed effects. All variables are transformed into percentiles.

Figure 2.24: Correlations Between Percentile of Share of Non-Westerners at School Level and School Characteristics (As Specified in Sub-figure Title)

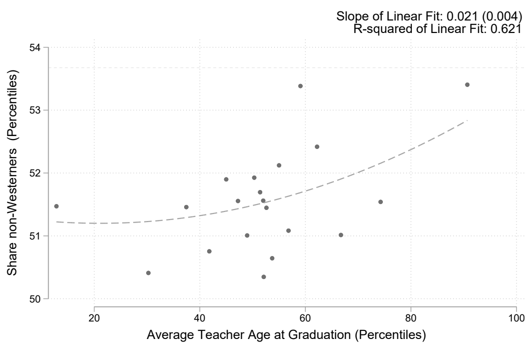
(a) School Average Test Scores



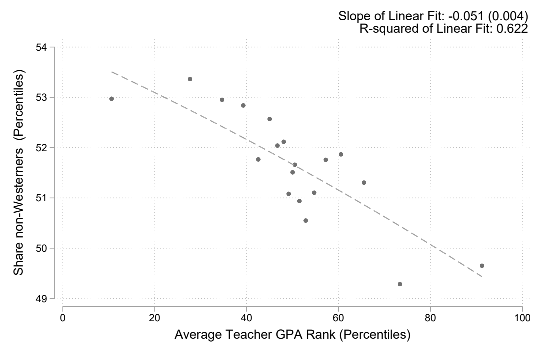
(b) School Value-Added



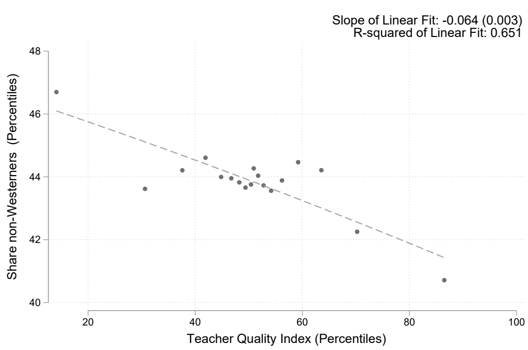
(c) Average Age at Graduation of Teachers



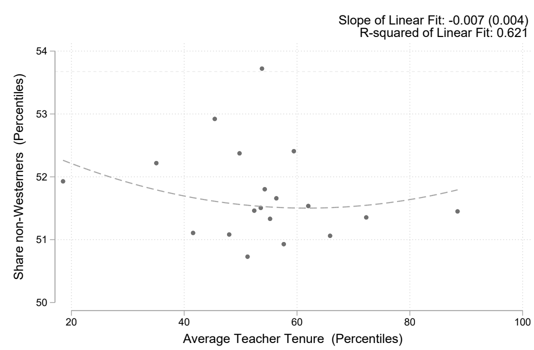
(d) Average Teacher GPA Rank at Teacher's College



(e) Teacher Quality Index



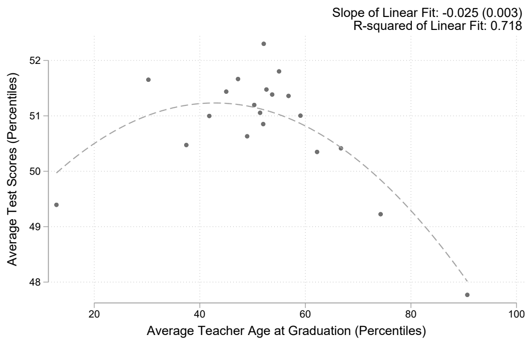
(f) Average Teacher Tenure



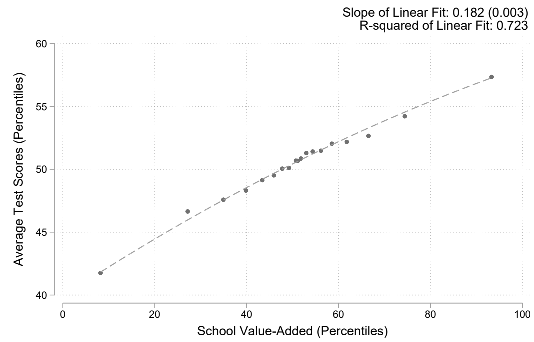
Notes: This figure presents a binned scatter plot (with 20 equal-sized groups) depicting relationship between school-level share of non-westerner, school inputs and our measure of school quality (measured by average test scores). These figures control for neighborhood-by-cohort fixed effects. All variables are transformed into percentiles.

Figure 2.25: Correlations Between Percentile of School Average Test Scores and School Characteristics (As Specified in Sub-figure Title)

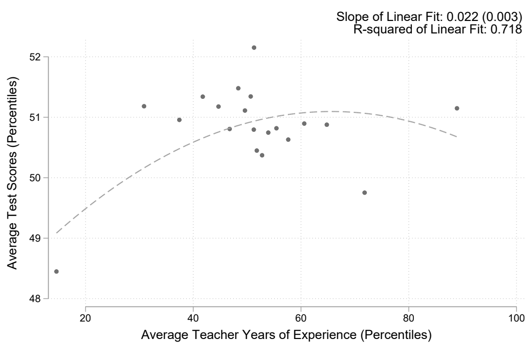
(a) Average Age at Graduation of Teachers



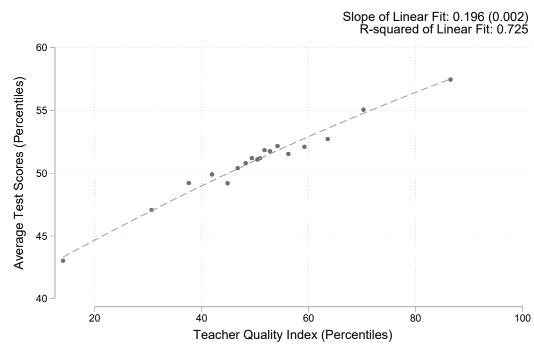
(b) School Value-Added



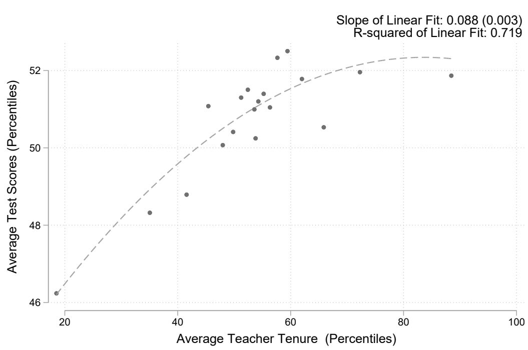
(c) Average Years of Experience of Teachers



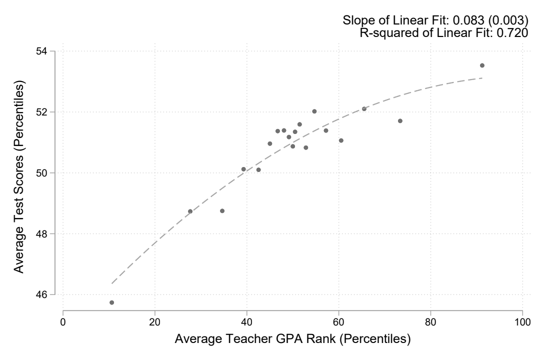
(d) Teacher Quality Index



(e) Average Teacher Tenure



(f) Average Teacher GPA Rank at Teacher's College

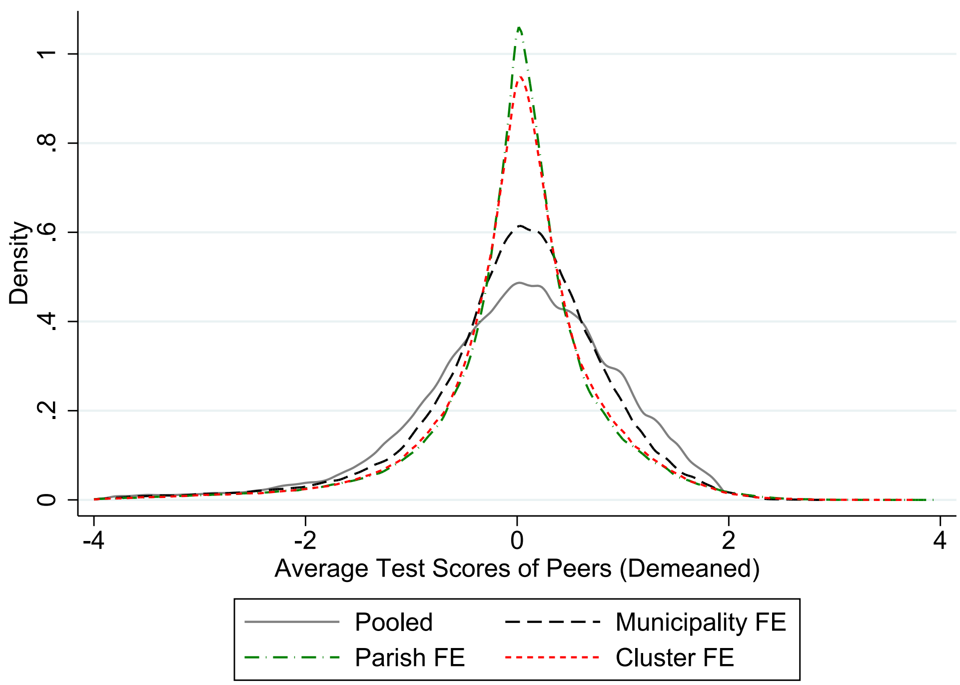


Notes: This figure presents a binned scatter plot (with 20 equal-sized groups) depicting relationship between school-level average test scores (our measure of "School Quality") and school inputs. These figures control for neighborhood-by-cohort fixed effects. All variables are transformed into percentiles.

Variation in School Attributes

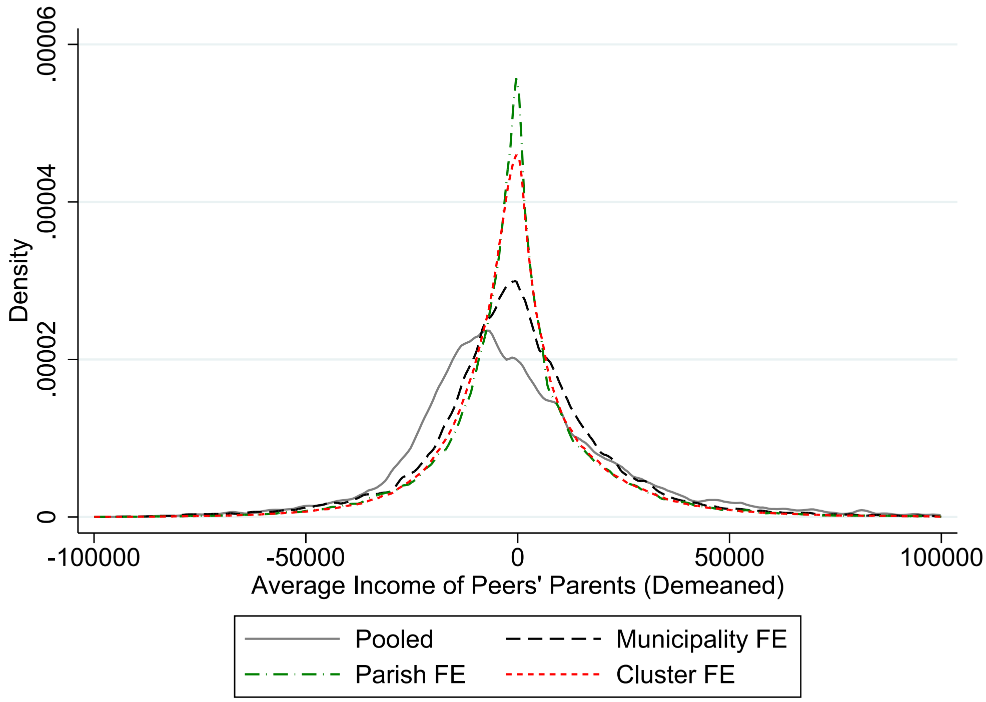
This section provides evidence on the variation in school attributes and average school test scores at different geographical levels in Denmark. By plotting these densities after controlling for fixed effects at various geographical levels, this section documents the important level of sorting of household in various attributes. Figures 2.26-2.30 present the results. Table 2.10 reports the corresponding variance of each of the school attributes in Figures 2.26-2.30 at different geographical levels.

Figure 2.26: Density of School's Average Test Scores by Geographical Level



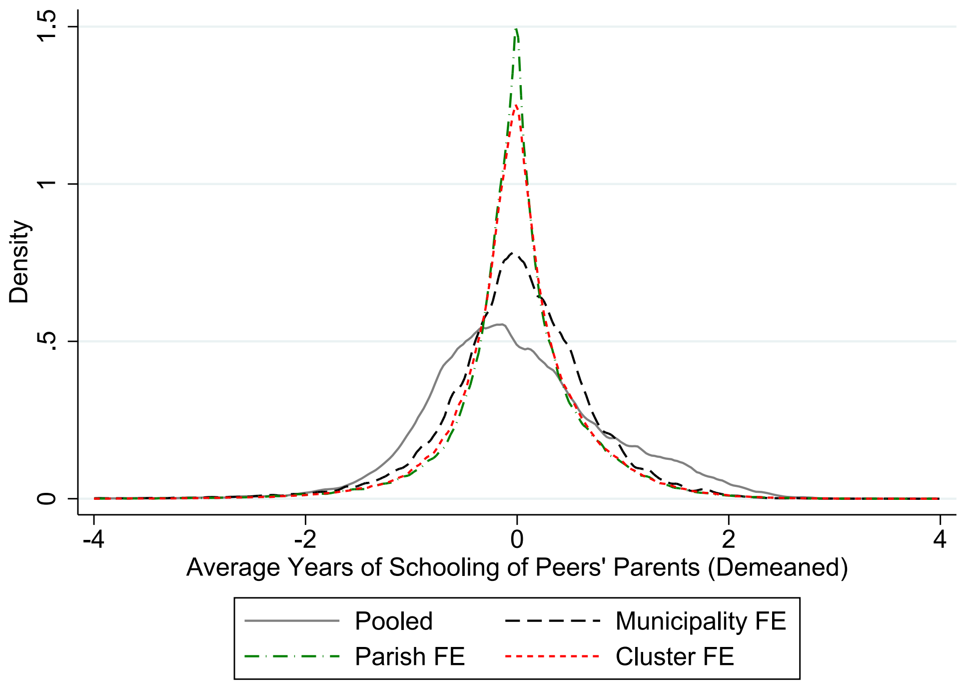
Notes: The figure shows the distribution of the school-level average of test scores at different geographical levels in Denmark. The solid line shows the distribution for the whole country after taking out school-by-year fixed effects. The dashed line presents the distribution after taking out school-by-municipality fixed-effects. The dash-dot line shows the distribution after taking out school-by-parish fixed-effects. Finally, the dot line presents the distribution after taking out school-by-cluster fixed-effects. The corresponding standard deviations are 0.92, 0.84, 0.75, and 0.75, respectively for the pooled, municipality FE, parish FE, and cluster FE.

Figure 2.27: Density of School's Average Parental Income by Geographical Level



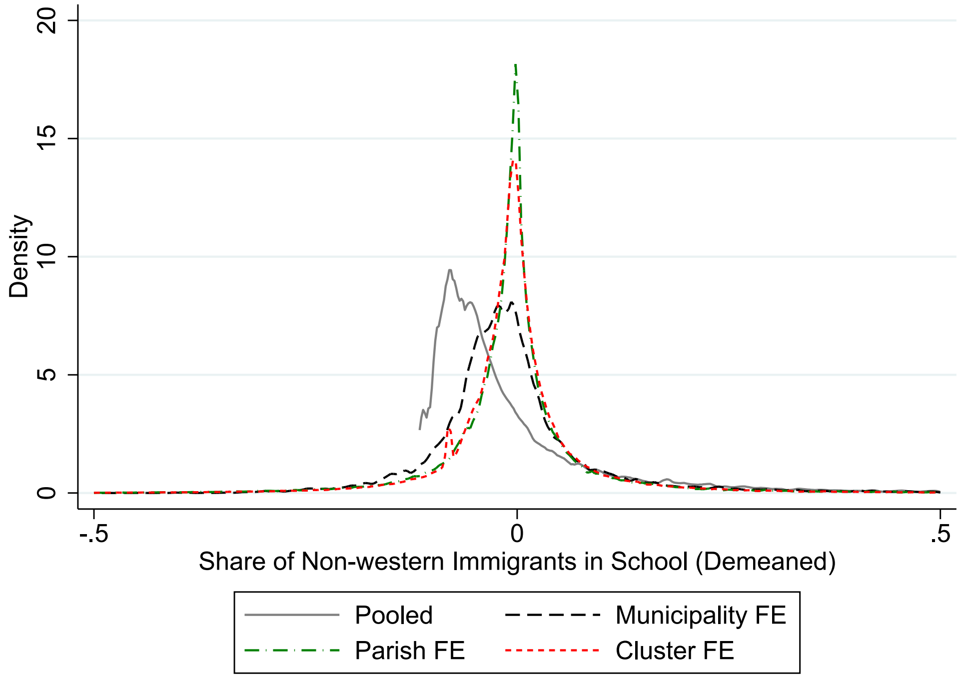
Notes: The figure shows the distribution of the school-level average of parental income of students at different geographical levels in Denmark. The solid line shows the distribution for the whole country after taking out school-by-year fixed effects. The dashed line presents the distribution after taking out school-by-municipality fixed-effects. The dash-dot line shows the distribution after taking out school-by-parish fixed-effects. Finally, the dot line presents the distribution after taking out school-by-cluster fixed-effects. The corresponding standard deviations are about 20300, 19000, 17000, and 16100, respectively for the pooled, municipality FE, parish FE, and cluster FE.

Figure 2.28: Density of School's Average Parental Education Level by Geographical Level



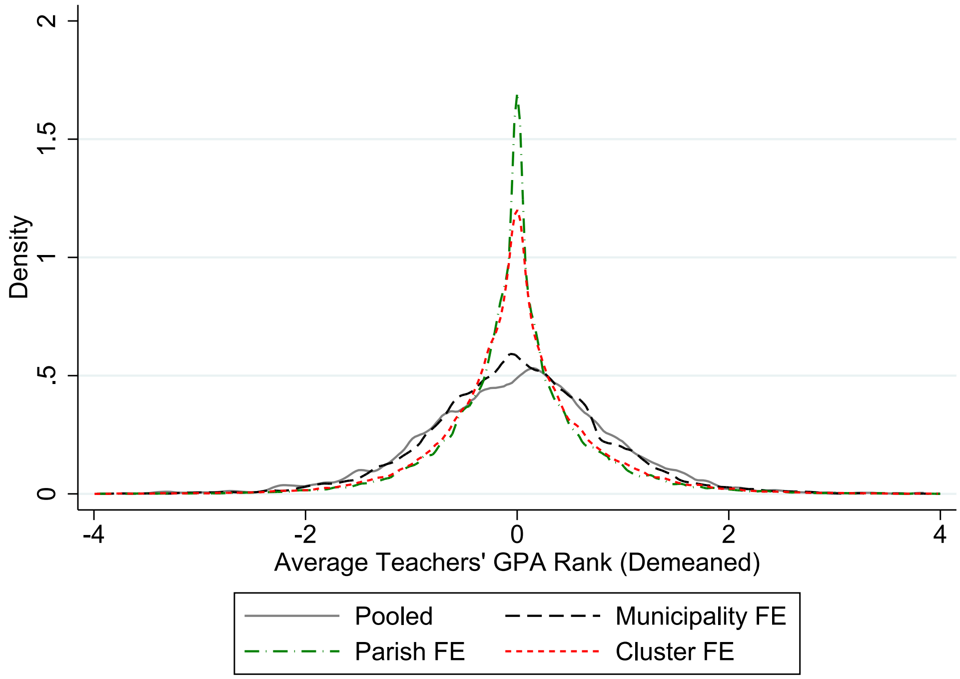
Notes: The figure shows the distribution of the school-level average of students' parental years of formal schooling at different geographical levels in Denmark. The solid line shows the distribution for the whole country after taking out school-by-year fixed effects. The dashed line presents the distribution after taking out school-by-municipality fixed-effects. The dash-dot line shows the distribution after taking out school-by-parish fixed-effects. Finally, the dot line presents the distribution after taking out school-by-cluster fixed-effects. The corresponding standard deviations are about 0.84, 0.67, 0.58, and 0.58, respectively for the pooled, municipality FE, parish FE, and cluster FE.

Figure 2.29: Density of Share of Immigrants in School by Geographical Level



Notes: The figure shows the distribution of the school-level share of students who are non-western immigrants, at different geographical levels in Denmark. The solid line shows the distribution for the whole country after taking out school-by-year fixed effects. The dashed line presents the distribution after taking out school-by-municipality fixed-effects. The dash-dot line shows the distribution after taking out school-by-parish fixed-effects. Finally, the dot line presents the distribution after taking out school-by-cluster fixed-effects. The corresponding standard deviations are about 0.098, 0.087, 0.077, and 0.076, respectively for the pooled, municipality FE, parish FE, and cluster FE.

Figure 2.30: Density of School's Average Quality of Teachers (Teachers' GPA Ranks) by Geographical Level



Notes: The figure shows the distribution of the school-level average of teachers' quality measures by teachers' GPA rank in college, at different geographical levels in Denmark. The solid line shows the distribution for the whole country after taking out school-by-year fixed effects. The dashed line presents the distribution after taking out school-by-municipality fixed-effects. The dash-dot line shows the distribution after taking out school-by-parish fixed-effects. Finally, the dot line presents the distribution after taking out school-by-cluster fixed-effects. The corresponding standard deviations are about 0.89, 0.83, 0.67, and 0.70, respectively for the pooled, municipality FE, parish FE, and cluster FE.

Table 2.10: Variance of School Attributes at Different Geographical Levels

	Pooled	Municipality	Parish	Cluster
Variance of School's Average Test scores	0.85	0.71	0.56	0.56
Variance of School's Average Parental Income	$412 * 10^6$	$361 * 10^6$	$289 * 10^6$	$260 * 10^6$
Variance of School's Average Parental Years of Schooling	0.71	0.45	0.34	0.34
Variance of Share of Non-western Immigrants	0.010	0.008	0.006	0.0005
Variance of School's Average Teachers' GPA Rank	0.89	0.83	0.67	0.70

Notes: This table reports the variance of different school characteristics in our analysis sample at various geographic levels, presented in Figures 2.26-2.30. Incomes are converted from DKK to 2010 USD. An exchange rate of 6.7 DKK per US dollar is used to obtain the dollar values.

Correlation between different neighborhood and school characteristics

Table 2.11 reports correlations between different neighborhood attributes. Table 2.12 presents the correlation between different school characteristics. Table 2.13 shows the correlation between various teachers' characteristics (averaged at school level) and other school characteristics such as average test scores and parental education level.

Table 2.11: Correlation Between Neighborhood Attributes

	Log Households Gross Income	Households Years of Educ.	Share Criminals	Share Married Households	Share non-Westerners
Log Households Gross Income	1				
Households Years of Educ.	0.838***	1			
Share Criminals	-0.196***	-0.207***	1		
Share Married Households	0.662***	0.360***	-0.275***	1	
Share non-Westerners	-0.290***	-0.136***	0.304***	-0.337***	1
Share Non-Intact Households	-0.316***	-0.217***	0.126***	-0.347***	0.0595***

Notes: This table reports the correlation between the different small cluster level neighborhood attributes we consider in our main strategy. The sample contains all individuals owning a property in Denmark who have a child attending ninth grade in a public school between 2002 and 2006. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 2.12: Correlation Between School Characteristics

	Avg Log HH Gross Income	Avg Parental Years of Educ.	Avg Test Scores	Share of Criminal Parents	Share Married HH	Share non-Westerners
Avg Parental Log HH Gross Income	1					
Avg Parental Years of Educ.	0.72***	1				
Avg Test Scores	0.61***	0.62***	1			
Share of Criminal Parents	-0.07***	-0.04***	-0.07***	1		
Share Married Households	0.23***	0.17***	0.40***	-0.07***	1	
Share non-Westerners	-0.46***	-0.14***	-0.29***	0.04***	-0.10***	1
Share Non-Intact Households	-0.06***	-0.04***	-0.21***	0.03***	-0.57***	-0.03***

Notes: This table reports the correlation between the different school characteristics in our analysis sample. We use an indicator for whether any individual in a given neighborhood ever committed a crime or not. We exclude traffic-related crimes. Parental incomes are measured in 2010 USD. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 2.13: Correlation Between Average Teachers' Characteristics at School Level and Average Test Scores at School Level

	Avg Test Scores	Avg Teachers' GPA Rank	Avg Teachers' Tenure	Avg Teachers' Experience	Avg Teachers' Age at Grad.
Avg Test Scores	1				
Avg Teachers' GPA Rank	0.20***	1			
Avg Teachers' Tenure	0.13***	0.06***	1		
Avg Teachers' Experience	-0.04***	-0.05***	0.25***	1	
Avg Teachers' Age at Graduation	-0.05***	-0.04***	0.05***	-0.18***	1
Avg Parental Years of Educ.	0.62***	0.22***	0.03***	-0.18***	0.01***

Notes: This table reports the correlation between the different school characteristics in our analysis sample. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

The correlation between school characteristics (test scores, characteristics of peers' family, and teachers' characteristics) across years are high ranging from 0.6 to 0.8 over the studied years.

Principal Component Analysis of Teachers, School Peers and Neighborhood Characteristics

This paper analyzes the high inter-correlation between different school and neighborhood attributes, which results from household sorting across neighborhoods and school catchment areas. In this section, we rely on a principal component analysis (PCA), to further study the dimensionality of the vector of peer, neighborhood and teacher characteristics. In line with our main specification in the paper, we conduct this analysis at the cluster-by-year level.

We consider the following attributes for teachers, peers and neighborhoods.

Data

Teacher

- Teacher GPA rank at teacher's college;
- Teacher age when graduating from teacher's college;
- Teacher tenure (only observed up to 3 years in a given school);
- Teacher years of experience.

Peers

- Average GPA
- Average household gross income excluding transfers;

- Average Maximum household years of education;
- Share married;
- Share non-intact households;
- Share non-western foreigners;

Neighborhood

- Average household gross income excluding transfers;
- Maximum household years of education;
- Share married;
- Share non-intact households;
- Share non-western foreigners;
- Share criminals.

We consider cohorts who graduate 9th grade between 2011 and 2014, where we observe teacher characteristics. For these cohorts, we also observe 8th grade test scores, allowing us to consider measures of school value-added.

Characteristics of Schools, Teachers and Neighborhoods This Section presents results from conducting a PCA with all characteristics.

Table 2.14: PCA Analysis for All Characteristics

	1st Comp.	2nd Comp.	3rd Comp.	4th Comp.
Teacher Characteristics				
Teacher Rank in Teacher's College	.0271706	.1473107	.0321433	.0196915
Teacher Tenure at School	.0108797	.0764253	.0002731	-.1141694
Teacher Years of Experience	.003582	.028741	.0187275	-.0309382
Teacher Age at Graduation	-.0127341	-.0823452	-.02759	-.0420844
School/Peers Characteristics				
School Value-Added	.0295779	.1864005	.049533	.0229591
School Quality	.0783029	.4650294	.0556236	.0122102
Log HH Gross Income	.0863491	.4891705	-.0085603	-.176162
HH Years Schooling	.0800897	.4607689	.0488045	-.0243734
Share Married HH	.0545231	.2890615	.1803574	.511938
Share non-Westerners	-.0630871	-.3561896	.1279932	.4279111
Share Non-Intact HH	-.0209754	-.10926	-.2020331	-.6396876
Neighborhood Characteristics				
Log HH Net Income	.3688293	-.0723542	.0320394	.0008346
HH Years of Educ.	.3721087	-.0583692	-.0339573	.0147747
Share Criminals	-.180049	.0357573	.0645153	-.0135187
Share Married HH	.3522192	-.0890711	.1202022	-.0144853
Share non-Westerners	-.2200087	-.0212978	.480456	-.1118107
Share of Non-Intact HH	-.1753892	.0612921	-.4292206	.1849959
Share of Variance Explained	27%	13%	8%	6%

Notes: First four principal components and their loadings.

PCA of Teacher Characteristics This Section presents the results from the PCA for teacher, peer and neighborhood characteristics, separately.

Table 2.15: PCA Analysis for Teacher Characteristics

	1st Comp.	2nd Comp.	3rd Comp.	4th Comp.
Teacher Rank in Teacher's College	.140	-.265	.946	.1224
Age at Graduation	-.344	.793	.214	.453
Teacher Tenure at School	.583	.547871	.142	-.582
Teacher Years of Experience	.721	-.012	-.196	.663
Share of Variance Explained	33%	27%	25%	16%

Notes: Four principal components and their loadings.

PCA of School Characteristics

Table 2.16: PCA Analysis for School Characteristics

	1st Comp.	2nd Comp.	3rd Comp.	4th Comp.
School Value-Added	.195	.042	.036	.170
School Quality	.478	.008	-.045	.025
Log HH Gross Income	.498	-.186	-.062	-.031
HH Years Schooling	.469	-.021	-.117	.008
Share Married HH	.309	.523	.042	.058
Share non-Westerners	-.336	.469	.023	.055
Share Non-Intact HH	-.129	-.673	-.047	-.023
Teacher Rank in Teacher's College	.15	.032	.017	-.065
Teacher Age at Graduation	-.084	-.037	-.327	.801
Teacher Tenure at School	.072	-.103	.568	.554
Teacher Years of Experience	.020	-.047	.73	-.084
Share of Variance Explained	43%	30%	12%	9%

Notes: First four principal components and their loadings.

PCA of Neighborhood Characteristics

Table 2.17: PCA Analysis for Neighborhood Characteristics

	1st Comp.	2nd Comp.	3rd Comp.	4th Comp.
Log HH Net Income	.509	.061	.208	.241
HH Years of Educ.	.507	-.051	.189	.078
Share Criminals	-.291	.311	.895	-.123
Share Married HH	.487	.205	.106	.330
Share non-Westerners	-.286	.670	-.241	.588
Share of Non-Intact HH	-.279	-.636	.223	.682
Share of Variance Explained	53%	17%	13%	11%

Notes: First four principal components and their loadings.

2.8.3 Estimation Approaches: BDD and Temporal Shocks

Proximity Theorem

A simple starting point for recovering the WTP for measured school quality uses the proximity theorems of Fisher [1966a]. Given the granularity and richness of our data, they provide a useful framework for estimating Equation (2.1).

Theorem 2 (Proximity Theorems, Strong and Weak)**.** *Let* $\boldsymbol{\omega}^0 = (\boldsymbol{\alpha}^0, \boldsymbol{\beta}^0, \boldsymbol{\gamma}_1^0, \boldsymbol{\gamma}_2^0)$ *be the true parameters of model (1). Assume the model is full rank. Define* $\hat{\boldsymbol{\omega}}$ *as the OLS estimator. The strong proximity theorem asserts that* $\operatorname{plim} \hat{\boldsymbol{\omega}} \to \boldsymbol{\omega}^0$ *as* $\frac{\mathrm{Var}(\varepsilon)}{\mathrm{Var}(\boldsymbol{\omega}'\boldsymbol{\mathcal{Z}})} \to 0$*. A weak form asserts that* $\operatorname{plim} \hat{\boldsymbol{\omega}'} \to \boldsymbol{\omega}^0$ *as* $\frac{\mathrm{Cov}(\boldsymbol{\mathcal{Z}},\varepsilon)}{\mathrm{Var}(\boldsymbol{\omega}'\boldsymbol{\mathcal{Z}})} \to 0$*, component-wise.*

The least squares estimator of ω^0 is consistent if (1) the variance of the disturbance is small relative to the observables or (2) if the probability limit of the correlation between the disturbances and regressors is small, or if both (1) and (2) hold. Our cluster fixed effect strategy controls for neighborhood sorting at a very local level, so the application of this method is plausible in our data.[66]

To gain intuition about this approach, consider a simple least squares estimator of bivariate regression $Y = \tau Q + U$, $\mathbb{E}(U) = 0$, $\mathrm{Var}(U) > 0$ with finite variances. Under independent sampling, the probability limit of the least squares estimator $\hat{\tau}$ for τ converges to

$$\operatorname{plim}\hat{\tau} = \tau + \frac{\mathrm{Cov}(Q,U)}{\sigma_Q^2}.$$

σ_Q^2 denotes the variance of Q. Q may be correlated with U. If $\frac{\mathrm{Cov}(Q,U)}{\sigma_Q^2}$ is small, the result follows. Alternatively, if the variance of U is "small" relative to that of Q, $\operatorname{plim}\hat{\tau} \to \tau$.

A test for the validity of the proximity theorem is that as the geographic unit studied becomes smaller, the R-squared of the hedonic regression should increase. We conduct this

66. Estimation based on fixed effects implicitly invokes the proximity theorem in that it assumes all essential heterogeneity within the unit represented by the fixed effect is negligibly small.

test, and confirm the hypothesis. Another approach applies methods based on Altonji et al. [2005] to our hedonic framework.[67] Our estimates are robust to the presence of selection on unobservables within reasonable ranges of assumptions about the importance of unobservables.

Boundary Discontinuity Design

Black [1999a] uses a boundary discontinuity design (BDD) to address the endogeneity of property prices and average student quality. She uses a regression discontinuity design (RDD) approach that is based on boundary fixed effects in order to compare houses that are near but on opposite sides of school catchment area borders. The identifying assumption for that approach is that unobserved amenities vary continuously at the border while school characteristics are determined by attendance zones that are discontinuous at boundaries. Estimates using this approach are typically five times smaller than cross-sectional estimates (see Bayer et al., 2007, Gibbons et al., 2013).

Analysts also exploit changes in boundaries (see, for instance, Bogart and Cromwell, 2000 and Ries and Somerville, 2010). Those studies lack information on both neighborhood quality and school characteristics. Moreover, variation can arise from houses that are geographically distant from each other, albeit close to the boundary (and thus in different types of neighborhoods). Finally, BDD designs rely on variation at the boundary, which, without further evidence, may not be representative of the broader population. Our approach using cluster fixed effects, by construction, is not subject to these issues.

67. We conduct their test in Section 2.5.5.

2.8.4 Spatial Decomposition of Inequality

This appendix studies inequality within neighborhood units in Denmark. Theil's T decomposition is computed as follows.[68] Consider the population of Danish households, N. Let y_i denote the income of household i, where $i = 1, ..., N$. Define arithmetic mean income as m. Consider the exhaustive partition of the population into mutually-exclusive neighborhoods $k = 1, ..., K$. Let N_k indicate the population of neighborhood k, where $\sum_{i=1}^{K} N_k = N$. Letting F_i be the frequency of Y_i, the Generalized Entropy class of inequality indices across neighborhoods is given by:

$$GE(a) = \frac{1}{a(a-1)}[\sum f_i(\frac{y_i}{m}^a) - 1], a \neq 0 \;\&\; a \neq 1,$$

$$T_1 = GE(1) = \sum f_i \frac{y_i}{m} \log \frac{y_i}{m}, \quad a = 1,$$

$$T_0 = GE(0) = \sum f_i \log \frac{y_i}{m}, \quad a = 0.$$

$GE(a)$ index can be decomposed as

$$GE(a) = GE_W(a) + GE_B(a)$$

where $GE_W(a)$ is within-group inequality and $GE_B(a)$ is between-group inequality and

$$GE_W(a) = \sum v_k^{1-a} s_k^a GE_k(a),$$

which, for Theil's T index is as follows:

$$GE_W(1) = \sum s_k GE_k(1)$$

68. Our exposition is based on Jenkins [2021].

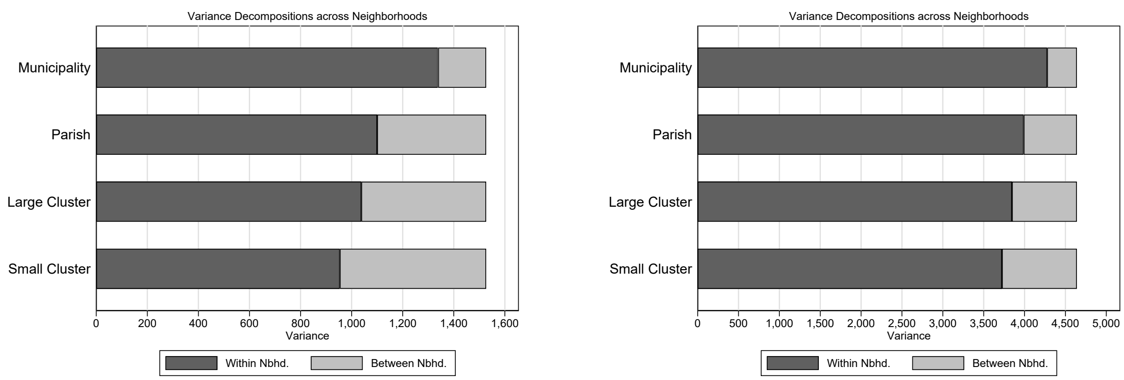
where $v_k = \frac{N_k}{N}$ is the number of persons in subgroup k divided by the total number of persons (subgroup population share), and s_k is the share of total income held by k's members (subgroup income share). $GE_k(a)$, inequality for subgroup k, is calculated as if the subgroup were a separate population, and $GE_B(a)$ is derived assuming every person within a given subgroup k received k's mean income, m_k.

Variance Decomposition of Housing Characteristics

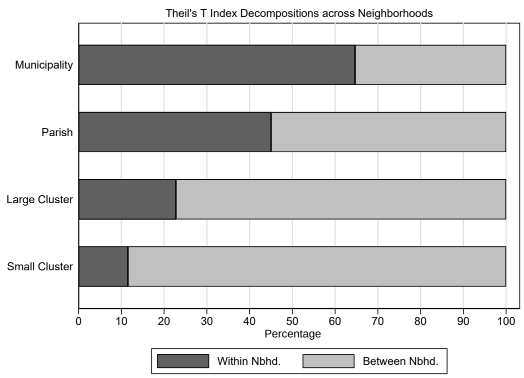
Figure 2.3 in the main text presents the variance decomposition of housing characteristics such as the number of apartments and the number of floors. Figure 2.31 presents the results for other housing attributes such as the age of the building and the living area. Figure 2.32 presents the Teil's T Index decomposition of building characteristics across different neighborhood units in Denmark. The patterns are similar to the variance decomposition.

Figure 2.31: Variance Decomposition of Housing Characteristics across Neighborhoods

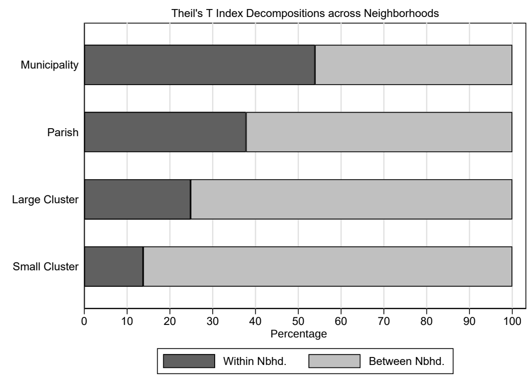
(a) Age of the Building (b) Living Area



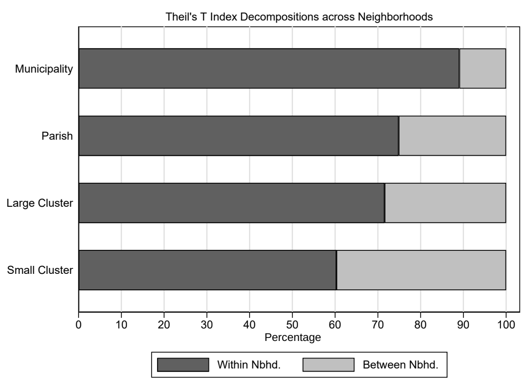
Notes: This figure presents the variance decomposition of building characteristics across different neighborhood units in Denmark. Panel (a) focuses on the age of the building and Panel (b) shows the statistics for the living area.



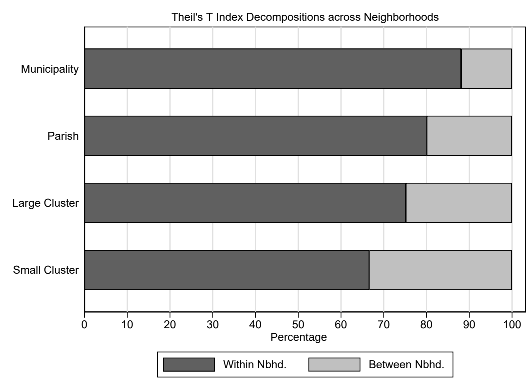
(a) Number of Apartments



(b) Number of Floors



(c) Age of the Building



(d) Living Area

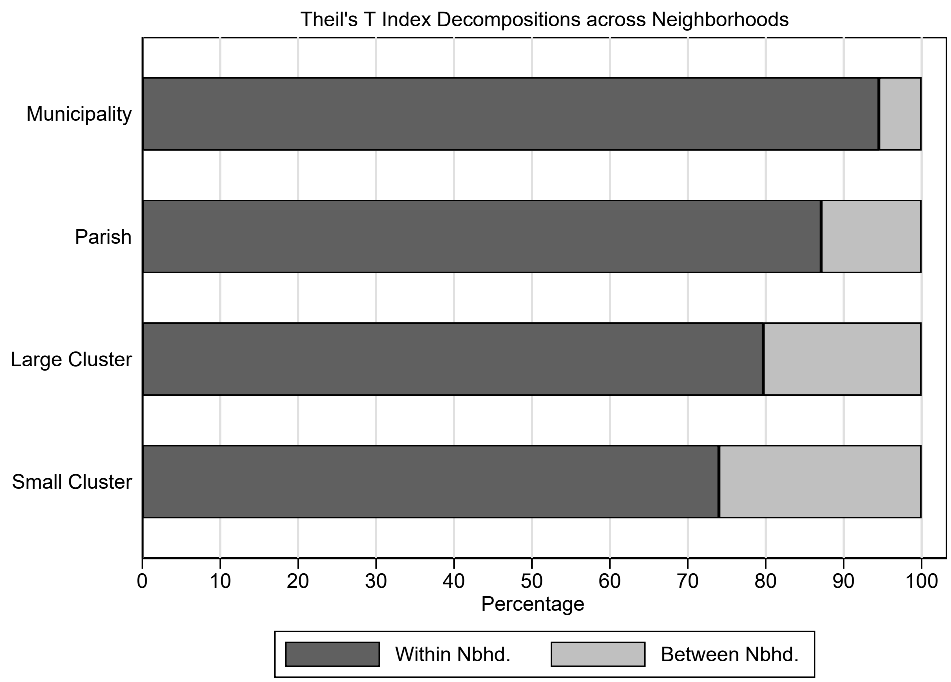
Figure 2.32: Theil's T Decomposition of Housing Characteristics across Neighborhoods

Notes: This figure presents the Theil's T decomposition of building characteristics across different neighborhood units in Denmark. Panel (a) focuses on the number of floors, Panel (b) shows the statistics for the number of apartments, Panel (c) uses the age of the building, and Panel (d) considers the living area.

Decomposition of Income Inequality

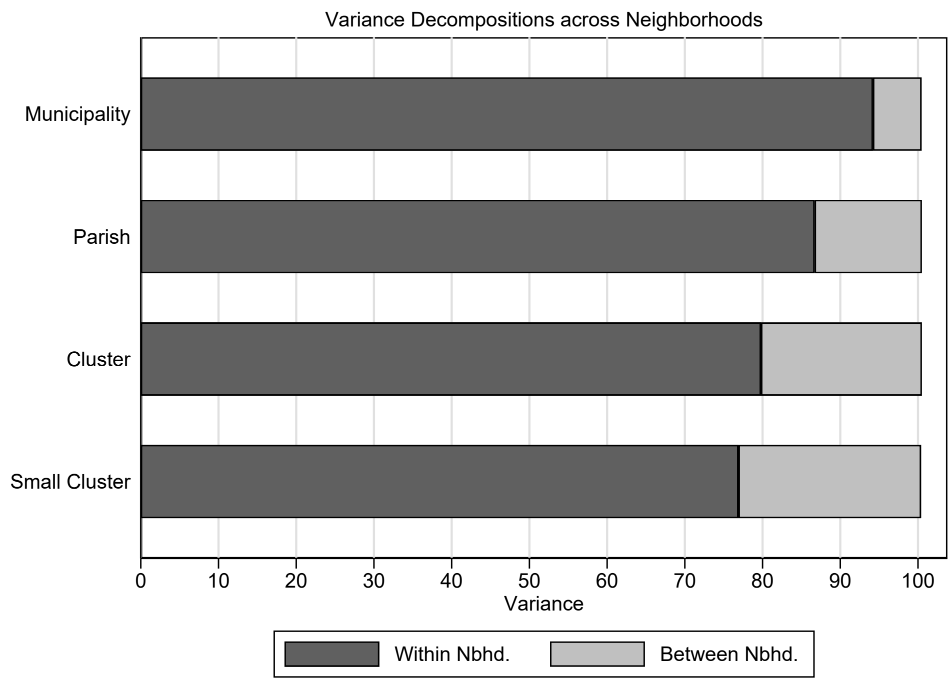
We also analyze the spatial decomposition of income inequality across neighborhoods in Denmark using the Theil's T Index. Figure 2.33 presents the Theil's T decomposition of family income across different neighborhood units in Denmark, i.e., municipality, parish, cluster, and small cluster levels. Also, Figure 2.34 presents the variance decomposition of family income across different neighborhood units in Denmark, which shows patterns simialr to Figure 2.33.

Figure 2.33: Theil's T Decomposition of Family Income across Neighborhoods



Notes: This figure presents Theil's T decomposition of family income across different neighborhood units in Denmark. The disposable income is used as the measure of income. Family income is averaged over 2010–2015.

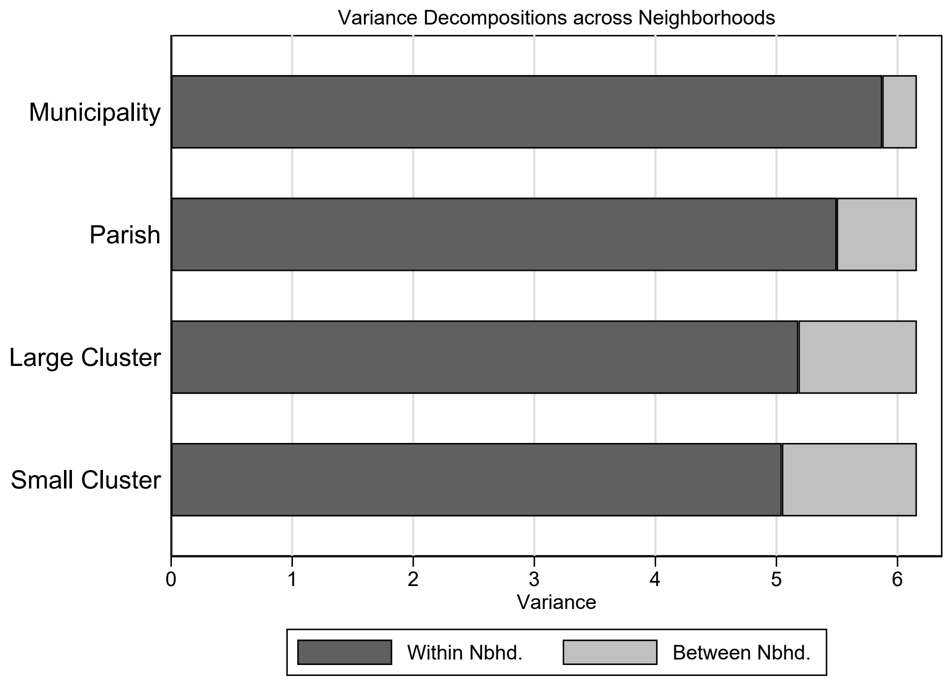
Figure 2.34: Variance Decomposition of Family Income across Neighborhoods



Notes: This figure presents the variance decomposition of family disposable income across different neighborhood units in Denmark. The disposable income is used as the measure of income. Family income is averaged over 2010–2015. The variances in the figure are presented in units of 10^7.

Overall, our results suggest that families who reside in the same cluster are significantly more alike in terms of their observed characteristics, compared to the pool of families who live in the same parish or municipality. Yet, much variation in observed characteristics remains within clusters. The results presented here focus on income measures, but we observe a similar pattern when we look at other characteristics such as education level (see Figure 2.35).

Figure 2.35: Variance Decomposition of Education Levels across Neighborhoods

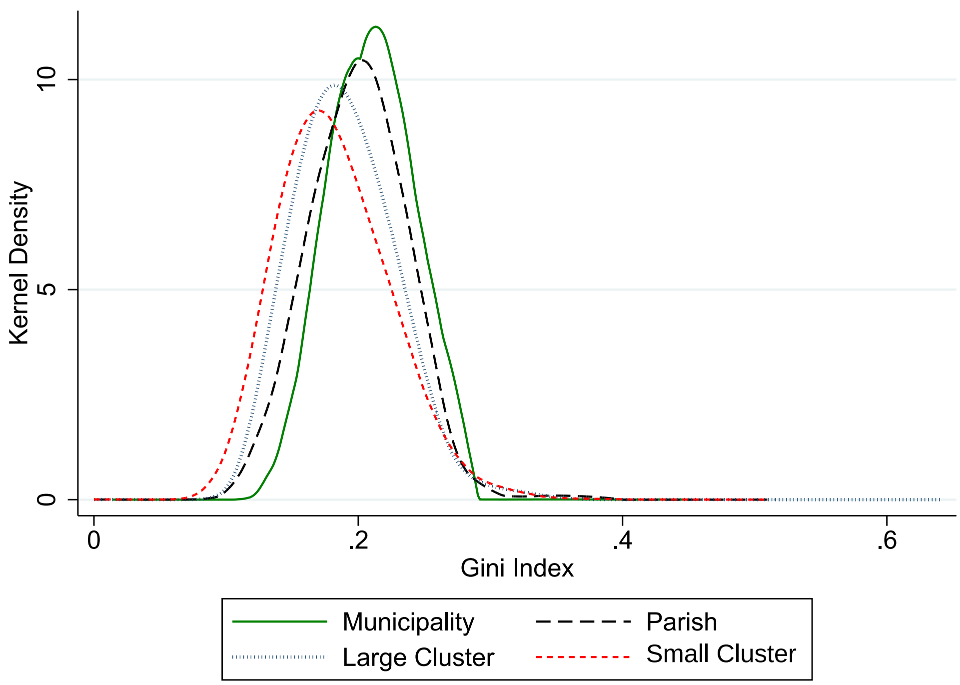


Notes: This figure presents the variance decomposition of education level of residents across different neighborhood units in Denmark. The completed years of schooling are used as the measure of education level.

Gini Index

We use the Gini Index measure to analyze the income inequality at different neighborhood units in Denmark. Figure 2.36 plots the distribution of neighborhood Gini index for various neighborhood units, i.e., municipality, parish, cluster, and small cluster, which shows that the income inequality is dramatically lower at cluster levels, suggesting more homogeneity among individuals living close to each other in a our neighborhood unit.

Figure 2.36: Gini Index Kernel Density by Neighborhood Unit

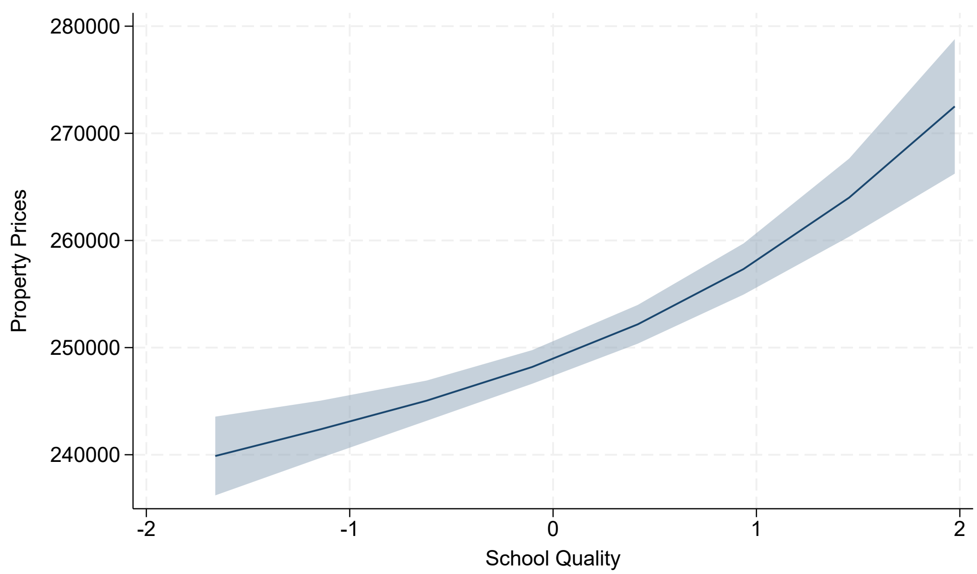


Note: Kernel density of neighborhood Gini coefficient of household income inequality in Denmark, 2015. The disposable income is used as the measure of family income. Family income is averaged over 2010-2015. The right axis (dash-dot line) indicates the ratio of segregation using parishes to that using clusters.

2.8.5 Local Linear Hedonic Price Function

To explore the heterogeneity of the WTP over the distribution of school quality, this Appendix provides local linear estimates of the WTP, controlling for the same neighborhood and housing characteristics as well as fixed effects at the cluster-by-cohort level. Specifically, Figure 2.37 depicts the nonlinear relationship between house prices and school quality—the hedonic price function. Its slope reflects the WTP for school quality.

Figure 2.37: Local Linear Estimates of the WTP



Notes: The figure shows a local linear fit of the relationship between property prices and school quality, after controlling for neighborhood and housing characteristics, as well as cluster-by-cohort fixed effects. School quality is measured by taking the average test scores of students attending a given school. The sample contains property values for parents who have a child attending the ninth school grade between 2002 and 2006.

2.8.6 An Overview of Diegert et al. [2022]

To address omitted variables bias, researchers have relied on sensitivity analyses which bound estimates when there is selection on unobservables. This approach, developed by Altonji et al. [2005] and refined by Oster [2019], requires an exogeneity assumption implying that omitted variables are uncorrelated with the included controls.

The method proposed by Diegert et al. [2022] relaxes this strong assumption. It allows analysts to use different sensitivity parameters to bound the parameter of interest in the presence of endogenous observables. One parameter, $\bar{r}_X$, captures the relative magnitude of selection on unobservables and selection on observables, i.e., the proportion of the variance explained of WTP due to unobservables compared to observables. A second sensitivity parameter, $\bar{r}_Y$, measures the relative magnitude of the effect of unobservables on the outcomes. Finally, a third sensitivity parameter, $\bar{c}$, captures the level of endogeneity – the relationship between the observables and unobservables.

This approach does not require analysts to set all three parameters simultaneously. Theorem 2 of Diegert et al. [2022] characterizes the identified set for the parameter of interest in a regression with both observed and unobserved covariates using only $\bar{r}_X$ as a sensitivity parameter.[69] It characterizes the *breakdown point*, $\bar{r}_X^{bp}$, which is the largest amount of selection on unobservables under which in the population our coefficient of interest – the WTP for school quality – would still be nonnegative. We use this theorem to compute the breakdown point of $\bar{r}_X^{bp} = 61\%$ presented in the main text. We note that in their framework, the breakdown point cannot be greater than 100%.

Key to the computation of the breaking point is the choice of covariates. Our approach is to use our specification presented in Section 2.5.5, where we include a large set of small-cluster-by-school-catchment-area level attributes and housing characteristics. We first residualize the data using our small neighborhood level fixed effects. The breakdown point is

69. This is in contrast with the analysis of Oster [2019], where two tuning parameters need to be chosen.

then to be interpreted in light of the number of relevant controls that are included in the regression of interest, comprising in our case of $\boldsymbol{X}$ and $\boldsymbol{H}$, the neighborhood and housing characteristics.

In the same vein, we estimate another breakdown point using the framework of Oster [2019] – commonly referred to as *Oster's delta*. Here, it is required to impose an additional assumption on R^2_{long}, the "long" regression including unobservables as regressors, as compared to the R^2_{short} of the short regression without unobservables. Her proposed rule of thumb is $R^2_{\text{long}} = 1.3 \times R^2_{short}$. Imposing exogeneity and following her rule of thumb, would imply a breakdown point of around 1143%. Alternatively, setting $R^2_{\text{long}} = 1$ implies a breakdown point of 51%.

2.8.7 Capitalization of Teacher Attributes

This section presents results of our main specification, where instead of average test score of students as our measure of school quality, we consider a set of teacher attributes. We also consider the first principal component of these teacher attributes as the main covariate of interest.

The results below largely support the conclusion that parents are also paying for better teachers, by locating in a better school catchment area, within clusters. This is particularly the case for teacher tenure, our teacher quality index, as well as when using the 1st principal component.

Table 2.18: Valuation of Teacher Characteristics

	(1) Rank	(2) Tenure	(3) Experience	(4) Graduation Age	(5) Quality Index	(6) 1st Principal Component
Estimates	.003	.006***	.002	-.001	.016***	.004*
	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)
R^2	.53	.53	.53	.53	.53	.53
Observations	129,507	129,507	129,507	129,507	129,507	129,507

Notes: This Table shows estimates from our main specification, replacing our measure of school quality by various measures of teacher characteristics. The sample includes all homeowners with children attending ninth grade in public schools between 2011 and 2015. Each column presents estimates of the capitalization of teacher characteristics into house prices. Column (1) considers teacher rank in teacher's college, column (2) uses the teacher's tenure in school, column (3), teacher's years of experience, column (4), teacher's age at graduation from teacher's college, column (5) our index of teacher quality, and column (6) the first principal component of the four first characteristics. Controls include small-cluster attributes measuring average household gross income, and education as well as fraction married, non-intact households, crime, and foreigners. Housing characteristics include the type of building, the number of floors, the number of units per building, the number of bedrooms, toilets, and bathrooms, the size of the living area, and the age of the property. Property values are logged and teacher quality measures are standardized such that the coefficients can be interpreted as the WTP, in percentage terms, for a one–standard deviation increase in teacher quality. Cluster-by-cohort fixed effects are included. Standard errors corrected for clustering at the school-cohort level are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

2.8.8 Summary Table of Estimates

Table 2.19 presents a variety of approaches we use to estimate WTP for school quality along with the estimates we find from each approach.

Table 2.19: Summary of the Strategies Employed to Identify the WTP for School Quality

	Method Description	Identifying Assumptions	Estimates
(1)	**Hedonic regressions:** Neighborhood fixed-effect model with neighborhood and housing attributes as controls (fixed-effects at varying levels of geographical granularity).	The residual variance of housing prices is small relative to that of the observables.	2.4%–4.0%
(2)	**Mixed continuous-discrete neighborhood choice model:** Estimating outcome equation while controlling for selection into neighborhoods and schools.	(i) Index sufficiency assumption in neighborhood choice equation; and (ii) as an the exclusion restriction, we use grandparents' neighborhood of residence as a factor that influences the household's neighborhood choice but not an individual's market house price.	1.6%–1.7%
(3)	**Capitalization effect:** Exploiting variation in school boundaries and the resulting variation in school quality.	School boundary changes are independent of unobservables in enrollment decisions.	2.2%
(4)	**Bounding the impact of unobservables:** Applying Altonji et al. [2005] and the extension to endogenous regressors by Diegert et al. [2022].	Deriving plausible magnitudes on the importance of unobservables.	breakdown point of 61%–1100%
(5)	**Alternative data:** Using transaction prices instead of government valuation of house values.	Measurement error in the transaction prices is independent of the measurement error in government valuations.	3.2%

CHAPTER 3

NEIGHBORHOOD EFFECTS AND CHILDREN'S LONG-TERM OUTCOMES WITH SADEGH ESHAGHNIA

Does growing up in a better neighborhood have positive consequences on long term labor market outcomes and educational attainment? We exploit a unique spatial dispersal policy that randomly resettled refugees across neighborhoods in Denmark from 1986-1998. We find that a higher quality neighborhood—as measured by the wage outcomes of children already living there—is significantly associated with increased market income in adulthood. Our mediation analysis reveals that the association between neighborhood quality and child adult income is fully mediated through the impact of assigned neighborhood on parental income.

3.1 Introduction

In this paper, we study the long-run effects of growing up in a better quality neighborhood on the children of refugees and asylum seekers in Denmark. We exploit quasi-random variation in assigned locations among refugees arising from the Danish Dispersal Policy (DDP), which was carried out from 1986 to 1998. We analyze the long-run impacts of assignment to these neighborhoods on educational attainment, labor force participation, and adult earnings of refugee children growing up in the randomly-assigned neighborhoods in Denmark.

Recent studies in the literature have exploited the Danish refugee dispersal policy to analyze the role of neighborhood characteristics on refugee outcomes such as labor market outcomes (Damm [2014]); Damm [2009]; Damm and Rosholm [2010]; Azlor et al. [2020]; Eckert et al. [2022]), labor market integration (Arendt [2022]), health (Hasager and Jørgensen [2021]; Foverskov et al. [2022]; Kim et al. [2023]), and participation in crime (Damm and Dustmann [2014]). Another strand of literature has used the dispersal policy to study the impact of refugees on natives' behavior and natives' outcomes such as voting behavior (Dustmann et al. [2019]) and skill acquisition (Foged and Peri [2016]).[1]

1. Some previous papers have studied refugee dispersal policies in other countries. See Edin et al. [2003]

While these studies analyze the impacts of the dispersal policy on the outcomes of refugees, this paper is the first, to the best of our knowledge, to examine the intergenerational effects of the neighborhood assignment by analyzing the long-run impacts on the children of refugees in terms of earnings, educational attainment, and employment. To this end, we exploit rich administrative panel data from Denmark. We use a mediation analysis framework to decompose the impacts of initial neighborhood assignment on child income in adulthood to the indirect impact through parents and the direct impact on children.

Another strand of the literature focuses on the sample of natives and exploits the variations in neighborhood exposure among children of families who move across neighborhoods at different ages of children. For example, using the US tax data, Chetty and Hendren [2018b] deploy a mover-design estimation strategy and exploit variation in the timing of children's moves across US commuting zones to identify the causal effects of place on children's long-term outcomes. They find that the adult incomes of children who move converge to the adult incomes of children of permanent residents in the destination at a rate of 4% per year of exposure.[2] In addition to other measures of neighborhood quality, we use a rank measure, inspired by Chetty and Hendren [2018b], which focuses on permanent residents' children in each area and uses their income rank in adulthood (after conditioning on cohort and parental income rank).

Exploiting the random assignment of refugees to Danish neighborhoods during the policy era, we find that the quality of the assigned neighborhood is positively and significantly related to both the parents' income and children's adulthood income. We find that assignment to a neighborhood where the mean income rank of children of permanent residents

for the causal effect on labor market outcomes of living in enclaves in Sweden; Chakraborty and Schüller [2022] for a similar analysis across numerous countries; Andersen et al. [2023] for integration of refugees in Norway; Kirstiansen et al. [2022] for the impact of neighborhood labor market characteristics on refugee employment outcomes in the Netherlands; Vogiazides and Mondani [2021] for determinants of inter-regional immobility of refugees in Sweden; Robinson et al. [2003] for an analysis of the dispersal policies in the Netherlands, Sweden, and the UK; Ravn et al. [2022] for the labor market policies for refugees in Denmark, Sweden, the Netherlands, and Germany. Brell et al. [2020] provide an overview of refugee labor market integration outcomes across nine different high-income countries. See Nielsen Arendt et al. [2022] for a review of the literature.

2. See Eshaghnia [2021] for a critical review of the mover-design estimation strategies for causal inference in the literature.

is 1 percentile higher (at a given level of parental income) increases child's income rank in adulthood by approximately .63 percentiles. In other words, children of refugees placed in better quality neighborhoods pick up 63% of the difference in permanent residents' outcomes associated with the assigned neighborhood.

To study the underlying mechanisms, we then conduct a simple mediation analysis. We find that these effects are fully accounted by the impact of assigned neighborhood quality on parental income. Our results provide evidence of significant neighborhood effects on the first generation of refugees in Denmark, while these translate to the second generation only through parents' labor market outcomes.

This paper unfolds as follows. Section 3.2 discusses the results of the dispersal policy in Denmark. Section Section 3.3 describes our data.

3.2 Dispersal Policy In Denmark

There have been heated debates about immigration policies among policy makers in many countries. In some Western European countries, policies around asylum seekers and refugees were politicized, where asylum seekers and refugees were perceived by some political parties as a burden. Some Western European governments used various policies to spatially "spread the burden" by dispersing ethnic minorities, especially asylum seekers and refugees (see Robinson et al. [2003] and Wren [2003]).

Denmark is one of the European countries that has adopted a refugee dispersal policy. As in many other European countries, the number of refugees increased sharply in Denmark during the 1980's. In this paper, we focus on the DDP, which was in place between 1986 and 1998.

Prior to the reform in 1986, refugees were allowed to choose where to settle, which led to a concentration of refugees in larger cities. A national dispersal policy implemented in 1986 aimed to distribute refugees more evenly across spatial units so that the costs of integration of immigrants could be more evenly allocated across municipalities.[3] The dispersal policy

3. First, the policy distributed refugees across the 15 Danish counties proportional to the number of inhabitants. Then, refugees were allocated to municipalities within counties (with a total of 278 municipalities)

succeeded in achieving a more dispersed geographical distribution of refugees.[4]

Damm [2005] investigates whether the DDP on new refugee immigrants carried out from 1986 to 1998 can be regarded as a natural experiment and whether refugees were randomly assigned to a location. She documents that the actual settlement has been influenced by five refugee characteristics and concludes that the initial location of new refugees 1986-1998 may be regarded as random, when controlling for age, origin, year of assignment, household size, and marital status – all of which are observable.

3.3 Data and Measures of Neighborhood Quality and Outcomes

3.3.1 Main Samples

Our main sample of analysis comprises all individuals whose parents were assigned quasi-randomly to neighborhoods in the first phase of the DDP from 1986 to 1998 and for whom we observe their later life outcomes. Child outcomes are last observed in the administrative registers between 2016 and 2021, depending on the specific variable of interest, which enables us to observe outcomes of the children of assigned refugees around the age of 30 years.

Next, we describe how we construct our neighborhood quality measure, as well as outcome variables.

3.3.2 Neighborhood Quality

Our measure of neighborhood quality is inspired by Chetty and Hendren [2018a]. Using the sample of native Danes, we compute the expected income rank of children of permanent residents (at age 30) in each neighborhood, conditional on their parental income rank and birth cohort.

according to population size (but also considering ethnic networks, access to education and job opportunities, as well as the availability of suitable housing). See Council [1996] for details.

4. Damm and Dustmann [2014] report that after 8 years, one in two households still lived in the area of initial assignment.Wren [2003] studies if the dispersal policy in Denmark has achieved its goals at macro vs micro levels. She argues that while, at a national level, the policy has achieved its objectives, at a regional scale the policy has effectively resulted in socio-ethnic spatial segregation in areas experiencing pre-existing deprivation and social exclusion.

Formally, let y_i denote the child's adult percentile rank based on their position in the national income distribution relative to all others in her birth cohort. Also, for child i, let $p(i)$ be the parents' percentile rank in the national distribution of parental income for the child i's birth cohort. Now, let $\bar{y}_{pks}$ denote the mean rank of children with parents at percentile p of the income distribution, residing in the neighborhood k for the entire childhood stage (from age 0 to 18), and birth cohort s. The mean child rank, given parents' rank in each neighborhood k and birth cohort s is approximated by a linear form:

$$y_i = \alpha_{ks} + \psi_{ks} p_i + \epsilon_i \, . \tag{3.1}$$

We obtain estimates of $\bar{y}_{pks}$ from the fitted values of the linear regression while restricting the sample to the native Danes:

$$w_{kp} \equiv \bar{y}_{pks} = \hat{\alpha}_{ks} + \hat{\psi}_{ks} p \, . \tag{3.2}$$

We obtain estimates of intercept α and slope ψ for each neighborhood using the sample of permanent resident Danes. Then, for a given refugee family with parental income rank p, we compute the quality of the assigned neighborhood (w_{kp}) from equation 3.2. We use the municipality as the neighborhood unit.[5]

3.3.3 *Outcome Variables*

We turn to describe the different outcomes we analyze.

Economic Outcomes In our main analyses, we focus on two definitions of children's adult economic outcomes. We study wage income excluding transfers and taxes to focus on earnings potential. Henceforth, we refer to this income definition as *pre-tax* income. This income measure is contrasted with a measure of annual net income, to account for the significant tax and transfer system in Denmark. Henceforth, we refer to this

5. There were 271 municipalities in Denmark during the sample years (i.e., 1982-2000). These 271 municipalities were merged into 98 large new municipalities in 2007. To maximize statistical precision, we use the post-2007 definition of municipalities.

income definition as *post-tax* income. Finally, we consider the employment status of the child. All these measures are computed at age 30 of the child.

Years of Education To capture human capital accumulation, we further compute a measure of individuals' years of education, by converting the degree completed to the theoretical years of completion. This measure is captured at age 28 of the child.

Tables 3.3, 3.4 and 3.5 in the Appendix report summary statistics for our sample.

3.4 Assigned Neighborhood Quality and Adult Outcomes of Children

3.4.1 Sorting Patterns

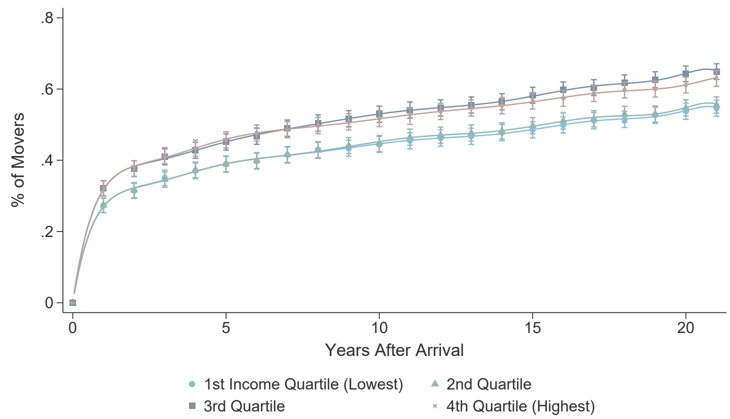
We examine the mobility patterns of refugees from their initially assigned neighborhoods over time. Figure 3.1 shows that even after ten years, more than 50% remain in their original location. This persistence is consistent across different treatment levels, though refugees assigned to higher-quality locations exhibit greater mobility. More importantly, this highlights that the DDP operates as a randomized controlled trial with imperfect compliance. As a result, we interpret our coefficients as intention-to-treat (ITT) estimates.

In addition, most studies on neighborhood effects rely on the Moving to Opportunity (MTO) experiment, which randomly provided voucher subsidies to help disadvantaged families move to better neighborhoods (Katz et al. [2001]; Goering et al. [2003]; Kling et al. [2007]; Gennetian et al. [2012]). Nearly half of the families did not use them, leading to noncompliance and selection bias, making it difficult to identify neighborhood effects by simply comparing outcomes across different neighborhoods.[6] Instead, the program measures the causal impact of offering vouchers by evaluating the difference in mean outcomes between families who received a voucher and those who did not (ITT). In contrast, DDP avoids self-selection into treatment, though noncompliance arose over time as individuals left

6. See Pinto [2018] for a more detailed discussion of this issue.

neighborhoods. This allows for the identification of a more relevant policy parameter: the ITT of assigning families to better neighborhoods on later life outcomes

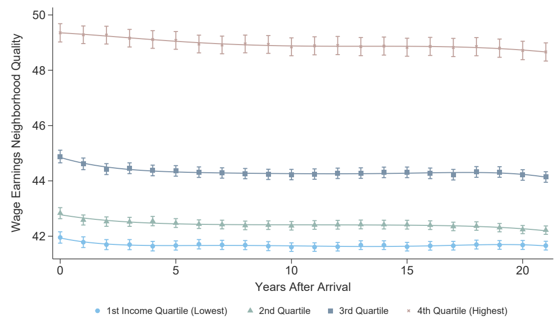
Figure 3.1: Percentage of Movers by Initial Neighborhood Quality at Assignment



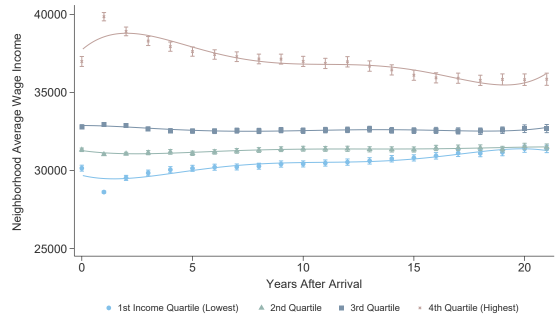
Notes: The figure illustrates the percentage of migrants who relocated from their initially assigned neighborhoods. To analyze relocation patterns, we categorize households into four quantiles based on their initial neighborhood quality at the time of assignment. The results suggest that even after ten years, more than 50% remain in their original location. This persistence is consistent across different treatment levels, though refugees assigned to higher-quality locations exhibit greater mobility. Using the `binsreg` command in `Stata`, a global polynomial regression model of degree 10 was fitted for plotting purposes. Confidence intervals were constructed using a piecewise polynomial of degree 1 with one smoothness constraint.

Even among movers, dividing the population by treatment levels suggests that individuals, on average, relocate to neighborhoods of similar quality. This pattern holds for the neighborhood quality measure defined in equation 3.2 (which is based on expected income rank of permanent resident children in each neighborhood, conditional on their parental income rank) as well as for annually updated measures of neighborhood quality like neighborhood average income and education levels. Figure 3.2, panels (a) and (b), illustrate these trends.

Figure 3.2: Sorting of Individuals by Initial Wage Neighborhood Quality of Assignment



(a) Neighborhood Quality based on Wage Income rank



(b) Neighborhood Avg. Wage Income

Notes: We divide the initial proportion of immigrant households into quartiles based on the initial neighborhood quality and track the neighborhood quality of residence over time. Panel (a) uses our neighborhood quality measure based on wage income, as defined in equation 3.2, while panel (b) performs the same exercise using the neighborhood's average wage income in each year as a proxy for neighborhood quality. Panel (a) does not update annually, as it captures information for the same cohort of permanent residents, whereas the neighborhood average wage income in panel (b) changes annually. Using the `binsreg` command in `Stata`, a global polynomial regression model of degree 10 was fitted for plotting purposes. The confidence intervals were constructed using a piecewise polynomial of degree 1 with one smoothness constraint.

3.4.2 Empirical Strategy

In this paper, we relate later life outcomes of children to the assigned neighborhood quality, through the following empirical specification:

$$y_{ikt} = \alpha + \beta w_{ikt-1} + \boldsymbol{\gamma'} \boldsymbol{x}_{it-1} + \varepsilon_{ikt} \tag{3.3}$$

where the variable y_{ikt} denotes the later life outcomes we presented in Section 3.3 for individual i measured in year t. Neighborhood quality is denoted as w_{ikt-1}, measured at the time of assignment, $t-1$. $\boldsymbol{x_{it-1}}$ denotes a vector of variables, known to the council and used at the time of assignment, including the age of the parents, country of origin, household size, and marital status.

Given the quasi-random allocation of refugees into neighborhoods, neighborhood quality at assignment is uncorrelated with ε_{ikt} in Equation (3.3), conditional on $\boldsymbol{x_{it-1}}$, the set of variables the council observed when assigning individuals. β can therefore be interpreted as an intent-to-treat (ITT) estimate of the effect of assignment to a better quality neighborhood on later life outcomes.

3.4.3 Results

We begin by presenting the ITT effects of neighborhood assignment on children's life outcomes in Panel A of Table 3.1. Column (1) reports the effect on pre-tax income of children at around age 30. There is a strong positive and statistically significant gradient of .63. In other words, assignment to a neighborhood where the mean income rank of children of permanent residents is 1 percentile higher (at a given level of parental income) increases child's income rank in adulthood by approximately .63 percentiles. Therefore, children of refugees placed in better quality neighborhoods pick up 63% of the difference in permanent residents' outcomes associated with the assigned neighborhood.

Table 3.1: Neighborhood Quality on Child Outcomes

	(1) Pre-Tax Income Rank	(2) Post-Tax Income Rank	(3) Employed	(4) Years of Educ.
Neighborhood Quality	0.630^{***}	0.544^{***}	0.006^{***}	0.063^{***}
	(0.095)	(0.100)	(0.001)	(0.009)
R-squared	0.066	0.051	0.048	0.115
N	7040	7040	6962	6467

Notes: This table shows results for our main specification which regresses an outcome, described in the column header, on our measure of neighborhood quality, which is the expected income rank of permanent resident children in each neighborhood, conditional on their parental income (as defined in equation 3.2). We include dummies for household size, country of origin, parental age, and marital status, information known by the council at assignment, as well as year of assignment fixed effects. Standard errors corrected for clustering at the municipality-year level are reported in parentheses. The sample includes all individuals whose parents were randomly assigned by the dispersal policy. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

We find similar, albeit slightly more muted effects, on post-tax income of children. The coefficient on neighborhood quality here stands at .54, as shown in column (2) of Table 3.1. This speaks to the previous literature which stresses the importance of the tax system in Denmark for reducing inequality (see for instance Landersø and Heckman, 2017, Eshaghnia et al., 2022). Thus, although pre-tax income is increased by better neighborhoods, this effect is reduced through the tax and transfer system.

Finally, in columns (3) and (4), we respectively report the effect on employment and education. Children assigned to better neighborhoods also experience significant returns on the extensive margin of the labor market, with a .6 percentage point increase in employment probability. Not only are the economic outcomes positively impacted, we also find that being assigned to a better neighborhood further increases educational attainment by 0.063 years.

3.5 Discussion

To delve into the mechanisms behind our results, we run a mediation analysis.

3.5.1 Framework

First, we implement a simple mediation analysis for the impact of the initial neighborhood assignment on the long-run income of children of the refugees and whether the effects on children are mediated through the impacts of neighborhood assignment on the income of parents. Let w denote the quality of the assigned neighborhood (w_{kp}) from equation 3.2 and M denote the mediator, i.e., the income of parents. Suppose y^c denotes the adult income of children. $\boldsymbol{X}$ denotes a set of baseline covariates.

Then, the total effect of neighborhood quality on adult income is already obtained (and reported in Table 3.1), as follows:

$$E(y^c|w, \boldsymbol{X}) = \beta_0 + \beta_1 w + \beta_2 \boldsymbol{X}$$

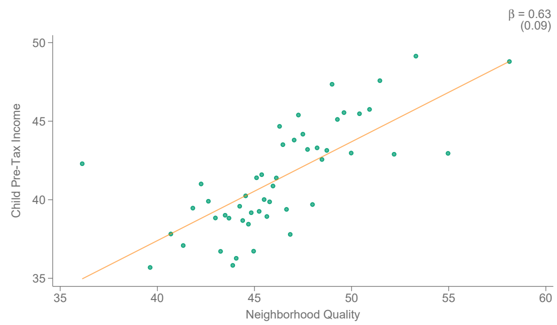
Our second regression includes the mediator as a regressor as follows:

$$E(y^c|w, m, \boldsymbol{X}) = \theta_0 + \theta_1 w + \theta_2 M + \theta_3 \boldsymbol{X}$$

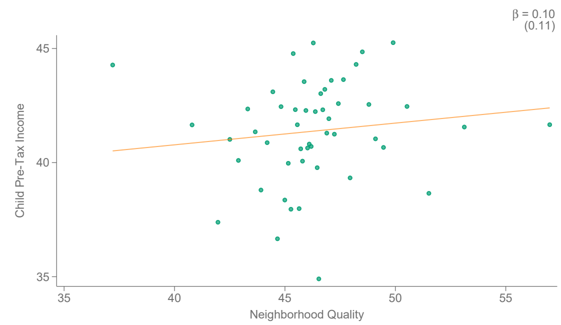
where θ_1 captures the direct effect and $(\beta_1 - \theta_1)$ gives us the mediated or indirect effect. The impact of neighborhood assignment on parents is obtained as follows:

$$E(M|w, \boldsymbol{X}) = \gamma_0 + \gamma_1 w + \gamma_3 \boldsymbol{X}$$

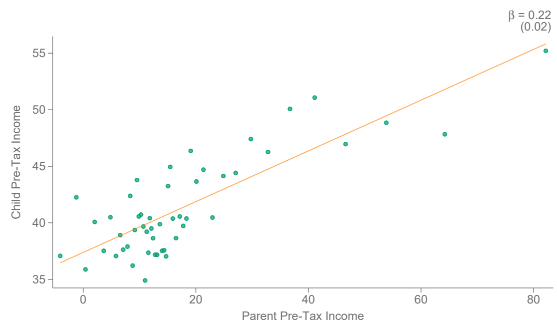
Figure 3.3: Mediation Analysis



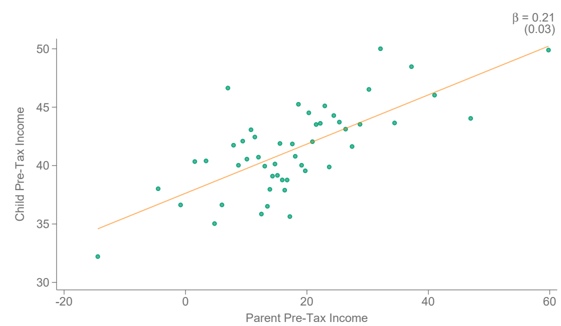
(a) Total Effect (β_1)



(b) Direct Effect (θ_1)



(c) Pre-Tax Income IGE



(d) Pre-Tax Income IGE, Controlling for Neighborhood Quality

Notes: Panel (a) presents the total effect of neighborhood quality on child pre-tax income. Panel (b) presents the direct effect, after controlling for parental income. Panel (c) depicts the intergenerational elasticity (IGE) between parent pre-tax income and child pre-tax income. Finally, panel (d) shows the same relationship, after controlling for assigned neighborhood quality.

Figure 3.3 reports results from our mediation analysis, where we first consider parental average gross income as a mediator. Panel (a) shows the total effect, which is a visual representation of the slope coefficient reported in Table 3.1. Panel (b) shows the direct effect. This is the relationship between neighborhood quality and child outcomes, after controlling for parental income. Strikingly, the slope reduces to .10 and becomes insignificant, suggesting that the mediated effect explains fully the effect of neighborhoods on outcomes for children.

Panel (c) of Figure 3.3 depicts the relationship between parental income and child income. To understand how neighborhood quality affects this relationship, we show the same correlation controlling for assigned neighborhood quality, in panel (d). The slope coefficient barely changes, indicating that the role of parental income on their offspring is not significantly changed after controlling for neighborhood quality.

Table 3.2: Neighborhood Quality on Parent Outcomes

	(1) Pre-Tax Income	(2) Post-Tax Income	(3) Employed	(4) Years of Educ.
Neighborhood Quality	2.544***	2.067***	0.032***	0.038**
	(0.139)	(0.124)	(0.002)	(0.015)
R-squared	0.463	0.318	0.197	0.050
N	7062	7050	7062	3481

Notes: This table shows results for our main specification, focusing on parental outcomes. Each columns shows estimates from a regression of an outcome, described in the column header, on our measure of neighborhood quality. We include dummies for household size, country of origin, parental age, and marital status, information known by the council at assignment, as well as year of assignment fixed effects. Standard errors corrected for clustering at the municipality-year level are reported in parentheses. The sample includes all individuals whose parents were randomly assigned by the dispersal policy. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Finally, Table 3.2 shows the effect of neighborhood quality at assignment on the mediator. The effect of neighborhood placement on parental income is of a much larger magnitude than the effect on income of the child. Assignment to a neighborhood where the mean income rank of children of permanent residents is one percentile higher increases parental income rank by approximately 2.5 percentiles. Similarly, the effects on parental employment and education are also stronger than for children, respectively standing at 0.032 and 0.038; assignment to a neighborhood where the mean income rank of children of permanent residents is one percentile higher is associated with around a three percentage point increase in employment and an increase of 0.038 in years of schooling.

Overall, these findings provide evidence that the first generation of refugees greatly benefits from being placed in higher quality neighborhoods, with positive returns on wages, employment status as well as educational attainment.[7] This then fully explains the impact of assigned neighborhood quality on the next generation, with little remaining variation in the outcomes of children directly explained by neighborhoods themselves.

7. Appendix 3.10.5 reports findings from the mediation analysis for post-tax income, employment status and educational attainment.

3.6 Sensitivity Analysis

As seen above, one natural parameter of interest is the *average direct effect* (ADE) of treatment:

$$ADE(d) \equiv \mathbb{E}[Y_i(M_i(d), 1) - Y_i(M_i(d), 0)]$$

Of separate interest is the effect of variation in the mediator due to treatment but holding the direct contribution of treatment itself fixed. More specifically, one can define the *average indirect effect* (AIE) or Average Causal Mediation Effect (ACME) of treatment as:

$$AIE(d) \equiv \mathbb{E}[Y_i(M_i(1), d) - Y_i(M_i(0), d)]$$

The ADE and AIE capture the direct and indirect mechanism, respectively. For instance, in the case of complete mediation the ADE is zero and the AIE contains the complete effect, or the *Average Treatment Effect* (ATE), which is what our previous mediation analysis suggests. Ignoring the presence of mediating variables and alternative mechanisms in the causal path from our treatment to our outcome, the typical ATE recovers:

$$ATE = \mathbb{E}[Y_i(M_i(1), 1) - Y_i(M_i(0), 0)]$$

$$= \mathbb{E}[Y_i(M_i(1), 1) - Y_i(M_i(1), 0)] + \mathbb{E}[Y_i(M_i(1), 0) - Y_i(M_i(0), 0)]$$

$$= ADE(1) + AIE(0)$$

By symmetry,

$$ATE = AIE(1) + ADE(0)$$

This shows that the shows that the ATE is a parameter that is a mix of direct and indirect effects. Furthermore, Imai, Keele, and Yamamoto [2010b] argue that the ACME and ADE are not identified in the standard design, where the treatment is randomized or ignorable conditional on pre-treatment covariates, and the mediator/outcome variables are measured. This is to the simple reason that a potential outcome required for the calculation of indirect and direct effects is never observed. Imai, Keele, and Yamamoto [2010b] prove that an additional assumption is therefore required for identification: sequential ignorability. This assumption can be written as:

Assumption 1 (Sequential Ignorability (SI)) (Imai et al. [2010b])

$$\{Y_i(d', m), M_i(d)\} \perp D_i \mid X_i = x, \quad Y_i(d', m) \perp M_i(d) \mid D_i = d, X_i = x,$$

where X_i is a vector of the observed pre-treatment confounders, $0 < Pr(D_i = d \mid X_i = x)$, and $0 < p(M_i = m \mid D_i = d, X_i = x)$ for $d = 0, 1$, and all x and m in the support of X_i and M_i, respectively.

The first step of the sequence of assumptions, is the well known exogeneity condition in econometrics. In the DDP, the assumption is expected to hold since treatment is randomized. The second step assumes that, given the actual treatment status and pre-treatment confounders, the observed mediator is ignorable. This second condition is much stronger and isn't expected to hold even in experimental settings.

The results shown in Imai, Keele, and Yamamoto [2010b] have vast implications for the Linear Structural Equation model. For instance, they show that the Baron and Kenny's [Baron and Kenny, 1986] interpretation is valid if the sequential ignorability holds. It is important to remember that Figure 3.3 is nothing more than a representation of Baron and Kenny. Hence, the arguments follow through if this assumption holds in practice.[8]

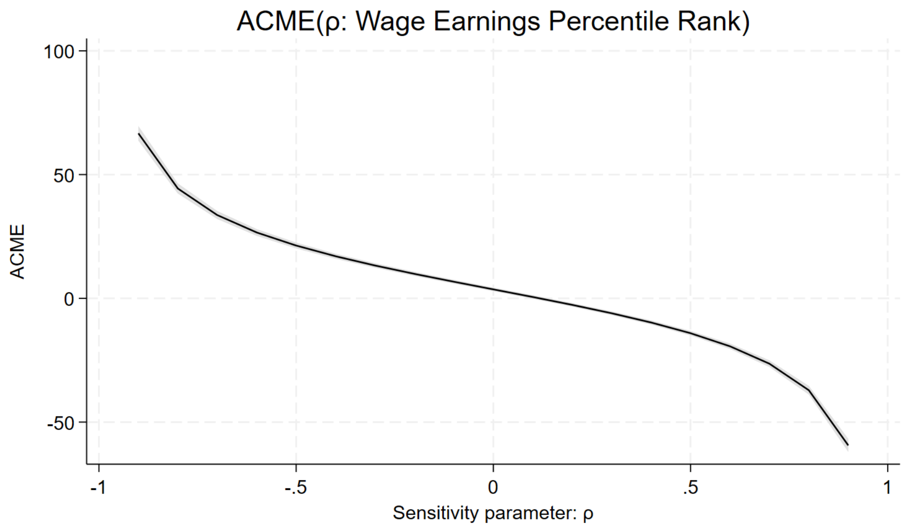
3.6.1 Simulation

The sequential ingnorability assumption cannot be tested with the data; therefore, a sensitivity analysis is necessary to assess the robustness of the results to violations of this assumption. We implement the parametric algorithm described in Imai, Keele, and Tingley [2010a]. For this, we categorize the treatment into a binary variable, classifying refugees into either low-quality neighborhoods or high-quality neighborhoods using the median value

8. Unfortunately, this assumption does not have testable implications and is known to be a nonrefutable assumption [Manski, 2009].

while the neighborhood quality is computed from equation 3.2. The sensitivity analysis indicates that the results are highly sensitive to even small violations of the assumption (see Figure 3.4).

Figure 3.4: Sensitivity Analysis Simulation



Notes: The treatment has been dummy-coded into high and low-income quality. ρ represents the correlation between ε_{i1} and ε_{i2} in equations 3.4 and 3.5. Under SI, ρ should be equal to zero. The simulation has been implemented in Stata using the mediate package [Hicks and Tingley, 2011].

Following Hicks and Tingley [2011] the mediation analysis in section 5 can be described with the following two equations,

$$M_i = \beta_0 + \beta_1 D_i + \xi_1^\top X_i + \varepsilon_{i1}, \tag{3.4}$$

$$Y_i = \theta_0 + \theta_1 D_i + \theta_2 M_i + \xi_2^\top X_i + \varepsilon_{i2}. \tag{3.5}$$

A violation of the SI assumption leads to a correlation between ε_{i1} and ε_{i2}, which is denoted by ρ in Figure 3.4. Under SI, ρ should be equal to zero. Using this theory, Figure 3.4 shows that for the point estimate of the ADE to be 0 the correlation between ε_{i1} and ε_{i2} must be approximately 0.10. Under SI, the mediation effect will then be given by $\beta_1\theta_2$. Or, similarly, θ_1 shows the direct effect while $\beta_1 - \theta_1$ gives us the mediated or indirect effect.

The algorithm uses the following structure:

Algorithm 1 Causal Mediation Analysis Procedure [Imai, Keele, and Yamamoto, 2010b]

Fit models for the observed outcome (equation 3.5) and mediator variable (equation 3.4).
Simulate model parameters from their sampling distribution. The multivariate normal distribution is used as an asymptotic approximation.
for each iteration **do**
    (a) Simulate the potential values of the mediator
    (b) Simulate the potential outcomes given the simulated values of the mediator
    (c) Compute the average causal mediation effect
end for
Compute the point estimate of the average causal mediation effect as well as its uncertainty estimates using the sample median and the sample standard deviation of the distribution of mediation effects.

The absolute value of ρ represents the degree of variation that a confounder would need to explain for the ACME to be unidentified—either due to a violation of the sequential ignorability assumption or because the ACME is not statistically different from zero. This value should be compared to the regression's R^2 as a benchmark.

3.7 Testing for Mechanisms: A simple case

Much of the literature on mediation analysis identifies the effect of M on Y (conditional on D) by assuming conditional unconfoundedness for M as discussed in the previous section. Alternative strategies include using an instrument for M [Frölich and Huber, 2017], a Difference-in-Differences (DiD) approach [Deuchert et al., 2019], and, when feasible, double randomization techniques such as parallel, paired and cross-over designs in experimental settings [Imai, Tingley, and Yamamoto, 2013]. In this paper, we follow Kwon and Roth [2024], which provides a methodology to explore mechanisms without imposing strong assumptions to identify the effect of M on Y (conditional on D), unlike previous methods that require stringent assumptions. In part, they can use less assumptions because they try to answer an easier question: is the effect of D on Y fully explained through its effect on M? Kwon and Roth [2024] define this as the sharp null of full mediation, which will be our object of interest moving forward.

To study mechanisms, suppose we observe $(Y, M, D) = (Y(D, M(D)), M(D), D)$ where

D is the binary treatment of interest (assigned to a high-income neighborhood) and M is a potential mediator (parental employment status or a dummy variable indicating whether a parent has high or low income). For simplicity, we assume that D and M is binary. The analysis in Kwon and Roth [2024] requires:

- Conditional Random assignment : $D|X \perp Y(\cdot,\cdot), M(\cdot)$ and $0 < \mathbb{P}(D=1) < 1$
- Monotonicity in D: $M(1) \geq M(0)$ almost surely

Random assignment is achieved by construction through the DDP. Monotonicity implies that being assigned to a high-income neighborhood weakly increases the likelihood of parents being employed.

Kwon and Roth [2024] focus on the sharp null hypothesis of full mediation [9,10] which is satisfied if and only if, $Y(d,m) = Y(m)(\text{ a.s. })\forall d, m$. Using the fact that under the sharp null of full mediation $Y(1,1) = Y(0,1)$ one can show that the following identifiable densities have to hold:

$$\mathbb{P}(y \in Y, M = \text{Low Inc. Parents}|D = \text{Low Qual. Neigh.}) \geq \mathbb{P}(y \in Y, M = \text{Low Inc.}|D = \text{High Qual. Nbhd.}) \tag{3.6}$$

$$\mathbb{P}(y \in Y, M = \text{High Inc. Parents}|D = \text{High Qual. Nbhd.}) \geq \mathbb{P}(y \in Y, M = \text{High Inc.}|D = \text{Low Qual. Nbhd.}) \tag{3.7}$$

The results in Kitagawa [2015] imply that these testable implications are sharp[11] but due to imperfect compliance in the Dispersal Policy, it is uncertain whether the property will hold. Nonetheless, the methodology in Kwon and Roth [2024] follows through by setting $D = Z$, based on the assumption that if Z is a valid instrument for the effect of D on Y, and if the sharp null hypothesis holds, then Z affects Y only through M. See Figure 3.5 for a visual representation of this argument. Robustness checks can also explore relaxing the monotonicity assumption. Kwon and Roth [2024] provide a framework for this.

9. Can the effect of D on Y be explained fully by a candidate mechanism (or set of mechanisms) M? This test has the usual interpretations of a null hypothesis test. If the sharp null is satisfied, then M is the only mechanism that matters. If we reject the sharp null, then there is evidence that mechanisms other than M are having an effect.

10. If the sharp null of full mediation is satisfied, then D is a valid instrument for the LATE of M on Y. Then, the instrument only affects Y through M. In case the null is not satisfied, there would be a direct effect from D to Y, invalidating the instrument.

11. Equivalent to testing for the exclusion restriction for the validity of an instrumental variable.

The proposed causal mediation relationship can be represented with a Directed Acyclic Graph (DAG) as in Figure 3.5. The path from Z to D to Y relies on the assumption that neighborhood assignment serves as a valid instrument for neighborhood of residence. The path from D through M to Y assumes the sharp null of full mediation holds. A shaded line from D to Y, if present, would violate the sharp null of full mediation, indicating a direct effect.

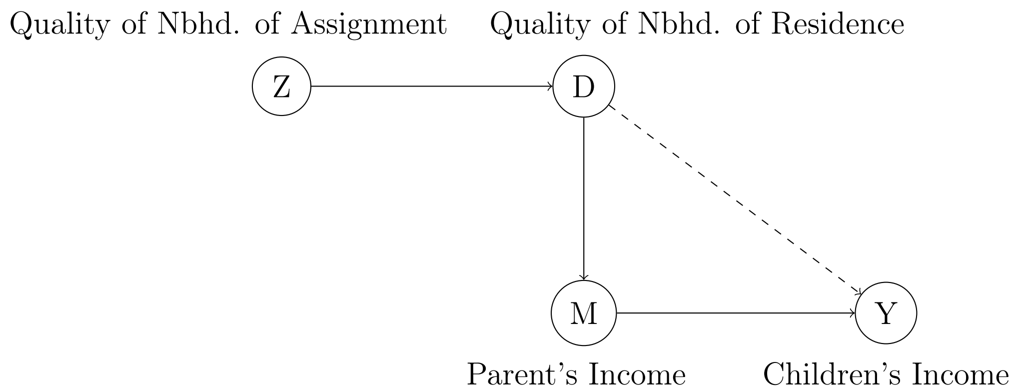


Figure 3.5: Directed Acyclic Graph illustrating the proposed structural model

Notes: Directed Acyclic Graph illustrating the proposed structural model, assuming the validity of instrument Z, neighborhood of assignment, and the null hypothesis of sharp mediation.

Accordingly, in this standard setting, following Kitagawa [2015], we utilize his algorithm and a variance-weighted Kolmogorov-Smirnov (KS) statistic to derive a p-value of 0.91. Hence, there is no evidence to reject the sharp null of full mediation. This provides additional support for the conclusion that the mediated effect fully explains the impact of neighborhoods on children's outcomes. In addition, we obtain similar results when using a mediator dummy that captures high and low levels of education and parental employment status. One concern is that the Kitagawa test has been criticized for being underpowered, and in this setting, we cannot guarantee sharpness, although Kwon and Roth [2024] conjecture that it will remain sharp.

Let P and Q be the conditional probability distributions of (Y, D) given $Z = 1$ and $Z = 0$, respectively. Let ξ be a tuning parameter. In our results, we do the bootstrap 500 times. The different steps for the test, suggested by Kwon and Roth [2024], are summarized below.

Algorithm 2 Kitagawa [2015] (Algorithm 2.1)

1: **Step 1:** Sample (Y_i^*, D_i^*), $i = 1, \ldots, m$, randomly with replacement from $H_N = \hat{\lambda} P_m + (1 - \hat{\lambda}) Q_n$ and construct the empirical distribution P_m^*.

2: Similarly, sample (Y_j^*, D_j^*), $j = 1, \ldots, n$, randomly with replacement from H_N and construct the empirical distribution Q_n^*.

3: **Step 2:** Calculate a bootstrap realization of the test statistic:

$$T_N^* = \left(\frac{mn}{N}\right)^{1/2} \max \left\{ \begin{array}{l} \sup\limits_{-\infty \leq y \leq y' \leq \infty} \left\{ \dfrac{\left|Q_n^*([y, y'], 1) - P_m^*([y, y'], 1)\right|}{\xi \vee \sigma_{P_m^*, Q_n^*}([y, y'], 1)} \right\}, \\ \sup\limits_{-\infty \leq y \leq y' \leq \infty} \left\{ \dfrac{\left|P_m^*([y, y'], 0) - Q_n^*([y, y'], 0)\right|}{\xi \vee \sigma_{P_m^*, Q_n^*}([y, y'], 0)} \right\} \end{array} \right\}$$

where

$$\sigma^2_{P_m^*, Q_n^*}([y, y'], d) = (1 - \hat{\lambda}) P_m^*([y, y'], d)(1 - P_m^*([y, y'], d)) + \hat{\lambda} Q_n^*([y, y'], d)(1 - Q_n^*([y, y'], d)).$$

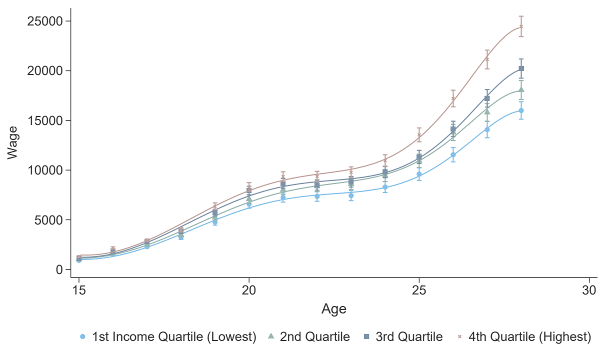
4: **Step 3:** Repeat Steps 1 and 2 many times to obtain the empirical distribution of T_N^*. For a nominal level $\alpha \in (0, 1/2)$, compute the bootstrap critical value $c_{N,1-\alpha}$ as the $(1 - \alpha)$ quantile of the empirical distribution.

5: **Step 4:** Reject the null hypothesis (1.1) if $T_N > c_{N,1-\alpha}$. The bootstrap p-value is the proportion of repetitions such that $T_N^* > T_N$.

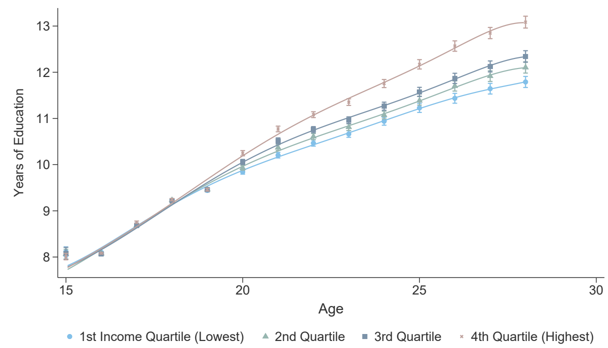
3.8 Dynamic Effects Over Time

In this section, we analyze how employment, wage, and education outcomes of children of refugees evolve over their life cycle. First, we examine wage, employment, and education levels over time by the initial neighborhood quality of assignment, as shown in Figure 3.6.

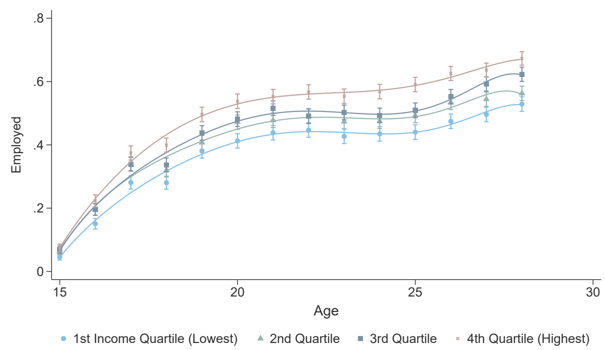
Figure 3.6: Life-Cycle Dynamics by Initial Neighborhood Quality of Assignment



(a) Wage Trajectories



(b) Education Trajectories



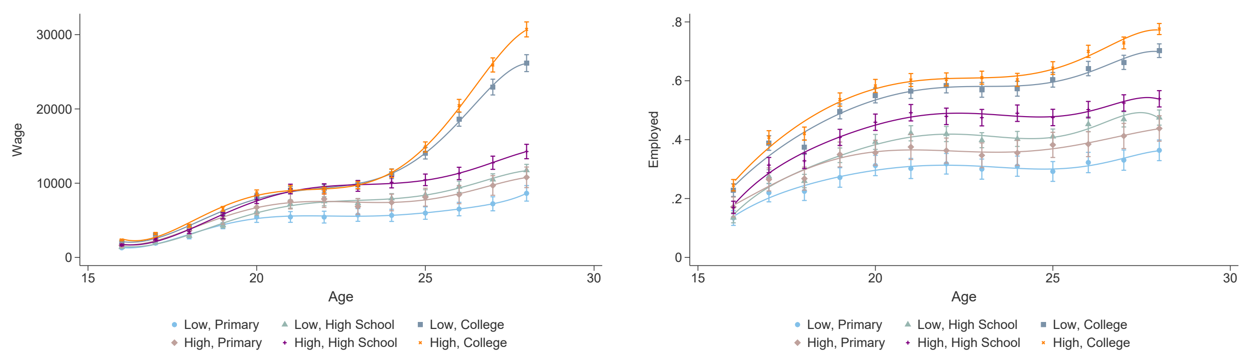
(c) Employment Trajectories

Notes: Panel (a) presents the wages of the children of refugees treated during the tenure of the DDP. Panel (b) shows years of education, while Panel (c) displays employment. We observe these outcomes until the individuals reach 28 years of age. To analyze heterogneity of outcomes, we categorize households into four quantiles based on their initial quality at the time of assignment. Using the `binsreg` command in `Stata`, a global polynomial regression model of degree 10 was fitted for plotting purposes. Confidence intervals were constructed using a piecewise polynomial of degree 1 with one smoothness constraint.

Second, we explore gender dynamics (see Figure 3.11). Women tend to have more years of schooling, aligning with broader patterns observed in Nordic countries. This trend remains consistent within our sample of refugees. However, there are no strong differences. In addition, neighborhood quality effects vary by gender (Figure 3.15). Women benefit more in education from high-quality neighborhoods, while wage trajectories remain similar across genders.

Third, Figure 3.12 illustrates how wages, employment, and education levels vary based on the family's timing of arrival in Denmark—before birth, between ages 0 and 5, or after age 5. All three outcomes decrease monotonically with age at arrival. Individuals whose parents resided in the country prior to their birth achieve better outcomes. These patterns may partly reflect a citizenship premium, a topic deserving further investigation. Furthermore, timing of arrival interacts with neighborhood quality (Figure 3.16). The largest wage and education gaps emerge among those born in Denmark, suggesting that early exposure to high-quality environments yields compounding benefits.[12]

Figure 3.7: Life-Cycle Dynamics by Education Level and Neighborhood Quality at Assignment



(a) Wage Trajectories by Education Level and Initial Neighborhood Quality

(b) Employment Trajectories by Education Level and Initial Neighborhood Quality

Notes: Panel (a) presents average wages and Panel (b) reports years of education for children of refugees treated under the DDP. Results are disaggregated by the child's education level—Primary, High School, and College—and neighborhood quality at assignment, where neighborhood quality is classified as low or high based on a median split of pre-tax income. Outcomes are observed until individuals reach 28 years of age. Using the `binsreg` command in `Stata`, a global polynomial regression model of degree 10 was fitted for plotting purposes. Confidence intervals were constructed using a piecewise polynomial of degree 1 with one smoothness constraint.

12. Moreover, we analyze wage outcomes across different levels of schooling—primary, high school completion, and college (see Figure 3.16b). Additionally, we examine the interaction between schooling levels and the initial neighborhood quality of assignment (see Figure 3.7).

3.9 Conclusion

We exploit quasi-random variation in the assigned neighborhood income among the refugee population in Denmark who were subject to the DPP. We find that a higher neighborhood quality is significantly associated with higher income for both parents and children (in their adulthood) and with children's long-run educational attainment. Children of refugees placed in better neighborhoods pick up about 63% of the difference in permanent residents' outcomes associated with the assigned neighborhood. We find that the impact of assigned neighborhood quality is significantly larger for parents' market income compared to children's market income. Strikingly, our findings show that the effect of neighborhoods on the later life outcomes of children is fully mediated by their parental income. We argue that these findings are sensitive to violations of the sequential ignorability assumption. To address this, we introduce a novel approach in the intergenerational mobility literature for testing indirect effects, leveraging the state-of-the-art methodology of Kwon and Roth [2024]. Specifically, we test the sharp null of full mediation using a variance-weighted Kolmogorov-Smirnov (KS) statistic and obtain a p-value of 0.91. This result suggests that the indirect effect of the neighborhood, operating through the parent to the child, is the sole mechanism driving the ITT estimates of neighborhood effects on refugee children documented in this paper for the first time in the literature.

3.10 Appendix

3.10.1 Summary Statistics

Table 3.3: Summary Statistics: Child Characteristics

	Mean	Median	SD	N
College Completion	0.330	0.000	(0.470)	7,514
Years of Education	12.465	12.000	(2.730)	6,882
Wage earnings	41.176	32.000	(28.929)	7,376
Disposable Income	41.261	35.000	(30.398)	7,376
Total Gross income	40.863	33.000	(30.482)	7,376
Disposable (net of tax) income	41.264	34.000	(30.599)	7,376
Gross income excl. transfers	39.970	32.000	(30.212)	7,376
Employment Status	0.629	1.000	(0.436)	7,400
Year of birth	1,988.649	1,989.000	(1.704)	7,514
Age at arrival	4.928	5.000	(3.245)	2,782
Household Size	5.154	5.000	(2.052)	2,782

Notes: Presents descriptive statistics on child characteristics for children of refugees treated during the DDP.

Table 3.4: Summary Statistics: Parent Characteristics

	Mean	Median	SD	N
Years of Education of Mother	11.270	12.000	(3.165)	3,570
Years of Education of Father	12.213	13.000	(3.186)	3,147
Parent Wage earnings	7,953.149	21.500	(13,912.935)	7,355
Parent Disposable Income	19,391.517	18,245.336	(6,978.566)	7,355
Parent Total Gross income	25,892.120	23,991.871	(10,438.040)	7,355
Parent Disposable (net of tax) income	20,298.133	19,020.207	(7,805.311)	7,343
Parent Gross income excl. transfers	9,836.042	3,100.581	(14,943.365)	7,343
Employment Status of Mother	0.219	0.000	(0.413)	7,514
Employment Status of Father	0.261	0.000	(0.439)	7,514

Notes: Presents descriptive statistics on the characteristics of refugees who had children, with both treated during the DDP.

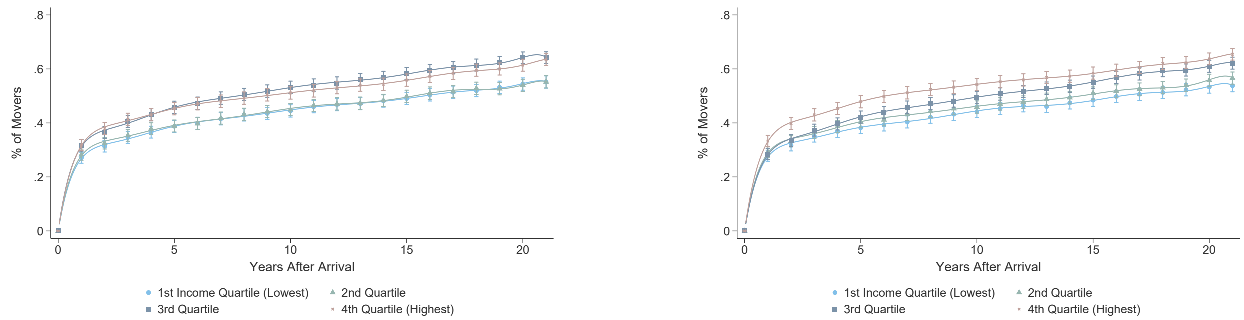
Table 3.5: Summary Statistics: Neighborhood Characteristics

	Mean	Median	SD	N
Employment Rate	77.503	77.992	(5.705)	5,240
Avg. Gross Income Excl. Transfers	37,507.243	36,099.781	(7,802.759)	5,240
Average Gross Income	47,219.834	46,251.223	(7,159.073)	5,240
Average Wage Income	30,943.943	29,961.603	(6,749.667)	5,240
Average Net Income	33,580.938	33,065.475	(4,998.698)	5,240
Avg. Years of Schooling	11.550	11.477	(0.546)	5,240
Proportion of Married HH	0.576	0.580	(0.045)	5,240
Proportion of Non-Westerners	0.017	0.011	(0.022)	5,240
Proportion of Immigrants	0.029	0.023	(0.020)	5,240
Proportion of Foreigners	0.034	0.026	(0.028)	5,240
Average Age	38.322	38.293	(2.229)	5,240
Porportion of Cohabitating HH	0.523	0.525	(0.025)	5,240
Proportion of Iraquis	0.001	0.000	(0.002)	5,240
Proportion of Iranians	0.001	0.000	(0.001)	5,240
Proportion of Vietnamese	0.001	0.000	(0.002)	5,240
Proportion of Sri Lankans	0.001	0.000	(0.002)	5,240
Proportion of Lebanese	0.001	0.000	(0.002)	5,240
Proportion of Ethiopians	0.000	0.000	(0.000)	5,240
Proportion of Afghans	0.000	0.000	(0.001)	5,240
Proportion of Somalis	0.001	0.000	(0.001)	5,240

Notes: Presents descriptive statistics on neighborhood characteristics for areas where refugees treated during the DPP were initially assigned.

3.10.2 Sorting Patterns

Figure 3.8: Neighborhood Departure Patterns by Initial Neighborhood Quality of Assignment



(a) Gross Income Exc. Transfers (b) Net of Tax Income

Figure 3.9: Sorting of Individuals by Initial Neighborhood Quality of Assignment

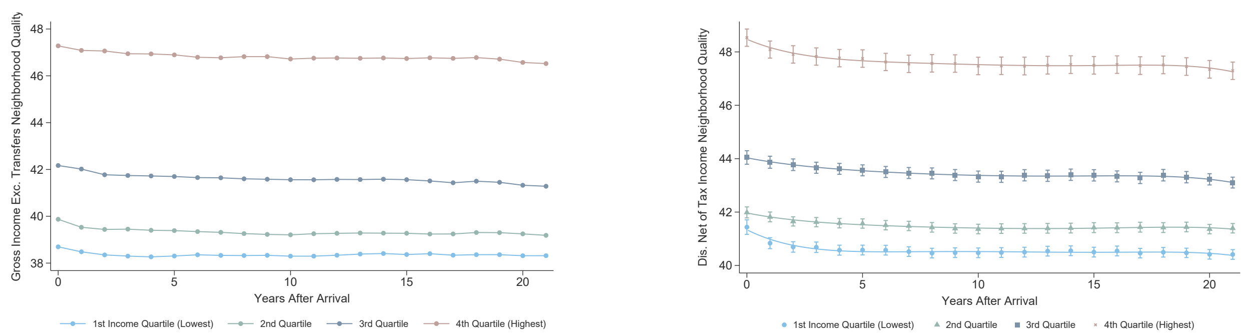
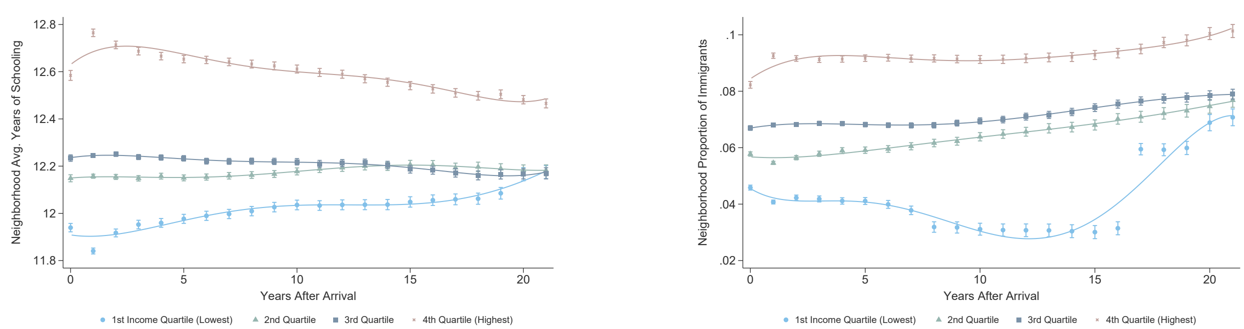


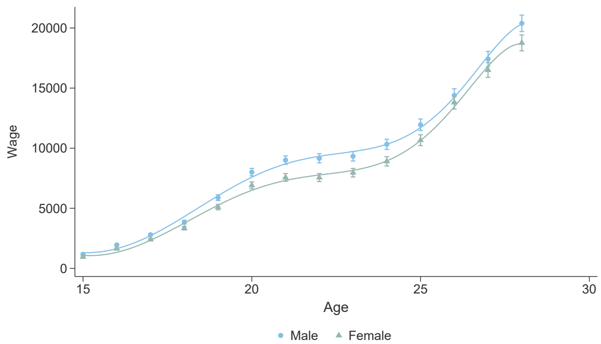
Figure 3.10: Sorting of Individuals by Initial Non-Monetary Measures of Neighborhood Quality of Assignment



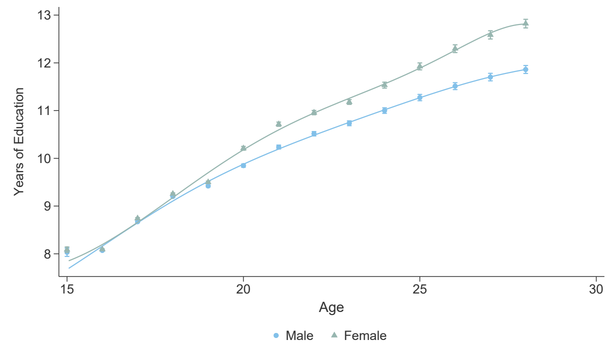
Notes: Presents neighborhood departure patterns and sorting of individuals by initial neighborhood quality of assignment. The first panel shows the percentage of individuals moving out of their assigned neighborhoods, categorized by gross income excluding transfers and disposable net income after taxes. The second panel illustrates the sorting of individuals by initial neighborhood quality of assignment based on gross and net income measures. The third panel presents sorting patterns based on initial non-monetary measures of neighborhood quality, including household years of schooling and the proportion of immigrants in the neighborhood. Using the `binsreg` command in `Stata`, a global polynomial regression model of degree 10 was fitted for plotting purposes. The confidence intervals were constructed using a piecewise polynomial of degree 1 with one smoothness constraint.

3.10.3 Life-Cycle Dynamics

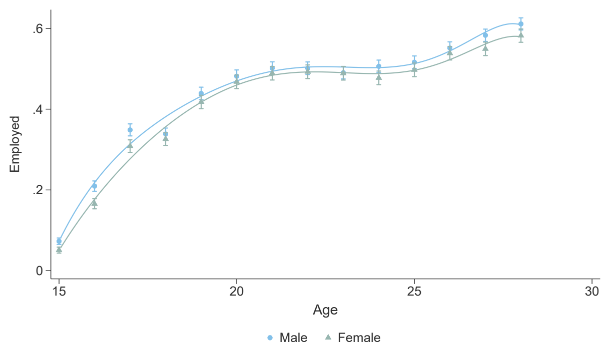
Figure 3.11: Life-Cycle Dynamics by Gender



(a) Wage Dynamics by Gender



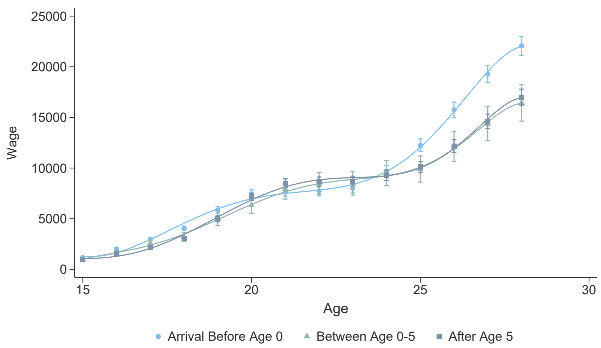
(b) Education Dynamics by Gender



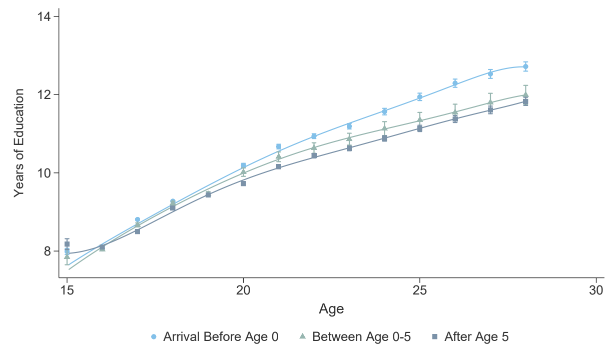
(c) Employment Dynamics by Gender

Notes: Panel (a) presents the wages of the children of refugees treated during the tenure of the DPP by gender. Panel (b) shows years of education, while Panel (c) displays employment. We observe these outcomes until the individuals reach 28 years of age. Using the `binsreg` command in `Stata`, a global polynomial regression model of degree 10 was fitted for plotting purposes. Confidence intervals were constructed using a piecewise polynomial of degree 1 with one smoothness constraint.

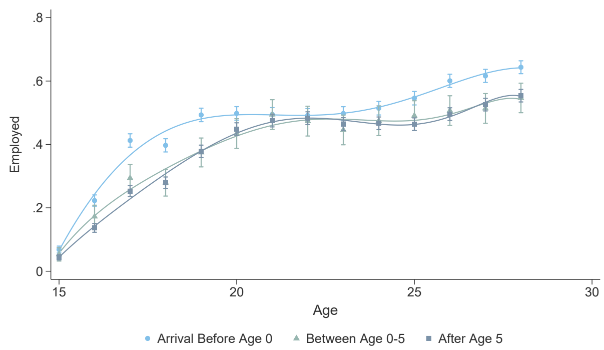
Figure 3.12: Life-Cycle Dynamics by Timing of Arrival



(a) Wage Dynamics by Timing of Arrival



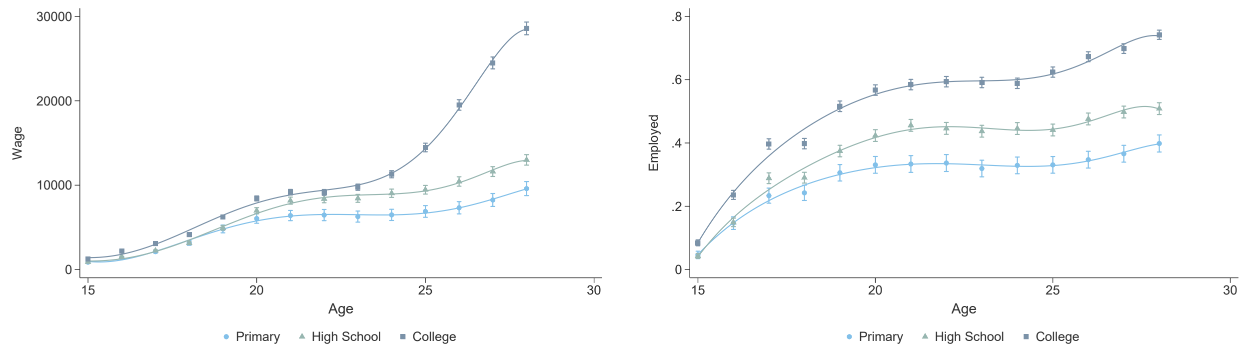
(b) Education Dynamics by Timing of Arrival



(c) Employment Dynamics by Timing of Arrival

Notes: Panel (a) presents the wages of the children of refugees treated during the tenure of the DPP, categorized by timing of arrival in Denmark—those born in Denmark (arrival before age 0) versus those who arrived between ages 1 and 5 or later. Panel (b) shows years of education, while Panel (c) displays employment. We observe these outcomes until individuals reach 28 years of age. Using the `binsreg` command in `Stata`, a global polynomial regression model of degree 10 was fitted for plotting purposes. Confidence intervals were constructed using a piecewise polynomial of degree 1 with one smoothness constraint.

Figure 3.13: Life-Cycle Dynamics by Different Levels of Schooling

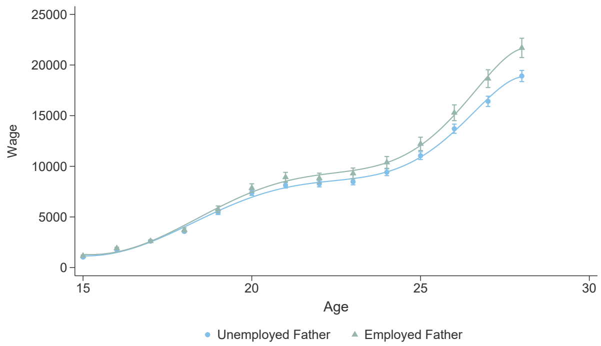


(a) Wage Dynamics by Schooling Level

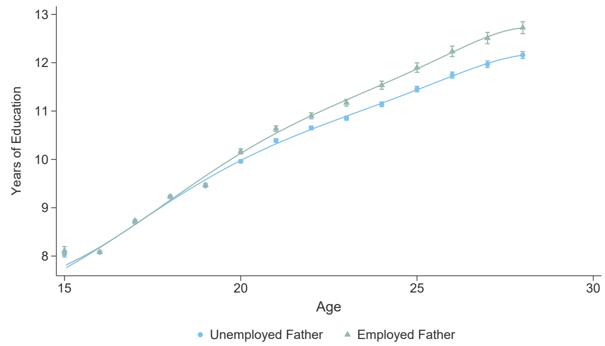
(b) Employment Dynamics by Schooling Level

Notes: Panel (a) presents the wages of the children of refugees treated during the tenure of the DPP, categorized by different levels of schooling. Panel (b) shows years in employment. We observe these outcomes until individuals reach 28 years of age. Using the `binsreg` command in `Stata`, a global polynomial regression model of degree 10 was fitted for plotting purposes. Confidence intervals were constructed using a piecewise polynomial of degree 1 with one smoothness constraint.

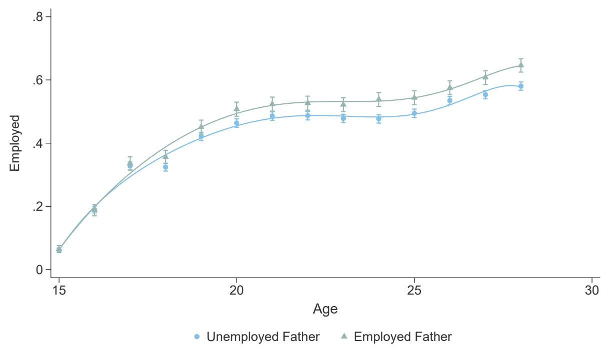
Figure 3.14: Life-Cycle Dynamics by Parents Employment Status



(a) Wage Dynamics by by Parents Employment Status



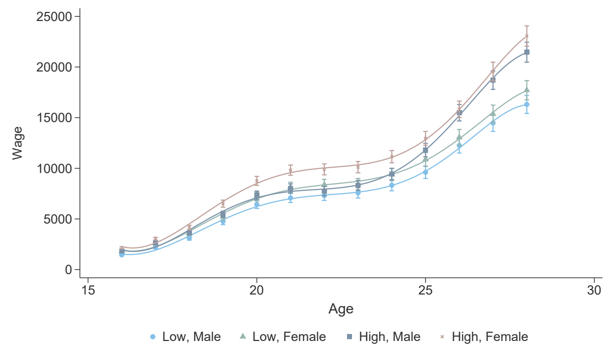
(b) Education Dynamics by by Parents Employment Status



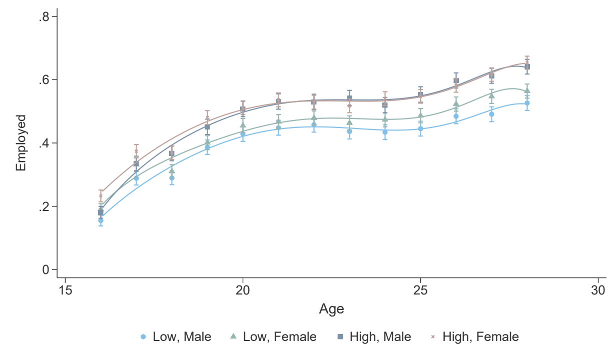
(c) Employment Dynamics by Parents Employment Status

Notes: Panel (a) presents the wages of the children of refugees treated during the tenure of the DPP, categorized by different levels of schooling. Panel (b) shows years in employment. We observe these outcomes until individuals reach 28 years of age. Using the `binsreg` command in `Stata`, a global polynomial regression model of degree 10 was fitted for plotting purposes. Confidence intervals were constructed using a piecewise polynomial of degree 1 with one smoothness constraint.

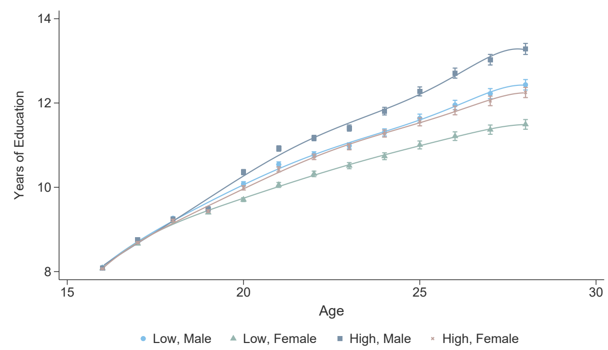
Figure 3.15: Life-Cycle Dynamics by Gender and Neighborhood Quality at Assignment



(a) Wage Trajectories by Gender and Neighborhood Quality



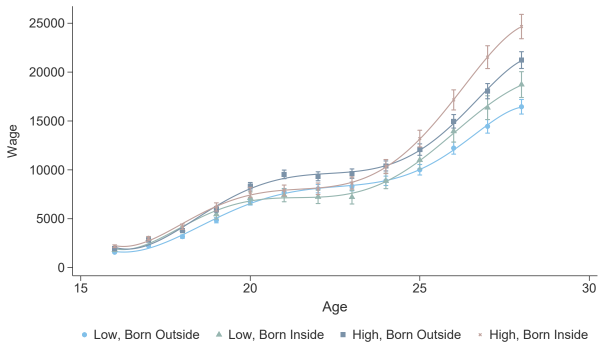
(b) Educational Trajectories by Gender and Neighborhood Quality



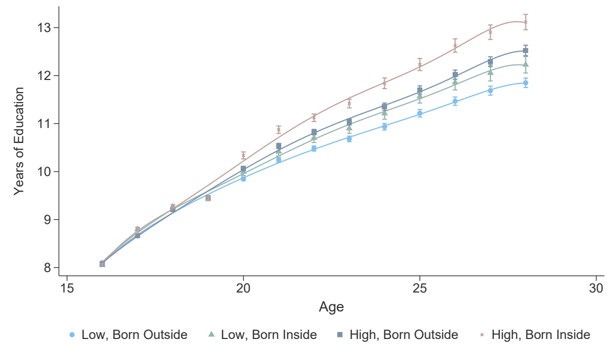
(c) Employment Trajectories by Gender and Neighborhood Quality

Notes: Panel (a) presents the wages of the children of refugees treated during the tenure of the DPP, categorized by gender and neighborhood quality at arrival (low vs. high, based on a median split of pre-tax income). Panel (b) shows years of education, while Panel (c) displays employment. We observe these outcomes until individuals reach 28 years of age. Using the `binsreg` command in `Stata`, a global polynomial regression model of degree 10 was fitted for plotting purposes. Confidence intervals were constructed using a piecewise polynomial of degree 1 with one smoothness constraint.

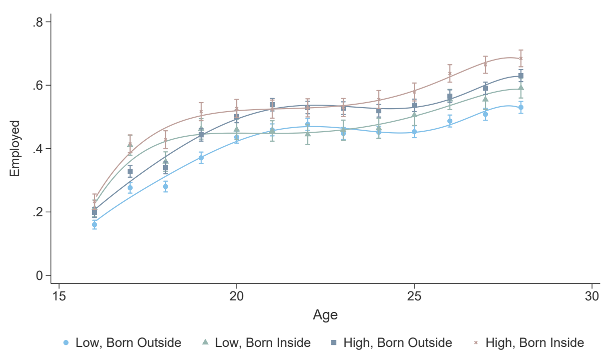
Figure 3.16: Life-Cycle Dynamics by Timing of Arrival and Neighborhood Quality at Assignment



(a) Wage Trajectories by Timing of Arrival and Neighborhood Quality



(b) Educational Attainment by Timing of Arrival and Neighborhood Quality



(c) Employment Trajectories by Timing of Arrival and Neighborhood Quality

Notes: Panel (a) shows average wages of children of refugees treated under the DPP, disaggregated by timing of arrival—those born in Denmark (arrival before age 1) versus those who arrived after birth—and by neighborhood quality at arrival (low vs. high, based on a median split of pre-tax income). Panel (b) reports years of education, and Panel (c) shows employment status. All outcomes are tracked until individuals reach age 28. There seems to be suggestive evidence of a "citizens premium". Using the `binsreg` command in `Stata`, a global polynomial regression model of degree 10 was fitted for plotting purposes. Confidence intervals were constructed using a piecewise polynomial of degree 1 with one smoothness constraint.

3.10.4 ITT Estimates: Binary Treatment

Table 3.6: Neighborhood Quality on Child Outcomes

	(1) Pre-Tax Income	(2) Post-Tax Income	(3) Employed	(4) Years of Educ.
Neighborhood Quality	9.074***	8.640***	0.093***	0.980***
	(0.105)	(0.102)	(0.002)	(0.024)
R-squared	0.037	0.032	0.027	0.063
N	6906	6906	6834	6357

Notes: This table presents results from our main specification, where the outcome variable—listed in each column header—is regressed on our measure of neighborhood quality. Neighborhood quality is categorized into low and high using a median split based on pre-tax income measures. We include dummies for household size, country of origin, parental age, and marital status, information known by the council at assignment, as well as year of assignment fixed effects. Standard errors corrected for clustering at the municipality-year level are reported in parentheses. The sample includes all individuals whose parents were randomly assigned by the dispersal policy. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

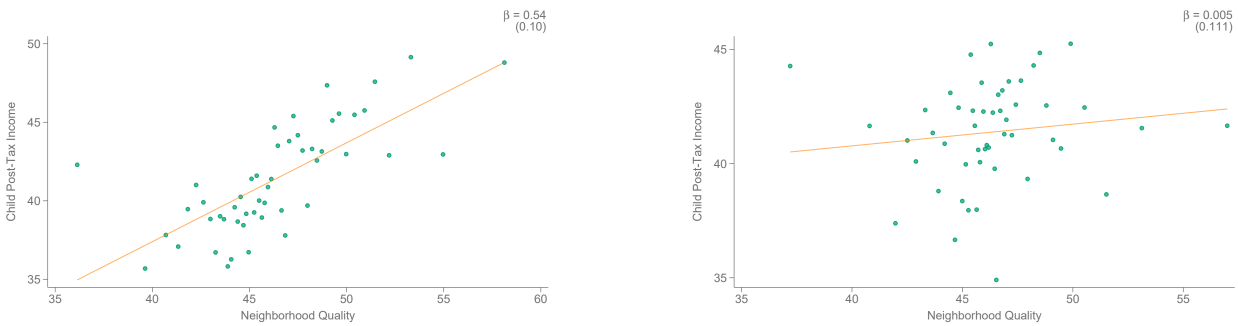
Table 3.7: Neighborhood Quality on Parent Outcomes

	(1) Pre-Tax Income	(2) Post-Tax Income	(3) Employed	(4) Years of Educ.
Neighborhood Quality	41.427***	33.923***	0.326***	1.084***
	(0.814)	(0.749)	(0.008)	(0.042)
R-squared	0.616	0.441	0.164	0.078

Notes: This table presents results from our main specification, focusing on parental outcomes under the treatment. Neighborhood quality is categorized into low and high using a median split based on pre-tax income measures. Each columns shows estimates from a regression of an outcome, described in the column header, on our measure of neighborhood quality. We include dummies for household size, country of origin, parental age, and marital status, information known by the council at assignment, as well as year of assignment fixed effects. Standard errors corrected for clustering at the municipality-year level are reported in parentheses. The sample includes all individuals whose parents were randomly assigned by the dispersal policy. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

3.10.5 *Mediation Analysis: Post-Tax Income, Employment and Educational Attainment*

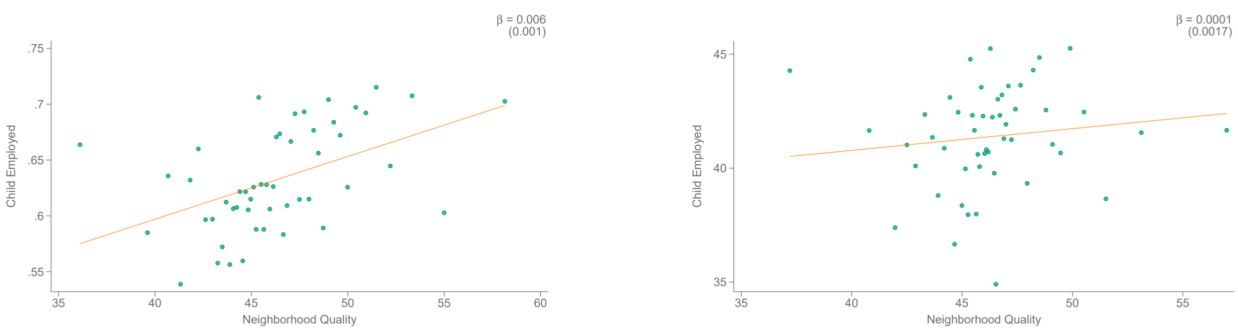
Figure 3.17: Mediation Analysis: Post-Tax Income



(a) Total Effect (β_1) (b) Direct Effect (θ_1)

Notes: Panel (a) presents the total effect of neighborhood quality on child post-tax income. Panel (b) presents the direct effect, after controlling for parental income. Using the `binsreg` command in `Stata`, a global polynomial regression model of degree 10 was fitted for plotting purposes. Confidence intervals were constructed using a piecewise polynomial of degree 1 with one smoothness constraint.

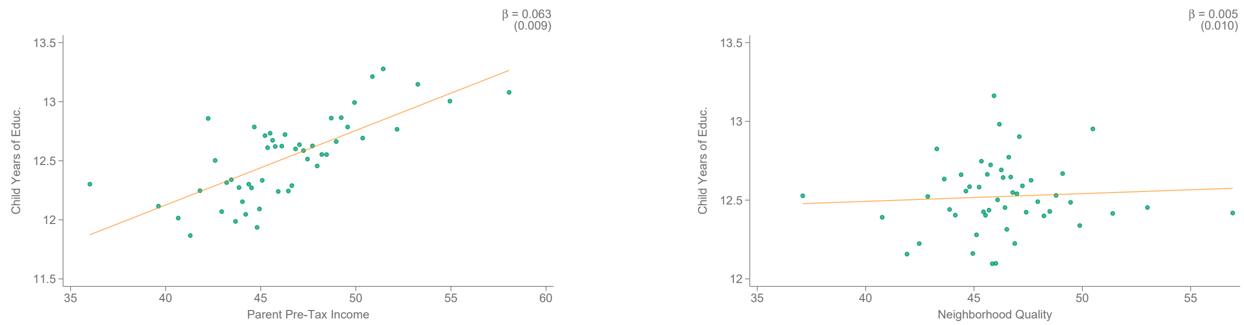
Figure 3.18: Mediation Analysis: Employment Status



(a) Total Effect (β_1) (b) Direct Effect (θ_1)

Notes: Panel (a) presents the total effect of neighborhood quality on child employment status. Panel (b) presents the direct effect, after controlling for parental income. Using the `binsreg` command in `Stata`, a global polynomial regression model of degree 10 was fitted for plotting purposes. Confidence intervals were constructed using a piecewise polynomial of degree 1 with one smoothness constraint.

Figure 3.19: Mediation Analysis: Educational Attainment

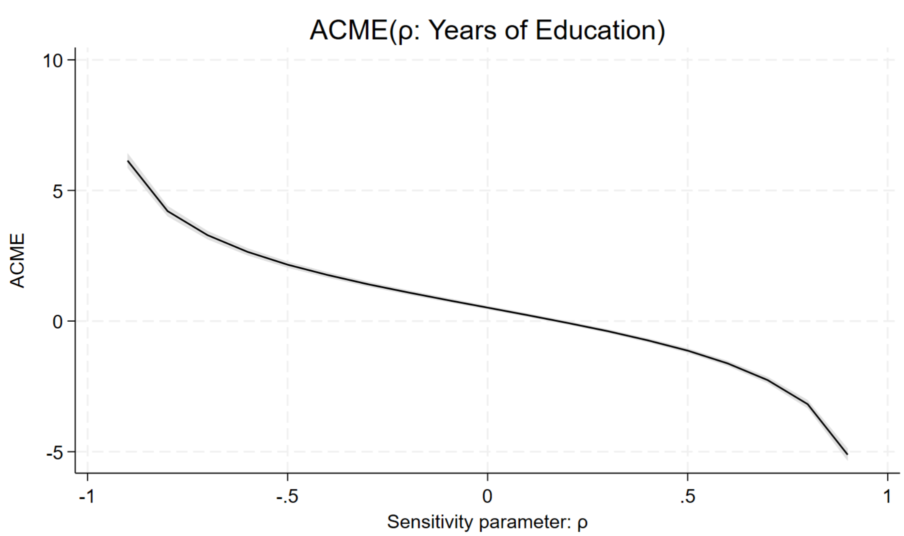


(a) Total Effect (β_1) (b) Direct Effect (θ_1)

Notes: Panel (a) presents the total effect of neighborhood quality on child educational attainment. Panel (b) presents the direct effect, after controlling for parental income. Using the `binsreg` command in `Stata`, a global polynomial regression model of degree 10 was fitted for plotting purposes. Confidence intervals were constructed using a piecewise polynomial of degree 1 with one smoothness constraint.

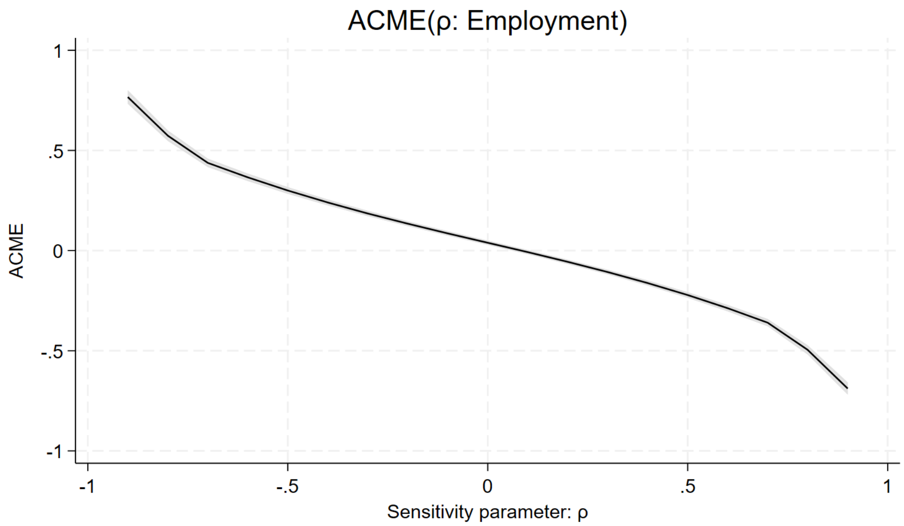
3.10.6 Sensitivity Analysis

Figure 3.20: Sensitivity Analysis for Years of Education



Notes: The treatment is dummy-coded into high- and low-income quality. The mediator is years of education. ρ represents the correlation between ε_{i1} and ε_{i2} in equations 3.4 and 3.5. Under the sequential ignorability assumption, ρ should equal zero. The simulation is conducted in Stata using the `mediate` package [Hicks and Tingley, 2011].

Figure 3.21: Sensitivity Analysis for Employment



Notes: The treatment is dummy-coded into high- and low-income quality. The mediator is the employment status of the parents. ρ represents the correlation between ε_{i1} and ε_{i2} in equations 3.4 and 3.5. Under the SI assumption, ρ should equal zero. The simulation is conducted in Stata using the `mediate` package [Hicks and Tingley, 2011].

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