The Dynastic Benefits of Neighborhood Sorting*

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Abstract

Using rich longitudinal data from Denmark, this paper uses a variety of empirical strategies to estimate the benefits of parental neighborhood sorting, as captured by better neighborhood and school peers, and teacher quality. Attending better schools significantly impacts later life outcomes of children, including increasing college graduation and wages while reducing criminal activity and teenage pregnancy. Linking these effects with the cost of sorting to neighborhoods, we estimate a private internal rate of return in the range 4%–15%.

JEL Codes: H0, H4, H7, I2, R0, R2, R3

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1 Introduction

Despite equal per capita school expenditure and equality of teacher salaries, Denmark has substantial differences between schools in terms of quality of teachers and the skill levels of the peer students. These differences are, due in part to residence-based assignment of students to public schools along sorting of families and teachers across neighborhoods (Eshaghnia *et al.* (2023)). This paper estimates the lifetime benefits for children living in neighborhoods and school districts with high-quality peers and teachers.

Sorting on the basis of preferences and income to produce local public goods has been studied since Tiebout (1956). Bénabou (1993, 1996), Durlauf (1996), and Durlauf & Seshadri (2018) analyze how neighborhood sorting affects child development through peer effects and provision of schooling quality. Epple *et al.* (2020) and Sieg (2020) summarize the local public goods literature on the provision of schooling. This literature focuses on the tax and spending decisions of agents living in neighborhoods. Impacts on child outcomes are usually viewed as outcomes of local public goods expenditures, although peer effects at schools and across neighborhoods are usually bundled with the outcomes studied.

This paper isolates peer effects in schools and neighborhoods on child outcomes apart from any effects of local public goods expenditures. In Denmark, expenditures and teacher salaries are mandated to be equal across public elementary and secondary schools except for remedial education for disadvantaged children and for cost of living adjustments. Tiebout sorting in terms of taxes to finance school quality is absent although sorting occurs for reasons of homophily and peer effects. High-quality teachers are attracted to schools with high-quality students and parents. This nonprice allocation mechanism is at work despite the inability of districts to determine salaries or their need to finance local public goods. The mechanism of unequal spending by districts, so often discussed in explaining inequality in child outcomes across districts is absent (see for instance Flyer & Rosen (1997), Jackson *et al.* (2016), Hyman (2017) and Lafortune *et al.* (2018)). Nonetheless, basic forces promoting sorting are at work.

Eshaghnia *et al.* (2023) estimate how much parents value neighborhood and peer characteristics, including schooling quality in terms of their willingness to pay for houses with identical attributes. Payment for peers and quality schooling is an investment very rarely analyzed with the kind of lifetime payoff data we use. By linking the lifetime benefits of attending better schools to the estimates of parental demand for peers and school quality in Eshaghnia *et al.* (2023), this paper investigates one possible mechanism behind notable inequities in Denmark (Eshaghnia *et al.* (2022); Landersø & Heckman, 2017), despite its strongly egalitarian welfare system.

Instead of analyzing only test scores of children when they complete primary and secondary schooling, we analyze measured life-cycle outcomes, including earnings, participation in crime, health, and a variety of other outcomes. We use this information to compute rates of return to parental investment decisions reflected in increases in the price of housing of the same attributes in neighborhoods with different peers and teachers of the order of 4%–15% per annum.

Our paper contributes to the active literature on teacher pay and teacher performance (see, e.g., Neal, 2018). Nonmonetary compensation is important. The current emphasis on enhanced pay leading to enhanced child school performance misses the crucial point that child quality is an important component of teacher pay and that student and parental quality are essential components of any compensation package.

We use a variety of approaches to reach these conclusions. One strength of this paper is the concordance of the estimates across the very different approaches used (see, e.g., Fogel, 1987). No one assumption or methodology drives our results.

1.1 The Flow of This Paper

Our paper develops in the following way. We first describe the rich administrative data covering the full Danish population that we use. Denmark has well-functioning private housing and labor markets like other Western economies.

We can identify individual residential street addresses. For each housing unit, we observe numerous attributes including the assigned school catchment area and charac-

teristics of the building of residence. School quality measures are constructed based on administrative data, by (1) averaging student grades and (2) creating an index based on teacher employment and academic records.

We study whether attending better schools is associated with better later life outcomes for the children who attend these schools. Early studies of school quality, beginning with Coleman (1968), find no association between school inputs and student achievement on standardized tests (see, e.g., Hanushek *et al.*, 1996; Betts, 1995; Betts, 1996; and Heyns, 1997, for surveys of early studies). Some studies such as Johnson & Stafford (1973) and Card & Krueger (1992) use variation across states or across age cohorts within states to identify the effects of school input measures on rates of return on schooling and repeat economically significant effects.[1] Some recent studies use microdata and analyze the impact of various school inputs such as teachers' characteristics and salaries, class size, pupil-teacher ratios, expenditures per pupil, school resources, and the composition of peers on student performance.[2] These papers report mixed results regarding the impacts of school and neighborhood quality on student performance. We find substantial effects of our measurement of school quality on a number of adult outcomes.

We estimate the impact of school quality on later life outcomes, using schooling production functions and a variety of approaches. Our first strategy relies on variation in school and neighborhood quality within narrowly defined square grids which we refer to as neighborhood clusters, holding constant a large vector of individual, household and neighborhood characteristics.[3] Our second strategy exploits within-family variation in school and neighborhood quality arising from variation in peers and teacher quality across cohorts. A third approach builds on within-school and across-cohort

[1] Heckman *et al.* (1995) show that misspecification in these analysis are partially responsible for their results and call into question the strength of the evidence for associations between school quality and earnings based on aggregate data.

[2] See, for example, Altonji & Dunn (1996); Dearden *et al.* (2002a); Rockoff (2004); Rivkin *et al.* (2005); Cullen *et al.* (2005); Hastings & Weinstein (2007); Clark (2010); Chetty *et al.* (2011); Jackson (2013); Deming *et al.* (2014); Bernal *et al.* (2016). Glewwe *et al.* (2011) review studies about the impact of school expenditures and teacher characteristics on students' years of completed schooling.

[3] See Eshaghnia *et al.* (2023) for more details on the construction of neighborhood clusters, i.e., large clusters, and small clusters, which are nested within the large clusters.

variation to pin down the effect of better schools on later life outcomes. We further conduct several robustness checks, including capitalizing on within school across classrooms variation, adding past grades as proxies for individual ability, providing bounds on the impact of unobservables and running placebo tests.

Finally, we exploit school-boundaries-variation across small neighborhoods in school quality, in the spirit of Billings *et al.* (2014). Estimates from this approach are close to our benchmark estimates. This analysis provides additional evidence that our benchmark shows the impact of peers at school and most likely does not pick up the impact of peers at the neighborhood level, nor the unobserved family attributes. Table 1 summarizes the different estimation strategies we use in this paper.

1.2 Contribution to the Literature

Our paper is distinct from previous studies in the literature in several important ways. First, we have better data on the life cycles of children. We link attending better schools, in better neighborhoods, to various long-run outcomes of children like earnings, a unique feature lacking from most previous studies in the literature that rely on test scores at different schooling levels and impute future earnings.[4] These outcomes include income, college completion, criminal behavior, and teenage births, among others. Second, we control for neighborhood quality and numerous parental characteristics to capture the impact of various factors correlated with school quality. Third, unlike other papers in the literature that focus on a particular school district or a local policy change, we use administrative data on the whole population of an entire country in which most children attend public schools freely available to all.

[4]For limitations of this approach, see García *et al.* (2018).

Table 1: Summary of the Methods Employed to Identify the Impacts of School on Lifetime Outcomes

	Method Description	Identifying Assumptions
	Identifying the Impact of School Quality on Lifetime Outcomes	
(1)	**Selection on observables:** Exploiting *within-neighborhood* (*clusters*) variation in school quality holding constant neighborhood characteristics and housing attributes.	School quality is orthogonal to unobserved determinants of outcomes conditional on covariates, i.e., large-cluster-by-cohort fixed effects and vector of housing, neighborhood (small cluster level) and individual-level controls.
(2)	**Within-school variation:** Exploiting *within-school and across-cohort* variation in school quality.	Changes in school quality, occurring *within schools across cohorts*, are orthogonal to unobserved determinants of outcomes conditional on our vector of covariates, i.e., housing, neighborhood (small cluster level) and individual-level controls.
(3)	**Within-family variation:** Exploiting idiosyncratic changes in school quality *across siblings* (across time).	Changes in school quality, *within families* are orthogonal to unobserved determinants of outcomes conditional on our vector of covariates, i.e., school-by-cohort fixed effects and housing, neighborhood (small cluster level) and individual-level controls.
(4)	**Within-school variation across classrooms:** Exploiting variation in school quality *across classrooms within school-by-cohort*.	Changes in school quality, occurring *within schools across classrooms in any given year*, are assumed to be orthogonal to unobserved determinants of outcomes conditional on covariates, i.e., housing, neighborhood (small cluster level) and individual-level controls.
(5)	**Within-school variation controlling for past grades:** Exploiting the within-school and across-cohort variation in school quality and lifetime outcomes while adding a control for grades in the previous (8th) grade.	Changes in school quality, occurring within schools across cohorts, are orthogonal to unobserved determinants of outcomes conditional on our vector of covariates, i.e., school-by-cohort fixed effects and housing, neighborhood (small cluster level) and individual-level controls, *including past grades*.
(6)	**Changes in school boundaries:** Exploit changes in school boundaries-by-small-neighborhood variation in school quality.	(i) Changes to school district boundaries are exogenous to enrollment decisions, (ii) the impact of school quality can be identified from variation in school boundaries-by-small-neighborhood in school quality, as in Billings *et al.* (2014).
(7)	**Bounding the impact of unobservables:** Applying alt, n.d.; Diegert *et al.*, 2022; Oster, 2019 to bound the impact of omitted variable bias on our estimates.	Selection on observables provides information about selection on unobservables.
(8)	**Placebo tests:** Using birth characteristics to test that selection does not plague our strategies for identifying the impacts of school quality on lifetime outcomes.	(i) Our pre-determined characteristic, i.e., birth weight, is correlated with later life achievement, (ii) School quality experienced later in childhood cannot *causally* affect children's birthweight realized at age 0.

Our findings are consistent with the results of Chetty *et al.* (2014), who use a value-added approach and find that students assigned to high value-added teachers are more likely to attend college and earn higher salaries as adults. Our paper, however, considers a much broader array of household inputs and examines both teachers' characteristics and the composition of peers at schools and neighborhoods of residence. By linking the school effects on later life outcomes with the parental valuation of neighborhood attributes in Eshaghnia *et al.* (2023), this paper speaks to the important role of sorting across neighborhoods in Denmark.

Our findings likely generalize to other societies with functioning housing markets. We find evidence that purposeful neighborhood sorting contributes to important differences in outcomes later in life. In fact, the combination of sorting together with the residence-based assignment rules of households to schools is associated with later life outcomes, increasing income by around 4%, for a one–standard deviation increase in school quality attended. We also show that attending schools one standard deviation above the mean, increases college completion by 0.7 percentage points, conditional on neighborhood quality and parental characteristics. Similarly, we show that school quality is positively associated with reduced criminality and teenage pregnancy. Household valuation of school quality, as capitalized in house prices (Eshaghnia *et al.* (2023)), is likely driven by parents realizing its important effect on later life outcomes.

By combining our estimates for the impact of school quality on later life outcomes with the estimates for the parental demand for school quality presented in Eshaghnia *et al.* (2023), we estimate the private internal rate of return for investing in children by living in neighborhoods with better peers and higher school quality to range from 4% to 15%.

2 The Public Schooling System in Denmark

The Danish schooling system is based on the principle of schooling for all, which is provided at no charge in public schools through the university level. Specifically, per-pupil

expenditure is equalized in Denmark through a system of redistribution across municipalities. Redistribution is needed since municipalities' primary sources of revenue (about 70%) is collected from local taxes. Despite the tax rate varying only minimally across municipalities (OECD, 2016), tax bases do vary, which prompts this redistributive system.

Figure 1 and Figure A.1 of Appendix A display the distributions of total expenditures across municipalities. The Danish distributions are much more concentrated than those for the US. Dalsgaard & Andersen (2016) show that more than 50% of the heterogeneity in schooling expenditure across municipalities is due to compensatory finance that accounts for factors, such as the fraction of non-westerners across municipalities.

Despite near equalization in school expenditure and teacher salary distributions, teachers still sort based on the quality of the students they are teaching. Differences in school quality are not driven by differences in schooling expenditure per se, but by the sorting of teachers and students.

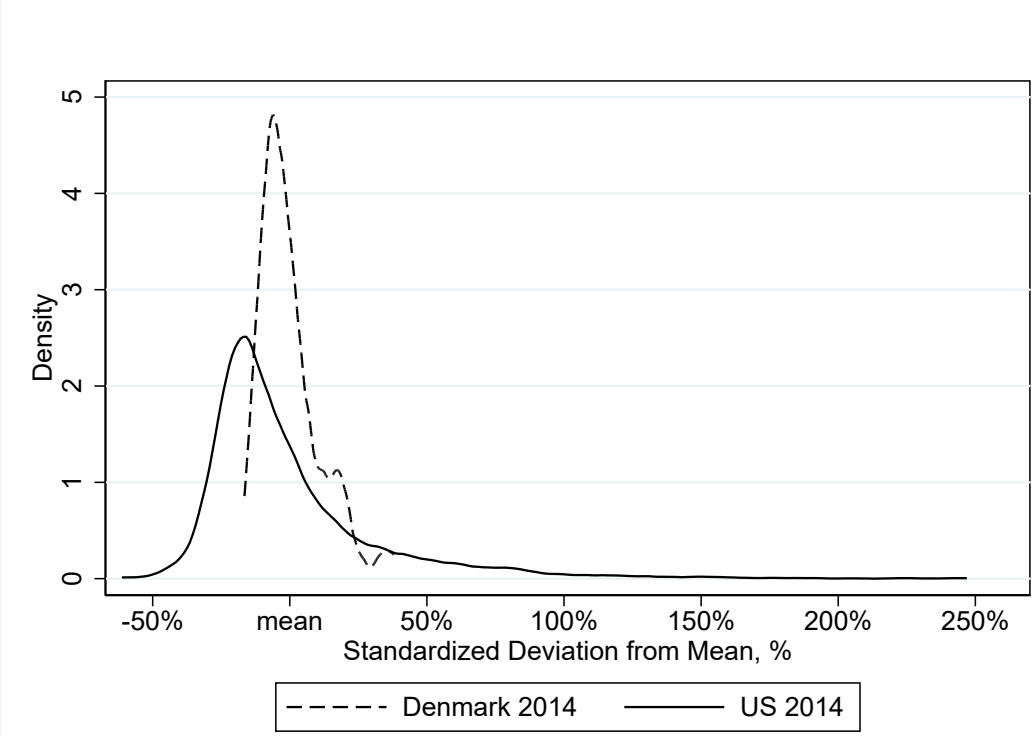
Access to public schools in Denmark is residence-based. Each housing unit is part of a school catchment area that is assigned to a single school. Parents can defy school catchment area rules in certain cases, although the possibility of doing so is contingent on available capacity in alternative schools.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

Figure A.2 of Appendix A.2 shows the variation in our measure of school quality (average test scores), in our various specifications reported in Table 1. Despite the equalized school expenditures and teacher salaries, there is a great deal of variation in the performance of peers across schools, which is due to the spatial sorting of families (and teachers) along the residence-based assignment of students to public schools.

3 Data

This paper relies on administrative data from Statistics Denmark, which gives data for the whole population. We focus on five cohorts of ninth graders, who attended ninth

Figure 1: Per-Pupil School Expenditure across Municipalities in Denmark and the US Relative to the Country Average



Notes: This figure replicates Figure A.2 (Panel a) of Gensowski *et al.* (2021), which shows the average per-pupil school expenditures in public schools in 2014 relative to the country average, for the US and Denmark.

grade between 2002 and 2006. Below we describe in more details the different key variables used in the analysis.

School Quality Measures We average student grades on exams taken in their last year of compulsory education. Our measure of school quality based on average test scores excludes one's own test score. This leave-one-out measure of school quality reduces the possibility that our results capture the student's own performance in school. These are national exams on a wide range of subjects, most taken at age 16. Average grades at the school level, broken down by subjects and are available publicly to parents.[5]

We also use a measure of teacher quality based on Gensowski *et al.* (2021). Using administrative records, all employees in teaching positions in schools between 2009 and

[5]Test scores (broken down by subject) at the school level are available to all and advertised on home buying websites such as www.dingeo.dk.

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.

Table 2: Summary Statistics

	Mean	St. Dev.	Coef. of Var.
School Level			
Average Household Gross Income (Excl. 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 Foreigners	.068	.066	.97
Fraction of Non-Western Foreigners	.030	.049	.1.63

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.

To compute neighborhood characteristics, we use Denmark's rich administrative data to capture a wide range of characteristics at small-cluster neighborhood levels. More specifically, we use variables pertaining to the household, including income,[6] education level,[7] crime[8], family structure[9] and ethnicity [10]. [11]

Measures of Later Life Outcomes We turn to describe the different outcomes we analyze.

Economic Outcomes We look at a number of child adult economic outcomes. We study total gross income excluding taxes and transfers to focus on earnings potential. We contrast it with annual disposable income–income after taxes, interests,

[6] We use gross household income excluding transfers.

[7] We use years of completed education. When computing small-cluster 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.

[8] We use an indicator for whether an individual as committed a crime or not in a given year.

[9] We include a measure of marital status and intact family 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.

[10] We distinguish between foreigners and non-western foreigners. The former denotes individuals who have at least one parent not born in Denmark. Non-western foreigners comprise of individuals who have at least one parent from a non-western country.

[11] We report the correlation between the different neighborhood attributes in Appendix A.3.

and rental value of owner-occupied housing–to examine the impact of redistribution in Denmark.[12] We also study wage growth of individuals, between the ages of 24-25 and 28-29, to explore the income path of individuals. We study net of tax income, wage income and gross income including transfers.

We also look at whether individuals who attend better schools are more likely to work. It is defined based on whether individuals earn more than $35,000 a year, the minimum wage in Denmark.[13] We explore homeownership as an outcome. An individual is defined as homeowner if reporting a housing value greater than $20,000.[14] Finally, we also consider both government transfers to individuals, as well as taxes paid, as outcomes.[15]

College or University Completion College completion is defined as having undertaken 15+ years of education and graduating with at least a bachelor's degree. University completion includes having undertaken 17+ years of education and graduating with at least a master's degree. In Denmark, individuals tend to stay in school until late in their 20s and beyond. Therefore, by focusing on cohorts in ninth grade between 2002 and 2006, we are able, for the latest cohort to observe college or university completion by age 30, for all our cohorts, given data availability up to 2020.

Criminal Record Our measures of participation in crime are dichotomous, considering whether an individual has ever committed various types of crime by age 25-29 We separately identify severe crimes for which individuals are sentenced to prison by age 25-29. We conduct a similar sample restriction when looking at

[12]The disposable income is computed as total personal income including public transfers and rental value of own home (for owner-occupied individuals) minus taxes, interest expenses, and child support.

[13]The law does not mandate a minimum wage, as it is negotiated through bargaining between unions and employers association. We set this amount based on the lowest wage agreed upon in an industry of 120 DKK per hour and a workweek of 37.5 hours. We run a robustness check with a cutoff that is half of this amount (to account for part-time workers). The effect becomes more muted and is insignificant.

[14]If house is co-owned by partner, this implies a property value of $40,000.

[15]With regards to data availability, the latest year for data on economic outcomes is in 2019. Since taking a single year would provide a noisy measure of earnings, we average incomes over two years. For our cohorts who graduate 9th grade between 2002 and 2006, we are thus able to measure these outcomes at ages 28-29. Regarding employment and homeownership, these outcomes are also measured between ages 28-29.

criminal record, by focusing on earlier cohorts who complete ninth grade between 2002 and 2006.

Age at First Birth We also study the effect of school quality on the timing of childbearing. We use two measures: teenage birth and age at first birth. From teenage birth, we focus on the whole sample, while for age at first birth we consider the same restriction as above, which is to focus on cohorts between 2002 and 2006. Teenage birth is defined here as having a child by age 19. For this outcome we look at cohorts of children who graduate from ninth grade between 2002 and 2012.

4 Conceptual Framework for Estimating Impacts of School Quality on Later Life Outcomes

In this section, we assess the relationship between school quality and child later life economic, educational and social outcomes, including earnings, college completion, criminal records and teenage pregnancy.

We use rich longitudinal administrative data from Denmark, which allows us to observe outcomes up to 18 years after completing ninth grade (age 16, on average). Data availability on school quality commences in 2002. We restrict our data to cohorts that complete ninth grade between 2002 and 2006, for the later life earnings analysis to get life cycle outcomes.

Estimating the effect of school quality on later life outcomes requires us to account for several important endogeneity issues, as discussed in the literature (see for instance Dearden *et al.*, 2002b; Heckman *et al.*, 1996; and for a review Meghir & Rivkin, 2011).

First, family location and schooling decisions are not made randomly and are part of a process of life cycle planning (see Eshaghnia *et al.* (2023)). First, we build on the neighborhood-fixed-effect methodology of Eshaghnia *et al.* (2023) for the valuation of school quality that controls for observed and unobserved neighborhood-level characteristics that may be correlated with school quality and could otherwise bias

the estimates. This could be particularly problematic if parents with stronger preferences to invest in their offspring human capital choose better neighborhoods. If these neighborhood characteristics are relevant to the development of a child's human capital, estimates of the effect of school quality on later life outcomes would be upward biased. On the other hand, neighborhood characteristics may also be investment goods properly accounted for in analyzing child investment.[16]

Moreover, parents who locate in catchment areas of better schools may also invest more in their children, a relationship that would lead to an upward bias of causal estimates of schooling quality alone. We need to control for parental investment. In line with this story, our previous analysis on the WTP clearly shows that there is sorting across school boundaries. To deal with this issue, we first control for a wide range of parental characteristics, such as income, education, family structure and crime. Despite our rich data and ability to control for proxies of parental investment, these may not be enough to capture all the differences across parents.

To reduce any remaining unobserved differences in parental investment across families, we estimate a family fixed effect model of the education production function. More specifically, we look at within family variation in school quality with multiple siblings. A substantial majority of these siblings attend the same school and live in the same neighborhood, but experience different school quality due to differences across cohorts related to differences in peers and teacher quality. We establish similarity for some of the estimates of school quality across the family and neighborhood fixed effects models.

As a further approach, we build on within-school variation across cohorts. This methodology, initially implemented by Hoxby (2000) relies on the exogeneity of the cohort composition within schools over time. This allows us to control for selection of households into specific school catchment areas. We establish similarity for some of the estimates of school quality across the family and school fixed effects models.

In other settings, school finances and laws differentially affect the allocation of re-

[16]The opposite bias could, however ensue, if parents who locate in better neighborhoods substitute away and invest less at home (as has been recently documented in Denmark by Gensowski *et al.* (2021).

sources to students or schools leading to biased estimates of the impact of school quality on later life outcomes. Teacher salaries and payments to schools are uniform across Danish neighborhoods and schools, except for adjustments to cost of living and disability status of children. Moreover, our ability to estimate within-neighborhood and, in some specifications, within-school variation, allows us to control for potential differences in school finances.

In Appendix B, we formalize our dynamic model of the education production function (drawing on Todd & Wolpin (2003)), which accounts for the different mechanisms explored above. We show how our reduced-form estimates map to structural parameters and clarify the necessary identifying assumptions.

4.1 Selection on Observables

Our baseline estimates of the education production function, builds on Eshaghnia *et al.* (2023). In particular, to control for family sorting across neighborhoods, we use the cluster fixed effects approach in Eshaghnia *et al.* (2023) together with small-cluster neighborhood controls capturing average income, years of education, criminality, share of intact, married and foreign families as well as share of private schools. We also note that given the equalization of expenditures across schools in Denmark, we do not need to add any school-level measures of differential school spending.

Building on the structural model presented in Appendix B, we consider the below regression analog of the true technology:

$$Y_{ilmkc} = \tau + \alpha S_{ilmkc} + \eta_m + \beta F_{ilmkc} + \mu_l^f + \mu_l^c + \gamma N_{ilmkc} + \rho_k + \nu_c + \varepsilon_{ilmkc}$$

where Y_{ilmkc} denotes later life achievements for individual i in household l attending school m in neighborhood k for a given cohort c. S_{ilmkc} denotes ninth grade school-supplied input, while F_{ilmkc} denotes the vector of parent-supplied inputs. Further, let a child's endowed mental capacity be denoted as μ_l. The superscript c and f, respectively correspond to a sibling-specific and family-specific endowment. Measurement

error in test scores is denoted by ε_{ilmc}.We also add several fixed effects at the school, neighborhood and cohort level, denoted respectively as η_m, ρ_k, ν_c.

Building on the above model, we estimate the following model of education production function:

$$Y_{ilmkc} = \tau + \alpha S_{ilmkc} + \beta F_{ilmkc} + \gamma N_{ilmkc} + \rho_{kc} + \underbrace{\eta_m + \mu_l^f + \mu_l^c + \varepsilon_{ilmkc}}_{\iota_{ilmkc}, \text{ composite error term}}. \tag{1}$$

This follows directly from Equation 9 after we replace the neighborhood ρ_k and cohort fixed effects ν_c by ρ_{kc}, i.e., neighborhood-by-cohort fixed effects.

Unbiased estimation of (1) relies on the following assumption:

Assumption 1 (Selection on Observables)**.** *School quality is orthogonal to unobserved determinants of outcomes conditional on our vector of covariates:*

$$\text{cov}(S_{ilmkc}, \iota_{ilmkc} \mid F_{ilmkc}, N_{ilmkc}, \rho_{kc}) = 0 \tag{2}$$

Our rich data allow us to control for a number of key inputs in the production function, including individual, family and neighborhood-level inputs. Moreover, the results from this specification are in line with those from our within-family specification, which we present next, which lends credence to this assumption.

4.2 Within-Family Variation

Our second approach, differences equation (9) within families, to get:

$$Y_{ilmkc} - Y_{i'lmkc} = [S_{ilmkc} - S_{i'lmkc}]\,\beta + \underbrace{\mu_{il}^c - \mu_{i'l}^c + \varepsilon_{ilmkc} - \varepsilon_{i'lmkc}}_{\omega_{ilmkc}, \text{ composite error term}}$$

In the context of the education production function, family fixed effects control for family-specific endowments and investments. Consistent estimation of the effect of school quality now relies on the following assumption:

Assumption 2 (Within-Family Variation)**.** *School quality is orthogonal to unobserved determinants of outcomes conditional on our vector of covariates, within families:*

$$\text{cov}(S_{ilmkc}, \omega_{ilmkc} \mid F_{ilmkc}, N_{ilmkc}, \nu_c, \rho_k, \mu_l^f) = 0 \tag{3}$$

Our key identifying assumption in the family fixed effect model can be restated as follows. Within families, siblings who are the same in terms of observables but who attended schools with varying levels of quality, do not differ on average, in terms of unobservables (for instance endowed mental capacity). There are three behavioral restrictions that arise from this assumption. First, parents do not change their input decisions based on innovations on sibling outcomes.[17] Second, input choices may depend on family-specific endowment but not on child-specific endowment. These assumptions may be justified insofar as parents may have limited information on the exact level of the child-specific endowment as well as on the sibling innovations. Third, any omitted input, varying across sibling, is orthogonal to included inputs.

It is important to further clarify the source of exogenous variation we use here to identify the effect of school quality. Most siblings in Denmark attend the same school (91% of siblings in our sample). Still, different cohorts within schools may be exposed to varying quality, due to differing teachers and peers. We note, however, that our approach does not allow us to distinguish whether the quality of the school is being driven by the change in the teacher quality and/or peers.

4.3 Within-School Variation

To alleviate concerns on the behavioral restrictions implied by Assumption (2), we propose a third specification, building on within-schools, within-neighborhood and across-cohort variation. We do this by differencing Equation (9) within schools, neighborhood and cohort.

[17] This assumption may not hold given the evidence presented in previous papers (Behrman *et al.* (1995), Yi *et al.* (2015)). Our other estimation strategies, such as the within-school variation across cohorts, don't rely on this assumption.

Unbiased and consistent estimation of the effect of school quality now relies on the following assumption:

Assumption 3 (Within-School Variation)**.** *Changes in school quality, occurring within schools across cohorts, are orthogonal to unobserved determinants of outcomes conditional on our vector of covariates:*

$$\text{cov}(S_{ilmkc}, \varepsilon_{ilmkc} \mid F_{ilmkc}, N_{ilmkc}, \eta_m, \nu_c, \rho_k) = 0 \tag{4}$$

Under this specification, variation in school quality comes from changes in teachers and peers in adjacent cohorts within the same school. Three implications are to be noted.

First, Assumption (3) implies that families experiencing idiosyncratic shocks to their children's ninth grade school quality, do not endogenously sort to a different neighborhood to attend a different school. This assumption is plausible given the cost for families to move to a new neighborhood to attend a different school. Moreover, our data on geographical locations of families over time allow us to verify that only very few do move in Denmark when their children reach ninth grade at school.

Second, underlying changes in students quality may invalidate Assumption (3). To that end, we build on the rich Danish administrative data to control for a host of family specific covariates. We show that our estimates are insensitive to adding these large set of family and children specific characteristics, which include income, education, origins and marital status of parents, order of birth, age of mother at birth, household size and gender of the child.

Finally, school quality changes should be uncorrelated with changes to the neighborhood, which may itself contribute to students' longer term outcomes. To alleviate such concern, we concurrently add small cluster fixed effects to control for neighborhood characteristics, as well as a vector of small cluster-level covariates, measuring neighborhood-level income, years of education, criminality, share of intact, married and foreign families as well as share of private schools.

5 Empirical Results

We present results on the impact of school quality on later life outcomes in Tables 3 and 4. The first two columns (1) and (2) present results of the within neighborhood specification, without and with controls. The second set of two columns (3) and (4) present results of the within household specification, without and with control. Finally columns (5) and (6), respectively present results on within school specifications, without and with controls.

We first discuss results of the effect of school quality on income, as presented in the first row of Table 3. The effect on later life income ranges from 3.6% to 2.4% for the household and school fixed effects specifications.[18] Since Danes tend to graduate from college in their late 20s, it is likely that this effect is a lower bound.

To explore the role of redistribution in Denmark, we consider disposable income as a further outcome variable in row (2) of Table 6.[19] In contrast to our previous gross measure of income, we see that there is here a much more muted effect of attending better schools, that is precisely estimated, of 0.8% for the within-school specification. This speaks to the previous literature which stresses the importance of the tax system in Denmark for reducing inequality (see for instance Landersø & Heckman, 2017). Thus, although gross income is increased by better schools, this does not translate into much higher disposable income during the early stages in the working life cycle.

In rows (4) and (5), we test directly the role of school quality on taxes and transfers. The effect on taxes is strong and positive, while the effect on transfers is negative, although noisily estimated for the within-school specification.

In rows (6) and (7), we evaluate the role of school quality, on the extensive margin, looking at the impact on the decision to take up work and to become homeowner. Results suggest that better schools play a small but positive role here, by increasing the probability of working by between 0.66 to .40 percentage points (albeit noisily estimated for the family fixed effects specification). Results for homeownership are very

[18] Using gross income including transfers or wage income provides similar results.
[19] Using net of tax income provides similar estimates.

similar. The school fixed effect model points to a small effect of 0.40 percentage point increase. These small effects may be driven by the fact that we observe these outcomes early in the life cycle of individuals.

Table 3: Regression Results of Model 9: Impact of School Quality on Economic Outcomes

	Controls	Nbhd. FE	HH FE	HH & Cont.	School FE	Sch. & Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
(1) Income (at Ages 28-29)	0.136***	0.158***	0.032	0.036*	0.031***	0.024**
	(0.008)	(0.009)	(0.020)	(0.021)	(0.011)	(0.011)
(2) Disposable Income (at Ages 28-29)	0.018***	0.020***	-0.006	0.0001	0.008***	0.008***
	(0.002)	(0.002)	(0.006)	(0.006)	(0.003)	(0.003)
(3) Income Growth (Ages 24-25 to 28-29)	1207***	1092***	423***	428***	284***	215***
	(46)	(51)	(136)	(146)	(75)	(77)
(4) Taxes (at Ages 28-29)	0.036***	0.036***	0.014	0.023*	0.021***	0.020***
	(0.004)	(0.004)	(0.012)	(0.012)	(0.006)	(0.006)
(5) Transfers (at Ages 28-29)	-0.108***	-0.127***	-0.035*	-0.044**	-0.021*	-0.013
	(0.007)	(0.007)	(0.020)	(0.020)	(0.011)	(0.011)
(6) Employment (at Ages 28-29)	0.018***	0.022***	0.005	0.007	0.005***	0.004**
	(0.001)	(0.002)	(0.003)	(0.004)	(0.002)	(0.002)
(7) Homeownership (at Ages 28-29)	0.015***	0.015***	0.006*	0.008**	0.004**	0.004**
	(0.001)	(0.001)	(0.003)	(0.004)	(0.002)	(0.002)
Individual Char.	Yes	Yes	No	Yes	No	Yes
Nbhd Char.	Yes	Yes	No	Yes	No	Yes
Housing Char.	Yes	Yes	No	Yes	No	Yes
Cohort FE	Yes	No	Yes	Yes	Yes	Yes
Family FE	No	No	Yes	Yes	No	No
Large Cluster-Cohort FE	No	Yes	No	No	No	No
School FE	No	No	No	No	Yes	Yes
Small-Cluster FE	No	No	No	Yes	No	Yes
Observations	∼230,000	∼230,000	∼70,000	∼60,000	∼250,000	∼230,000

Notes: Column (1) shows a specifications with a host of individual, neighborhood and housing characteristics. Individual controls include income, education, origins and marital status of parents, order of birth, age of mother at birth, household size and gender of the child. Neighborhood and housing characteristics are as in the previous hedonic models. We do not include origins of the parents in the within-family strategy. Column (2) shows a neighborhood FE fixed effects specification, with controls. Column (3) and (4) depict our family fixed effect models, respectively, without and with controls. Finally columns (5) and (6) respectively report the school fixed effects specification, without and with controls. Standard errors corrected for clustering at the school-cohort level are reported in parentheses. Sample includes all parents in Denmark whose children attend ninth grade between 2002 and 2006. For teenage birth, the sample includes cohort of children from 2002 to 2015. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

We now turn to present our results on college completion in the first row of Table 4. As discussed above, we find that the naive OLS estimates are larger than estimates obtained from other approaches. This would imply that a one–standard deviation increase in school quality is associated with a 9.9 percentage point increase in college completion. In contrast, the neighborhood fixed effects regression which also includes

small cluster neighborhood controls provide an estimate of 4.9 percentage point (cf. column (2)). This difference is driven by parents who willingly sort across neighborhoods and thus leads to correlate neighborhood attributes with school quality. When controlling for family fixed effects, in column (3), we find that the coefficient further drops to 2.8 percentage points. After adding controls, the coefficient reduces further to 2.5. The estimate from our school fixed effects specification with controls is slightly more muted and stands at 0.78 percentage points, which relative to the sample mean, represents a 1.8% increase.

This evidence shows that parents are willing to pay a significant amount for better schools, and they also recoup this investment through the improved educational outcomes of their offspring.

Row (3) presents results on criminal record. A one–standard deviation increase in school quality leads to a 1.8 percentage point decrease in criminality. In the family and school fixed effect model, the estimate respectively stand at -1.57 and -0.40 percentage point —the latter represents close to a 2% drop in crime at the mean. There also seems to be an effect on the reduction of severe crimes, as showcased in row (4).

In row (5), we provide evidence regarding teenage birth. The effect from both the household and school fixed effects specification imply that attending schools with one–standard deviation higher average grades leads to decrease teenage birth by between 4%–9% at the mean.

Finally, row (6) looks at whether higher school quality is associated with a later arrival of children. Both model estimates presented in columns (2) and (4) provide similar estimates, reflecting a 0.16–0.13 year increase in the age at first birth, while the school fixed effects specification in column (6) reflects a less pronounced effect, with an estimate standing at 0.023, that is insignificant statistically.

5.1 Heterogeneity

We now assess how our results vary by family characteristics. To this end, we focus on the within-school specification and include all family, neighborhood and individual-

Table 4: Regression Results of Model 9: Impact of School Quality on Later Life Outcomes (Education, Participation in Crime, and Fertility)

	Controls	Nbhd. FE	HH FE	HH & Cont.	School FE	Sch. & Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
(1) College Completion (at Ages 29-30)	0.050***	0.049***	0.028***	0.025***	0.012***	0.008***
	(0.001)	(0.001)	(0.003)	(0.003)	(0.002)	(0.002)
(2) University Completion (at Ages 29-30)	0.027***	0.022***	0.011***	0.011***	0.004***	0.002
	(0.0008)	(0.001)	(0.002)	(0.003)	(0.001)	(0.001)
(3) Crime (at Ages 25-29)	-0.027***	-0.029***	-0.021***	-0.016***	-0.006***	-0.004**
	(0.001)	(0.001)	(0.003)	(0.003)	(0.002)	(0.002)
(4) Severe Crime (at Ages 25-29)	-0.006***	-0.007***	-0.005***	-0.005***	-0.001*	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
(5) Teenage Birth	-0.003***	-0.003***	-0.0004**	-0.001*	-0.001***	-0.001***
	(0.0002)	(0.0002)	(0.0004)	(0.0004)	(0.0003)	(0.0003)
(6) Age at First Birth (at Ages 25-29)	0.165***	0.157***	0.111**	0.129**	0.044**	0.024
	(0.012)	(0.014)	(0.054)	(0.059)	(0.019)	(0.019)
Individual Char.	Yes	Yes	No	Yes	No	Yes
Nbhd Char.	Yes	Yes	No	Yes	No	Yes
Housing Char.	Yes	Yes	No	Yes	No	Yes
Cohort FE	Yes	No	Yes	Yes	Yes	Yes
Family FE	No	No	Yes	Yes	No	No
Large Cluster-Cohort FE	No	Yes	No	No	No	No
School FE	No	No	No	No	Yes	Yes
Small Cluster FE	No	No	No	Yes	No	Yes
Observations	∼230,000	∼230,000	∼70,000	∼60,000	∼250,000	∼230,000

Notes: See note of Table 3

level controls. The heterogeneity analysis looks at how the effect of school quality varies along several dimensions presented in Table 5.

Table 5: Individual and Household Characteristics Definition for Heterogeneity Analysis

Child's Gender	Columns (1) and (2) of Table 6 explore heterogeneity by gender of the children. In our within-family specification this implies that we necessarily focus on families with at least two daughters or two sons.
Parental Household Income	Columns (3), (4) and (5) split our sample in three categories, parents whose gross household income excluding transfers is below the first quartile, those who earn between the first and third quartile, and those who earn above the third quartile.
Parental Education	Columns (6) and (7) consider heterogeneity based on the maximum level of education of the parents being either high school or college.
Parental Ethnicity	Columns (8) and (9) assess the role of parental origins, splitting the sample based on whether any parent or grandparent is foreign, versus having Danish ancestry from both parents.
Parental Family Structure	Finally, columns (10) and (11) look at the role of family structure, in particular whether being raised in a non-intact family has a differential impact. We define non-intact households as those whose parents have separated during the first 18 years of the child.

Table 6 presents results from the heterogeneity analysis. We investigate the het-

erogeneity in impact of school quality on later life outcomes (i.e., college attendance, income, participation in crime, and teenage pregnancy) by parental background and the gender of the child. Each coefficient is estimated from a separate regression of an outcome on school quality conditional on a large set of covariates, within neighborhoods.

Several lessons emerge from this analysis. First, the effect of attending better schools has larger benefits for individuals with lower parental income and education as well as for those in households of Danish origins.

For instance, with respect to college completion, the estimated effect of school quality on college completion for low income parents stands at 1.28 percentage point, while it decreases to 0.84 percentage points for children of high income parents. In terms of later life income, the contrast is even more stark, with the effect standing at 6.8% for lower-income families, compared to an insignificant effect for higher-income families. Lower income families also benefit more in terms of reduction in criminality and teenage pregnancy. This suggests that implementing policies that improve the quality of public schools can be effective in reducing the gap in academic achievement between children of affluent families and less advantaged children.

There is also an important gradient in terms of parental education. Our results show that an increase in school quality attended for less educated families improves income by around 3.6%, while the effect is insignificant for college educated families. With regards to criminality and teenage birth, we find similar contrasting effects.

With regards to parental ethnicity, we see very muted and statistically insignificant effects to attending better schools, in contrast to the significant positive effect of school quality on later life outcomes for households with Danish origins.

Table 6: Heterogeneity in Impact of School Quality on Later Life Outcomes by Parental Background

	Child's Gender		Parental Income			Parental Education		Parental Ethnicity		Parental Family Structure	
	(1) Female	(2) Male	(3) Low	(4) Medium	(5) High	(6) High School	(7) College	(8) Dane	(9) Foreign	(10) Non-Intact	(11) Intact
College	0.0108***	0.0059**	0.0128***	0.0070**	0.0084**	0.0081***	0.0079**	0.0088***	0.0009	0.0142***	0.0056**
	(0.0028)	(0.0027)	(0.0036)	(0.0027)	(0.0041)	(0.0025)	(0.0032)	(0.0021)	(0.0055)	(0.0034)	(0.0024)
Income	0.0301*	0.0218	0.0678***	0.0165	0.0074	0.0364***	0.0011	0.0278**	0.0037	0.0277	0.0237**
	(0.0158)	(0.0143)	(0.0257)	(0.0139)	(0.0174)	(0.0134)	(0.0174)	(0.0112)	(0.0352)	(0.0222)	(0.0119)
Crime	-0.0029	-0.0050*	-0.0069*	-0.0042*	-0.022	-0.0060***	0.0003	-0.0045**	-0.0001	-0.0082**	-0.0026
	(0.0022)	(0.0028)	(0.0037)	(0.0025)	(0.0037)	(0.0022)	(0.0029)	(0.0019)	(0.0050)	(0.0034)	(0.0021)
Teen Birth	-0.0013***	-0.0005**	-0.0027***	-0.0002	-0.0002	-0.0012***	-0.0004	-0.0011***	-0.0002	-0.0024***	-0.0001
	(0.0005)	(0.0002)	(0.0008)	(0.0004)	(0.0003)	(0.0004)	(0.0003)	(0.0003)	(0.0008)	(0.0007)	(0.0003)

Notes: This Table presents results from the heterogeneity analysis where we estimate equations (1) with different sets of controls and on different split samples. Columns (1) and (2) contrast the effect on females versus males. Columns (3) and (4) look at heterogeneity based on household income. Columns (6) and (7) turn to parental education heterogeneity, while columns (8) and (9) look at the differential role of origins of parents. Finally, columns (10) and (11) provide results contrasting non-intact and intact families. The estimated models include a host of individual, neighborhood and housing characteristics. Individual controls include income, education, origins and marital status of parents, order of birth, age of mother at birth, household size, and gender of the child. Neighborhood and housing characteristics are as in the previous hedonic models. Standard errors corrected for clustering at the school-cohort level are reported in parentheses. Sample includes all parents in Denmark whose children attend ninth grade between 2002 and 2006. For teenage birth, the sample includes cohorts of students from 2002 to 2015. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

A second important lesson emerging from the heterogeneity analysis is with regards to gender effects. In particular, attending better school leads to a 1.1 percentage point increase in college completion for females, while the effect is half as large for males. This effect seems to translate to differences in the effect of school quality on later life incomes, albeit the effect is here more muted. This is in line with the literature (see for instance Chetty *et al.*, 2014).

6 Robustness, Sensitivity Checks and Extension

In this section, we report several robustness and sensitivity checks pertaining to the above empirical strategies. We also present an analysis which includes covariates pertaining to peers at the school to tease out the effect of peers' characteristics on later life outcomes.

6.1 Placebo Test

Our within-school strategy relies on cohort-to-cohort variation in school quality. This strategy provides exogenous variation in school quality because parents have only limited ability to select the specific cohort their children attend. School quality variation is then driven by shocks to cohort quality either arising through peers or teachers. To test that selection does not plague this strategy we conduct a placebo test where we replace our outcome variable with a pre-determined variable—birth weight. Although pre-determined, birth weight is known to be correlated with later life achievement (Black *et al.* (2007); Eshaghnia (2021)). We find that school quality has a statistically insignificant effect on birth weight (measured in grams), with a coefficient of -0.73 and a standard error of 4.5.

6.2 School Boundary Changes as Exogenous Variation

To further confirm the magnitude and sign of our results, we capitalize on changes in schooling boundaries occurring occasionally in Denmark. Our strategy consists in us-

ing within old school boundaries-by-small-neighborhood variation in school quality, in the spirit of Billings *et al.* (2014). The fact that these students lie in the same neighborhood reduces potential biases arising from parents' neighborhood selection based on school quality. Still, over time school boundaries change in ways that parents' may not be able to predict. Variation in school quality arises from changes in school boundaries which leads some students within the old school boundary (and within a small cluster) to attend different schools. We fix school boundaries at the earliest time in which we observe them, i.e., in 2006. We then focus on cohorts between 2007 and 2010. We then focus on assessing the impact of school quality on gross income before transfers. Estimating this model returns a statistically significant (p-value < 0.01) coefficient that stands at 10.8%.

6.3 Within Schools Across Classrooms

Given the availability of data on classrooms for students graduating 9th grade from 2007 onwards, we utilize data up to 2013 to run a specification using variation in school quality across classrooms within schools-by-year. Our measure of school quality is computed using average test scores of students at the classroom level. In the presence of sorting of students and teachers across classrooms, such specification may lead to biased estimates. However, Beuchert-Pedersen (2016) shows there is limited scope for such sorting in Denmark. We present estimates for such specification in Appendix C.1 and contrast it with a within school specification using the same sample between 2007 and 2013. We show that our estimates are largely robust.

6.4 Past Grades

A potential concern in our specification is the lack of controls for student past performance. Reassuringly, our placebo test using birthweight as an outcome has shown that we are not picking up variation in innate features of individuals, which the literature has shown to be correlated with student performance and outcomes. Still, the issue of past performance confounding our results may be of concern. Due to data availability

restrictions, we have not been able thus far to include a measure of past performance in our specifications. By modifying the sample, we are able to do so, for students who graduate 9th school grade between 2010 and 2014. We present the results of this specification in Appendix C.2, where we add a control for 8th grade average test score. The results are contrasted with a baseline specification using the same sample and within school variation without adding past test scores as a regressor.

6.5 Peers

We ran additional specifications adding the mean and variance of peers' (or their parents') characteristics. We focus on the within-school specification. We find that the education and income of the peer's parents matter for better later life outcomes, as well as the variance of these covariates. This is in line with our WTP estimates that parents' education and income are capitalized into house prices.

6.6 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 is in contrast with used test provided by Oster (2019), since it allows for endogenous controls. We provide a brief overview of this approach in Appendix D. We report results from both methodologies in Table 7. We focus on our specification with school fixed effects as specified in Equation.

Column (1) of Table 7 presents Oster's δ for each of the outcomes of interest, under the assumption that $R^2_{\text{long}} = 1.3 \times R^2$, where R^2_{long} is the R-squared of the long regression (i.e., the regression including the unobservables) and R^2 is the R-squared of the regression without unobservables. We find that the breakdown point, i.e. the amount of selection on unobservables compared to the selection on observables that would overturn the sign of our results, is in most cases significantly higher than 1.

Similarly, Column (2) presents breakdown points under Diegert *et al.*'s framework 2022. We find that our estimates for key outcomes of interest are robust to the pres-

ence of selection of unobservables. This is the case for income, income growth, taxes, transfers, as well as age at first birth where the breakdown point lies between 60% and 100%. Given the large number of covariates we add as controls, such magnitudes seem reassuringly high[20]. Indeed, as explained by Diegert *et al.* (2022), this relative magnitude needs to be interpreted in light of the number of relevant regressors that can be controlled for in the main regression.

Table 7: Breakdown Points from Oster and Diegert *et al.* Methodologies

	Oster's δ	Diegert *et al.*'s δ
	(1)	(2)
(1) Income (at Ages 28-29)	3.53	.72
(2) Disposable Income (at Ages 28-29)	11.6	.33
(3) Income Growth (Ages 24-25 to 28-29)	4.46	1
(4) Taxes (at Ages 28-29)	6.66	.65
(5) Transfers (at Ages 28-29)	14.4	.49
(6) Employment (at Ages 28-29)	5.94	.17
(7) Homeownership (at Ages 28-29)	2.4	.15
(8) College Completion (at Ages 29-30)	2.87	.31
(9) University Completion (at Ages 29-30)	.96	.10
(10) Crime (at Ages 25-29)	3.39	.17
(11) Severe Crime (at Ages 25-29)	3.27	.04
(12) Teenage Birth	2.98	.02
(13) Age at First Birth (at Ages 25-29)	.98	.60

Notes: Column (1) presents Oster's δ, while Column (2) presents the breakdown point under the framework of Diegert *et al.* (2022) for each the outcome variables. For each of these outcome variables we run the specification, as specified in Equation (9).

[20]We note that under Diegert *et al.* (2022), the breakdown point cannot be larger than 100%. See Appendix D for more details.

7 The IRR for Investing in Children by Locating in Better Neighborhoods

In this section, we provide estimates of the private IRR for investing in children through living in neighborhoods with higher-quality schools, we take public expenditures as given. The estimates of the impact of attending better schools (being exposed to high-quality peers and teachers) on various outcomes later in adulthood in Section 4 provides us with the benefits of such investments. We relate these estimates to the WTP estimates presented in Section Eshaghnia *et al.* (2023), which provides us with the cost of such investments.

In what follows, we compute the IRR under two alternative approaches. In the first approach, we assume that the increment in children's incomes in their late 20s (due to attending better schools) will recur every year until they retire at age 65, i.e., the same dollar amount for all future ages. In the second approach, we assume that income effects of attending a better school remain the same as a percentage of income going forward until age 65. To this end, we use the income trajectory of previous cohorts over their working life cycle to project the future income of younger cohorts in our sample (whom are observed only up to age 33).

7.1 Constant Income Effects in Levels

Estimates of the WTP for school quality in Eshaghnia *et al.* (2023) imply that households are, on average, willing to pay around $6,600 for a one–standard deviation increase in school quality. Results from Section 4 relate this quality increase to approximately $470 to $2600 higher income (at the mean of the later life earnings of our sample) at ages 28–33 for the various models we used. Assuming this same differential in income recurs every year until retiring at age 65, for two siblings of the median family, yields internal rates of return ranging from 4.0% to 12.4% based on the school fixed effect and the family fixed effect models, respectively.

We also repeat this exercise using our heterogeneity results to allow for different

treatment effects by the gender of children. Under this scenario, we find IRR estimates ranging from 6.4% to 14.9% based on the school fixed effect and the family fixed effect models, respectively.

7.2 Constant Income Effects in Percentage

The hump-shaped life cycle income trajectory is well known in the literature. In our second approach, we allow the income effects of better schools to change over the life cycle. For this purpose, we use the log specification for estimating the impact of attending better schools on adulthood income presented earlier in Section 4. We assume that the school effect is constant across all future ages as percentage of income, but the base income level changes over the life cycle. To this end, we use 1955 birth cohorts (whom we observe from age 25 to 64) to forecast the future income of children in our sample.[21] Under this scenario, we reach IRR estimates ranging from 6.4% to 14.0% based on the school fixed effect and the family fixed effect models, respectively.

To put our IRR estimates in Sections 7.1 and 7.2 in perspective, they are in the range of the estimates of internal rates of return in García *et al.* (2021) for the Perry Preschool Project (PPP) —a pioneering high-quality early childhood education program implemented in the US before Head Start that targeted disadvantaged African-Americans and was evaluated by a randomized trial.[22]

8 Conclusion

The Scandinavian welfare state is often regarded as an exemplary system to reduce inequalities and equalize opportunities, inter alia, by providing an education system that is free for all. Yet, despite an equalized school expenditure and teacher salary distribution, there exist substantial differences between schools in terms of quality of teachers and the skill levels of the peer students. These differences are, in part, due

[21] We adjust for cohort effects separately for males and females. The adjustment factor is computed by comparing the income trajectories across cohorts over ages 29–33.

[22] We should note that here we only present the private IRR, whereas García *et al.* (2021) computes a social return.

to residence-based assignment of students to public schools along sorting of families and teachers across neighborhoods. More advantaged families sort into neighborhoods where school quality is higher.

We study the impacts of attending schools with more able peers and teachers on long-run outcomes of children. Taking advantage of our rich longitudinal data, we find that attending better schools significantly impacts later life outcomes, increasing college education and wages while reducing criminality and teenage pregnancy. We also find that the impacts of schools are heterogeneous across children of families with different socioeconomic statuses.

Linking our estimates of school effects with the WTP estimates presented in Eshaghnia *et al.* (2023), we provide estimates of the internal rate of return for investment in children through the choice of the neighborhood of residence by parents. Parents invest in children, among other things, by residing in places with higher-quality schools, which is associated with payoffs in adulthood of children in form of education, earnings, and risky behaviors. Our results indicate that the internal rate of return (in real terms) for such investment is about 4%–15%.

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The Dynastic Benefits of Neighborhood Sorting Appendix

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Appendix

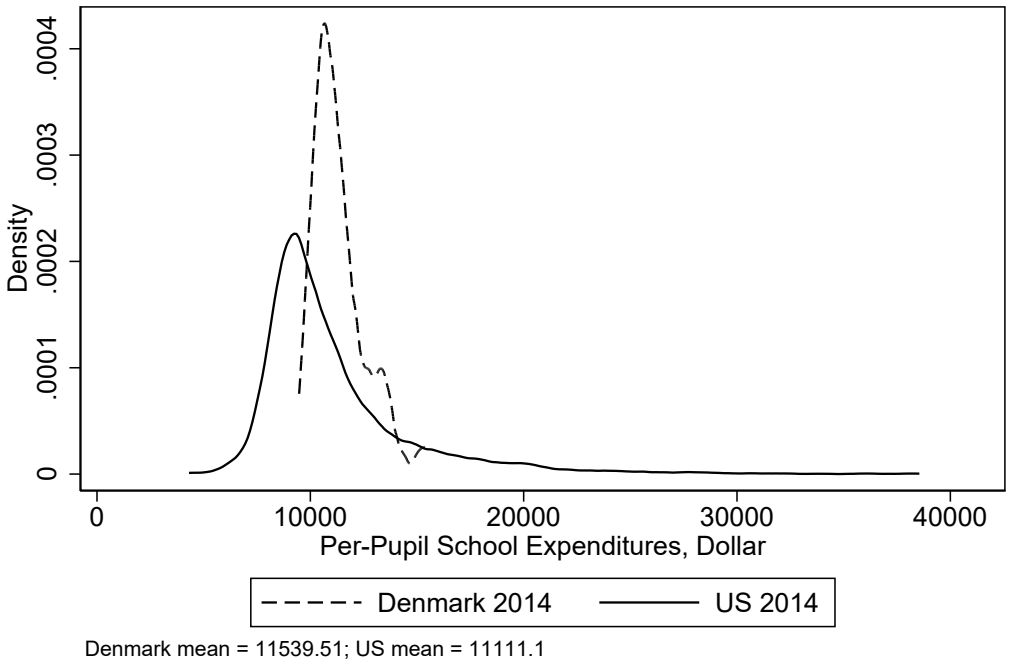
A Data Description

This appendix presents summary statistics of the data we use for the empirical analysis in the paper. It also discusses our procedure for constructing the neighborhood clusters. Section A.1 presents the distribution of the per-pupil school expenditure for Denmark and compares it to the distribution of the per-pupil school expenditure for the US. Section A.3 documents the correlation between different neighborhood attributes.

A.1 Per-Pupil School Expenditure

This section presents densities of the per-pupil school expenditure for the US and Denmark in 2014. Figure A.1 presents the distribution of the average per-pupil school expenditures in public schools for both countries.

Figure A.1: Comparing Per-Pupil School Expenditure across Municipalities in Denmark and the US (2014 US Dollar)

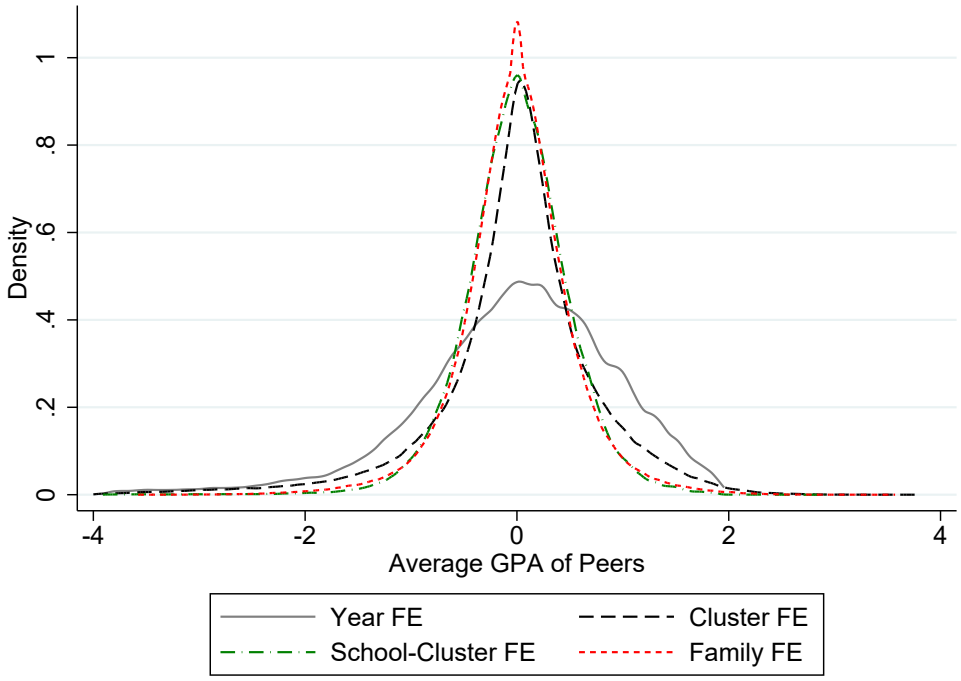


Notes: This figure shows the average per-pupil school expenditures in public schools in 2014 for the US and Denmark.

A.2 Variation in School Quality

In this section, we provide evidence of the variation remaining in our measure of school quality (leave-one-out average test scores), in our various specifications reported in Panel B of Table 1. Figures A.2 presents the results. The standard deviation of school quality is 0.82 in our pooled sample. Due to the spatial sorting of families (and teachers) along the residence-based assignment of students to public schools, the variance drops by about half, to around 0.57, 0.47, and 0.45, respectively for the cluster fixed effect, school-by-cluster fixed effect, and family fixed effect specifications.

Figure A.2: Variation in School Quality



Notes: The figure shows the distribution of the school-level average of test scores after demeaning by the corresponding fixed effect level in Denmark. The solid line shows the distribution for the whole country after taking out year fixed effects (demeaning by year average). The long-dashed line presents the distribution after taking out cluster fixed-effects (demeaning by the cluster's average). The dash-dot line shows the distribution after taking out school-cluster fixed-effects (demeaning by the corresponding school-by-cluster average). Finally, the dash line presents the distribution after taking out family fixed-effects (demeaning by the average over siblings). The corresponding standard deviations are 0.82, 0.57, 0.47, and 0.45, respectively for the year FE, cluster FE, school-by-cluster FE, and family FE.

A.3 Correlation Matrix of Neighborhood Attributes

Table A.1 reports the correlation between different neighborhood attributes.

Table A.1: Correlation Between Neighborhood Attributes

	Log Households Gross Income	Households Years of Educ.	School Quality	Share Criminals	Share Married Households	Share Foreigners	Share non-Westerners	Share Private Schools
Log Households Gross Income	1							
Households Years of Educ.	0.838***	1						
School Quality	0.431***	0.490***	1					
Share Criminals	-0.196***	-0.207***	-0.203***	1				
Share Married Households	0.662***	0.360***	0.197***	-0.275***	1			
Share Foreigners	-0.238***	-0.0401***	-0.175***	0.301***	-0.390***	1		
Share non-Westerners	-0.290***	-0.136***	-0.232***	0.304***	-0.337***	0.958***	1	
Share Private Schools	0.0589***	0.131***	0.102***	0.0322***	-0.0769***	0.0954***	0.0657***	1
Share Non-Intact Households	-0.316***	-0.217***	-0.194***	0.126***	-0.347***	0.0703***	0.0595***	0.00251

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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

B Structural Interpretation of Reduced-Form Estimates of School Quality

In this Appendix, we formalize how the reduced-form estimates of the impact of school quality on later life outcomes should be interpreted through the lens of a dynamic model of the school-quality-achievement relationship, based on a modified version of Todd & Wolpin (2003).

Conceptual Framework Consider a two-period model, where $t = 0$ corresponds to time interval prior to the age the child enters school. $t = 1$ corresponds to the period in school.

Let Y_0 denote the child's human capital at the end of period $t = 0$ while F_0 and N_0 represent family as well as neighborhood quality inputs into the education production during $t = 0$. Finally, let μ be a measure of the child's ability, determined at birth.

Achievement at the time of school depends only on family and neighborhood quality inputs as well as ability:

$$Y_0 = g_0(F_0, N_0, \mu)$$

Family and neighborhood inputs in the pre-school period are assumed to be determined by the family's permanent resources, W, and the child's endowment μ; whereby $F_0 = \phi_0(W, \mu)$. We consider a similar production function for neighborhood quality inputs in period $t = 0$ with $N_0 = \Phi_0(W, \mu)$.

Achievement post-schooling depends on the entire history of family and neighborhood quality inputs (F_0, F_1, N_0, N_1) and school inputs S_1 as well as endowments:

$$Y_1 = g_1(S_1, F_0, F_1, N_0, N_1, \mu) \tag{5}$$

$$T_1 = \tau_1(S_1, F_0, F_1, N_0, N_1, \mu) \tag{6}$$

where T_1 denotes test scores upon school completion and Y_1 the later life outcomes we observe at around age 30 of the individual.

Along with the above technology for combining inputs to create outcomes, the level of input is determined by decision rules from the parents and the schools.

Let S_1 denote the actual school-input level that is relevant to the child and $\bar{S}_1$ the expected school-level quality. This distinction can arise due to parents having incomplete information – at the time of the neighborhood choice decision, viz. school choice decision – about the actual level of school-input. The decision rule of the family and the school inputs associated with their neighborhood decisions are given by:

$$F_1 = \phi_1(Y_0, W, \mu, S_1 - \bar{S}_1)$$

$$N_1 = \Phi_1(Y_0, W, \mu)$$

$$\bar{S}_1 = \theta(Y_0, W, \mu)$$

We assume here that the school chooses input levels purposefully, accounting the child's achievement level Y_0 as well as the endowments W and μ.

School input decision at $t = 1$ is determined by the following relationship, where family resources do not play a role:

$$S_1 = \kappa_1(\text{P, E, } \Pi\text{, A}) \tag{7}$$

where P, E, Π and A, respectively denote peer quality, school expenditure, teacher quality and student ability.

Parameter of Interest In this simple model, the total effect of an increase in the school input, S_1, on achievement Y_2 is given by:

$$\frac{dY_2}{dS_1} = \frac{\partial g_1}{\partial S_1} + \frac{\partial g_1}{\partial F_1}\frac{\partial F_1}{\partial (S_1 - \bar{S}_1)} \tag{8}$$

Thus, to disentangle the different channels, one requires knowledge of the family and neighborhood input decision rule. Without such knowledge, one can learn about the total effect of an exogenous change in school quality on achievement, without holding other inputs constants. The latter is what we claim we are capturing in the empirical models below.

Model of School Effects To formalize our model of school effects on later life outcomes, we start by assuming linear relationships in the production function of test scores T_1 and later life outcomes Y_1. We further impose linearity on the production function for the inputs. Plugging in those linear relationships for the inputs into the production functions for T_1 and Y_1, we get the following:

$$T_1 = \tau_1(\kappa_1(\bullet), \phi_0(\bullet), \phi_1(\bullet), \Phi_0(\bullet), \Phi_1(\bullet), \mu) \tag{9}$$

$$
\begin{aligned}
T_1 \;=\; & \underbrace{\alpha_1 P + \alpha_2 E + \alpha_3 \Pi + \alpha_4 A + \eta}_{\kappa_1} \\
& + \underbrace{\beta_1^F W + \beta_2^F \mu}_{\phi_0} + \underbrace{\gamma_1^F \underbrace{[\delta_1^F F_0 + \delta_2^F N_0 + \delta_3^F \mu]}_{g_0} + \gamma_2^F W + \gamma_3^F \mu}_{\phi_1} \\
& + \underbrace{\beta_1^N W + \beta_2^N \mu}_{\Phi_0} + \underbrace{\gamma_1^N \underbrace{[\delta_1^N F_0 + \delta_2^N N_0 + \delta_3^N \mu]}_{g_0} + \gamma_2^N W + \gamma_3^N \mu}_{\Phi_1} \\
& + \theta\mu + \varepsilon
\end{aligned} \tag{10}
$$

where we have abstracted from individual, school, neighborhood and cohort subscripts to simplify notation.

We assume rational expectations such that $S_1 - \bar{S} = 0$. Collecting terms, we get:

$$
\begin{aligned}
T_1 \;=\; & \alpha_1 P + \alpha_2 E + \alpha_3 \Pi + \alpha_4 A + \eta \\
& + W[\beta_1^F + \gamma_2^F + \beta_1^N + \gamma_2^N] \\
& + \mu[\beta_2^F + \gamma_1^F \delta_3^F + \gamma_3^F + \gamma_1^N \delta_3^N + \gamma_3^N + \theta] \\
& + F_0[\gamma_1^F \delta_1^F + \gamma_1^N \delta_1^N] \\
& + N_0[\gamma_1^F \delta_2^F + \gamma_1^N \delta_2^N] \\
& + \varepsilon
\end{aligned} \tag{11}
$$

Residualizing with respect to W, μ, F_0, N_0, we get:

$$
\begin{aligned}
\tilde{T}_1 \;&=\; \alpha_1 \tilde{P} + \alpha_2 \tilde{E} + \alpha_3 \tilde{\Pi} + \alpha_4 \tilde{A} + \tilde{\eta} + \epsilon \\
&=\; \alpha_k \mathcal{Z} + \tilde{\eta} + \epsilon
\end{aligned} \tag{12}
$$

where $\mathcal{Z} = [\tilde{P}, \tilde{E}, \tilde{\Pi}, \tilde{A}]'$ is a vector of residualized school characteristics with coefficients $\alpha_k = [\alpha_1, \alpha_2, \alpha_3, \alpha_4]$. Let $\alpha_k \mathcal{Z} = z$.

Now, let $\tilde{T}_m$ denote the mean test score in school m and $\tilde{T}_{mc}$ denote the mean test score in school m (in cohort c). Let I denote the number of students per cohort, C the number of cohort by school and N the number of schools.

The mean test score in cohort c is

$$
\frac{1}{I} \sum_i \tilde{T}_{imc} = \eta_m + z_{mc} + \frac{1}{I} \sum_i \epsilon_{imc}
$$

To simplify notation, assume that the mean value of $\mathcal{Z}_{mc}$ across cohorts within a school is 0 ($z_m = 0$). Then the difference between mean test scores in cohort c and mean scores in the school is

$$\Delta\tilde{T}_{mc} = \tilde{T}_{mc} - \tilde{T}_m = z_{mc} + \left[\frac{1}{I}\sum_{j=1}^{I}\epsilon_{jmc} - \frac{1}{IC}\sum_{c=1}\sum_{j=1}\epsilon_{jmc}\right]$$

The equation above shows that $\Delta\tilde{T}_{mc}$ is a noisy observable measure of school quality z_{mc}.

The noise may arise from variation in student abilities across classes.

As the number of students grows large, $\Delta\tilde{T}_{mc}$ converges to the true underlying school quality S_{mc} if all students are conditionally randomly assigned to cohorts within schools.

This equation motivates substituting $\Delta\tilde{T}_{mc}$ for S_1 in Equation 10.

C Further Robustness Checks for the Impact of School Quality on Later Life Outcomes

This appendix presents further robustness checks for the estimates of the impact of school quality on various outcomes later in adulthood presented in Section 4 of the main paper. Section C.1 exploits variation in the 9th grade test scores across classrooms within schools-by-year to estimate the impact of school quality on later life outcomes. Section C.2 shows the robustness of our baseline estimates where we add a control for student past performance to our main specification.

C.1 Using Across Classroom and Within School Variation

Given the availability of data on classrooms for students graduating 9th grade from 2007 onwards, we utilize data up to 2012 to run a specification using variation in school quality across classrooms within schools-by-year. Our measure of school quality is computed using average test scores of students at the classroom level. Table C.1 presents the results for such specification and contrast it with a within school specification using the same sample between 2007 and 2012. Our estimates are largely robust.

Table C.1: Sensitivity Test: Within Schools Across Classrooms

	Baseline	Within Schools Across Classroom
	(1)	(2)
Income (at Ages 20-21)	0.0453***	0.0563***
	(0.0115)	(0.0113)
Disposable Income (at Ages 20-21)	0.0045*	0.0024
	(0.0024)	(0.0038)
Taxes (at Ages 20-21)	0.0157**	0.0069
	(0.0068)	(0.073)
Transfers (at Ages 20-21)	0.0031	-0.0271***
	(0.0073)	(0.0075)
Employment (at Ages 20-21)	0.0001	-0.0003
	(0.0002)	(0.0002)
Homeownership (at Ages 20-21)	0.0002	0.0009
	(0.0011)	(0.0009)
Crime (at Ages 18-21)	-0.0016	-0.0011
	(0.0018)	(0.0017)
Severe Crime (at Ages 18-21)	-0.0005	-0.0017***
	(0.0005)	(0.0005)
Teenage Birth	-0.0007*	-0.0011***
	(0.0004)	(0.0004)
Age at First Birth (at Ages 18-21)	0.0877**	0.0356
	(0.0382)	(0.0382)
Nbhd Characteristics	Yes	Yes
Housing Characteristics	Yes	Yes
School and Cohort FE	Yes	No
School-by-Cohort FE	No	Yes
Observations	$\sim$270,000	$\sim$270,000

Notes: As a baseline, column (1) shows coefficients on school quality for several regressions where we vary the outcome, as noted in each row. This specification includes a host of individual, neighborhood and housing characteristics as controls, as well as school and cohort fixed effects. Individual controls include income, education, origins and marital status of parents, order of birth, age of mother at birth, household size and gender of the child. Neighborhood and housing characteristics are as in the previous hedonic models. Column (2) presents coefficient on school quality measured at the classroom level. Fixed effects are included at the school-by-cohort level. We add the same individuals, housing and neighborhood controls as in column (1). Standard errors corrected for clustering at the school-cohort level are reported in parentheses. Sample includes all parents in Denmark whose children attend ninth grade between 2007 and 2012. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

C.2 Controlling for Past Test Scores of Students

Due to data availability restrictions, we do not include a measure of past performance in our baseline specifications in the main paper. By modifying the sample, we are able to do so, for students who graduate 9th school grade between 2011 and 2014. This section presents the results of such robustness check where we add a control for 8th grade average test score. The results are contrasted with a baseline specification using the same sample and within school variation without adding past test scores as a regressor. Table C.2 reports the results.

D An Overview of Diegert *et al.* (2022)

To address omitted variables bias, researchers have relied on sensitivity analyses which allow to bound estimates in presence of selection on unobservables. These methods, as developed for instance by Altonji *et al.* (2005) and Oster (2019), require 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 to use one, two or three different sensitivity parameters to bound the parameter of interest in the presence of endogeneous unobservables. The first parameter, $\bar{r}_X$, captures the relative magnitude of selection on unobservables and selection on observables. The second sensitivity parameter, $\bar{r}_Y$, measures the relative magnitude of the effect of unobservables on the outcomes. Finally, The third sensitivity parameter, $\bar{c}$, captures the level of endogeneity – the relationship between the observables and unobservables.

Conveniently, this approach doesn't require to set all three parameters simultaneously. In fact, Theorem 2 of Diegert *et al.* (2022) characterizes the identified set for the parameter of interest in a long regression with both observed and unobserved covariates using only $\bar{r}_X$ as a sensitivity parameter. This is in contrast with Oster (2019), where two tuning parameters need to be chosen. In other words, their work characterizes the *breakdown point*, $\bar{r}_X^{bp}$, which is the largest amount of selection on unobservables under which our coefficient of interest – the WTP for school quality – would still be nonnegative. We use this result to compute the breakdown point of $\bar{r}_X^{bp} = 61\%$ presented in the main text. We note that in their framework, this breakdown point cannot be greater than 100%.

Table C.2: Sensitivity Test: Adding Past Test Scores

	Baseline	Past Test Score As Regressor
	(1)	(2)
Income (at Ages 22-23)	0.0342**	0.0374**
	(0.0150)	(0.0151)
Disposable Income (at Ages 22-23)	0.0058	0.0074**
	(0.0038)	(0.0038)
Taxes (at Ages 22-23)	0.0089	0.0121
	(0.0129)	(0.130)
Transfers (at Ages 22-23)	-0.0280***	-0.0351***
	(0.0082)	(0.0082)
Employment (at Ages 22-23)	0.0007***	0.0007***
	(0.0002)	(0.0002)
Homeownership (at Ages 22-23)	0.0005	0.0006
	(0.0008)	(0.0008)
Crime (at Ages 20-25)	-0.0017	-0.0001
	(0.0020)	(0.0020)
Severe Crime (at Ages 20-25)	0.0002	0.0002
	(0.0004)	(0.0004)
Teenage Birth	0.0001	0.0001
	(0.0003)	(0.0003)
Age at First Birth (at Ages 20-25)	-0.0318	-0.0319
	(0.1957)	(0.1966)
Nbhd Characteristics	Yes	Yes
Housing Characteristics	Yes	Yes
School and Cohort FE	Yes	Yes
Observations	$\sim$170,000	$\sim$170,000

Notes: As a baseline, column (1) shows coefficients on school quality for several regressions where we vary the outcome, as noted in each row. This specification includes a host of individual, neighborhood and housing characteristics as controls, as well as school and cohort fixed effects. Individual controls include income, education, origins and marital status of parents, order of birth, age of mother at birth, household size and gender of the child. Neighborhood and housing characteristics are as in the previous hedonic models. Column (2) shows the same specification and adds past test score in 8th school grade as a control. Standard errors corrected for clustering at the school-cohort level are reported in parentheses. Sample includes all parents in Denmark whose children attend ninth grade between 2011 and 2014. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$