THE UNIVERSITY OF CHICAGO

ESSAYS ON THE MEASUREMENT AND ORIGINS OF INEQUALITY

A DISSERTATION SUBMITTED TO
THE FACULTY OF THE DIVISION OF THE SOCIAL SCIENCES
IN CANDIDACY FOR THE DEGREE OF
DOCTOR OF PHILOSOPHY

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CHICAGO, ILLINOIS
AUGUST 2021

To my parents, Tammy and Vince.

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Acknowledgments

I am deeply grateful for the encouraging words and guidance of my thesis committee, Steven Durlauf, Stephane Bonhomme, and Richard Hornbeck. I also thank participants of the Econometrics Advising Group, as well as my friends and colleagues in the department for helpful comments. In particular, I'd like to thank Claire Ding, Maxwell Kellogg, Mauricio Chikitani, Kuan-Ming Chen, Joshua Shea, Ezra Karger, and Taeho Kim. I also thank James Heckman. This work would not have been possible without the staff of the University of Chicago and Harold Washington libraries. I must also recognize Angel Rose Valdivia, Rosallie Liboon Valdivia, and Benjamin Diego for their help digitizing data on the Chicago Public Schools.

Without family and friends, I would not have found the fortitude to push past the failures I could overcome nor the humor to laugh at those I could not. I thank my parents, Tammy and Vince, for their love and encouragement throughout my life and especially during the writing of these essays. I can only aspire to their strength, vigor and creativity. For their wise counsel and antics, I thank my sisters Gina and Nikki. And finally, I thank my friends for suffering my less polished discourse.

ABSTRACT

This thesis contains three essays related to the measurement and origins of inequality. The first two essays consider the Chicago public schools during the early 20th century. In the first essay, I evaluate the impact of school quality on child outcomes. Then I consider the impact of racial turnover on school funding. In the third and last chapter, I show how to adjust for effort when estimating latent skills from measures of academic performance.

CHAPTER 1

THE CHICAGO PUBLIC SCHOOLS AND SOCIAL MOBILITY IN THE ERA OF MASS MIGRATION: EVIDENCE FROM SCHOOL BOUNDARY CHANGES

1.1 Introduction

When Southern blacks made their first great push into the industrial cities of the North, they would replace the once constant flow of European immigrants that comprised the lion's share of the population. These black immigrants sought refuge from Southern persecution, higher wages in Northern industry, and better opportunities for their children in Northern schools. In this regard, they were similar to many of the white immigrants from Europe. But unlike their white counterparts, black immigrants did not have access to the same jobs nor the same quality schools. Social and economic mobility, both within and across generations, would be more difficult to achieve.

This paper considers the role of the public schools in promoting the social and economic mobility of both native and immigrant children in the city of Chicago. By digitizing historical records of elementary school attendance boundaries from 1910 to 1930, I develop a new dataset that allows–for the first time–the systematic study of early 20th century segregation and school quality *within* a Northern city. By linking this data to the census, I estimate the effects of school quality and student composition on the adult outcomes of children.

The early 20th century is an ideal time period for studying the origins of urban inequality. It marks a time when immigration flows to Northern cities transitioned from primarily white European immigrants to black Southerners. As such, it coincided with the beginning of demographic changes to urban areas that would last roughly half a century. By focusing on this earlier period, I can compare black and white immigrants of the same generation, contrasting their experiences at school and in their neighborhoods, with the goal of explaining initial gaps in education and employment.

Before I proceed, I will define some terms for ease of exposition. I will use the term "black" when referring to either black immigrants from the South or blacks native to Chicago. I make this simplification for two reasons. First, the vast majority of these children will be first or second generation immigrants from the South. Second, I'd like to reserve the term "immigrant" for first or second generation foreign white immigrants, and "native" for 3rd generation or greater white immigrants.

In the first part of the paper, I discuss the data. I fill an important empirical gap in the historical record. To my knowledge, I construct the first comprehensive dataset of school quality and enrollment in a major Northern city during the period 1910 to 1930. Though a national study of local schools would be prohibitively expensive, if not impossible, I've assembled a substantial amount of resources for the city of Chicago. From annual reports and proceedings of the school board, and published books of school attendance boundaries, I can link households and their children to their local neighborhood schools and associated budgets. I can also determine the demographic composition of children in the attendance area.

Constructing the data would not be possible without two recent and ongoing census data projects. First, the Urban Transition Historical GIS (HGIS) project, which links IPUMS full count census data to their near exact location within major cities (Shertzer, Walsh, & Logan, 2016). And second, IPUMS' Multigenerational Longitudinal Panel (MLP) project, which I use to link individuals across decennial census years (Helgertz et al., 2020; Ruggles et al., 2020).

I document the demographic composition and funding of schools across space and over time. Both immigrants and blacks were segregated from their native peers, but black segregation is extreme and unique from other immigrants by 1930. Any disparities in funding between white natives and immigrants is eliminated by 1930, but the average black school loses substantial ground.

In the second part of the paper, I study the effects of schools on the outcomes of the 1920 elementary school cohort. My identification strategy is motivated by a spatial discontinuity design, and is similar to the approach of Billings, Deming, and Rockoff (2014). I exploit differences in school assignments at attendance boundaries, comparing children initially born in the same attendance area, but who would be on opposite sides of a newly drawn boundary at elementary school age. That is, I fix the child's location to their and/or their family's location in the 1910 census, leveraging variation in school assignment induced by changes to boundaries between 1910 and 1920, when the children are of elementary school age.

Unlike Billings et al. (2014), however, I can only measure location up to the street-enumeration district level. In regression discontinuity (RD) designs, this imprecision is analogous to measurement error in the running variable, which can be pathological (Battistin, Brugiavini, Rettore, & Weber, 2009; Davezies & Le Barbanchon, 2017). Thus, I first discuss identification of school effects in the presence of measurement error within an RD framework. To simplify the discussion, I consider just a single boundary separating two schools. This framework also allows me to clearly state my assumptions regarding sorting and manipulation around school boundaries.

The aforementioned RD design motivates my regression estimator. I pool the changes induced by all of the boundary changes into a single regression with initial school and enumeration district fixed effects. Because enumeration districts are small, they effectively form a tight bandwidth around the new boundary lines. Only variation induced by boundary changes informs the coefficients on the school characteristics. Note that this design does not manipulate individual school characteristics, so that it is more of a mediation exercise. I cannot separate the effects of observed from unobserved school characteristics. However, I do select what I consider to be important attributes of the schools, including funding and student composition. And finally, my estimates more accurately reflect intent-to-treat (ITT) effects, i.e., the effects of the 1920 school characteristics associated with a household's 1910 location.

Though data limitations prevent direct analysis of black children, I show that white immigrants assigned to schools with more native peers increase their educational attainment. Native children respond similarly to the native share, but reduce their education when assigned to schools with more funding. This reduction coincides with an increase in employment. To put the magnitudes into context, I study the implications of assigning white immigrants to schools with characteristics similar to those attended by the average black child. Closing the gap by as little as 10% dramatically reduces educational attainment. My results are robust to a variety of specifications, including different thresholds for school assignments and choice of population weights. I also find evidence that high rates of parochial school enrollment among immigrants may attenuate the impacts of public school characteristics.

With the estimates in hand, I then interpret the heterogeneity in the coefficients within an economic model of human capital and education choice. I attribute the different responses to differences in social skills[1] between immigrant and native children upon entry to elementary school. When social skills and elementary school investments (i.e., funding) are strong complements, an increase in elementary funding reduces high school attendance the higher one's initial level of social skill. On the other hand, an improvement to one's peer group–in terms of average social skill–increases educational attainment regardless of initial social skill. The improved social interactions in elementary school increase social skills, which improve the productivity of further social interactions and cognitive development in high school.

The paper is organized as follows. In Section 1.2, I discuss related literature and this paper's contribution. In Section 1.3, I describe how I construct my sample of children from the census and match them to their neighborhood school. I then compare this sample to the population. I also document variation in school quality across and within demographic groups, and describe sorting and segregation patterns. Section 1.4 describes my identification

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[1] For example, native children come from families with greater proficiency in the English language, and are probably more familiar with American culture. Both of which may improve their interactions with the public school system and the labor market.

strategy and estimator. In Section 1.5, I provide estimation details, describe results, and conduct robustness exercises. I then use the results to calibrate an economic model of human capital and education choice and perform counterfactual exercises. Section 1.6 concludes and discusses avenues for future research.

1.2 Literature Review

A significant number of papers and books describe migration to and within the US during the early 20th century. The time period marked the transition from the era of mass European migration to the Great Migration of black Southerners to Northern cities.

1.2.1 Mass Migration from Europe

Abramitzky and Boustan (2017) provide a short summary of the work in economic history on the immigration of Europeans to the US. Much of the work focuses on immigrant selection, the impacts of migration on migrants, and the political and economic consequences to their destinations and native populations. The line of research most relevant to this paper considers immigrant assimilation and economic mobility within and across generations.

Abramitzky, Boustan, and Eriksson (2014) describe how the cross-section overstates the convergence of European immigrants to their native counterparts. Less skilled immigrants were more likely to return to Europe, generating positive selection in the cross-section. With a panel, however, Abramitzky et al. (2014) show that long-term immigrants (i.e., those that did not return to Europe) held similar occupations as natives upon arrival, and followed similar patterns of occupational transition. Heterogeneity by sending country persisted over time, with gaps closing slowly, if at all, both within and across generations.

The literature is a little thin on the determinants of the economic and social mobility of European immigrants. Abramitzky et al. (2014) attribute the slow rate of convergence between poor immigrants and natives to the lack of integration. More integration with natives might not have only improved labor networks, but also skills such as English proficiency.[2] For children, gaining command of English and other basic skills often took place at school. Yet we know very little about the nature of immigrants' interactions with the school system, and its role in shaping their opportunities and outcomes.

Studies of the school system typically rely on state or city level variation induced by the passage of new laws. Lleras-Muney and Shertzer (2015) find that laws increasing the age requirements for work permits increased educational attainment and earnings, with larger effects on immigrants. Using city level variation, Schmick and Shertzer (2020) is the only paper that considers the impact of school funding on child outcomes for the period. They show that post WWI funding increases induced by the German Americanization movement led to increased education and, for less advantaged populations, increased wages.

Schmick and Shertzer (2020) do not observe how school spending is distributed within cities, even by broad racial or ethnic group. Nor do they see the demographic composition of local schools, which is important for understanding exposure to peer groups. Thus, there is virtually no evidence on the role of peer effects in assimilating migrant children in the early 20th century, despite the importance of assessing whether peer effects mediate the negative impacts of segregation (Ananat, 2011). While immigrants were only moderately segregated, black segregation was extreme. If peer effects mediated the impact of early 20th century segregation, they could explain part of the initial gap in outcomes between black and white immigrants.

1.2.2 The Great Migration

Collins (2021) summarizes the work of economic historians on the Great Migration. By 1930, about 20% of Southern blacks born in the 1900s resided outside of the South, many having moved to the industrial cities of the North where they found work as unskilled laborers. The primary reason for the sudden increase in black migration was the rapid rise in labor

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[2] However, the returns to English proficiency appear lower in the earlier 20th century (Ward et al., 2015).

demand brought about by WWI, coupled with new constraints to European migration. However, other causes, e.g., Jim Crow and damage to Southern cropland, certainly played a role. These initial waves of migration then laid the foundation for future waves, easing access to jobs and establishing the businesses and communities into which future immigrants would settle.

Like some of their European counterparts, black immigrants from the South faced labor market discrimination and residential segregation, but to a much greater degree. And despite earnings gains (see, e.g., Gardner (2020)) and more freedom in public life, black migrants experienced negative health impacts and higher incarceration rates (Black, Sanders, Taylor, & Taylor, 2015; Eriksson, 2019).

The Great Migration also increased local spending on police, and lowered rates of upward mobility for future generations (Derenoncourt, 2019). Focusing more specifically on the early 20th century, Tabellini (2020) finds a strong negative impact of the migration on local spending due to declines in property values. This line of research, however, is limited by the coarseness of the geographic unit of analysis. Indeed, Derenoncourt (2019) suggests that school funding at the commuting zone level may mask differences between the city periphery and suburbs, and the inner city schools serving increasingly black communities.

The unique nature of black segregation lead to differences in school quality that cannot be captured at the aggregate level. It is only within cities that we can compare the different experiences of European and black immigrants in the school system. We can then assess whether these differences help explain initial gaps in outcomes that presage nearly a century of black-white inequality in the North. This paper seeks to overcome the empirical hurdles of the literature by digitizing historical records of Northern schools. Lacking a comprehensive national source, I instead focus on a single city: Chicago. Chicago was not only a major destination for black and European immigrants, but one of the few cities that was both widely studied and for which comprehensive archives exist.

1.2.3 Migration to Chicago

Scholars of all stripes and disciplines have studied Chicago at the turn of the 20th century. At the time, it cemented itself as an economic and cultural pillar of the US, but nonetheless underwent the same demographic and economic transformations shared by many industrial cities of the North. Among these many changes included the development of a modern school system, and the transition from a steady flow of European immigrants to a sudden influx of Southern blacks. Drake, Cayton, et al. (1970) and Grossman (1991) describe the development of "black Chicago" and the lives of residents, while Massey and Denton (1993) paints a more national picture. Philpott (1978) goes a bit deeper into the formation of immigrant and black neighborhoods, and the broader issues of housing development.

Looking specifically at the school system, Homel (1984) is an excellent source on the emergence of racial segregation, while Sanders (1977) discusses the history of the parochial school system. By 1920, "manual training" became an important component of elementary education, with many children–especially foreign immigrants–foregoing further schooling in favor of entering the workforce. Many immigrants attended elementary school through the parochial system, which could account for almost 50% of enrollment in some neighborhoods. Interestingly, parochial schools tended to attract poorer immigrants. Community members donated time and money to support their local parochial school, often in hopes of securing their ethnic identity and language for future generations.

Congestion was a common problem in both immigrant and black schools. Though it was more likely to affect immigrant schools prior to 1920, black schools were significantly oversubscribed by the late 1920s. Schools managed crowding by building new schools, adjusting boundaries, and employing double shifts and portable classrooms. By 1930, the disparities between black and white schools were substantial. While much of the literature is anecdotal, Homel (1984) produces some empirical facts about segregation and racial inequality in Chicago's school system. But the analysis does not paint a comprehensive picture, nor does it attempt to estimate causal relationships between schools and child outcomes. The little we know about such causal relationships for the period is from the South (Card & Krueger, 1992). The lack of similar data for the North motivates my data collection and empirical analysis.

1.2.4 Empirical Methods

While a shift-share design is a common empirical approach for studying the impact of migration (e.g., see Derenoncourt (2019); Shertzer and Walsh (2019); Tabellini (2020)), the goal of this paper is to consider the impacts of schools. Because school attendance areas have well-defined boundaries, I can utilize variation in school assignments induced by boundary changes to identify school effects. This is the approach taken by Billings et al. (2014), who investigate the impact of the minority share on students of the Charlotte-Mecklenburg schools in the early 2000s. Using addresses measured prior to a boundary change, they compare students that lived in the same neighborhood but on opposite sides of a newly drawn boundary.

Their approach is motivated by a spatial discontinuity design, where distance to the new boundary is the running variable. In practice, however, they run a regression with initial school and neighborhood fixed effects, so that only the variation induced by a boundary change is used to identify the impact of a school's minority share. This approach benefits from pooling across the many boundary change experiments in the data. However, the identified effect of minority share cannot be separately identified from other school characteristics.

In this paper, I utilize the same fixed effects approach, but measure the effect of a broad set of school characteristics. I also overcome additional empirical challenges related to the precision of household location. Ultimately, the measurement error in location is not pathological in my setting because of its compositional nature (Battistin et al., 2009).

1.3 Data

1.3.1 School Data

The Chicago Public Schools kept very detailed records prior to 1930. While I am not aware of any cache of student-level records, there is a lot of information on the schools, including enrollment, facilities, budgets and attendance boundaries. For this paper, I combine data from several archival sources. Most of the sources contain information on elementary, junior high, and high schools. I focus on elementary schools to avoid selection into schooling. At the time, most children attended at least elementary school to comply with compulsory schooling laws. Furthermore, while all public elementary schools maintained local attendance areas, there were different selection criteria for high schools. Junior high schools, to which students were assigned based on more complex rules, were not featured prominently until the mid-1920s. For these reasons, I restrict estimates of school effects to the elementary schools of the 1920 cohort. In the data section, however, I'll describe the elementary schools for the entire period from 1910 to 1930.

The two main data sources are the *Official Report of the Proceedings of the Board of Education of the City of Chicago* and two books describing school attendance boundaries for the 1912-13 and the 1928-29 school years. The *Proceedings* include a comprehensive description of attendance boundaries for the 1920-21 school year, and report any changes thereafter. All of the descriptions usually start at an intersection and then give directions along streets, rail and water ways until they connect back to where they started. In most cases, children on opposite sides of a street along a boundary would be assigned to the school on their side of the street. But in some rare cases, both sides of the street are assigned the same school. I generate shapefiles for these boundaries in GIS software by tracing the boundaries along historical street centerlines, railways, and water features (Costa & Fogel, 2015). Figure 1.1 shows two examples of boundary descriptions.

School-level budget data for 1917 onward comes from the *Proceedings.* I digitized these records for 1917, 1920, and 1928, and supplemented them with budget tables from the *Annual Reports for the Board of Education for the City of Chicago* for the years 1910 to 1913. In addition to total funds per pupil, the budgets are broken down into operations, maintenance, capital outlays, and instruction, which are further itemized into general instruction, and manual and household training. They also detail funds for special instruction, such as for the deaf and blind, or subnormal children.

While data from the *Reports* are already on a per pupil basis, I make the same adjustment for the budget data from the *Proceedings*, dividing total funding by student membership as listed in the *Directory of the Chicago Public Schools*.[3] To obtain a single funding measure for each decade, I take the average of the adjacent years (i.e., 1910-1913 for 1910, 1917 and 1920 for 1920, and 1928 for 1930). Table 1.1 provides descriptive statistics of the public schools, weighted by student population. Some measures are not available in 1910 and 1920. All funding measures are in 1920 dollars.

Figure 1.2 depicts the variation in school funding across the elementary school attendance areas for 1920. To identify school effects, I will compare students that lived on opposite sides of these boundaries in 1910, but who would have been assigned to the same school according to the 1910 boundaries.

1.3.2 Panel Links Across Decennial Census Years

To construct my sample, I link children and their families across years in the decennial census. For this task, I rely on the Multigenerational Longitudinal Panel (MLP) from IPUMS. Below, I summarize their linking procedure. More information about this procedure is provided in their working paper (Helgertz et al., 2020).

The project develops a machine-learning algorithm to match individuals in a two-step process. Step one matches male records, and step two matches records of the household

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[3] These directories contain information on schools and their associated branches, student enrollment, the number of teachers, portable classrooms, construction dates, and specifications for assembly halls and playgrounds.

members from step one. Both steps begin by selecting a larger pool of potential matches, which are then whittled down to a single match based on estimated match probabilities.

The pool of potential matches in the first step includes all male records in the second dataset that match the first dataset on place of birth, a window for birth year and name similarity. From this pool, the probability of each potential match is calculated using a logit model that was estimated on training data. Matches are assigned to the best match if its probability exceeds both an absolute and relative (next-best) threshold. The training data comprises 1,354 males in the 1900 census that were manually linked to their 1910 census record. The model was trained using a train-test-split approach to calibrate hyper-parameters governing assignment thresholds–a probability cutoff and a relative probability cutoff. The features used in the model include: name similarity, birth year difference, place of birth, immigration year, country of origin distance, similarity of other household member characteristics (e.g., name), and similarity of neighbors and street name.

In the second step, the initial pool consists of household members of the linked males from the first step. From this pool, the probability of each potential match is calculated using another logit model, estimated from a different training set. The probabilities are then used to assign matches to the best match, subject again to similar absolute and relative thresholds. The training data consist of individuals from the same households as those matched in the first step's training data. The features used to train the model include: name similarity, birth year difference, place of birth, race, sex, and relationship within household.

Compared to other linking procedures, e.g., those of Feigenbaum (2016) and Abramitzky, Boustan, Eriksson, Feigenbaum, and Pérez (2019), the step one procedure produces nearly twice as many matches. And with much greater precision. When comparing to "high quality" links documented in the Family Search database, there is only 2% disagreement in the matches produced by MLP as opposed to 13% disagreement for the method of Feigenbaum (2016). The high match rate and precision may not be a surprise given the large number of features used to estimate match probabilities. The only drawback may be that with more features, the matched sample becomes even less representative of the population. There is some evidence that suggests the MLP links over represent individuals living with parents and under represent lifetime immigrants, even when compared to the other procedures. But, encouragingly, there appears to be little difference in the rate of incorrect links between the foreign and native born. All procedures tend to over represent natives and those from higher SES households.

The additional links from the second step also have low error rates (about 2%) and suffer from similar issues regarding representation of the population, including further over representation of individuals from large families.

1.3.3 Child Sample

Given the school attendance boundaries, MLP panel links, and detailed location of census households from the Urban Transition Historical GIS (HGIS) project, I construct the sample of children I'll use to assess school effects.

I first select the subset of male children aged 8 to 12 from the 1920 decennial census whose family resided in Chicago in 1910. I focus on the 1920 elementary school cohort for several reasons. First, it is the first cohort for whom I can observe the school attendance areas prior to elementary school entry. Second, this cohort is old enough to measure outcomes by 1940. Third, junior high schools become more prominent in the mid-1920s, and their selection criteria are more complicated. And finally, sorting and manipulation of boundaries may be worse during the 1920s, when white residents took collective action to segregate their communities. I restrict to 8 to 12 years olds because they are young enough to be affected by the boundary change, but old enough to be in elementary school. Location in the 1910 census is determined by linking individual household members to their 1910 location, and then taking the first observed value in order of the child himself, then the household head, the spouse, and finally, other children in the household. The children are then linked to their outcomes in the 1940 census. Table 1.2 compares the resulting sample from the cohort of male children aged 8-12 living in Chicago at the time of the 1920 census. The sample children are less likely to be immigrants, black, and of lower socioeconomic status.

In my estimates of school effects, I weight the sample to make it comparable with the population. These weights were estimated by race-immigrant group using a random forest algorithm to classify sample individuals using the following features: head employment and socioeconomic status, head married, age of head, head literacy and English proficiency, and the child's age.

1.3.4 Linking Children to Schools

When constructing the data, the most important and challenging task was to link children to their local neighborhood schools. Each household is tied to a particular enumeration district (ED) and street. Using GIS software, I use previously constructed shapefiles of EDs and streets to map households to their neighborhood schools. In some cases, a household's street segment overlaps with more than one school.

I start by matching the street name-enumerate district pairs in the census HGIS project to those in existing shapefiles of enumeration districts and streets. I first overlay (intersect) the EDs with the street grid to associate each ED with its intersecting streets. I then use natural language processing (NLP) to match these streets with the pool of streets associated with the same ED in the census. This NLP matching procedure permits variation in spelling between the two datasets. The matches produced by this algorithm are very good because of the initial restriction of the potential set of streets to those in the same ED. The biggest reason for failing occurs when there is no street name in the census. In 1920, 85% of households are matched to a street in the shapefile.

Now that I have the associated ED-specific street segment, I can match households to schools. I first compute the length of each segment. Then I overlay these segments with the school district boundaries, and compute the length of the intersection. From these two lengths, I calculate the fraction of the segment within each school attendance area. Children are assigned to schools which contain the greatest fraction of their street segment. This assignment procedure thus introduces measurement error in school assignments whenever this fraction is less than 100%. For my sample, I plot the distribution of the fraction of the street segment contained in the assigned school in Figure 1.3. About half are fully contained in the assigned school and almost 80% are at least 75% contained in their assigned school area.

Figure 1.4 depicts a particularly problematic example of these street segments within an ED, and the boundaries of two adjacent schools. Segments that run perpendicular to the school boundary are not 100% contained in either school area. But most of the segments parallel to the boundary are fully contained in one of the school areas. As one moves closer to the boundary between schools A and B, some street segments remain fully contained in one of the school areas.

This procedure produces a particular pattern in the measurement error. Even though I don't know a household's precise location on a street segment, there are always households I can match to schools with certainty as I approach the boundary line. These households live in street segments parallel to the boundary line. I exploit this fact in my identification strategy.

After matching children and households to schools, I can determine the demographic composition of neighborhoods and produce school statistics by race and immigration status. School area demographics are computed by assigning children to schools in proportion to the fraction of their street segment lying within a school area. Figures 1.5-1.7 describe the schools attended by the average native, immigrant and black child. The 1910 data may overstate the funding in black schools if, in practice, they were segregated into different branches. By 1930, however, it is clear that black children were severely segregated from both immigrant and native peers, and their funding drops significantly relative to other children. In comparison, immigrants are increasingly living in areas with more native children, and their funding is on par with natives by 1930.

Figures 1.8-1.10 document the sorting patterns along school attendance boundaries. Immigrants tended to live close to industrial centers, while blacks are limited to the strip of land known as the black belt, with some satellite regions to the south and west of the city center.

1.3.5 Compliance to School Assignments

There were two primary ways households could send their children to a different public school. They could apply for a transfer permit or falsify their address. Homel (1984) suggests both tactics were used by white families to avoid predominantly black schools in the 1920s. It is difficult to get a sense of the magnitude of the problem, but all of the examples given in Homel (1984) are from the 1920s and apply mainly to high schools, not elementary schools.

Another concern is parochial school enrollment. The fraction of elementary school pupils in the parochial system grew from 21% in 1910 to 24.8% and 27.9% in 1920 and 1930. But this fraction was larger in immigrant, and especially Catholic immigrant and high poverty areas (Sanders, 1977). In 1915, 47% of children in the Northwest side inner city wards, which were heavily polish, attended parochial schools, in addition to 43% in the industrial river district and the stockyards. 37.7% attended Catholic schools in South Chicago where the steel mills employed many Catholic immigrants.

1.4 Methodology

To estimate school effects, I run a regression of child outcomes on a vector of assigned school characteristics with fixed effects for initial school assignment and local neighborhood. For all variables, the child's location is based on their family's 1910 location of residence. Initial school fixed effects are based on the 1910 boundaries, whereas the school characteristics are based on the 1920 data. Thus, only variation induced by a boundary change is used to identify the effect of school characteristics.

Though imprecise, it is useful to think of the regression as combining two steps: (1) identify school effects using variation along new school boundaries, and (2) project the effects onto a vector of school characteristics. For step (1), the initial school and local neighborhood fixed effects restrict comparisons to those near the boundary. The finer the local neighborhood, the smaller the bandwidth around the boundary.

To motivate the regression estimator, this section proceeds by first discussing the identification of school effects for a single boundary change in an RD framework. Then I discuss the regression approach and its connection to the RD design.

1.4.1 Identifying School Effects from Boundary Changes

In what follows, I implicitly condition on children living in the same initial school area. Distance, the running variable, will reflect the distance to the new boundary. Let Z^* and Z be the actual and observed distance to the boundary. School assignments, D, are determined by distance to the boundary such that $D \equiv \mathbb{1}\{Z^* \geq 0\}$. Finally, denote counterfactual outcomes by $Y(d)$, where $d \in \{0,1\}$. Notice that assignments are determined by actual distance, whereas observed distance Z may contain error. Observed distance can be written with the switching equation $Z = MZ^* + (1-M)\tilde{Z}$, where $\tilde{Z}$ is the mismeasured distance and M indicates whether the measurement is exact. $M = 1$ indicates the street segments running parallel to the boundary and $M = 0$ those running perpendicular to the boundary. These two groups exist even as Z^* shrinks to 0. $\tilde{Z}$ can be interpreted as the distance associated with randomly choosing a point along the street segment.

In what follows, I'll first describe how to identify the local average treatment effect (ATE), $E[Y(1) - Y(0)|Z^* = 0]$, when there is no measurement error. Identification will follow the standard approach in regression discontinuity designs, but it is useful to discuss what this assumption means for household behavior in my context. After the standard case, I'll discuss identification when only the imperfect measurement Z is observed.

When distance is measured without error, identification follows from the standard assumption of regression discontinuity designs. The following assumption says that potential outcomes are continuous in the actual running variable Z^*.

Assumption RD1.

1. *Expectations of potential outcomes* $E[Y(0)|Z^* = z]$ *and* $E[Y(1)|Z^* = z]$ *are continuous in* z.

Under Assumption RD1, if $Z = Z^*$, we could identify the average treatment effect at the boundary $Z^* = 0$. The assumption that potential outcomes are continuous suggests that we can compare children born on either side of the boundary to identify the effect of school assignment. Note the contrast with a design that uses existing rather than future boundaries. Households have much better information on existing boundaries, and can sort accordingly, so that while certain neighborhood characteristics are continuous at the boundary (e.g., distance to work, libraries), household characteristics would likely change discontinuously.

Assumption RD1 is violated if households sort in expectation of the new boundary or if the boundaries are manipulated in a way that induces differences in unobservables around the boundary. If boundaries are set by a centralized school board, unswayed by local interests, it is unlikely that households even know about an upcoming boundary change, let alone anticipate the precise location.

Given the historical context, manipulation of the boundary poses a greater concern. While I am unable to find a detailed discussion of boundary changes between 1910 and 1920, Homel (1984) discusses the 1920s and 30s, before briefly remarking that racial gerrymandering was unlikely before 1915. Because he focuses on racial gerrymandering, thought to be the most extreme form of manipulation, it can serve as an upper bound for the degree of local influence on the drawing of new boundaries for the period 1910-20.

During this period, new boundaries were drawn "based on pupil's travel distance to class, capacity of school buildings, and location of barriers and safety hazards like parks, railroads, and busy streets." Homel (1984) further states that "since changes in attendance districts might evoke protests from affected parents, authorities generally preferred to retain existing boundaries as long as possible." In his discussion of elementary schools, he contrasts the border of the black belt with poorer immigrants to the west and more affluent native white families to the east. The districts on the western side tended to be more integrated than those on the eastern side, presumably because of the greater political influence of those on the affluent side. In one example, the boundaries of the Oakland school on the eastern edge of the black belt were redrawn along the new racial boundary, and the school changed from 26% black in 1920 to 98% black in 1930. In terms of the high schools, Willard elementary, which transitioned from all white to black in the 1920s, continued to send its students to mostly white Hyde Park High as late as 1935.

These examples show that the local population could exert influence on the location of boundaries, but that only the more affluent households held this privilege. Further, when boundaries did change to promote segregation, the reaction to demographic changes was often delayed. Additionally, race was only one of the many factors that went into boundary decisions. Together, this evidence suggests that for the period 1910 to 1920, while the potential for manipulation exists, it is probably not as severe as it was in later years, and exerting influence over the precise timing and location of boundary changes was difficult. In either case, I assess whether observed characteristics differ along the new boundaries when I estimate school effects.

Next, I discuss identification under measurement error in the running variable. In practice, the observed distance to the boundary Z is measured with error. However, I observe a subsample where $Z = Z^*$. Under the following assumption, I can directly condition on the correctly measured sample, $M = 1$, to identify the local ATE.

Assumption RD2. $(M, Z) \perp Y | Z^*$

Assumption RD2 says that conditional on actual distance to the boundary, the measurement error is not associated with outcomes. Under RD2, $E[Y|M = 1, Z = z] = E[Y|Z^* = z]$, so that, in addition to RD1, $E[Y(1) - Y(0)|Z^* = 0] = E[Y|M = 1, Z = 0^+] - E[Y|M =$

$1, Z = 0^-]$.

Though we can directly condition on the $M = 1$ subsample, it is worth understanding how mismeasurement introduces bias into the naive difference in means around the boundary. Several papers discuss the implications of measurement error in the running variable on the validity of regression discontinuity designs (Battistin et al., 2009; Davezies & Le Barbanchon, 2017). If measurement error is continuous, then there is no discontinuity in assignment at the boundary, and identification fails. On the other hand, if measurement error is compositional, i.e., only a fraction of observations are measured with error, then the discontinuity persists, and identification is possible. I formally assess the implications of this measurement error in my setting. First, by the law of iterated expectations, write

$$\begin{aligned} E[Y|Z = z] = {} & E[Y|Z^* = z, M = 1]E[M|Z = z] \\ & + (1 - E[M|Z = z]) \int_{-\infty}^{\infty} E[Y|Z = z, Z^* = \tau, M = 0] f_{Z^*|Z,M}(\tau|z, 0) d\tau \end{aligned} \tag{1.1}$$

which can be further simplified with Assumption RD2 and the following new assumption.

Assumption RD3. *$E[M|Z = z]$ and $\forall\tau$, $f_{Z^*|Z,M}(\tau|0, z)$ continuous at $z = 0$*

Under Assumption RD2, Equation (1.2) becomes

$$\begin{aligned} E[Y|Z = z] = {} & E[Y|Z^* = z]E[M|Z = z] \\ & + (1 - E[M|Z = z]) \int_{-\infty}^{\infty} E[Y|Z^* = \tau] f_{Z^*|Z,M}(\tau|z, 0) d\tau. \end{aligned} \tag{1.2}$$

Then, by Assumption RD3, we can subtract the left and right limits of $E[Y|Z = z]$ around $z = 0$, to obtain

$$\begin{aligned} E[Y|Z = 0^+] - E[Y|Z = 0^-] & = (E[Y|Z^* = 0^+] - E[Y|Z^* = 0^-])E[M|Z = 0] \\ & = E[Y(1) - Y(0)|Z^* = 0]E[M|Z = 0] \\ & = E[Y(1) - Y(0)|Z^* = 0](1 - E[M = 0|Z = 0]), \end{aligned} \tag{1.3}$$

where the last line follows from Assumption RD1. Assumption RD3 implies that the measurement error looks similar just to the right and left of the boundary. It ensures that the integral components of the left and right limits are equal and cancel out in the difference. It also ensures that I can factor out the term $E[M|Z = 0]$.

This analysis shows that the treatment effect of school assignment is attenuated by the probability of mismeasurement in the running variable. Because restricting the analysis to the $M = 1$ group can considerably reduce sample size, Equation 1.3 suggests that I can include observations for which I am not 100% certain of the new school assignment. Because I observe M, I could adjust the mean difference by dividing by $E[M|Z = 0]$. But because I end up pooling across boundary changes in a fixed effects regression, I end up just restricting the sample. By setting a threshold for the fraction of one's street segment contained in the new school area, I can trade-off variance for attenuation bias. Lowering the threshold increases the sample size, but introduces more measurement error. In the Appendix, Section A.2, I show explicitly how error in school assignments associated with my assignment rule–based on the overlap between street segments and school areas–causes attenuation bias proportional to the fraction of the street segment in the alternative school area.

Figure 1.11 depicts how my research design works in practice. In particular, it shows a boundary change induced by a new school. Panel (a) shows the original 1910 school area (blue dotted). Panel (b) adds the 1920 boundaries (solid line), after a new school (in red) is introduced. Panel (c) adds EDs (blue shaded) around the new boundary between the school, and Panel (d) shows the street grid (green lines) within these EDs. My identification strategy compares children within the same ED and initial school area, but who live on opposite sides of the new 1920 boundary drawn down the middle. Looking at the ED second from the bottom, the streets running parallel to the new boundary are 100% contained in one of the school areas. My design compares children from households that lived on streets just to the left and right of the boundary.

1.4.2 Boundary Change Regression

The RD design of Section 1.4.1 motivates a regression that uses only variation induced by boundary changes to identify the effects of school characteristics. Suppose that outcomes are determined by the following equation:

$$Y_i = \beta_0' SQ_{s_1(i)} + \beta_1' X_i + \eta_{j_0(i)s_0(i)} + \epsilon_i, \tag{1.4}$$

where subscripts denote the child i, the neighborhood j_0 and school s_0 associated with the household's initial (1910) location, and the 1920 school s_1 associated with the household's initial location. Y is the outcome, SQ is a vector of observed and unobserved school attributes, X is a vector of observed individual and family characteristics, η is a fixed effect, and ϵ other unobserved determinants of Y. Both ϵ and η may be correlated with SQ. Choosing the geographic unit for the "local neighborhood" is analogous to choosing a particular bandwidth around a school boundary. Smaller units of geography tighten the bandwidth. In practice, I'll use the enumeration district.

In Section 1.4.1, I discussed identification of school effects for a single pair of schools along a new boundary. Under the specific parametric form of Equation (1.4), this would translate to identifying $\beta_0'(E[SQ|D = 1] - E[SQ|D = 0])$ for a particular treatment and control comparison. The fundamental identification challenge was to separate the school effect from differences in unobserved individual characteristics and local neighborhoods. Section 1.4.1 discusses how to utilize boundary changes to identify the effect of school assignment along the boundary. In practice, I do this by including a local neighborhood and 1910 school fixed effect $\eta_{j_0 s_0}$ as in Equation (1.4). Thus, only variation induced by a boundary change will be used to identify the impacts of schools. I assume that by using only this variation, I break any dependence between ϵ and SQ.

The next challenge is to identify components of β_0. There are many boundary changes in the data, so that I can estimate many school effects and associate this variation to differences in school attributes. However, without exogenous variation in these attributes, I cannot separately identify the role of observed and unobserved attributes.

For ease of discussion, however, I make the assumption that the observed attributes are uncorrelated with the unobserved attributes, so that I can identify the associated components of β_0, which I'll simply refer to as β_0. I focus on historically important school attributes–racial, immigrant and socioeconomic composition and school funding per pupil. If the assumption of uncorrelated school attributes fails, β_0 represents their true causal effect on Y in addition to the effect of correlated unobserved school attributes.

1.5 Results

1.5.1 Estimation Details

I estimate models with initial ED-school fixed effects, as in Equation (1.4). Only variation in school quality within the intersection of a 1910 ED and school area is used to identify β_0. Such variation only occurs if a new boundary line bisects this region. For the immigrant subsample, there are 72 boundary regions, containing 1,183 children. The numbers for the native subsample are 47 and 436.

In some specifications, I include additional controls for individual and family characteristics. To account for the attenuation bias caused by imprecise location, I restrict the sample to those children whose street segment is more than 75% contained in the assigned school area.[4] I chose this cutoff as a trade-off between attenuation bias and sample size, and show that my results are robust to small changes in this threshold. Additionally, I exclude children who live on streets directly on school boundary lines, and children assigned to schools with a significant amount of funding for special programs (e.g., for the deaf or blind).

Estimation is conducted separately for natives and immigrants, using weights to match these samples to their counterparts in the population.

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[4] I use small buffers around streets when matching to school areas, so 75% is a lower bound. Additionally, for the initial school area, I ensure that at least 85% of the street segment is in the school area.

School quality measures include the log of funds for instruction per pupil, percent of school area children that are black and white immigrant, and the median socioeconomic indicator of households.

Outcomes come from the 1940 census and include years of education, high school graduate, employment, weeks worked, hours per week, whether business income is greater than $50, the log of weekly wages, and occupation score. Weekly wages excludes the unemployed and self employed, while occupation score excludes only the unemployed. Because I do not account for selection, I relegate the results on wages to the appendix.

Controls include individual/family controls, and age by ward fixed effects. Individual controls include whether the child's household head was employed, their marital status, sex, age and birthplace.

In Section 1.5.5, I conduct robustness checks. These include: different threshold values for school assignments, and unweighted estimates. Additionally, I obtain estimates on a subsample of immigrants from non-Catholic sending countries as a way to check if my results are affected by parochial school enrollment.

1.5.2 Balance Checks

I use variation in school assignment induced by newly drawn boundaries in hopes that households are less likely to sort along these boundaries prior to the change. However, school officials may still draw the new boundaries to reflect pre-existing differences between households.

While I cannot directly assess whether there are differences in unobservables along the new boundaries, I can check for differences in observables. I do so by estimating fixed effects regressions of school attributes on individual characteristics. The fixed effect is the same one used in Equation (1.4). The coefficients from this regression test whether individual characteristics are associated with differences in the school attribute induced by a boundary change.

Tables 1.3 and 1.4 show the results of this regression on the native and immigrant subsamples. At the 10% level, I fail to reject the joint null hypothesis of zero coefficients for natives for every measure of school quality. However, for immigrants, I reject this hypothesis for the school area's SEI, which appears to be slightly correlated with the head's occupation and employment. But overall, there appears to be little association between individual and school characteristics.

1.5.3 First Stage Estimates

I estimate school effects using the school the child would have attended based on their family's 1910 location (pre-change), which is around the time they were born. I can also assign them to schools based on their 1920 location (post-change), when they are aged 8 to 12. If the household doesn't move, both the pre-change and post-change schools will be the same. Using the same fixed effects specification used for the outcomes, I regress the post-change school characteristics on the pre-change characteristics as an analog to a first stage regression. This regression checks whether variation in school quality induced by a new boundary is associated with post-change school characteristics.

Tables 1.5 and 1.6 show the estimates from this regression for natives and immigrants. The coefficients all have the expected sign, but the associations are somewhat weak. Because the pre and post-change schools only change if households move, I can partly explain these estimates by considering moving patterns.

Figure 1.12 breaks down the moving patterns in my sample. Around a third of the sample stays in the same school area between census years, while another 60% move within Chicago. Fewer than 10% move elsewhere in Illinois or to other states. Surprisingly, the fraction of movers does not increase when households are affected by a boundary change. However, if this boundary change would increase the black and immigrant share in the school area, more households move. With no change, 68% of natives move compared to 61% of immigrants. Movers increase to 81% and 75% when the change increases the share of immigrants at the school. When the black share increases, 73% and 66% of natives and immigrants move. Additionally, when school funding increases, both groups are actually more likely to move, jumping by 3 percentage points for natives and 5 percentage points for immigrants.

This evidence suggests we should be careful when interpreting the effects of pre-change school assignment. Many households move in response to changes in the local school, especially demographic changes. Thus, the effect of a greater black share in the pre-change school may have less to do with actual exposure to black peers as it does how children and households react to being assigned to a school with a greater share of black pupils.

There are some drawbacks to the first stage regression described above. Without knowing the precise timing of moves, I cannot form an accurate measure of exposure. If households are moving in response to demographic transition, their children may get some exposure to the pre-change school before a move. A more accurate measure of exposure would probably be more correlated with the pre-change school characteristics.

Given the relatively weak first stage, and the large number of moves between census years, I estimate the effect of school assignment, i.e., the reduced form. Even when instruments are weak, the reduced form is capable of testing for effects in the second stage (Chernozhukov & Hansen, 2008).

1.5.4 Main Results

I now turn to the main regression estimates. Figures 1.13-1.18 show school effects from standardized regressions, so that units are in standard deviations. In the Appendix, Tables A.1-A.8 provide the same effects, but from unstandardized regressions, in addition to comparisons across specifications with and without controls. The units for the share variables in the tables are in percentage points. The significance and magnitude of results are robust to excluding controls unless stated otherwise. All standard errors are clustered at the 1910 school area level.

Education School funding increases native educational attainment, while greater immigrant and black shares reduce it. A 1% increase in school funding reduces years of education by 0.05 years, and high school graduation by 1 percentage point. A 1 percentage point increase in the black (immigrant) share reduces years of education by .1 (.08) years, though the latter is not statistically significant. For high school graduation, the increase is around 2.5 (2) percentage points.

Immigrants see a decline in their educational attainment with an increase in the black share, and in the Appendix, Section A.1.5, I show this extends to the immigrant share on the subsample of non-Catholic immigrants. A 1 percentage point increase in the black share reduces high school graduation by 2.4 percentage points. On the non-Catholic subsample, the black (immigrant) share reduces years of education by 0.12 (.09) years and high school graduation by 2.8 (1.4) percentage points.

The impacts of the immigrant and black shares appear similar, indicating that increases to the native share would increase education for both natives and immigrants. This could suggest that interactions with Americanized peers in elementary school increases the productivity of additional education.

Employment School funding appears to increase native employment at the extensive margin, despite decreasing their educational attainment. A 1% increase in funding per student increases employment by about 0.35 percentage points.

I also find some evidence for the immigrant share increasing self-employment among immigrants. A 1 percentage point increase in the share is associated with a 1 percentage point increase in the self-employment rate.

Wages and Occupation I do not find many consistent effects on wages and occupation. For immigrants, I find some evidence that funding (black share) decreased (increased) wages, but I don't account for selection into employment and wage work. For this reason, I will not emphasize these findings.

1.5.5 Robustness Checks

Thresholds for School Assignments Recall that each child is given a number that represents the fraction of his street segment within a particular school area. In the main specification, I use 0.75 as the cutoff. When I test a higher threshold of 0.80 and a lower threshold of 0.70, the results are very similar.

Sample Weights The main specification weights the sample to match the population of immigrants and natives in Chicago. The estimates are very similar when I exclude weights from the analysis.

1.5.6 Discussion

Natives assigned to better funded elementary schools were more likely to be employed, but acquired fewer years of education. This fact would be consistent with a human capital model where better elementary education increases the opportunity costs of additional schooling relative to its benefits. On the other hand, both immigrants and natives assigned to schools with more native children tended to increase their education. A higher native share in elementary school may have improved social interactions, increasing skills that complemented further education.[5] With data on employment and income in subsequent years, future work could determine whether this increased educational attainment improved labor market outcomes.

Interpreting the Evidence: An Economic Model of Education Choice

My key empirical results imply heterogeneous responses to elementary school improvements between natives and immigrants. While more funding decreases native educational attainment, it has no effect on immigrants. In contrast, both immigrants and natives tend to

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[5] An alternative explanation considers the implications of the research design, namely, using school assignment as the treatment of interest. It could be that families assigned to schools with greater immigrant or black shares chose to move and/or leave school earlier to avoid poor social interactions.

increase years of education when they attend elementary school with more native peers.

In the Appendix, Section A.3, I develop an economic model of education choice to help explain the heterogeneity in my results. I'll summarize the key mechanisms here. Human capital is accumulated in elementary school, after which the child chooses to work or attend high school, where human capital can be augmented further. Thereafter, all children work. Human capital is fixed after leaving school. Social skills are produced from interactions with one's peers, and cognitive skills from school work. That is, I make the assumption that cognitive skills and school quality (e.g., funding) are excluded from the production of social skills, while the average social skills of one's peers are excluded from the production of cognitive skills. Importantly, a child's social skills will complement the production of cognitive skills. The distinction between the two skills is natural to the extent that schools had a more direct effect on learning through instruction and a separate, assimilating effect through social interactions.

The key idea of the model will be to show that for children with high initial social skills (natives), increasing elementary school funding will increase the opportunity costs of high school more than the benefits. On the other hand, improving social interactions (increasing average social skills at elementary school) increases the benefits of high school more than the opportunity cost by raising complementary social skills in elementary school, regardless of initial skill level.

Model parameters are calibrated to replicate the key empirical results of the paper, namely: higher rates of high school attendance among natives, a decrease in high school attendance among natives with increases in funding, and an increase in high school for both immigrants and natives with increases in the native share. Immigrants and natives differ only in their initial social skills and in the average social skill of their peers. These differences reflect early deficits in language and/or familiarity with American culture among immigrant children. Differences in initial social skills, together with complementarities in the production of cognitive skills, can produce heterogeneous responses to elementary school funding between natives and immigrants. If social skills are strong complements to funding in elementary cognitive development, more funding can increase the opportunity costs–relative to the benefits–of further education for those with higher initial social skills. On the other hand, improving one's elementary school peers enhances future social skills, which boosts the productivity of both high school cognitive and social development. This boost in productivity can be greater than the increase in opportunity costs, regardless of initial social skill.

Counterfactuals and the Origins of Black-White Inequality

I find evidence consistent with the hypothesis that in the early 20th century, immigrant children benefited from social interactions with native peers in the public schools. As a result, segregation might have reduced the upward mobility of immigrants, which could partly explain the relatively poorer outcomes of newly arriving black immigrants from the South.

Though I cannot directly assess the impact of schools on black children, I can use my estimates to study what would have happened to immigrants had they been assigned to schools similar to the average black child. To form the counterfactuals, I use the coefficients associated with the non-Catholic immigrant subsample. Figure 1.19, Panel (a) compares high school graduation rates of 2nd generation (white) immigrants aged 25-35 with their black peers in 1940 Chicago. The percentage term gives the ratio of the yellow to the blue bar, which is the associated figure for natives. The dotted line is the actual figure for immigrants, while the yellow bar is the counterfactual after changing the student composition of their assigned schools to make them 10% more similar to the average black school. Though in 1920 the Chicago public schools are not nearly as segregated as in 1930, this experiment has immense consequences for immigrant educational attainment, reducing high school graduation rates from about 35% to 31%. Panel (b) shows the result of a similar exercise, but on total years of education. The high school effect is so large because black schools have a much greater black share, and the much larger (in magnitude) negative coefficient on the black share produces a large, net-negative effect, despite the concomitant drop in the immigrant share. Because there is less disparity between the two coefficients on the share variables for years of education, the associated counterfactual difference is much smaller.

While these counterfactual exercises probably extrapolate too far outside the support in the data, they do give a sense of the magnitude of the effects of the native share on immigrants. And it suggests that the relatively greater segregation experienced by black immigrants might have contributed to initial disparities that would presage nearly a century of racial inequality in Northern cities.

1.6 Conclusion

I find evidence that the public schools helped assimilate early 20th century immigrants to Chicago. Both school funding and the demographic composition of school areas affected key labor market and education outcomes.

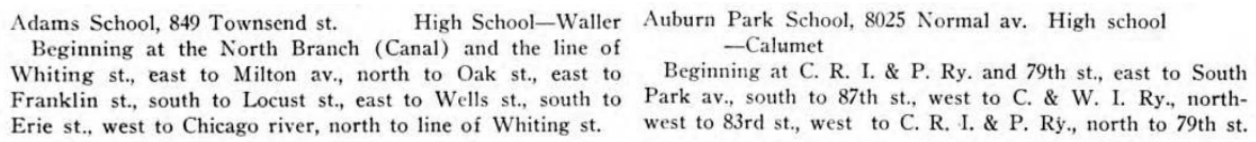
To produce my estimates, I overcome several empirical and econometric challenges. By digitizing records from historical archives of the Chicago Board of Education, I link children to the characteristics of their local public schools, providing the first look inside an urban school system during this crucial period in history. Geocoded school boundaries were critical to this analysis. They form the backbone of my econometric strategy, which accounts for sorting by exploiting changes to school boundaries. I identify school effects by comparing children born in the same school area, but who would be on opposite sides of a newly drawn boundary when they enter elementary school. And while the data introduce measurement error in location, I show how my empirical approach avoids the pathological effects of this error in discontinuity designs.

I find that school funding improved native employment rates, but reduced their educational attainment. The native share, on the other hand, improved the educational attainment of both immigrants and natives. I show that these effects can be explained by a simple model of human capital accumulation and education choice, where agents trade off the opportunity costs and benefits of further education. The model attributes the different responses to school characteristics between natives and immigrants to initial differences in social skills.

In counterfactual exercises, I show how immigrants would have fared had they been assigned to schools with characteristics similar to those of black children. The educational attainment of immigrants would fall substantially below their black peers had they been assigned to schools with a more similar demographic makeup. Thus, while I only estimate school effects for immigrants and natives, my results nonetheless speak to how initial differences in migrant experiences might have contributed to the racial inequality that would persist for generations.

Future work could expand on and address several shortcomings of this paper. In particular, a more rigorous study of sorting and moving patterns could shed light on the mechanisms of school assignment. For instance, the decrease in educational attainment observed when assigned to schools with greater black or immigrant shares may result from a disruptive move rather peer effects. Additionally, a non-negligible share of immigrants attended catholic schools. Part of the mechanism of school assignment may operate through the margin of public-parochial school choice. This choice is critical to our understanding of immigrant assimilation, as many immigrant families sought to retain their language and cultural identity through the parochial system. In fact, I find that impacts on non-Catholic immigrants, who would be less likely to attend parochial schools, appear to be greater. Future research into other aspects of the schools, such as manual training and crowding, might also be valuable, given their historical significance. Finally, my results point to a potential trade-off between future schooling and its associated opportunity costs, and how peer composition and school funding might operate on these margins. While I do explain my results within the framework of an economic model of education choice, I do not attempt to prove identification of the model. Future work could improve this model and produce more formal arguments of identification.

Figure 1.1: School Boundary Descriptions



Adams School, 849 Townsend st. High School—Waller

Beginning at the North Branch (Canal) and the line of Whiting st., east to Milton av., north to Oak st., east to Franklin st., south to Locust st., east to Wells st., south to Erie st., west to Chicago river, north to line of Whiting st.

Auburn Park School, 8025 Normal av. High school —Calumet

Beginning at C. R. I. & P. Ry. and 79th st., east to South Park av., south to 87th st., west to C. & W. I. Ry., northwest to 83rd st., west to C. R. I. & P. Ry., north to 79th st.

Figure 1.2: Instruction Funds per Pupil Across 1920 Elementary School Attendance Areas in Chicago

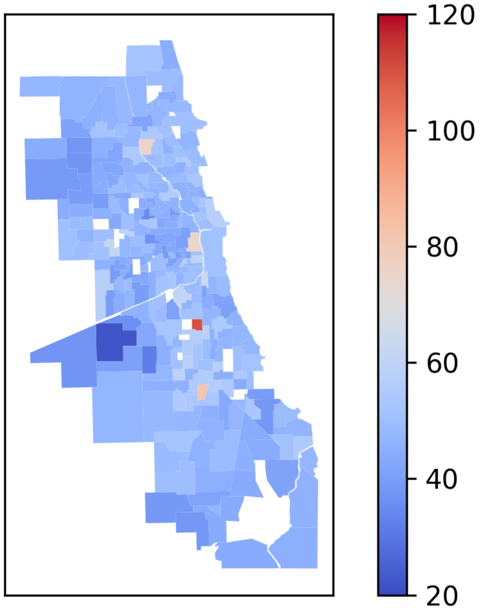


Table 1.1: School Funding and Pupil-teacher Ratio

	1910	1920	1930
Total Funds	72.46	56.56	108.02
Total Instruction Funds	59.89	47.35	82.21
General Instruction Funds		37.28	59.32
Operating Funds	11.15	9.21	17.88
Manual Training Funds	0.48	0.82	1.83
Household Training Funds		0.82	1.95
Special Program Funds		0.95	1.47
Subnormal Funds		0.79	1.44
Capital Funds			1.59
Maintenance Funds	3.69		7.81
Pupil-Teacher Ratio	40.55	40.97	42.51

Table 1.2: Descriptive Statistics

Child: 1920 Census

	Population	Sample
Nat. White	0.29	0.34
Imm. White	0.69	0.65
Black	0.02	0
So. Black	0.01	0
Age	9.97	10.08
Head Married	0.92	0.94
Head SEI	30.09	33.37
Head Occ. Score	27.33	28.66
Head Employed	0.94	0.95
N	115,818	11,699

Adult: 1940 Census

	Sample
HS Grad	0.44
Works	0.92
SEI	39.8
Occ. Score	28.99
Wage Inc.	1285.46
Own HH	0.46
Chicago	0.87
N	11,699

Note: Sample includes children of households living in Chicago in 1910, who were aged 8-12 in the 1920 census, and can be linked to the 1940 census. Additionally, the sample includes additional restrictions: 1910 street segments > 75% contained in school area, not at special school, and not directly on school boundary line. Population includes the whole population of Chicago aged 8-12 in 1920.

Figure 1.3: Fraction of Street Segment Contained in Assigned School Area

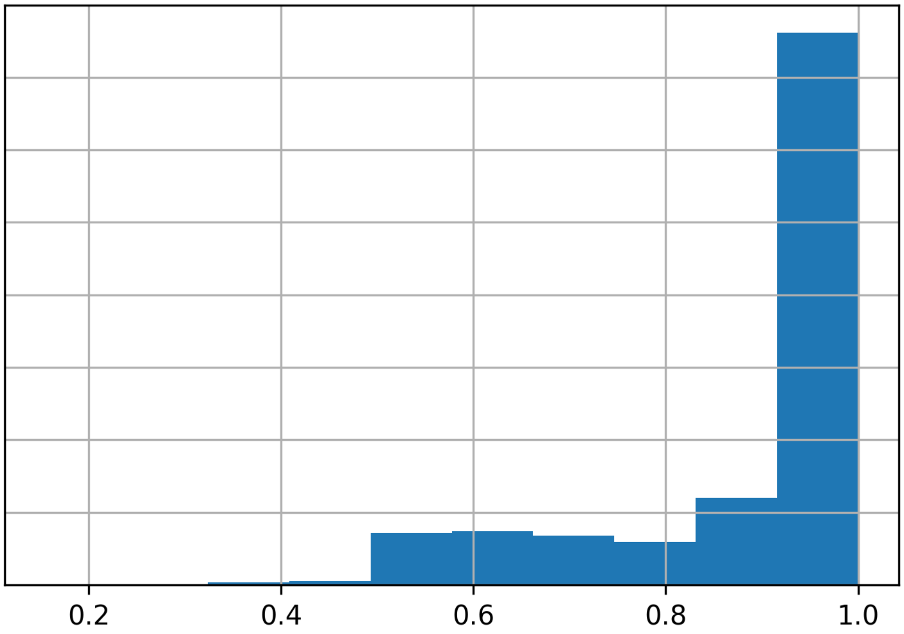


Figure 1.4: Illustration of Street Segments Overlaid with Enumeration District and School Attendance Areas

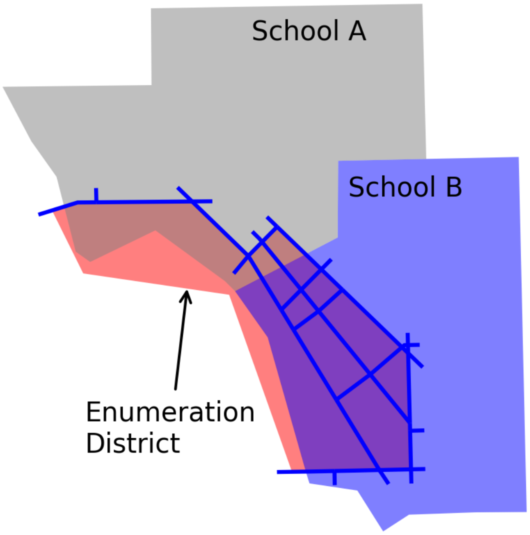


Figure 1.5: Instruction Funds per Pupil for Average Student

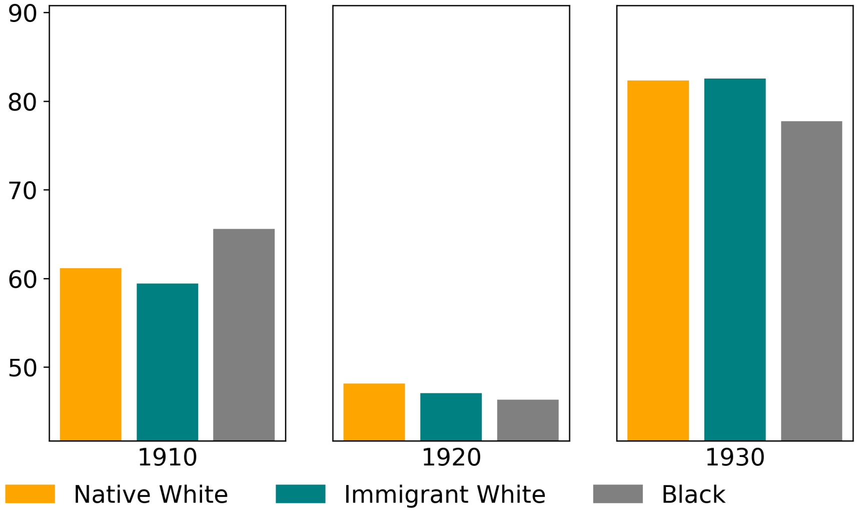


Figure 1.6: Pct. Native White for Average Student

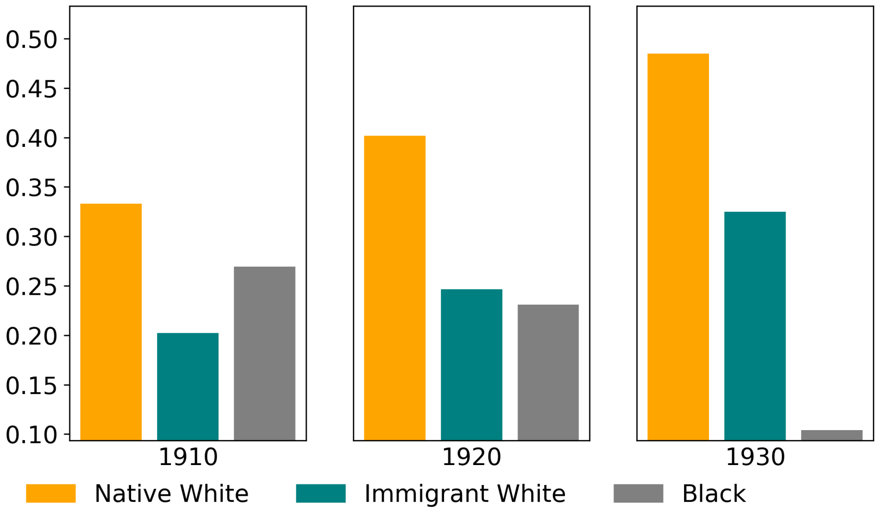


Figure 1.7: Pct. Black for Average Student

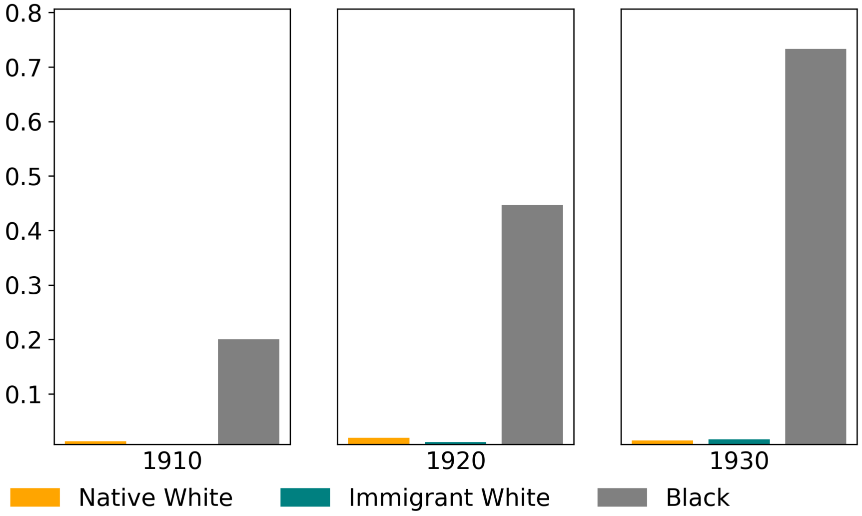
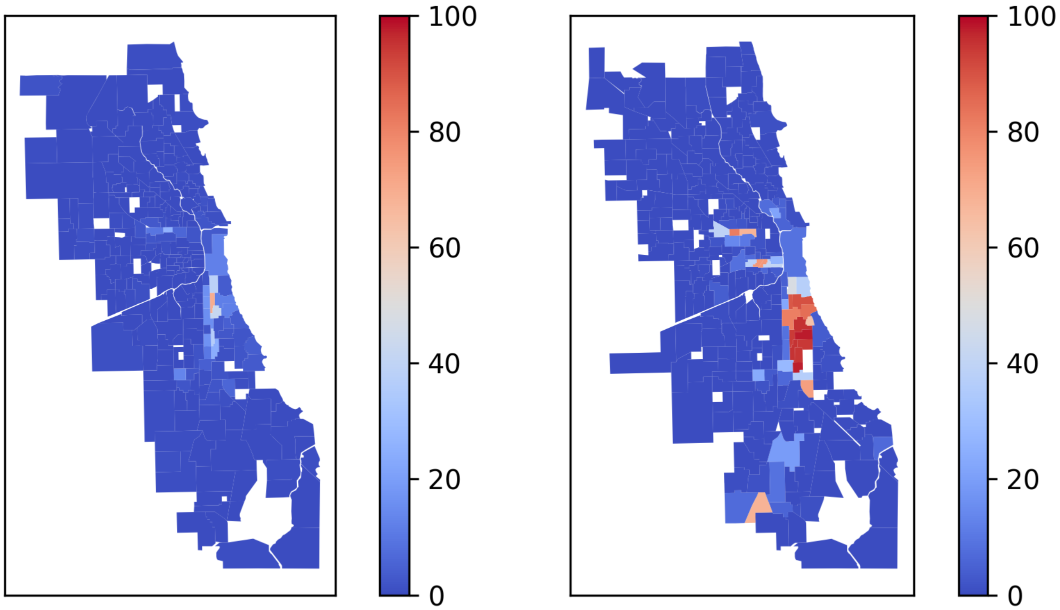


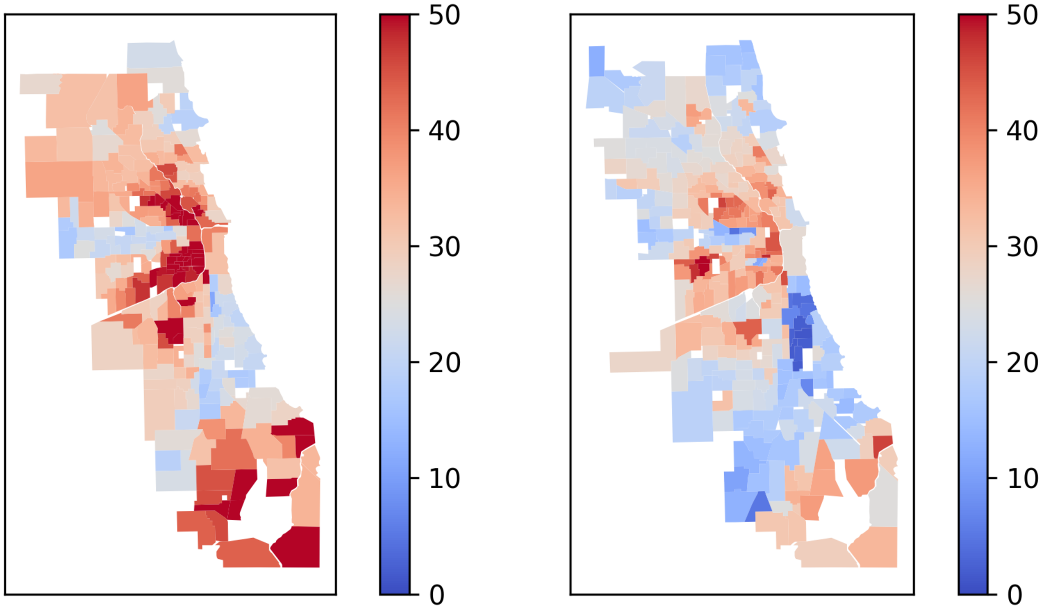
Figure 1.8: Sorting Patterns in Elementary School Districts: Percent Black



(a) 1910

(b) 1930

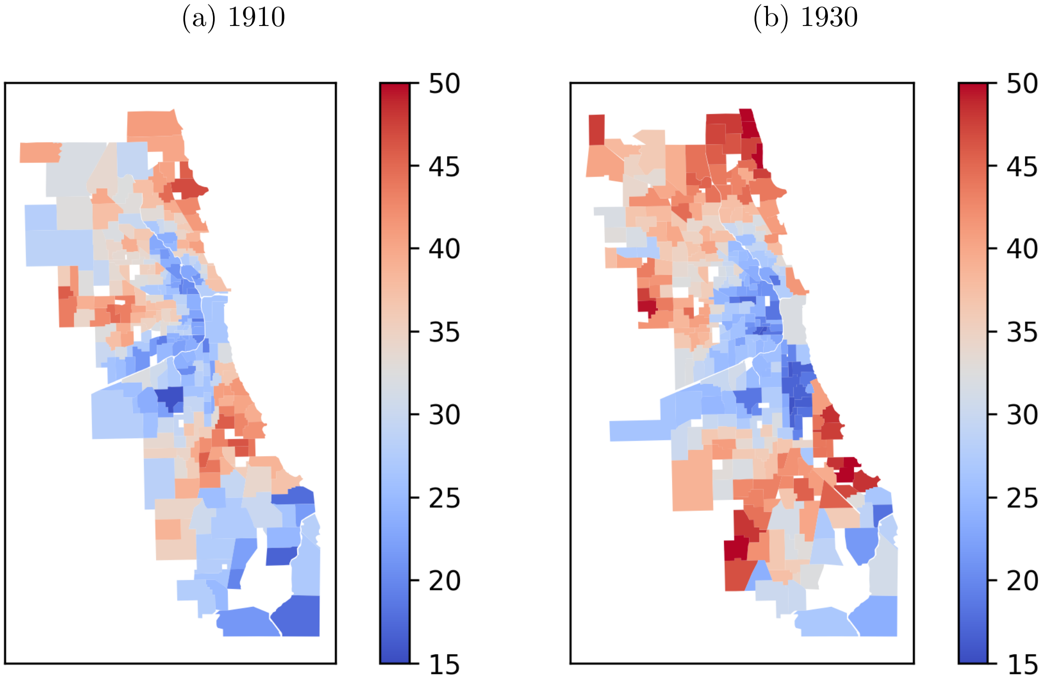
Figure 1.9: Sorting Patterns in Elementary School Districts: Percent Foreign Immigrant



(a) 1910

(b) 1930

Figure 1.10: Sorting Patterns in Elementary School Districts: Mean SEI



(a) 1910 (b) 1930

Table 1.3: Balance Check for Immigrants

	log(School fund)	Pct. Black	Pct. Imm. White	Median SEI
H. Employed	0.005	0.070	−0.168	−0.222
	(0.003)	(0.060)	(0.290)	(0.372)
H. Employed in No Cat.	−0.001	−0.014	-0.124^{*}	0.092^{**}
	(0.001)	(0.032)	(0.072)	(0.036)
H. Employed in Cat.×H. SEI	−0.000	−0.001	−0.001	0.002^{*}
	(0.000)	(0.001)	(0.002)	(0.001)
H. Married	0.002	−0.027	−0.145	0.051
	(0.001)	(0.026)	(0.118)	(0.072)
H. Male	−0.003	−0.042	0.561	−0.257
	(0.002)	(0.045)	(0.418)	(0.207)
H. Age	0.000	−0.001	0.003	−0.001
	(0.000)	(0.001)	(0.003)	(0.002)
R^2 (proj)	0.001	0.001	0.004	0.005
F-stat P-value (all=0)	0.480	0.608	0.104	0.056
Num. obs.	7197	7197	7197	7197

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates regress the treatment variable (see column label) on individual characteristics and fixed effects from boundary change design.

Table 1.4: Balance Check for Natives

	log(School fund)	Pct. Black	Pct. Imm. White	Median SEI
H. Employed	−0.001	0.065	−0.087	−0.052
	(0.001)	(0.150)	(0.175)	(0.078)
H. Employed in No Cat.	0.000	0.021	−0.046	0.033
	(0.002)	(0.062)	(0.071)	(0.037)
H. Employed in Cat.×H. SEI	0.000	−0.000	0.000	0.000
	(0.000)	(0.001)	(0.001)	(0.001)
H. Married	−0.002	0.183	−0.004	−0.138
	(0.001)	(0.153)	(0.148)	(0.106)
H. Male	0.001	−0.129	−0.038	0.097
	(0.001)	(0.139)	(0.126)	(0.094)
H. Age	0.000	−0.002	0.003	0.001
	(0.000)	(0.003)	(0.003)	(0.001)
R^2 (proj)	0.001	0.000	0.001	0.001
F-stat P-value (all=0)	0.342	0.343	0.423	0.562
Num. obs.	3688	3688	3688	3688

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates regress the treatment variable (see column label) on individual characteristics and fixed effects from boundary change design.

Table 1.5: First Stage: Immigrants

	log(School fund)	Pct. Black	Pct. Imm. White	Median SEI
log(School fund)	0.356^{***}	3.728	−18.109	1.899
	(0.122)	(4.664)	(14.463)	(8.724)
Pct. Black	0.002	0.169^{**}	0.388	−0.272
	(0.002)	(0.079)	(0.252)	(0.184)
Pct. Imm. White	0.003^{*}	0.043	0.558^{*}	−0.299
	(0.002)	(0.068)	(0.310)	(0.190)
Median SEI	−0.000	0.115	0.141	0.039
	(0.003)	(0.114)	(0.398)	(0.269)
R^2 (proj)	0.004	0.001	0.004	0.003
F-stat (proj)	3.167	1.947	1.154	1.034
Num. obs.	7584	7584	7584	7584

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level.

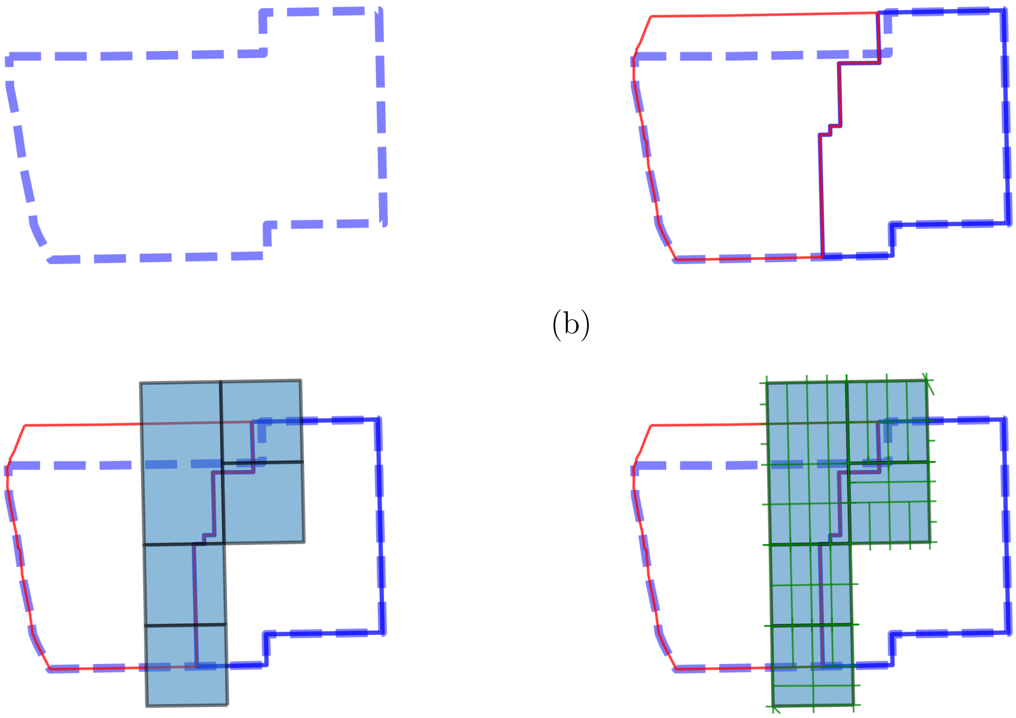
Table 1.6: First Stage: Natives

	log(School fund)	Pct. Black	Pct. Imm. White	Median SEI
log(School fund)	0.103	5.677	-1.687	2.050
	(0.108)	(4.227)	(9.542)	(6.221)
Pct. Black	-0.007^{**}	0.032	0.152	-0.104
	(0.003)	(0.129)	(0.153)	(0.150)
Pct. Imm. White	-0.003^{**}	-0.055	0.335^{***}	-0.078
	(0.001)	(0.056)	(0.121)	(0.060)
Median SEI	-0.007^{*}	-0.056	-0.209	0.122
	(0.004)	(0.187)	(0.297)	(0.244)
R^2 (proj)	0.004	0.001	0.002	0.001
F-stat (proj)	2.440	1.369	2.772	2.171
Num. obs.	3884	3884	3884	3884

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level.

Figure 1.11: Example of Research Design



(a)

(b)

(c)

(d)

Note: Panel (a) shows the original 1910 school area (blue dotted). Panel (b) adds the 1920 boundaries (solid line), after a new school (in red) is introduced. Panel (c) adds enumeration districts (blue shaded) around the new boundary between the school, and Panel (d) shows the street grid (green lines) within these enumeration districts. My identification strategy compares children within the same enumeration district and initial school area, but who live on opposite sides of the new 1920 boundary drawn down the middle.

Figure 1.12: Moves out of Pre-change School Area

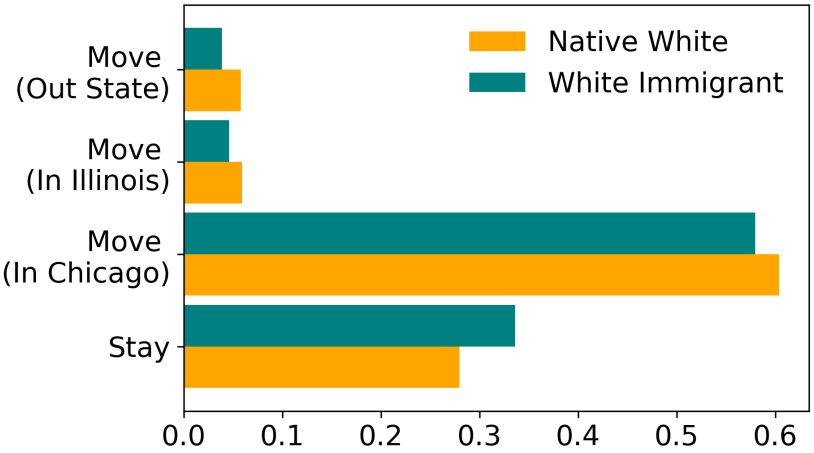
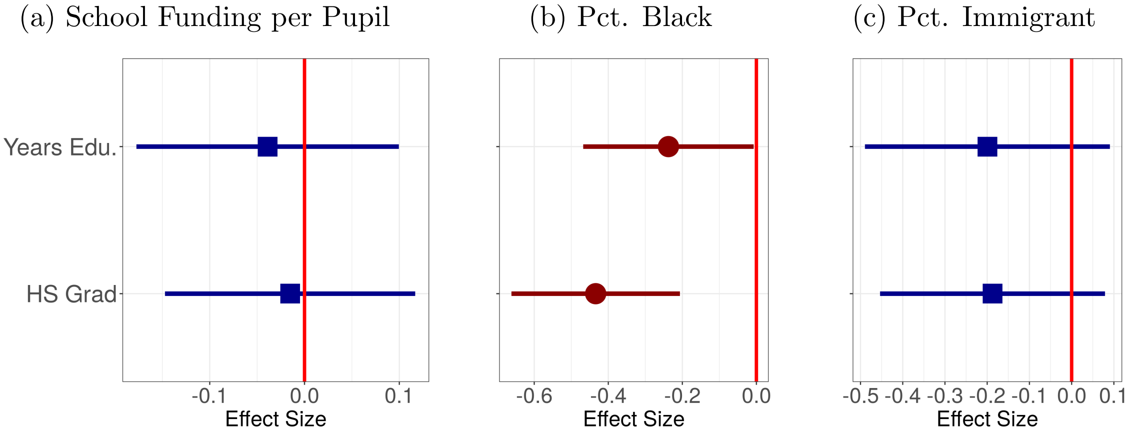
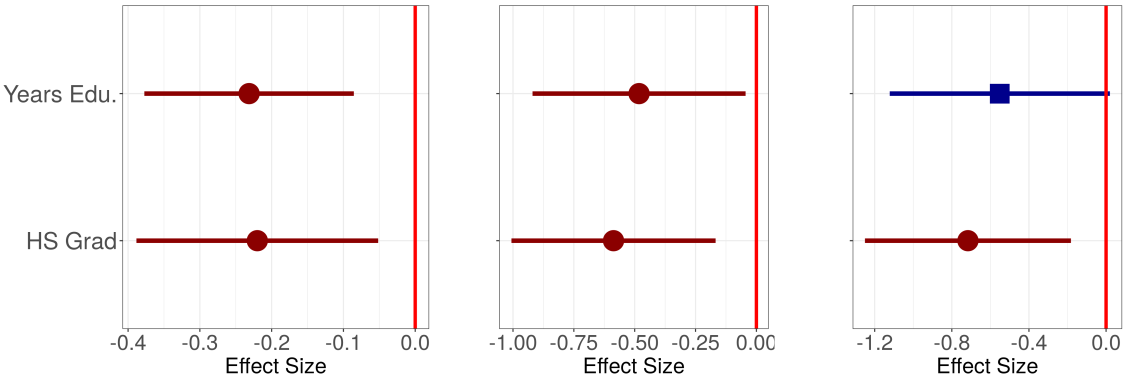


Figure 1.13: Effects of Assigned School Characteristics on Education for Immigrants



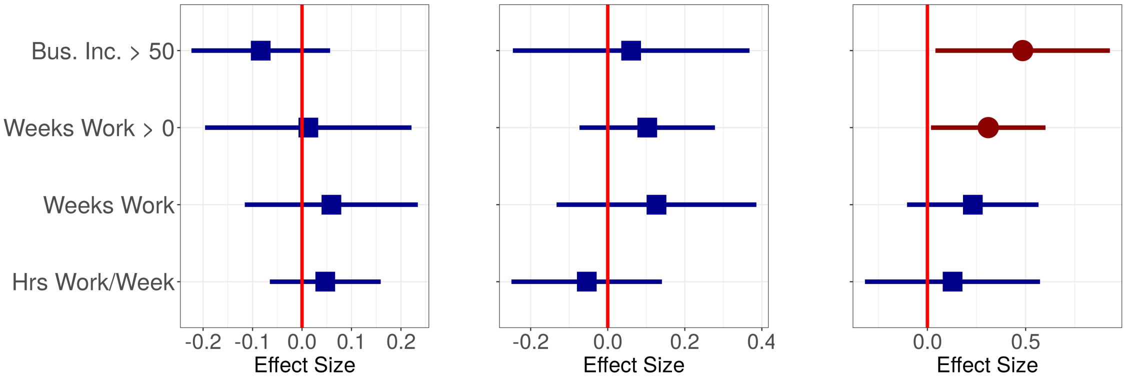
Note: Std. errors clustered at initial school level. Bars represent **90% confidence intervals**. Estimates from boundary change design with vector of initial school characteristics. School characteristics include funding per pupil, black and immigrant share, and median socioeconomic status. Includes additional controls at the individual level, in addition to age-by-ward fixed effects.

Figure 1.14: Effects of Assigned School Characteristics on Education for Natives


(a) School Funding per Pupil (b) Pct. Black (c) Pct. Immigrant

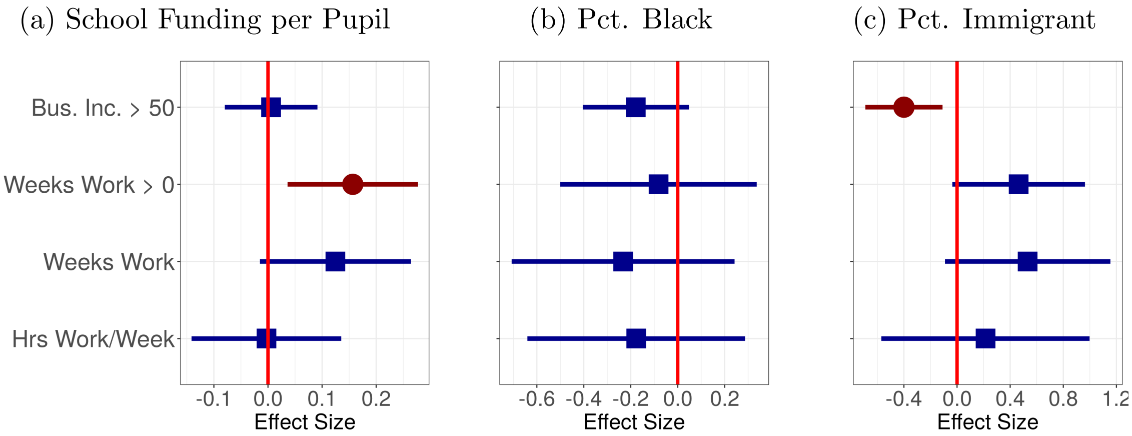
Note: Std. errors clustered at initial school level. Bars represent **90% confidence intervals**. Estimates from boundary change design with vector of initial school characteristics. School characteristics include funding per pupil, black and immigrant share, and median socioeconomic status. Includes additional controls at the individual level, in addition to age-by-ward fixed effects.

Figure 1.15: Effects of Assigned School Characteristics on Employment for Immigrants


(a) School Funding per Pupil (b) Pct. Black (c) Pct. Immigrant

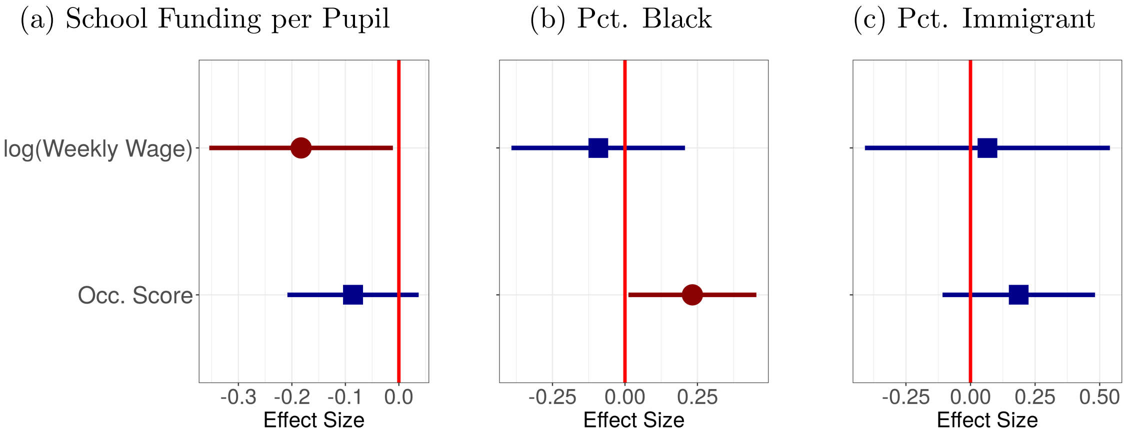
Note: Std. errors clustered at initial school level. Bars represent **90% confidence intervals**. Estimates from boundary change design with vector of initial school characteristics. School characteristics include funding per pupil, black and immigrant share, and median socioeconomic status. Includes additional controls at the individual level, in addition to age-by-ward fixed effects.

Figure 1.16: Effects of Assigned School Characteristics on Employment for Natives



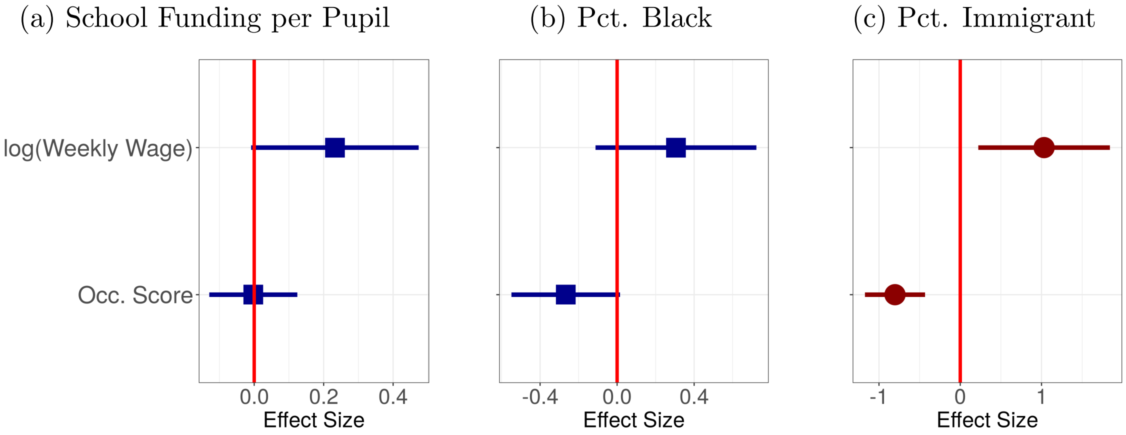
Note: Std. errors clustered at initial school level. Bars represent **90% confidence intervals**. Estimates from boundary change design with vector of initial school characteristics. School characteristics include funding per pupil, black and immigrant share, and median socioeconomic status. Includes additional controls at the individual level, in addition to age-by-ward fixed effects.

Figure 1.17: Effects of Assigned School Characteristics on Wages for Immigrants



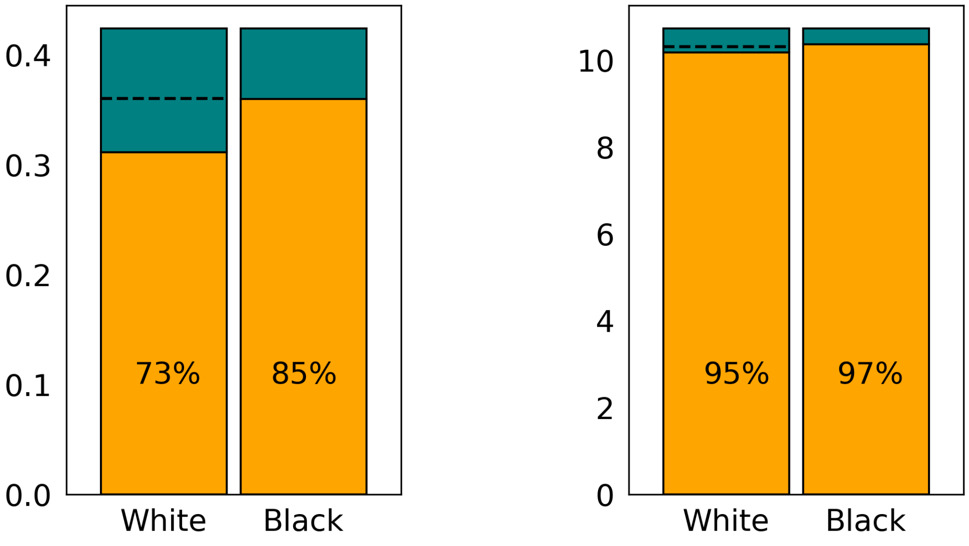
Note: Std. errors clustered at initial school level. Bars represent **90% confidence intervals**. Estimates from boundary change design with vector of initial school characteristics. School characteristics include funding per pupil, black and immigrant share, and median socioeconomic status. Includes additional controls at the individual level, in addition to age-by-ward fixed effects.

Figure 1.18: Effects of Assigned School Characteristics on Wages for Natives



Note: Std. errors clustered at initial school level. Bars represent **90% confidence intervals**. Estimates from boundary change design with vector of initial school characteristics. School characteristics include funding per pupil, black and immigrant share, and median socioeconomic status. Includes additional controls at the individual level, in addition to age-by-ward fixed effects.

Figure 1.19: Counterfactual Outcomes



(a) High School Grad

(b) Years of Schooling

Note: All panels use the regression estimates to form counterfactual. Yellow bars represent outcomes for 2nd generation immigrants and blue bars represent the values for white natives. The percentages refer to the ratio of the yellow to blue bars. Dotted lines represent the actual figures for white immigrants. Panels (a) and (b) show immigrant education after imposing a change to their student composition so as to make it 10% more similar to those of black schools in 1920.

CHAPTER 2

RACIAL TURNOVER AND THE DECLINE OF THE CHICAGO PUBLIC SCHOOLS: EVIDENCE FROM THE GREAT MIGRATION, 1910-1930

2.1 Introduction

In pursuit of a better life, many black Southerners moved Northward to answer the call of Northern industry at the outbreak of WWI. This initial wave of migration kick-started an exodus that would continue for nearly half a century. This movement would profoundly shape the character of effected cities and the fortunes of future generations, both in the South and North.

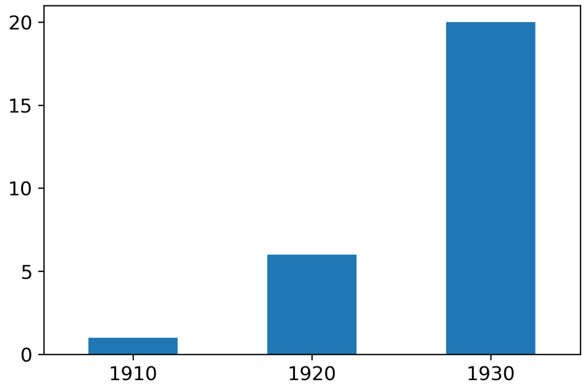
Free from the codified discrimination of the South, the newcomers faced prejudice that took shape in less overt, yet sometimes equally harsh, forms. By the late 1920s, the more violent means of restricting black residential choice gave way to restrictive covenants, which forbade blacks from residing in whole sections of Northern cities. It is thought that this segregation helped promote inequality in access to public services, jobs, and quality housing, and ultimately paved the way for black-white disparities that would persist for generations.

Recent studies suggest greater inflows of Southern blacks lead to cuts in public spending. For the 1910-30 period, Tabellini (2020) uses a shift-share design to show that cities with greater predicted inflows experienced deeper cuts, with inflows of 10,000 more blacks reducing spending per capita by 1.15 dollars, relative to a 1910 sample mean of 15.2 dollars. He attributes this decrease entirely to a decline in property values. But by using variation at the city level, he cannot determine how the migration affected the distribution of spending within cities. A similar critique applies to the results in Derenoncourt (2019).

In this paper, I utilize a newly constructed data series of elementary schools in a major Northern city to show how the distribution of funding changed during the first Great Migration. The data covers the city of Chicago for the period 1910-30, and was constructed by digitizing historical school boundaries and budgets (see Torcasso (2021) for details).

My empirical strategy leverages the abrupt increase in black enrollment between 1920 and 1930. Figure 2.1 shows the number of majority black schools over time. From 1920 to 1930, the number of majority black schools expanded from 6 to 20. I restrict my analysis to the set of schools with little to no black enrollment in 1920, and define the treatment as having majority black enrollment in 1930.

Figure 2.1: Number of Schools with Majority Black Attendance Areas



By comparing changes between majority and non-majority black schools before and after racial transition, I hope to capture the causal effect of racial turnover on the local school budget. I find that when a school transitioned from virtually no black students to a black majority, annual funding per pupil fell by $20 (1920 dollars) or about 25% of the 1930 average. This effect is explained in part by the rapid rise in membership per school in black neighborhoods, likely stemming from the era's restrictive and discriminatory housing policy.

The paper is organized as follows. In Section 2.2, I describe the data. Section 2.3 discusses the empirical approach and identifying assumptions. In Section 2.4, I discuss the results. And finally, Section 2.5 concludes.

2.2 Data

In this section, I summarize the construction of the data. For additional details, see Torcasso (2021). The data construction follows two broad steps: (1) produce GIS maps of historical elementary school boundaries, and (2) use the maps from step (1) to link households in the decennial census to their local neighborhood school.

The first step is relatively straightforward, but tedious, and relies on existing maps of historical street grids, railway lines, and water features. Each boundary is then mapped by following the path described in a book of the attendance boundaries. This process is repeated for years 1910, 1920, and 1930.

To link census households to schools, the near exact location of census respondents, obtained from Shertzer et al. (2016), is overlayed with the school boundaries. In practice, because only the enumeration district and street is known for most census respondents, the match is not always unique. Thus, to compute school area demographics, children are assigned to schools in proportion to the fraction of their street segment lying within a school area. So if a particular street segment is 40% contained in School A, and 60% in School B, and 100 black children live on the street, then effectively 60 black children are assigned to School A and 40 to School B. Most street segments are 100% contained within a single school area.

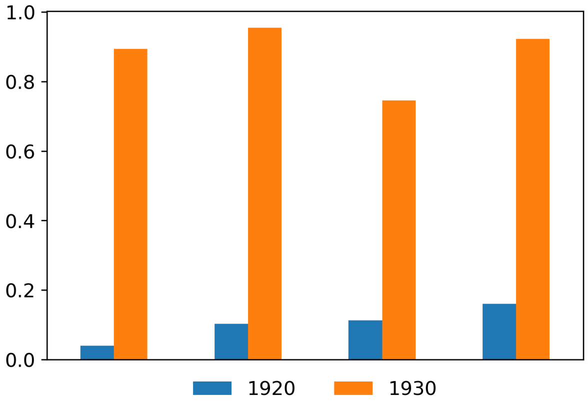
In addition to the constructed school demographics, budget data are collected from reports and proceedings of the school board. All figures are either taken or converted to a per pupil basis, using data on membership from historical school directories.

I restrict my attention to schools that were not majority black in 1920, and specifically, to schools with less than a 25% black share at the time. This restriction ensures that I only compare newly majority black schools to never majority black schools, and that the difference between the post and pre shares is at least 25% for the treated group–though in practice it is much greater. Table 2.1 provides descriptive statistics of the control and treated (majority black in 1930) schools in 1920. Figure 2.2 depicts the black share at the treated

Table 2.1: Descriptive Statistics for DID School Sample in 1920

	Control	Treated
Black	0.01	0.10
Immigrant	0.67	0.46
SEI	32.59	41.35
Pupils	1101.89	1321.88
Funds per Pupil	47.21	51.26
N	159	4

Figure 2.2: Black Share of Treated Schools: 1920 and 1930



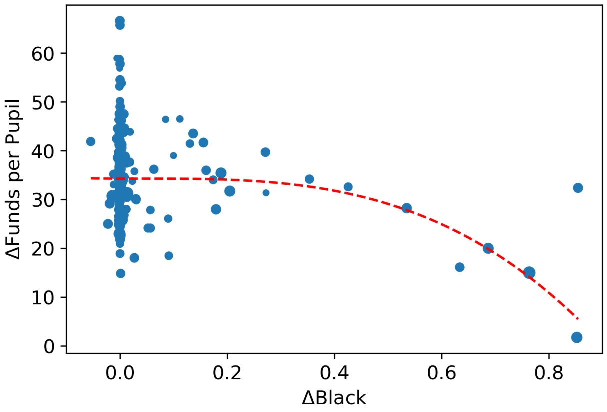
schools over time. They all change from nearly 100% white in 1920 to 100% black in 1930.

2.3 Empirical Approach

To assess the impact of racial change on the schools, I compare changes in school funding over time between majority black schools and a set of comparison schools. I choose a simple majority because the largest changes in school funding coincide with increases in the black share of more than 50 percentage points. Figure 2.3 shows the non-linear association between changes in the black share and changes in school funding between 1920 and 1930. A sharp drop-off occurs after 50 percentage points.

I estimate the impact of racial change using a two-way fixed effects approach, comparing

Figure 2.3: Changes in School Funding and Black Share between 1920 and 1930



the base year, 1920, with a comparison year τ, which is either 1910 or 1930.

$$Y_{jt} = \alpha_{j\tau} + \delta_\tau \mathbb{1}\{t = \tau\} + \gamma_\tau X_{jt} + \beta_\tau \mathbb{1}\{t = \tau\} D_j + \epsilon_{jt} \tag{2.1}$$

The fixed effects for school j and time t are captured by the first and second terms of Equation (2.1). The third term controls for observable time-varying characteristics such as white immigrant share and average socioeconomic status. The parameter β_τ is the coefficient of interest. When $\tau = 1910$, the coefficient tests for differences in trends prior to any racial transition in the sample. Goodman-Bacon (2018) describe how β_τ can be interpreted as a weighted average across all of the simple 2 × 2 difference-in-difference (DID) comparisons.

As with all DID designs, to assign a causal interpretation to β_τ requires a parallel trends assumption. That is, in absence of treatment, the treated and control school funding would have moved in parallel. This assumption is difficult to justify in settings where agents may sort in response to changes in ϵ_{jt}. For instance, if black families moved into school areas that experienced a decline in ϵ_{jt}, the parallel trends assumption would not hold. For instance, a factory could open up near the school and increase pollution, thereby reducing property values and school funding. Relatively poor, uneducated blacks may then move into the area in search of lower rents.

I try to assess the validity of the parallel trends assumption by considering the historical context. At the time, black residential choices were restricted by violence and discriminatory real estate practices. Neighborhoods were opened up to black residents in actions coordinated by white residents of the city. While this coordination reduces the sorting problem from the standpoint of individual residential choices, it is consistent with whites selecting declining school areas for racial turnover. I try to assess the validity of this concern by looking for pre-trends in school funding prior to racial turnover. Additionally, I can consider other supporting evidence consistent with a causal role for race. For instance, I show that an increase in the black share is associated with dramatic increases in membership. Crowding was a known problem of racial segregation in Chicago, and it spilled over to the schools.

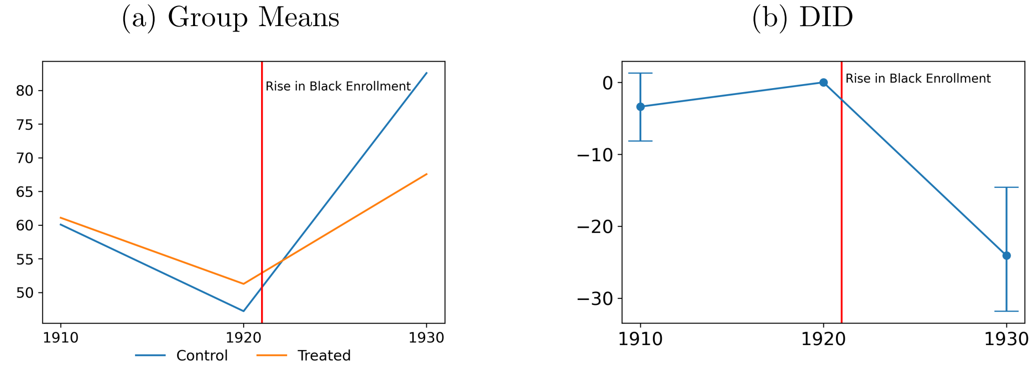
I cannot test if declines in funding are associated with decreases in property values, as in Tabellini (2020). However, future work could utilize data from Olcott's Land Values Blue Book of Chicago. Other explanations could include discriminatory practices of the Chicago Board of Education, or changes to local property tax rates that reflect the education preferences of residents. For instance, white residents may have preferred not to spend resources on black schools. Alternatively, blacks may not have had strong preferences for education. Though the latter seems to contradict qualitative evidence from the period.

2.4 Results

Figures 2.4-2.5 show estimates of β_T next to the raw group means. Figure 2.4 shows that when a school attains a black majority, its funding per pupil falls by over $20 in 1920 dollars, even after controlling for the white immigrant share and socioeconomic status. There are no differences in pre-trends between the treatment and control schools in terms of school funding or controls (see Figures B.3-B.4 in the Appendix).

The estimates in Figure 2.5 shed light on one potential mechanism. By restricting residential choice, segregation crowded black immigrants into a small section of the city. As a result, majority black neighborhoods saw membership per school rise substantially. Schools were either unable or unwilling to adjust resources, and, as a result, funding per pupil fell.

Figure 2.4: Impact of Black Majority on School Funding per Pupil

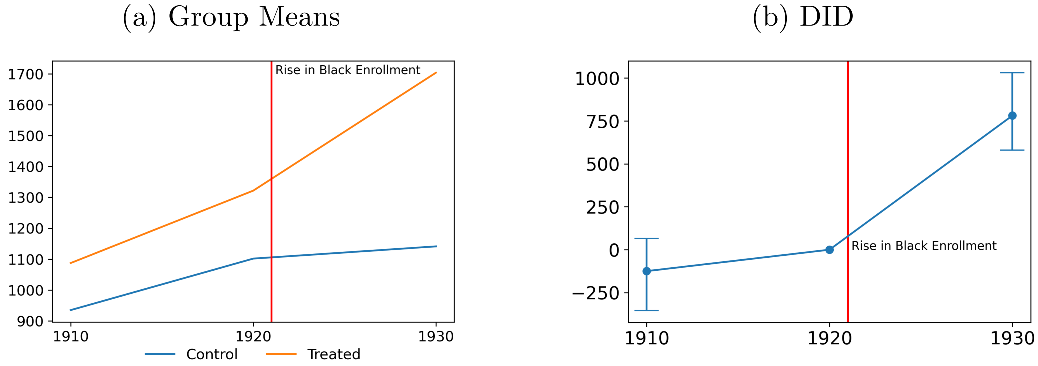


(a) Group Means

(b) DID

Note: DID estimates shown with 95% confidence interval estimated using method of Conley and Taber (2011) to account for the small number of treated units. Time-varying controls include immigrant share and mean socioeconomic status.

Figure 2.5: Impact of Black Majority on No. Pupils per School



(a) Group Means

(b) DID

Note: DID estimates shown with 95% confidence interval estimated using method of Conley and Taber (2011) to account for the small number of treated units. Time-varying controls include immigrant share and mean socioeconomic status.

2.5 Conclusion

By 1930, Chicago neighborhoods were nearly perfectly segregated by race, producing an all but separate system of unequal schools. In this paper, I show that when schools went from serving mostly white to mostly black students, their funding per pupil fell by about \$20, or 25% the 1930 average. This drop coincided with an increase in membership, which suggests segregation as a mechanism for the decline. My results are consistent with racially discriminatory housing practices leading to congestion problems and capacity constraints in the provision of public education.

Future work could consider the role of tax policy and property values in explaining the lack of funding in black schools. Granular information about the dynamics of property values could shed light on why school funding didn't increase in black neighborhoods to prevent a fall in resources per capita. This question is related to the seemingly paradoxical rise in rents and fall in property values in black neighborhoods discussed in the literature (Akbar, Li, Shertzer, & Walsh, 2019). And, in what is perhaps a situation unique to Chicago, more work should investigate tax collection and the *inconsistent* assessment practices (e.g., see Simpson (1930)) of the era.[1]

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[1] White absentee landlords may have had an individual incentive to influence tax assessors, even though it may have been in the collective interest of all landlords to increase tax collection for the sake of the public schools.

CHAPTER 3

ACCOUNTING FOR EFFORT IN FACTOR MODELS OF ACADEMIC PERFORMANCE WITH IMPLICATIONS FOR THE GENDER GAP IN SKILLS

3.1 Introduction

Both psychologists and economists use factor models to identify the distribution of latent skills. In many cases, they rely on measures of academic performance like test scores or school grades. But if we conceptualize these measurements as the output of a production function, failing to account for effort as an input can introduce bias in our estimates of skills (Almlund, Duckworth, Heckman, & Kautz, 2011; Kautz, Heckman, Diris, Ter Weel, & Borghans, 2014).

This critique applies to other inputs to the production function for tasks, like goods, time and the environment (G. Becker, 1977), and to other types of measurements. Some authors define tasks broadly enough to include self-reports of personality, which may be influenced by the agent's experiences and reference frame (Almlund et al., 2011; Kautz et al., 2014). In some cases, the questionnaire can be designed to include vignettes in hopes of standardizing the situation across respondents. Measures of behavior have been proposed as an alternative to direct elicitation of skills, but behaviors, like tasks, are also influenced by incentives and other environmental characteristics.

Empirical evidence suggests that incentives influence test scores. For example, Gneezy et al. (2017) and Borghans, Meijers, and ter Weel (2013) use experimental variation in incentives to show that effort has a substantial impact on test scores. Table 1.5 of Almlund et al. (2011) lists several studies where incentives ranging from test feedback to M&M candies, cash and tokens increased performance on tests. The degree of increase may depend on non-cognitive skills or culture (Borghans, Meijers, & Ter Weel, 2008; Gneezy et al., 2017).

In addition to tests, several papers show how local labor market conditions, by shifting the value of education, affect effort and school performance (Black, McKinnish, & Sanders, 2005; Marchand & Weber, 2018; McMullen, 2011).

Despite this evidence, the measurement literature has only recently acknowledged the difference between skills and effort, and no work attempts to separate the two empirically. Many papers separate measurements into those capturing cognitive and non-cognitive skills (Borghans, Golsteyn, Heckman, & Humphries, 2016; Cunha, Heckman, & Schennach, 2010; Heckman, Stixrud, & Urzua, 2006). But neither set of measurements are allowed to load on effort. They sometimes allow the parameters of the measurement equations to depend on background characteristics, which could capture effort, but they identify these parameters assuming independence between these characteristics and skills. Meanwhile, "new" skills are discovered and assessed on their power to predict life outcomes. Grit (e.g., see Duckworth, Peterson, Matthews, and Kelly (2007)) and other non-cognitive skills have dominated recent discourse on skills because of their importance in labor markets (Cortes, Jaimovich, & Siu, 2018). But it is difficult to know whether these measures reflect capabilities or circumstances.

Estimates of skills are sometimes deemed "valid" because they are stable across the life-cycle. But effort may also be stable, especially if determined in part by one's environment, which is highly correlated over time. Others argue for the validity of non-cognitive skills because of their importance in explaining the benefits of childhood interventions. I find this argument more compelling; improvements in skills likely play a huge role in explaining long-run outcomes. However, the most successful interventions include home visits and parent components which can alter skills and the home environment alike. In fact, the high degree of fade-out in cognitive impacts could suggest that programs alter the incentives for test performance, which fade when the student leaves the program. If changes in the home persist, they may continue to regulate the child's behavior and manifest in estimates of non-cognitive skills.

In some cases, the distinction may not matter. For instance, when factor models are used to produce conditioning sets in the analysis of treatment effects (Carneiro, Hansen, & Heckman, 2003; Heckman, Humphries, & Veramendi, 2016; Todd & Zhang, 2017). In other cases, the distinction is central to the economic hypothesis. One example applies to explaining the gender reversal in college attendance. G. S. Becker, Hubbard, and Murphy (2010) argue that the difference in the distribution of psychic costs between men and women (in particular, the relative thickness in the tails for men) caused college attendance to favor women as the returns to college grew over time.[1] The authors rely on evidence from test scores, grades and self-reports to support their hypothesis, but without knowing if higher scores imply greater effort or higher skills. If differences are driven by effort, we should understand the causes of this disparity in choices. Such differences would be natural to expect if students study in anticipation of future occupation and family decisions, behavior already linked to gender gaps (Adda, Dustmann, & Stevens, 2017; G. S. Becker, 1985; Polachek, 1981).

The literature validates skill estimates on whether they are predictive of long-run outcomes. But if we are interested in designing policies to enhance these outcomes, prediction is the wrong metric. If a child receives low grades but performs well on achievement tests, conventional analysis would conclude the child has low consciousness and high cognitive skills. But the low marks in school could reflect a disruptive home environment or that the student spends time taking care of family members. What would be the implications for policy? We might recommend a program to bolster the student's conscientiousness when alleviating the stresses of home would be much more effective at enhancing outcomes.

In this paper, I show how to separately identify skills from other inputs of the production function. I consider the case where measurements consist of noisy measures of task output. Within this framework, I discuss the shortcomings of current approaches for estimating skills and how my approach is an improvement over these methods. My proposed strategy uses restrictions implied by a model of effort allocation to disentangle the impact of effort and

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[1] Other work explains the gap in terms of mean differences in non-cognitive skills (Goldin, Katz, & Kuziemko, 2006; Jacob, 2002).

skills on academic performance. I use these restrictions to write the factor loading matrix in a form that allows me to apply Theorem 2.6 of Williams (2017). I therefore deviate from the dedicated factor structure or independence assumptions typically imposed to resolve the indeterminacy in confirmatory factor analysis. This allows the measurements to separately load on effort and skills, with no assumptions on the correlation between the two. I continue to make normalizing assumptions to pin down the scale on all of the factors, and the rotation *within the skill space.*

In my empirical application, I utilize detailed data on test scores, grades and self-reports of personality from Project Talent, a national longitudinal survey representative of the U.S. high school population in 1960. I show that the gender gap in cognitive skills increases when accounting for effort. Girls tend to exert more effort in school and on tests, lowering their adjusted scores relative to boys.

The paper proceeds as follows. Section 3.2 describes my behavioral model and the identification of skills from measurements of task output. Section 3.3 applies the identification strategy to estimate skill distributions in Project Talent, a nationally representative sample of high school students in 1960. I compare these results with an alternative strategy that does not adjust for effort. Section 3.4 concludes.

3.2 Model

Suppose the data contain conventional measures of skills. For instance, self-reports of skills, low-stakes tests and school grades. I first develop a behavioral model to describe how the data is generated. Then I describe the measurement model and discuss identification of this model. I show that most studies measuring skills make identifying assumptions that contradict my behavioral model, confounding effort with skills. I then use the behavioral model to make more plausible identifying assumptions which separately identify skills from effort.

3.2.1 Behavioral Model

Agents allocate effort and time across tasks to maximize utility in the spirit of G. Becker (1977). Suppose individual i's preferences are described by the expression

$$U(G_i^1, ..., G_i^J) = \prod_j (G_i^j)^{R_i^j}, \tag{3.1}$$

where G_i^j is the output of task j and R_i^j captures the rewards to output. R_i^j will be a function of skills θ_i, observable reward shifters Z_i^j and unobservable reward shifters u_i^j. Suppose I can write the production function for task j as

$$\begin{aligned} G_i^j &= \alpha_j(\theta_i, X_i^j)(E_i^j)^{\alpha_j^e \alpha_j^E}(t_i^j)^{\alpha_j^e \alpha_j^t} \\ &= \alpha_j(\theta_i, X_i^j)(\mathcal{E}_i^j)^{\alpha_j^e} \end{aligned} \tag{3.2}$$

where E_i^j, t_i^j and X_i^j are effort, time and environmental variables for task j, and θ_i is a vector of skills. The mapping $\alpha_j(\theta_i, X_i^j)$ will also be Cobb-Douglass, but I will not expand the term to simplify notation. $\mathcal{E}_i^j = (E_i^j)^{\alpha_j^E}(t_i^j)^{\alpha_j^t}$ is effective time. I will primarily work with effective time as most data sources do not differentiate between time and effort. The budget constraints for time and effort are

$$T_i = \sum_j t_i^j \tag{3.3}$$

and

$$E_i = \sum_j E_i^j. \tag{3.4}$$

The model contains no direct disutility of effort. All costs of time and effort are opportunity costs, consistent with recent work in psychology and neuroscience on the phenomenology of effort (Kurzban, Duckworth, W Kable, & Myers, 2013; Shenhav et al., 2017).

Maximizing the log of 3.1 with respect to $(E_i^j, t_i^j)_j$ subject to the constraints, I have the following first order conditions:

$$\frac{R_i^j \alpha_j^e \alpha_j^E}{E_i^j} = \varepsilon_i$$

and

$$\frac{R_i^j \alpha_j^e \alpha_j^t}{t_i^j} = \tau_i,$$

where ε_i and τ_i are the marginal utility of effort and time, respectively. The marginal benefit of effort and time is equal to it's opportunity cost. Using the budget constraints, I can solve for these marginal utilities.

$$\varepsilon_i = \frac{\sum_j R_i^j \alpha_j^e \alpha_j^E}{E_i}$$

$$\tau_i = \frac{\sum_j R_i^j \alpha_j^e \alpha_j^t}{T_i}.$$

Rewriting the FOCs, I have

$$E_i^j = \frac{R_i^j \alpha_j^e \alpha_j^E}{\varepsilon_i} = \frac{R_i^j \alpha_j^e \alpha_j^E E_i}{\sum_k R_i^k \alpha_k^e \alpha_k^E}.$$

and

$$t_i^j = \frac{R_i^j \alpha_j^e \alpha_j^t}{\tau_i} = \frac{R_i^j \alpha_j^e \alpha_j^t T_i}{\sum_k R_i^k \alpha_k^e \alpha_k^t}$$

Taking logs, I have

$$\log E_i^j = \log R_i^j + \log \alpha_j^e \alpha_j^E - \log \varepsilon_i \tag{3.5}$$

and

$$\log t_i^j = \log R_i^j + \log \alpha_j^e \alpha_j^t - \log \tau_i. \tag{3.6}$$

The log of effective time, which I denote as $e_i^j \equiv \log \mathcal{E}_i^j$, can be written as

$$e_i^j = \alpha_j^e(\alpha_j^E + \alpha_j^t)\log R_i^j + \alpha_j^e \alpha_j^E(\log \alpha_j^E - \log \varepsilon_i) + \alpha_j^e \alpha_j^t(\log \alpha_j^t - \log \tau_i). \tag{3.7}$$

In what follows, I may refer to e_i^j as effort, even though it is formally a combination of time and effort. The log of the production function, denoted $g_i^j \equiv \log G_i^j$, is

$$\begin{aligned} g_i^j &= \log \alpha_j(\theta_i, X_i^j) + \alpha_j^e e_i^j \\ &= \alpha_j^X X_i^j + \alpha_j^\theta \theta_i + \alpha_j^e e_i^j \end{aligned}$$

where I've expanded the function α_j in the second line, interpreting X_i^j and θ_i in logs. Depending on the task, the production function might incorporate some restrictions. I consider three types of tasks below: self-reports, low-stakes tests and school grades.

Self-Reports If j is a self-report, then the affect of effort and time is 0, and α_j^X may or may not be zero depending on whether the response is affected by the agent's reference frame. In this paper, I will assume that the output of self-report tasks is just

$$g_i^j = \alpha_j^\theta \theta_i,$$

i.e., some linear combination of skills. The linear combination of skills might not include all elements of θ_i.

Low-stakes Tests If j is a low-stakes test, then X_i^j likely doesn't vary across i and variation in effective time is due solely to effort because the duration of each test is predetermined by the testing agency. As a result,

$$g_i^j = \alpha_j^X X^j + \alpha_j^\theta \theta_i + \alpha_j^e e_i^j$$

where

$$e_i^j = \alpha_j^e \alpha_j^E R_i^j + \alpha_j^e \alpha_j^E (\log \alpha_j^E - \log \varepsilon_i) + \alpha_j^e \alpha_j^t t^j.$$

School Grades If j is a grade received in school, there is likely no restrictions on the production function. But for the purposes of this paper, I will assume $\alpha_j^X = 0$. Thus, I have

$$g_i^j = \alpha_j^\theta \theta + \alpha_j^e e_i^j.$$

Next I will describe the measurement model, which includes noisy measures of task output for the three types of tasks described above.

3.2.2 Measurement Model

Suppose for each individual i, I have a $J \times 1$ vector of measurements $M_i = (M_i^1, ..., M_i^J)'$, which capture a $K \times 1$ vector of factors F_i. Suppose the measurements are demeaned so I can ignore any constant term. If the relationship between M_i and F_i is linear, then we have the standard factor model

$$M_i = AF_i + \epsilon_i. \tag{3.8}$$

Many measurements are noisy indicators[2]of (log) task output, as described in Section 3.2.1. That is, for each task j,

$$M_i^j = g_j + \epsilon_i^j, \tag{3.9}$$

where g_j is as in Section 3.2.1 and ϵ_i^j is measurement error. The restrictions of the behavioral model imply that each measure, after subtracting its mean, can be written in the form

$$M_i^j = \alpha_j^0 e_i^j + \alpha_j^1 \theta_i + \epsilon_i^j. \tag{3.10}$$

While I've made the assumption that the environment X_i^j does not enter directly into this function, I can impose similar identifying assumptions on X_i^j.

3.2.3 Identification

I maintain the assumption that ϵ_i is independent of F_i and independent among its components, but I do not impose independence among the components of F_i. I don't include an intercept term for ease of notation and because this parameter would be pinned down by the mean. In practice I will demean the data. Let $Var[M] = \Sigma$, $Var[F] = \Phi$ and $Var[\epsilon] = \Delta$ so that $\Sigma = A\Phi A' + \Delta$. In some cases, F will contain observed variables so that some elements of Φ, A and Δ are known.

Suppose I have a sufficient number of repeated measurements such that $A\Phi A'$ is identified. A sufficient condition is for the matrix A to satisfy the row deletion property, which says for each measurement j there exists two non-overlapping sets of K measurements that are linearly independent. This assumption is not necessary when F_i contains observed components. See the Appendix, Section C.2.2 for more details.

Once $A\Phi A'$ is identified, I need to make further assumptions to separately identify A, Φ

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[2] In this paper, I assume effort and time allocations are based on subjective performance, before the measurement error is realized. Future work should more carefully understand how measurement error might affect these decisions and its implications for identification.

and the factors F_i. These assumptions are commonly referred to as a rotation. The rotation is captured by some orthogonal matrix C, which rotates the factors F_i within the factor space, such that $A\Phi A' = ACC'\Phi CC'A'$. Section 3.2.3 describes the shortcomings of current methods for resolving this indeterminacy in the identification of skills from measures of task output. In the remainder of this section, I describe a result from Williams (2017) on the identification of the matrix A that I'll use to devise new identification strategies that avoid the shortcomings of the literature.

Now suppose the loading matrix A from factor system 3.8 can be written as

$$A = \begin{pmatrix} I & 0 \\ A_{21} & A_{22} \\ A_{31} & A_{32} \end{pmatrix} \tag{3.11}$$

where I is an identity matrix and A_{22} is a square matrix. Since $A\Phi A'$, is identified, if A_{22} is also invertible, then I can apply Theorem 2.6 of Williams (2017) to show that $A_{32}A_{22}^{-1}$ and $A_{31} - A_{32}A_{22}^{-1}A_{21}$ are identified. Furthermore, if a column of A_{21} is 0, then the corresponding column of A_{31} is identified. See the Appendix, Section C.2.1 for a copy of the proof.

Shortcomings of Current Approaches for Identifying Skills

As described earlier, factor models are only identified up to some orthogonal rotation C. For instance, if $F_i = (e_i', \theta_i')'$, where e_i includes e_i^j for all j, then I can identify AC and $C'F$ for some orthogonal matrix C. The rotation I choose will determine how to interpret the factors. In most cases, C is chosen by normalizing each factor on some measurement. For instance, researchers often choose one test to be a dedicated measure of a "cognitive" factor. But if performance on tests is determined by both e_i and skills θ_i, then the factors estimated with this normalization would confound θ_i with e_i.

In some cases, confounding skills with e_i may not have dire implications. If e_i is constant throughout the life-cycle and we're interested in finding strong predictors of success, then we need not worry about confounding skills with effort. But if we're interested in policies that shift Z_i^j, and Z_i^j is a major determinant of e_i^j, then separating these variables from skills is more important. In other words, the distinction becomes more important the more one's circumstances influence their effort on tasks.

For instance, participants in job programs tend to improve during the duration of the program, but once it stops, any gains made tend to fade away. What was a temporary boon to motivation was due solely to context, not a lasting change beyond the program. Similarly, high achieving students in high school may have very active parents pushing them to succeed. They study hard and are well-behaved, so that they seem as though they have high cognitive and non-cognitive skills, but these perceptions were overly optimistic, their performance slipping once they enter college and the workforce. The role of context, and specifically, incentives, has also been demonstrated for achievement tests. Gneezy et al. (2017) show how incentives substantially affect the performance of U.S. students, but have no influence on a similar group of Chinese students, presumably due to the latter having greater intrinsic motivation to perform on tests for which there are no real world consequences.

To address these concerns, in the following sections I show how normalizations consistent with a model of effort allocation can help us identify skills θ_i without confounding them with effort and/or the determinants of effort.

Identification with Observed Effort

To demonstrate my approach, consider a measurement system which includes T tasks and T measures of effort for these tasks. The measurement equation for each task is written as in Equation 3.10, so that $F_i = (e_i', \theta_i')'$ in Equation 3.8. Suppose $A\Phi A'$ is identified. By arranging the measures so that the first T measures are the observed effort on the T tasks and the next T measures are the outputs, I can apply Theorem 2.6 of Williams (2017) to identify the impact of effort. That is, I write

$$
\begin{aligned}
M_i &= AF_i + \epsilon_i \qquad (3.12)\\
&= \begin{pmatrix}
1 & 0 & \cdots & 0 & 0 \\
0 & 1 & \cdots & 0 & 0 \\
\vdots & \vdots & \ddots & 0 & 0 \\
0 & 0 & 0 & 1 & 0 \\
\alpha_1^0 & 0 & \cdots & 0 & \alpha_1^1 \\
0 & \alpha_2^0 & \cdots & 0 & \alpha_2^1 \\
\vdots & \vdots & \ddots & 0 & \vdots \\
0 & 0 & 0 & \alpha_T^0 & \alpha_T^1
\end{pmatrix}
\begin{pmatrix}
e_i^1 \\ e_i^2 \\ \vdots \\ e_i^T \\ \theta_i
\end{pmatrix} + \epsilon_i
\end{aligned}
$$

where in the second line, the effort exclusions provides us many zeros in what would be sub-matrix A_{21} of equation system 3.11. Provided the measures sufficiently capture θ_i, then the rank condition for A_{22} is satisfied and I can identify A_{21} and A_{31}, or the $(\alpha_j^0)_j$ from equation system 3.12. Even though I assume effort is observed, identification is not trivial because effort may still be correlated with skills. The presence of the exclusions allows us to condition on the skills using measurements from one set of tasks so that I can obtain independent variation in the effort for the other set of tasks. Hence, ignoring measurement error, my identifying assumptions have a flavor similar to matching. Once A_{21} and A_{31} are identified, I can identify A_{22} and A_{32}, or the $(\alpha_j^1)_j$ of 3.12, up to some rotation of the skill space. For instance, by assuming a dedicated system on the skills.

In some cases, it might be plausible and more pragmatic to use fewer measures of effort than the number of tasks. Equation 3.7 shows that if we can group tasks according to the similarity in their rewards R_i^j, then it might be possible to use a single measure of effort as a proxy for the effort across all tasks in the group. For instance, suppose we can write effort for tasks j of group $g(j)$ in the form

$$e_i^j = a_j^0 + a_j^1 \bar{e}_i^{g(j)} + \nu_i^j,$$

where $\nu_i^j \perp\!\!\!\perp \theta_i$, then we can use a measure of $\bar{e}_i^g$ in place of e_i^j for all j such that $g(j) = g$. Identification would be achieved with just two groups, provided we meet the rank conditions on skills for each group of measures.

When X_i^j is included in the measurement equations, it is harder to appeal to a task framework to obtain the type of exclusions used above to secure identification. However, if X_i^j captures the reference frame, and the environment varies across reports, e.g., reports about behavior in school vs. at home, then these provide similar types of exclusions. Behavior at home may exclude characteristics of the school and behavior at school may exclude characteristics of the home.

Identification without Measures of Effort

If we don't observe effort e_i^j, we can make use of the derived demand for effort on tasks. Writing Equation 3.7 in terms of Z_i^j, θ_i, and u_i^j and ignoring the mean, we have

$$e_i^j = \gamma_j^0 Z_i^j + \gamma_j^1 \theta_i + u_i^j. \tag{3.13}$$

By substituting 3.13 into 3.10, we obtain

$$M_i^j = \alpha_j^0 \gamma_j^0 Z_i^j + (\alpha_j^1 + \alpha_j^0 \gamma_j^1)\theta_i + \alpha_j^0 u_i^j + \epsilon_i^j, \tag{3.14}$$

so that now the factors are $F_i = (Z_i', \theta_i')'$, where Z_i includes Z_i^j for all tasks j. Assume $u_i^j \perp\!\!\!\perp \theta, Z_i$. This assumption allows us to treat the additive term $\alpha_j^0 u_i^j + \epsilon_i^j$ as measurement error. While θ_i is unobserved, we often observe components of Z_i. For instance, sex, family income, and parental education may shift the extrinsic rewards to studying. However, it's unclear how we would identity this equation system because there aren't clear exclusions from one set of tasks to another, and assuming $Z_i^j \perp\!\!\!\perp \theta_i$ does not seem plausible. Experimental variation in Z_i^j would help, but in this paper I focus on observational variation.

3.3 Empirical Application

I estimate the distribution of skills of 9th graders using data from Project Talent, a nationally representative sample of high school students in 1960. These data contain test scores, grades and self reports of personality, as well as a survey of study habits, interests, work, family and education. Subsequent follow-ups were collected for a subsample and contain data on work, family and education through age 30. For the measurement system, I use a battery of tests, grades, and self reports of personality and study behavior collected during the base year in 1960. Tables 3.1 and 3.2 provide summary statistics of these data. See C.1.1 for more information about data collection in Project Talent.

3.3.1 Identification

Prior to estimation, I first determine the number of factors to include in the system. A scree test revealed that four factors should be sufficient for the set of measurements. I label the four factors test and school effort, and cognitive and non-cognitive skill. Thus, I utilize the assumptions of Section 3.2.3, dividing the task measures into school grades, low-stakes tests, and self-reports of personality. School grades will load on skills and school effort. Low-stakes tests will load on (low-stakes) test effort and cognitive skills. Self-reports of personality will load only on non-cognitive skills, though for some measures I relax this assumption as it is not necessary for identification. The grouping of tasks into low-stakes tests and school grades, with only one measure of effort for each group, is a strong assumption. While the grouping is intuitive–the incentives to perform well in school are different from those on low-stakes tests–if students choose different levels of effort between verbal and math tests in a way that is correlated skills, the strategy breaks down.

The dedicated measures of test effort include the clerical checking, object inspection, and table reading tests. These are very simple tests that I assume load only on effort. This assumption wouldn't hold if literacy affects a student's grasp of the instructions or if the child has poor eyesight. The first two measures of test effort have the student determine the consistency in a set of objects. In the clerical checking test, the student is asked to identify which pairs of names are spelled differently. For the object inspection test, the student must determine which object from a set of 5 is different from the others. For example, there are 5 identical cartoon photographs of an elephant, but the last one doesn't have a trunk. The table reading task asks students to find elements in a table after being given the row and column. The simplicity of these tasks suggests that more motivated students, not smarter students, will perform well. Segal (2012) uses a similar measure of test motivation. I measure school effort with self-reported measures related to the student's work effort in class.

I assume that all other tests load only on the cognitive factor (in addition to test effort) while grades are allowed to load on both cognitive and non-cognitive factors (in addition to school effort). The assumption that grades measure more than tests is common in other studies of cognitive and non-cognitive skills (Heckman et al., 2016, 2006). In order to meet the rank requirements for identification, I require at least one additional measure of non-cognitive skills that doesn't load on school effort. For this purpose, I use the social sensitivity scale obtained from self-reports of personality. I allow other measures of personality to load freely on school effort, cognitive and non-cognitive skills. To fix the scale of the factors I impose unit variance.[3] The mean is set to zero. In the current estimates, I don't account for the reference frame. Though the analysis in this paper suggests I may be able to account for the reference frame in future estimates.

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[3] For estimation reasons, I estimate the model by fixing the scale of the factors to the measurements by normalizing one loading for each factor to 1. Then I normalize the loading matrix A and the covariance matrix Φ so that Φ has unit variance. This procedure is just a rescaling and does not change the estimated value of $A\Phi A'$.

To compare the effort-adjusted estimates against a baseline, I identify a similar system of four factors. These baseline estimates impose a dedicated factor system on the same measures with the same number of factors. As dedicated measures of test effort I use the clerical checking, object inspection and table reading tests. For school effort I use self-reports of effort in class. For cognitive skill I use the total math score and for non-cognitive skill the social sensitivity scale. In summary, as opposed to the effort adjusted estimates, in the baseline estimates the math score no longer loads on test effort. Other tests and grades are allowed to freely load on both test and school effort. This is consistent with other studies that impose a dedicated factor system to pin down the rotation on the factors, which confounds skills with effort.

Before discussing estimation, I'd like to note the drawbacks of the current approach. While in the introduction I discussed a host of potential confounders to skills in conventional approaches to identification, I only address effort. While for low-stakes tests, effort might be the only confounder, environmental factors are still likely to affect grades. Additionally, I don't address the problem of reference bias in self-reports. Future work should account for these inputs.

3.3.2 Estimation

Prior to estimation, I standardize the measurements to have 0 mean and unit variance. Then I estimate the free parameters of A, Φ and Δ by maximum likelihood. In the Appendix, Section C.3.1, I provide the exact specification of the likelihood under the assumption of normality. Rubin and Thayer (1982) discuss the potential problem of several modes in the likelihood function. To deal with this problem, I start the numerical optimizer with multiple initial parameters to find the global optimum. Once the parameters are estimated I use the regression method to estimate the factor scores. Standard errors are computed using 1000 bootstrap samples.

3.3.3 Results

Section 3.4 contains tables and figures for the effort-adjusted estimates. Section 3.4 contains those for the baseline. I'll discuss the figures for the effort-adjusted estimates but you can find analogous figures for the baseline. Figure 3.1 shows kernel densities of the estimated factor scores by sex. Table 3.7 shows the standard errors of the means and standard deviations of the factor marginal distributions by sex. Table 3.5 provides the correlation matrix of the factor scores and Table 3.6 the estimated factor loadings. In all tables standard errors are in parentheses.

The loadings on the factors are statistically significant, have the expected sign and both tests and grades have high signal to noise ratios. The loadings on non-cognitive skills are strongly negative and statistically significant for grades. Other studies typically show a strong positive effect of non-cognitive skills on grades. I argue that this is a finding of my approach. While the total effect of non-cognitive skills, including the indirect effect through effort, may be positive, the direct effect of non-cognitive skills is negative. Since non-cognitive skills capture socioemotional skills, it is possible that while they increase effort, they also cause them to socialize too much in class, leading to lower marks.

Now to the discussion of the main findings. To highlight the difference between the baseline and effort-adjusted estimates, I consider the gender gap in skills. While for both specifications women on average exert more effort in school and on tests, and have greater non-cognitive skills, the specifications differ in their findings for cognitive skills. In the baseline, the factor estimates for cognitive skills suggest that men and women are quite comparable. Neither men nor women tend to outperform the other across all tests. Table 3.2 shows that women tend to outperform men on language related tests and men outperform women on mechanical tests. But after I adjust for effort, the cognitive skill distribution for men is far to the right of women. So why do women tend to dominate in some tests and not others? If I take the ratio of effort to cognitive elasticities, the correlation of this ratio's magnitude with the mean gender difference in raw scores is -0.47 for tests and -0.83 for grades, suggesting that women tend to do better on tasks that have relatively higher effort elasticities.

To investigate the disparity in effort between men and women I estimate a linear regression of test and school effort on various background characteristics and skills. Tables 3.3 and 3.4 provide the regression estimates. Cognitive skills appear to reduce effort while non-cognitive skills increase effort, suggesting that non-cognitive skills may reduce the cost of effort and cognitive skills may substitute for effort on tests and grades. Conditional on skills, men and women appear to choose different levels of effort on tests and in school, with men exerting more effort, not less effort, even though unconditionally men exert less effort. Men seem to exert less effort on tests and at school because of their higher, on average, cognitive skills. I should emphasize that this does not suggest inherent differences between men and women, only that by grade 9 the gender differences in skills is quite large. Future work should perform a similar exercise on more recent cohorts to see if the gap has diminished over time, and whether these trends are explained by secular changes in K-8 education.

3.4 Conclusion

I've shown that because effort only directly affects one task, it is possible to use multiple tasks, e.g., school and standardized exams, to help separate the effect of skills from effort on performance. I can then use these estimates to obtain measures of skills that are not contaminated with effort.

In the empirical application, I utilize Project Talent to investigate the gender gap in skills after adjusting for effort. Girls exert more effort in school and on low-stakes tests, and consequently perform better on tasks with greater effort elasticities. Compared to conventional techniques, adjusting for effort widens the gender gap in cognitive skills. The gender gap in effort may result from the gap in cognitive skills, which can substitute for effort.

Future work should further investigate the gender differences in effort, which persist after conditioning on skills. Such gender differences may result from the anticipation of occupation choices known to differ by gender. Alternatively, girls may simply differ from boys in how they report their effort in school, which would invalidate my estimates of gender gaps.

Table 3.1: Measurements

	mean	**std**	**max**	**min**	**N**
Clerical checking	34.24	14.33	74.00	0.00	99,652
Object inspection	21.35	7.17	40.00	0.00	99,654
Table Reading	10.41	6.87	72.00	0.00	99,587
Hours studying	8.97	5.61	20.00	0.00	94,492
Grades reflect ability	3.25	1.19	5.00	1.00	95,435
More than Required	2.79	1.14	5.00	1.00	96,714
Inattention causes lower marks	2.37	1.22	5.00	1.00	96,046
Too quick to do best work	2.47	1.13	5.00	1.00	95,819
Inattention caused missed assignments	2.11	1.10	5.00	1.00	96,158
Do just enough to get by	2.57	1.32	5.00	1.00	96,047
Careless errors lowers grades	2.36	1.18	5.00	1.00	96,325
Attention strays in class	2.41	1.14	5.00	1.00	96,256
Math total	18.61	7.21	53.00	0.00	99,503
Vocabulary	10.41	4.05	21.00	0.00	102,141
Literature	9.92	3.99	24.00	0.00	102,141
Music	5.25	2.84	13.00	0.00	102,141
Social studies	12.21	5.42	24.00	0.00	102,141
Mathematics	6.54	3.73	23.00	0.00	102,141
Physical science	7.71	3.80	18.00	0.00	102,141
Biological science	4.86	2.33	11.00	0.00	102,141
Scientific attitude	5.12	2.04	10.00	0.00	102,141
Aeronautics and space	3.37	2.15	10.00	0.00	102,141
Electricity and electronics	6.56	3.62	20.00	0.00	102,141

Continued on next page

Table 3.1 – *Continued from previous page*

	mean	**std**	**max**	**min**	**N**
Mechanical	8.48	3.70	19.00	0.00	102,141
Vocabulary	4.98	2.34	9.00	0.00	99,946
Memory for sentences	8.84	3.10	16.00	0.00	101,195
Memory for words	10.33	5.00	24.00	0.00	101,188
Disguised words	12.69	6.48	30.00	0.00	101,128
Spelling	8.13	3.00	16.00	0.00	101,185
Capitalization	28.10	4.92	33.00	0.00	101,178
Punctuation	15.90	4.69	27.00	0.00	101,249
English usage	15.40	3.52	25.00	0.00	101,254
Effective expression	7.66	2.50	12.00	0.00	101,205
Word functions in sentences	8.42	4.84	24.00	0.00	101,076
Reading comprehension	25.11	10.81	48.00	0.00	101,189
Creativity	7.35	3.66	20.00	0.00	101,036
Mechanical reasoning	9.43	4.08	20.00	0.00	100,947
Visualization in 2d	11.59	5.66	24.00	0.00	101,091
Visualization in 3d	7.69	3.10	16.00	0.00	100,961
Abstract reasoning	8.02	3.16	15.00	0.00	100,907
Arithmetic computation	34.49	10.22	72.00	0.00	99,647
Math grades	3.50	1.37	6.00	1.00	92,403
Science grades	3.57	1.36	6.00	1.00	80,565
Language grades	3.61	1.49	6.00	1.00	56,351
Social studies grades	3.65	1.34	6.00	1.00	75,889
English grades	3.66	1.32	6.00	1.00	90,694
Vocational grades	3.75	1.34	6.00	1.00	46,811

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Table 3.1 – *Continued from previous page*

	mean	**std**	**max**	**min**	**N**
Business grades	3.64	1.39	6.00	1.00	41,150
Social sensitivity	4.13	2.29	9.00	0.00	101,306
Mature personality	10.07	5.00	24.00	0.00	101,306
Sociability	6.22	2.89	12.00	0.00	101,306
Impulsiveness	1.84	1.57	9.00	0.00	101,306
Vigor	3.43	2.07	7.00	0.00	101,306
Calmness	3.67	2.37	9.00	0.00	101,306
Tidiness	5.17	2.73	11.00	0.00	101,306
Culture	4.73	2.31	10.00	0.00	101,306
Leadership	1.24	1.31	5.00	0.00	101,306
Self-confidence	4.74	2.34	12.00	0.00	101,306

Table 3.2: Measurements, by Sex

	Female					Male				
	mean	**std**	**max**	**min**	**N**	**mean**	**std**	**max**	**min**	**N**
Clerical checking	35.98	14.22	74.00	0.00	49,939	32.49	14.22	74.00	0.00	49,713
Object inspection	21.93	6.92	40.00	0.00	49,925	20.76	7.36	40.00	0.00	49,729
Table Reading	10.82	6.72	72.00	0.00	49,888	10.00	7.00	72.00	0.00	49,699
Hours studying	9.41	5.60	20.00	0.00	47,105	8.52	5.57	20.00	0.00	47,387
Grades reflect ability	3.37	1.15	5.00	1.00	47,450	3.13	1.21	5.00	1.00	47,985
More than Required	2.84	1.12	5.00	1.00	48,176	2.75	1.16	5.00	1.00	48,538
Inattention causes lower marks	2.20	1.17	5.00	1.00	47,969	2.54	1.25	5.00	1.00	48,077
Too quick to do best work	2.28	1.05	5.00	1.00	47,849	2.66	1.17	5.00	1.00	47,970
Inattention caused missed assignments	1.91	0.99	5.00	1.00	48,026	2.32	1.16	5.00	1.00	48,132
Do just enough to get by	2.34	1.27	5.00	1.00	47,933	2.79	1.33	5.00	1.00	48,114
Careless errors lowers grades	2.15	1.10	5.00	1.00	48,075	2.58	1.21	5.00	1.00	48,250
Attention strays in class	2.27	1.09	5.00	1.00	48,058	2.55	1.17	5.00	1.00	48,198
Math total	18.35	6.95	53.00	0.00	49,460	18.87	7.46	52.00	0.00	50,043
Vocabulary	9.85	3.94	21.00	0.00	50,693	10.96	4.09	21.00	0.00	51,448

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Table 3.2 – *Continued from previous page*

	Female					**Male**				
	mean	**std**	**max**	**min**	**N**	**mean**	**std**	**max**	**min**	**N**
Literature	9.82	3.87	24.00	0.00	50,693	10.02	4.11	24.00	0.00	51,448
Music	5.56	2.87	13.00	0.00	50,693	4.94	2.78	13.00	0.00	51,448
Social studies	11.44	5.03	24.00	0.00	50,693	12.96	5.68	24.00	0.00	51,448
Mathematics	6.15	3.46	23.00	0.00	50,693	6.92	3.94	23.00	0.00	51,448
Physical science	6.88	3.51	18.00	0.00	50,693	8.53	3.89	18.00	0.00	51,448
Biological science	4.44	2.19	11.00	0.00	50,693	5.27	2.39	11.00	0.00	51,448
Scientific attitude	5.19	1.97	10.00	0.00	50,693	5.05	2.11	10.00	0.00	51,448
Aeronautics and space	2.57	1.60	10.00	0.00	50,693	4.15	2.33	10.00	0.00	51,448
Electricity and electronics	5.20	2.58	18.00	0.00	50,693	7.89	3.98	20.00	0.00	51,448
Mechanical	6.66	2.79	17.00	0.00	50,693	10.26	3.62	19.00	0.00	51,448
Vocabulary	5.17	2.25	9.00	0.00	49,589	4.79	2.41	9.00	0.00	50,357
Memory for sentences	9.22	3.08	16.00	0.00	50,153	8.47	3.07	16.00	0.00	51,042
Memory for words	11.06	5.15	24.00	0.00	50,153	9.61	4.75	24.00	0.00	51,035
Disguised words	13.29	6.60	30.00	0.00	50,138	12.10	6.30	30.00	0.00	50,990
Spelling	8.85	2.86	16.00	0.00	50,155	7.42	2.95	16.00	0.00	51,030

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Table 3.2 – *Continued from previous page*

	Female					**Male**				
	mean	**std**	**max**	**min**	**N**	**mean**	**std**	**max**	**min**	**N**
Capitalization	28.88	4.48	33.00	0.00	50,154	27.34	5.21	33.00	0.00	51,024
Punctuation	16.84	4.57	27.00	0.00	50,176	14.98	4.62	27.00	0.00	51,073
English usage	15.98	3.29	25.00	0.00	50,169	14.83	3.65	25.00	0.00	51,085
Effective expression	8.08	2.31	12.00	0.00	50,159	7.26	2.62	12.00	0.00	51,046
Word functions in sentences	9.06	5.08	24.00	0.00	50,114	7.79	4.51	24.00	0.00	50,962
Reading comprehension	25.91	10.45	48.00	0.00	50,149	24.33	11.10	48.00	0.00	51,040
Creativity	7.14	3.49	20.00	0.00	50,080	7.57	3.80	20.00	0.00	50,956
Mechanical reasoning	7.78	3.34	20.00	0.00	50,077	11.06	4.09	20.00	0.00	50,870
Visualization in 2d	10.69	5.46	24.00	0.00	50,106	12.47	5.72	24.00	0.00	50,985
Visualization in 3d	7.33	2.88	16.00	0.00	50,028	8.05	3.26	16.00	0.00	50,933
Abstract reasoning	7.95	3.15	15.00	0.00	50,004	8.08	3.17	15.00	0.00	50,903
Arithmetic computation	35.90	10.30	72.00	0.00	49,897	33.08	9.94	72.00	0.00	49,750
Math grades	3.51	1.36	6.00	1.00	45,859	3.49	1.39	6.00	1.00	46,544
Science grades	3.57	1.34	6.00	1.00	38,294	3.57	1.38	6.00	1.00	42,271
Language grades	3.78	1.46	6.00	1.00	25,968	3.47	1.50	6.00	1.00	30,383

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Table 3.2 – *Continued from previous page*

	Female					**Male**				
	mean	**std**	**max**	**min**	**N**	**mean**	**std**	**max**	**min**	**N**
Social studies grades	3.72	1.32	6.00	1.00	36,498	3.58	1.36	6.00	1.00	39,391
English grades	3.86	1.28	6.00	1.00	45,114	3.46	1.32	6.00	1.00	45,580
Vocational grades	3.81	1.33	6.00	1.00	17,950	3.72	1.35	6.00	1.00	28,861
Business grades	3.73	1.37	6.00	1.00	17,591	3.58	1.40	6.00	1.00	23,559
Social sensitivity	4.66	2.31	9.00	0.00	50,619	3.59	2.16	9.00	0.00	50,687
Mature personality	10.44	4.97	24.00	0.00	50,619	9.70	5.01	24.00	0.00	50,687
Sociability	6.77	2.88	12.00	0.00	50,619	5.67	2.80	12.00	0.00	50,687
Impulsiveness	1.84	1.58	9.00	0.00	50,619	1.85	1.56	9.00	0.00	50,687
Vigor	3.45	2.11	7.00	0.00	50,619	3.41	2.04	7.00	0.00	50,687
Calmness	3.88	2.43	9.00	0.00	50,619	3.47	2.30	9.00	0.00	50,687
Tidiness	5.69	2.71	11.00	0.00	50,619	4.65	2.66	11.00	0.00	50,687
Culture	5.26	2.25	10.00	0.00	50,619	4.21	2.26	10.00	0.00	50,687
Leadership	1.28	1.33	5.00	0.00	50,619	1.19	1.28	5.00	0.00	50,687
Self-confidence	4.84	2.41	12.00	0.00	50,619	4.64	2.26	12.00	0.00	50,687

Tables and Figures for Effort Adjusted Estimates

Table 3.3: Regression Estimates of School Effort

Intercept	-0.472	(0.031)
Male	0.194	(0.011)
Fraction Black	0.001	(0.000)
Public School	0.000	(0.009)
No. of Books	0.000	(0.000)
SES	-0.010	(0.005)
Parent's Highest Edu.	0.011	(0.002)
No. of Siblings	-0.009	(0.002)
Hours Chores	0.002	(0.001)
Hours Work	-0.004	(0.001)
Sports	-0.007	(0.001)
Group Activities	0.003	(0.000)
Hobbies	0.003	(0.000)
Expect College	0.264	(0.027)
Expect HS Grad	0.113	(0.022)
Cognitive	-0.663	(0.045)
Noncognitive	0.334	(0.036)
R squared	0.723	
Obs.	55,570	

Table 3.4: Regression Estimates of Test Effort

Intercept	-0.597	(0.092)
Male	0.347	(0.033)
Fraction Black	-0.006	(0.001)
Public School	0.023	(0.012)
No. of Books	0.000	(0.000)
SES	-0.057	(0.013)
Parent's Highest Edu.	0.034	(0.007)
No. of Siblings	-0.035	(0.007)
Hours Chores	-0.001	(0.001)
Hours Work	-0.006	(0.002)
Sports	0.004	(0.001)
Group Activities	-0.007	(0.001)
Hobbies	0.001	(0.000)
Expect College	0.301	(0.071)
Expect HS Grad	0.348	(0.086)
Cognitive	-0.722	(0.126)
Noncognitive	0.067	(0.024)
R squared	0.752	
Obs.	55,570	

Table 3.5: Effort Adjusted Factor Correlations

	Test Effort	**School Effort**	**Cognitive**	**Noncognitive**
Test Effort	1.00	0.73	-0.77	0.41
	(0.000)	(0.064)	(0.114)	(0.014)
School Effort	0.73	1.00	-0.77	0.63
	(0.064)	(0.000)	(0.037)	(0.015)
Cognitive	-0.77	-0.77	1.00	-0.37
	(0.114)	(0.037)	(0.000)	(0.020)
Noncognitive	0.41	0.63	-0.37	1.00
	(0.014)	(0.015)	(0.020)	(0.000)

Table 3.6: Factor Loadings

	Test Effort	School Effort	Cognitive	Noncognitive	Signal
Clerical checking	0.14	-	-	-	0.02
	(0.056)	-	-	-	
Object inspection	0.23	-	-	-	0.05
	(0.004)	-	-	-	
Table Reading	0.27	-	-	-	0.07
	(0.009)	-	-	-	
Hours studying	-	0.08	-	-	0.01
	-	(0.065)	-	-	
Grades reflect ability	-	0.35	-	-	0.12
	-	(0.007)	-	-	
More than Required	-	0.31	-	-	0.10
	-	(0.012)	-	-	
Inattention causes lower marks	-	-0.37	-	-	0.14

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Table 3.6 – *Continued from previous page*

	Test Effort	**School Effort**	**Cognitive**	**Noncognitive**	**Signal**
	-	(0.007)	-	-	
Too quick to do best work	-	-0.28	-	-	0.08
	-	(0.007)	-	-	
Inattention caused missed assignments	-	-0.38	-	-	0.14
	-	(0.005)	-	-	
Do just enough to get by	-	-0.40	-	-	0.16
	-	(0.005)	-	-	
Careless errors lowers grades	-	-0.37	-	-	0.13
	-	(0.004)	-	-	
Attention strays in class	-	-0.38	-	-	0.15
	-	(0.006)	-	-	
Math total	1.15	-	0.57	-	0.64
	(0.231)	-	(0.267)	-	
Vocabulary	1.33	-	0.90	-	0.75
	(0.288)	-	(0.321)	-	

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Table 3.6 – *Continued from previous page*

	Test Effort	**School Effort**	**Cognitive**	**Noncognitive**	**Signal**
Literature	1.14	-	0.66	-	0.58
	(0.231)	-	(0.262)	-	
Music	1.00	-	0.47	-	0.50
	(0.192)	-	(0.222)	-	
Social studies	1.26	-	0.81	-	0.69
	(0.267)	-	(0.300)	-	
Mathematics	1.15	-	0.73	-	0.57
	(0.240)	-	(0.269)	-	
Physical science	1.24	-	0.99	-	0.64
	(0.287)	-	(0.313)	-	
Biological science	1.10	-	0.88	-	0.50
	(0.253)	-	(0.277)	-	
Scientific attitude	0.93	-	0.50	-	0.40
	(0.184)	-	(0.210)	-	
Aeronautics and space	1.07	-	1.07	-	0.53

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Table 3.6 – *Continued from previous page*

	Test Effort	**School Effort**	**Cognitive**	**Noncognitive**	**Signal**
	(0.270)	-	(0.285)	-	
Electricity and electronics	1.08	-	1.12	-	0.57
	(0.278)	-	(0.293)	-	
Mechanical	1.08	-	1.15	-	0.58
	(0.279)	-	(0.293)	-	
Vocabulary	1.09	-	0.52	-	0.60
	(0.213)	-	(0.247)	-	
Memory for sentences	0.48	-	0.10	-	0.17
	(0.077)	-	(0.092)	-	
Memory for words	0.67	-	0.14	-	0.33
	(0.110)	-	(0.135)	-	
Disguised words	0.90	-	0.28	-	0.50
	(0.156)	-	(0.184)	-	
Spelling	0.68	-	-0.05	-	0.50
	(0.091)	-	(0.121)	-	

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Table 3.6 – *Continued from previous page*

	Test Effort	**School Effort**	**Cognitive**	**Noncognitive**	**Signal**
Capitalization	0.70	-	0.11	-	0.38
	(0.110)	-	(0.138)	-	
Punctuation	0.94	-	0.17	-	0.66
	(0.153)	-	(0.189)	-	
English usage	0.84	-	0.20	-	0.49
	(0.137)	-	(0.168)	-	
Effective expression	0.73	-	0.15	-	0.38
	(0.118)	-	(0.146)	-	
Word functions in sentences	0.86	-	0.24	-	0.49
	(0.147)	-	(0.177)	-	
Reading comprehension	1.21	-	0.55	-	0.75
	(0.233)	-	(0.271)	-	
Creativity	1.05	-	0.67	-	0.47
	(0.221)	-	(0.248)	-	
Mechanical reasoning	1.09	-	1.04	-	0.53

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Table 3.6 – *Continued from previous page*

	Test Effort	**School Effort**	**Cognitive**	**Noncognitive**	**Signal**
	(0.269)	-	(0.286)	-	
Visualization in 2d	0.70	-	0.52	-	0.20
	(0.156)	-	(0.168)	-	
Visualization in 3d	0.83	-	0.63	-	0.29
	(0.187)	-	(0.204)	-	
Abstract reasoning	0.95	-	0.50	-	0.42
	(0.188)	-	(0.214)	-	
Arithmetic computation	0.66	-	0.04	-	0.40
	(0.095)	-	(0.118)	-	
Math grades	-	1.22	0.64	-0.33	0.45
	-	(0.077)	(0.074)	(0.028)	
Science grades	-	1.36	0.80	-0.31	0.55
	-	(0.088)	(0.080)	(0.029)	
Language grades	-	1.26	0.61	-0.34	0.50
	-	(0.083)	(0.086)	(0.026)	

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Table 3.6 – *Continued from previous page*

	Test Effort	**School Effort**	**Cognitive**	**Noncognitive**	**Signal**
Social studies grades	-	1.38	0.75	-0.32	0.59
	-	(0.094)	(0.093)	(0.028)	
English grades	-	1.34	0.60	-0.32	0.61
	-	(0.085)	(0.087)	(0.026)	
Vocational grades	-	1.15	0.68	-0.24	0.41
	-	(0.085)	(0.086)	(0.024)	
Business grades	-	1.14	0.65	-0.26	0.39
	-	(0.085)	(0.089)	(0.024)	
Social sensitivity	-	-	-	0.78	0.60
	-	-	-	(0.003)	
Mature personality	-	0.27	0.27	0.74	0.68
	-	(0.047)	(0.036)	(0.013)	
Sociability	-	-0.20	-0.15	0.73	0.44
	-	(0.030)	(0.019)	(0.014)	
Impulsiveness	-	-0.10	0.11	0.43	0.13

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Table 3.6 – *Continued from previous page*

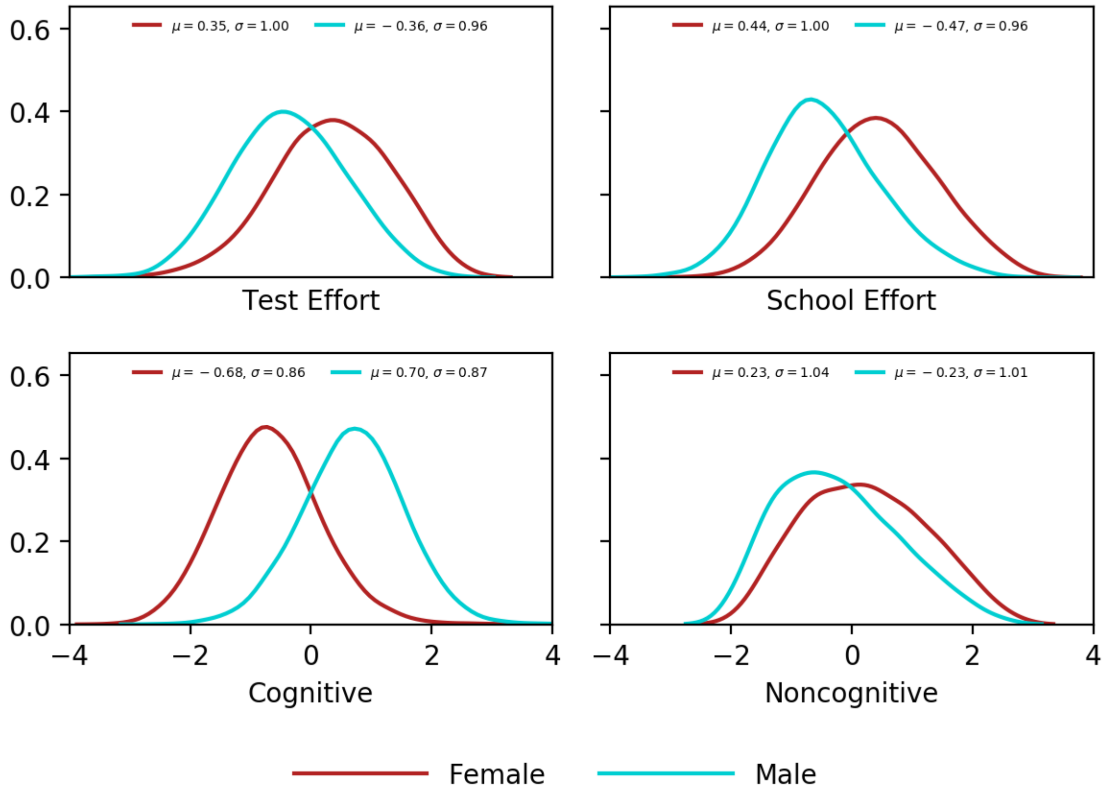
	Test Effort	**School Effort**	**Cognitive**	**Noncognitive**	**Signal**
	-	(0.029)	(0.021)	(0.010)	
Vigor	-	-0.02	0.12	0.70	0.42
	-	(0.043)	(0.025)	(0.016)	
Calmness	-	0.06	0.15	0.76	0.56
	-	(0.041)	(0.026)	(0.014)	
Tidiness	-	0.02	-0.01	0.74	0.57
	-	(0.040)	(0.028)	(0.016)	
Culture	-	0.00	-0.01	0.77	0.59
	-	(0.041)	(0.029)	(0.017)	
Leadership	-	0.16	0.24	0.54	0.32
	-	(0.055)	(0.050)	(0.014)	
Self-confidence	-	0.10	0.10	0.45	0.23
	-	(0.026)	(0.016)	(0.011)	

*Fixed at this value.

Table 3.7: Standard Errors of Means and Std. Deviation of Effort Adjusted Factor Distributions

		Test Effort	**School Effort**	**Cognitive**	**Noncognitive**
Female	mean	0.07	0.01	0.02	0.01
	std	0.01	0.01	0.02	0.00
Male	mean	0.07	0.01	0.02	0.01
	std	0.02	0.01	0.00	0.00

Figure 3.1: Effort Adjusted Factor Marginal Distributions by Sex, Grade 9, 1960



Tables and Figures for Baseline Estimates

Table 3.8: Baseline Factor Correlations

	Test Effort	**School Effort**	**Cognitive**	**Noncognitive**
Test Effort	1.00	-0.84	0.88	0.58
	(0.000)	(0.012)	(0.030)	(0.027)
School Effort	-0.84	1.00	-0.60	-0.56
	(0.012)	(0.000)	(0.019)	(0.017)
Cognitive	0.88	-0.60	1.00	0.34
	(0.030)	(0.019)	(0.000)	(0.011)
Noncognitive	0.58	-0.56	0.34	1.00
	(0.027)	(0.017)	(0.011)	(0.000)

Table 3.9: Factor Loadings

	Test Effort	**School Effort**	**Cognitive**	**Noncognitive**	**Signal**
Clerical checking	0.16	-	-	-	0.02
	(0.019)	-	-	-	
Object inspection	0.22	-	-	-	0.05
	(0.006)	-	-	-	
Table Reading	0.26	-	-	-	0.07
	(0.006)	-	-	-	
Hours studying	-	0.08	-	-	0.01
	-	(0.008)	-	-	
Grades reflect ability	-	-0.32	-	-	0.10
	-	(0.005)	-	-	
More than Required	-	-0.25	-	-	0.06
	-	(0.006)	-	-	
Inattention causes lower marks	-	0.38	-	-	0.15

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Table 3.9 – *Continued from previous page*

	Test Effort	**School Effort**	**Cognitive**	**Noncognitive**	**Signal**
	-	(0.004)	-	-	
Too quick to do best work	-	0.29	-	-	0.09
	-	(0.003)	-	-	
Inattention caused missed assignments	-	0.40	-	-	0.16
	-	(0.005)	-	-	
Do just enough to get by	-	0.38	-	-	0.15
	-	(0.005)	-	-	
Careless errors lowers grades	-	0.39	-	-	0.15
	-	(0.004)	-	-	
Attention strays in class	-	0.38	-	-	0.15
	-	(0.005)	-	-	
Math total	-	-	0.80	-	0.64
	-	-	(0.004)	-	
Vocabulary	-0.18	0.17	1.10	-	0.75
	(0.106)	(0.036)	(0.078)	-	

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Table 3.9 – *Continued from previous page*

	Test Effort	**School Effort**	**Cognitive**	**Noncognitive**	**Signal**
Literature	-0.28	-0.04	0.97	-	0.58
	(0.092)	(0.032)	(0.067)	-	
Music	-0.26	-0.17	0.83	-	0.49
	(0.071)	(0.024)	(0.053)	-	
Social studies	-0.07	0.14	0.96	-	0.69
	(0.086)	(0.030)	(0.062)	-	
Mathematics	0.18	0.23	0.72	-	0.57
	(0.053)	(0.023)	(0.034)	-	
Physical science	0.24	0.51	0.83	-	0.64
	(0.070)	(0.027)	(0.049)	-	
Biological science	0.04	0.37	0.83	-	0.50
	(0.083)	(0.034)	(0.058)	-	
Scientific attitude	-0.42	-0.16	0.90	-	0.40
	(0.086)	(0.029)	(0.068)	-	
Aeronautics and space	0.29	0.72	0.73	-	0.53

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Table 3.9 – *Continued from previous page*

	Test Effort	**School Effort**	**Cognitive**	**Noncognitive**	**Signal**
	(0.107)	(0.044)	(0.070)	-	
Electricity and electronics	0.47	0.84	0.64	-	0.56
	(0.077)	(0.038)	(0.049)	-	
Mechanical	0.25	0.79	0.78	-	0.57
	(0.100)	(0.037)	(0.069)	-	
Vocabulary	-0.50	-0.28	1.04	-	0.60
	(0.106)	(0.034)	(0.083)	-	
Memory for sentences	-0.44	-0.37	0.54	-	0.17
	(0.056)	(0.022)	(0.047)	-	
Memory for words	-0.36	-0.40	0.61	-	0.32
	(0.052)	(0.023)	(0.042)	-	
Disguised words	-0.49	-0.41	0.86	-	0.49
	(0.083)	(0.029)	(0.066)	-	
Spelling	-0.69	-0.76	0.74	-	0.50
	(0.077)	(0.032)	(0.064)	-	

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Table 3.9 – *Continued from previous page*

	Test Effort	**School Effort**	**Cognitive**	**Noncognitive**	**Signal**
Capitalization	-0.89	-0.67	0.94	-	0.40
	(0.110)	(0.041)	(0.092)	-	
Punctuation	-0.80	-0.70	1.03	-	0.66
	(0.099)	(0.036)	(0.083)	-	
English usage	-0.86	-0.63	1.02	-	0.50
	(0.112)	(0.035)	(0.094)	-	
Effective expression	-0.86	-0.61	0.95	-	0.39
	(0.101)	(0.038)	(0.087)	-	
Word functions in sentences	-0.21	-0.33	0.65	-	0.48
	(0.056)	(0.024)	(0.041)	-	
Reading comprehension	-0.54	-0.31	1.14	-	0.76
	(0.112)	(0.033)	(0.090)	-	
Creativity	-0.26	0.03	0.92	-	0.47
	(0.097)	(0.034)	(0.070)	-	
Mechanical reasoning	-0.01	0.56	0.92	-	0.53

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Table 3.9 – *Continued from previous page*

	Test Effort	**School Effort**	**Cognitive**	**Noncognitive**	**Signal**
	(0.114)	(0.041)	(0.081)	-	
Visualization in 2d	-0.27	0.07	0.70	-	0.21
	(0.081)	(0.028)	(0.059)	-	
Visualization in 3d	-0.28	0.11	0.81	-	0.29
	(0.082)	(0.030)	(0.061)	-	
Abstract reasoning	-0.46	-0.16	0.94	-	0.42
	(0.091)	(0.027)	(0.075)	-	
Arithmetic computation	-0.36	-0.49	0.58	-	0.38
	(0.059)	(0.024)	(0.046)	-	
Math grades	2.20	0.55	-1.15	-0.35	0.45
	(0.187)	(0.069)	(0.180)	(0.037)	
Science grades	2.51	0.78	-1.23	-0.34	0.55
	(0.212)	(0.075)	(0.203)	(0.039)	
Language grades	2.22	0.44	-1.27	-0.35	0.50
	(0.211)	(0.071)	(0.200)	(0.038)	

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Table 3.9 – *Continued from previous page*

	Test Effort	**School Effort**	**Cognitive**	**Noncognitive**	**Signal**
Social studies grades	2.58	0.67	-1.39	-0.35	0.60
	(0.219)	(0.082)	(0.209)	(0.043)	
English grades	2.30	0.40	-1.29	-0.34	0.62
	(0.214)	(0.078)	(0.200)	(0.040)	
Vocational grades	2.28	0.65	-1.25	-0.27	0.42
	(0.188)	(0.071)	(0.182)	(0.036)	
Business grades	2.27	0.61	-1.30	-0.29	0.42
	(0.192)	(0.058)	(0.193)	(0.039)	
Social sensitivity	-	-	-	0.77	0.59
	-	-	-	(0.004)	
Mature personality	-	0.09	0.12	0.82	0.67
	-	(0.013)	(0.010)	(0.006)	
Sociability	-	0.01	-0.01	0.66	0.42
	-	(0.010)	(0.005)	(0.005)	
Impulsiveness	-	0.24	0.05	0.44	0.14

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Table 3.9 – *Continued from previous page*

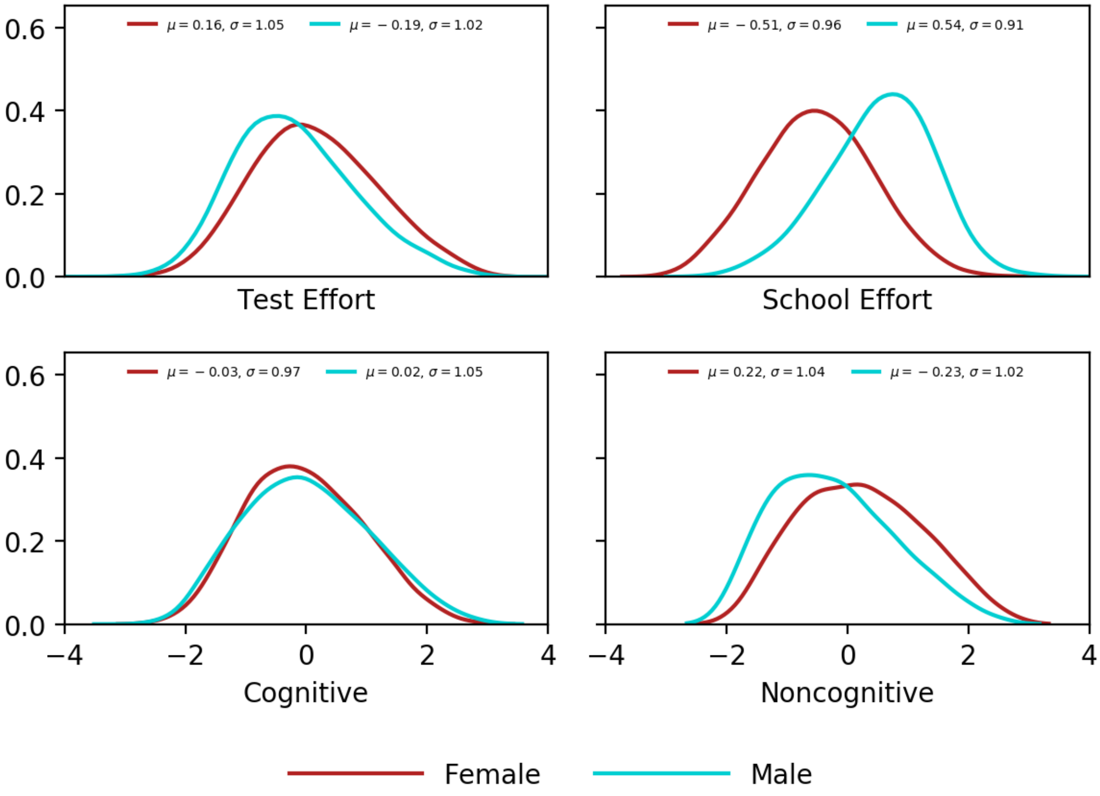
	Test Effort	**School Effort**	**Cognitive**	**Noncognitive**	**Signal**
	-	(0.010)	(0.006)	(0.008)	
Vigor	-	0.22	0.17	0.70	0.43
	-	(0.012)	(0.009)	(0.007)	
Calmness	-	0.16	0.13	0.78	0.56
	-	(0.012)	(0.008)	(0.007)	
Tidiness	-	-0.03	-0.01	0.74	0.57
	-	(0.011)	(0.006)	(0.005)	
Culture	-	-0.02	-0.01	0.77	0.60
	-	(0.011)	(0.006)	(0.006)	
Leadership	-	0.09	-0.04	0.63	0.33
	-	(0.014)	(0.009)	(0.007)	
Self-confidence	-	0.07	0.15	0.45	0.23
	-	(0.010)	(0.005)	(0.007)	

*Fixed at this value.

Table 3.10: Standard Errors of Means and Std. Deviation of Effort Baseline Factor Distributions

		Test Effort	**School Effort**	**Cognitive**	**Noncognitive**
Female	mean	0.01	0.01	0.01	0.01
	std	0.00	0.00	0.00	0.00
Male	mean	0.01	0.00	0.01	0.01
	std	0.00	0.00	0.00	0.00

Figure 3.2: Baseline Factor Marginal Distributions by Sex, Grade 9, 1960



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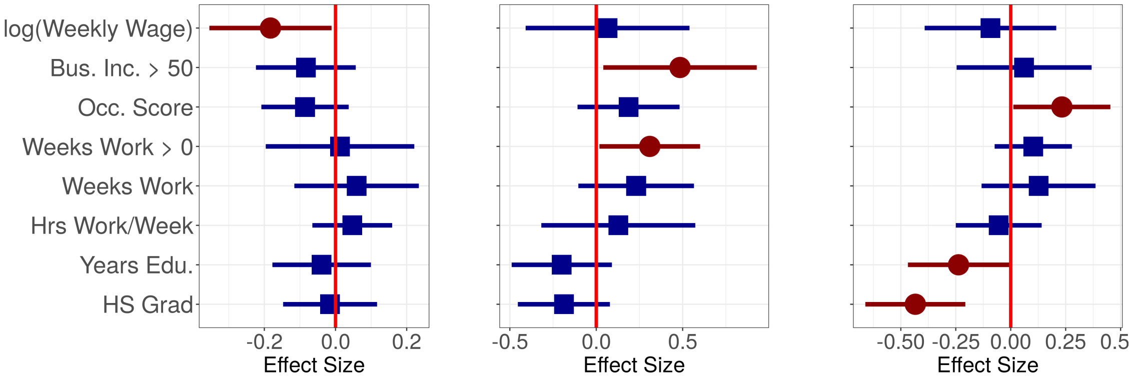
APPENDIX A

APPENDIX TO CHAPTER 1

A.1 Main Results and Robustness Checks

A.1.1 Main Specification

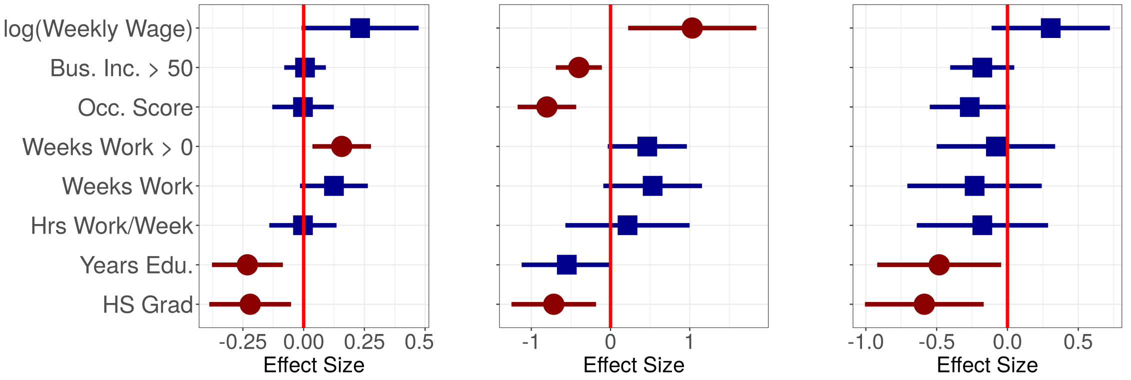
Figure A.1: Effects of Assigned School Characteristics on Immigrants



(a) School Funding per Pupil (b) Pct. Immigrant (c) Pct. Black

Note: Std. errors clustered at initial school level. Bars represent **90% confidence intervals**. Estimates from boundary change design with vector of initial school characteristics. Includes additional individual controls and age-by-ward fixed effects.

Figure A.2: Effects of Assigned School Characteristics on Natives



(a) School Funding per Pupil (b) Pct. Immigrant (c) Pct. Black

Note: Std. errors clustered at initial school level. Bars represent **90% confidence intervals**. Estimates from boundary change design with vector of initial school characteristics. Includes additional individual controls and age-by-ward fixed effects.

Table A.1: Effects on Years Education

	Immigrants	
	(1)	(2)
log(School fund)	−0.895	−1.305
	(1.935)	(1.793)
Pct. Black	−0.073*	−0.053
	(0.043)	(0.053)
Pct. Imm. White	−0.030	−0.039
	(0.026)	(0.025)
Median SEI	−0.030	−0.031
	(0.051)	(0.050)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.396	0.331
Num. obs.	7171	7554

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	−5.785***	−4.919*
	(2.218)	(2.628)
Pct. Black	−0.115*	−0.110*
	(0.063)	(0.058)
Pct. Imm. White	−0.084	−0.080**
	(0.053)	(0.032)
Median SEI	−0.081	−0.046
	(0.111)	(0.111)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.501	0.411
Num. obs.	3669	3864

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.2: Effects on High School Grad

	Immigrants	
	(1)	(2)
log(School fund)	−0.064	−0.068
	(0.340)	(0.344)
Pct. Black	−0.024***	−0.017*
	(0.008)	(0.010)
Pct. Imm. White	−0.005	−0.006
	(0.004)	(0.005)
Median SEI	−0.008	−0.007
	(0.008)	(0.010)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.364	0.299
Num. obs.	7197	7584

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	−0.988**	−0.733
	(0.460)	(0.488)
Pct. Black	−0.025**	−0.025***
	(0.011)	(0.009)
Pct. Imm. White	−0.020**	−0.021***
	(0.009)	(0.005)
Median SEI	−0.027	−0.022
	(0.019)	(0.019)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.467	0.393
Num. obs.	3688	3884

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.3: Effects on Weeks Work > 0

	Immigrants	
	(1)	(2)
log(School fund)	0.031	−0.009
	(0.306)	(0.299)
Pct. Black	0.003	0.003
	(0.003)	(0.003)
Pct. Imm. White	0.005*	0.003
	(0.003)	(0.003)
Median SEI	0.009	0.010
	(0.006)	(0.006)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.219	0.186
Num. obs.	7197	7584

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	0.355**	0.316**
	(0.166)	(0.133)
Pct. Black	−0.002	−0.004
	(0.005)	(0.005)
Pct. Imm. White	0.006	0.002
	(0.004)	(0.003)
Median SEI	−0.003	−0.005
	(0.011)	(0.009)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.359	0.286
Num. obs.	3688	3884

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.4: Effects on Weeks Work

	Immigrants	
	(1)	(2)
log(School fund)	8.726	6.044
	(15.665)	(15.746)
Pct. Black	0.248	0.192
	(0.308)	(0.246)
Pct. Imm. White	0.222	0.197
	(0.195)	(0.218)
Median SEI	0.310	0.365
	(0.329)	(0.345)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.225	0.186
Num. obs.	7197	7584

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	17.673	14.836
	(12.025)	(11.794)
Pct. Black	−0.313	−0.349
	(0.389)	(0.353)
Pct. Imm. White	0.460	0.102
	(0.328)	(0.295)
Median SEI	−0.635	−0.374
	(0.677)	(0.578)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.363	0.293
Num. obs.	3688	3884

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.5: Effects on Hrs Work

	Immigrants	
	(1)	(2)
log(School fund)	7.320	5.107
	(10.605)	(10.338)
Pct. Black	−0.113	0.048
	(0.245)	(0.217)
Pct. Imm. White	0.130	0.139
	(0.275)	(0.252)
Median SEI	−0.101	0.105
	(0.336)	(0.339)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.246	0.206
Num. obs.	7197	7584

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	−0.463	−3.342
	(13.217)	(11.903)
Pct. Black	−0.264	-0.674^{**}
	(0.421)	(0.330)
Pct. Imm. White	0.205	−0.427
	(0.457)	(0.325)
Median SEI	−0.286	−0.566
	(0.715)	(0.590)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.372	0.303
Num. obs.	3688	3884

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.6: Effects on Bus. Inc. > 50

	Immigrants	
	(1)	(2)
log(School fund)	−0.253	−0.260
	(0.259)	(0.254)
Pct. Black	0.002	0.004
	(0.008)	(0.008)
Pct. Imm. White	0.010^{*}	0.012^{**}
	(0.005)	(0.006)
Median SEI	0.013^{**}	0.017^{**}
	(0.006)	(0.007)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.262	0.213
Num. obs.	7122	7502

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	0.018	0.369
	(0.163)	(0.224)
Pct. Black	−0.005	0.003
	(0.004)	(0.006)
Pct. Imm. White	-0.008^{**}	0.000
	(0.003)	(0.004)
Median SEI	−0.007	0.008
	(0.008)	(0.009)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.404	0.329
Num. obs.	3641	3832

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.7: Effects on Weekly Wages

	Immigrants	
	(1)	(2)
log(School fund)	-0.813^{*}	-0.803^{*}
	(0.464)	(0.454)
Pct. Black	-0.005	-0.009
	(0.011)	(0.012)
Pct. Imm. White	0.002	-0.002
	(0.008)	(0.008)
Median SEI	0.009	0.009
	(0.012)	(0.012)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.295	0.246
Num. obs.	5885	6207

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	1.099	0.774
	(0.694)	(0.649)
Pct. Black	0.014	0.008
	(0.011)	(0.011)
Pct. Imm. White	0.030^{**}	0.012
	(0.014)	(0.013)
Median SEI	0.012	0.010
	(0.024)	(0.022)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.445	0.370
Num. obs.	3167	3336

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.8: Effects on Occ. Score

	Immigrants	
	(1)	(2)
log(School fund)	-7.194	-9.135
	(6.263)	(6.505)
Pct. Black	0.260^{*}	0.286^{**}
	(0.150)	(0.132)
Pct. Imm. White	0.102	0.125
	(0.098)	(0.109)
Median SEI	0.048	0.106
	(0.227)	(0.242)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.302	0.255
Num. obs.	6963	7340

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

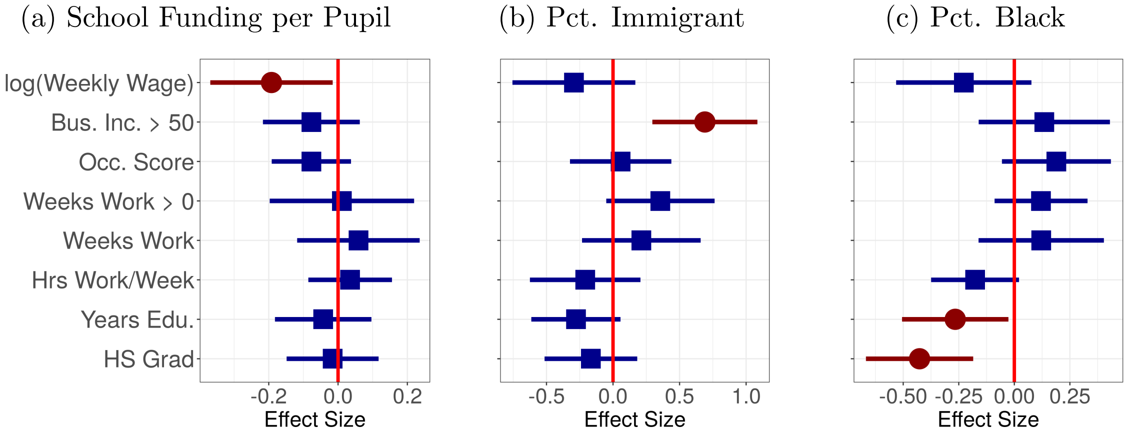
	Natives	
	(1)	(2)
log(School fund)	-0.229	8.434
	(6.852)	(6.305)
Pct. Black	-0.226	0.028
	(0.145)	(0.147)
Pct. Imm. White	-0.436^{***}	-0.151
	(0.122)	(0.116)
Median SEI	0.091	0.482^{**}
	(0.269)	(0.238)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.376	0.312
Num. obs.	3584	3772

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

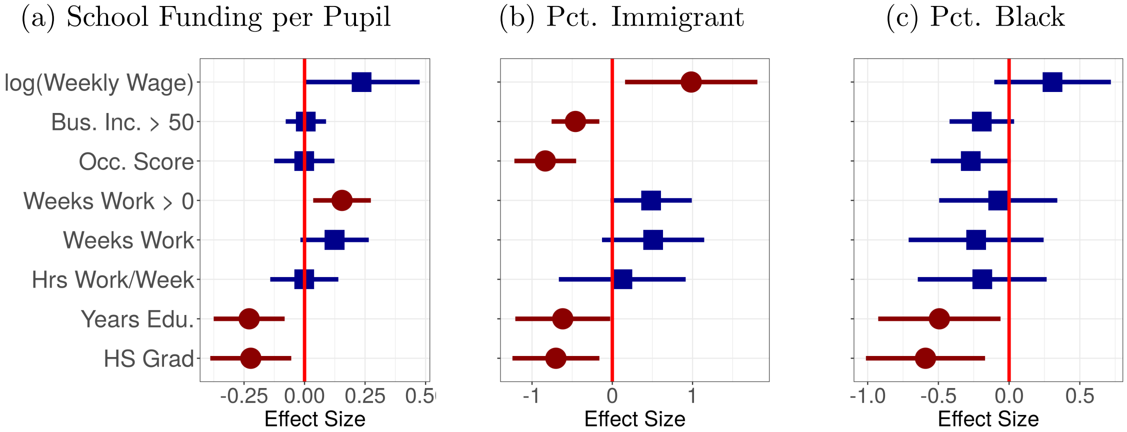
A.1.2 Higher Threshold for School Matches

Figure A.3: Effects of Assigned School Characteristics on Immigrants



Note: Std. errors clustered at initial school level. Bars represent **90% confidence intervals**. Estimates from boundary change design with vector of initial school characteristics. Includes additional individual controls and age-by-ward fixed effects.

Figure A.4: Effects of Assigned School Characteristics on Natives



Note: Std. errors clustered at initial school level. Bars represent **90% confidence intervals**. Estimates from boundary change design with vector of initial school characteristics. Includes additional individual controls and age-by-ward fixed effects.

Table A.9: Effects on Years Education

	Immigrants	
	(1)	(2)
log(School fund)	−0.990	−1.325
	(1.953)	(1.802)
Pct. Black	−0.081*	−0.056
	(0.044)	(0.056)
Pct. Imm. White	−0.042	−0.043
	(0.031)	(0.032)
Median SEI	−0.039	−0.034
	(0.052)	(0.052)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.397	0.332
Num. obs.	7128	7508

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	−5.727**	−4.975*
	(2.231)	(2.640)
Pct. Black	−0.117*	−0.097
	(0.062)	(0.065)
Pct. Imm. White	−0.094*	−0.052
	(0.055)	(0.062)
Median SEI	−0.076	−0.044
	(0.111)	(0.116)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.502	0.412
Num. obs.	3639	3833

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.10: Effects on High School Grad

	Immigrants	
	(1)	(2)
log(School fund)	−0.067	−0.058
	(0.343)	(0.347)
Pct. Black	−0.024***	−0.015
	(0.008)	(0.010)
Pct. Imm. White	−0.005	−0.003
	(0.006)	(0.006)
Median SEI	−0.008	−0.005
	(0.009)	(0.010)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.364	0.301
Num. obs.	7154	7538

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	−1.000**	−0.743
	(0.458)	(0.494)
Pct. Black	−0.025**	−0.022**
	(0.011)	(0.011)
Pct. Imm. White	−0.019**	−0.014
	(0.009)	(0.009)
Median SEI	−0.028	−0.021
	(0.019)	(0.020)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.468	0.394
Num. obs.	3658	3853

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.11: Effects on Weeks Work > 0

	Immigrants	
	(1)	(2)
log(School fund)	0.027	−0.004
	(0.307)	(0.301)
Pct. Black	0.004	0.004
	(0.004)	(0.004)
Pct. Imm. White	0.006	0.004
	(0.004)	(0.004)
Median SEI	0.009	0.010
	(0.007)	(0.007)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.219	0.186
Num. obs.	7154	7538

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	0.352**	0.313**
	(0.165)	(0.132)
Pct. Black	−0.002	−0.003
	(0.005)	(0.005)
Pct. Imm. White	0.007	0.004
	(0.004)	(0.004)
Median SEI	−0.003	−0.005
	(0.011)	(0.009)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.361	0.289
Num. obs.	3658	3853

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.12: Effects on Weeks Work

	Immigrants			Natives	
	(1)	(2)		(1)	(2)
log(School fund)	8.703	6.562	log(School fund)	17.621	14.706
	(15.916)	(15.896)		(12.197)	(10.385)
Pct. Black	0.237	0.210	Pct. Black	−0.314	−0.138
	(0.335)	(0.273)		(0.390)	(0.355)
Pct. Imm. White	0.204	0.219	Pct. Imm. White	0.440	0.469
	(0.260)	(0.264)		(0.335)	(0.308)
Median SEI	0.304	0.402	Median SEI	−0.614	−0.217
	(0.364)	(0.365)		(0.683)	(0.572)
Ind. Controls	Yes	No	Ind. Controls	Yes	No
Age-Ward FE	Yes	No	Age-Ward FE	Yes	No
R^2	0.226	0.187	R^2	0.366	0.296
Num. obs.	7154	7538	Num. obs.	3658	3853
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$			$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$		

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.13: Effects on Hrs Work

	Immigrants			Natives	
	(1)	(2)		(1)	(2)
log(School fund)	5.459	3.901	log(School fund)	−0.172	−2.886
	(11.467)	(11.206)		(13.440)	(10.876)
Pct. Black	−0.365	−0.167	Pct. Black	−0.283	−0.406
	(0.248)	(0.230)		(0.412)	(0.334)
Pct. Imm. White	−0.212	−0.146	Pct. Imm. White	0.119	−0.035
	(0.256)	(0.255)		(0.460)	(0.395)
Median SEI	−0.377	−0.113	Median SEI	−0.236	−0.274
	(0.349)	(0.358)		(0.719)	(0.566)
Ind. Controls	Yes	No	Ind. Controls	Yes	No
Age-Ward FE	Yes	No	Age-Ward FE	Yes	No
R^2	0.248	0.207	R^2	0.376	0.307
Num. obs.	7154	7538	Num. obs.	3658	3853
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$			$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$		

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.14: Effects on Bus. Inc. > 50

	Immigrants			Natives	
	(1)	(2)		(1)	(2)
log(School fund)	−0.235	−0.246	log(School fund)	0.017	0.372*
	(0.259)	(0.255)		(0.159)	(0.221)
Pct. Black	0.005	0.007	Pct. Black	−0.006	0.002
	(0.007)	(0.007)		(0.004)	(0.007)
Pct. Imm. White	0.014***	0.016***	Pct. Imm. White	−0.009**	−0.002
	(0.005)	(0.005)		(0.003)	(0.006)
Median SEI	0.016***	0.020***	Median SEI	−0.007	0.008
	(0.006)	(0.006)		(0.008)	(0.010)
Ind. Controls	Yes	No	Ind. Controls	Yes	No
Age-Ward FE	Yes	No	Age-Ward FE	Yes	No
R^2	0.262	0.212	R^2	0.405	0.328
Num. obs.	7079	7456	Num. obs.	3611	3801
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$			$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$		

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.15: Effects on Weekly Wages

Immigrants		
	(1)	(2)
log(School fund)	-0.859^{*}	-0.823^{*}
	(0.480)	(0.474)
Pct. Black	-0.013	-0.016
	(0.011)	(0.012)
Pct. Imm. White	-0.009	-0.011
	(0.008)	(0.009)
Median SEI	0.001	0.002
	(0.012)	(0.013)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.297	0.247
Num. obs.	5847	6167

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Natives		
	(1)	(2)
log(School fund)	1.121	0.780
	(0.691)	(0.680)
Pct. Black	0.014	0.013
	(0.011)	(0.012)
Pct. Imm. White	0.028^{*}	0.020
	(0.015)	(0.018)
Median SEI	0.014	0.016
	(0.024)	(0.022)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.445	0.371
Num. obs.	3139	3307

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.16: Effects on Occ. Score

Immigrants		
	(1)	(2)
log(School fund)	-6.501	-8.245
	(5.870)	(6.139)
Pct. Black	0.211	0.257^{*}
	(0.166)	(0.146)
Pct. Imm. White	0.031	0.083
	(0.127)	(0.130)
Median SEI	0.041	0.125
	(0.237)	(0.245)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.305	0.257
Num. obs.	6920	7294

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

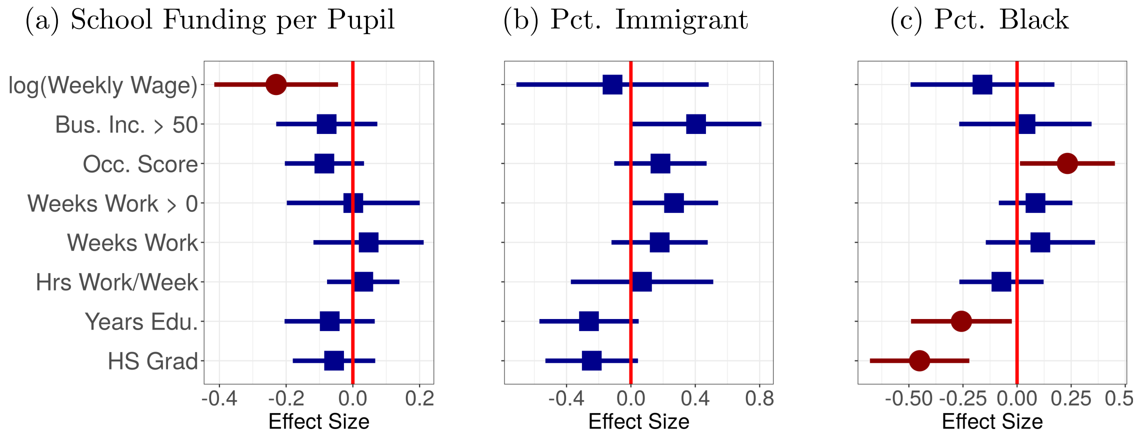
Natives		
	(1)	(2)
log(School fund)	-0.089	7.948
	(6.754)	(6.294)
Pct. Black	-0.229	0.001
	(0.145)	(0.175)
Pct. Imm. White	-0.454^{***}	-0.181
	(0.127)	(0.171)
Median SEI	0.103	0.438^{*}
	(0.268)	(0.264)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.376	0.313
Num. obs.	3554	3741

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

A.1.3 Lower Threshold for School Matches

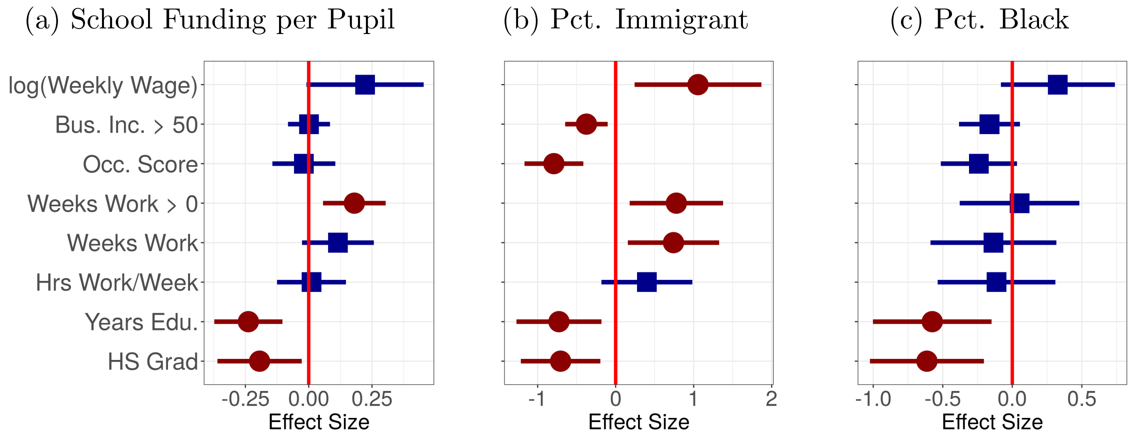
Figure A.5: Effects of Assigned School Characteristics on Immigrants



(a) School Funding per Pupil (b) Pct. Immigrant (c) Pct. Black

Note: Std. errors clustered at initial school level. Bars represent **90% confidence intervals**. Estimates from boundary change design with vector of initial school characteristics. Includes additional individual controls and age-by-ward fixed effects.

Figure A.6: Effects of Assigned School Characteristics on Natives



(a) School Funding per Pupil (b) Pct. Immigrant (c) Pct. Black

Note: Std. errors clustered at initial school level. Bars represent **90% confidence intervals**. Estimates from boundary change design with vector of initial school characteristics. Includes additional individual controls and age-by-ward fixed effects.

Table A.17: Effects on Years Education

	Immigrants	
	(1)	(2)
log(School fund)	−1.599	−1.967
	(1.889)	(1.755)
Pct. Black	-0.079^{*}	−0.059
	(0.043)	(0.053)
Pct. Imm. White	−0.039	-0.048^{*}
	(0.028)	(0.027)
Median SEI	−0.036	−0.037
	(0.049)	(0.049)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.395	0.330
Num. obs.	7213	7597

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	-5.956^{***}	-4.690^{*}
	(2.046)	(2.473)
Pct. Black	-0.137^{**}	-0.123^{**}
	(0.062)	(0.057)
Pct. Imm. White	-0.111^{**}	-0.094^{***}
	(0.051)	(0.030)
Median SEI	−0.113	−0.066
	(0.106)	(0.107)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.498	0.408
Num. obs.	3695	3892

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.18: Effects on High School Grad

	Immigrants	
	(1)	(2)
log(School fund)	−0.239	−0.252
	(0.318)	(0.331)
Pct. Black	-0.025^{***}	-0.018^{*}
	(0.008)	(0.010)
Pct. Imm. White	−0.007	−0.007
	(0.005)	(0.005)
Median SEI	−0.010	−0.008
	(0.008)	(0.010)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.363	0.298
Num. obs.	7239	7627

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	-0.870^{*}	−0.580
	(0.454)	(0.479)
Pct. Black	-0.026^{**}	-0.027^{***}
	(0.011)	(0.009)
Pct. Imm. White	-0.019^{**}	-0.021^{***}
	(0.008)	(0.005)
Median SEI	-0.031^{*}	−0.025
	(0.018)	(0.018)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.465	0.392
Num. obs.	3714	3912

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.19: Effects on Weeks Work > 0

	Immigrants	
	(1)	(2)
log(School fund)	0.003	−0.034
	(0.292)	(0.281)
Pct. Black	0.003	0.003
	(0.003)	(0.003)
Pct. Imm. White	0.004	0.003
	(0.003)	(0.003)
Median SEI	0.008	0.009
	(0.006)	(0.006)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.219	0.186
Num. obs.	7239	7627

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	0.407^{**}	0.371^{***}
	(0.170)	(0.141)
Pct. Black	0.001	−0.003
	(0.006)	(0.005)
Pct. Imm. White	0.011^{**}	0.005
	(0.005)	(0.004)
Median SEI	−0.000	−0.004
	(0.010)	(0.009)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.358	0.285
Num. obs.	3714	3912

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.20: Effects on Weeks Work

	Immigrants	
	(1)	(2)
log(School fund)	6.924	3.923
	(14.781)	(14.575)
Pct. Black	0.211	0.164
	(0.301)	(0.233)
Pct. Imm. White	0.171	0.152
	(0.175)	(0.195)
Median SEI	0.292	0.359
	(0.318)	(0.333)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.223	0.185
Num. obs.	7239	7627

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	16.323	15.630
	(12.196)	(11.769)
Pct. Black	−0.183	−0.254
	(0.372)	(0.358)
Pct. Imm. White	0.641**	0.220
	(0.307)	(0.340)
Median SEI	−0.474	−0.258
	(0.667)	(0.573)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.362	0.293
Num. obs.	3714	3912

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.21: Effects on Hrs Work

	Immigrants	
	(1)	(2)
log(School fund)	4.841	1.757
	(10.264)	(9.807)
Pct. Black	−0.149	0.020
	(0.245)	(0.213)
Pct. Imm. White	0.070	0.088
	(0.274)	(0.247)
Median SEI	−0.093	0.107
	(0.340)	(0.339)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.245	0.205
Num. obs.	7239	7627

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	1.723	−0.127
	(12.992)	(11.562)
Pct. Black	−0.170	−0.612*
	(0.386)	(0.328)
Pct. Imm. White	0.384	−0.325
	(0.340)	(0.323)
Median SEI	−0.220	−0.525
	(0.696)	(0.586)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.371	0.303
Num. obs.	3714	3912

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.22: Effects on Bus. Inc. > 50

	Immigrants	
	(1)	(2)
log(School fund)	−0.237	−0.249
	(0.279)	(0.271)
Pct. Black	0.002	0.003
	(0.008)	(0.007)
Pct. Imm. White	0.008	0.010**
	(0.005)	(0.005)
Median SEI	0.015**	0.018***
	(0.006)	(0.006)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.258	0.209
Num. obs.	7164	7545

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	0.004	0.360*
	(0.157)	(0.214)
Pct. Black	−0.005	0.003
	(0.004)	(0.005)
Pct. Imm. White	−0.007**	0.000
	(0.003)	(0.004)
Median SEI	−0.007	0.008
	(0.008)	(0.009)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.404	0.328
Num. obs.	3667	3860

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.23: Effects on Weekly Wages

	Immigrants	
	(1)	(2)
log(School fund)	−1.023**	−1.020**
	(0.502)	(0.487)
Pct. Black	−0.009	−0.013
	(0.012)	(0.013)
Pct. Imm. White	−0.003	−0.007
	(0.011)	(0.010)
Median SEI	0.006	0.005
	(0.013)	(0.014)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.294	0.245
Num. obs.	5923	6245

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	1.050	0.756
	(0.664)	(0.620)
Pct. Black	0.015	0.008
	(0.011)	(0.010)
Pct. Imm. White	0.030**	0.013
	(0.014)	(0.013)
Median SEI	0.013	0.011
	(0.023)	(0.021)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.442	0.368
Num. obs.	3189	3360

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.24: Effects on Occ. Score

	Immigrants	
	(1)	(2)
log(School fund)	−7.153	−9.312
	(6.055)	(6.196)
Pct. Black	0.260*	0.286**
	(0.149)	(0.131)
Pct. Imm. White	0.101	0.121
	(0.096)	(0.109)
Median SEI	0.059	0.115
	(0.222)	(0.236)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.302	0.255
Num. obs.	7005	7383

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

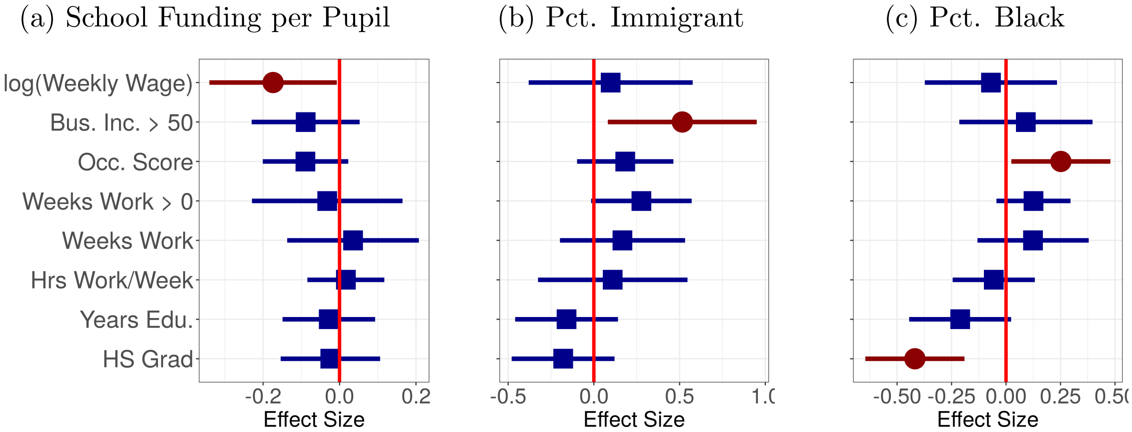
	Natives	
	(1)	(2)
log(School fund)	−1.703	7.578
	(6.696)	(6.120)
Pct. Black	−0.203	0.037
	(0.141)	(0.146)
Pct. Imm. White	−0.428***	−0.147
	(0.124)	(0.116)
Median SEI	0.138	0.503**
	(0.255)	(0.237)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.375	0.311
Num. obs.	3609	3799

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

A.1.4 No Sample Weights

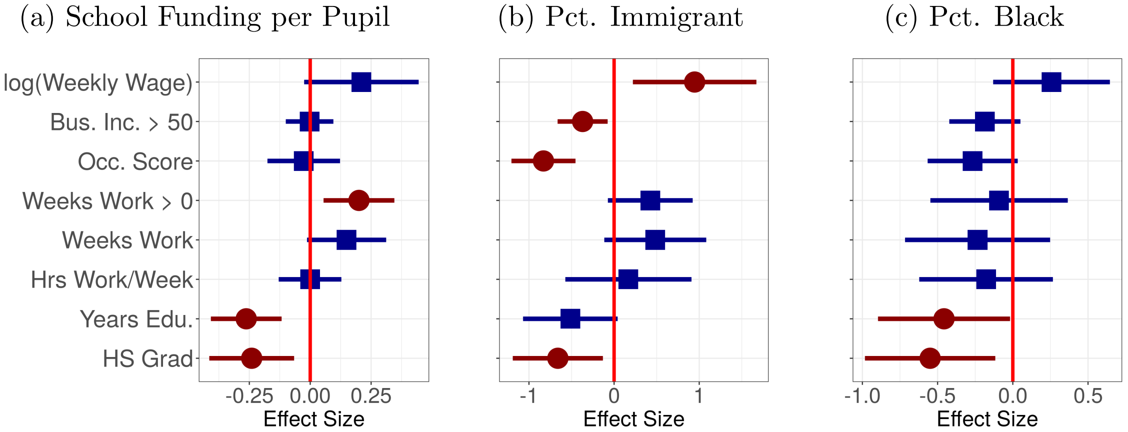
Figure A.7: Effects of Assigned School Characteristics on Immigrants



(a) School Funding per Pupil (b) Pct. Immigrant (c) Pct. Black

Note: Std. errors clustered at initial school level. Bars represent **90% confidence intervals**. Estimates from boundary change design with vector of initial school characteristics. Includes additional individual controls and age-by-ward fixed effects.

Figure A.8: Effects of Assigned School Characteristics on Natives



(a) School Funding per Pupil (b) Pct. Immigrant (c) Pct. Black

Note: Std. errors clustered at initial school level. Bars represent **90% confidence intervals**. Estimates from boundary change design with vector of initial school characteristics. Includes additional individual controls and age-by-ward fixed effects.

Table A.25: Effects on Years Education

	Immigrants	
	(1)	(2)
log(School fund)	−0.645	−0.668
	(1.695)	(1.521)
Pct. Black	−0.064	−0.041
	(0.043)	(0.052)
Pct. Imm. White	−0.024	−0.033
	(0.027)	(0.027)
Median SEI	0.005	0.008
	(0.052)	(0.048)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.390	0.323
Num. obs.	7198	7599

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	−6.561***	−5.263**
	(2.206)	(2.487)
Pct. Black	−0.108*	−0.107*
	(0.063)	(0.058)
Pct. Imm. White	−0.078	−0.078**
	(0.051)	(0.034)
Median SEI	−0.082	−0.038
	(0.113)	(0.112)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.489	0.400
Num. obs.	3679	3881

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.26: Effects on High School Grad

	Immigrants	
	(1)	(2)
log(School fund)	−0.101	−0.056
	(0.335)	(0.320)
Pct. Black	−0.023***	−0.015
	(0.008)	(0.009)
Pct. Imm. White	−0.005	−0.006
	(0.005)	(0.005)
Median SEI	−0.005	−0.004
	(0.009)	(0.009)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.353	0.290
Num. obs.	7224	7629

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	−1.080**	−0.789*
	(0.476)	(0.476)
Pct. Black	−0.023**	−0.024**
	(0.011)	(0.009)
Pct. Imm. White	−0.018**	−0.020***
	(0.009)	(0.006)
Median SEI	−0.024	−0.019
	(0.020)	(0.019)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.455	0.380
Num. obs.	3698	3901

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.27: Effects on Weeks Work > 0

	Immigrants	
	(1)	(2)
log(School fund)	−0.078	−0.109
	(0.289)	(0.284)
Pct. Black	0.004	0.003
	(0.003)	(0.003)
Pct. Imm. White	0.004	0.003
	(0.003)	(0.003)
Median SEI	0.009	0.010
	(0.006)	(0.006)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.215	0.181
Num. obs.	7224	7629

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	0.453**	0.376**
	(0.200)	(0.174)
Pct. Black	−0.002	−0.005
	(0.006)	(0.005)
Pct. Imm. White	0.006	0.002
	(0.004)	(0.003)
Median SEI	−0.000	−0.006
	(0.011)	(0.010)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.344	0.274
Num. obs.	3698	3901

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.28: Effects on Weeks Work

	Immigrants	
	(1)	(2)
log(School fund)	5.211	3.379
	(15.445)	(15.559)
Pct. Black	0.241	0.151
	(0.302)	(0.251)
Pct. Imm. White	0.161	0.118
	(0.213)	(0.218)
Median SEI	0.395	0.440
	(0.356)	(0.368)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.218	0.180
Num. obs.	7224	7629

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	21.163	17.394
	(13.996)	(13.548)
Pct. Black	−0.315	−0.337
	(0.394)	(0.360)
Pct. Imm. White	0.418	0.123
	(0.314)	(0.292)
Median SEI	−0.453	−0.373
	(0.670)	(0.588)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.348	0.279
Num. obs.	3698	3901

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.29: Effects on Hrs Work

	Immigrants	
	(1)	(2)
log(School fund)	2.576	2.693
	(9.535)	(9.132)
Pct. Black	−0.116	−0.019
	(0.235)	(0.218)
Pct. Imm. White	0.112	0.106
	(0.269)	(0.246)
Median SEI	−0.028	0.161
	(0.315)	(0.332)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.240	0.199
Num. obs.	7224	7629

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	−0.118	−4.161
	(12.295)	(11.646)
Pct. Black	−0.265	−0.615*
	(0.403)	(0.327)
Pct. Imm. White	0.161	−0.380
	(0.432)	(0.321)
Median SEI	−0.047	−0.382
	(0.665)	(0.575)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.363	0.294
Num. obs.	3698	3901

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.30: Effects on Bus. Inc. > 50

	Immigrants	
	(1)	(2)
log(School fund)	−0.268	−0.246
	(0.260)	(0.260)
Pct. Black	0.004	0.005
	(0.007)	(0.007)
Pct. Imm. White	0.010*	0.012**
	(0.005)	(0.005)
Median SEI	0.015**	0.019***
	(0.006)	(0.006)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.252	0.205
Num. obs.	7149	7547

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	−0.008	0.299
	(0.186)	(0.201)
Pct. Black	−0.006	0.003
	(0.004)	(0.006)
Pct. Imm. White	−0.007**	−0.000
	(0.003)	(0.004)
Median SEI	−0.012	0.005
	(0.009)	(0.009)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.396	0.321
Num. obs.	3651	3848

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.31: Effects on Weekly Wages

	Immigrants	
	(1)	(2)
log(School fund)	−0.773*	−0.723
	(0.451)	(0.488)
Pct. Black	−0.004	−0.007
	(0.011)	(0.012)
Pct. Imm. White	0.003	−0.001
	(0.008)	(0.008)
Median SEI	0.018	0.015
	(0.011)	(0.013)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.289	0.241
Num. obs.	5908	6246

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	0.996	0.828
	(0.677)	(0.646)
Pct. Black	0.012	0.005
	(0.011)	(0.010)
Pct. Imm. White	0.027**	0.011
	(0.013)	(0.012)
Median SEI	0.009	0.012
	(0.024)	(0.020)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.432	0.356
Num. obs.	3176	3351

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.32: Effects on Occ. Score

	Immigrants	
	(1)	(2)
log(School fund)	−7.464	−8.544
	(5.701)	(5.943)
Pct. Black	0.278*	0.270**
	(0.153)	(0.128)
Pct. Imm. White	0.100	0.113
	(0.093)	(0.100)
Median SEI	0.176	0.223
	(0.217)	(0.224)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.301	0.253
Num. obs.	6987	7382

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

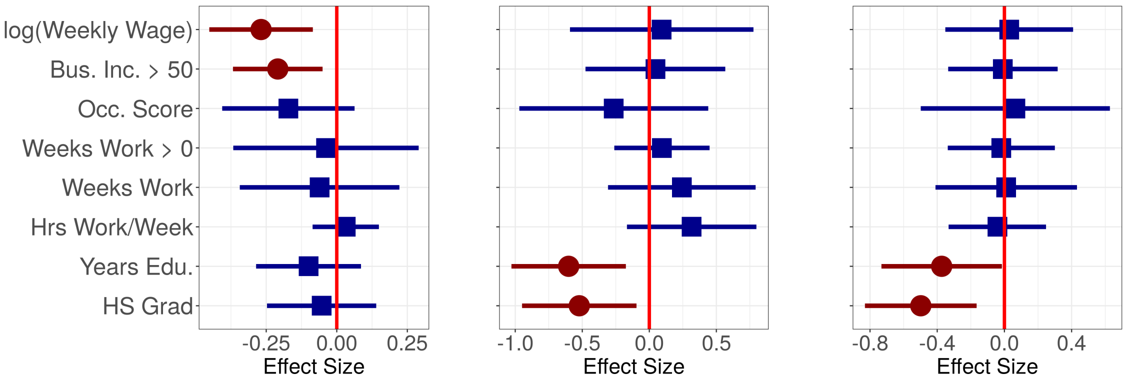
	Natives	
	(1)	(2)
log(School fund)	−2.365	6.961
	(8.050)	(6.494)
Pct. Black	−0.225	0.023
	(0.153)	(0.149)
Pct. Imm. White	−0.449***	−0.163
	(0.124)	(0.117)
Median SEI	0.007	0.498**
	(0.296)	(0.241)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.368	0.307
Num. obs.	3594	3788

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

A.1.5 Exclude non-Catholic Immigrants

Figure A.9: Effects of Assigned School Characteristics on Immigrants



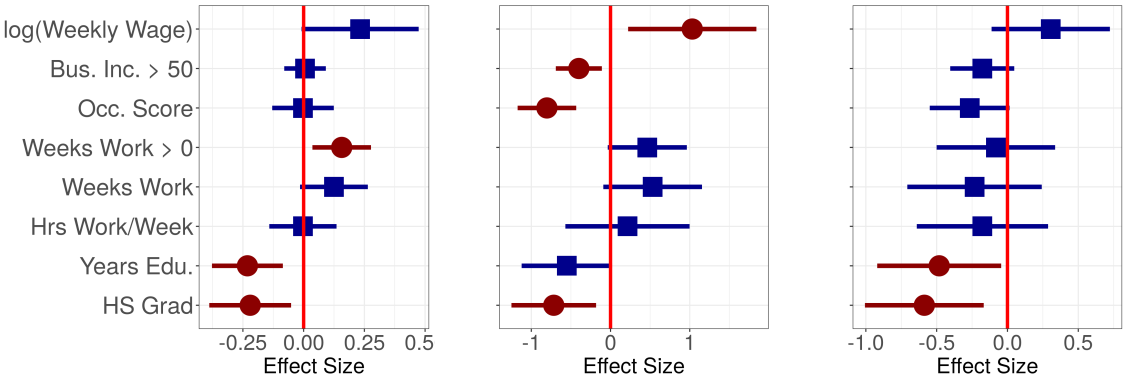
(a) School Funding per Pupil

(b) Pct. Immigrant

(c) Pct. Black

Note: Std. errors clustered at initial school level. Bars represent **90% confidence intervals**. Estimates from boundary change design with vector of initial school characteristics. Includes additional individual controls and age-by-ward fixed effects.

Figure A.10: Effects of Assigned School Characteristics on Natives



(a) School Funding per Pupil

(b) Pct. Immigrant

(c) Pct. Black

Note: Std. errors clustered at initial school level. Bars represent **90% confidence intervals**. Estimates from boundary change design with vector of initial school characteristics. Includes additional individual controls and age-by-ward fixed effects.

Table A.33: Effects on Years Education

	Immigrants			Natives	
	(1)	(2)		(1)	(2)
log(School fund)	-2.516	-3.267	log(School fund)	-5.785^{***}	-4.919^{*}
	(2.843)	(2.543)		(2.218)	(2.628)
Pct. Black	-0.118^{*}	-0.132	Pct. Black	-0.115^{*}	-0.110^{*}
	(0.069)	(0.080)		(0.063)	(0.058)
Pct. Imm. White	-0.091^{**}	-0.081^{*}	Pct. Imm. White	-0.084	-0.080^{**}
	(0.039)	(0.049)		(0.053)	(0.032)
Median SEI	-0.217^{*}	-0.143	Median SEI	-0.081	-0.046
	(0.119)	(0.126)		(0.111)	(0.111)
Ind. Controls	Yes	No	Ind. Controls	Yes	No
Age-Ward FE	Yes	No	Age-Ward FE	Yes	No
R^2	0.500	0.425	R^2	0.501	0.411
Num. obs.	3573	3733	Num. obs.	3669	3864
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$			$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$		

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.34: Effects on High School Grad

	Immigrants			Natives	
	(1)	(2)		(1)	(2)
log(School fund)	-0.243	-0.348	log(School fund)	-0.988^{**}	-0.733
	(0.533)	(0.425)		(0.460)	(0.488)
Pct. Black	-0.028^{**}	-0.026^{**}	Pct. Black	-0.025^{**}	-0.025^{***}
	(0.012)	(0.013)		(0.011)	(0.009)
Pct. Imm. White	-0.014^{**}	-0.012	Pct. Imm. White	-0.020^{**}	-0.021^{***}
	(0.007)	(0.008)		(0.009)	(0.005)
Median SEI	-0.024	-0.009	Median SEI	-0.027	-0.022
	(0.021)	(0.023)		(0.019)	(0.019)
Ind. Controls	Yes	No	Ind. Controls	Yes	No
Age-Ward FE	Yes	No	Age-Ward FE	Yes	No
R^2	0.473	0.402	R^2	0.467	0.393
Num. obs.	3584	3746	Num. obs.	3688	3884
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$			$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$		

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.35: Effects on Weeks Work > 0

	Immigrants			Natives	
	(1)	(2)		(1)	(2)
log(School fund)	-0.093	-0.019	log(School fund)	0.355^{**}	0.316^{**}
	(0.484)	(0.478)		(0.166)	(0.133)
Pct. Black	-0.001	0.001	Pct. Black	-0.002	-0.004
	(0.006)	(0.004)		(0.005)	(0.005)
Pct. Imm. White	0.001	0.001	Pct. Imm. White	0.006	0.002
	(0.003)	(0.003)		(0.004)	(0.003)
Median SEI	-0.003	0.002	Median SEI	-0.003	-0.005
	(0.009)	(0.009)		(0.011)	(0.009)
Ind. Controls	Yes	No	Ind. Controls	Yes	No
Age-Ward FE	Yes	No	Age-Ward FE	Yes	No
R^2	0.337	0.276	R^2	0.359	0.286
Num. obs.	3584	3746	Num. obs.	3688	3884
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$			$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$		

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.36: Effects on Weeks Work

	Immigrants	
	(1)	(2)
log(School fund)	−9.187	−3.894
	(25.948)	(26.725)
Pct. Black	0.021	0.031
	(0.487)	(0.397)
Pct. Imm. White	0.221	0.236
	(0.304)	(0.315)
Median SEI	−0.334	−0.015
	(0.873)	(0.743)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.361	0.290
Num. obs.	3584	3746

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	17.673	14.836
	(12.025)	(11.794)
Pct. Black	−0.313	−0.349
	(0.389)	(0.353)
Pct. Imm. White	0.460	0.102
	(0.328)	(0.295)
Median SEI	−0.635	−0.374
	(0.677)	(0.578)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.363	0.293
Num. obs.	3688	3884

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.37: Effects on Hrs Work

	Immigrants	
	(1)	(2)
log(School fund)	5.132	9.229
	(11.523)	(11.575)
Pct. Black	−0.086	0.173
	(0.358)	(0.334)
Pct. Imm. White	0.307	0.317
	(0.285)	(0.282)
Median SEI	0.101	0.534
	(0.500)	(0.606)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.367	0.309
Num. obs.	3584	3746

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	−0.463	−3.342
	(13.217)	(11.903)
Pct. Black	−0.264	−0.674**
	(0.421)	(0.330)
Pct. Imm. White	0.205	−0.427
	(0.457)	(0.325)
Median SEI	−0.286	−0.566
	(0.715)	(0.590)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.372	0.303
Num. obs.	3688	3884

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.38: Effects on Bus. Inc. > 50

	Immigrants	
	(1)	(2)
log(School fund)	−0.715**	−0.660**
	(0.330)	(0.297)
Pct. Black	−0.000	0.004
	(0.009)	(0.008)
Pct. Imm. White	0.001	0.008
	(0.007)	(0.008)
Median SEI	−0.005	0.003
	(0.013)	(0.015)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.393	0.323
Num. obs.	3545	3706

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	0.018	0.369
	(0.163)	(0.224)
Pct. Black	−0.005	0.003
	(0.004)	(0.006)
Pct. Imm. White	−0.008**	0.000
	(0.003)	(0.004)
Median SEI	−0.007	0.008
	(0.008)	(0.009)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.404	0.329
Num. obs.	3641	3832

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.39: Effects on Weekly Wages

	Immigrants	
	(1)	(2)
log(School fund)	−1.229**	−1.071***
	(0.511)	(0.285)
Pct. Black	0.002	−0.017*
	(0.013)	(0.010)
Pct. Imm. White	0.003	−0.009
	(0.011)	(0.010)
Median SEI	0.004	−0.016
	(0.020)	(0.015)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.433	0.361
Num. obs.	2882	3012

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	1.099	0.774
	(0.694)	(0.649)
Pct. Black	0.014	0.008
	(0.011)	(0.011)
Pct. Imm. White	0.030**	0.012
	(0.014)	(0.013)
Median SEI	0.012	0.010
	(0.024)	(0.022)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.445	0.370
Num. obs.	3167	3336

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

Table A.40: Effects on Occ. Score

	Immigrants	
	(1)	(2)
log(School fund)	−16.251	−15.482
	(13.514)	(13.052)
Pct. Black	0.078	0.147
	(0.409)	(0.339)
Pct. Imm. White	−0.151	−0.013
	(0.245)	(0.230)
Median SEI	−0.645	−0.316
	(0.793)	(0.696)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.398	0.331
Num. obs.	3483	3641

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Natives	
	(1)	(2)
log(School fund)	−0.229	8.434
	(6.852)	(6.305)
Pct. Black	−0.226	0.028
	(0.145)	(0.147)
Pct. Imm. White	−0.436***	−0.151
	(0.122)	(0.116)
Median SEI	0.091	0.482**
	(0.269)	(0.238)
Ind. Controls	Yes	No
Age-Ward FE	Yes	No
R^2	0.376	0.312
Num. obs.	3584	3772

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: Std. errors clustered at initial school level. Estimates from boundary change design.

A.2 Identification with Errors in School Assignment

In Section 1.4.1, I discuss identification in the limit using a typical RD framework. I showed that the attenuation bias from measurement error was in proportion to the fraction of mismeasured observations. Because of the limit argument, however, it was not possible to discuss how the attenuation bias varied with errors in school assignments for an assignment mechanism that uses the fraction of the street segment within the school area.

In this section, I discuss identification in a narrow bandwidth around a school boundary when there are errors in school assignments. This analysis is meant to show how the assignment mechanism, based on the fraction of one's street segment within a school area, generates bias in the estimation of local average treatment effects.

Let D denote actual assignment and $\tilde{D}$ observed assignment. $\tilde{D}$ is based on the fraction of one's street segment within a school area. Z denotes actual distance to the boundary, where distance is measured from the furthest point of the street segment from the boundary. I will focus on a narrow bandwidth around the boundary, such that $Z \in (-\epsilon, \epsilon)$.

To simplify the exercise, suppose there are two segments, one such that $\tilde{D} = 1$ and another such that $\tilde{D} = 0$. Suppose further that $P_d \equiv P(D = 1|\tilde{D} = d, Z)$ can be estimated using the fraction of the street segment within school area $D = 1$. If street segments are assigned to schools with the greatest fraction, P_1 should be high, and P_0 low.

First, begin with the difference in means within the bandwidth:

$$
\begin{aligned}
&E[Y|Z \in (-\epsilon, \epsilon), \tilde{D} = 1] - E[Y|Z \in (-\epsilon, \epsilon), \tilde{D} = 0] \qquad \text{(A.1)}\\
&= E[Y|D = 1, \tilde{D} = 1, Z \in (-\epsilon, \epsilon)]P_1 + E[Y|D = 0, \tilde{D} = 1, Z \in (-\epsilon, \epsilon)](1 - P_1)\\
&- \Big[E[Y|D = 0, \tilde{D} = 0, Z \in (-\epsilon, \epsilon)](1 - P_0) + E[Y|D = 1, \tilde{D} = 0, Z \in (-\epsilon, \epsilon)]P_0\Big]
\end{aligned}
$$

and assume $\tilde{D} \perp Y|D, Z$ and $Y(d) \perp D|Z \in (-\epsilon, \epsilon)$, so that

$$\begin{aligned} &= E[Y(1)|Z \in (-\epsilon, \epsilon)]P_1 + E[Y(0)|Z \in (-\epsilon, \epsilon)](1 - P_1) \\ &- \Bigg[E[Y(0)|Z \in (-\epsilon, \epsilon)](1 - P_0) + E[Y(1)|Z \in (-\epsilon, \epsilon)P_0]\Bigg]. \end{aligned} \tag{A.2}$$

After rearranging terms, we then have

$$\begin{aligned} &= E[Y(1) - Y(0)|Z \in (-\epsilon, \epsilon)]P_1 + E[Y(0)|Z \in (-\epsilon, \epsilon)] \\ &- E[Y(1) - Y(0)|Z \in (-\epsilon, \epsilon)]P_0 - E[Y(0)|Z \in (-\epsilon, \epsilon)] \\ &= E[Y(1) - Y(0)|Z \in (-\epsilon, \epsilon)](P_1 - P_0), \end{aligned} \tag{A.3}$$

so that the local ATE is reduced by $1 - P_1 + P_0$, i.e., the fraction of street segments within the unassigned school area. If $P_0 = 1 - P_1$, this would simplify to $2P_0$. Thus, if 75% of each street segment was in its respectively assigned school, the treatment effect would be reduced by 50%. Since I assume P_1 and P_0 can be estimated using the fraction of the street segment in school $D = 1$, the bias in the treatment effect can be corrected by dividing the mean difference by $P_1 - P_0$.

A.3 A Model of Education Choice and Human Capital

In this section, I propose a model of human capital accumulation and education choice to explain some of my main findings on the impacts of elementary school characteristics on total schooling. In particular, I focus on high school education, for whom both immigrants and natives saw large increases associated with higher native shares. On the other hand, native high school attendance decreased with more funding, whereas immigrants were relatively unaffected.

I first describe the model and then I simulate a population of immigrants and natives. In these simulations, I am able to replicate my results on the heterogeneous responses of immigrants and natives to elementary school characteristics. I discuss the elements of the model that help explain this heterogeneity.

A.3.1 Model

Suppose children live for three periods. Let $t = 0$ denote elementary school, $t = 1$ high school and $t = 2$ adulthood. Children must attend elementary school but they can choose whether or not to attend high school. In each period t, they either work or produce human capital using social and cognitive skills θ_t^s and θ_t^c. Both the quality of the school (e.g., funding) I_t and the average social skills $\bar{\theta}_t^s$ of its students affect human capital production.

Social skills are produced through interactions with peers. I assume cognitive skills and school quality do not affect the production of social skills, but that they are self-productive.

$$\theta_{t+1}^s = f_t^s(\theta_t^s, \bar{\theta}_t^s) \tag{A.4}$$

Cognitive skills, on the other hand, increase with both prior cognitive and social skills, and school quality.

$$\theta_{t+1}^c = f_t^c(\theta_t^c, \theta_t^s, I_t) \tag{A.5}$$

In my simulations, I choose the constant elasticity of substitution production function, so that

$$f_t^s(\theta_t^s, \bar{\theta}_t^s) = [\gamma^t (\theta_t^s)^{\phi_s^t} + (1 - \gamma^t)(\bar{\theta}_t^s)^{\phi_s^t}]^{1/\phi_s^t} \tag{A.6}$$

and

$$f_t^c(\theta_t^c, \theta_t^s, I_t) = [\gamma_1^t (\theta_t^c)^{\phi_c^t} + \gamma_2^t (\theta_t^s)^{\phi_c^t} + \gamma_3^t I_t^{\phi_c^t}]^{1/\phi_c^t}, \tag{A.7}$$

where $0 < \gamma^t, \gamma_1^t, \gamma_2^t$, and γ_3^t; $\phi_s^t, \phi_c^t \leq 1$; and $\sum_{k=1}^3 \gamma_k^t = 1$. Both ϕ_s^t and ϕ_c^t capture the substitutability of the inputs. When equal to 1, inputs are perfect substitutes. As this value decreases, inputs are more complementary, until the perfect complements case at negative infinity.

The parameters determine whether initial skills and investments in elementary school are complements or substitutes to future investments in high school.[1] Thus, they determine the opportunity costs and benefits of future schooling. To make this discussion concrete, I denote w as the relative price of cognitive skills, and let potential earnings out of elementary school be $Y_0 \equiv w\theta_1^c + \theta_1^s$. Potential earnings out of high school are then $Y_1 \equiv w\theta_2^c + \theta_2^s$. Earnings are then fixed over the life cycle. To introduce opportunity costs, I discount high school earnings by the factor $\beta \in (0,1)$,[2] which reflects the fact that the child did not earn anything while they attended high school.

Children attend high school if the benefits are greater than the costs:

$$D = \mathbb{1}\{\beta(w\theta_2^c + \theta_2^s) - (w\theta_1^c + \theta_1^s) > C\}, \tag{A.8}$$

where C could include both the direct monetary and psychic costs of attending high school. Agents take θ_1^c, θ_1^s, I_1, $\bar{\theta}_1^s$, β, w, C, and the human capital production technology as given. For ease of exposition, denote the net benefit of high school as $V = \beta(w\theta_2^c + \theta_2^s) - (w\theta_1^c + \theta_1^s) - C$.

---

[1] See Heckman and Mosso (2014) for a broader discussion of human capital production technologies and their implications for optimal investment.

[2] This simplification can be justified by computing the present discounted value of the two earnings streams. If Y is annual earnings and r is the annual discount factor, the present value of earnings is $\sum_{t=0}^T (1/(1+r)^t)Y = Y\sum_{t=0}^T (1/(1+r)^t)$. Because only relative earnings matter, if we compare the present discounted value of earnings with no high school education Y_0 with high school Y_1, we have $Y_1 \sum_{t=\tau}^T (1/(1+r)^t) > Y_0 \sum_{t=0}^T (1/(1+r)^t)$ if and only if $Y_1 \sum_{t=\tau}^T (1/(1+r)^t) / \sum_{t=0}^T (1/(1+r)^t) > Y_0$. So that defining $\beta \equiv \sum_{t=\tau}^T (1/(1+r)^t) / \sum_{t=0}^T (1/(1+r)^t)$, high school earnings are discounted to reflect the fact that children are not working the τ periods of high school.

A.3.2 Simulation

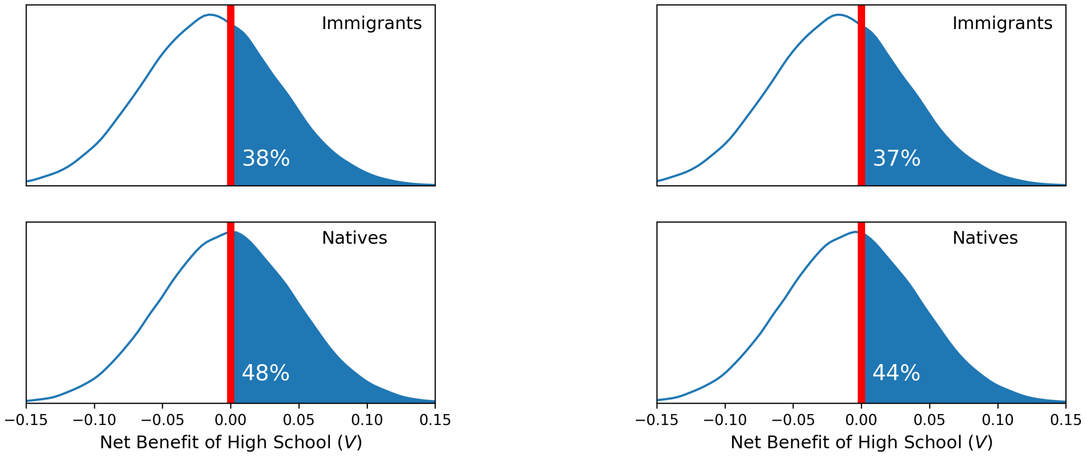
In an attempt to simplify the comparison, I assume immigrants and natives only differ in initial social skills θ_0^s and in the average social skills at their local elementary and high schools, $\bar{\theta}_0^s$ and $\bar{\theta}_1^s$. Immigrants have lower social skills than natives. This difference mainly reflects the fact that immigrants tended to have less fluency in the English language and came from homes that could set them apart culturally from their native peers. Table A.41 lists the parameters used for the simulation. Natives have both higher initial social skills and attend schools with higher average social skills. Otherwise, immigrants and natives are identical. The parameters were chosen to approximate the high school attendance rates in the data, resulting in about 38% for immigrants and 48% for natives.

The net benefit, V, under the baseline is plotted for both immigrants and natives in Panel (a) of Figure A.11. Panels (b) and (c) show the net benefit after increasing elementary school funding (b) and average social skills at school (c). Increasing I_0 by 0.1 decreases high school attendance for natives from 48% to 44%, while the impact on immigrants is nearly zero. Increasing average social skills, $\bar{\theta}_0^s$ and $\bar{\theta}_1^s$, by 0.1 increases graduation rates for natives to 57% and for immigrants to 44%. These changes are consistent with the signs of my coefficient estimates.

The partial derivative $\partial\theta_2^c/\partial\theta_1^c$ determines the sign of $\partial V/\partial I_0$ since $\partial V/\partial I_0 = w\frac{\partial\theta_1^c}{\partial I_0}\left(\beta\frac{\partial\theta_2^c}{\partial\theta_1^c} - 1\right)$. When $\partial\theta_2^c/\partial\theta_1^c < 1/\beta$, then $\partial V/\partial I_0 < 0$. If ϕ_c^1 is sufficiently large, the complementarities in the second period may be small enough to satisfy this condition. If in addition, ϕ_c^0 is sufficiently negative, the large complementarities in the first period can can increase $\frac{\partial\theta_1^c}{\partial I_0}$, causing a larger decline in V for those with high initial social skills θ_0^s.

Unlike I_0, peer quality, captured by $\bar{\theta}_0^s$ and $\bar{\theta}_1^s$, tends to increase both immigrant and native high school attainment. First, notice that $\partial V/\partial\bar{\theta}_0^s > 0$ if and only if $w\partial\theta_2^c/\partial\theta_1^s + \partial\theta_2^s/\partial\theta_1^s > 1/\beta$ since $\partial V/\partial\bar{\theta}_0^s = \frac{\partial\theta_1^s}{\partial\bar{\theta}_0^s}\left[\beta(w\partial\theta_2^c/\partial\theta_1^s + \partial\theta_2^s/\partial\theta_1^s) - 1\right]$. Because social skills are both self and cross productive, the increase in social skills from an increase in $\bar{\theta}_0^s$ boosts the productivity of both social and cognitive development in high school. Thus, improving social interactions in elementary school is more likely to raise the benefits of high school over the opportunity costs, regardless of θ_0^s. To the extent that $\bar{\theta}_1^s$ and θ_1^s are complements in high school, $\bar{\theta}_1^s$ needs to be high enough in order for increases to $\bar{\theta}_0^s$ to increase high school attendance–especially for higher θ_0^s. This is one reason why I increase average social skills in both periods in the counterfactual experiment.[3]

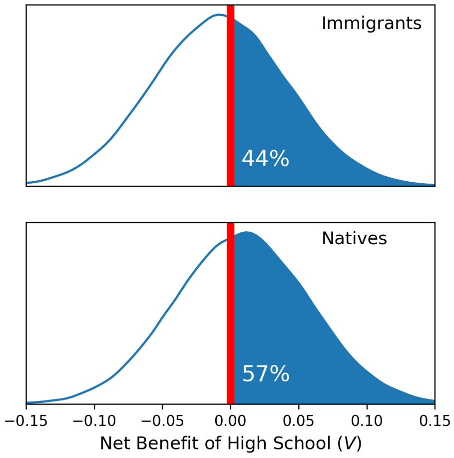
Figure A.11: Simulation: Net Benefit of High School



(a) Baseline

(b) Increase I_0

(c) Increase $\bar{\theta}_0^s$,$\bar{\theta}_1^s$



Note: Panel (a) shows the baseline simulation with parameters described as in Table A.41. Panel (b) increases I_0 from 0.5 to 0.6. Panel (c) increases average social skills at school by 0.1.

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[3] This is likely to match the data as well. Because high school assignment is also related to location, changes to elementary school assignment can coincide with changes to high school assignment.

Table A.41: Simulation: Baseline Parameters

Parameter	Natives	Immigrants
N	100,000	–
β	0.9	–
w	0.7	–
C	$\sim N(0.04, 0.05)$	–
$\bar{\theta}_0^s$	0.5	0.3
$\bar{\theta}_1^s$	0.8	0.6
I_0	0.5	–
I_1	0.8	–
θ_0^c	0.3	–
θ_0^s	0.75	0.2
γ^t	0.8	–
γ_1^t, γ_2^t	0.3	–
ϕ_c^0, ϕ_c^1	-2.5, -1	–
ϕ_s^0,ϕ_s^1	-0.5, -1	–

Note: Immigrant parameters are same as native's when "–" is shown.

APPENDIX B

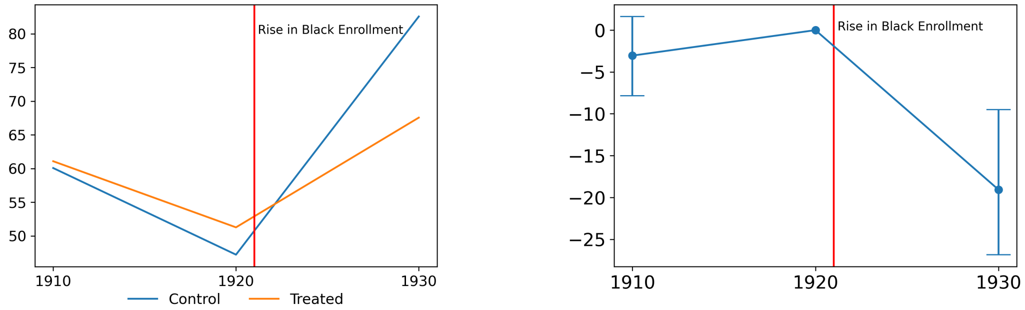
APPENDIX TO CHAPTER 2

B.1 Additional Estimates of Black Share on Schools

B.1.1 No Controls

Figure B.1: Impact of Black Majority on School Funding per Pupil

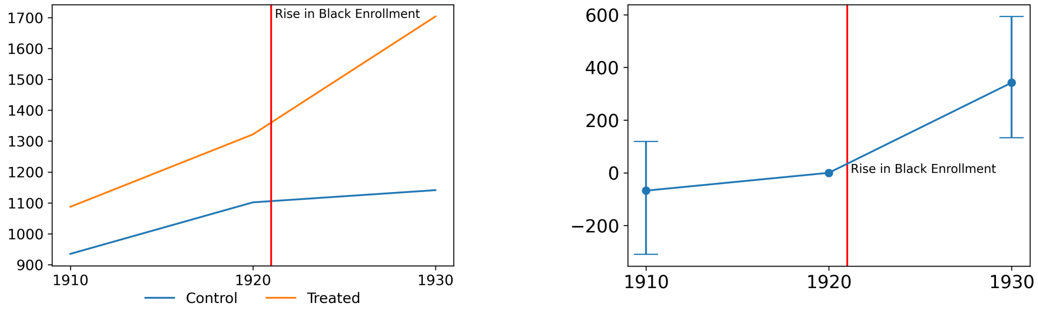
(a) Group Means (b) DID



Note: DID estimates shown with 95% confidence interval estimated using method of Conley and Taber (2011) to account for the small number of treated units. Time-varying controls not included.

Figure B.2: Impact of Black Majority on No. Pupils per School

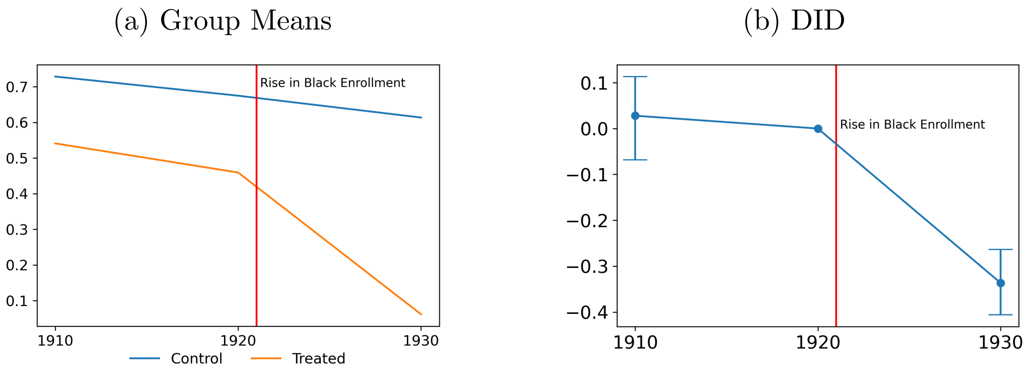
(a) Group Means (b) DID



Note: DID estimates shown with 95% confidence interval estimated using method of Conley and Taber (2011) to account for the small number of treated units. Time-varying controls not included.

B.1.2 Impacts on Control Variables

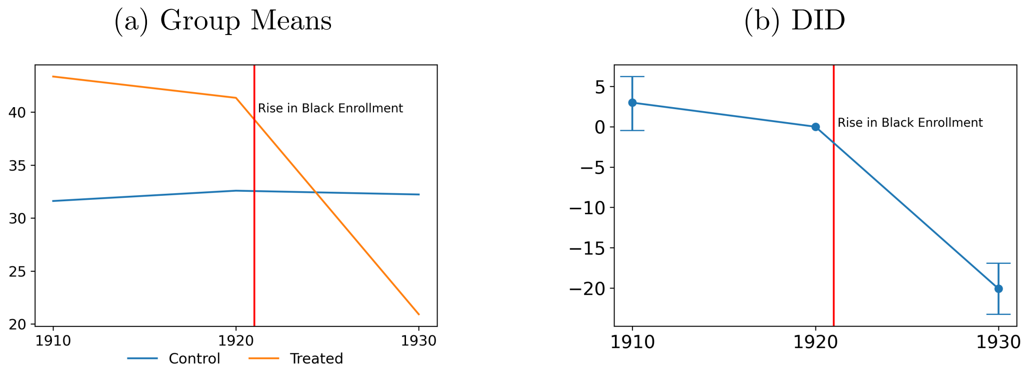
Figure B.3: Impact of Black Majority on Immigrant Share


(a) Group Means

(b) DID

Note: DID estimates shown with 95% confidence interval estimated using method of Conley and Taber (2011) to account for the small number of treated units. Time-varying controls not included.

Figure B.4: Impact of Black Majority on SEI


(a) Group Means

(b) DID

Note: DID estimates shown with 95% confidence interval estimated using method of Conley and Taber (2011) to account for the small number of treated units. Time-varying controls not included.

APPENDIX C

APPENDIX TO CHAPTER 3

C.1 Data

C.1.1 Project Talent

Project Talent was a nationally representative study of the U.S. high school population in 1960[1] (Wise, McLaughlin, & Steel, 1979). The study sampled only those individuals in school, excluding dropouts, with almost 400,000 people in the base year between grades 9 through 12. Base year sample members were surveyed again in multiple follow-ups, at 1, 5, and 11 years after graduation, with response rates to the mail survey of 49, 32, and 23 percent, respectively. However, additional effort was made to resurvey a representative subsample of non-respondents. The follow-up surveys focused on education, work and income, and relationship history. We focus on the base year estimates for our measurements of skills.[2]

In the base year, information was collected about students, and their families, schools, and communities. Students were given a two-day battery of tests and inventory questions. The data contains information related to skills, interests and personal characteristics. Specifically, the tests measured the following domains: language, mathematics, visualization, and general and specific knowledge. The Student Activities Inventory asked respondents about their "dispositional traits" using a 5-point Likert scale, the Interest Inventory about their interest in various activities and occupations,[3] and The Student Information Blank asked about

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[1] There are some special samples along with the nationally representative or "Talent National Probability Sample" included in the data. These include the sampling of all 15-year-olds in in the areas served by 10% of the participating school districts, the "TALENT Fifteen-Year-Old Sample;" a saturated sample of all 8th-12th graders in Knox County, Tennessee; and samples collected from other schools that asked to be in the study or were part of a special sub-study. Many students in these other samples were part of the Talent National Probability Sample.

[2] Skills were only measured in the base year, except for a 1963 retest sample of 12th graders of the same high schools from the base year, 3/4 of which had been tested in the 9th grade in 1960. Only a subset of the test batteries were given during this retest study.

[3] The Interest Inventory included 205 items, 122 dealing with occupations and 83 to activities. They were asked how well they would like or dislike the work or activity, "disregarding educational requirements, salary, social standing, or other factors."

health, activities, family background, and career plans. Tables C.1, C.2, C.3, and C.4 contain detailed information about the items collected for each of these instruments.

Table C.1: Testing Battery

Name	Items	Minutes	Description
Information Test Part I	**252**	**90**	**Part I focuses on information likely acquired at school.**
Screening	12		This scale contains questions that are extremely simple and are basic knowledge to all elementary school children. It was designed to help in identifying mentally retarded students, others who are functionally illiterate, and those who took the test with a flippant or apathetic attitude.
Vocabulary	21		This score gives some indication of the relative size of the students general vocabulary.
Literature	24		This score measures familiarity with the world of literature, including both prose and poetry. Some of the literary works on which test items are based are required reading in many schools and are on recommended reading lists in other schools. The tests broad coverage makes it likely that a student who has acquired the habit of recreational reading will score reasonably well regardless of what specific books are required reading in his or her school.
Music	13		This score is intended to indicate amount of musical information, musical talent. Those who enjoy going to concerts and operas, or listening to serious music on radio or phonograph are as likely to get a good score as those having formal training in music.
Social Studies	24		This scale covers facts and concepts from fields of history, economics, government and civics, geography, and current affairs.

Continued on next page

Table C.1 – *Continued from previous page*

Name	**Items**	**Minutes**	**Description**
Mathematics	23		These items are concerned with definitions, the vocabulary of mathematics, mathematical notation, other kinds of factual information, and the understanding of mathematical concepts. None of the items are covered by other tests in the battery (the Arithmetic Computation Test and the Mathematics Tests).
Physical Sciences	18		This scale includes items about chemistry, physics, astronomy, and other physical sciences. Many of the items cover information that might readily be acquired in other ways than through formal instruction.
Biological Sciences	11		This scale includes items about botany, zoology, and microbiology. A few items about nature lore are included, though most of the items are concerned with more formal aspects of biological science.
Scientific Attitude Scale	10		These items provide a subscore that should be indicative of how students view the worldwhether they view it as a place where there are logic cause-and-effect relationships, or whether they regard it as a place where consequences are illogical or arbitrary.
Aeronautics and Space	10		These items cover such topics as flying technique, navigation, jet planes, and space exploration. Much of the information that the student has in this area is likely to have been acquired out of school.
Electricity and Electronics	20		These items stress information that is acquirable through direct experience in the construction and maintenance of electrical and electronic equipment. Students who have worked on radios, hi-fi sets, or other electronic equipment, or on mechanisms with electric motors should score well.

Continued on next page

Table C.1 – *Continued from previous page*

Name	**Items**	**Minutes**	**Description**
Mechanics	19		Many of these items are concerned with automobiles; others are concerned with other common machines and tools with which people who are interested in mechanical activities are likely to be familiar. The emphasis is on information that is likely to be acquired through direct experience with tools, engines, and motors. The scores should also be influenced by the amount of experience and training in mechanics the student has had.
Farming	12		These items are intended to assess the information that children who grow up on farms and ranches are likely to acquire.
Home Economics	21		These items test information on cooking, sewing, caring for babies, cleaning, and other activities of a domestic nature. Students who have engaged in such activities, whether as a chore, or part-time job (e.g., babysitting), or just because they enjoy them, as well as students who have had formal instruction in home economics, should perform well on these items.
Sports	14		These items are intended to measure general familiarity with a wide range of sports teams. Knowledge of specific sports figures is not required.
Information Test Part II	**143**	**35**	**Part II focused on information likely acquired outside of school.**
Art	12		This scale measures general knowledge about art, but does not cover technical knowledge relating to proficiency as an artist. It includes items about well known artists and art works, as well as artists materials.
Law	9		The items in this scale relate to general knowledge about law that can be acquired through books or news reports concerning legal affairs.

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Table C.1 – *Continued from previous page*

Name	**Items**	**Minutes**	**Description**
Health	9		This scale includes items relating to practical health maintenance and nutrition, and to relatively common health care techniqures.
Engineering	6		These items deal with common engineering tools and applied engineering principles.
Architecture	6		These items focus on architectural styles, asking students to define the styles associated with specific building characteristics.
Journalism	3		This scale covers basic knowledge relating to newspaper journalism.
Foreign Travel	5		The items in this scale deal with characteristics of foreign countries likely to be familiar to those who have visited there or read about them.
Military	7		This scale deals with various branches of the armed services and the ranks associated with each.
Accounting, Business, Sales	10		This scale focuses on knowledge about finance and business occupations.
Practical Knowledge	4		The items in this scale draw upon knowledge of basic civic services such as postal services and traffic signs.
Clerical	3		These assess the students familiarity with typing, shorthand, and correspondence.
Bible	15		General knowledge about the characters and teachings in the Bible are covered by these questions.
Colors	3		These items deal with the composition of non-primary colors.
Etiquette	2		This scale assesses the students awareness of social conventions.
Hunting	5		These items deal primarily with hunting terms.
Fishing	5		These items deal primarily with knowledge of fishing equipment.
Outdoor Activities	9		This scale assesses students practical knowledge relating to swimming, boating and camping.

Continued on next page

Table C.1 – *Continued from previous page*

Name	**Items**	**Minutes**	**Description**
Photography	3		These items in this scale relate to standard table games, such as chess or card games, and require a basic knowledge of how each is played.
Games (sedentary)	5		The items in this scale relate to standard table games, such as chess or card games, and require a basic knowledge of how each is played.
Theater and Ballet	8		These items deal primarily with theater and ballet terms.
Foods	4		The items in this scale require a knowledge about various kinds of foods, including some foreign as well as domestic dishes.
Vocabulary	?		This scale overlaps several of the other scales in this part of the test. It is comprised of selected items requiring knowledge of the definitions of non-technical words.
Memory for Sentences	16	10	This score assess one particular kind of memorythe ability to memorize simple descriptive statements and to recall a missing word when the rest of the sentence is provided sometime later.
Memory for Words	24	4	The purpose of this test it to measure another type of rote memorythe ability to memorize foreign words corresponding to common English words.
Disguised Words	30	3	This score indicates the ability to form connections between letters and sounds. This is believed to be related to the ability to puzzle out from the context and appearance the meaning of a word that is reminiscent of a familiar English word.
English	**185**		**The purpose of this test is to measure ability to express oneself adequately in English. A**

Continued on next page

Table C.1 – *Continued from previous page*

Name	**Items**	**Minutes**	**Description**
Spelling	16	12	This score is intended to indicate ability to spellnot size of vocabulary. Students who do not have especially large vocabularies, but who are able to spell most of the words they have encountered should score well.
Capitalization	33	6	This score indicates degree of mastery of the rules of capitalization.
Punctuation	27	14	This test is designed to measure knowledge of the appropriate use of standard punctuation marks. Emphasis is placed on whether the student has mastered the concept of what constitutes a sentence.
English Usage	25	10	This score measures knowledge of preferred usage.
Effective Expression	12	10	This score measures the ability to recognize whether an idea has been expressed clearly, concisely and smoothly.
Word Functions in Sentences	24	15	This test is intended to measure the students sensitivity to grammatical structure. The fact that the terminology of grammar is not used at all in the test helps reduce the effects of formal training to a minimum. To score well, one must understand sentence structure and be able to recognize the function of each word or phrase in the sentence. This ability is probably relate to the ability to learn formal rules of English grammar.
Reading Comprehension	48	30	The purpose of this test is to measure the ability to comprehend written materials. The test includes passages on a wide range of topics. The student reads each passage and then answers a number of questions about it, referring back to the passage at will. The items are designed not to be answerable without reading the passage. The skills measured by this test are a good predictor of school success in an academic or liberal arts curriculum.

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Table C.1 – *Continued from previous page*

Name	**Items**	**Minutes**	**Description**
Creativity	20	20	The purpose of this test is to measure the ability to find ingenious solutions to a variety of practical problems. Items on this test require the student to generate tentative solutions and match them to multiple-choice alternatives indicated by a single letter of the solution word.
Mechanical Reasoning	20	11	The purpose of this test is to measure the ability to deduce the effects of the operation of everyday physical forces (such as gravity) and basic kinds of mechanisms (such as gears, pulleys, wheels, springs, and levers)
Abstract Reasoning	15	11	This is a non-verbal test designed to measure the ability to determine a logical relationship or progression among the elements of a complex pattern and to apply this relationship to identify an element that belongs in a specified position in a pattern.
Visualization in Two Dimensions	24	4	This test measures the ability to visualize how diagrams would look after being turned around on a flat surface, in contrast to the way they would look after being turned over.
Visualization in Three Dimensions	16	9	This test measures the ability to visualize how a two dimensional figure would look afer it had been folded to make a three-dimensional figure.
Mathematics	**54**	**52**	
Arithmetic Reasoning	16		This test is designed to measure the ability to reason in the manner required to solve arithmetic problems. Computation, except at the very simplest level, is excluded from the test.

Continued on next page

Table C.1 – *Continued from previous page*

Name	**Items**	**Minutes**	**Description**
Introductory	24		The purpose of this subtest is to measure achievement in all kinds of mathematics generally taught up to and including 9th grade, with the exception of the areas covered in the Arithmetic Computation Test and in Mathematics Part I (Arithmetic Reasoning). The primary emphasis of this test is on elementary algebra; other topics include fractions, decimals, percents, square roots, intuitive geometry, and elementary measurement formulas. While the topics covered are taught in Grade 9 or earlier in most schools, curricula differ considerably in regard to grade placement of various topics.
Advanced	14		This subtest covers topics normally taught in Grades 10-12 in college-preparatory courses. The items are intended primarily to test understanding and application of basic concepts and methods rather than rote memory. The topics covered are plane geometry, solid geometry, algebra, trigonometry, elements of analytic geometry, and introductory calculus. It should be noted that some of these subjects are not offered in most high schools. However, students who have successfully completed college preparatory mathematics beyond the Grade 9 level should be able to score well.
Arithmetic Computation	72	9	The purpose of this test to measure speed and accuracy of computation. The test is limited to the four basic operations (addition, subtraction, multiplication and division) and to whole numbers.
Table Reading	72	3	The purpose of this test is to measure speed and accuracy in a non-computational clerical task, involving obtaining information from tables. This kind of clerical aptitude is somewhat more complex than that measured by the Clerical Checking Test described below.

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Table C.1 – *Continued from previous page*

Name	**Items**	**Minutes**	**Description**
Clerical Checking	74	3	This test is designed to measure speed and accuracy of perception in a simple clerical task. The test involves comparing pairs of names to determine whether they are identical.
Object Inspection	40	3	The purpose of this test is to measure speed and accuracy in perception of form. More specifically, the test is intended to measure the ability to spot differences in small objects quickly and accurately when comparing them visually.

Table C.2: Interest Inventory Items

Bookkeeper
Bank teller
Surgeon
Chemist
Civil engineer
Dentist
Toolmaker
Automobile mechanic
Butcher
Tailor or dressmaker
Dietitian
Cab driver
Longshoreman
Foreman
Army officer
College president
Insurance agent
Stock salesman
Foreign correspondent
Editor
Musician
Aviator
Rancher
Airline hostess or steward
Social worker
Statistician
Astronomer
Research scientist
Office clerk
Store clerk
Plumber
Electrician
Fireman
Dish washer
Maid
Naval officer
Personnel administrator
Credit manager
Lawyer
Reporter
Sculptor
Forester
Elementary school teacher
Nurse
Chemical engineer
Doctor
Pharmacist
Aeronautical engineer
Secretary
Technician
Electronics technician
Bricklayer
Riveter
House painter
Mail carrier
Building superintendent
President of a large company
Author of a novel
Librarian
Economist
Actor or actress
Professional athlete
Policeman
Clergyman
Certified public accountant
Spaceman
Biologist
Electrical engineer
Mining engineer
Typist
Laboratory technician
Repairman
Beautician
Railroad brakeman
Shoemaker
Factory worker
Deliveryman
Truck driver
Building contractor
Marine corps officer
Real estate agent
Interpreter
Writer
Musical composer
Architect
Decorator
Sports umpire or referee
Guidance counselor
Accountant or auditor
Mechanical engineer
Mathematician
Switchboard operator
Machinist
Welder
Paper hanger
Carpenter
Type setter
Draftsman
Housewife
Air force officer
Office manager
Banker
Salesman
College professor
Poet
Artist
Designer
Farmer
High school teacher
Religious worker
School principal
Psychologist
Member of presidents cabinet
Judge
Us senator
Politician
Us congressman
Mayor
President of the us
Vice president of the us
State govenor
Public administrator
Take care of members of family
Make out income tax returns
Biology
Physics
Study muscles or nerves
Calculus
Keep records for a store
Invent new tools
Fix furniture
Work on an automobile assembly line
Wash and iron clothes
Plan work for other people
Own your own business
Reading
Sociology
Fishing
Basketball
Tennis
Raise sheep or cattle for market
Help your parents
Work arithmetic problems
Prepare cost estimates
Fortune telling
Typewriting
Make a radio set
Fix a clock
Operate a power machine
Fire a person
Manage a large store
Save money
Work for myself
Write letters
Practice music all day
Art galleries
Football
Track
Operate farm machinery
Operate a calculating machine
Physiology
Chemistry
Play chess
Solve puzzles
Do clerical work
Repair an auto
Operate a crane or derrick
Work in a steel mill
Hire a person
Give orders to workers in a factory
Buy stocks
Sell furniture
Watch tv
Act in plays
Trap wild animals
Foreign language
Teach children
Help the poor
Keep accounts
Algebra
Learn about diseases
Become a millionaire
Sell merchandise to stores
Literature
Write themes
Go to school
Symphony concerts
Hunting
Swimming
Feed hogs and cattle
Sell tickets for a railroad or airline
Shop work
Do odd jobs with small tools
Direct people
Arrange a strike settlement with management
Invest money
Poetry
Play an instrument
Studying
Visit museums
Exploring
Military drill
Baseball
Gardening
Campaign for political office

Table C.3: Student Activities Inventory (SAI) Items

Item	**Scale**
I like to spend a good deal of time by myself	Sociability
Id rather be with a group of friends than at home by myself	
People consider me the quiet type	
People seem to think I make new friends more quickly than most people do	
I couldnt get along without having people around me most of the time	
I enjoy getting to know people	
I like to be with people most of the time	
I go out of my way to be with friends	
I prefer reading a good book to going out with friends	
People consider me good-natured	
People consider me socioable	
I am friendly	
I like to tease people	Social Sensitivity
I never hurt another persons feelings if I can avoid it	
I seem to know how other people will feel about things	
I sympathize with my friends and encourage them when they have problems	
People consider me a sympathetic listener	
People consider me very tactful in dealing with other people	
I am sympathetic	
I am considerate	
People consider me understanding	
I like to do things on the spur of the moment	Impulsiveness
I usually act on the first plan that comes to mind	
I feel that Im impulsive	
People seem to think I sometimes make decisions too quickly	

Continued on next page

Table C.3 – *Continued from previous page*

Item	**Scale**
I am impulsive	
I dont believe in rushing into things	
I am cautious	
When I have a problem, I make up my mind and dont worry about it	
It takes me quite a while to come to a decision	
I often lose my temper	Calmness
I can usually keep my wits about me even in difficult situations	
People seem to think I get angry easily	
People seem to think I have good self-control	
People consider me level-headed	
I am even-tempered	
I am calm	
I am stable	
I am usually self-controlled	
I am never sloppy in my personal appearance	Tidiness
I have a definite place for all my things	
Before I start a task, I spend some time getting it organized	
It bothers me to be with someone who dresses carelessly	
I like to do things systematically	
My work suffers from lack of neatness	
People consider me very careless about my personal appearance	
I am tidy	
I am neat	
I am orderly	
I tend to be untidy	
I enjoy beautiful things	Culture
I feel that good manners are very necessary for everyone	
I think culture is more important than wealth	

Continued on next page

Table C.3 – *Continued from previous page*

Item	**Scale**
I enjoy cultural things	
I am a cultured person	
People seem to think that I have good taste	
I take part in the cultural activities in my community	
I tend to have good taste	
I am refined	
I am sometimes crude	
I am the leader in my group	Leadership
I am influential	
I have held a lot of elected offices	
People naturally follow my lead	
I like to make decision	
I am confident	Confidence
Id enjoy speaking to a club group on a subject I know well	
Being around strangers makes me feel ill-at-ease	
Im troubled by people making fun of me	
People seem to think my feelings are hurt too easily	
I am usually at ease	
People seem to think I am easily discouraged when criticized	
I am often self-conscious	
People consider me shy	
I am sensitive	
I am often worried	
People seem to think I usually do a good job on whatever Im doing	
I make good use of all my time	
I never seem to get things done on time	
I work fast and get a lot done	
When I say Ill do something I get it done	
It bothers me to leave a task half done	
I can turn out a lot more work than average	
I am hard-working	

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Table C.3 – *Continued from previous page*

Item	**Scale**
People consider me an efficient worker	
I do my job, even when I dont like it	
I find it hard to keep working toward long-range goals	
I am productive	
As soon as I finish one project or assignment, I always have something else I want to begin	
I never volunteer for a tough job	
I think that if something is worth starting its worth finishing	
I do things the best I know how, even if no one checks up on me	
I lose interest in most projects before I get them done	
People seem to think they can count on me	
People consider me persistent	
I am dependable	
People have criticized me for leaving things undone	
I am conscientious	
I am persistent	
I am reliable	
People consider me determined	
I work better with ideas than things	Theoreticality
Philosophy interests me	
Id rather build things than develop theories	
I spend a lot of time thinking	
Id rather read a book than go to a party	
People say that I tend to be a thinker rather than a doer	
I tend to be theoretical	
I am imaginative	
I do what the group decides to do even if I dont particularly like it	Group centeredness
Id give up my place on a team if that would insure that the team would win	
I enjoy helping my group get ahead	

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Table C.3 – *Continued from previous page*

Item	**Scale**
If I dont agree with the groups decision, I go my own way	
I take great pride in the accomplishments of my group	
I am a team-player	
I can work or play outdoors for hours without getting tired	
I am a fast worker	
I am full of pep and energy	
People seem to think I lead a vigorous life	
I am active	
I am vigorous	Vigor
I am energetic	

Table C.4: Student Information Blank (SIB) Items

Item	**Label**
1	School newspaper, magazine, or annual
2	School subject matter clubs
3	Debate, drama, or music
4	Hobby clubs
5	Farm youth clubs
6	Organized nonschool youth groups
7	Informal neighborhood group
8	Political club
9	Social clubs, fraternities, or sororities
10	Military or drill units
11	Organization president other than athletic
12	Organization officer not president other than athletic
13	Captain of athletic team
14	Drawing, painting, sculpting, or decorating
15	Acting, singing, or dancing in public performance
16	Collecting stamps, coins, rocks, insects etc.
17	Building models
18	Working with photographic equipment, not taking snapshots
19	Making jewelry, pottery, or leatherwork
20	Making or repairing electronic or electrical equipment
21	Cabinet making or woodworking
22	Metal working
23	Mechanical or auto repair
24	Raising or caring for animals or pets
25	Sewing, knitting, crocheting, or embroidery
26	Cooking
27	Playing baseball, football, or basketball
28	Flower or vegetable gardening
29	Hunting or fishing
30	Attending cultural events, not movies
31	Playing golf or tennis

Continued on next page

Table C.4 – *Continued from previous page*

Item	**Label**
32	Playing hockey, lacrosse, or handball; boxing, wrestling, or track
33	Biking, ice skating, skiing, canoeing, horseback riding
34	Hours a week on chores
35	Age when first earned money outside the home
36	How many summers spent with a regular job
37	Work hours per week during school
38	Delivering newspapers, baby sit, mow lawns, house cleaning, etc.
39	Clerical work
40	Farm or orchard work
41	Lab assistant
42	Factory work
43	Retail store work
44	Sales work
45	Camp counselor
46	Other work for pay
47	Spending money from regular allowance
48	Spending money from family as needed
49	Spending money from a job
50	Spending money from other sources
51	Age of first date
52	Average dates per week
53	Days per week allowed to go out for fun 8 or later during school year
54	How many steadies past three years
55	How many evenings out a week during school
56	Books read not for school
57	Westerns, adventure, or mysteries
58	Science fiction
59	Science nonfiction
60	Plays, poetry, essays, literary criticism, or classics
61	Politics, world affairs, biographies, autobiographies, historical novels
62	Religious books or magazines
63	Comic books
64	Love stories (they mean romances)

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Table C.4 – *Continued from previous page*

Item	**Label**
65	More than required
66	Difficulty expressing self in reports exams assignments
67	Speed reading helps complete lessons quickly
68	Grades accurately reflect ability
69	Understand what to do before starting assignments
70	Little accomplishment for time spent studying
71	Lack of interest increases distraction
72	Enjoy writing reports and compositions
73	Inattention in class causes lower marks
74	Difficult assignments are challenges to ability
75	Too quick to do best work
76	Inattention caused missed assignments and announcements
77	Teachers critical of sloppy assignments
78	Do just enough to get by unless really like course
79	Difficulty with mechanics of english composition
80	Attention strays in class
81	Get behind in assignments
82	Careless errors lower grades
83	Slow reading holds me back
84	Pronounce words to self while reading them
85	Courses not much help to occupation after school
86	Can pick out important points when studying for test
87	Easily distracted when reading
88	Keep up to date and do work every day
89	Trouble remembering reading
90	Read and reread without comprehension
91	Most like your high school
92	Times changed schools not by promotion
93	Time last changed schools
94	Age started first grade
95	Full semesters missed
96	Days absent 58-59 school year
97	Hours studied each week including study hall

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Table C.4 – *Continued from previous page*

Item	**Label**
98	How many semesters of science courses
99	How many semesters of foreign languages
100	How many semesters of social studies
101	How many semesters of english courses
102	How many semesters of business or commercial courses
103	How many semesters of vocational shop or agriculture
104	How many semesters of mathematics
105	Has had any calculus
106	My grade in mathematics
107	My grade in science
108	My grade in foreign languages
109	My grade in history and social studies
110	My grade in english
111	My grade in vocational courses
112	My grade in business or commercial courses
113	My grade in all courses
114	Principal or teacher on college or college plans
115	Principal or teacher on jobs or occupation
116	Principal or teacher on high school work
117	Principal or teacher on personal problems
118	Counselor on college or college plans
119	Counselor on jobs or occupation
120	Counselor on high school work
121	Counselor on personal problems
122	Discussed plans with father
123	Discussed plans with mother
124	Discussed plans with sibling
125	Discussed plans with school counselor
126	Discussed plans with teacher or official at school
127	Discussed plans with clergyman
128	Discussed plans with friends own age
129	Discussed plans with an adult not listed above
130	Family breadwinner

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Table C.4 – *Continued from previous page*

Item	**Label**
131	Does father work more than one job
132	Is father a supervisor
133	Father's job responsibility for money and property
134	Has mother worked for pay
135	How long has mother worked
136	Father's rank in the military
137	Were parents born in us
138	Father active in church or religious group
139	Father active in school organizations
140	Father active in political groups
141	Father active in labor or trade union
142	Father active in business or professional organization
143	Father active in hobby groups
144	Father active in sports club team or organization
145	Father active in social groups
146	Father active in civic organizations
147	Father active in military reserve or veterans' organization
148	Mother active in church or religious group
149	Mother active in school organizations
150	Mother active in political groups
151	Mother active in labor or trade union
152	Mother active in business or professional organization
153	Mother active in hobby groups
154	Mother active in sports club team or organization
155	Mother active in social groups
156	Mother active in civic organizations
157	Mother active in auxillary or veterans' organization
158	How well does parent speak french
159	How well does parent speak german
160	How well does parent speak italian
161	How well does parent speak spanish or portuguese
162	How well does parent speak russian or slavic
163	How well does parent speak hebrew or yiddish

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Table C.4 – *Continued from previous page*

Item	**Label**
164	How well does parent speak scandinavian language
165	How well does parent speak oriental language
166	How well does parent speak some other foreign language
167	How long in present community
168	Best descriptor of prior community
169	How many houses lived in last three years
170	Best descriptor of building living in
171	How much rent a month
172	Present value of home
173	Total family income
174	Best finance description
175	Largest source of income
176	How many books in home
177	How many ladies magazines
178	How many mens magazines
179	How many movie, romance, and detective magazines
180	How many mechanics magazines
181	How many farm magazines
182	How many news magazines
183	How many business magazines
184	How many opinion magazines
185	How many professional or trade journals
186	How many science fiction magazines
187	How many atlantic monthly, harpers, national geographic, fortune, new yorker
188	How many reader's digest, coronet, saturday evening post
189	How many parents' magazine, boys' life, american girl, scouting
190	How many appliances in home
191	How many electronics
192	How many fancy furnishings
193	How many music and art equipment
194	How many sporting goods
195	Do you have your own room desk or typewriter
196	How many hand tools in home

Continued on next page

Table C.4 – *Continued from previous page*

Item	**Label**
197	How many electric power tools
198	How many cars or wagons not trucks in household
199	What year is car or wagon not truck
200	Twin, triplet, or quadruplet
201	How many siblings dropped out
202	How many siblings went to vocational school
203	How many siblings attended business or commercial school
204	How many siblings attended junior college
205	How many siblings attended four year college
206	Describes work of father
207	What profession or technical work does father do
208	Describes work of mother
209	What profession or technical work does mother do
210	College specialty
211a	Expected career after college, coding 1
211b	Expected career after college, coding 2
212	Occupation you most want to do
213	For whom does your father work
214	For whom does your mother work
215	How old is your father
216	How old is your mother
217	Military service of father
218	Education of father
219	Education of mother
220	Who is head of household
221	Total living children in family
222	How many brothers are older than you
223	How many sisters are older than you
224	Where did you live just before moving to this community? (release)
225	How many rooms in your house
226	How many people live in your house
227	Height
228	Weight

Continued on next page

Table C.4 – *Continued from previous page*

Item	**Label**
229	Age when learned social dancing
230	How many athletic teams over last three years
231	How many clubs or organizations over last three years
232	Military service expectation
233	Military service ideally
234	Most important reason for choice of military entry
235	Which branch will you serve in
236	Most important reason for choice
237	Best descriptor of college you expect to attend
238	Expected age at marriage
239	Yearly earnings expected 20 years later
240	Yearly earnings minimum 20 years later
241	How many times sick in bed last year
242	Longest peroid of time in bed sick
243	Description of usual health over last three years
244	Description of usual health prior to ten years old
245	How many times treated by doctor for injury or illness in the past six months
246	Average hours of sleep each night
247	How late do you usually stay up on weekends
248	Do you wear glasses all the time
249	Do you have trouble with distance vision
250	Do you wear glasses for specific purposes (reading, watching tv, etc.)
251	Do you have trouble hearing people talk
252	Are you hard of hearing
253	Do you wear a hearing aid
254	Are you able to speak clearly all the time
255	Is your speech easily understood
256	Do you have more trouble with your skin than others your own age
257	Do you have more trouble with skin on your face more than others your age
258	Have you ever been knocked unconscious
259	Do you have normal use of both your legs
260	Do you have normal use of both your arms
261	Do you have normal use of both your hands

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Table C.4 – *Continued from previous page*

Item	**Label**
262	Do you have trouble with your back or spine
263	Do you have frequent stomach trouble or indigestion
264	Have you ever worn a shoulder brace, corrective shoes, leg brace, or other corrective appliance
265	Have you ever had mumps
266	Have you ever had rheumatic fever
267	Have you ever had asthma
268	Have you ever had hay fever
269	Has a doctor told you you have allergies
270	Has a doctor told you you have heart trouble
271	Have you ever had polio
272	Have you had frequent sore throats
273	Have you ever had severely aching joints
274	Do you often get severe headaches
275	Have you ever had spells of dizziness and faintness
276	Do you often get aches and pains not severe enough for doctor
277	Do you catch colds very often
278	Do you have trouble getting rid of colds
279	Do you follow a doctor prescribed diet for health reasons
280	Do you get more sleep more than others for health reasons
281	Do you avoid strenuous exercise for health reasons
282	Do you take special exercise for health reasons
283	Do you take prescription medicines for health reasons
284	Awards won for science
285	Awards won for mathematics
286	Awards won for foreign languages
287	Awards won for writing, debate, or oratory
288	Awards won for art
289	Awards won for music
290	Awards won for athletics
291	Awards won for activities
292	Age learned to drive
293	How many times on motorcycle past 6 months
294	How many times a week driven car other than for work

Continued on next page

Table C.4 – *Continued from previous page*

Item	**Label**
295	Do you have a car of your own or one for your use
296	Did you take driver training at school
297	Do you think you will quit high school before graduation
298	Do you think you will attend a vocational school after high school
299	Do you think you will attend a business or commercial school after high school
300	After high school, are you likely to attend a part time 2 or 4 year college
301	After high school, are you likely to attend a full time 2 or 4 year college
302	Which college type are you likely to attend
303	When do you plan to start college
304	Greatest amount of education expected
305	Have you taken, or will you take the national merit scholarship test
306	Willing to take college loans
307	Is college necessary for type of work desired
308	Father wants me to go to college
309	Mother wants me to go to college
310	Believes college graduates earn more money
311	Want to learn more about career possibilities
312	Want to meet a spouse
313	I enjoy learning
314	Teachers think i should go to college
315	Expect to be a collegiate athlete
316	Many of my friends are going to college
317	I want to participate in college social life
318	Wish to make good contacts for business
319	College would not help me to do things i am interested in
320	Wish to get job and earn money sooner
321	Need to work for living
322	College would cost more than parents can afford
323	College would cost more than parents want to pay
324	Would rather get married
325	High school grades too low for college
326	Do not like to study
327	I don’t think i have the ability for college

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Item	**Label**
328	Believe college costs more than it is worth
329	Mother does not want me to go
330	Father does not want me to go
331	Most friends are not going to college
332	Probably would not use education on job
333	Believe success is due to ability and effort not education
334	Girls should go to college only for job use
335	Country needs trained girls
336	Believe college is not necessary to lead or earn good salary
337	How much education do your parents want you to have
338	How much education are your friends planning
339	Would like a lifetime career in military
340	Believes will make a lifetime career in military
341	Which is most likely condition to make lifetime military
342	Longest period active duty you would consider in the army
343	Longest period active duty you would consider in the navy
344	Longest period active duty you would consider in the air force
345	Longest period active duty you would consider in the marines
346	Longest period active duty you would consider in the coast guard
347	How many different seriously considered occupations
348	How definite is occupation choice
349	School grade when decided on occupation
350	How important to choice of job is good income to start or soon
351	How important to choice of job is job security
352	How important to choice of job is important work
353	How important to choice of job is decision making freedom
354	How important to choice of job is opportunity for promotion and advancement
355	How important to choice of job is meeting and working with sociable people
356	How important in choice of switching jobs is better pay elsewhere
357	How important in choice of switching jobs is work not interesting
358	How important in choice of switching jobs is more important work elsewhere
359	How important in choice of switching jobs is poor supervisor
360	How important in choice of switching jobs is disliked coworkers

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Item	**Label**
361	How important in choice of switching jobs is no expected raises or promotions
362	How many children expected after marriage
363	Financial well being hoped for
364	Financial well being expected
365	For a man with a wife and children how important is a life insurance policy
366	Estimated life insurance policy ten years after high school
367	Estimated savings ten years after high school
368	Estimated investments ten years after high school
369	Estimated real estate ten years after high school
370	Expected best method to pay for stuff
371	Expected best way to save
372	Expected best savings plan first five years
373	Most important item saving for now
374	Most important item saving for after education completed
375	Maximum amount borrowed per year of college
376	Maximum total amount to borrow for college
377	If loans are taken which repayment plan preferred
378	Largest amount to repay per year
379	Maximum interest you would pay for college loan
380	How long would you take to repay at 3 percent
381	How long would you take to repay at 6 percent
382	How much money needed to pay for one year tuition books and fees
383	How much money expected to pay for all other college expenses one year
384	Percentage of money for college from college loan funds
385	Percentage of money for college from loans from other sources
386	Percentage of money for college from scholarships
387	Percentage of money for college from parents
388	Percentage of money for college from savings from work before college
389	Percentage of money for college from part time or summer jobs
390	Number of colleges applied to
391	How far from home is college
392	How close to home is eligible college
393	Where will you live while attending college

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Table C.4 – *Continued from previous page*

Item	**Label**
394	Best description of college you intend to attend

C.2 Notes on Identification

In this section, I provide some notes on the identification of linear factor models. Some of the assumptions are pure normalizations, affecting only the location and scale of the distribution of θ. In other cases, the assumptions affect how we interpret the factors. Because we hope to interpret some factors as effort (if effort is directly observed then we may assume measurement error variance is 0) and others as skills, some assumptions typically grouped under "normalizing" assumptions can affect our interpretation. We try to distinguish these assumptions from the more pure normalizing assumptions used to set the scale and location of the factors.

C.2.1 Identification of the Linear Model

Below we summarize some key analysis in Williams (2017). Suppose we have a linear factor structure that can be described by the system of equations

$$M_i = AF_i + u_i \tag{C.1}$$

where $M_i \in \mathbb{R}^m$ is a vector of measurements, $F_i \in \mathbb{R}^k$ a vector of factors, A a $m \times k$ matrix of loadings and u_i a vector of measurement errors. Maintain throughout that u_i is independent of F_i and define $\Sigma = Var[M_i]$, $\Delta = Var[u_i]$, and $\Phi = Var[F_i]$. By our independence assumption $\Sigma = A\Phi A' + \Delta$.

Identification can be split into two parts: (i) identifying $A\Phi A'$ and Δ and (ii) identifying A and Φ. The intuition for (i) is to use variation that is common across measures to identify $A\Phi A'$ while the remaining variation identifies Δ. Part (i) can also be accomplished without addressing the indeterminacy common in factor models, where normalizations are needed to identify A and Φ. Normalization is relegated to (ii).

Identification of $A\Phi A'$ and Δ

We gave some intuition for identification of these two components above. $A\Phi A'$ is identified from common variation among measurements and Δ the remaining variation. In one typical case, $A\Phi A'$ and Δ are identified if A satisfies the row deletion property, i.e., after removing any row of A, you can form two non overlapping sets of k linearly independent rows. Without further assumptions on A, this requires $m >= 2k + 1$ even if Δ is diagonal. The intuition behind this condition is that you have enough variation in your measurements to proxy all of the factors (hence linear independence) and the additional variation necessary to serve as instruments for the first set of measures. This way, you are able to sort out shared variation (due to the factors) versus idiosyncratic variation (due to the errors).

We can loosen up the assumptions on Δ only by making further restrictions on A. As an example of this trade-off consider a block diagonal structure for Δ with at least 3 blocks. Then Δ and $A\Phi A'$ are identified if each sub-matrix of A corresponding to a block in Δ is rank k. This can be relaxed to a *group-wise row deletion* property on A. But in either case, we can use an IV analogy to explain the identification strategy. We need enough variation in the measurements so that one set of measurements can proxy for F_i and another set can serve as instruments. The assumption that each sub-matrix of A corresponding to a block be rank k assures that each block captures all of the factors. The additional blocks also capture all of the factors, but because of the block diagonal structure, they can serve as valid instruments for the other blocks. The assumption of at least 3 blocks pins down the relative variance of Φ and Δ. The instrument analogy is useful for even more cases. Consider a case where $m < 2k + 1$, i.e., our data does not achieve the basic requirements for the row deletion property alone to secure identification. We could however, impose a dedicated measurements assumption on A. As an example if $m = 4$ and $k = 2$, we could have two measurements measure factor 1 and the other measurements measure factor 2. Within each group, one measure can serve as the instrument of another, and if we further assume zero correlation between factors, the two components are identified.

Identification of A and Φ

Once Δ and $A\Phi A'$ are identified, we can think of identifying the components A and Φ separately. Note that the conditions described in this section should be consistent with the conditions imposed to identify $A\Phi A'$ and Δ. In much of the literature on factor analysis, the separate identification of A and Φ is described as a normalization (Schennach, 2012). While any valid rotation is observationally equivalent, in the sense that $A\Phi A'$ can remain unchanged, the choice of rotation will affect A and, perhaps more importantly, the underlying factor distribution. While some of the assumptions made to identify A are normalizations, e.g., setting the mean and scale, we refrain from describing every identifying assumption as a normalization, as some can dramatically change our interpretation of the factors, especially if our goal is to separately identify effort from skills. In fact, if our goal is to use the factors as control variables in the analysis of treatment effects, the need for separate identification of A and Φ is eliminated (Heckman, Schennach, & Williams, 2010).

We first discuss identification of A and Φ up to scale. Then we discuss normalizing assumptions that are used to set the scale. To identify A and Φ up to scale, we must either restrict the loadings on A, e.g., by imposing dedicated measurements, or restrict covariances in Φ. The only way to obtain information about dependence between elements in F_i and dependence of measurements on F_i is from the covariances in Σ. Correlation between two measures can either mean they load on the same factors or that they load on different factors that are correlated. There is no way to tell the difference empirically, so one must make theoretical arguments for restricting Φ or A. Of course, increasing the number of measurements enables the researcher to restrict some components of A while leaving some measurements free to load on all factors, even with no restrictions on Φ. One can also combine restrictions across A and Φ. For instance, we could assume A follows a block diagonal structure where the corresponding blocks in Φ are diagonal. Once we have identified A and Φ up to scale, we can set the scale by either fixing a loading for each factor F_i or by fixing the corresponding variance in Φ. Other normalizations are also possible

(Williams, 2017).

In some cases, restricting A may be consistent with our measurement model and help us separately identify skills from effort. As an example, suppose the first $k_1 = 2$ elements of θ are test and grade effort, respectively. The remaining $k - k_1$ factors are skills. Suppose we have pure measurements of grade and test effort and that we've separately identified $A\Phi A'$ and Δ. Suppose we can write the matrix A as

$$A = \begin{pmatrix} A_1 \\ A_2 \end{pmatrix} = \begin{pmatrix} I_{k_1} & 0 \\ A_{21} & A_{22} \\ A_{31} & A_{32} \end{pmatrix},$$

where

$$A_1 = \begin{pmatrix} I_{k_1} & 0 \\ A_{21} & A_{22} \end{pmatrix}$$

are the first k rows of A and

$$A_2 = \begin{pmatrix} A_{31} & A_{32} \end{pmatrix}$$

the remaining rows (at least $k + 1$). The first k_1 rows are an identity matrix, and serve as our dedicated measures of test and grade effort, respectively. If A_{22} is invertible, then by Theorem 2.6 of Williams (2017), $A_{32}A_{22}^{-1}$ and $A_{31} - A_{32}A_{22}^{-1}A_{21}$ are identified. We reproduce the proof here. Suppose $A\Phi A'$ is identified, and both Φ and A_{22} are invertible. Since $A\Phi A'$ is identified, then its sub-matrices $A_1\Phi A_1'$, $A_1\Phi A_2'$, $A_2\Phi A_1'$ and $A_2\Phi A_2'$ are identified. Then $A_2A_1^{-1}$ is identified (A_1 is invertible because A_{22} is invertible) because

$$A_2\Phi A_1'(A_1\Phi A_1')^{-1} = A_2A_1^{-1}.$$

Then since

$$A_1^{-1} = \begin{pmatrix} I_{k_1} & 0 \\ A_{21} & A_{22} \end{pmatrix}^{-1} = \begin{pmatrix} I_{k_1} & 0 \\ -A_{22}^{-1}A_{21} & A_{22}^{-1} \end{pmatrix}$$

we have

$$\begin{aligned} A_2A_1^{-1} &= \begin{pmatrix} A_{31} & A_{32} \end{pmatrix} \begin{pmatrix} I_{k_1} & 0 \\ -A_{22}^{-1}A_{21} & A_{22}^{-1} \end{pmatrix} \\ &= \begin{pmatrix} A_{31} - A_{32}A_{22}^{-1}A_{21} & A_{32}A_{22}^{-1}. \end{pmatrix} \end{aligned}$$

Further, if we assume that a column of A_{21} is equal to 0, then we identify the corresponding column of A_{31}. When would a column of A_{11} equal 0? In the case that test scores only load on test effort, not grade effort. A similar argument can be made for grade effort. As a result, we can identify the loadings on the test and grade effort factors so that we can interpret the remaining factors as skills, not effort. A similar argument can be made if test and grade effort are directly observed, see Section C.2.2 for details.

C.2.2 Identification of Linear Model with Observed Components

A generalization of the factor model includes observed covariates X_i. Let $M_i = (X_i', Y_i)'$, $F_i = (X_i', \theta_i')'$ and $u_i = (0, \epsilon_i')'$ where $\epsilon_i = (\epsilon_{i1}, ..., \epsilon_{iJ})'$. Further define

$$A = \begin{pmatrix} I_{d_X} & 0 \\ \beta & \alpha \end{pmatrix} \tag{C.2}$$

where α are the factor loadings on θ_i and β the coefficients on X_i. Moreover, we can write

$$\Delta = \begin{pmatrix} 0 & 0 \\ 0 & \Delta_Y \end{pmatrix} \tag{C.3}$$

where $\Delta_Y = Var[\epsilon]$. Finally let

$$\Sigma = \begin{pmatrix} \Sigma_X & \Sigma_{XY} \\ \Sigma'_{XY} & \Sigma_Y \end{pmatrix} \tag{C.4}$$

and

$$\Phi = \begin{pmatrix} \Phi_X & \Phi_{X\theta} \\ \Phi'_{X\theta} & \Phi_\theta \end{pmatrix} \tag{C.5}$$

where $\Phi_X = \Sigma_X$.

Identification of $A\Phi A'$ and Δ The identification argument for these reduced form parameters is similar to the case with no covariates with the exception that we need only identify Δ_Y to identify Δ and therefore $A\Phi A'$. The problem can be recast by considering the Schur complement of Σ with respect to Σ_X, denoted Σ_Y^S and the Schur complement of Φ with respect to Φ_X, denoted Φ_θ^S: $\Sigma_Y^S = \alpha \Phi_\theta^s \alpha' + \Delta_Y$. The Schur complements Σ_Y^S and Φ_θ^S reflect the variances of Y_i and θ_i, respectively, after partialing out the effect of X_i. Thus, we can use the intuition for identification in the no covariates case with a slight twist. We need enough variation in one set of measurements to serve as proxies for θ, and enough variation in another set of measurements to serve as instruments for the first set *after partialing out the effect of X_i.*

Identification of β, α and Φ Suppose $A\Phi A'$ and Δ are identified. We now consider identification of the various components. Identification is simple if X_i and θ_i are uncorrelated.

A regression of M_i on X_i identifies β. Then we can proceed as in the no covariates case using $M_i - \beta' X_i$ as the measures.

The trickier case occurs when X_i and θ_i are correlated. In this case, we need exclusion restrictions to identify the coefficients on the regressors. Or some combination of exclusion and restrictions on the covariance between X_i and θ. Williams (2017) gives the following proof. First, partition A into two matrices,

$$A = \begin{pmatrix} A_1 \\ A_2 \end{pmatrix} = \begin{pmatrix} I_{d_X} & 0 \\ \beta_{(1)} & \alpha_{(1)} \\ \beta_{(2)} & \alpha_{(2)} \end{pmatrix}$$

so that A_1 corresponds to rows $(\beta_{(1)}, \alpha_{(1)})$ and above. Suppose Φ and α_1 are invertible, which requires $\alpha_{(1)}$ to be invertible (full rank). Since $A\Phi A'$ is identified, then its sub-matrices $A_2\Phi A_1'$ and $A_1\Phi A_1'$ are identified. Thus, $A_2A_1^{-1} = A_2\Phi A_1'(A_1\Phi A_1')^{-1}$ is identified. Since

$$A_1^{-1} = \begin{pmatrix} I_{d_X} & 0 \\ \beta_{(1)} & \alpha_{(1)} \end{pmatrix}^{-1} = \begin{pmatrix} I_{d_X} & 0 \\ -\alpha_{(1)}^{-1}\beta_{(1)} & \alpha_{(1)}^{-1} \end{pmatrix}$$

then

$$\begin{aligned} A_2A_1^{-1} &= \begin{pmatrix} \beta_{(2)} & \alpha_{(2)} \end{pmatrix} \begin{pmatrix} I_{d_X} & 0 \\ -\alpha_{(1)}^{-1}\beta_{(1)} & \alpha_{(1)}^{-1} \end{pmatrix} \\ &= \begin{pmatrix} \beta_{(2)} - \alpha_{(2)}\alpha_{(1)}^{-1}\beta_{(1)} & \alpha_{(2)}\alpha_{(1)}^{-1}. \end{pmatrix} \end{aligned}$$

Thus, $\beta_{(2)} - \alpha_{(2)}\alpha_{(1)}^{-1}\beta_{(1)}$ and $\alpha_{(2)}\alpha_{(1)}^{-1}$ are identified and whenever a column of $\beta_{(1)}$ is 0, we identify the corresponding column of $\beta_{(2)}$.

C.3 Likelihood

C.3.1 Simple Likelihood

For estimation, we let $\theta \sim \mathcal{N}(0, \Phi)$ and $\epsilon \sim \mathcal{N}(0, \Delta)$ and assume independence between the two. As a result our likelihood has the form:

$$L(\gamma) \propto -\ln|\Sigma(\gamma)| - tr(\Sigma(\gamma)^{-1}\hat{\Sigma})$$

where $\Sigma(\gamma) = A(\gamma)\Phi(\gamma)A(\gamma)' + \Delta(\gamma)$, $\hat{\Sigma}$ is the sample covariance and γ includes all of the "free" elements in A, Φ and Δ. In the Appendix, Section C.3.1 we give expressions for the gradient, which we use in the numerical optimizer for fast convergence.

Gradient

Suppose our likelihood has the following form, as in Williams (2017):

$$L(\gamma) \propto -\ln|\Sigma(\gamma)| - tr(\Sigma(\gamma)^{-1}\hat{\Sigma})$$

where $\Sigma(\gamma) = A(\gamma)\Phi(\gamma)A(\gamma)' + \Delta(\gamma)$ and $\hat{\Sigma}$ is the sample covariance. γ includes all of the "free" elements in A, Φ and Δ. We can find derivatives for arbitrary elements of these matrices to obtain the gradient used in optimization. First consider the derivative with respect to elements in A. First notice that

$$\frac{\partial \Sigma}{\partial A_{ij}} = A\Phi\frac{\partial A}{\partial A_{ij}}' + \frac{\partial A}{\partial A_{ij}}\Phi A',$$

$$\frac{\partial \Sigma}{\partial I_{ij}} = A\frac{\partial I}{\partial I_{ij}}A'$$

and

$$\frac{\partial \Sigma}{\partial V_{ij}} = \frac{\partial V}{\partial V_{ij}}.$$

Then for an arbitrary ij element of any of these matrices, call it X_{ij}, we have

$$\frac{\partial L}{\partial X_{ij}} = -tr\left(\Sigma^{-1}\frac{\partial \Sigma}{\partial X_{ij}}\right) + tr\left((\Sigma^{-1}\hat{\Sigma}\Sigma^{-1})'\frac{\partial \Sigma}{\partial X_{ij}}\right).$$

We obtain this expression using well known identifies for derivatives of matrices. See, for example, Petersen and Pedersen (2012).