Can Individualized Student Supports Improve Economic Outcomes for Children in High Poverty Schools?*

Benjamin Goldman, Harvard University
Jamie Gracie, Harvard University
Sonya R. Porter, U.S. Census Bureau

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Abstract

How can we improve outcomes for low-income students? We analyze the adult earnings impacts of the largest comprehensive student support program in the United States. Communities in Schools (CIS) places a "navigator" in high-poverty schools who provides an integrated system of supports to students, including academic (e.g., tutoring), economic (e.g., access to food assistance, housing), and mentoring. In 2023, CIS worked with 1.8 million students in 3,750 schools. Using later-treated CIS schools as a control, we estimate that four years of exposure to CIS generates a $1,500 (6% of control mean) increase in earnings at age 30. Effects are larger for students from low-income families and are driven by a reduction in non-employment and an increase in the probability of having a low-paying job. Each child exposed to four years of CIS is expected to pay an additional $9,000 in taxes between ages 18-65, which compares favorably to the direct cost of the program. Our results are relevant for the growing community school movement and illuminate a possible path for improving economic mobility in low opportunity neighborhoods.

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I Introduction

One third of children born into poverty in the US remain poor in adulthood (Chetty et al., 2018). Recent research has shown that neighborhoods play an important role in determining whether children are able to break the intergenerational cycle of poverty (Chyn and Katz, 2021). Within a given city, there are large, hyper-local disparities in rates of economic mobility. What can be done to increase economic mobility for children in low-opportunity neighborhoods?

There are two main policy approaches to this question. The first involves increasing access to existing high opportunity neighborhoods. There is a growing body of evidence showing that children benefit when they move away from low-mobility neighborhoods (e.g Chetty, Hendren and Katz, 2016; Chetty and Hendren, 2018; Chyn, 2018), but there is a limit to how far these programs can be scaled. The second approach involves investing directly in low mobility neighborhoods, which has the potential to improve outcomes for incumbent residents at large. In this paper we focus on one type of place-based program that connects children in high poverty schools to individualized services designed to meet their academic and non-academic needs.

Our setting is "Communities in Schools" (CIS), a national organization considered to be the country's largest dropout prevention program. CIS was founded in the late 1970's and today serves approximately 1.8 million students annually in more than 3,000 schools across the country. Like a growing number of interventions in a variety of contexts (e.g Bergman et al., 2019; Evans et al., 2020, 2023; van der Steeg, van Elk and Webbink, 2015; Weiss et al., 2019), the key component of the program is a navigator, called a "site coordinator," who is placed in the school. The site coordinator's job is to ensure that all students have their basic needs met.

CIS is part of class of interventions, known as integrated student support (ISS) programs, that take a "whole-child" approach to improving outcomes for students. The site coordinator helps broker access to resources that address traditional educational inputs, such as tutoring, as well as a variety of challenges outside of the classroom including food insecurity, housing instability, lack of access to health care, and social-emotional development. These services may be provided by the school, the government, or the local community.

When CIS enters a school, all students in the surrounding neighborhoods who attend the school are eligible to receive services. We start by documenting that CIS programs have effectively targeted low mobility neighborhoods across the country. Even conditional on their high poverty rates, neighborhoods with CIS programs have particularly poor outcomes for low-income children prior to the intervention. We ask whether the introduction of the program improves economic mobility for children in these neighborhoods.

In prior work there have been two key practical challenges to answering this question. First, there is a substantial time lag between when the interventions occur, during childhood, and when earnings are realized, in adulthood. It is not possible to measure the effects of recent ISS interventions on economic mobility directly, as the targeted students will not have reached adulthood. Other proxies for long-run outcomes, such as test scores may not be well suited to short-run analysis due to the fade-out patterns documented in other settings (e.g Cascio and Staiger, 2012; Chetty et al., 2011); interventions that increase test scores in a given year tend to diminish in each subsequent year. Second, even for programs that are sufficiently old, the analysis requires longitudinal data that tracks individuals from childhood into adulthood.

We use administrative records from the Census and IRS covering nearly the entire US population to estimate the long-run effects of one of the country's largest and longest running ISS programs. We start with the universe of federal tax records from 1979 to 2019. We focus on children born between 1978 and 1992, who are linked to their parents via dependent claiming on the 1040 form. We determine which students were exposed to CIS programs during childhood based on the neighborhood they grew up in and the year that each CIS program began. We then track these children longitudinally into adulthood to measure the effects of program exposure on adult outcomes. In our main analysis we focus on earnings at age 27, but show the results are robust to using other ages.

We find that students exposed to CIS programs during childhood have substantially higher earnings in adulthood. Four years of CIS exposure increases earnings measured at age 27 by approximately $1,500 (5.8%). Effects are larger for students exposed to more years of CIS, consistent with a dose response treatment effect. We estimate these effects through a design that compares the outcomes of students in schools that received CIS programs earlier to those treated in later years. The key identifying assumption is that absent the introduction of CIS, the outcomes of children in the treated schools would have evolved similarly to students in the control schools. We also estimate treatment effects using a matched control group of never-treated schools as well as a cross-sibling design, which both yield very similar estimates.

The increase in income persists well into adulthood. We find large effects on household earnings in 2019, the latest year we can measure in the data, when the children in our sample are in their late thirties on average. These increases are not driven by increases in marriage rates or by an increased propensity to move to a lower poverty neighborhood in adulthood.

Although all students in a school are eligible to receive supports when a CIS program begins, many of the services are tailored to challenges most often facing the lowest income students. Though we are unable to directly observe which students receive services from CIS in these data, we are able to look at heterogeneity by parent income levels. We find large positive effects on earnings for children in the bottom quartile of the in-sample parent income distribution and null effects for children from the top quartile. We show that the effects for low-income children are driven by a reduction in the probability of non-employment in adulthood and an increase in the probability of having a low-paying job. This is consistent with increased rates of high school graduation, which is a key outcome targeted by CIS.

From the perspective of CIS, there are large benefits to treated students relative to the costs that they incur. The cost to CIS is primarily the salary of the site coordinator, as well as administrative overhead costs. We estimate these costs to be approximately $1,000 per student for four years of the program (Cardinali, 2014). Many of the services provided to students are brokered through the site coordinator, but not directly paid for by CIS.

To compare the cost effectiveness of CIS to other, similar programs, the benefits to students must be weighed against the total cost of the program, not just the direct cost to CIS (Hendren and Sprung-Keyser, 2020). For example, CIS may provide a student with food from a food bank or help a family receive a Section 8 housing voucher, each of which has a cost incurred by a local nonprofit or the government. In our current data, we do not observe which services are provided, so we cannot directly estimate the costs. However, our earnings estimates imply that access to four years of CIS raises lifetime federal income taxes paid by $9,000 in net present value, which would offset costs incurred by the federal government. In ongoing work, we are attempting to measure the total costs more directly.

Our results build on a growing body of work focused on the role of student supports in improving outcomes. Prior evidence thus far has been mixed. While some interventions have been ineffective in the long run (Rodriguez-Planas, 2012), recent evidence shows that comprehensive support for students in public housing has persistent effects on labor market outcomes (Lavecchia, Oreopoulos and Brown, 2020; Oreopoulos, Brown and Lavecchia, 2017). A key difference in our work is that the treatment is at the school level, as opposed to at the individual student level. More broadly, our work builds on a growing body of work that highlights the value of personal assistance in programs targeted at young people (Gallego, Oreopoulos and Spencer, 2023).

The CIS model is similar in spirit to the growing "community school" movement, which is increasing in popularity across the country. In 2014, New York City piloted 45 community schools, designed to increase attendance and high school completion. In the 2022-23 school year there were 421 community schools in New York across the entire state (NYC Public Schools, n.d.). In 2021, the California State Legislature set aside more than $2 billion to expand the state's ISS program over the next ten years (Cal. Ed. Code § 8900). Our results suggest that these programs may have meaningful impacts on economic mobility in the years to come.

The rest of the paper is organized as follows. Section II provides details on the history and implementation of the Communities in Schools program. Sections III and IV describe the data and empirical strategy, respectively. Section V summarizes the results and Section VI concludes.

II CIS Program Details

Communities in Schools is one of the longest running ISS programs in the country. It was founded in 1977 in New York City by education activist Bill Milliken with the goal of bringing community resources into high-poverty schools in order to make them more accessible. Over the past several decades the program has expanded across the country and during the 2021-2022 school year CIS worked in 3,270 schools in 25 states and the District of Columbia. Among these, 42% of schools are elementary schools, 24% are middle schools, 21% are high schools, and the rest are combination schools. Organizationally, there are 111 affiliate organizations that sit under the umbrella national organization and run programs in a particular location (e.g. CIS of Richmond, Virginia) (Communities in Schools, 2023*b*). The primary goal of the program is to prevent students from dropping out of school. Each organization receives its funding from a variety of sources including from school districts and local/state governments, Title I funding, other organizations such as AmeriCorps, and private philanthropy (Communities in Schools, 2023*a*).

The key component of the CIS model is the site coordinator, who serves as a navigator for students and connects them to the resources they need to succeed. The site coordinators are full time employees who are almost always paid by the CIS affiliates directly. In large schools, there may be more than one site coordinator assigned to the campus. Nearly all site coordinators have a college degree and approximately 30% have some graduate degree, often in education or social work (Communities in Schools, 2023*a*).

When CIS enters a school, all students are eligible to be referred to the site coordinator and receive supports that address their basic needs. Among students who received services in 2022, approximately 90% were through this channel. In addition, a smaller subset of students, the remaining 10%, receive case management services from the site coordinator. These students, typically the most at risk of dropping out, meet frequently with the site coordinator and receive more hands on support.[1] In our data, we do not observe which students receive any particular service, including case management, so we instead focus on the aggregate effect of attending a CIS school.[2]

The services provided vary across students and schools and seek to address a wide array of challenges. They may include traditional academic interventions such as tutoring, but may also address many challenges outside of the classroom. This support may come from local non-profits or traditional government sources. For example, many CIS affiliates have partnerships with local food pantries that send students home with needed meals, but the coordinator may also help eligible

[1] In 2022, the most common reason for referral to case management was academic, followed by behavioral challenges, basic needs, attendance, and social emotional concerns (Communities in Schools, 2023*a*)

[2] We are currently in the process of building student level microdata in Texas that links student educational outcomes to information on CIS services received, which will allow for a more formal exploration of the effects of case management.

students and their families enroll in the SNAP program. Similarly, the site coordinator may help secure emergency rental assistance for a family on the brink of eviction, but may also help eligible families sign up for HUD programs. In addition, there are many lighter touch interventions such as providing students with glasses if they need them. Students may also be connected with students that provide emotional support and counseling as well as mentorship programs.

The schools that receive CIS are chosen at the discretion of the local affiliates; there is no systematic rule. From conversations with many CIS affiliate directors, it appears that they often target the schools they think are the most in need, but the sequence is often subject to idiosyncratic changes.[3] Affiliates often strive for continuity of care in their choices. For example, they are wary of creating a program in an elementary school if it would not be possible to have the program in the middle school that receives those students in later years.

Our sense is that schools, especially during this earlier period, did not solicit the CIS programs. It does not seem to be the case that a new principal hired in a particular school would be able to bring a CIS program to her school, unilaterally. The concern would be that a new principal comes in and establishes a CIS program, hires better teachers, and implements other changes all at the same time. This would make it challenging to identify the effect of CIS, specifically. Though we do not currently have data on school principals, we are able look at changes in personnel for CIS programs in North Carolina, using administrative school records. We do not see any change in the share of teachers that are licensed or hold advanced degrees at the time the CIS program begins in these schools.

III Data

Our primary analysis relies on two sources of data housed at the US Census Bureau: the 2000 and 2010 Census short forms and data from federal income tax returns in 1979, 1984, 1989, 1994-1995, and 1998-2019. The datasets are linked using a unique person identifier called a Protected Identification Key (PIK). PIKS are assigned using information including Social Security Numbers (SSNs), names, addresses, and dates of birth. The record linkage process is described in Wagner, Lane et al. (2014). We limit our analysis to individuals who are assigned a PIK.

We follow Chetty et al. (2020) to construct our analysis sample. Full details are available in that paper, but we highlight key elements here. We focus on children born in the 1978-1992 birth cohorts who we can successfully link to parents via dependent claiming on the tax returns. A child is linked to a potential parent if they are claimed on a 1040 tax form at some point beginning in 1994 by an adult who appears in the 2020 Numident and who was between the ages 15-50 in

[3] For example, one director mentioned that some of the CIS funding in his region cannot legally be used in elementary schools, which affected the order of the roll-out to new schools.

the year the child was born. We also impose that the child appears in the 2020 Numident. This process excludes children who themselves are unauthorized immigrants or who are claimed by unauthorized immigrants as only individuals with SSNs appear in the Numident file. We assign the first such person(s) to claim the child in the tax years available as the invariant parent(s).

After assigning parents to children, we construct measures of parent income. We define parent income as mean income between child ages 13-17, assigning 0's in years that parents do not file a tax return. We exclude families with 0 or negative mean income over the age range, as this typically signals high levels of parent wealth. This leaves us with approximately 57 million children born between 1978-1992 in our primary sample.

Our goal is to determine which children were exposed to CIS programs during childhood. Exposure is a function of which schools students attended in which years. We start with a list of National Center for Education Statistics (NCES) IDs and the year in which a CIS program began at those schools.The list of schools has two key sample restrictions. First, we only have programs that were operating in the 2019-2020 school year. We do not have any information on programs that began at some point and then shut down. The schools in the list may be positively selected because we condition on persistence. Second, we do not have any data on schools in Texas. Texas comprises a large part of the national CIS network, approximately half of CIS schools today are in Texas, but these programs are operated directly by the Texas Education Agency and treated differently. These schools that are in the sample are distributed across the entire country, as shown in Figure I Panel *A*. We use this list to determine which cohorts of students at a given school would have been exposed to CIS. We do not directly observe school enrollment in our data. Instead, we rely on the precise location where children lived during childhood. We assign addresses to parents using the address supplied on the 1040 tax return in each year for filers. For non-filers, beginning in 2005 we assign address information using information returns including W-2 forms. For a given child's address at each age, we assign expected school using a school catchment zone crosswalk also used in Chetty et al. (2018).[4]

This leaves us with 689,200 children who were born in ever-treated tracts in our sample. Among this total, approximately 400,000 would be predicted to be exposed to the program, based on the timing of when it began. This group comprises our main analysis sample. In the remainder of the section we define the key variables used in our analysis.

Variable Definitions

Parent Income. We define parent income as pre-tax income at the household level. We use

[4] We assign Census tracts to school catchment areas using data generously provided to us by Peter Bergman on the intersection of Census tracts with high school catchment boundaries, obtained from Maponics (2017).

Adjusted Gross Income in years in which a parent files a tax return and for non-filers prior to 2005 we code a zero. Starting in 2005, we impute household income for non-filers using W-2 income. Our baseline definition of parent income is the mean income over child ages 13-17. We focus primarily on parent percentile income ranks, which we define relative to other parents with children in the same birth cohort.

Parent Location. We use parent location to determine where children grew up. In each year, parents are assigned the address from which they filed their 1040 tax return. For non-filers, we use address information from information returns such as W-2s.

Child Income. We define child household income analogously to parent household income. In addition we construct a measure of individual income, ignoring income from spouses for married individuals. We primarily focus on child income at age 27. We rank this income against other children in the same birth cohort.

Child Race. We assign race and ethnicity to children using the information they report on the 2010 Census short form, 2000 Census short form or the American Community Survey.[5]

Child Employment. We construct a measure of employment using a child's individual income. Children are working if they have non-zero individual income in a particular year.

Child Location in Adulthood. Children's locations are measured based on the address from which they file tax returns in a particualr year or the most recent year in which an address is available. For non-filers, we obtain address information from W-2 forms and other information returns.

IV Empirical Strategy

Our goal is to estimate the effect of CIS exposure on adult earnings. In an ideal experiment, the schools with CIS programs would have been randomly selected and we could compare the adult outcomes of students in those schools to their peers in schools that were not selected. In practice, however, CIS targets schools in high poverty neighborhoods with low levels of student achievement. In Figure I Panel *B* we show the characteristics associated with having a CIS program. In the first series we show that CIS tends to locate programs in cities with higher levels of racial diversity and low levels of education. We also see that they are particularly high poverty places with low levels of economic mobility. In the second series we show that this is not because they tend to locate in cities with these characteristics generally; even within a city CIS programs serve neighborhoods that standout on these dimensions.

[5] We define four mutually exclusive race/ethnicity groups. We assign all individuals with Hispanic ethnicity to a single group, then assign non-Hispanic white and non-Hispanic Black individuals to their own groups. All other respondents are in the fourth group. For simplicity, we refer to this classification of race and ethnicity as race, throughout.

Nationally, there is a negative relationship between economic mobility and neighborhood poverty rates. In Figure I Panel *B* we show that CIS neighborhoods have high poverty rates and low rates of economic mobility. In Panel *C*, we unpack this relationship further. The typical CIS neighborhood clearly diverges from the national relationship. That is, conditional on local poverty rates, the rates of economic mobility are particularly low.

We use a difference in differences design that compares the change in outcomes for students in a school with a CIS program to the changes in other, comparison schools. This approach controls for any time invariant differences between the treated and comparison locations. There are many ways to choose potential comparison schools. In our primary specification we focus on later treated CIS schools, but in robustness analysis we also match treated schools to never treated schools and the results are essentially unchanged.

In this setting, we think about treatment as beginning for a particular birth cohort as opposed to in a particular year. When a program begins in a school, all cohorts after the first treated birth cohort have the potential to be treated, whereas older students had already aged out of the school by the time the program began. We start by defining this first treated cohort for each school. Concretely, if we consider an high school with a CIS program that begins in 2000, we say the first treated cohort were the children born in 1987. For a high school with a program that begins in 2000, we say the first treated cohort were the children born in 1983.

Each census tract in our crosswalk is assigned to its local elementary school, middle school, and high school. In many cases, CIS begins programs at multiple schools in the same neighborhood. To accurately capture which cohort was first treated in the entire neighborhood, we take the minimum first treated cohort at the tract level. For simplicity, we refer to this as the "treatment cohort".

The goal is to run an event study with $\underline{T}$ pre-periods and $\bar{T}$ post-periods. In our baseline specification, we construct a stacked dataset analogous to the approach in Sun and Abraham (2021). For each treatment cohort, T, we stack as the control group all individuals with treatment cohort $T+\bar{T}$. The resulting data contain one row per individual X treatment cohort. We estimate a 5 period pre-period and 5 period post-period, but we show our results are robust to extending this window.

In our baseline specification, for each treatment cohort, we define the pool of control schools as the set of schools that receive a CIS program and have a first treated cohort more than 5 cohorts later. In this stacked dataset we run an event study with a 5 cohort pre- a 5 cohort post-period, fully interacted with the treatment cohort. Figure A.1 contains a visualization of this procedure. Our baseline specification is:

$$Y_i = \sum_{\substack{t \in [-5,5] \\ t \neq -1}} \beta_t \mathbf{1}\{c(i) - \bar{C}(l(i)) = t\} \cdot T_{l(i)} + \delta_{c(i),\bar{C}(l(i)),r(i),p(i)} + \phi_{l(i),\bar{C}(l(i)),r(i),p(i)} + \varepsilon_i \tag{1}$$

for some outcome Y, such as age 27 earnings rank, measured for individual i. We denote the childhood location (census tract) and birth cohort for person i as $l(i)$ and $c(i)$, respectively. $\bar{C}(l(i))$ captures the treatment cohort, defined at the tract level, as described above. We include interactions with parent income quartile, $p(i)$, and race, $r(i)$, to make comparisons between individuals with the same demographic background. The fixed effects in δ capture the path for the untreated individuals in each cohort by parent income by race group and the ϕ fixed effects allow for a tract by parent income by race group specific intercept. We cluster standard errors at the high school catchment zone level.

In some instances, we focus on a more parsimonious specification that considers a single (omitted) pre-period, an intermediate post-period, and a longer-run post-period. In these cases, we use the following specification and focus on the coefficient $\beta_{post,3-5}$ as our measure of the post-period:

$$\begin{aligned} Y_i = \; & \beta_{post,0-2} \mathbf{1}\{0 \leq c(i) - \bar{C}(l(i)) \leq 2\} \cdot T_{l(i)} + \beta_{post,3-5} \mathbf{1}\{3 \leq c(i) - \bar{C}(l(i)) \leq 5\} \cdot T_{l(i)} \\ & + \tilde{\delta}_{c(i),\bar{C}(l(i)),r(i),p(i)} + \tilde{\phi}_{l(i),\bar{C}(l(i)),r(i),p(i)} + \varepsilon_i \end{aligned} \tag{2}$$

The key identifying assumption in both the event study and the more parsimonious design is that absent the introduction of the CIS program, the outcomes in treated and comparison schools would have evolved similarly. This would be violated if the timing of CIS treatment were correlated with other changes in the school that also impact adult earnings. For example, suppose a struggling school hires a new principal who is motivated to improve outcomes and brings in CIS in addition to hiring higher quality teachers. In our design we would estimate the joint effect of CIS and the higher quality teachers. In practice, we believe the start dates for CIS program, especially going back in time were idiosyncratic.

Another potential concern is that families may select into CIS program participation by moving to or from neighborhoods with a CIS program. This would bias our estimates of program exposure. To account for this, we assign CIS exposure to students based on the locations where their parents lived in the year they were born, which is less likely to be chosen directly because of CIS. We track family location throughout childhood to estimate the “first stage” on our measure of exposure. We then use this to scale up our estimates when considering magnitudes.

V Results

We start by showing the first stage. To do so, we estimate equation 1 with the number of actual years in childhood spent in a CIS tract on the left hand side, with treatment determined by age 0 location. Figure II Panel *A*, shows the event study coefficients. It is clear from the figure that the treated students have higher exposure to CIS than the control students and that age 0 location is persistent throughout childhood. To understand the magnitudes, consider the coefficient for the first treated cohort (event time = 0). If no children moved from their age 0 tract, this coefficient would mechanically be 1. Instead, because families move during childhood, we estimate a coefficient of 0.5.[6] This ratio is fairly consistent as we consider coefficients to the right of 0. The potential amount of exposure, i.e. the no-move counterfactual, exhibits a similar shape that increases and then tapers off due to the combination of elementary schools, middle schools, and high schools in the sample.

The key question is whether exposure to CIS improves outcomes in the long run. In Panel *B* of Figure II we plot our baseline estimates for adult household income rank measured at age 27. There are a couple of key things to note. First, the pre-period estimates are near 0 and statistically insignificant. Prior to the introduction of CIS in the treated tracts, students in the treated and control neighborhoods are on very similar trajectories; outcomes only begin to diverge after the program begins. Second, the shape of the coefficients is very similar to that of the first stage, consistent with a dose response treatment effect. Birth cohorts exposed to more years of CIS in the treatment tracts have better outcomes relative to the control students compared to students in the first treated cohorts.

Using the difference in differences design in equation 2, we estimate that exposure to CIS increases age 27 percentile income ranks by 1.14 ranks. This is equivalent to an $850 increase, off a baseline mean of approximately $26k. Scaling up by the first stage, we estimate that 4 years of exposure to CIS increases earnings in adulthood by approximately $1,500 at age 27. For this sample, that amounts to a 5.8% increase in annual earnings. For comparison, Chetty, Friedman and Rockoff (2014) find that 4 years of a 1 SD higher quality teacher increases earnings by approximately $1,000 at age 27.

In Figure III, we show that this design is robust to a variety of specifications. One potential concern is that CIS programs enter a city right when outcomes are about to improve, regardless of CIS exposure. We estimate a version of Equation 2, adding a CZ interaction to the cohort fixed

[6] Our first stage only captures deviations in exposure due to moving. In practice, however, there may be further attenuation if students do not attend their local public school or attend private school. For the time period we study, these forms of school choice were less common than they are today (Wang, Rathbun and Musu, 2019). Using the same crosswalk, Chetty et al. (2018) show in North Carolina that the crosswalk is very reliable over time. Our first stage is an upper bound for true exposure and any re-scaled estimates will be a lower bound.

effects. In this specification we only compare treated schools in a given city to later treated schools in the same city, and we show that the results are essentially unchanged. We also address concerns that the results are driven by our choice of the window around treatment or our use of a restricted set of treatment cohorts. In the third bar we show results for cohorts 6-8 after treatment (as opposed to 3-5) for the maximum set of treatment cohorts and again the results look very similar.

We then turn our attention to the role of the specific control group. In the fourth bar we modify the design to use a within family design. We compare siblings who were exposed in different amounts to the CIS program based on their birth cohort. We show that the younger siblings (those exposed to more years of CIS) have better outcomes than their older siblings and that the magnitudes of these differences are again similar to our baseline specification. In the remaining bars, we move away from the later treated design entirely. Instead, we match each treated school to a group of never treated control schools in CZ's that never have a CIS program. Again, the treatment effect is very similar to our baseline specification.

We also show that our design is robust to using different measures of income as the outcome. First, we show that treatment effects are essentially unchanged when we use individual income ranks as opposed to household income ranks; the increases are not driven through an increase in spousal income. We also show that the effects are not unique to measuring income at age 27. In the final bar we measure income in 2019, the last year available in the tax data. The children in our sample are in their mid- to late-thirties at this point. This suggests the effects we measure persist throughout the students' time in the labor market.

These effects capture the effect for the average student in a CIS neighborhood. While all students in a school are eligible to receive supports, in practice many of these services are targeted at lower-income students. In our data, we do not directly observe which students receive services or even which types of services were offered at each school. Instead, we start by estimating treatment effects separately by groups of student parent income. Because the income distribution in schools with CIS skew toward the bottom of the distribution, we split students into four in-sample quartiles and estimate the DID coefficients for each quartile with separate regressions.

Figure IV Panel *A* plots the DID coefficients for students from the bottom and top 25% of the parent income distribution. The estimates are noisy, but the effects are suggestively larger for the students at the bottom of the distribution, who are more likely to be in need of the types of services that CIS helps to provide. For students from high-income families, we find a null effect of the introduction of the CIS program.

What drives these earnings effects for low-income students? Understanding this is central for policy makers potentially considering increasing access to ISS-style programs. The stated goal of the program is increasing persistence in school so that students ultimately graduate from high school. We do not have information on high school graduation for the full population in our data.

We have graduation information for the subset of individuals who receive the ACS in adulthood, but we do not have sufficient power to estimate treatment effects on this subsample. We can however, assess whether CIS helps students avoid the worst labor market outcomes, which may be consistent with increased rates of high school graduation.[7]

To do so, we construct four outcome variables – an indicator for being in each of the adult income quartiles at age 27. Using the sample of students from the bottom 25% of the parent income distribution, we run the DID specification using each outcome to look at how the probability that a students ends up in each quartile changes with the introduction of the CIS program. Figure IV Panel *B* plots these four treatment effects. We find that the probability that a low-income student ends up in the bottom quartile of the income distribution, earning less than $15,000 per year at age 27, decreases by approximately 2.5 percentage points. There is essentially a one to one substitution into the second quartile; we estimate that low-income students are 2.4 percentage points more likely to have earnings in the second quartile. We can show that these effects are driven by a decrease in the probability that the low-income students are unemployed at age 27. We test this directly by estimating the treatment effect on the probability that an individual is working at age 27, as measured by having a W-2. We cannot reject that the treatment effects on the bottom quartile are entirely driven by increased employment.

We started by showing that CIS programs operate in high poverty, low economic mobility neighborhoods. How much of the earnings effects are driven by students leaving these neighborhoods in adulthood? We test this by estimating the DID treatment effects on the poverty rate of an individual's Census tract age age 27. We estimate a precise null effect on poverty rate. We can rule out poverty rates that are more than 0.4 percentage points lower in adulthood for treated students. This suggests that our effects are driven by improved labor market outcomes in a given neighborhood as opposed to driven by flight out of higher poverty neighborhoods.

VI Discussion and Conclusion

Taken together, our results show that the introduction of the CIS program increased earnings for incumbent residents, particularly those growing up in low-income families. These effects are primarily driven through increased employment in adulthood, as opposed to geographic mobility to more prosperous labor markets. For the average student in a CIS school, the treatment effects are large. We find that the typical student has approximately 6% higher earnings at age 27 after four years of CIS. Supposing this proportional increase is constant over the lifetime, we project a lifetime increase of approximately $100,000 using the framework from Hendren and Sprung-Keyser

[7] Porowski and Passa (2011) and Somers and Haider (2017) use publicly available school data to show suggestively that high school graduation rates do increase in schools that receive CIS, but these effects are not precisely estimated.

(2020). Discounting the future earnings impacts to present day value and assuming this group of individuals would face approximately a 20% average tax rate, we estimate that the typical student who receives CIS generates more than $9,000 in federal tax revenue.

How does this compare to the cost? Without knowing exactly which services students are receiving, it is difficult to say precisely. In ongoing work, we are constructing data to estimate treatment effects on federal program participation, including SNAP, Medicaid, and programs run by HUD. Increased participation in these programs is a cost of CIS from the perspective of the federal government. Other services, such as food from a local food pantry, does not fall directly on the government's books. Regardless of the particular suite of services offered, the cost of the site coordinators depends on the annual salary they receive. We believe this equates to approximately $200 per student per year (Cardinali, 2014). Conservatively, if the cost per student to CIS was $1,000 per student for four years, then there would still be an $8,000 wedge between the estimated increase in tax revenue per student and the costs. If the cost of the services do not offset this, then it could be financially beneficial for the federal government to increase access to programs like CIS.

Another way to think about the overall magnitudes of the effects we find is to consider the neighborhood level as opposed to the per student effects. We began by showing that CIS programs tend to locate in neighborhoods with low levels of economic mobility. The typical CIS program in our sample is located in a neighborhood at the 17th percentile of the national distribution of economic mobility. If we were to apply our earnings effects to these neighborhoods and recompute the distribution, the CIS tracts would fall at the 26th percentile of the distribution. This is a substantial increase, but also highlights that large improvements in economic mobility at the neighborhood level require multi-pronged approaches that layer a variety of programs together in a given community.

How would these results extend to other programs, such as the community schools movement in New York, California, Michigan and beyond? Without clearer evidence on the precise mechanisms of these earnings effects, it is challenging to say. In ongoing work in Texas, we plan to estimate contemporaneous effects on in-school outcomes such as attendance, test scores, and completion rates. This will allow us to trace the effects out over the life cycle of the students. In addition, we will be able to match students to the services they receive, which will shed light on which components of the intervention seem most beneficial for students. Nonetheless, these results add to a growing body of evidence that comprehensive student supports can have large and persistent effects on outcomes long after students have left school (Oreopoulos, Brown and Lavecchia, 2017; Lavecchia, Oreopoulos and Brown, 2020).

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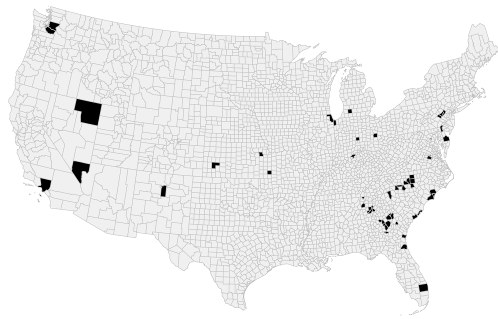
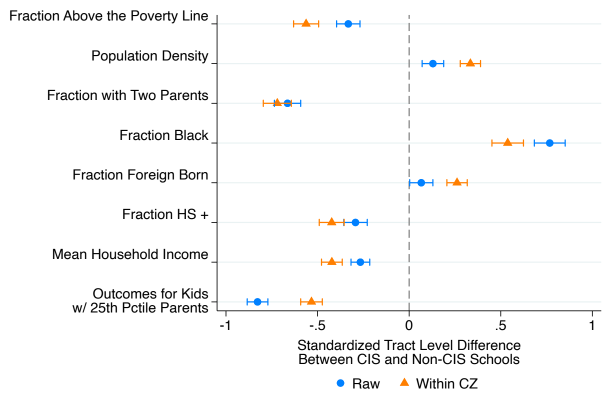
FIGURE I: CIS Location Characteristics

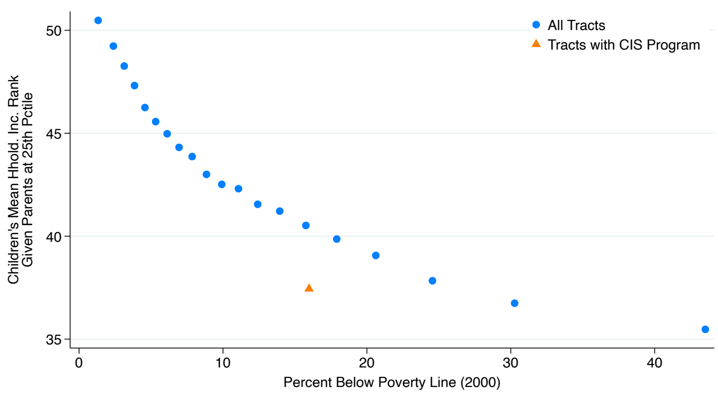
A: Counties with CIS Programs in Sample



B: Neighborhood Correlates of CIS Programs



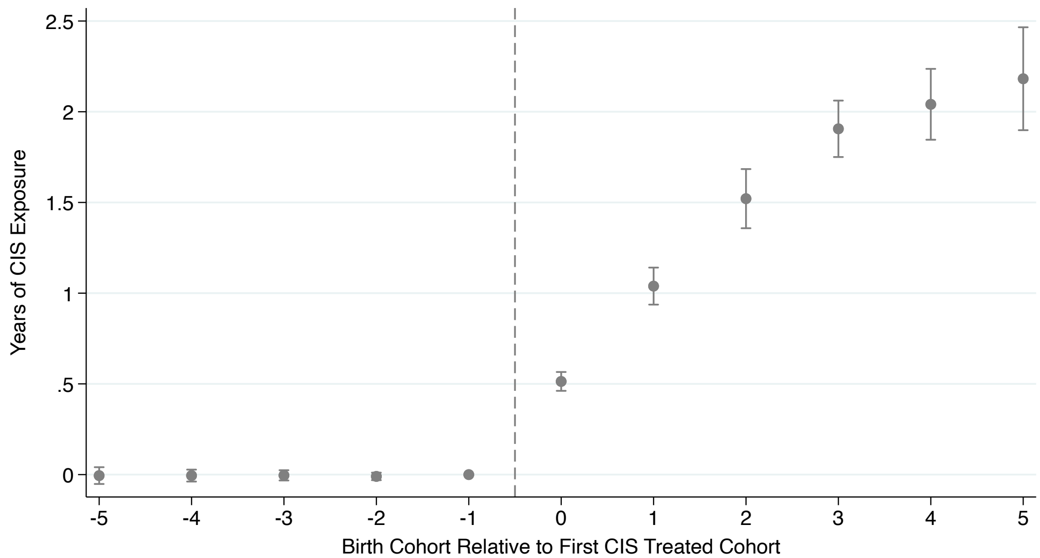
C: Relationship Between Tract-Level Economic Mobility and Poverty Rates



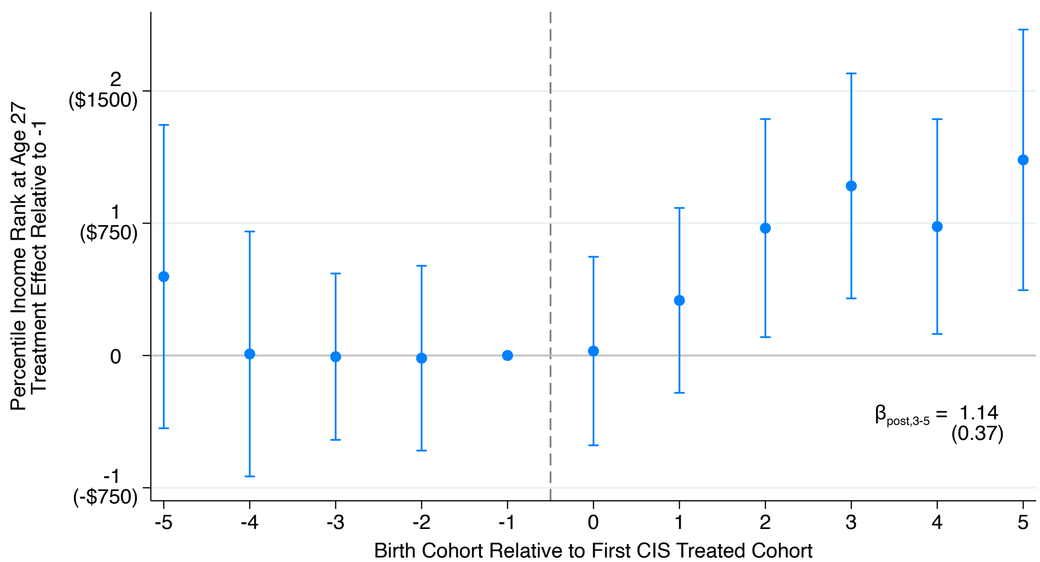
Notes: This figure provides details on the CIS locations in our sample. In Panel *A* we show in black the counties with a CIS school in our analysis sample. In Panel *B* we present correlations with CIS locations and tract level covariates. Each cofficient is from a separate tract-level regression where the dependent variable is a standardized covariate and the independent variable is an indicator for the tract having a CIS program. All covariates are measured in the 2000 Census, except for economic mobility, which comes from Chetty et al. (2018) .The circle series is the baseline regression and the triangle series adds commuting zone fixed effects. In Panel *C*, we show a binned scatter plot of the relationship between economic mobility and tract level poverty rates. We construct ventile bins of the poverty rate, weighted by tract population, and plot the mean rates of economic mobility and poverty in each bin. The triangle shows the mean poverty rate and level of economic mobility for Census tracts with a CIS program in our sample.

FIGURE II: First Stage and Reduced Form

A: First Stage: Number of Years of CIS Exposure

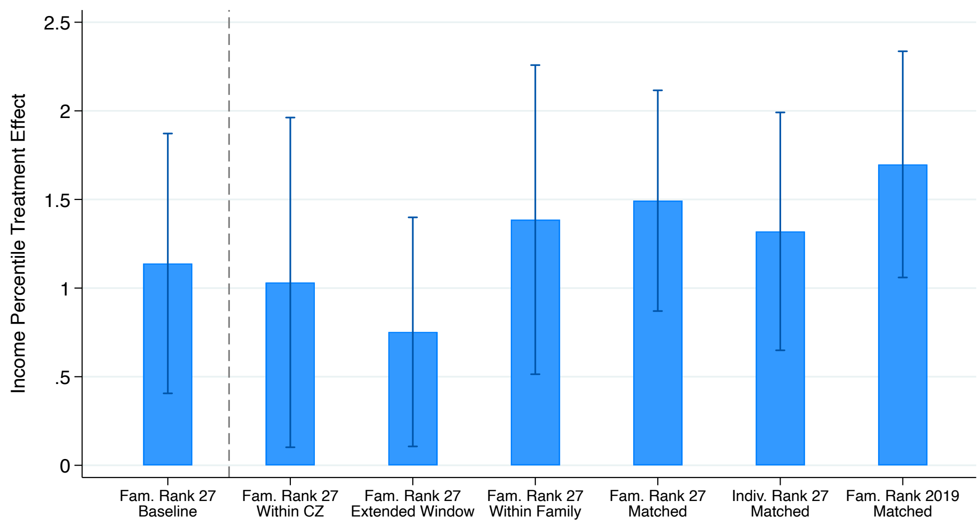


B: Reduced Form: Percentile Income Rank at age 27



Notes: This figure plots the β_t coefficients from Equation 1. In both panels, we restrict the sample to treatment cohorts 1983-1987, ensuring a balanced set of birth cohorts contributing to each coefficient. Panel *A* plots the first stage where the left hand side is number of actual years of CIS exposure constructed by tracking moves throughout childhood. Panel *B* pots the reduced form with age 27 household percentile income rank on the left hand side. The coefficient $\beta_{post,3-5}$ shown in Panel *B* is estimated by Equation 2 on the same sample. Standard errors in this and subsequent figures are clustered at the high school catchement zone boundary.

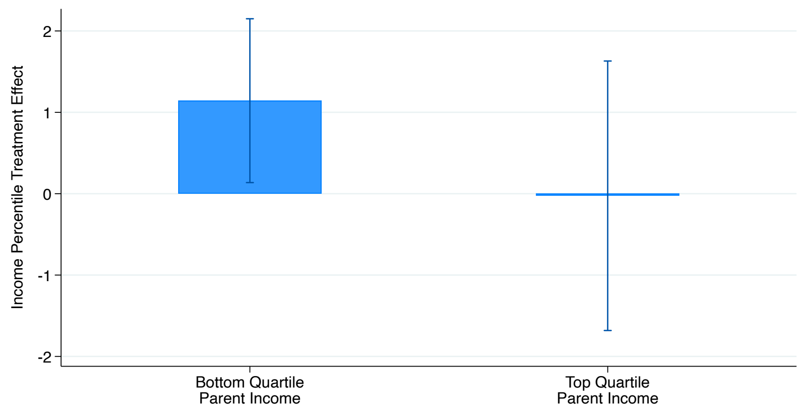
FIGURE III: Treatment Effect Robustness



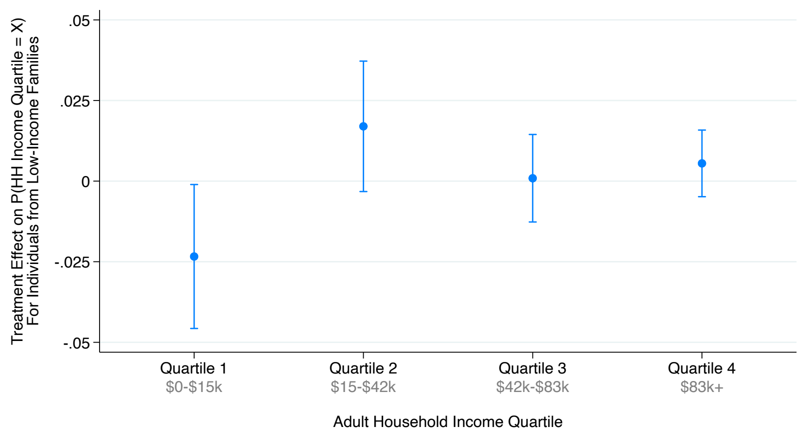
Notes: This figure presents our baseline DID specification, as shown in Figure II Panel *B* as well as a variety of robustness specifications. In the first bar to the right of the dashed line we fully interact commuting zone fixed effects to estimate within CZ treatment effects. In the next bar we estimate a longer post period, showing the average effect 6-7 cohorts after the program begins (as opposed to 3-5). In the third robustness check, we run a within-family design comparing siblings exposed to different amounts of CIS. We present the average coefficient 6-7 cohorts after the program begins. In the fourth check, we present results from an alternative design comparing treated schools to matched, never-treated control schools. In the final two bars, we continue to use the matched design, but change the outcome variable to individual income ranks (excluding spousal income if present) and measuring income in 2019, as opposed to age 27.

FIGURE IV: Unpacking the Treatment Effects

A: Parent Income Heterogeneity

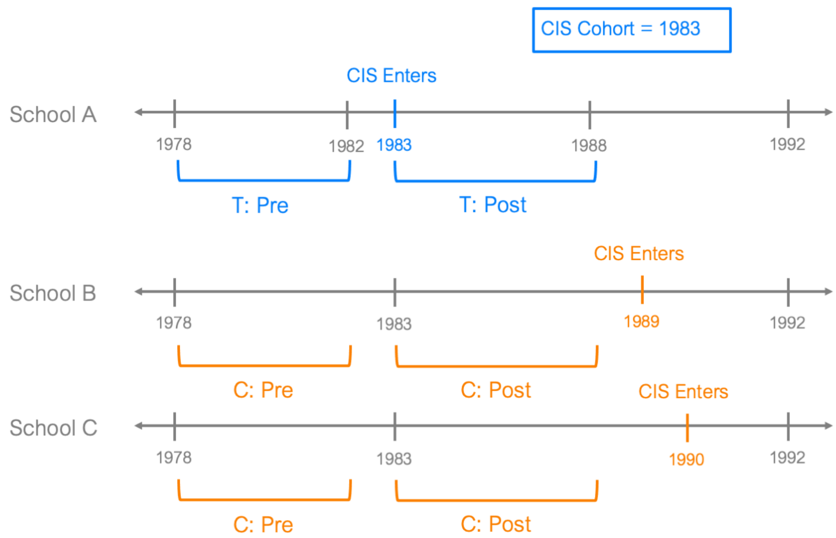


B: Effects on Adult Income Quantiles for Low-Income Students



Notes: In this figure, we present results from two exercises designed to shed light on the main treatment effects documented in Figure II Panel *B*. In Panel *A* we present two separate DID coefficients estimated using equation 2. We split our main analysis sample into four quartiles of parent income and estimate separate treatment effects. We plot coefficients from the bottom 25% and top 25% of families. In Panel *B*, we restrict the sample to the bottom 25% of families on parent income and estimate DID treatment effects on the quantile outcomes of the children in adulthood. We define an indicator for having age 27 income in each of the four adult income quartiles and estimate the treatment effect of CIS on each outcome separately.

FIGURE A.1: Visualization of Data Stacking



Notes: This figure shows an illustration of the stacking procedure we use, described in Section IV. In School A the first cohort to receive CIS is the 1983 cohort. In School B and School C we construct a control group using these later treated schools. In each case, we estimate a 5 cohort pre-period and a 6 cohort post-period.