In the School, Down the Block: Achievement Effects of Peers in the School, Neighborhood, and Cohort

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Abstract

I estimate the effect of mean peer ability on students' test scores using data on all Michigan public school students over thirteen years. I consider peers in the same cohort at school—as well as peers in adjacent cohorts, and peers living on the same block. I contribute two novel findings to the literature. First, school peer effects are much stronger than block effects. For peers in the same cohort, the school effect is 10 times larger. Second, cohort membership plays a substantial role in determining peer influence in schools but not in neighborhoods. For students in the same school, the adjacent-cohort peer effect is 40-80% smaller than the same-cohort effect. Meanwhile, for students living on the same block, peer effects are similar, regardless of cohort. These results are robust to a regression discontinuity design focusing on students near the birthdate cutoff for entry into kindergarten. I also find evidence that peers in the older cohort matter more than peers in the younger cohort, particularly in the school context, suggesting that relative age also plays a role in determining peer influence.

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1 Introduction

When it comes to improving the economic mobility of children, it is hard to find stronger tools than the schools they attend and the neighborhoods they grow up in. Research on factors that relate to intergenerational and economic mobility finds that both schools and neighborhoods likely play a strong role in explaining how individuals and families improve their socioeconomic well-being and transmit that well-being to their children (Black and Devereux, 2010). On a more micro level, the causal case for schools and education as sources of economic mobility has been repeatedly made, with evidence finding that changes in schools (Deming et al., 2014), teachers (Chetty et al., 2014), and school resources (Jackson et al., 2016) have substantial impacts on earnings and employment in adulthood. However, this education literature often focuses solely on the effects of schools, neglecting neighborhoods as another important pillar of the economic mobility, education, and long-term welfare we are ultimately trying to foster.

School and neighborhood effects on children are deeply intertwined, particularly in the US. Although this is often understood to be because living in certain neighborhoods lends access to higher quality schools (such as in Laliberté (2018)), improvements in education outcomes from growing up in better neighborhoods are observed even in cases where there are no observable increases in school quality (Chyn, 2018). While schools themselves are certainly important for a child's education, it should not be surprising that neighborhoods influence a child's growth and educational development through channels other than schools themselves, as students spend only part of their lives at school. Indeed, we can conceive of neighborhood effects in general, and on education specifically, as a bundle of different effects, with local school quality as only one component of that bundle. Lower-poverty neighborhoods can mean less crime (Oreopoulos, 2003), better health (Sanbonmatsu et al., 2011), higher parental earnings (Baum-Snow et al., 2019), and

better quality peers (Agostinelli et al., 2020), all of which have evidence supporting their positive impacts on education in their own right. The purpose of this study is to hone in on one particular component of this bundle of aspects that a child's neighborhood provides to their education and bring it into the school context: peer effects. That is, how do educational effects of the peers a child is exposed to by living in their neighborhood influence and compare to the effects of the peers a child is exposed to in school?

While the school peer effects literature is broad (see Sacerdote (2014) for a thorough review), only a handful of papers in the economics literature have estimated the causal effects of neighborhood peers. In the most related paper to this one, Agostinelli (2018) uses variation in cohort ability levels within schools to estimate that a one percentile rank increase in peer ability at age 16 increases own ability by 0.63 percentile ranks. Fernández (2021) finds that having neighborhood peers enroll in a university increases the likelihood that students will enroll in university themselves. And finally, List et al. (2020) finds that nearby neighbors of children randomly assigned to a high-quality pre-K program also experience positive spillovers from this intervention. Collectively, these papers show that neighborhood peers do influence students' education in numerous impactful ways. Building on that work, this paper is the first in the economics literature, to the best of my knowledge, to estimate neighborhood peer effects jointly with school peer effects and examine their interaction with peer cohorts.

One difficulty with both peer effects and neighborhood effects research is the selection problem. That is, peer groups and neighborhoods both form endogenously, with agents selecting into neighborhoods and peer groups based on the characteristics of other agents. Research such as Heckman and Landersø (2021) shows how failing to account for this endogenous sorting can bias estimates of neighborhood effects. School peer effects papers, such as Carrell et al. (2013) and

Imberman et al. (2012), often rely on experimental or quasi-experimental variation in classroom peer assignment to identify their estimates. To overcome this selection problem, my research design combines two approaches: 1) controlling for the abilities of students within the same school or block but adjacent cohorts and 2) instrumenting for actual school cohorts with assigned cohorts based on birthdate. The identifying assumption of this approach is that selection into schools and neighborhoods is not specific to the characteristic of peers in each cohort, but rather occurs on a broader set of neighborhood, school, and peer characteristics. In addition, the use of assigned cohorts based on birthdate eliminates the possibility of endogenous cohort selection itself, such as through academic redshirting or grade retention. To complement this method, I also test the robustness of my main results by running a quasi-border discontinuity design with a bandwidth of one month around the cohort birthdate cutoff for kindergarten entry. This approach yields similar results and validates that my main model adequately deals with selection.

Applying this identification strategy to data on all public school students in the state of Michigan between the 2007-2008 and 2019-2020 school years, I show that 1) school peer effects are substantially stronger than neighborhood peer effects, and 2) the cohorts of one's peers play a strong role in influencing peers' effects on each other in the school context but a much weaker role in the neighborhood context. I estimate that own-cohort school peers increase students' test scores by 0.3 standard deviations for a one standard deviation increase in average peer ability, similar to school peer effect sizes found in previous literature (Sacerdote, 2014). The effect drops off by 0.15 standard deviations for school peers in the cohort above and 0.2 standard deviations for school peers in the cohort below. In contrast, block peers increase students' test scores by 0.04 standard deviations for a one standard deviation change in peer ability. Block peer effects vary by cohort by 0.02 standard deviations at most, indicating that the school cohort does not play nearly as strong of

a role among peers in the neighborhood context as in the school context. Finally, I find the effect of school peers in the cohort above is 0.05 higher than for school peers in the cohort below, providing evidence that age also plays a role in peer influence, particularly in the school context.

The remainder of this paper is structured in the following way. Section 2 describes my model of selection bias as well as my identification strategy to overcome these biases. Section 3 describes the Michigan education dataset used for this study and defines the terms used for the remainder of the paper. Section 4 details the empirical methodology I use applying my identification strategy to obtain my estimates. Section 5 details my results. Section 6 runs through numerous robustness checks to my main empirical strategy. Section 7 details several heterogeneity analyses, and Section 8 concludes.

2 Bias and Identification Strategy

In this section, I lay out the potential sources of bias that may confound estimating block-cohort or school-cohort peer effects without random or quasi-random peer group assignment. For each source of bias, I also describe how parts of my identification strategy deal with this source of bias, and what assumptions need to be true to eliminate any remaining bias. For simplicity, I will be referring to school peers for the remainder of this section, but all of the concepts discussed generalize to block peers as well.

To formalize my model of unobserved selection, let $Y_{i,t}$ be the observed outcome of interest Y (e.g. test scores) of observation i at time t, $\overline{Y}_{-i,c,s,t}$ be the average of the same for the all other individuals in cohort c and school s at time t, following the standard linear-in-means model from the literature (Sacerdote, 2014). A simple model seeking to determine the peer effects of block-

cohort peers would be:

$$\overbrace{Y_{i,t}}^{\text{Own Test Score}} = \beta_0 \overbrace{+\beta_1 \overline{Y}_{-i,c,s,t-1}}^{\text{Own Cohort Avg Test Score}} + \nu_{i,t} \tag{1}$$

Where $t-1$ peer scores in place of t are used to mitigate the reflection problem, denoted as $\overline{Y}_{-i,c,s,t-1}$.[1] The issue of the reflection problem is described in further detail in Appendix Section B. My first potential source of bias in the equation above is selection into schools (or blocks). Let $\alpha_{s,t}$ be an unobserved time-varying school-specific (or block-specific) component that is correlated with both $Y_{i,t}$ and $\overline{Y}_{-i,c,s,t-1}$ in $\nu_{i,t}$. Because my main specifications include school (and block group) fixed effects, only time-varying unobserved effects are potentially confounding. Let $\nu_{i,t} = \eta_{i,t} + \alpha_{s,t}$. Rewriting equation 1, we have:

$$\overbrace{Y_{i,t}}^{\text{Own Test Score}} = \beta_0 \overbrace{+\beta_1 \overline{Y}_{-i,c,s,t-1}}^{\text{Own Cohort Avg Test Score}} \underbrace{+\alpha_{s,t}}_{\text{Unobserved Time-Varying School Effect}} + \eta_{i,t} \tag{2}$$

$$\alpha_{s,t} = \gamma_0 + \underbrace{\gamma_1 \overline{Y}_{-i,c,s,t-1}}_{\text{Confounding Correlation}} + \nu_{s,t}, \quad \gamma_1 \neq 0 \tag{3}$$

Which shows that β_1 will be biased by $\alpha_{s,t}$.[2] In the selection story, $\alpha_{s,t}$ is the tendency of families to select into improving schools that have increasingly higher-ability students over time or to sort into similarly improving neighborhoods, as shown in Heckman and Landersø (2021). That is, $\alpha_{s,t}$ is the set of unobserved family characteristics that influenced observation i to live in school s with the other $-i$ peers that also affect student i's outcomes.

Many papers in the peer effects literature attempt to overcome this unobserved selection prob-

[1] Results are also robust to alternative methods, such as using peer scores lagged by two years or from third grade.
[2] Substituting in equation 3 for $\alpha_{s,t}$ in equation 2 shows that the parameter estimated on $\overline{Y}_{-i,c,s,t-1}$ will be $\beta_1 + \gamma_1$.

lem with random or quasi-random variation in peer groups (Sacerdote, 2014). My solution to this selection problem, in the absence of random assignment to exploit. is to control for the outcomes of other school peers in adjacent cohorts:

$$\overbrace{\overline{Y}_{c-1,s,t-1}}^{\text{Cohort Below Avg Test Score}} \quad , \quad \underbrace{\overline{Y}_{c+1,s,t-1}}_{\text{Cohort Above Avg Test Score}}$$

$$\alpha_{s,t} = \gamma_0 + \gamma_1 \overline{Y}_{-i,c,s,t-1} + \gamma_2 \overline{Y}_{c-1,s,t-1} + \gamma_3 \overline{Y}_{c+1,s,t-1} + \nu_{s,t} \tag{4}$$

$$Y_{i,t} = \beta_0 + \beta_1 \overline{Y}_{-i,c,s,t-1} + \beta_2 \overline{Y}_{c-1,s,t-1} + \beta_3 \overline{Y}_{c+1,s,t-1} + \alpha_{s,t} + \eta_{i,t} \tag{5}$$

Where $\overline{Y}_{c-1,s,t-1}$ and $\overline{Y}_{c+1,s,t-1}$ are average outcomes for peers in the cohort below and cohort above student i but still in school s, measured in $t-1$. As equation 5 shows, β_1 is identified if $\gamma_1 = 0$. That is, if unobserved time-varying school (or block) effects only correlate with adjacent cohorts simultaneously such that no more correlation remains own cohort, then we interpret the own-cohort school (or block) effects are causal. Even if $\gamma_1 \neq 0$ in equation 4, the inclusion of $\overline{Y}_{c-1,s,t-1}$ and $\overline{Y}_{c+1,s,t-1}$ likely reduce the magnitude of γ_1 as long as the time-varying school selection occurs across cohorts over time rather than sharply diverging between adjacent cohorts.

We can also further relax the identification assumption of $\gamma_1 = 0$ in equation 4 by looking at relative effects between cohorts. To identify $\beta_1 - \beta_2$ and $\beta_1 - \beta_3$, the identification assumption instead becomes $\gamma_1 = \gamma_2 = \gamma_3$. That is, the relative peer effects between adjacent cohorts are identified if the remaining endogenous correlation is the same or similar magnitude. Like the argument over the reduction of the magnitude of γ_1 in the paragraph above, this identification

assumption for relative cohort effects holds if time-varying selection into block or school does not vary sharply year-to-year. If parents are changing their degree of selection into schools or blocks over time, particularly in a way that correlates with peer ability (e.g. parents observe a school has started having higher ability students more recently and want to send their kids there), this assumes that this affect is only observed at the level of several cohorts, rather than based on the abilities of a single cohort. In my robustness check that uses a quasi-border discontinuity design that restricts the set of instrumenting peers to those born within one month of the school cutoff, I take this restriction further by lowering the likelihood that parents would be able to select into a block based on observed age differences ex-ante. Section 6.1 details this identification strategy further and shows the robustness of my main specification to this weaker identification assumption.

However, selection may not occur only at the block or school level. If families sort into cohorts based on the abilities of children in those cohorts,[3] then the estimates in equation 5 will still be biased. Let $\eta_{i,t} = \varepsilon_{i,t} + \alpha_{c,t}$, where $\alpha_{c,t}$ represents time-varying selection into cohort c that correlates with both $\overline{Y}_{-i,c,s,t-1}$ and $Y_{i,t}$. Equation 5 would then look like:

$$Y_{i,t} = \beta_0 + \beta_1 Y_{-i,c,s,t-1} \overbrace{+\alpha_{c,t}}^{\text{Unobserved Time-Varying Cohort Effect}} + \beta_2 Y_{c-1,s,t-1} + \alpha_{c-1,t} + \beta_3 Y_{c+1,s,t-1} + \alpha_{c+1,t} + \alpha_{s,t} + \varepsilon_{i,t}$$

This final issue is where the use of cohort entry dates comes in. By using only the abilities of peers assigned to their cohort based on month and year of birth in my main specification, α_c, selection into cohort, is eliminated.

[3] E.g. through policies such as redshirting or grade retention/promotion

3 Data

Before describing my empirical strategy in detail, I will describe my dataset and terms for this analysis to provide context to my methodology. The data for this analysis comes from the Michigan Education Data Center (MEDC).[4] It contains data on the universe of public school students in Michigan between the 2007-2008 and 2019-2020 school years, comprising tens of millions of observations over those thirteen years. The dataset I created for this analysis comes from three source datasets: student enrollment data, which contains variables on student characteristics (birth date, race, gender, poverty status, IEP status, etc.), student assessment data, which contains test scores, and student geocode information, which contains student census block. All of these datasets are at the student-year level.

A student is considered to be living in the same neighborhood as another if they share the same census block. Although a census block is not a perfect match for a true city block in all cases, it is the smallest geographic unit available. Figure 5 contains an example map of downtown East Lansing, near the Michigan State University Economics Department. The entire map in red is one census tract, and each small square is both one city block and one census block. One issue with the use of census blocks as neighborhoods is they extend beyond one city block in more sparsely populated areas. Because of this, I exclude any student living in a census block classified in the 2010 census as rural.

While dropping students in rural census blocks is one of my sample restrictions, I also make several others before running my OLS regressions. First, I drop students not in grades 5 through 8, since test scores are first produced in grade 3, peer scores are calculated in the previous year, and

[4] I owe thanks to the Michigan Education Research Institute (MERI) for guiding me through the application process and making this data available. This data source is not identical to the data maintained by the Michigan Department of Education (MDE) and Michigan's Center for Educational Performance and Information (CEPI).

students in the cohort below grade 4 do not have lagged test scores. Next, I drop any observations missing control or outcome variables, either for themselves or peers, including demographics (race, gender, special education status), month and year of birth, and math and reading test scores. After that, I drop students who have zero students in their own or adjacent cohort in school or block. This leaves 2,999,834 observations in my OLS math sample and 2,983,697 observations in my OLS reading sample.

To get from my OLS analysis sample to my main specification sample, which instruments for actual cohorts with assigned cohorts, I make two additional sample restrictions. First, I drop all students who do not have at least one student assigned to own or adjacent cohorts in either school or block.[5] Second, I drop all students who are missing average lagged test score data or peer demographic data for any of the own or adjacent assigned cohorts in school or block. These two changes leave a final assigned cohort sample of 2,666,141 observations for math and 2,656,132 for reading.

In Tables 1 and 6, I compare my assigned cohort sample to the full sample of Michigan public school students in grades 5 through 8. Looking at Table 1, the most striking difference between the two samples is the urbanicity of school attended, where students are 12.3 percentage points more likely to attend a suburban school and 13.1 percentage points less likely to attend a rural school. This is mostly due to dropping students in rural census blocks. Additionally, sample students are less likely to be white (4.7 percentage points) and are more likely to be Black (3.9 percentage points). Michigan's rural population is overwhelmingly white, so this likely reflects the rural/urban urbanicity sample restriction. Finally, students in the analysis sample live in slightly

[5]Counts of students assigned to school-cohorts are set to zero if there are no students in that actual school-cohort. For example, if a school is K-5 and the student of interest is in grade 5, they will have no cohort-above-assigned students in grade 6, even if there are students in their cohort who should be in grade 6 based on their birthday.

denser neighborhoods and attend slightly larger schools, with 36 more students per cohort in school (22%) and 0.3 more students per cohort in the neighborhood (19%). In Table 6, I again compare my assigned cohort analysis and full samples, but this time using ACS 5-year data on block groups. Table 6 confirms the racial composition changes shown in Table 1, while also showing that my sample census blocks are more populous (56 more residents) and have higher household incomes ($2,375 higher median household income on average). In total, the sample is more urban, slightly more advantaged and higher performing, and denser than the full Michigan grade 5-8 public school population.

To help illustrate the motivation for the use of assigned cohort as an instrument, Figure 1 shows compliance with cohort assignment in my analysis sample, both overall and by birth month. As the figure shows, almost all cohort non-compliance comes from students in a lower cohort than assigned by the statewide cutoff and their birthdate. 17% of students are in a lower cohort than assigned, while only 0.01% are in a higher cohort. The second panel of Figure 1 separates the first panel's histogram by birth month and shows there is considerable heterogeneity throughout the year. Notably, the tendency to attend a lower cohort than assigned increases throughout the year, as students become younger relative to their assigned classmates. These two trends in assigned cohort compliance are likely a combination of three factors 1) academic redshirting (holding children back from entering kindergarten at age 5), 2) grade retention, and 3) individual districts' (unobserved) earlier entry cutoffs. Factors 1 and 2 are the most pressing for the case of endogeneity since they are more individual decisions. Factor 3 may also be endogenous, depending on the decision-making process that districts engage in. Regardless, as long as being in a district with an earlier cutoff data does not *increase* the likelihood of staying in a cohort the student is no longer assigned to, the IV estimation using the assigned cohort should still have a tractable and valid interpretation while

dealing with remaining potential sources of endogeneity, such as grade retention/promotion and academic redshirting.

4 Empirical Strategy

To lay out my empirical strategy, I start by detailing my naive OLS regression, followed by my instrumental variables specification. The basic structure of my dependent variable and independent variables of interest is:

4.1 Naive OLS

$$\overbrace{Y_{i,t}}^{\text{Own Test Score}} = \beta_0 \overbrace{+\beta_1 \overline{Y}_{-i,c,s,t-1}}^{\text{Own cohort, }\textbf{school}} \underbrace{+\beta_2 \overline{Y}_{c-1,s,t-1}}_{\text{Cohort below, }\textbf{school}} \overbrace{+\beta_3 \overline{Y}_{c+1,s,t-1}}^{\text{Cohort above, }\textbf{school}}$$

$$\overbrace{+\beta_4 \overline{Y}_{-i,c,b,t-1}}^{\text{Own cohort, }\textit{block}} \underbrace{+\beta_5 \overline{Y}_{c-1,b,t-1}}_{\text{Cohort below, }\textit{block}} \overbrace{+\beta_6 \overline{Y}_{c+1,b,t-1}}^{\text{Cohort above, }\textit{block}}$$

Where $Y_{i,t}$ is outcome Y of student i in school year t. Each $\overline{Y}$ is the mean standardized test score of peers in each relative school-by-cohort and block-by-cohort combination.[6] $\overline{Y}_{-i,c,s,t-1}$ is the mean test score Y of all other $(-i)$ students in cohort c and school s in year t, measured in $t-1$, and $\overline{Y}_{-i,c-1,s,t-1}$ and $\overline{Y}_{-i,c+1,s,t-1}$ are the mean test scores Y of all other students in cohorts $c-1$ an $c+1$ and school s. Each $\overline{Y}_{-i,c+j,b,t-1}$ is the same but for peers within census block b. The notable

[6] When there are zero students in any of the groups, the mean is instead replaced with zero. To prevent this from causing the regression to conflate having zero peers with having average-ability peers, a dummy variable is included in all regressions for each peer group category that is equal to one if there are no students in each respective peer group.

difference from equation 5 in Section 2 is that both block and school peer effects are estimated simultaneously. This has two benefits: 1) the resulting estimates show the value-added of peers in each block or school context, reflecting that both sources of skill formation occur together, rather than in isolation, and 2) estimating simultaneously further enhances my identification strategy by controlling for how selection into block may affect school peer estimates, and vice versa.

Adding in my control variables and fixed effects (and combining terms into summations), my full naive OLS equation is:

$$Y_{i,t} = \beta_0 + \sum_{(j=-1)}^{1} \beta_j \overline{Y}_{-i,c+j,s,t-1} + \sum_{(j=-1)}^{1} \alpha_j \overline{Y}_{-i,c+j,b,t-1}$$
$$+ \delta_1 X_{i,t} + \sum_{(k \in s,b)} \sum_{(j=-1)}^{1} \delta_{j,k} \overline{X}_{-i,c+j,k,t} + \Gamma_{m,c,g,s,b_g,t} + \varepsilon_{i,t}$$

All X variables are controls for individual, relative school-cohort, and relative block-cohort characteristics. $X_{i,t}$ is a vector of individual characteristics including race/ethnicity, gender, and special education status. Each $\overline{X}_{-i,c+j,k,t}$ is a vector of means of controls for the same characteristics, as well as number of students in group and average age of students in group, among the other peers $(-i)$ in relative cohorts $c-1$, c, and $c+1$ and school s and block b. Finally, $\Gamma_{m,c,g,s,b_g,t}$ is a matrix of fixed effects for month of birth, cohort (year of birth), cohort, school, census block group, and school year, respectively.

As discussed in Section 2, this estimation strategy controls for any endogenous variation common between adjacent peer cohort groups. With the addition of school and block group fixed effects, any unchanging endogenous correlation between own ability and peer abilities should be taken care of as well. However, as also discussed in Section 2, this estimation strategy is vulnerable

to sorting into cohorts based on peer abilities (or other factors correlated with both own and peer abilities, such as income and education levels).

4.2 Assigned Cohort IV

4.2.1 Reduced Form

To address this, I use an instrumental variables strategy that instruments for school-by-cohort peer ability and block-by-cohort peer ability with the abilities of students who are assigned to own or adjacent cohorts based on their birthdate and the Michigan school cohort entry cutoff date at age 5. The reduced form equation for this IV strategy is:

$$Y_{i,t} = \beta_0 \sum_{(j=-1)}^{1} \beta_j \overline{Y}_{-i,ac+j,s,t-1} + \sum_{(j=-1)}^{1} \alpha_j \overline{Y}_{-i,ac+j,b,t-1}$$
$$+ \delta_1 X_{i,t} + \sum_{(r \in c,ac)} \sum_{(k \in s,b)} \sum_{(j=-1)}^{1} \delta_{j,k,r} \overline{X}_{-i,r+j,k,t} + \Gamma_{m,c,g,s,b_g,t} + \varepsilon_{i,t}$$

The main difference between the reduced form equation and the naive equation is the use of assigned cohort peer groups instead of actual cohort peer groups. Each $\overline{Y}$, X, and γ represent the same concepts as in the previous equation, mean peer group ability, controls for average peer group characteristics, and fixed effects, but now $\overline{Y}$ and $\overline{X}$ are indexed by ac, which I am using to represent assigned cohort. The $\overline{X}$'s indexed by ac are the proportions of observable characteristics and the number of students for each assigned cohort, in addition to the controls for each actual cohort. For each Y and $\overline{X}$ in each school ac, the value is set to zero if there are no members in the actual cohort at school. For example, if a school is K-5 and student i is in grade 5, there will be no assigned

students in cohort $c+1$, even if there are students in their cohort who should be in grade 6 based on their birthday. Instead, those students are not included in any of the ac measures and all $ac+1$ values are set to zero, and an indicator variable for zero students in $c+1$ is set to one.

4.2.2 Instrumental Variables

The first stage equations mirror the reduced form equation on the right-hand-side, but now have $\overline{Y}_{-i,c,s,t-1}$, $\overline{Y}_{c-1,s,t-1}$, $\overline{Y}_{c+1,s,t-1}$, $\overline{Y}_{-i,c,b,t-1}$, $\overline{Y}_{c-1,b,t-1}$, and $\overline{Y}_{c+1,b,t-1}$ on the left-hand-side instead as follows:

$$\begin{aligned}\overline{Y}_{-i,c,s,t-1} = \beta_0 + \sum_{(j=-1)}^{1} \beta_j \overline{Y}_{-i,ac+j,s,t-1} + \sum_{(j=-1)}^{1} \alpha_j \overline{Y}_{-i,ac+j,b,t-1} \\ + \delta_1 X_{i,t} + \sum_{(r\in c,ac)} \sum_{(k\in s,b)} \sum_{(j=-1)}^{1} \delta_{j,k,r} \overline{X}_{-i,r+j,k,t} + \Gamma_{m,c,g,s,b_g,t} + \varepsilon_{i,t}\end{aligned}$$

With the other five first stage equations having $\overline{Y}_{c-1,s,t-1}$, $\overline{Y}_{c+1,s,t-1}$, $\overline{Y}_{-i,c,b,t-1}$, $\overline{Y}_{c-1,b,t-1}$, and $\overline{Y}_{c+1,b,t-1}$ on the left-hand-side instead of $\overline{Y}_{-i,c,s,t-1}$. The second stage equation combines the naive OLS equation and the predicted values from the first stages:

$$\begin{aligned}Y_{i,t} = \beta_0 + \sum_{(j=-1)}^{1} \beta_j \widehat{\overline{Y}}_{-i,c+j,s,t-1} + \sum_{(j=-1)}^{1} \alpha_j \widehat{\overline{Y}}_{-i,c+j,b,t-1} \\ + \delta_1 X_{i,t} + \sum_{(r\in c,ac)} \sum_{(k\in s,b)} \sum_{(j=-1)}^{1} \delta_{j,k,r} \overline{X}_{-i,r+j,k,t} + \Gamma_{m,c,g,s,b_g,t} + \varepsilon_{i,t}\end{aligned}$$

where β_j through α_j now gives our exogenous peer effects for school and block peers.

5 Results

5.1 OLS Results

Before turning to the results of my assigned cohort IV estimation, I will briefly discuss the results of OLS estimation described in Section 4.1. Column 1 of Tables 2 and 3 display the results of the OLS estimation for math and reading scores, respectively. The top panel shows the estimated peer effects for school-by-cohort peers and the bottom panel shows the same for block-by-cohort peers. Each of the three rows within each panel shows the effects by each relative peer cohort: cohort below, $c-1$, own cohort, c, and cohort above, $c+1$. As with all regressions in this paper, robust standard errors are used, with clustering at the school level. Notably, the large sample size, displayed in the bottom row of the table, results in small standard errors, with almost all results in both tables significant at the 0.01 level.

For the math scores in column 1 of Table 2, the results show peer effects of a 0.3 standard deviation increase in own test scores for a one standard deviation increase in the ability of peers in the same cohort at school. These estimates show an own-cohort school effect in line with the classroom peer effects literature (Sacerdote, 2014), a drop off of 0.2 standard deviations when compared to the cohort above, and an even larger drop in effect size of 0.25 standard deviations for the cohort below. Effect sizes for reading scores, in column 1 of Table 3, follow a similar pattern of differences in relative school-cohort effects, with lower overall magnitudes. This latter result of varying adjacent-cohort drops in effect size suggests 1) relative cohort plays a substantial role in influencing peer effects in the school context, and 2) relative *age*, in addition to cohort, plays an important role as well.

The story for block peers is markedly different. First, the effect sizes for block peers are

much smaller, ranging from a 0.04 to a 0.05 standard deviation increase in own score for a one standard deviation increase in peer score for math, and a 0.03 to 0.04 range for reading. In this OLS specification, we can also detect differences between the three adjacent cohorts among block peers, although the sizes of the differences, a maximum of 0.01, are much less substantial for block peers. Notably, and most robustly to specifications shown later in the paper, effect sizes are larger for block-cohort peers in the cohort above, indicating the presence of a relative age effect in the block context as well.

As discussed in Section 2, we should expect these OLS estimates to deal with school-level and block-level bias, particularly for differences between adjacent cohort peer groups, but not address sorting into cohorts. For a cohort sorting example, if parents choose to engage in academic redshirting (delay Kindergarten entry by one year) in response to the increasing abilities over time of school or neighborhood peers, either to increase contact with high-performing peers or reduce contact with low-performing peers, then these estimates will be biased.

5.2 Main Results

Next, we turn to my preferred specification, which instruments for the abilities of peers in each school-by-cohort and block-by-cohort group with the abilities of peers *assigned* to each cohort group based on birthdate and cohort entry cutoff at age 5 (laid out in Sections 4.2). Estimates for the remainder of this section follow the same format as the OLS columns, with the addition of Kleibergen-Paap F Stats for the IV estimates.[7]

[7] Similar to a Cragg-Donald test for weak instruments with multiple endogenous variables, as proposed by Stock et al. (2002), a Kleibergen-Paap Wald test uses an appropriate cluster-robust degrees of freedom. See Andrews et al. (2019) for further discussion.

5.2.1 Reduced Form Results

To explore the assigned cohort IV, I first show reduced form results produced by regressing own ability on the average ability of students in assigned cohorts at school and in the block. Column 2 of Tables 2 and 3 show the results of this estimation, using the equation shown in Section 4.2.1. Importantly, all estimates in both tables are highly significant (p <0.01), indicating that the weak instrument problem is likely not a concern. As a second precaution against weak instruments, all results will be reported with a Kleibergen-Paap Wald F statistic. Overall, estimates follow a similar pattern to the OLS estimates, especially for block-cohort peers. [8]

5.2.2 IV Results

The Assigned Cohort IV results, shown in Figure 2, with statistics listed in column 3 of Tables 2 and 3, are very similar to the OLS estimates. Across both math and reading scores and peer and block groups, assigned cohort point estimates are slightly larger, although not always to a statistically detectable ($p < 0.05$) degree. Math own-cohort peer effects range from 0.33 standard deviations for own cohort, with a 0.16 standard deviations decrease in effect size for the cohort

[8] However, there are some differences for school peers: lower magnitudes for their own cohort and higher magnitudes for adjacent cohorts. The first potential explanation for this is that most assigned cohort noncompliance is into the cohort below, not above, as shown in Figure 1. This means that using assigned cohort peers is, for the most part, reassigning some peers to the cohort above their actual cohort. Because the OLS estimates suggest that actual own cohort peers have the strongest influence, assigned cohort own effects are weakened by adding actual below cohort peers and subtracting actual own cohort peers to the assigned own cohort peer pool. In turn, above effects are strengthened by adding actual own cohort peers to the assigned above-cohort pool. Thus, relative cohort effects shown in the OLS estimation explain two of the three changes in school-cohort magnitude.

To help answer why assigned cohort-below peer effects are stronger than actual cohort below peer effects, we need an additional explanation. One potential cause of this is that cohort sorting is endogenous: lower-ability peers may be more likely to either be held back or enter kindergarten a year later. This effect would be offset for the assigned cohort above peers because of the stronger cohort exposure effect (the potentially lower ability peers are actually in own cohort), but not for the cohort below, which is receiving peers from the use of assigned cohorts who are two actual cohorts below, who likely have an even weaker cohort exposure effect. In short, using assigned cohort peers means also potentially reassigning endogenously cohort-switching peers, which can, as a result, boost peer effects.

above, and a 0.26 standard deviations decrease in effect size for the cohort below. The own-cohort school findings continue to match classroom peer sizes effects found in the literature, with the novel finding of substantial drop-offs in peer effects for students not in the same cohort combined with a somewhat-countervailing age-influence effect. My additional novel finding of substantially lower block peer effects holds as well. Math block-by-cohort effects range from 0.03 for own cohort to 0.05 for cohort below and 0.06 for cohort above, still showing little variation by cohort with a slight age-influence effect.

5.3 Summary

Across the two specifications, OLS and assigned cohort IV, there are four main takeaways: (1) peer effects vary substantially by relative cohort in the school context but not the block context, (2) math peer effects are stronger than reading peer effects, (3) own cohort school peer effects are substantially larger than block peer effects, about 0.3 standard deviations for a one standard deviation increase in peer ability and block-cohort peer effects are 0.04 standard deviations regardless of the relative cohort, and (4) cohort above school peer effects are higher than both cohort below and all block-cohort peer effects, at about 0.15 standard deviations. This own-cohort school peer effect is about the same effect size found in the quasi-experimental classroom peer effects literature of about 0.3 standard deviations (Sacerdote, 2014). In contrast, this effect is much smaller than the effect of academic redshirting, which is about 0.7 standard deviations (Elder and Lubotsky, 2009; Cascio and Schanzenbach, 2016), though like academic redshirting and other education findings, the effect is stronger for math scores than reading scores.

6 Robustness Checks

Although all four conclusions[9] are consistent across both OLS and assigned cohort IV specifications, there are still several specification changes that the results could be sensitive to. First, I will introduce my main robustness check, a birth cutoff discontinuity IV, explain the motivations, and show the results, which are similar to the main specification. Next, I will also show that results are robust to a handful of other alternative specification changes, including the inclusion of extra controls for low-income and English language learner status, the use of peer abilities measured in 3rd grade instead of $t-1$, the addition of own ability in 3rd grade as an extra control, and dropping a school year where Michigan changed its standardized testing scheme.

6.1 Birth Cutoff Discontinuity IV

6.1.1 Cutoff Student IV Estimation

For this specification check, I restrict the set of peers used to instrument for school-cohort and block-cohort ability to peers born within one month of the cohort entry cutoff date when they are age five. This relaxes the assumption made for the assigned cohort IV that parents do not select into schools and blocks based on peer age as long as the peers are within several cohorts of each other. Now, the assumption is that parents may select based on the characteristics of peers in the same cohort, but because age is used by parents as a proxy for cohort, are unlikely to be able to distinguish the cohort of peers born within one month of each other. Appendix Section C goes into further detail about Michigan's cutoff policy, statistics for the cutoff groups, and additional sample restrictions used for this robustness check.

[9] Peer effect variation by cohort within school but not block, stronger math score effects, stronger school effects than block effects, and stronger cohort-above effects than cohort-below effects.

When using birth cutoffs, we have a subset of neighborhood peers who are plausibly otherwise identical but differentiated only based on whether they are in the same cohort as the student of interest. To help illustrate, Figure 3 shows a student in a census block with six other children of similar age, who are then sorted into cohorts at school by their birthdates.[10] As Figure 3 shows if the student of interest, i, is born in January 2005 and the cohort entry cutoff is December 1st the sorted peers are then separated into three separate groups when they attend school: the cohort below the student of interest, the same cohort as the student of interest, and the cohort above. The four neighborhood peers born in November and December 2004, as well as November and December 2005, are the two groups of cutoff peers in this example: plausibly similar in most characteristics except for their interaction with the student of interest at school as part of membership in the same cohort. This exercise can also be repeated for any different combination of neighborhood peers where at least one cutoff student in any of the four cutoff months[11] and at least one other non-cutoff student is present in the neighborhood within one cohort of the cutoff student.

The estimation strategy for the cutoff student IV is similar to a fuzzy border discontinuity. Figure 4 helps illustrate the connection between the first stage equations above and the identification strategy and Figure 3. The first two terms in the equation below use the average ability of cutoff school peers born just before or after the cutoff for being the oldest peers in student i's cohort. The second two terms do the same, but with the average abilities of cutoff school peers born just before or after the cutoff for being the *youngest* peers in student i's cohort. The next four terms repeat the process, but now for block cutoff peers, rather than school. Adding in controls for counts of students and proportions of observable characteristics in each cutoff group, the first stage equation

[10] This example is an unusually population-dense block. The majority of students in the sample have only one cutoff student in their block-cohort or block-and-adjacent-cohort

[11] November of cohort above, December of the same cohort, November of the same cohort, and December of cohort below.

looks like this:

$$\begin{aligned}\overline{Y}_{-i,c,s,t-1} = \beta_0 + \beta_1 \overline{Y}_{Dec,c,s,t-1} + \beta_2 \overline{Y}_{Nov,c+1,s,t-1} + \beta_3 \overline{Y}_{Nov,c,s,t-1} + \beta_4 \overline{Y}_{Dec,c-1,s,t-1} \\ + \alpha_1 \overline{Y}_{Dec,c,b,t-1} + \alpha_2 \overline{Y}_{Nov,c+1,b,t-1} + \alpha_3 \overline{Y}_{Nov,c,b,t-1} + \alpha_4 \overline{Y}_{Dec,c-1,b,t-1} \\ + \delta_1 X_{i,t} + \sum_{(m \in Nov,Dec)} \sum_{(k \in s,b)} \sum_{(j=-1)}^{1} \delta_{j,k,m} \overline{X}_{-i,m,c+j,k,t} + \Gamma_{m,c,g,s,b_g,t} + \varepsilon_{i,t}\end{aligned}$$

With the other five first stage equations having $\overline{Y}_{c-1,s,t-1}$, $\overline{Y}_{c+1,s,t-1}$, $\overline{Y}_{-i,c,b,t-1}$, $\overline{Y}_{c-1,b,t-1}$, and $\overline{Y}_{c+1,b,t-1}$ on the left-hand-side instead of $\overline{Y}_{-i,c,s,t-1}$.

6.1.2 Cutoff IV Results

The results of this alternative estimation strategy are displayed in column 4 of Tables 2 and 3. These tables use the equations in Section 4.2.2 to estimate peer effects by instrumenting for actual school-by-cohort and block-by-cohort abilities with those of students born on either side of their cohort entry cutoff date when they entered kindergarten. Joint Kleibergen-Paap F Stats in column 4 of Tables 2 and 3 are 126 and 155, respectively, suggesting that weak identification is not biasing these results. Standard errors are slightly higher for school peer effects and up to 8 times larger for block peer effects. Although there is some loss of statistical power, there is still enough to validate the assigned cohort IV findings. For each peer group, school and block, in each panel, I also show a row for "Joint Test Pval". This is the p-value from an F test of whether all three group-cohort estimates are jointly zero.

Although the Cutoff IV point estimates are smaller across the board than the assigned cohort IV, the overarching story remains the same. For math, school-cohort peer effects for own cohort

are now 0.25 (instead of 0.33), 0.07 for cohort above, and 0.003 for cohort below (cohort below not statistically significant at the 0.1 level). Math block-cohort peer effects are 0.02 for own cohort, 0.03 for the cohort above, and 0.01 for the cohort below, jointly significant at the 0.01 level. As in previous estimates, reading results are lower in magnitude. These results suggest that, while the assigned cohort specification may slightly overestimate the specific school peer effects point estimates, the main story of own-cohort estimates close to the literature, substantial effect size drop for adjacent cohorts, larger school peer effects than block peer effects, and higher effects for cohort-above than cohort-below peers still hold. The results of this robustness check validate that, although there may be a small degree of selection bias in my main estimates, the four key takeaways still largely hold even under weaker identification assumptions on sorting.

6.2 Other Checks

Next, in Tables 4 and 5, I examine robustness to four different alternative specifications: including controls for low-income and English learner statuses, using grade 3 scores for peer ability, including a control for own ability in 3rd grade, and dropping the year of test type transition in Michigan, respectively. Unlike previous tables, the effects for each relative cohort are now in descending rows, instead of columns. Each column now represents each of the four robustness checks. All four regressions have similarly high Kleibergen-Paap F statistics, indicating that, even with lower sample sizes (and correspondingly larger standard errors), the first stages are still strong enough to avoid weak instrument bias.

The first column of Tables 4 and 5, ”All Controls”, includes additional controls for low-income and English learner statuses that are excluded from the main analysis due to lack of availability in the first five years of the data. Controls are added for student i's own low-income and English

learner status, the proportions of peers in cohort below, own, and cohort above school and block peer groups with low-income and English learner statuses, and the same proportions of peers in own and adjacent assigned cohorts. Point estimates are somewhat lower across the board, although the differences between individual coefficients and the main assigned cohort IV cannot be rejected at the 5% level. Although there is some loss in precision, the relatively small change in point estimates for the inclusion of additional controls suggests that not controlling for low-income and English learner statuses has a minimal effect on the estimates, if anything, and does not overall change the interpretation of the four takeaways.

"G3 Reflection Adj", the second column of Tables 4 and 5, measures peer ability using peer scores in grade 3, instead of in the year before, $t-1$. As I discuss further in Appendix Section B, the reflection problem arises because peers affect each other's scores simultaneously. Although using peer scores measured $t-1$ eliminates reflection bias from the present year, year t, it is possible some reflection bias remains.[12] "G3 Reflection Adj" shows that most of the main conclusions of my preferred specification remain intact, with potentially lower estimates for own cohort. The conclusion that is not robust to this specification is above-cohort school peers' stronger effect size than below-cohort peers. However, this is likely a mechanical result of the robustness check itself: peers in the cohort above have third-grade scores further back in time as a direct result of being older, which mechanically reduces their 3rd-grade abilities' correlation with both their contemporaneous abilities and student i's abilities.

[12]This is because student i's own scores from $t-1$ have reflection bias from the peer scores in $t-1$, so the previous year's reflection bias may still artificially inflate peer effects estimates to a smaller degree. Using peer scores from grade 3, the earliest available year, is the most robust option available in this dataset to this potential source of bias because the further back in time peer scores are sourced, the weaker the correlation of student i's ability with their own ability from the year of the peer scores, the less reflection bias will be present. However, this benefit is also its drawback: weakening correlation with reflection bias also means weakening correlation with peer ability in the current year, which is the year we are ultimately interested in proxying for.

Tables 4's and 5's third column, "Own Score Control", includes a control for student i's ability as measured in third grade. This control is included to eliminate any possible remaining individual heterogeneity from before we first observe each student in grade 3. Because students in grade 3 are not included in the analysis sample, all of the fixed effects, including school and block group, do not account for unobserved heterogeneity from grade 3 and before. That is, students could have unobserved endogenous peer group sorting that occurs before the analysis sample begins and that is not fully ameliorated by my preferred specification. Then, controlling for 3rd-grade ability takes a more value-added approach, only showing the effects of changes in peer abilities after students enter my sample. The results suggest that this control may somewhat lower own-cohort school ability effects to 0.2 standard deviations and lower all block-cohort peer effects to 0.02 standard deviations from 0.04, but otherwise keeps the main takeaways intact: a large role for cohort among school peers, small or no role among block peers, substantially larger school peer effects than block peer effects, and some role for relative age effects.

Finally, the fourth column of Tables 4 and 5, shows the robustness of the results to dropping the school year where Michigan changed its standardized testing scheme: 2014-2015. In the 2014-2015 school year, Michigan changed both its statewide test from the MEAP to the M-STEP and the timing of the standardized exams from the fall to the spring. Because peer abilities are measured in $t-1$ to reduce reflection bias, estimates using the 2014-2015 school year would use peer abilities from a different exam taken at a different time of year as the exam used to produce own scores, which may bias my estimates if test score standardization and school-year fixed effects do not fully account for this change. Results are nearly identical to the main specification, showing that changes in test schemes are not unduly influencing my results.

7 Heterogeneity Analysis

To explore the possibility of other heterogeneous effects, I break down my sample by several different categories and rerun the cutoff IV analysis: grade, gender, race/ethnicity, economically disadvantaged status, and 3rd-grade test score. Results are displayed in Tables 7 to 13 in Appendix Section A.3. For the most part, the results are consistent across groups, including gender, race, and disadvantaged status. There are some potentially suggestive differences, such as higher point estimates for white students than black and non-economically-disadvantaged students than disadvantaged ones, but these differences are largely not statistically significant at the 0.05 level.

In contrast, Tables 7, 8, and 13, which break down effects by grade and own 3rd grade test scores, have some stronger evidence of heterogeneity. For Tables 7 and 8, there is some evidence of heterogeneity by grade, especially as own-cohort school effects diminish between grades 5 and 7. However, because results increase again for grade 8, and both grade 5 and grade 8 lack cohort-above school peer controls,[13] the results should be interpreted with caution, as this may be driven by the structure of the estimation method instead of reflecting true underlying heterogeneity. For Table 13's math scores, displaying effects by ability in 3rd grade, above median students have stronger peer effects in both the school and the block from their own cohort peers. This provides suggestive evidence of nonlinear peer effects, as a strict interpretation of the linear-in-means model would predict there should be no differences in effect size by own ability.

In total, these heterogeneity effects demonstrate that the four takeaways, variation in cohort peer effects for school and not block, own-cohort school peer effects of 0.3, block-cohort peer effects of 0.04, and cohort-above school peer effects 0.15 lower than own-cohort effects, are not

[13] Grade 8 almost always lacks a cohort above in the same school, which is driving its low Kleibergen-Paap F Stat. Grade 5 has some cases with K-6 schools, but still has a large enough proportion of students with no cohort-above peers in the same school that the controlling effects of cohort-above ability on own-cohort effects may be diminished

driven by one group and are consistent across the sample. Like the robustness checks, we see some fluctuation in the point estimates, but most of these changes are either statistically insignificant or insubstantial. Suggestive evidence for nonlinear peer effects and fluctuation by grade provide potential avenues for heterogeneous effects without refuting the four takeaways, though both should be interpreted with caution.

8 Conclusion

In this paper, I bring the neighborhood context into the school and explore the role of peers' relative cohorts in influencing educational peer effects. Controlling for peers in adjacent cohorts and instrumenting for peer cohort with cohort assignment on the universe of Michigan public school students over thirteen years, I show that the cohorts of one's peers play a strong role in influencing peers' effects on each other in the school context. While own-cohort school peer effects of a 0.3 standard deviation increase for a one standard deviation increase in average peer ability are in the ballpark of school peer effect sizes found in previous literature (Sacerdote, 2014), I add the novel finding that the effect drops off by 0.15 standard deviations for school peers in the cohort above and by 0.2 standard deviations for school peers in the cohort below.

However, not only are block peer effects substantially lower than school peer effects, this relative cohort effect does not hold for block peers, indicating that peer group formation in the school environment is fundamentally different from the neighborhood environment. Neighborhood peers have a much smaller effect than school peers on educational outcomes. Finally, I provide evidence that school peers in the cohort above have an effect about 0.05 standard deviations higher than school peers in the cohort below, with a small increase for above cohort block peers as well, sug-

gesting that age also plays a role in influencing peer influence in the peer group formation process. In total, these four takeaways broaden the literature by combining the school and block peer contexts, uncovering the role of relative cohorts, and suggesting the importance of age effects by other peers.

Collectively, these results add significant detail to our understanding of the role of peers in the education production function. My paper shows that focusing on only own cohort and ignoring adjacent cohorts leaves out substantial parts of the peer experience, and that research designs that rely on year-by-year cohort fluctuation in peer characteristics for identification may need to consider the possibility of spillovers. Additionally, bringing neighborhood peers into the school context shows that neighborhoods play a smaller but still important role in the educational process and that peer group formation in this context may operate in unexpected ways compared to schools. In total, policymakers and researchers should take away the lesson that education comes from a broad variety of peers and environments that we may not traditionally focus on. So, if we wish to maximize human capital, economic mobility, and long-term well-being, we should take more opportunities to step back and evaluate students' education with a broader and more all-encompassing lens.

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10 Figures

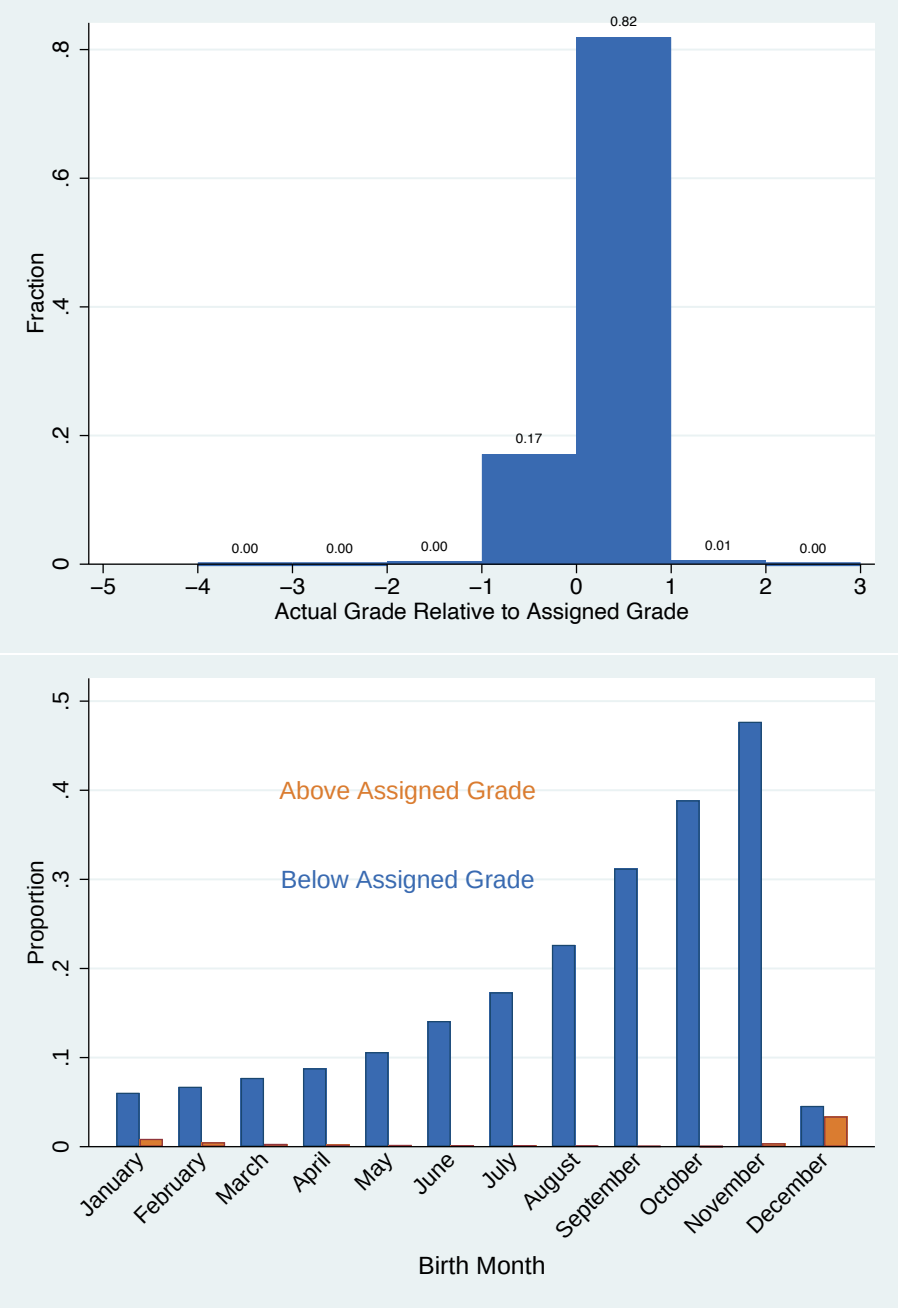

Figure 1: Cohort Compliance, Overall and by Birth Month

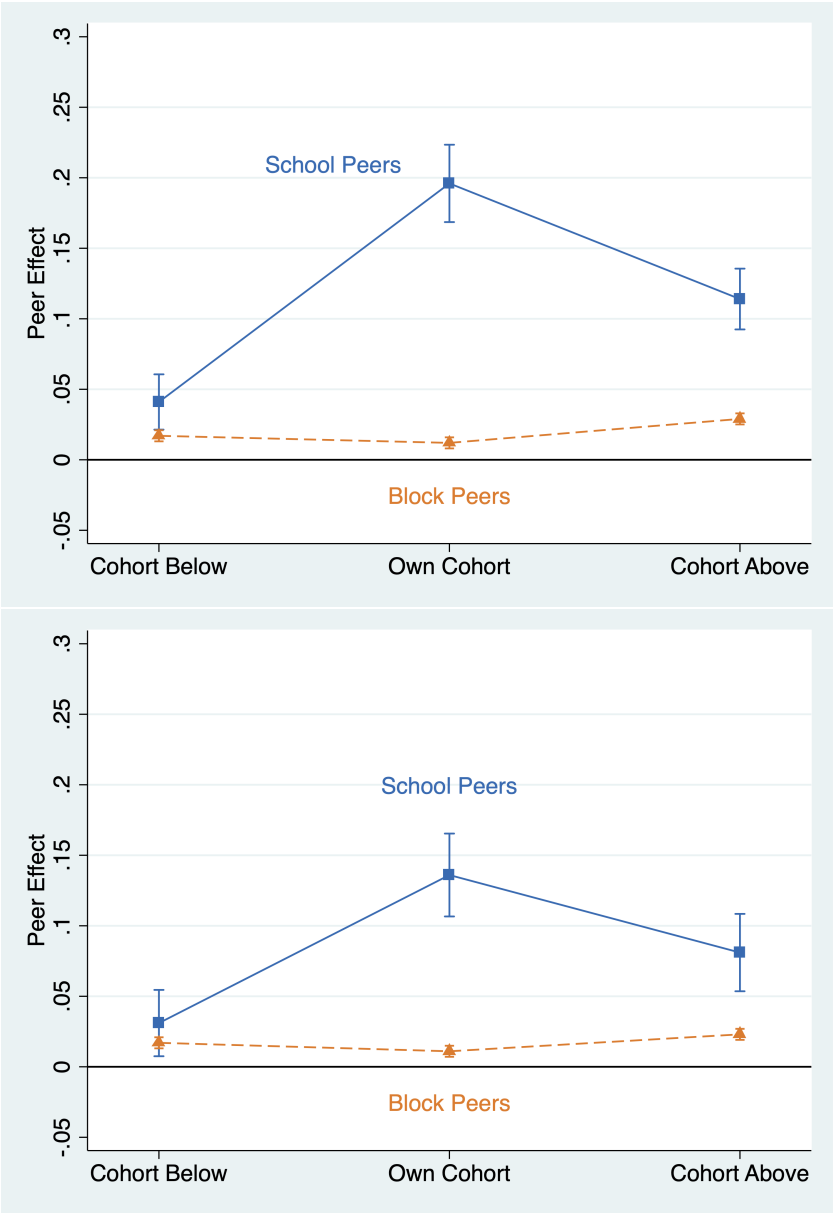

Figure 2: Assigned Cohort IV Peer Effects, Math and Reading Scores

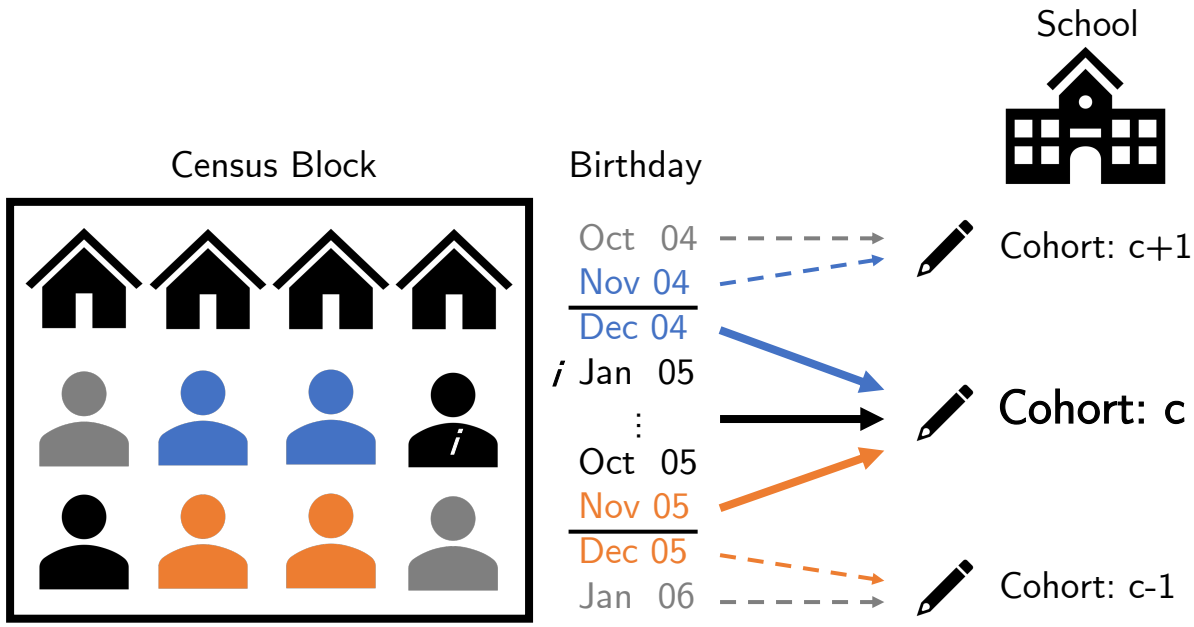

Figure 3: Sorting Neighborhood Peers into School Cohorts by Birth Month and Year

$$\bar{Y}_{-i,c} = \beta_1 \bar{Y}_{Dec,c} + \beta_2 \bar{Y}_{Nov,c+1} + \beta_3 \bar{Y}_{Nov,c} + \beta_4 \bar{Y}_{Dec,c-1} + \ldots$$

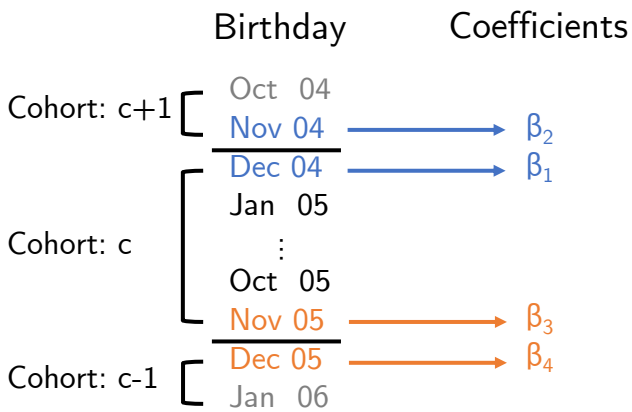

Figure 4: Connecting Identification Strategy to First Stage Equations

11 Tables

Table 1: Summary Statistics, Full versus Analysis Samples

Table 1: Summary Statistics

Variable	Analysis Sample	Full Sample (Grades 5-8)	Differences
Math scores	0.038	0.000	0.038**
	(1.026)	(1.000)	[0.000]
Reading Scores	0.026	-0.000	0.026**
	(1.006)	(1.000)	[0.000]
# Students in School-Cohort	198.0	162.4	35.6**
	(124.7)	(118.0)	[0.1]
# Students in Block-Cohort	2.589	2.142	0.447**
	(3.724)	(3.282)	[0.002]
Female	49.8	48.7	1.1**
White	64.7	69.5	-4.7**
Black	21.9	18.0	3.9
Hispanic	6.6	6.2	0.3**
Asian/PI	4.0	3.0	0.9**
Other race	2.9	2.9	0.0
Eligible for Special Ed services	10.3	13.3	-3.0
Free or Reduced-Price Lunch	48.9	51.9	-3.1**
Limited English Proficiency	6.6	6.1	0.4**
Locale of student's school: city	26.8	22.1	4.7**
Locale of student's school: suburb	55.5	43.2	12.3**
Locale of student's school: town	8.3	12.1	-3.8**
Locale of student's school: rural	9.4	22.6	-13.2**
Observations	2,722,950	7,864,719	

** p<0.01, * p<0.05, + p<0.1. Robust standard errors in brackets, standard deviations in parentheses.

Table 2: School and Block Peer Effects by Cohort, Math Scores

Specification Relative Cohort	OLS	Assigned Cohort Reduced Form	Assigned Cohort IV	Cutoff Student IV
School Peers				
Grade Below	0.047**	0.068**	0.066**	0.046**
	[0.008]	[0.007]	[0.010]	[0.015]
Own Grade	0.303**	0.269**	0.327**	0.344**
	[0.011]	[0.010]	[0.013]	[0.019]
Grade Above	0.110**	0.160**	0.163**	0.128**
	[0.009]	[0.008]	[0.010]	[0.015]
Block Peers				
Grade Below	0.036**	0.035**	0.043**	0.020**
	[0.001]	[0.001]	[0.002]	[0.007]
Own Grade	0.037**	0.035**	0.031**	0.049**
	[0.001]	[0.002]	[0.003]	[0.010]
Grade Above	0.049**	0.044**	0.056**	0.028+
	[0.001]	[0.001]	[0.002]	[0.016]
Kleibergen-Paap F Stat			1621	126
Observations	2,999,834	2,676,385	2,666,141	2,612,911

** p<0.01, * p<0.05, + p<0.1.
Robust standard errors in brackets, clustered at the school level.

Table 3: School and Block Peer Effects by Cohort, Reading Scores

Specification Relative Cohort	OLS	Assigned Cohort Reduced Form	Assigned Cohort IV	Cutoff Student IV
Grade Below	0.041**	0.061**	0.058**	0.029
	[0.006]	[0.008]	[0.012]	[0.019]
Own Grade	0.244**	0.196**	0.256**	0.244**
	[0.013]	[0.011]	[0.015]	[0.025]
Grade Above	0.065**	0.126**	0.130**	0.115**
	[0.008]	[0.009]	[0.011]	[0.019]
Block Peers				
Grade Below	0.030**	0.030**	0.037**	0.016*
	[0.001]	[0.001]	[0.002]	[0.007]
Own Grade	0.031**	0.029**	0.027**	0.047**
	[0.001]	[0.001]	[0.002]	[0.009]
Grade Above	0.038**	0.034**	0.044**	0.005
	[0.001]	[0.001]	[0.002]	[0.016]
Kleibergen-Paap F Stat			1653	155
Observations	2,983,697	2,662,688	2,656,132	2,593,933

** p<0.01, * p<0.05, + p<0.1.
Robust standard errors in brackets, clustered at the school level.

Table 4: Robustness Checks, Math Scores

Robustness Check Relative Cohort	All Controls	G3 Reflection Adj	G3 Own Score Control	Test Change
School Peers				
Cohort Below	0.026	0.101**	0.037**	0.071**
	[0.017]	[0.012]	[0.010]	[0.009]
Own Cohort	0.230**	0.184**	0.195**	0.350**
	[0.018]	[0.018]	[0.014]	[0.013]
Cohort Above	0.127**	0.062**	0.104**	0.172**
	[0.014]	[0.011]	[0.011]	[0.010]
Block Peers				
Cohort Below	0.029**	0.046**	0.018**	0.043**
	[0.003]	[0.003]	[0.002]	[0.002]
Own Cohort	0.019**	0.026**	0.012**	0.031**
	[0.003]	[0.003]	[0.002]	[0.003]
Cohort Above	0.037**	0.032**	0.030**	0.057**
	[0.003]	[0.002]	[0.002]	[0.002]
Kleibergen-Paap F Stat	1451	99	1592	1620
Observations	1,385,584	1,737,958	1,879,759	2,444,208

** p<0.01, * p<0.05, + p<0.1.
Robust standard errors in brackets, clustered at the school level.

Table 5: Robustness Checks, Reading Scores

Robustness Check Relative Cohort	All Controls	G3 Reflection Adj	Own Score Control	Test Change
School Peers				
Cohort Below	-0.005	0.063**	0.031**	0.079**
	[0.020]	[0.012]	[0.012]	[0.010]
Own Cohort	0.175**	0.116**	0.133**	0.272**
	[0.021]	[0.018]	[0.015]	[0.014]
Cohort Above	0.081**	0.071**	0.078**	0.144**
	[0.019]	[0.012]	[0.012]	[0.012]
Block Peers				
Cohort Below	0.021**	0.040**	0.017**	0.038**
	[0.003]	[0.002]	[0.002]	[0.002]
Own Cohort	0.017**	0.019**	0.011**	0.026**
	[0.003]	[0.003]	[0.002]	[0.002]
Cohort Above	0.028**	0.030**	0.023**	0.045**
	[0.003]	[0.002]	[0.002]	[0.002]
Kleibergen-Paap F Stat	239	70	747	1695
Observations	1,381,070	1,726,584	1,874,032	2,429,319

** p<0.01, * p<0.05, + p<0.1.
Robust standard errors in brackets, clustered at the school level.

A Appendix Tables and Figures

A.1 Appendix Figures

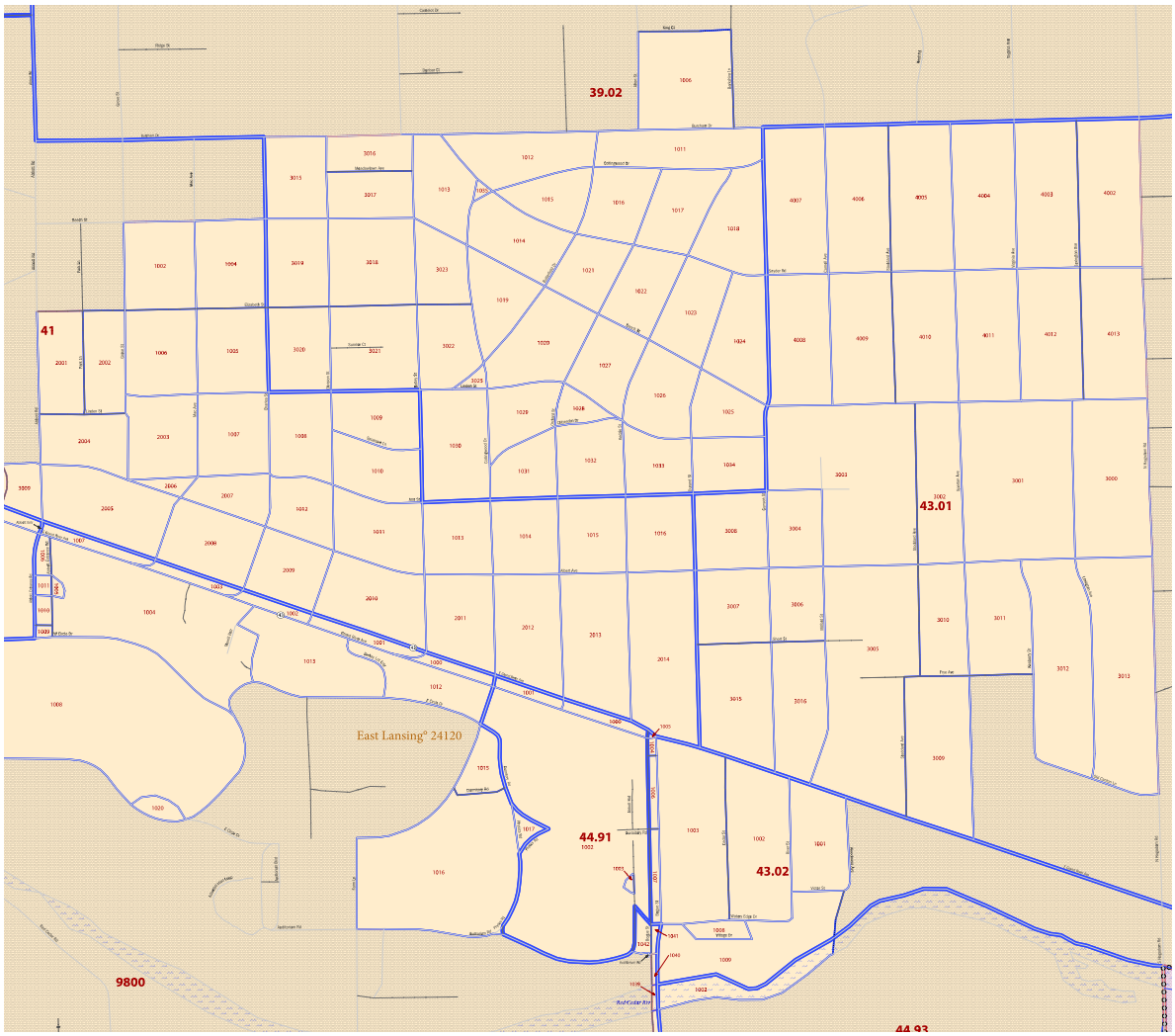

Figure 5: Census Map of Downtown East Lansing, MI

A.2 Appendix Tables

Table 6: Block Group Summary Statistics, Full versus Analysis Samples

Variable	Analysis Sample	Full Sample (Grades 5-8)	Differences
Block group total population	1,626	1,569	56**
	(1,011)	(911)	[0]
Female	51.5	51.0	0.5**
White	71.2	75.8	-4.6**
Black	17.3	13.7	3.5**
Hispanic	5.2	5.0	0.2**
Asian/PI	3.4	2.7	0.8**
Other race	2.9	2.8	0.1**
Median household income	$66,494	$64,120	$2,375**
	(35,336)	(31,767)	[16]
Per-capita income	$30,880	$29,883	$997**
	(14,811)	(13,432)	[7]
# of Census Block Groups	6,645	8,129	

** p<0.01, * p<0.05, + p<0.1. Robust standard errors in brackets, standard deviations in parentheses.

A.3 Heterogeneity Tables

Table 7: Assigned Cohort IV, By Grade, Math Scores

Grade/Relative Cohort	5	6	7	8
School Peers				
Cohort Below	0.094**	0.049	0.083**	0.104**
	[0.021]	[0.030]	[0.021]	[0.022]
Own Cohort	0.390**	0.344**	0.199**	0.264**
	[0.019]	[0.024]	[0.021]	[0.026]
Cohort Above	0.108**	0.265**	0.231**	0.049*
	[0.024]	[0.025]	[0.020]	[0.022]
Block Peers				
Cohort Below	0.035**	0.047**	0.048**	0.044**
	[0.003]	[0.003]	[0.003]	[0.003]
Own Cohort	0.028**	0.029**	0.029**	0.030**
	[0.004]	[0.004]	[0.005]	[0.004]
Cohort Above	0.057**	0.056**	0.047**	0.030**
	[0.003]	[0.003]	[0.004]	[0.003]
Kleibergen-Paap F Stat	1484	810	508	11
Observations	689,118	720,788	714,241	498,924

** p<0.01, * p<0.05, + p<0.1.
Robust standard errors in brackets, clustered at the school level.

Table 8: Assigned Cohort IV, By Grade, Reading Scores

Grade/Relative Cohort	5	6	7	8
School Peers				
Cohort Below	0.069**	0.064+	0.024	0.093**
	[0.020]	[0.037]	[0.027]	[0.027]
Own Cohort	0.281**	0.210**	0.197**	0.240**
	[0.020]	[0.031]	[0.026]	[0.023]
Cohort Above	0.086*	0.151**	0.169**	0.028
	[0.037]	[0.029]	[0.022]	[0.034]
Block Peers				
Cohort Below	0.030**	0.044**	0.039**	0.038**
	[0.003]	[0.003]	[0.004]	[0.003]
Own Cohort	0.025**	0.025**	0.023**	0.023**
	[0.004]	[0.004]	[0.004]	[0.004]
Cohort Above	0.046**	0.042**	0.042**	0.024**
	[0.003]	[0.003]	[0.003]	[0.003]
Kleibergen-Paap F Stat	1427	837	131	7
Observations	684,910	719,792	711,246	496,478

** p<0.01, * p<0.05, + p<0.1.
Robust standard errors in brackets, clustered at the school level.

Table 9: Assigned Cohort IV, By Gender

Subject	Math		Reading	
Gender/Relative Cohort	Female	Male	Female	Male
School Peers				
Cohort Below	0.066**	0.066**	0.069**	0.051**
	[0.011]	[0.011]	[0.013]	[0.012]
Own Cohort	0.324**	0.330**	0.233**	0.268**
	[0.014]	[0.013]	[0.016]	[0.016]
Cohort Above	0.157**	0.164**	0.151**	0.137**
	[0.011]	[0.011]	[0.013]	[0.012]
Block Peers				
Cohort Below	0.041**	0.045**	0.037**	0.037**
	[0.003]	[0.003]	[0.002]	[0.003]
Own Cohort	0.032**	0.029**	0.031**	0.022**
	[0.003]	[0.004]	[0.003]	[0.003]
Cohort Above	0.055**	0.057**	0.043**	0.045**
	[0.003]	[0.003]	[0.002]	[0.003]
Kleibergen-Paap F Stat	1847	1284	624	1271
Observations	1,327,525	1,338,463	1,320,717	1,328,745

** p<0.01, * p<0.05, + p<0.1.
Robust standard errors in brackets, clustered at the school level.

Table 10: Assigned Cohort IV, By Race/Ethnicity, Math Scores

Race/Relative Cohort	White	Black	Hispanic	Asian/PI
School Peers				
Cohort Below	0.061**	0.068**	0.030	0.086*
	[0.010]	[0.017]	[0.020]	[0.033]
Own Cohort	0.314**	0.287**	0.340**	0.350**
	[0.013]	[0.029]	[0.023]	[0.044]
Cohort Above	0.177**	0.127**	0.110**	0.121**
	[0.011]	[0.017]	[0.022]	[0.028]
Block Peers				
Cohort Below	0.045**	0.019**	0.032**	0.042**
	[0.002]	[0.003]	[0.007]	[0.009]
Own Cohort	0.033**	0.007+	0.009	0.035**
	[0.003]	[0.004]	[0.008]	[0.010]
Cohort Above	0.058**	0.030**	0.039**	0.042**
	[0.003]	[0.004]	[0.008]	[0.009]
Kleibergen-Paap F Stat	1087	1326	754	639
Observations	1,731,127	576,896	173,698	105,810

** p<0.01, * p<0.05, + p<0.1.
Robust standard errors in brackets, clustered at the school level.

Table 11: Assigned Cohort IV, By Race/Ethnicity, Reading Scores

Race/Relative Cohort	White	Black	Hispanic	Asian/PI
School Peers				
Cohort Below	0.031*	0.089**	0.044+	0.042
	[0.013]	[0.019]	[0.023]	[0.037]
Own Cohort	0.227**	0.246**	0.255**	0.199**
	[0.016]	[0.030]	[0.035]	[0.047]
Cohort Above	0.139**	0.126**	0.106**	0.094**
	[0.011]	[0.022]	[0.025]	[0.032]
Block Peers				
Cohort Below	0.040**	0.019**	0.023**	0.016+
	[0.002]	[0.003]	[0.007]	[0.009]
Own Cohort	0.028**	0.012**	0.026**	0.032**
	[0.003]	[0.004]	[0.009]	[0.009]
Cohort Above	0.048**	0.024**	0.022**	0.029**
	[0.003]	[0.003]	[0.008]	[0.009]
Kleibergen-Paap F Stat	643	381	722	158
Observations	1,720,828	575,812	171,523	103,109

** p<0.01, * p<0.05, + p<0.1.
Robust standard errors in brackets, clustered at the school level.

Table 12: Assigned Cohort IV, By Economic Disadvantage Status

Subject	Math		Reading	
Economic Disadvantage/ Relative Cohort	Yes	No	Yes	No
School Peers				
Cohort Below	0.030*	0.055**	0.015	0.009
	[0.014]	[0.015]	[0.018]	[0.018]
Own Cohort	0.235**	0.276**	0.194**	0.184**
	[0.020]	[0.018]	[0.023]	[0.021]
Cohort Above	0.107**	0.124**	0.096**	0.091**
	[0.014]	[0.013]	[0.016]	[0.015]
Block Peers				
Cohort Below	0.033**	0.028**	0.027**	0.018**
	[0.003]	[0.003]	[0.003]	[0.003]
Own Cohort	0.011**	0.028**	0.017**	0.022**
	[0.004]	[0.004]	[0.004]	[0.004]
Cohort Above	0.039**	0.043**	0.037**	0.030**
	[0.003]	[0.004]	[0.003]	[0.003]
Kleibergen-Paap F Stat	1093	1132	579	174
Observations	783,234	831,175	777,673	827,633

** p<0.01, * p<0.05, + p<0.1.
Robust standard errors in brackets, clustered at the school level.

Table 13: Assigned Cohort IV, By 3rd Grade Score

Subject	Math		Reading	
Initial Score/Relative Cohort	Above Median	Below Median	Above Median	Below Median
School Peers				
Cohort Below	0.049**	0.057**	0.033**	0.061**
	[0.010]	[0.010]	[0.012]	[0.012]
Own Cohort	0.266**	0.224**	0.160**	0.186**
	[0.013]	[0.013]	[0.013]	[0.016]
Cohort Above	0.147**	0.140**	0.105**	0.116**
	[0.012]	[0.010]	[0.011]	[0.011]
Block Peers				
Cohort Below	0.027**	0.024**	0.022**	0.023**
	[0.002]	[0.002]	[0.002]	[0.002]
Own Cohort	0.026**	0.006*	0.019**	0.014**
	[0.003]	[0.002]	[0.002]	[0.002]
Cohort Above	0.043**	0.032**	0.025**	0.033**
	[0.003]	[0.002]	[0.002]	[0.002]
Kleibergen-Paap F Stat	1433	1530	277	1285
Observations	1,333,424	1,332,550	1,267,636	1,381,796

** p<0.01, * p<0.05, + p<0.1.
Robust standard errors in brackets, clustered at the school level.

B Reflection Problem

Let $Y_{i,t}$ be the observed outcome of interest Y (e.g. test scores) of observation i at time t, $\overline{Y}_{-i,c,b,t}$ be the average of the same for the all other individuals in cohort c and block b at time t.[14] A simple model seeking to determine the peer effects of block-cohort peers would be:

$$Y_{i,t} = \beta_0 + \beta_1 \overline{Y}_{-i,c,b,t} + \nu_{i,t} \tag{6}$$

Which formalizes the effects of block-cohort as a linear combination of observed peer outcomes $\overline{Y}_{-i,c,b,t}$ and other unobserved factors. The first issue with equation 6 is the reflection problem, as described by Manski (1993). That is, if we believe equation 6, then it also follows that:

$$Y_{j,t} = \beta_0 + \beta_1 \overline{Y}_{-j,c,b,t} + \nu_{i,t} \quad \forall j \in c,b,t$$

If we plug this equation back into equation 6 for each $\overline{Y}_{-i,c,b,t}$, we can see that, because $Y_{i,t} \in \overline{Y}_{-j,c,b,t}$, estimating equation 6 directly would bias β_1 upward because $Y_{i,t}$ is mechanically correlated with itself. Intuitively, if student i's block-cohort peers affect i's outcomes and student i's outcome affects their block-cohort peers simultaneously, then any estimation of equation 6 would overestimate the peer effects of block-cohort peers on i. To correct this, I use peer scores lagged by one year[15], denoted as $\overline{Y}_{-j,c,b,t-1}$.[16] Because $Y_{i,t}$ has changed in the year following $t-1$, the dependence of $\overline{Y}_{-j,c,b,t-1}$ on $Y_{i,t}$ should be more limited as it only exists to the extent that $Y_{i,t}$ is

[14] This generalizes for any function f of $Y_{-i,c,b,t}$ where $\frac{\partial f(f(Y_i, Y_{-i,-j,c,b,t}))}{\partial Y_i} \neq 0$.

[15] Results are also robust to alternative methods, such as using peer scores lagged by two years or from third grade.

[16] The best solution is to use pre-measures from before the block-cohort peers interacted with each other, as is done in Imberman et al. (2012). However, for test scores, students are not tested until grade 3, and some block-cohort students have been interacting since kindergarten or before.

still dependent on $Y_{i,t-1}$, observation i's own one year lagged outcome. While this likely does not eliminate the reflection problem entirely, it does reduce it to an extent that should limit the issue of overestimation. Now, rewriting equation 6, we have:

C Cutoff IV

C.1 Michigan School Cohort Entry Policy

Historically, Michigan had one of the latest birthdate cutoffs in the nation. As of the 2012-2013 school year, a child had to be age 5 by 12/1[17] in order to be old enough to enter kindergarten. In contrast, by 2018, no other state had an entry cutoff date later than 10/15, and most had cutoff dates in September or earlier.[18] However, in June 2013, Michigan revised its school code so that the cohort entry cutoff date would move up by one month each school year[19] until the 2015-2016 school year, when the cohort entry cutoff date would be 9/1. Parents may still enroll their child in kindergarten if they do not meet this cutoff and their child is born before 12/1 and they submit a waiver to the school district, but the school district may make recommendations against enrolling this child "due to age or other factors".[20] What this means for the use of cohort entry cutoffs as an instrument is twofold. First, parents' ability to waive their child even past the cutoff means there will likely not be strict compliance, requiring the use of assigned cohort based strictly on age and month of birth as an instrument for actual cohort to capture estimates of treatment-on-treated. For almost all of the analysis sample, the relevant legal cutoff date was 12/1 when the students were

[17] There is anecdotal evidence that some districts implemented earlier recommended entry dates closer to the national norm than this later date.

[18] Source: https://nces.ed.gov/programs/statereform/tab53.asp

[19] Cutoff in 2013-2014: 11/01. Cutoff in 2014-2015: 10/01

[20] Source: https://www.michigan.gov/documents/kindergarten1225547.pdf

entering kindergarten. Because of this, I will be referring to 12/1 as the cutoff date and November and December as the cutoff months for the remainder of the paper.

Table 14: School Cutoff Peers Comparison

	Own-Above Cohort			Own-Below Cohort		
Variable	Dec-Own	Nov-Above	Own-Above-Diff	Nov-Own	Dec-Below	Own-Below-Diff
# of students	15.20	9.64	5.56**	12.69	10.06	2.63**
	(10.55)	(10.87)	[0.01]	(9.22)	(10.27)	[0.01]
Lagged math scores	0.101	-0.024	0.125**	0.015	0.105	-0.090**
	(0.603)	(0.616)	[0.000]	(0.616)	(0.617)	[0.000]
Lagged reading scores	0.096	-0.039	0.135**	-0.005	0.097	-0.103**
	(0.551)	(0.569)	[0.000]	(0.569)	(0.569)	[0.000]
Female	49.0	48.1	0.9**	51.0	49.3	1.7**
White	64.6	63.0	1.6**	63.4	62.2	1.2**
Black	21.5	23.3	-1.7**	21.6	23.6	-2.0**
Hispanic	6.7	6.8	-0.1**	7.1	6.9	0.2**
Asian/PI	4.0	3.9	0.1**	4.6	4.1	0.5**
Other Race	3.0	2.8	0.1**	3.1	3.1	0.0**
Eligible for Special Ed services	12.2	13.7	-1.4**	12.0	11.8	0.2**
Free or Reduced-Price Lunch	49.1	50.2	-1.1**	49.6	50.7	-1.1**
Limited English Proficiency	5.8	6.7	-1.0**	7.1	6.6	0.5**
Observations	1,864,041	1,185,126		1,902,919	1,419,362	

** p<0.01, * p<0.05, + p<0.1. Standard errors in brackets, standard deviations in parentheses.

In Tables 14 and 15, I examine whether students born on either side of the school entry cutoff are valid comparison groups by comparing their observable characteristics. All statistics presented, except number of peers, are conditional on having at least one student in the group. These peer groups are defined based strictly based on *assigned* cohort, which is determined by their month of birth, year of birth, and kindergarten entry cutoff date when they were age 5. If birth timing is random, following my identification assumption, then these two groups should be very similar on both observable and unobservable characteristics. As we can see in Tables 14 and 15, examining

Table 15: Block Cutoff Peers Comparison

	Own-Above Cohort			Own-Below Cohort		
Variable	Dec-Own	Nov-Above	Own-Above-Diff	Nov-Own	Dec-Below	Own-Below-Diff
# of students	0.19	0.18	0.01**	0.18	0.19	-0.01**
	(0.51)	(0.50)	[0.00]	(0.50)	(0.51)	[0.00]
Lagged math scores	0.181	0.089	0.092**	0.082	0.196	-0.114**
	(1.022)	(1.025)	[0.003]	(1.021)	(1.013)	[0.003]
Lagged reading scores	0.154	0.051	0.104**	0.043	0.164	-0.121**
	(0.981)	(0.987)	[0.003]	(0.986)	(0.982)	[0.003]
Female	49.8	49.6	0.2	49.6	49.7	-0.1
White	64.6	65.1	-0.5**	64.6	64.1	0.5**
Black	19.6	19.1	0.5**	19.0	19.6	-0.6**
Hispanic	7.4	7.1	0.3**	7.5	7.6	-0.1
Asian/PI	5.3	5.5	-0.2**	5.7	5.4	0.2**
Other race	2.9	2.9	-0.0	3.0	3.1	-0.1+
Eligible for Special Ed services	10.2	11.2	-1.0**	11.4	10.3	1.1**
Free or Reduced-Price Lunch	45.9	44.9	1.0**	45.7	46.4	-0.7**
Limited English Proficiency	6.4	7.5	-1.1**	8.1	7.2	0.8**
Observations	282,636	273,213		272,653	280,656	

** p<0.01, * p<0.05, + p<0.1. Standard errors in brackets, standard deviations in parentheses.

school and block cutoff peers, respectively, the two groups are largely similar in terms of gender, race, special education, free/reduced-price lunch, and limited english proficiency status. The two areas of observable characteristics where they differ are number of students, for schools, and test scores, for both. Numbers of students in schools differ because some schools do not have above or below cohorts for certain grade levels: for example, a grade K-5 school does not have above-cohort school peers for fifth graders, and a grade 6-8 school does not have below-cohort school peers for sixth graders.[21] Other than test scores, the observable data suggests that the assumption of random assignment holds.

While test scores are consistently higher for students born in December than November, prior literature suggests this should not be a cause for concern on other unobservable background characteristics.Test scores are 0.1 standard deviation higher for December students in both school-cohorts and block-cohorts. This likely stems from relative age effects, a well-known effect in education in which the oldest students within a cohort perform better than the youngest, especially at younger ages (Elder, 2010; Black et al., 2011). Given the literature showing that this results directly from cohort assignment, we can reasonably conclude that this does not pose a problem for selection on other confounding unobservable background characteristics. As long as 1) students' innate ability is constant across the cutoff threshold, 2) the test score difference largely captures remaining differences in student ability due to age differences at test time and different schooling experiences as the oldest/youngest student in cohort, and 3) the linear-in-means assumption holds (i.e., higher levels of test scores at baseline aren't a cause for concern, since changes in test scores are the primary driver of results), this particular difference should not confound my estimates.

[21] As discussed further in Section 4, I include dummy variables for all cases where students have zero school-cohort, block-cohort, or school or block cutoff peers. Excluding these cases would severely restrict the sample to only a limited number of school-cohorts.

Before running my cutoff IV specification, I need to make several changes to the sample. First, I drop all students born within one month of the school-entry cutoff for their year. As described in Section C.1, this mostly means dropping students born in November or December, but includes November and October for the 2018-2019 school year and October and September for the 2019-2020 school year. Second, I drop all students who do not have at least one cutoff student in any of the four peer cohort cutoff groups in either school or block: November of cohort above, December of own cohort, November of own cohort, and December of cohort below. Third, I drop all students who are missing test score data on any of the four peer cohort cutoff groups in either school or block. These three changes leave a final cutoff analysis sample of 1,914,686 observations for math and 1,906,253 for reading.

C.2 Cutoff Student IV Estimation

The estimation strategy for the cutoff student IV estimation is similar to a fuzzy border discontinuity. Figure 4 helps illustrate the connection between the first stage equations above and the identification strategy and Figure 3. The first two terms use the average ability of cutoff students born just before or after the cutoff for being the oldest peers in student i's school-cohort. The second two terms do the same, but with the average abilities of cutoff students born just before or after the cutoff for being the *youngest* peers in student i's school-cohort. The next four terms repeat the process, but now for block-cohort cutoff peers, rather than school-cohort. Adding in controls for counts of students and proportions of observable characteristics in each cutoff group, and the first stage equation looks like:

The second stage equation is the same as the main estimation strategy, but with the inclusion of the controls for counts of students and proportions of observable characteristics in each cutoff

group instead of in each assigned cohort group:

$$Y_{i,t} = \beta_0 + \sum_{(j=-1)}^{1} \beta_j \widehat{\overline{Y}}_{-i,c+j,s,t-1} + \sum_{(j=-1)}^{1} \alpha_j \widehat{\overline{Y}}_{-i,c+j,b,t-1}$$
$$+ X_{i,t} + \sum_{(m \in Nov,Dec)} \sum_{(k \in s,b)} \sum_{(j=-1)}^{1} \overline{X}_{-i,m,c+j,k,t} + \Gamma_{m,c,g,s,b_g,t} + \varepsilon_{i,t}$$

where β_j through α_j are again our exogenous peer effects for school and block peers.