The Effects of Foreign-Born Peers in US High Schools and Middle Schools

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This study examines the short-term and long-term impact of being educated with immigrant peers. We leverage a quasi-experimental design using across-grade, within-school variation in cohort/grade composition for students in the National Longitudinal Study of Adolescent Health. We find positive effects for foreign-born students compared with native-born students from increasing exposure to other foreign-born students, including on risky health behaviors, social isolation, mental health, and academic effort. While we find negative effects on language and educational attainment, these differences do not translate into worse socioeconomic status in adulthood. Finally, we present evidence suggesting that these differential effects stem from highly segregated, school friendship networks.

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I. Introduction

Many domains of adolescent life are shaped by the behaviors and attributes of school-aged peers. Peers influence academic performance, health-related behaviors, socioemotional development, and even criminal activity (Gavira and Raphael 2001; Kao 2001; Duncan et al. 2005; Ream and Rumberger 2008; Burke and Sass 2013; Eren 2017; Wodtke and Parbst 2017; Billings, Deming, and Ross 2019). As a result, the composition of adolescents' schools—and the attendant formation of students' peer relationships—is an important pathway through which education experiences influence socioeconomic and socioemotional development. Studies of classroom and grade-level composition highlight a number of potentially relevant traits among students' peers, including familial social capital, racial composition, and language skills (Fletcher 2010; Bifulco, Fletcher, and Ross 2011; Lavy and Schlosser 2011; Gamoran, Collares, and Barfels 2016).

In recent years, the multidecade growth and shifting geographic distribution of the US immigrant population has brought questions of school composition into public debate. In 1980, eight US states had public school systems in which at least one-tenth of students came from immigrant households. By 2000, this was true of 20 US states, and by 2015, this had grown to 32 US states (Camarota, Griffith, and Zeigler 2017). By 2000, 20% of US counties had immigrant population shares that exceeded 5% (Census Bureau 2017). The composition of school districts has changed over the last 30 years—shifting toward a growing share of primary and secondary schools in which foreign-born and US-born students attend school together.

The effects of attending school with a growing share of immigrant students are a priori ambiguous. Though sometimes portrayed otherwise, foreign-born adolescents exhibit fewer risk behaviors, better health behaviors, and fewer behavioral problems than their US-born peers (Harris 1999; Bleakley and Chin 2004), with some variation across group and across place. To the extent that peer effects operate through the modeling of behaviors for others in one's peer group, attending school with a larger share of foreign-born students may have positive effects on the students' own behavior. At the same time, concerns have been raised about possible academic detriments of attending schools with a larger share of foreign-born students, largely because of English language learning needs. Evidence on academic assessments is mixed, though most studies have focused on short-run testing outcomes (Crosnoe and Lopez-Gonzalez 2005; Diette and Oyelere 2014; Conger 2015).

For immigrant students, the effects of attending schools with large shares of immigrants may differ from the effects on US-born students. Schools are sites of assimilation, socialization, and network formation. Adopting the health or risk behaviors of more numerous, US-born peers may worsen long-run outcomes for the foreign born (Harris 1999; Antecol and Bedard 2006). At the same time, social ties to US-born students and

their families could generate forms of upward socioeconomic mobility for immigrant students from less wealthy families (Ream 2005; Gandara et al. 2009). Increasing populations of foreign-born students may insulate those students from both the positive and the negative influences of US-born students. Further, schools can also be sites of harassment and discrimination (Silver 2015; Salerno and Reynolds 2017); larger shares of US-born students may create environments that worsen the mental health and academic performance of students from immigrant families. This may be exacerbated if the share of foreign-born students in a school is mirrored in the background of the teaching staff—or, more broadly, the institution's preparedness to work with immigrant families (Gandara 2010). The schooling environments of immigrant youth in "new destination" regions—that is, areas with a relatively recent history of foreign-born residents—have appropriately received attention (Potochnick 2014; Spees, Perreira, and Fuglini 2017).

A key challenge in estimating the effects of peer traits—on both US-born and foreign-born students—is that causal attribution is difficult. The composition of students' peers is a complicated function of multiple factors that sort students into schools, such as neighborhood home prices, proximity to employment sources, and parental ability to invest in schooling quality. Taking advantage of an approach developed by Hoxby (2000) and elaborated by others (Hanushek et al. 2003; Bifulco, Fletcher, and Ross 2011), several studies have used comparisons across grades within schools to characterize the effects of attending schooling with foreign-born peers on measures captured in administrative data: matriculation, grades, and test scores (Conger 2015; Hermansen and Birkelund 2015; Hardoy, Mastekaasa, and Schøne 2018). These measures provide an important assessment of intermediate education outcomes. Qualitative evidence suggests, however, that school peer effects may shape many other aspects of adolescent development that are relevant for long-run well-being.

In this study, we investigate a comprehensive set of schooling and developmental outcomes for a large cohort of foreign- and US-born adolescents in schools spread across the United States. We measure multiple domains while students are in school. In addition to identifying peer composition effects on standardized language vocabulary tests, we study effects on measures of social acceptance, social participation, mental health, risk behaviors, and academic effort. We then follow the students forward as they age, to assess how enduring the implications of these adolescent peer effects affect long-run welfare. Our study provides a comprehensive assessment of the peer implications of migration-driven changes to school composition. In so doing, the findings contribute to recent discussions about the presence of migrant families in communities across the United States.

The effects of school composition on foreign-born students relative to the effects on US-born students are meaningful in size, are statistically significant, and vary across domains. Relative to native students, immigrant

students express more social acceptance, better mental health, increased academic effort, and reduced participation in risky behaviors when their peers are composed of more foreign-born students. On the other hand, their scores on standardized assessments in the Picture-Vocabulary Test (PVT) are worse. While we find no evidence that the share of foreign-born students in adolescents' grades negatively affects the adolescent measures of well-being among their US-born classmates, our estimates do not have sufficient precision to rule out effects. Similarly, the estimates of the absolute effect of foreign-born peers on foreign-born students are generally statistically insignificant, so all results are interpreted relative to effects on US-born students. This caveat is especially important for mental health and academic effort effects, where the peer effects on US-born students are similar in magnitude and in the opposite direction to the foreign-born interactive estimates. Note that these effects operate net of the confounding variation that sorts students into schools.

When we follow a subset of the students into adulthood, we find minimal evidence that the protective effects of enclaves in adolescence relative to native-born students persist into adulthood. We find only modest reductions in health risk behavior from changes in peer composition, again relative to effects on US-born students. Rather, the relative negative effect of attending school with a larger share of foreign-born students on foreign-born student PVT scores remains, and an additional reduction in years of schooling arises as the students enter adulthood. Nonetheless, we do not observe any negative effects on labor market earnings or the quality of residential location, possibly because improved within-group networks compensate for any negative effects on human capital.

These effects tend to be smaller when the school immigrant population is more similar to the focal student in terms of racial and ethnic composition, consistent with peer effects requiring some threshold number of similar individuals with which to form friendships. Specifically, we estimate a model where the impact of the share of foreign-born students depends on the share of students in the school who share the focal student's race/ethnicity or (for Hispanic and Asian students) their country of origin. In both cases, when foreign-born students attend a school with more similar foreign-born students, the estimated peer effects of the share foreign born on foreign-born students are smaller. Similarly, the peer effects on foreign-born students are smaller when the school has more foreign-born students overall, more students not speaking English at home, and more second-generation immigrants. These findings are all consistent with a smaller effect of foreign-born peers on foreign-born students when the school contains a large share of students who are similar to the foreign-born students, perhaps because the students already have substantial opportunities for homophilous friendships (Weinberg 2013; Fletcher and Ross 2018; Fletcher, Ross, and Zhang 2020).

To further investigate the possible role of friendships and the proposed friendship opportunity phenomenon above, we examine the friendship

patterns of native and foreign-born students. Specifically, we find that foreign-born students are more likely to have foreign-born friends when the number of foreign-born students in their cohort increases. Given the better average health risk behaviors and mental health of foreign-born students and the lower language skills of those students, these results are consistent with an exposure mechanism where an increased number of foreign-born friends decreases foreign-born students' exposure to friends exhibiting risky behaviors and/or with mental health problems but also decreases exposure to friends with better language skills (higher PVT test scores). Further, these effects are largest when the share of foreign-born students is smaller. This finding is consistent with increases in the opportunity for homophily (more foreign-born peers) having the largest effect when the opportunities for homophilous friendships are most limited, as originally proposed by Weinberg (2013).

In sum, we find evidence of considerable protective effect for foreign-born adolescents when the number of foreign-born peers increases, with negative effects arising in language skills. On net, the long-run effects of foreign-born peers on the relative outcomes of foreign-born students are quite modest. The protective enclave effects observed in adolescence tend not to persist into adulthood, and while negative language effects do persist, we do not find any evidence that these effects translate into worse economic outcomes. These findings underscore the value of studying movement into adulthood as an important transition period shaping outcomes among the foreign born in the United States (Gonzales 2015), especially given the late-twentieth-century expansion of migrant families into school districts across the United States.

II. School Composition and Well-Being in Adolescence and Adulthood

Social science provides a long history of research on the effects of the school environment, both on student development generally (Wang and Holcombe 2010; Reyes et al. 2012) and on the development of immigrant students in particular (Stanton-Salazar 1995; Ream 2005). Models of immigrant incorporation in adolescence emphasize the importance of the local "context of reception," in which the school environment is a key site of socialization (Portes and Zhou 1993; Zhou 1997).

For some outcomes of adolescent and adult welfare, peer composition may be a particularly important aspect of the school environment; peers shape many features of students' experiences both within and beyond the classroom (Suárez-Orozco, Suárez-Orozco, and Todorova 2008). In the present study, we focus on a broad set of outcomes—standardized test scores, academic effort, risk behaviors, social acceptance, and mental health in adolescence. In adulthood, we measure outcomes that plausibly extend from these—earnings, education, health behavior, criminal activity, mental health, and even features of individuals' residential context.

Multiple mechanisms suggest a plausible link between the composition of students' peers and their socioeconomic and socioemotional development: behavioral modeling, effects on classroom content and pacing of learning, student experiences of inclusion or exclusion, and formation of network ties that may develop or hinder socioeconomic mobility.

Peer effects that operate through the adoption of academic, risk-taking, and health-related behaviors may advantage students attending school with a larger share of immigrants. Foreign-born adolescents on average engage in less risky behaviors—including alcohol and drug use—and better health-promoting behaviors—including diet and exercise—relative to their US-born peers (Harris 1999; Bleakley and Chin 2004). In addition, immigrant students report higher levels of academic effort than do US-born students (Peguero, Ovink, and Li 2016). Scholars have attributed this difference in part to the academic orientation of immigrant parents whose motivation for migration is often to support schooling opportunities for children (Suárez-Orozco, Suárez-Orozco, and Todorova 2008; Hagelskamp, Suárez-Orozco, and Hughes 2010). Goldsmith (2003) demonstrates that the positive correlation between Latino students' test scores and the share of Latino students in schools is partially attributable to the share of students with immigrant parents.

Stronger academic effort may have spillover effects that operate through pathways other than behavioral modeling, too; effort among immigrant students may also contribute to the acceleration of class pacing and may raise teachers' expectations. At the same time, some have raised concerns that English language learners require a larger share of teacher attention and school resources. To date, studies have found limited evidence that attending school with foreign-born students who are English language learners has negative effects on academic performance (Crosnoe and Lopez-Gonzalez 2005; Diette and Oyelere 2014; Conger 2015). For immigrant students, the classroom spillover effects of attending school with a larger share of other foreign-born students may be positive for learning—particularly if teaching resources are tailored to the language, culture, and acclimation needs of students.

A third mechanism that links peer composition to student welfare operates by shaping how students are treated by classmates—and accordingly, how comfortable the student is inside and outside the classroom setting (Ispa-Landa 2013; Ackert 2018). A priori, the effects of the relative share of foreign-born students on the receptive climate are ambiguous. Immigrant students with primarily US-born peers may feel isolated linguistically and socially. They may have fewer peers who understand the challenges of navigating cultural gaps between their home and school environments (Portes and Rumbaut 2001). They may be more regularly exposed to overt or subtle forms of racism and xenophobia by US-born peers (Salerno and Reynolds 2017); a larger share of foreign-born students may provide a protective enclave that minimizes exposure to these forms of hostility. In contrast, some scholarship indicates that anti-immigrant

sentiment grows when group size is perceived as threatening (Hainmuller and Hopkins 2014). If so, school climate for immigrants may worsen with a larger share of foreign-born students.

Finally, peer effects also arise if the formation of social networks provides access to forms of education- or employment-specific capital. If US-born students on average have parents with more experience navigating the US higher education system and ties to better local employment opportunities, attending school with a larger share of US-born students may improve schooling and work outcomes for immigrant adolescents (Stanton-Salazar 2000). On the other hand, the establishment of networks with native-born students who have higher rates of negative behaviors may lead to worse outcomes for immigrant students. For example, Piil Damm and Dustmann (2014) find that immigrants who were assigned to neighborhoods with higher rates of youth crime were more likely to be arrested for criminal activity.

The links between peer composition and student outcomes are undoubtedly conditioned by the extent of intra- and intergroup contact. In many high schools, the foreign-born population is a heterogeneous group, and attending school in the same grade does not necessarily imply sufficient contact to generate behavior changes or network ties. Most research on intergroup interaction in US schools suggests that ethnic and racial identification shapes friendship formation (Moody 2001; Currarini, Jackson, and Pin 2010). For immigrant students who are in classes with a larger share of students from the same origin country or, more broadly, who identify as sharing a common ethnic identity, these effects may be more salient. Alternatively, it is possible that the presence of additional foreign-born students matters less for students in schools with many opportunities for finding similar peers—for example, schools with a large share of the same-origin group or encompassing a network of second-generation native-born students. For example, if peer effects arise because of the formation of friendships, additional foreign-born peers may matter most when there are very few similar students available from which foreign-born students might form friendships. In this case, marginal peer effects would be zero where students have a large number of similar peers because the peer effects arising from the tendency toward homophilous friendships has reached it maximum. In fact, Weinberg (2013) demonstrates that with strong homophily in friendships, students are likely to experience the influence of their most similar peers once the population crosses a modest size threshold because students can strategically seek out similar students within a school.[1]

Given the long-term implications of better health in adolescence (Haas and Fosse 2008; Jackson 2010; Fletcher 2014), we might expect these effects to persist into adulthood. That is, the conditions that improve

[1] See also Fletcher and Ross (2018) and Fletcher, Ross, and Zhang (2020) for studies that directly examine peer effects that operate through friendship patterns.

students' mental health and health risk behaviors in adolescence might improve their health and economic outcomes in adulthood. Conditions that affect students' schooling attitudes and test scores should influence their likelihood of staying in school, their attendant employment outcomes, and the economic context of their neighborhoods—that is, where they are able to live—as adults.

III. Identifying Effects of Peer Composition

Estimating peer effects is complicated by several potential sources of bias (Manski 1993; Hoxby 2000). Peer composition is affected by the sorting of families across neighborhoods and into schools. If foreign-born students on average attend schools in less wealthy communities, the unmeasured correlates of these residential patterns will be misinterpreted as the effects of peer composition. Bias may also occur when data are generated from students' reports of peer characteristics. Correlated measurement error—the tendency to report similarly positive or negative outcomes for oneself and one's peers, for example—will upwardly bias peer effects.

To address these issues, we leverage a within-school, across-cohort identification strategy with data collected from a census of students within a representative sample of US high schools and middle schools. We estimate specifications that model student outcomes as a function of the composition of their cohort peers, conditional on school fixed effects. The design is motivated by an assumption: while families purposely select schools for their children, they are unlikely to select the specific cohort of students that their child will join, conditional on the school (Bifulco, Fletcher, and Ross 2011; Conger 2015). Variation in the share of foreign-born students across cohorts in a given school can be assumed to be quasi-random. We provide evidence for the validity of across-cohort identification by presenting balancing tests. After conditioning on school fixed effects, student foreign-born status, and race/ethnicity, we find at most modest and statistically insignificant correlations between peer composition and predetermined observable characteristics of families (such as maternal education). And because we use data from school censuses—students' reports about themselves—we reduce potential measurement error from sampling error or reporting bias (e.g., when individual and peer schooling outcomes are reported by the same respondent).

The causal effects of attending school with smaller and larger shares of foreign-born peers have been primarily documented in settings outside the United States. This research describes intermediate education outcomes—grade/level completion and standardized test scores. Several of these studies suggest that the presence of foreign-born peers lowers student performance on exams and rates of matriculation. Gould, Lavy, and Paserman (2009) find negative effects of immigrant students in classrooms on high school matriculation in Israel. Using Program for International Student Assessment data in Denmark, Jensen and Rasmussen (2011) find

negative effects of immigrant peers on both reading and math scores. Exploiting the rules of class formation in Italy, Ballatore, Fort, and Ichino (2014) find negative effects from immigrant classroom peers on native students for the language and math scores. Tonello (2015) finds a small negative effect on the Italian students' language test scores. Schneeweis (2011) uses an across-cohort, within-school strategy in Austria to show adverse effects on grade repetition and track attainment for migrant students in cohorts with a large share of migrant peers. Hardoy and Schone (2013) find negative peer effects from immigrant students in the classroom in Norway. By contrast, a handful of studies on native-born students in the Netherlands (Ohinata and Van Ours 2013, 2016), England (Geay, McNally, and Telhaj 2013), and Israel (Chachashvili-Bolotin 2016) do not detect negative effects on education outcomes of attendance with immigrant peers.

Despite the large, rich literature on immigrant schooling in the United States, few studies examine peer effects with strategies that address issues of confounding and measurement bias described above. Two exceptions provide arguably quasi-experimental evidence on the effects of having immigrant peers in the United States; both describe effects on schooling and testing outcomes.[2] Schwartz and Stiefel (2011) use administrative data in New York City and find that the share of immigrant students at the grade level has small, negative effects on foreign-born students' math scores but no association with native-born students' math scores. Conger (2015) uses administrative data in Florida and does not find evidence that foreign-born peers—including English language learners—affect their high school classmates' academic performance.

We build on these studies by applying a similarly rigorous strategy to the study of a much broader set of socioemotional and socioeconomic outcomes among a cohort of students from a nationally representative sample of schools across the United States. Unlike previous work, we follow these students into adulthood and consider the longer-run implications of cohort composition in school. In so doing, we also consider how the diversity of the foreign-born population in a given cohort shapes student well-being. Before turning to the data, we describe the across-cohort, within-school design in more detail.

IV. Method

A. *Approach*

We estimate a series of specifications that include the share of students with each school-specific cohort who is foreign born (eq. [1], $\bar{F}_{cs}$)[3] and the interaction of this share with whether the student is foreign born (F_{ics}):

[2] Vidgor and Nechyba (2007) estimate school fixed effects models but examine peers at the classroom level and so identify peer effects using variation that might be contaminated by the student-to-teacher assignment process.

[3] We use the term "cohort" to reference a student's grade or year in school.

$$y_{ics} = \beta_0 + \beta_1 X_{ics} + \beta_2 W_{cs} + \beta_3 \bar{F}_{cs} + \beta_4 \bar{F}_{cs} F_{ics} + C_c + S_s + \varepsilon_{ics}. \quad (1)$$

The outcome y of student i in cohort c in school s is a function of individual covariates X_{ics}, including the individual student's foreign-born status, school-cohort composition over the percentage of foreign-born students $\bar{F}_{cs}$, and additional school-cohort-level covariates W_{cs}, as well as cohort and school fixed effects C_c and S_s and an error term ε_{ics}.

We then explore heterogeneity in the peer effects captured by β_3 and β_4. Specifically, we examine whether peer effects are more or less salient when the cohort composition of foreign-born students shares ethnic background with the focal student. We do so by introducing an interaction term between the peer effect variable and a measure of how similar the school immigrant population is on racial and ethnic composition to the student's own race and ethnicity. Using a detailed measure of students' reports of race and ethnicity (described below), we measure the fraction of immigrants in each school who identify as members of group g (α_{gs}). For each student, we measure L_{is}, the fraction of the school's immigrant students whose ethnic identification matches that of the focal student:

$$L_{is} = \sum_g \delta_{ig} \alpha_{gs}, \quad (2)$$

where δ_{ig} equals one if the individual (whether immigrant or US born) belongs to racial or ethnic group g and zero otherwise. We interact this variable with the indicators that the student is foreign born ($F_{ics} L_{is}$), the share foreign born in the cohort ($\bar{F}_{cs} L_{is}$), and with the product of the cohort share foreign born and whether the student is foreign born—creating a triple interaction ($\bar{F}_{cs} F_{ics} L_{is}$):

$$\begin{aligned} y_{ics} = {} & \beta_0 + \beta_1 X_{ics} + \beta_2 W_{cs} + \beta_3 \bar{F}_{cs} + \beta_4 \bar{F}_{cs} F_{ics} + \beta_5 \bar{F}_{cs} L_{is} \\ & + \beta_6 \bar{F}_{cs} F_{ics} L_{is} + L_{is} + F_{ics} L_{is} + C_c + S_s + \varepsilon_{ics}. \end{aligned} \quad (3)$$

The specification tests whether attending school with a larger share of foreign-born students has larger or smaller effects for foreign-born students if the schools' foreign-born population shares ethnic identification with the focal student.

Finally, in a supplemental analysis, we ask whether peer effects differ by key school-level attributes. First, we allow the effect of foreign-born peers to be nonlinear by interacting peer effects with whether the school has a share of foreign-born students that is above the median for our sample. Second, we allow peer effects to differ by whether the fraction of students whose families do not speak English at home is above the median and whether the fraction of students whose parents are foreign born is above the median. These tests provide additional evidence about the role of

language and the relevance of immigrant parents for first- and second-generation students in the studies' findings.

B. Data

The National Longitudinal Study of Adolescent Health (Add Health) is a school-based longitudinal study of health and education-related behaviors of adolescents. The survey follows individuals through young adulthood by collecting information on health, behavioral, and labor market outcomes. The data are nationally representative and were collected through stratified clustered sampling of schools and students. A sample of 132 schools—each associated with one of 80 communities—was drawn across the United States through a sample frame that was stratified by region, urbanicity, school type, ethnic mix, and size. Wave I of the survey was conducted between 1994 and 1995 and consisted of an in-school and an in-home survey. The in-school survey was administered between September 1994 and April 1995 to all students present at school on the given day and so represents a virtual census of the school population, with a sample size of over 90,000 students. Subsequently, an in-home sample of students was selected from the population of all students to be administered a more detailed survey and then to be followed longitudinally. From each school, the in-home sample included 17 male and 17 female students from each grade, and this sample was enhanced through oversampling based on characteristics such as race, twin status, and disability.

We use data from the in-school survey as well as waves I, III, and IV of the in-home surveys. The wave I in-home survey was conducted soon after the in-school survey between April and December 1995 and was completed by 20,745 students. The wave III in-home survey was conducted in 2001 and 2002, about 7 years after the original in-school survey (participants were 18–26 years old), and was completed by 15,197 individuals. The wave IV survey was conducted in 2008, 14 years after the original survey (participants were 24–32 years old). The wave IV in-home survey was completed by 15,701 individuals. The survey questionnaire administered in wave IV included modules on labor market activities, health, crime behavior, and characteristics of the neighborhood where the individual resides.

By definition, a study of the long-term implications of school composition must examine school composition in an earlier period than the present. School composition in the United States has undergone further change since the recruitment of the Add Health cohort. We discuss this and the potential implications of this study's findings for today's adolescents in the final section of the study.

C. Samples

The study analyzes two sets of outcomes: (*a*) short-run outcomes observed when students are still in school and (*b*) long-run outcomes observed when

students are young adults. All analysis measures school grade composition with data from the near-complete census collected from the in-school sample (90,118 students). Analysis of most of the short-term outcomes uses an analytical sample of ~73,000–79,000 students (depending on the outcome) from 132 schools, in grades 7–12. Of the 90,118 students originally surveyed, 6.6% are missing data on sex, age, or foreign-born status information and must be excluded; additional exclusions are from item nonresponse for each outcome. The only short-run outcome that uses a substantially smaller sample is the measure of vocabulary skill (described below), which was collected for the subset of students sampled for the in-home survey and as a result is estimated on a sample of approximately 13,800 students.

Analysis of long-term outcomes uses the in-home sample followed through wave IV, approximately 13 years after baseline. Approximately 20% of 13,800 sampled individuals attrite from the Add Health data by wave IV, resulting in an empirical sample of between 9,000 and 11,000 observations, depending on the outcome; samples vary somewhat across outcomes because of item nonresponse. Attrition across survey waves risks selection bias in the estimates, particularly if attrition is correlated with the predictor of interest, the percent foreign born by grade. We tested whether percent foreign born by grade predicted missingness in the measures and attrition by wave IV (see table A4; tables A2–A15 are available online). We find little evidence that missing data or sample attrition is selective on grade composition. Our results show a small, positive, and not statistically significant relationship between peer exposure and remaining in the sample for foreign-born students (a 3 percentage point increase in exposure is associated with a 0.6 percentage point increase in the likelihood of being retained in the sample) and no association for US-born students.

D. Measures

The aim of the study is to provide a multidimensional characterization of peer composition effects by examining a broad set of social and educational outcomes, first when students are in school (short-term effects) and then 14 years later in adulthood (long-term effects). The independent variable of interest for the analysis—school cohort composition—is measured from the in-school survey of over 90,000. We calculate the percent of students in each grade of each school who report being foreign born. Because the data come from a near-census of schools (vs. a sample), measurement error is minimized.

Measures of short-term outcomes also come from the in-school survey, which collected information on student's sociodemographic characteristics, family background, health status, risk behaviors, academic achievement, mental health, and school factors. Most of the self-reported outcome measures are subjective and attitudinal and may suffer from measurement or reporting error. The standard solution in the face of measure error is to group related outcome measures into specific domains, in our case

academic effort, social acceptance, health risk behaviors, and mental health (Kling, Liebman, and Katz 2007).[4] For each domain, we conduct principal component analysis (PCA) on the contributing survey measures and extract the first component to be used as the outcome measure. The PCA combines both dichotomous and continuous measures and allows individual measures to be negatively weighted to address reverse coding. Higher values on the resulting components indicate, respectively, greater academic effort, greater social acceptance, more engagement in health risk behaviors, and better mental health. PCA weights for the underlying measures are provided in supplemental materials. In the short run, we do have more objective information on language skills captured by the PVT score administered in wave I of the in-home survey, and we examine peer effects on the PVT test directly.

The analysis of adult outcomes draws from the wave III and IV in-home surveys. Akin to the analysis of adolescent outcomes, we examine more objective outcomes, such as earnings, schooling, and the postschool PVT test directly. However, for more subjective questions, we group responses into three domains: health risk behavior, mental health, and residential economic status.[5] We construct the residential economic status measure with information about poverty, employment, and average labor income values in the census tract in which respondents live in wave IV. To do this, we use geocodes with the restricted Add Health data to merge on tract-level information from the American Community Survey (ACS). We also examine effects of peers on a PVT test score administered in wave III and on wave IV years of education and earnings where the logarithm of

[4] The composite dependent variables for the adolescent analysis were constructed through extracting the first principal component of survey variables from the wave I in-school survey grouped as follows: academic effort (Try to do homework well, Time spent watching TV on school day, Skipped school during last 12 months, Trouble getting homework done, Trouble getting along with teacher), social acceptance (student responses to questions indicating the following: Feel close to peers, Feel part of school, Feel that students at school are prejudiced, Are happy to be at this school, Feel socially accepted), health risk behavior (Smoked cigarettes last 12 months, Drank alcohol last 12 months, Got drunk last month, Raced on bike or car last 12 months, In danger due to a dare last 12 months), and mental health (Felt depressed last month, Trouble relaxing last month, Moody last month, Cried a lot last month). Descriptive statistics and component weights are provided in tables A2 and A3.

[5] The composite dependent variables for the long-term analysis were constructed through extracting the first principal component of the underlying variables from the wave IV in-home survey grouped as follows: health risk behavior (Days smoked last month, Days drunk last year, Days smoked marijuana last month, Has used hard drugs), mental health (How often feel isolated, Not in control of life, Confident to handle problems, Things go respondent's way, Overwhelmed with difficulties, Bothered by things last week, Blues last week, Feel as good as others last week, Trouble concentrating last week, Depressed last week, Tired last week, Happy last week, Enjoyed life last week, Sad last week, Feel disliked last week, Feel not respected last week), and residential economic status (Unemployment rate, Male labor force participation rate, Log income per capita, Proportion persons in poverty, Proportion of housing vacant, Log median house value, Total adult arrests, Total juvenile arrests). Descriptive statistics and component weights are provided in tables A2 and A3.

one plus earnings is used to address skewness in the earnings data and avoid dropping observations with zero earnings.

E. Descriptive Statistics

Roughly 10% of the sample reports being foreign born. These students are distributed unevenly across schools and grades, creating wide variation in the share of foreign-born students by grade; this variable ranges from 0% to 100%. The clustering of immigrant students results in a large difference in average cohort composition for foreign-born students (25% of grade-mates are foreign born) versus US-born students (8% of grade mates are foreign born).[6]

Table 1 presents the mean, standard deviation, minimum, and maximum of outcome and control variables. The table illustrates several differences between the US- and foreign-born students in background characteristics, as well as academic, behavioral, and health outcomes. Foreign-born students are advantaged in some domains but disadvantaged in others compared with their US-born counterparts. Foreign-born students express social acceptance that is about 5% of a standard deviation lower and mental health that is 11.5% of a standard deviation higher than US-born students, as captured by the principal component scales. However, foreign-born students are also 16% of a standard deviation lower in pursuing risky health behaviors and 6% of a standard deviation lower in their academic effort. In terms of the family background variables, foreign-born students have parents who are less likely to be in professional occupations but have a similar likelihood of attending college relative to parents of their US-born peers.

F. Balancing Tests

The study's identification assumes that though families might systematically select into schools, they do not sort into schools based on their child's cohort in school, which is primarily a function of the school and the child's birth date. We conduct a battery of balancing tests that examine the extent to which the deviations from the school average of cohort composition are correlated with student demographic and background characteristics. If the school fixed effects capture systematic selection on observed family and student characteristics, then we expect to find little or no correlation between student attributes and cohort deviations. In the peer effects literature, it is common to validate this assumption by testing whether families sort into schools based on their child's cohort using observable attributes, such as student demographics or family background. The logic behind these tests is that if students have not systematically sorted based on key observables, such as race or parental income, then they are unlikely

[6] Distributions are shown in fig. A1 (available online).

TABLE 1
Sample Means by Students' Foreign-Born Status

	Minimum (1)	Maximum (2)	US Born (3)	Foreign Born (4)	*t*-Statistic of (3)–(4) (5)
Outcomes in adolescence (wave 1):					
Social acceptance	−2.522	1.413	.004	−.044	4.099
Mental health	−2.769	1.046	−.012	.103	10.400
Health risk behavior	−.749	3.773	.015	−.151	14.356
Academic effort	−2.646	1.204	.008	−.055	6.827
PVT scores	−5.648	2.594	.130	−.521	24.359
Outcomes in young adulthood (waves 3 and 4):					
Family income	.000	13.732	9.500	9.672	1.852
Mental health	−4.370	1.387	−.003	.038	1.236
Health risk behavior	−.585	2.805	.016	−.206	8.901
PVT scores	−5.353	1.376	.080	−.254	10.146
Years of schooling	8.000	19.000	13.507	13.599	1.407
Residential economic status	−5.171	2.521	−.013	.170	5.781
Individual and family measures:					
Student's age	10	19	14.935	15.550	31.186
Student is female	0	1	.501	.494	1.197
Student race/ethnicity:					
White	0	1	.650	.256	71.908
Black	0	1	.201	.083	26.195
Asian	0	1	.045	.292	88.257
American Indian	0	1	.057	.028	11.223
Hispanic	0	1	.134	.503	89.177
Mother college graduate or higher	0	1	.295	.287	1.331
Father college graduate or higher	0	1	.343	.353	1.529
Either parent works for pay	0	1	.958	.924	13.040
Either parent in professional occupation	0	1	.410	.293	18.996
Live with both parents	0	1	.731	.712	3.762
Household size	1	6	4.267	4.609	25.133
Peer measure:					
% peers foreign born	.000	1.000	.079	.252	132.332
Friendship measure:					
Number of foreign-born friends	0	5	.038	.251	64.895
Proportion of foreign-born friends	.000	1.000	.022	.153	68.788

Source.—Add Health.
Note.—Minimum and maximum values are presented in cols. 1 and 2. Unweighted mean values are presented in cols. 3 and 4. Sample size varies for estimates that include the full school sample (wave 1 outcomes) and estimates that draw from the in-home sample followed into waves 3 and 4. Sample sizes are shown in tables 3 and 4.

to have sorted based on the factors that were not recorded in the survey, which presumably were not recorded often because they were less central than the observables (Altonji, Elder, and Taber 2005).

Table 2 presents the results of these balancing tests, in which the dependent variables are the family and student characteristics. These variables are regressed on the grade share foreign born using the same specification as shown in equation (2). Only two of the 28 balancing test

estimates are significant at the 5% level, which represents a set of results that could easily have arisen because of type 1 error. However, both of these failures are on variables related to race and ethnicity, and some of the estimates on race and ethnicity variables are sizable in magnitude whether statistically significant or not. Given the nature of immigration within the United States, it is unsurprising that we cannot easily separate foreign-born status from race and ethnicity, and so we repeat the balancing conditional on the race and ethnicity variables. Those tests are

TABLE 2
Balancing Tests

	% Peers Foreign Born		× Foreign Born	
Conditional on	Foreign Born (1)	+ Race and Ethnicity (2)	Foreign Born (3)	+ Race and Ethnicity (4)
Age	.122	.111	.219	.247
	(.227)	(.226)	(.164)	(.169)
Female	.121	.125	.027	.014
	(.101)	(.099)	(.026)	(.026)
White	−.094		.475*	
	(.096)		(.230)	
Black	.054		−.154	
	(.051)		(.081)	
Asian	.137*		−.313	
	(.058)		(.178)	
American Indian	.014		.019	
	(.044)		(.032)	
Others	−.130		−.035	
	(.075)		(.044)	
Hispanic	−.047		−.137	
	(.078)		(.115)	
Mother college graduate or higher	.134	.132	.056	.051
	(.094)	(.090)	(.046)	(.043)
Father college graduate or higher	−.064	−.068	.042	.028
	(.115)	(.111)	(.068)	(.072)
Either parent works for pay	−.051	−.047	.020	.009
	(.037)	(.037)	(.023)	(.022)
Either parent in a professional occupation	.028	.033	−.041	−.078
	(.067)	(.065)	(.050)	(.049)
Live with both parents	−.023	−.012	.112*	.077
	(.080)	(.076)	(.048)	(.040)
Household size	.190	.171	−.318	−.211
	(.208)	(.193)	(.167)	(.158)

Note.—Each cell contains coefficients from separate regressions that include an indicator that the student is foreign born, school fixed effects, and grade fixed effects. Columns 1 and 2 show estimates on percent foreign born, and cols. 3 and 4 show estimates on the interaction of percent foreign born with the foreign-born dummy. All regression specifications in cols. 2 and 4 control for race/ethnicity dummies. Robust standard errors are clustered at the school level. All estimates are based on the wave 1 in-school sample.
* $p < .05$.

supportive of our identification strategy as long as the models condition on both whether the individual is foreign born and their racial and ethnic background.[7]

V. Results

A. *Effects on Outcomes in Adolescence and Early Adulthood*

The equation (1) estimates are shown in table 3. Panel A presents the estimates with no additional controls, panel B includes controls for race and ethnicity, panel C includes controls for all balancing test variables, and panel D includes additional peer composition or cohort controls, including controls for cohort size, average parental education, and proportion black. The effects of the share of foreign-born students on the outcomes of US-born students can be detected from the zero-order term (percent peers foreign born). We find evidence of peer effects of foreign-born students on their US-born counterparts only for mental health, and those effects erode substantially as we add controls. We do not find any statistically significant effects for the other domains considered: academic effort, social acceptance, or risk-taking behavior. We note, however, that the estimates on mental health and academic effort are both negative but imprecisely estimated. We then turn to the effect on PVT scores, collected in the in-home survey. The PVT tests vocabulary of Standard American English and is meant as an estimate of verbal and scholastic ability. We similarly find little evidence that attending school with a larger share of foreign-born students reduces the test scores of US-born students, although again the precision of our estimates is limited. We confirm these findings with identical analysis limited to the US-born sample (not reported).

When we consider the effects of grade composition on foreign-born students relative to the effects on natives, we observe a different pattern. We find evidence for differences in peer effects for foreign-born students for all in-school outcomes tested and across all panels. Compared with the native-born students, we see that as the share of foreign-born peers increases in the cohort, the foreign-born students feel more socially accepted, have better mental health, engage in less risky behavior, and exhibit greater academic effort, relative to the effects for US-born students.[8] These effects are meaningful in size. Foreign-born students report

[7] Identification also requires that sufficient across-cohort variation in the share foreign born remains after controlling for school fixed effects. Table A5 shows that approximately 3.5% of the variation in share foreign born survives conditioning on school.

[8] Of course, these measures can be constructed in alternate ways. To consider the robustness of the findings, we tested regression specifications predicting individual component variables one by one. Out of 23 outcome variables considered, we find statistically meaningful peer effects on the interaction term with foreign-born students in all but seven cases. These effects operate in the direction signaled in table 3—they increase social inclusion and decrease harmful behavior.

TABLE 3
PEER EFFECTS ON STUDENT'S SHORT-TERM OUTCOMES

	Social Acceptance (1)	Mental Health (2)	Health Risk Behavior (3)	Academic Effort (4)	PVT Scores (5)
	A. Conditional on Foreign Born				
% peers foreign born	.038	−.379*	.162	−.260	.020
	(.204)	(.166)	(.316)	(.231)	(.352)
% peers foreign born × student foreign born	.309***	.219*	−.475+	.199**	−.788***
	(.086)	(.091)	(.260)	(.075)	(.224)
R^2	.040	.038	.080	.034	.224
	B. Also Conditional on Race and Ethnicity				
% peers foreign born	.070	−.368*	.175	−.215	.226
	(.199)	(.164)	(.310)	(.224)	(.352)
% peers foreign born × student foreign born	.264**	.231**	−.535*	.149*	−.992***
	(.087)	(.080)	(.209)	(.073)	(.252)
R^2	.044	.044	.091	.035	.261
	C. Conditional on All Covariates				
% peers foreign born	.077	−.258	.212	−.238	.084
	(.194)	(.168)	(.289)	(.209)	(.364)
% peers foreign born × student foreign born	.266**	.236**	−.545*	.156*	−1.030***
	(.091)	(.087)	(.220)	(.076)	(.214)
R^2	.058	.144	.120	.065	.322
	D. Conditional on All Covariates and Cohort-Level Controls				
% peers foreign born	.081	−.206	.134	−.253	.092
	(.186)	(.167)	(.261)	(.199)	(.390)
% peers foreign born × student foreign born	.262**	.236**	−.533*	.154*	−1.044***
	(.091)	(.086)	(.215)	(.075)	(.219)
R^2	.058	.144	.121	.065	.323
Observations	73,482	78,239	78,546	78,065	14,082

Note.—This table shows coefficient estimates from regressions predicting five outcome measures from wave 1 of Add Health. Standard errors are given in parentheses. All specifications include grade and school fixed effects, as well as a control for foreign born. Panel B presents estimates after adding controls for the race and ethnicity variables, and panel C presents estimates after adding all controls listed in table 2. Cohort-level controls include cohort size, average parental education, and proportion black. Columns 1–4 are based on the wave 1 in-school sample. Column 5 gives results for PVT scores based on the wave 1 in-home sample.

+ $p < .10$.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

feelings of social acceptance that are 6% of a standard deviation higher than the effect for US-born students, with a 1 standard deviation increase in the share of the grade that is also foreign born (19.9%). Similarly, foreign-born students' mental health is improved by 5%, engagement in risky behavior is reduced by 12%, and academic effort increases by 4% of

a standard deviation, again relative to the effect for native-born students.[9] However, it is important to note that for mental health and academic effort, we observe negative (but noisily estimated) peer effects for native-born students, and those effects for US-born students are similar in magnitude and similar to the interactive effect estimates for foreign-born students. All of the zero-order estimates for foreign-born students are shown in table A6. The first-order estimates are substantially noisier than the interacted effects. Therefore, while we have robust evidence that the effects on foreign-born students differ meaningfully from those estimated on US-born students (table 3), we cannot draw conclusions concerning the level of peer effects for foreign-born students.

When we turn to the effect on vocabulary scores, we see that the PVT scores of foreign-born students suffer with increased exposure to foreign-born peers, relative to their native-born counterparts. This result is precisely estimated, and the magnitude of this effect is striking. The standard deviation of the standardized PVT score is 1.24; therefore, a 19.9 percentage point increase of the share of foreign born induces 17% of a standard deviation decrease in the PVT score.

As discussed above, table 3 presents results first without controls, then with controls for race and ethnicity, and finally with all balancing test controls. If the study design holds and the school and grade fixed effects have absorbed the processes that sort families into different schools, we would not expect the coefficients estimated on cohort composition to change much with the inclusion of additional controls. As discussed alongside the balancing test results, it is difficult to separate foreign-born status from race and ethnicity, and the coefficients exhibit some parameter instability between panels A and B when we introduce measures of race. Focusing on the interaction between foreign born and the share foreign born: the parameters are quite stable as the additional controls are added—that is, between panels B and C. We further test the study's identifying assumption with an analysis of effect size changes to covariate inclusion, drawing from Oster (2014), who compares parameter changes with changes in R^2 as the controls are added. These tests indicate that the study results are robust to all but extreme forms of selection on unobserved measures.[10] The results are additionally robust and stable in magnitude between panels C and D, where we add controls for other variables that capture cohort composition.

We then investigate whether these effects endure as the adolescents age into adulthood. For this purpose, we take advantage of the panel nature of

[9] While the standard deviation of these outcome factors is 1 in the entire sample, these effects are adjusted by the standard deviation of each outcome for the foreign-born sample. The standard deviation of social acceptance is 0.86, of mental health is 0.87, of risky health behavior is 0.92, and of academic effort is 0.77.

[10] These tests are described in detail in the appendix (available online). The results are shown in table A7.

Add Health and examine outcomes found in waves III and IV of the in-home survey. Standardized PVT scores were collected in wave III, when the respondents were 18–24 years old. We use information on labor market, health, social, and behavioral outcomes found in wave IV, completed when the respondents were between 24 and 30 years old. Importantly, the wave III and IV surveys follow (by design) a subset of the in-school survey respondents—approximately 15,000 of the original 90,000—this survey feature has implications for our power to detect long-term effects in relation to the short-term effects we have outlined above.

The results of this analysis are found in table 4. We find effects for health behavior and PVT score in early adulthood that operate in the same direction as the effects in adolescence but are attenuated in magnitude. Similar to the results for PVT, we find relative declines in years of education completed. We find no effects on either earnings or residential environment, which would be expected to correlate with permanent income. In terms of magnitude, we find that a 1 standard deviation difference in exposure to foreign-born peers lowers the likelihood of risky health behavior of foreign-born students by 7% of a standard deviation and lowers the wave III PVT scores by about 8% of a standard deviation, and the same change reduces years of education by 0.14 years relative to effects on US-born students.[11] The lack of any effects on earnings may seem surprising given the negative effects on PVT and education but may arise because these negative effects of peers may be offset by improved employment-referral networks with other immigrant students (Bayer, Ross, and Topa 2008). Note that while the estimates in table 4 exhibit more parameter instability, likely because of the small sample size, these estimates still pass the Oster tests for outcomes with statistically significant peer effect estimates, again for all but extreme forms of selection.

The estimation of our adolescence and early adulthood models leads to 22 parameter estimates: five level and five interaction estimates based on the wave 1 survey and six level and six interaction estimates based on waves 3 and 4. The large number of parameter estimates raises concerns

[11] The standard deviation of the risk health behaviors index is 0.61, and the standard deviation of the wave III PVT score is 1.14. We also reestimate these models without school fixed effects to identify the conditional correlation between peer attributes and student outcomes. Failure to control for school fixed effects leads to evidence that might suggest larger effects on native-born students, such as less health risk behavior and higher earnings, and more long-term effects on foreign-born students, such as better mental health and worse residential economic status. See table A8. We repeat these analyses using students who are either first- or second-generation immigrants and measures of the share of first- or second-generation immigrants in the student's cohort—i.e., either foreign born or having at least one foreign-born parent. The results in tables A9 and A10 are similar, but the effect sizes are smaller than the results in tables 3 and 4. These results suggest that second-generation immigrants also contribute to and experience peer effects within schools but that first-generation immigrants experience larger effects. Similarly, the peer effect in some cases, especially social acceptance and mental health, appear smaller for immigrants who have a longer tenure in the country (table A11).

TABLE 4
Peer Effects on Student's Long-Term Outcomes

	Log Earnings (1)	Mental Health (2)	Health Risk Behavior (3)	PVT Scores (4)	Years of Schooling (5)	Residential Economic Status (6)
	A. Conditional on Foreign Born					
% peers foreign born	1.266	.235	.028	−.081	.821	−.321
	(1.764)	(.317)	(.288)	(.357)	(.875)	(.396)
% peers foreign born × student foreign born	.265	.116	−.152	−.352*	−.658**	−.007
	(.389)	(.126)	(.105)	(.171)	(.228)	(.145)
R^2	.036	.033	.061	.200	.158	.285
	B. Also Conditional on Race and Ethnicity					
% peers foreign born	1.326	.255	.063	.078	1.046	−.272
	(1.771)	(.310)	(.287)	(.350)	(.893)	(.370)
% peers foreign born × student foreign born	.283	.065	−.216*	−.389*	−.676*	−.028
	(.404)	(.129)	(.105)	(.154)	(.284)	(.136)
R^2	.038	.039	.069	.233	.174	.309
	C. Conditional on All Covariates					
% peers foreign born	1.268	.214	.108	.074	.492	−.339
	(1.696)	(.321)	(.276)	(.306)	(.820)	(.346)
% peers foreign born × student foreign born	.282	.073	−.201*	−.444**	−.724**	−.033
	(.390)	(.127)	(.101)	(.144)	(.250)	(.138)
R^2	.078	.057	.108	.269	.286	.321
	D. Conditional on All Covariates and Cohort-Level Controls					
% peers foreign born	1.266	.183	.050	−.052	.810	−.593
	(1.910)	(.379)	(.335)	(.293)	(.968)	(.370)
% peers foreign born × student foreign born	.178	.058	−.185+	−.448**	−.752**	−.042
	(.388)	(.127)	(.099)	(.141)	(.256)	(.140)
R^2	.079	.057	.108	.270	.286	.321
Observations	11,058	11,271	11,193	10,549	11,292	11,214

Note.—This table shows coefficient estimates from regressions predicting six outcome measures from waves 3 and 4 of Add Health. Standard errors are given in parentheses. All specifications include grade and school fixed effects, as well as a control for foreign born. Panel B presents estimates after adding controls for the race and ethnicity variables, and panel C presents estimates after adding all controls listed in table 2. Cohort-level controls include cohort size, average parental education, and proportion black. All columns are based on the wave 4 sample, except for the col. 4 results for PVT score that are based on the wave 3 sample.

+ $p < .10$.
* $p < .05$.
** $p < .01$.

about multiple hypothesis testing bias. A common way for assessing this bias is a simple Bonferroni correction where the *p*-value is adjusted by multiplying the value by the number of tests. We apply the Bonferroni correction using a step-down approach where the total number of tests (22) is used to correct the *p*-value for the test with the lowest *p*-value; then,

if significant, we proceed to the next-lowest *p*-value and reduce the number of tests for correction by one (21) and so on, moving to higher *p*-values and reducing the number of relevant tests as long as the previous test was significant.

Focusing on panel D of tables 3 and 4, many of our estimates on the differential response of native-born and immigrant students to peers are statistically significant at the 10% or lower level of confidence after the correction. In the short run, the larger negative peer effects for immigrant students on the PVT test score are significant at the 0.001 level even after multiplying the *p*-value by 22, and the larger positive peer effects for immigrant students on social acceptance are significant at the 0.1 level ($p = .07$) and on mental health above that level ($p = .12$). For early adulthood, the larger negative peer effects for immigrant students are significant on the PVT test score ($p = .04$) and years of college ($p = .07$). Even dropping these five results following a Bonferroni correction, the likelihood that the remaining three significant findings—adolescent health risk behaviors and academic effort and early adult health risk behaviors—arose by chance is low (approximately .04).[12]

B. Heterogeneous Peer Effects in Adolescence

To this point, the study does not distinguish among foreign-born adolescents in terms of ethnic origin or race. The US foreign-born population is heterogeneous in many dimensions, including ethnicity and associated country of origin. Research on peer networks in adolescence point to the importance of race and ethnicity in the process of friendship formation (Moody 2001; Currarini, Jackson, and Pin 2010). As a result, we might expect that the effects of attending school with foreign-born peers matters more if these peers share ethnic origin or race because the students are more likely to interact with more similar foreign-born peers. Alternatively, they may matter less if small variation in peers is more important when there are few similar foreign-born peers available. Specifically, if foreign-born students already have many friendship opportunities among similar students, then an increase in the share foreign born may not impact the students' social relationships.

For this purpose, we construct a variable that indicates the proportion of the foreign-born peers at the school level who share the same racial/ethnic group as a given student, whether or not the student is foreign

[12] One can calculate the likelihood of observing these three significant findings—adolescent health risk behaviors and academic effort and early adult health risk behavior—among the 17 remaining tests using the following equation:

$$\Pr[\beta = 0] = \frac{K!}{t!(K-t)!}(0.05)^{t}(0.95)^{K-t},$$

where K represents the number of tests and t represents the number of tests where the null of zero is rejected with a maximum likelihood of type I error of 0.05 (Ross et al. 2008). We find a 0.041 likelihood that all three of the remaining results could have arisen by chance among the remaining hypothesis tests.

born. We first construct this variable based on broad, pan-ethnic indicators for race and ethnicity. For students identifying as Asian and Hispanic in Add Health, it is possible to then create a second variable indicating the proportion of foreign-born peers who share the same country of origin. The means of these variables are shown in table 5—panel A (race and ethnicity) and panel B (country of origin; for Asian and Hispanic students only)—separately by student foreign-born status and by whether the school has an above- or below-median share of foreign-born students. We then interact each of these variables with the share of foreign-born peers, the indicator for foreign born, and their interaction. In light of the previous findings and the sample needed to support this analysis, we focus the tests on the short-run outcomes.

The results for the interaction between foreign-born peers and foreign-born status are presented in table 6.[13] Panel A repeats the short-run results from panel D in table 3 for reference. Panel B presents the results based on share of same race or ethnicity, and panel C presents the results based on shared country of origin. The results are similar for both share of same race or ethnicity and for share of same country of origin. Specifically, in the third row (percent peers foreign born interacted with student foreign born) in both panels B and C, the peer effects are larger and in the same direction as before, while the interaction of this variable with percent shared is in the opposite direction and sizable. Most of the effects of more foreign-born peers on foreign-born students are concentrated among foreign-born students who attend a school with more diverse foreign-born populations—or where fewer of their foreign-born classmates share their race, ethnicity, or country of origin.

This finding suggests that having additional foreign-born classmates is more meaningful when foreign-born students have fewer peers of similar origin from which to select friends or acquaintances.[14] As discussed above, one should not interpret these results as implying that there are no peer effects among the foreign born when foreign-born peers are similar on race, ethnicity, or country of origin. Rather, at a sufficiently high level of similarity, schools may present sufficient homophilous friendship opportunities such that students who are disposed to form these friendships will experience the same peer effects from friends even if the number of foreign-born peers increases, see Weinberg (2013).

[13] Note that we have included two-way interactions between student foreign born, percent schoolmates ethnicity, and percent peer foreign born in these models. We suppress those estimates because they interact a student-level variable with a school-level variable and so are identified by across-school variation rather than across-cohort, within-school variation. Therefore, we do not believe that we should be interpreting these specific estimates and prefer to focus entirely on the estimates arising from the within-school variation. Full results are available upon request.

[14] Estimates are noisy, and effects on long-term outcomes are difficult to detect given the smaller samples. See table A11.

TABLE 5
Mean of Percent School Shared Subgroup

Student's Race/Ethnicity or Country of Origin	% Foreign Born below Median		% Foreign Born above Median	
	Native Born (1)	Foreign Born (2)	Native Born (3)	Foreign Born (4)
	A. Shared Race and Ethnicity			
White	.406	.474	.264	.313
Black	.299	.330	.130	.177
Hispanic	.095	.170	.249	.349
Asian	.045	.146	.148	.225
American Indian	.012	.095	.004	.020
Others	.049	.131	.053	.059
	B. Shared Country of Origin for Hispanic and Asian Students			
Hispanic–Mexican	.187	.271	.425	.467
Hispanic–Chicano	.018	.097	.010	.024
Hispanic–Cuban	.027	.105	.253	.297
Hispanic–Puerto Rican	.058	.106	.063	.066
Hispanic–Central/South American	.063	.174	.222	.392
Hispanic–others	.053	.115	.079	.113
Hispanic–multiple/missing	.000	.000	.000	.000
Asian–Chinese	.053	.093	.168	.159
Asian–Filipino	.039	.122	.331	.369
Asian–Japanese	.014	.068	.039	.085
Asian–Asian Indian	.035	.145	.040	.046
Asian–Korean	.057	.125	.189	.336
Asian–Vietnamese	.118	.236	.087	.138
Asian–others	.056	.167	.039	.078
Asian–multiple/missing	.000	.000	.000	.000

Note.—This table presents the mean value of the school-level fraction of foreign-born students who share the same race/ethnicity or, in the case of panel B, the same country of origin as the current student. Columns 1 and 3 present the means for native-born students, and cols. 2 and 4 present the means for foreign-born students. Columns 1 and 2 present means for schools that have a percent of students who are foreign born below the median, and cols. 3 and 4 present the means for the school that have an above-median percent foreign born.

If the role of similarity is driven by homophily in friendships, one might also expect that peer effects for foreign-born share would be nonlinear where the importance of increasing the share of additional foreign-born students is smaller at schools with higher shares of foreign-born students. Similarly, both the presence of second-generation immigrant students and students who do not speak English at home may increase the opportunities for friendships that are isolated from the broad population of nonimmigrant students in the school. We create dummy variables for whether each school has an above-median share of foreign-born students, second-generation immigrant students, and students who do not speak English at home, and we estimate models that interact the foreign-born peer variables with these school attributes. We then compare the estimates on the interaction of student foreign born and share foreign born with a triple

TABLE 6
ROLE OF PERCENT FOREIGN-BORN SAME SUBGROUP ON STUDENT'S SHORT-TERM OUTCOMES

	Social Acceptance (1)	Mental Health (2)	Health Risk Behavior (3)	Academic Effort (4)	PVT Scores (5)
	A. Baseline Peer Effects				
% peers foreign born	.077	−.258	.212	−.238	.084
	(.194)	(.168)	(.289)	(.209)	(.364)
% peers foreign born × student foreign born	.266**	.236**	−.545*	.156*	−1.030***
	(.091)	(.087)	(.220)	(.076)	(.214)
	B. Peer Effects by % Shared Race and Ethnicity				
% peers foreign born	.126	−.211	.194	−.245	.148
	(.212)	(.175)	(.308)	(.211)	(.359)
% peers foreign born × % school shared ethnicity	−.096	−.083	−.034	.044	−.139
	(.169)	(.106)	(.141)	(.106)	(.168)
% peers foreign born × student foreign born	.469*	.335+	−1.145**	.137	−1.960***
	(.207)	(.178)	(.402)	(.216)	(.422)
% peers foreign born × student foreign born × % school shared ethnicity	−.458+	−.254	1.206**	−.025	1.279*
	(.244)	(.191)	(.402)	(.263)	(.591)
	C. Peer Effects by % Shared Country of Origin for Hispanics and Asians				
% peers foreign born	.023	−.320+	.213	−.304	.071
	(.195)	(.170)	(.307)	(.212)	(.375)
% peers foreign born × % school shared ethnicity	.720**	.601+	−.734	.665*	−.364
	(.248)	(.359)	(.482)	(.290)	(.798)
% peers foreign born × student foreign born	.401***	.328**	−.775**	.237+	−1.220***
	(.117)	(.119)	(.274)	(.138)	(.243)
% peers foreign born × student foreign born × % school shared ethnicity	−1.270**	−.986*	2.485**	−.807+	1.731+
	(.422)	(.427)	(.801)	(.416)	(1.016)
Observations	73,482	78,239	78,546	78,065	14,082

Note.—This table shows coefficient estimates from regressions predicting five outcome measures. Standard errors are given in parentheses. All specifications include all balancing test controls, including race and ethnicity controls, grade and school fixed effects, and a control for foreign born. Panel A presents the baseline peer effect estimates from table 3. Panel B presents estimates that interact the peer effect variables with a measure of the school-level fraction of foreign-born students who share the same race or ethnicity as the current student. Panel C presents similar estimates except that the school fraction foreign born is based on the fraction sharing the same country of origin for Asians and Hispanics and is set to zero otherwise. The models estimated for panel C also include dummies for each country of origin for the Asian and the Hispanic students. Columns 1–4 are based on the wave 1 in-school sample. Column 5 results for PVT scores are based on the wave 1 in-home sample.

+ $p < .10$.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

interaction of those two variables with the school dummy variables described above, as shown in equation (3). For all three school attributes and all outcomes, the triple-interaction coefficient is opposite in sign and approximately the same magnitude as the estimate on the interaction of student foreign born and share foreign born. The effect of additional foreign-born peers on foreign-born students in the same cohort is near zero when schools have above-median share foreign born, share second-generation immigrants, and share not speaking English at home, a pattern very similar to the results in table 6 based on the racial, ethnic, or country of origin similarity. These results are shown in table 7.[15]

C. Friendship Formation

Finally, to consider more directly the possibility that friendship networks shape the impacts of exposure to foreign-born students and to test when peer composition matters for the formation of those friendship links, we create additional outcome variables using the friendship network data in Add Health. First, in table 8, we show extraordinarily high levels of homophily based on student nativity. While foreign-born students make up nearly 40% of close friendships with other foreign-born students, they make up only 4% of close friendships of native-born students. In absolute terms, these differences between native and foreign-born students grow as the share foreign born in the school increases. Second, we examine how the number and proportion of foreign-born friends respond to the within-school variation in exposure to foreign-born grade mates. Column 1 of table 9 shows that while all (both native and foreign-born) students exposed to more foreign-born grade mates have more foreign-born friends, the effect is nearly twice as large for foreign-born students. These results taken together suggest homophily in friendships as one potential mechanism for why we find differential effects of exposure to foreign-born grade mates on foreign-born students (relative to native-born students).[16]

Columns 2 and 3 of table 9 show evidence consistent with our findings in tables 6 and 7 and with Weinberg (2013), who argues that changes in potential peers from a preferred group will matter less above some critical threshold. The argument is that since most people can maintain only a relatively small number of close friends, once the number of friendship

[15] Table A13 presents the estimates for US-born students, and table A14 presents the interaction estimates for the long-run outcomes.

[16] Note that we have included two-way interactions between student foreign born, percent schoolmates foreign born, and percent peer foreign born in these models. We suppress those estimates because they interact a student-level variable with a school-level variable and so are identified by across-school variation rather than across-cohort, within-school variation. Therefore, we do not believe that we should be interpreting these specific estimates and prefer to focus entirely on the estimates arising from the within-school variation. Full results are available upon request.

TABLE 7
Peer Effects on Foreign-Born Student's Short-Term Outcomes by School Attributes

	Social Acceptance (1)	Mental Health (2)	Health Risk Behavior (3)	Academic Effort (4)	PVT Scores (5)
	A. School % Foreign Born above Median				
% peers foreign born × student foreign born	−1.591	2.464+	−2.704	.988	−7.313
	(1.701)	(1.328)	(2.824)	(2.071)	(5.012)
% peers foreign born × student foreign born × school-level dummy	1.746	−2.337+	2.441	−.939	6.430
	(1.706)	(1.330)	(2.827)	(2.071)	(5.021)
	B. School % Non-English Speaking at Home above Median				
% peers foreign born × student foreign born	.975+	1.359***	−2.557***	1.215***	−8.724*
	(.519)	(.311)	(.479)	(.268)	(3.774)
% peers foreign born × student foreign born × school-level dummy	−.823	−1.265***	2.294***	−1.190***	7.867*
	(.525)	(.318)	(.506)	(.274)	(3.785)
	C. School % Second Generation above Median				
% peers foreign born × student foreign born	−.230	2.636**	−1.626	1.899**	−2.659
	(.939)	(.903)	(1.425)	(.581)	(2.704)
% peers foreign born × student foreign born × school-level dummy	.354	−2.481**	1.317	−1.808**	1.897
	(.944)	(.909)	(1.440)	(.589)	(2.715)
Observations	73,482	78,239	78,546	78,065	14,082

Note.—This table shows coefficient estimates for the foreign-born student interactions from regressions predicting five outcome measures. Standard errors are given in parentheses. All specifications include balancing test controls, grade and school fixed effects, and a control for foreign born. In order, the panels test whether the foreign-born interaction varies based on whether the school has a fraction of students above the median who are foreign born, who do not speak English at home, or who are second-generation immigrants based on one or more parent not being born in the United States, respectively. Columns 1–4 are based on the wave 1 in-school sample. Column 5 results for PVT scores are based on the wave 1 in-home sample.

+ $p < .10$.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

options is large enough, additional options become irrelevant for friendship composition. This theory would suggest that for foreign-born students, the effects of adding more foreign-born students on friendships should be smaller in schools that have many foreign-born students. That is what the interaction coefficients in columns 2 and 3 of table 9 show—for foreign-born students in schools with a large number of foreign-born

TABLE 8
NUMBER AND DISTRIBUTION OF FRIENDSHIP NOMINATIONS BY INDIVIDUAL- AND SCHOOL-LEVEL NATIVITY

		Based on School-Level % Foreign Born			
	Total (1)	Q1 (2)	Q2 (3)	Q3 (4)	Q4 (5)
A. Total students:					
Number of friends	.906	1.105	.914	.866	.729
Number of foreign-born friends	.058	.011	.017	.050	.158
Number of native-born friends	.848	1.094	.897	.816	.571
Observations	81,672	21,303	19,429	21,118	19,822
B. Foreign-born students:					
Number of friends	.632	.858	.534	.710	.609
Number of foreign-born friends	.251	.018	.045	.130	.322
Number of native-born friends	.381	.840	.490	.580	.287
Observations	7,737	282	629	1,525	5,301
C. Native-born students:					
Number of friends	.935	1.108	.926	.878	.772
Number of foreign-born friends	.038	.011	.016	.044	.097
Number of native-born friends	.897	1.097	.911	.834	.675
Observations	73,935	21,021	18,800	19,593	14,521

Note.—A friend tie is defined as a two-way nomination of friendship. "Number of friends" presents the mean of the number of mutual friends. The next two rows present the distribution of the friend's nativity. Columns 2–5 present the statistics by school-level percent of foreign-born students.

students, there are smaller increases in foreign-born friendship nominations as the share of foreign-born grade mates in school increases. We also find reductions in peer effects on native student friendship patterns when in schools with a large number of foreign-born students. Assuming that foreign-born grade mates are not native students' preferred group, this evidence is consistent with foreign-born segregation of friendship networks in schools when foreign-born student numbers are larger, possibly owing to the self-segregation of foreign-born students as their options for homophilous friendships increase.

VI. Discussion

Changes in regional mobility have reshaped the residential settlement patterns of immigrant families. An important, widely recognized implication of this trend is the accompanying compositional shift in peers with whom children and adolescents encounter at home and at school. Being educated with a larger (or smaller) share of foreign-born students has potential implications for a number of developmental markers for both foreign-born and US-born students. We estimate the effect of the presence of immigrant students in schools. We build on previous work—that almost exclusively leverages point-in-time administrative data—to study peer composition effects on a broad array of outcomes, including human capital, mental health, social acceptance, and health behavior. The data also allow us to follow individuals longitudinally and investigate the long-term consequences of

TABLE 9
Heterogeneity in Peer Effects on Student's Short-Term Outcomes: Friendship Nominations

Control for Schoolmates Foreign Born	None (1)	Schoolmates Foreign Born: % (2)	Schoolmates Foreign Born: Share >.1 (3)
A. Number of foreign-born friends:			
% peers foreign born	.364***	.501***	.513***
	(.085)	(.099)	(.090)
Foreign born × % peers foreign born	.336*	.289	1.196**
	(.167)	(.315)	(.408)
% peers foreign born × schoolmates foreign born		−.161	−.226
		(.724)	(.144)
Foreign born × % peers foreign born × schoolmates foreign born		−1.716**	−1.060*
		(.569)	(.435)
Observations	81,671	81,671	81,671
R^2	.118	.121	.120
B. Proportion of foreign born friends:			
% peers foreign born	.171***	.269***	.281***
	(.045)	(.068)	(.053)
Foreign born × % peers foreign born	.178*	.240	.860**
	(.089)	(.207)	(.269)
% peers foreign born × schoolmates foreign born		−.292	−.175*
		(.484)	(.079)
Foreign born × % peers foreign born × schoolmates foreign born		−1.035***	−.808**
		(.269)	(.280)
Observations	81,671	81,670	81,671
R^2	.109	.112	.111

Note.—This table shows coefficient estimates from regression for the number of foreign-born friends in panel A and for the share of friends who are foreign born in panel B. Column 1 presents the direct effects of share of the foreign-born peers in the cohort. Columns 2 and 3 allow these effects to vary based on the school foreign-born composition, either percent foreign born in col. 2 or percent greater than 10% foreign born in col. 3. All specifications include all balancing test controls, including race and ethnicity controls, foreign-born status, and grade and school fixed effects, as well as additional controls for cohort composition. These cohort-level controls include cohort size, average parental education, and proportion black. Standard errors are given in parentheses.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

peer composition in adolescence. We consider whether these effects differ for students who share ethnic identification with a larger share of the foreign-born students in their schools.

We find that the presence of immigrant peers has significantly different short-term effects on other immigrants relative to US-born students. For several outcomes, such as social inclusion and health risk behavior, schooling with a larger share of foreign-born students appears to improve immigrant students' well-being relative to the effects on US-born students. We also find significant negative relative effects on the standardized PVT score of the foreign-born students, providing a more

complicated picture of the impact of more and less segregated schooling experiences for foreign-born students. We find little evidence that presence of foreign-born peers negatively affects their US-born classmates, but our estimates are not precise enough to rule out peer effects on native students.

When we follow individuals longitudinally, we find that the negative relative effects on the PVT score persist. This effect is accompanied by small, negative effects on completed schooling by early adulthood. By contrast, we find no evidence of enduring differences between foreign-born and US-born students in the effect of peer composition on adult earnings. Although education and earnings are correlated, the effects on language and schooling appear to be small enough that they are not translating into a decline in labor market returns. Alternatively, these negative effects may be offset by advantages arising from having a better foreign-born job network.

We also observe that the short-term positive results on health behavior relative to US-born students fade by the time the respondents are 24–30 years old. The diminishment of these effects warrants attention in future research. The loss of these health benefits by adulthood is consistent with research that demonstrates an association between time spent in the United States and worsening health (Lopez-Gonzalez, Aravena, and Hummer 2005; Antecol and Bedard 2006; Hamilton, Palermo, and Green 2015). It is also consistent with research finding that networks in high school may be less protective when first-generation students leave home and navigate new institutions—college, service, employment, and so forth (e.g., Gonzales 2016).

In terms of mechanisms, several of our findings suggest that homophilous social relationships and interactions may help explain the peer effects between foreign-born students. Estimated peer effects are smaller when a foreign-born student shares the same race, ethnicity, or country of origin with the other foreign-born students in the school and also when the share of foreign-born students in the school is smaller and/or when there are fewer second-generation immigrants. These findings appear consistent with threshold effects where the effects of additional foreign-born students are most important when foreign-born students have smaller numbers of similar peers available, presumably because under those circumstances the opportunities for homophilous friendships are limited. We then investigate friendship patterns directly. We find that foreign-born students are more likely to have foreign-born friends when the number of foreign-born peers in their cohort increases, and just like the results above, these peer composition effects are smaller when the share of foreign-born peers in the school is low.

The results shown here differ in significant ways from other research using quasi-experimental design to peer effects. Most studies in European populations have examined testing outcomes. Our results demonstrate that an exclusive focus on testing in the European research may be

missing other domains in which immigrant peer effects are beneficial. Further, while we find negative effects of attending school with a larger share of foreign-born students on vocabulary testing, these effects are limited to foreign-born students. The absence of effects on native-born students for testing outcomes is consistent with Conger's (2015) study of Florida students and Schwartz and Stiefel's (2011) research on New York City students.

The Add Health data allow us to examine a rich array of outcomes. However, the study does not support analysis of English language learning (ELL) status among students. ELL status is a theoretically important source of heterogeneity in peer effects research, though one that has not borne out as empirically significant to peer outcomes in the United States (e.g., Conger 2015). The Add Health in-school survey does not ask about gradations of language skill. The survey metadata record the language in which the survey is taken, but an indicator of responding in a language other than English represents a tiny share of respondents (<1%). Tests using this data in triple interactions were uninformative.[17]

The foreign-born population in the United States is heterogeneous in many ways. The advantages of using longitudinal survey data are accompanied by some limitations in the kind of heterogeneity that can be tested. When we considered variation in the effects on short-term outcomes by maternal education, by Hispanic ethnicity, and by adolescent gender, we observed estimates that were similar in direction and—in most cases—magnitude across these groups (results are available upon request). It is essential to note that the sample is not well powered to detect small variation across these dimensions. Research diving deeper into heterogeneity in the peer effects of schooling with migrant classmates may be better supported with point-in-time administrative testing data (e.g., Schwartz and Stiefel 2011; Conger 2015).

Interpreting the results of this study requires attention to a few points. To improve measurement and to reduce the number of statistical tests—and the attendant risk of showing only a subset of findings—we use a data-reduction exercise that incorporates a large set of indicators from the rich Add Health data. We chose these items based on theoretical grounds (vs. data exploration) and generate outcome measures using the first principal component of each of these variable sets (see, e.g., Kling, Liebman, and Katz 2007). For both the short-run and the long-run outcomes, we attempt to use all relevant measures within the domain available in the survey (vs. selecting a subsample based on results).

It is also important to stress that the identification strategy used in the present study focuses on the causal effect of attending school in a cohort with a larger or smaller share of immigrant peers. It is, of course, possible

[17] The survey does collect information in the in-home survey about languages spoken at home, and we find no effects of higher foreign-born share in schools with above-median share of students not speaking English at home. Whether this represents ELL status for students is ambiguous. It may also represent multilingualism.

that shifts in immigrant peers generate change that is not grade specific but school specific. Consider, for example, a school that hires a new principal or additional teaching aids or that tailors the curriculum in response to a rising share of immigrant families in a community. If this type of response has a positive effect on all of the foreign-born students in a school, the cross-cohort, within-school identification will sweep out this variation and underestimate the salutary effects on immigrant students of having the share of immigrants in the school increase (i.e., increases beyond that reflected in the grade alone). Similarly, there is some evidence that parents respond to the overall school immigrant composition by moving to other school districts or by moving children to private schools (Cascio and Lewis 2012). The differential selection of students into the school in response to the cohort composition might affect the overall learning environment, but this study cannot speak to the effect of this selection mechanism. Finally, immigration in the local labor market might affect students' outcomes through demand for particular skills in the labor market or through competition for college admissions (e.g., Burstein et al. 2017). Such overall impacts of the immigrants in a given labor or college market operate above and beyond the impacts that we uncover in this paper and deserve a dedicated study of its own.

The identification used here is useful in considering a broad class of policies that likely have consequences for changes in peer interactions between US-born and foreign-born students. For example, many policies around school choice measures and charter schools may create modest changes in the composition of students (Bifulco, Ladd, and Ross 2009). Our results suggest that these policies could indeed differentially affect foreign- and native-born students in the domains described above. Busing policies are another example that might change the average level of contact between foreign-born and native-born students. Policies that shift the likelihood of contact with foreign-born students could have impacts similar to those described here. It is worth noting that qualitative research on busing policies suggest that lower-income students are highly racialized and that the effects of intergroup interaction are heavily shaped by students' gender (Ispa-Landa 2013). Future work using the type of the design explored here would benefit from explicit attention to how peer effects differ by gender.

More broadly, the results suggest opportunities to leverage the positive effects of school-based ethnic enclaves in terms of social cohesion and health risk behaviors and at the same time deploy additional resources to further enhance English proficiency in first-generation students. And for immigrant students who are educated in places with larger shares of US-born peers, ongoing research on mechanisms of support—particularly in new destination regions—is warranted (e.g., Silver 2015; Spees, Perreira, and Fuglini 2017). By definition, our research on the longer-term effects of school composition are focused on the outcomes of schools in the 1990s. Since then, immigration patterns and internal migration patterns have

continued to change. Movement into new destination regions increased substantially in the 1990s—the number of immigrants increased 14% per year between 1990 and 2000 (Terrazas 2011). The Add Health cohort was recruited in 1997, and migration has continued to change the composition of southern and eastern regions of the country since then. We use data from the 1990 and 2000 censuses and the 2015–17 ACS to shed some light on this (table A14). The evidence indicates that foreign-born adolescents are less likely to drop out of school and less likely to be employed now than in the 1990s; they also live in smaller, better-educated families. Alongside these changes, the results here provide little evidence that a rising share of foreign-born students in adolescents' classrooms in new destination regions will negatively affect US-born students' well-being, undermining the evidentiary support for political rhetoric to this effect.

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