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ABSTRACT

Does Immigration Affect the Long-Term Educational Outcomes of Natives? Quasi-Experimental Evidence*

This paper uses the mass migration wave to Israel in the 1990s to examine the impact of immigrant concentration during elementary school on the long-term academic outcomes of native students in high school. To identify the causal effect of immigrant children on their native peers, the empirical strategy must address two sources of bias: the endogenous sorting of immigrants across schools, and the endogenous grade placement of immigrants within schools. We control for the endogeneity of immigrant placement across schools by conditioning on the total number of immigrants in a school and exploit random variation in the number of immigrants across grades within the same school. To address the endogenous grade placement of immigrants within schools, we use the immigrants' dates of birth as an instrument for their actual grade placement. The results suggest that the overall presence of immigrants in a grade had a significant and large adverse effect on two important outcomes for Israeli natives: the dropout rate and the chances of passing the high school matriculation exam which is necessary to attend college.

JEL Classification: I20, J24

Keywords: school quality, natural experiment, peer effects, dropout rates, immigrant absorption

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1. Introduction

An increasingly important issue faced by Western countries at the turn of the 21st century is the social tension created by growing numbers of immigrants from less developed countries. The effect of immigration on the local labor market has received considerable attention in the literature, but little is known about the impact of immigration on the school system. This examines the impact of immigrant concentration during elementary school on the long-term academic outcomes of native students in high school. The focus on the long-term outcomes of natives, seven years after their exposure to the "treatment," is one of the distinguishing features of this paper.

Our analysis exploits a unique opportunity to identify peer effects of immigrant children in the classroom using the mass migration wave from the former Soviet Union ("FSU") to Israel in the early 1990's. The sheer size of the immigration wave, nearly 20 percent of the total population within a decade, produced large variation in the absorption level of recent immigrants across schools throughout the country. The raw data show that native students who went to school with more immigrants in 5th grade typically had lower achievements in high school. This correlation is not surprising, given that immigrants tended to locate in poorer areas where housing costs were lower. Therefore, identification of the causal effect of immigrants on their native peers must account for the endogenous placement of immigrants into areas that are more likely to be populated with lower-achieving native students, regardless of the local level of immigrant concentration.

In addition, the data reveal that immigrant students are much more likely to be held back a grade than native students. For example, only 55 percent of the immigrant children in our sample who are supposed to be in 5th grade according to their date of birth are actually in the 5th grade. The decision to hold an immigrant student back a grade within a school could be influenced by the characteristics of the native students in the two adjacent grades. It could be the case that immigrant students were more likely to be placed with the stronger or weaker students within the same school, thus producing a spurious correlation between the presence of immigrants in the grade and the future outcomes of native students. Therefore, identification of the causal effect of immigrants on their native peers must correct for two potential sources of bias: the endogenous sorting of immigrants across schools (mostly into poorer areas), and the endogenous grade placement (holding back) of immigrants within schools.

To control for the endogeneity of immigrants across schools, our empirical strategy exploits random variation in the number of immigrants across grades within the same school. For example, conditional on the total number of immigrants in a given school in grades 4 to 6, the actual number of immigrants in grade 5 can be considered as being determined solely by random factors such as variation in the year of birth among the pool of immigrant children in the school district. This natural experiment is best thought of as the random drawing of balls from an urn in which one third of the balls are marked with the number "4", one third with the number "5", and one third with the number "6". Conditional on the total number of balls drawn, the number of "5" balls is as good as randomly assigned. Conditioning on the total number of immigrants in the advanced grades in elementary school removes much of the heterogeneity across schools that may confound our estimates: schools that absorbed the same number of immigrants are likely to be similar among themselves, not only in their propensity to absorb immigrants, but also in other characteristics.

To account for the second type of selection, the potentially endogenous holding back of immigrants within a school, as well as to address any potential attenuation bias due to measurement error in the grade placement of students in our administrative data, we instrument for the actual percentage of immigrants in 5th grade with the *predicted* percentage of immigrants. The predicted percentage is calculated by assigning to each child the grade he/she should have attended based on his/her exogenously determined date of birth. Our identifying assumption in this case is that the predicted number of immigrants in grade 5 is as good as random, conditional on the *predicted* number of immigrants in grades 4 to 6. Summing up, the empirical strategy controls for the endogenous placement of immigrants across schools by conditioning on the total number of immigrants in grades 4 to 6, and controls for the endogenous grade placement of immigrants within schools by using their dates of birth as an instrument for their actual grade placement.

To implement our empirical strategy, we use administrative panel data on school enrollment and test scores for each 5th grade child from the 1993-1994 school year until the 2000-2001 school year, the year this cohort was scheduled to graduate from high school. A set of balancing tests indicate that both the actual percentage of immigrants and the predicted percentage of immigrants in 5th grade are not systematically related to school and student background characteristics, once we control for the number of immigrants in grades 4 to 6 and the total number of children in grade 5. When we regress the high school achievements of native students on the *actual* percentage of immigrants in 5th grade, we find that the concentration of immigrants in elementary school has essentially no effect on their dropout rates, while it has a relatively small and marginally significant effect on high school matriculation rates. In contrast, using the instrumental variable strategy to account for the endogenous placement of immigrants across grades, we find larger and more significant adverse effects of the presence of immigrants on the outcomes of natives.

We also show that high school achievements of children who were in 5th grade in 1994 are affected only by the percentage of immigrants in 5th grade, but not by the percentage of immigrants in first and second grades. This lends strength to the notion that our estimates are indeed picking up the effect of immigrant concentration in the grade, and not the effect of other unobserved school characteristics. Finally, we look separately at the effects of immigrant boys and immigrant girls on the achievements of native boys and girls. We find that native children are especially affected by the presence of immigrant children of their own gender, suggesting that part of the mechanism driving the effect may be due to the influence of peers.

The rest of the paper is structured as follows: the next section reviews the existing literature that is most closely related to our work. Section 3 presents a brief background of the immigration episode we are exploiting and describes our data. Section 4 describes the empirical methodology for dealing with the endogenous placement of immigrants across schools. Section 5 adapts the strategy in Section 4 in order to generate instrumental variables (IV) results that take care of the endogeneity of immigrants across schools and the potential endogenous placement of immigrants across grades within a school. Section 6 performs robustness checks and Section 7 examines the gender composition of the immigrants and discusses the magnitude of the effects. Section 8 concludes.

2. Literature Review

While there is a vast literature on the effects of immigration on native labor market outcomes,[1] the question of whether immigration affects natives' educational outcomes has received relatively little attention. Exceptions include Betts (1998), who examines whether immigration reduces the contemporaneous high school graduation rate of natives, and Hoxby (1998) and Borjas (2004), who look at whether immigrants crowd-out natives from slots in college and graduate programs. Also, Betts and Fairlie (2003) investigate whether immigration in California induced native flight from public to private schools.

Closely related to our paper is also the literature on desegregation and classroom peer effects: Angrist and Lang (2004) study the effects of Metco, a long-running desegregation program that sends minority children out of the Boston public school district to a large and affluent suburban district. Overall, they find little evidence that the test scores of non-Metco

[1] See for example Card (1990, 2001); Altonji and Card (1991), Lalonde and Topel (1991), Hunt (1992), Borjas, Freeman and Katz (1996), Borjas (2003); in the Israeli context, see Friedberg (2001) and Cohen-Goldner and Paserman (2004 and forthcoming).

students are affected by the presence of disadvantaged peers, although there seems to be some evidence of a negative effect on the test scores of non-Metco minority students. Guryan (2004) studies the effects of desegregation from the 1950s to the 1980s, and concludes that black dropout rates fell as a result of desegregation, while desegregation did not affect the dropout rates of whites. Other studies that have looked directly at peer effects in the classroom using large administrative data sets include Boozer and Cacciola (2001), Hanushek et al. (2003), Lefgren (2004), Betts and Zau (2004), Vigdor and Nechyba (2004) and Burke and Sass (2004). Sacerdote (2001) and Zimmermann (2003) study peer effects in a university setting, and find that academic and social outcomes of college students are affected by the quality of randomly assigned roommates. Hoxby (2000a) uses an identification strategy similar to our own, exploiting idiosyncratic variation in gender and race composition of adjacent cohorts in Texas public schools, and in a wide range of specifications she finds that children's elementary school test scores are affected by those of their peers, with intra-race peer effects appearing to be particularly strong.[2] In contrast to Hoxby, our focus is on the effect of immigrants on natives and we examine long-run effects, rather than the contemporaneous effects of peers.

Recent years have also seen a flurry of research that uses exogenous variation created by true or natural experiments to study the effects of peers and the environment at the neighborhood level on a variety of educational, health, and economic outcomes (Katz, Kling and Liebman, 2001; Edin, Fredriksson and Åslund, 2003; Oreopoulos, 2003; Jacob, 2004; Weinberg, Reagan and Yankow, 2004). In the Israeli context, Gould, Lavy and Paserman, (2004a) found that Ethiopians who arrived during Operation Solomon generally benefited from attending better schools. However, before claiming that sending immigrants to better-performing schools represents a net gain from the point of view of society, it is crucial to

[2] An identification strategy based on random variation in cohort size at the school level is also used by Angrist and Lavy (1999) and Hoxby (2000b) to estimate the effects of class size on student achievements.

assess whether children in immigrant or minority-receiving schools are affected by the influx of disadvantaged peers, as we do in the current study.

3. Background and Data

The mass migration from the former Soviet Union to Israel since 1989 can be viewed as a unique laboratory for the social sciences. More than one million immigrants moved to Israel since 1989, increasing its population by a remarkable 20 percent within a decade. The migration wave was also highly concentrated, with approximately 330,000 immigrants arriving in 1990 and 1991. The fall of the Berlin wall in November 1989 was the trigger that started the whole process, as a number of factors combined to induce migration. First, the Soviet Union lifted its emigration restrictions. Second, the political instability and the uncertain economic climate in the beginning of the 1990s greatly increased the incentives to migrate. Third, Israel was one of the few viable options, as it imposed no entry restrictions and no lengthy waiting periods. Along with the mass migration from the former Soviet Union, the early 1990s also saw the exodus of 15,000 Ethiopian Jews in the context of Operation Solomon in May 1991, and increasing immigration rates from other countries, mainly the United States and Argentina.

Not surprisingly, there are large differences in the family background of immigrants from different source countries, and the overall immigrant population is quite different from native Israelis in terms of family background. These differences are displayed in panel A of Table 1, which shows the background characteristics of 5th grade students in 1994, by immigrant status and by country of origin, based on the Ministry of Education administrative data that is used throughout the paper. We define as immigrants all children who were born outside of Israel and immigrated after January 1st, 1989. The table reveals that immigrant children from the former Soviet Union generally have parents who are more educated than native Israeli children (a little more than one additional year of education for each parent), and come from smaller families (1.10 siblings versus 2.41 for native Israelis). In contrast, Ethiopian immigrant children come from much larger families (an average of 4.28 siblings) but have parents with only one or two years of education. The vast majority of immigrants during this period came from the former Soviet Union, so the characteristics of the overall immigrant population are dominated by the characteristics of the Soviet immigrants.

While it appears that native Israeli children were exposed to a new immigrant population with more favorable family background characteristics, it is also true that the new immigrant population displayed several signs of social and economic distress. Panel B of Table 1 presents characteristics of households with children between the ages of 8 and 12 drawn from the 1995 Israeli Census.[3] Compared to native Israeli children, Table 1 reveals that immigrant children came from poorer households which tend to rent rather than own their own homes, and where the head of the household is much more likely to be female and/or unemployed. Again, the Ethiopian immigrants display much larger signs of weakness compared to the Russians, but both groups of immigrants are less favorable compared to the native Israeli population. So, while it is true that the overall immigration wave during this period flooded the country with immigrants who were generally well-educated, the overall picture reveals that these immigrants displayed several signs of socio-economic difficulties, which made them weaker in many respects than the native population. Therefore, despite the unique characteristics of this mass migration episode, immigrants to Israel share characteristics that are common to immigrants in all Western societies, making our results relevant beyond the Israeli context.

To examine whether the influx of this immigration wave into elementary schools influenced the high school outcomes of native Israeli children several years later, we link detailed information on each child's school environment in the 5th grade to his or her achievements throughout high school. The data comes from administrative records collected

[3] The "long questionnaire" of the Israeli Census was administered to 20% of Israeli households.

by Israel's Ministry of Education on the universe of Israeli elementary schools in the 1993-1994 school year, the first year a computerized file is available. The data is based on reports from school authorities to the Ministry of Education at the beginning of the school year. The file contains an individual identifier, a school and class identifier, and detailed demographic information on all immigrant pupils in grades 1 to 6, and all pupils in grades 1, 2, and 5.[4] We are able to exactly identify pupils by their country of origin and date of immigration (month and year), so we know precisely who is a native Israeli student and how many immigrant children were present in the 5th grade class of each elementary school. We focus our attention on the 5th grade cohort in 1993-1994 for two reasons. First, this is one cohort for which we have complete data on native Israeli pupils. Second, 5th grade students in the 1993-1994 school year had enough time within our sample period (which ends with the 2000-2001 school year) to finish high school if they progressed through the system without repetition. The data set also contains the birth date of each child, which is mapped into each child's *predicted* grade level using the enrollment cut-off dates for that cohort.[5] The predicted grade level will be used in our instrumental variable strategy.

We link the elementary school records to individual data on high school enrollment and matriculation exam outcomes in the 1998-1999, 1999-2000, and 2000-2001 school years. Therefore, we are able to follow each native Israeli student from 5th grade in 1993 all the way through the advanced stages of high school. We study two important high school outcomes: dropping out before completing 12th grade, and passing the matriculation exams. The latter outcome is particularly important in Israel since it is required to attend college. Similar high school matriculation exams are found in many countries and in some states in the United

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[4] Note that we do not have any data on native children enrolled in grades 4 and 6 in 1993/1994. This prevents us from using a traditional identification strategy based on multiple cohorts and school fixed effects. In practice, though, the estimator we use is very similar to a fixed effects estimator.

[5] In Israel, children roughly enter first grade in September of the calendar year in which they turn six years old. We say "roughly" because the relevant threshold dates are based on the Hebrew calendar. For example, the first grade class of September 2005 is composed of children born between the 1st of Tevet 5759 (December 20th, 1998) and the 30th of Kislev 5760 (December 9th, 1999). For conversion between Hebrew dates and Gregorian dates, see http://www.hebcal.com.

States. Examples include the French Baccalaureate, the German Certificate of Maturity (Reifezeugnis), the Italian Diploma di Maturità, the New York State Regents examinations, and the recently instituted Massachusetts Comprehensive Assessment System.

We should note that our key variable of interest, exposure to immigrants in 5th grade, is taken as a proxy for the total exposure to immigrants throughout elementary school. By the end of 5th grade, native students had been exposed to immigrants for an average of 31.7 school months (slightly more than 3 school years). Interestingly, the length of exposure to immigrants is nearly completely uncorrelated with the proportion of immigrants in the school.

Table 2 presents summary statistics for various measures of the 5th grade learning environment faced by native Israeli students. We classify the learning environment variables into two groups, "peer characteristics" and "school characteristics." The "peer characteristics" describe the family background of the native Israeli 5th grade students in the school. The "school characteristics" contain additional aspects measured at the school level: average scores for the 4th and 5th grade students on the 1991 standardized math and verbal tests[6] and a 1991 socio-economic index of students in each elementary school.[7] All of the "school characteristics" are taken from the 1991 school year, which is two years before the sample of native students used in Table 2 attended 5th grade.

Summary statistics are presented in four columns in Table 2, whereby the sample used in each column is defined by the total number of immigrants absorbed into the elementary school in grades 4 to 6. For example, the first column presents summary statistics for only the native Israeli 5th graders who attended an elementary school in 1993-1994 where the total

[6] In June of 1991, near the end of the school year, all fourth and fifth grade students were given achievement tests designed to measure mathematics and Hebrew reading skills. The scores used here consist of the average score on some of the basic and all of the more advanced questions in the test, so that scores are scaled from 1 to 100. The low average and the high variation of scores on this achievement test generated considerable public controversy in Israel, leading to the abandonment of the national testing program in 1992. This is the same data used in Angrist and Lavy [1999].

[7] The socioeconomic index is based on a function of the pupils' fathers' education and continent of birth, and of family size. The raw index is recorded as the percentage of students in the school who come from what is defined to be a disadvantaged background. Our socioeconomic status index is obtained by standardizing this variable and multiplying it by –1, so that high socioeconomic status schools have a high value of the index.

number of post-1989 immigrants in grades 4 to 6 was between 1 and 10. The next column uses a range of immigrants in 4th to 6th grades between 11 and 20, and the next two columns use ranges of 21-30 and 31-40 respectively. The last column shows the summary statistics for all schools that had between 1 and 40 immigrants in grades 4 to 6.

A close inspection of the numbers in Table 2 reveals why it is important to break down the sample in this manner. When comparing the means across columns, a clear pattern emerges: schools with larger numbers of total immigrants in grades 4 to 6 are increasingly worse off in terms of the characteristics of the native Israeli students. For example, as the number of immigrants in the school increases, the parental education of native students falls, the percent of students with origins from Asia and Africa (which is widely considered to be a disadvantaged ethnic group in Israel) increases, the average math and verbal scores of the elementary school fall, and the socio-economic index of the school falls. In addition, immigrants represent only 4.04 percent of the 5th grade class when the total number of immigrants in 4th to 6th grades is between 1 and 10, but increases to 15.47 percent when there are 31-40 immigrants in the same three grades. Overall, a clear pattern emerges: larger numbers of immigrants in grades 4 to 6 are associated with larger concentrations of immigrants in 5th grade, weaker native Israeli students in terms of their family background, and weaker schools in terms of student test scores in previous years. These results are not very surprising since new immigrants were not placed or directed to areas where the best schools and most-educated parents are likely to be. Instead, immigrants typically started out in areas with lower-cost housing, and therefore native Israelis from disadvantaged backgrounds and lower-performing schools were more likely to attend schools with a higher concentration of immigrants.

The lower panel of Table 2 presents the high school outcomes of native Israeli students according to the number of immigrants in grades 4-6 in their school in 1993-1994, while they were in 5th grade. Not surprisingly, the table indicates that native students who attended schools with a larger proportion of immigrants in 5th grade also had lower outcomes in high school: they dropped out of high school more often, and were less likely to pass the matriculation exams. The first column in Table 3 demonstrates these correlations more clearly by regressing each outcome variable on the percent of immigrant students in each native child's 5th grade class. For both outcomes, the coefficient on the proportion of immigrants in 5th grade is large and highly significant. A 10 percentage point increase in the immigrant share is associated with a 1.3 percentage point increase in the dropout rate, and a 5.7 percentage point decrease in the matriculation rate. However, the coefficients are reduced dramatically in size and significance in the second column of Table 3, which controls for the individual and school characteristics described in Table 1. Adding an additional control for the number of immigrants in the school from grades 4 to 6 in column (3) in Table 3 has little effect on the size of the coefficients, but it increases the standard errors and, as a result, the coefficient on the dropout rate becomes insignificant.

Overall, the results in Tables 2 and 3 show that natives from weaker backgrounds tend to be with more immigrants in 5th grade, and that the proportion of immigrants in 5th grade is negatively correlated with natives' high school outcomes. As a result, we cannot draw any inferences about causality since natives who attended elementary schools with more immigrants may have achieved less in high school because of the presence of the immigrants, or because of other characteristics that are correlated with a larger concentration of immigrants. Although we can control for many observable characteristics which explain much of the strong correlation between the proportion of immigrants and high school performance in Table 3, it is likely that immigrant concentration is correlated with unobservable characteristics of the native students and their schooling environment, given that it is so strongly correlated with observable characteristics of the students and the schools they attended. For this reason, we call the estimates in Table 3 "naïve" in the sense that they are likely to suffer from an omitted variable bias. In the next section, we develop a strategy to overcome this identification problem.

4. OLS Stratification: Identification Strategy and Results

To estimate the causal effect of the presence of new immigrants on their native Israeli peers, we need variation in the presence of immigrant children in the classroom that is uncorrelated with potential outcomes of the native children. It is clear from Table 2 that immigrant concentration is not randomly assigned – native students in poorer and lower achieving schools are more likely to attend schools with larger concentrations of immigrant students. In general, the non-random assignment of children to schools, either because of the choice of schools exercised by the families, or because of the endogenous matching of pupils to schools done by education authorities, makes it difficult to identify the effect of one group of peers on the outcomes of others. As such, the identification of a causal link between peers has proven to be a difficult task in the literature.

To overcome this obstacle, our strategy is to exploit random variation in the number of immigrants in 5th grade conditional on the school absorbing a given range of immigrant students in grades 4 to 6. Initially, we assume that the *actual* number of immigrants in grade 5 is random, conditional on the actual number of immigrants in grades 4 to 6. This allows us to state clearly the assumptions needed for identification and to describe in detail the estimation strategy in the simple case where the placement of immigrants *across* grades *within* a given school can be thought of as random. To account for selective grade placement of immigrants within a school, as well as to address potential attenuation bias due to measurement error in our administrative data, we will later employ a second identification strategy, which uses the number of immigrants that should have attended 5th grade based on their birth dates as an instrument for the actual number of immigrants.

Conditional Random Assignment of the Actual Number of Immigrants

Formally, we define the treatment variable t as the percentage of immigrant students in grade 5 in a given school:

$$t = 100 \times \frac{\text{Number of immigrants in grade 5}}{\text{Number of children in grade 5}}.$$

We have a random sample of units, indexed by $i=1,2,\ldots,N$. For each unit, we postulate the existence of a potential outcome, $Y_i(t)$, for $t \in \mathcal{T} \equiv [0,100)$. We observe for each unit the actual treatment T_i and a set of covariates X_i. The observed outcome for unit i is $y_i^{obs} = \int_{t\in\mathcal{T}} \mathbf{1}(T_i = t) Y_i(t) dt$.

Assumption 1 (Conditional random assignment):

$$Y(t) \text{ independent of } T \mid X, \quad \text{for all } t \in [0,100).$$

Assumption 2 (Constant linear treatment effect):

$$Y_i(t) = Y_i(0) + \beta t.$$

The first assumption is at the heart of our strategy to account for the non-random placement of immigrants across schools. In our case, the critical conditioning covariates are the number of immigrant children in grades 4 to 6, and the total number of children in grade 5. We know that schools that absorbed large numbers of immigrants are different in terms of student and school characteristics relative to schools that absorbed fewer immigrants. However, holding constant the number of immigrant children in grades 4 to 6, the number of immigrants in grade 5 (the numerator in the treatment variable) is determined solely by random variation in the grade distribution among the pool of immigrant children in a school district. Since the treatment variable is the immigrant *ratio*, we must also condition on the total number of children in grade 5. Our strategy is based on the assumption that among schools of the same size that absorbed the same number of immigrant children in grades 4 to

6, variation in the percentage of immigrant students in grade 5 is determined primarily by random variation in the size of each immigrant cohort.

We now consider the expectation of the observed outcome, conditional on the actual treatment received and on the covariates:

$$\begin{aligned} E\left(y^{obs} \mid T = t, X\right) &= E\left[Y_i(0) \mid T = t, X\right] + \beta t \\ &= E\left[Y_i(0) \mid X\right] + \beta t \end{aligned}$$

where the first equality follows from the linearity assumption, and the second equality follows from the conditional random assignment assumption. The above formulation illustrates that, conditional on any particular value of the covariates, we can obtain a consistent estimate of the treatment effect β by running a regression of the observed outcome on a constant and the treatment variable.

In practice, this approach is not feasible, because the two key conditioning covariates are essentially continuous, so that there are not enough observations for any given value of *X*. For example, there are few native students who attended schools with, say, 70 total students in grade 5 and 15 total immigrants in grades 4 to 6. Therefore, we divide the sample into eight different intervals defined by the number of immigrants in grades 4 to 6 in a given school: the first interval includes schools with 1 to 5 immigrants in grades 4 to 6, the second interval includes schools with 6 to 10 immigrants in grades 4 to 6, and so on. Then, within each interval, we regress the outcome variable on the treatment and on a flexible function of the key conditioning variables. This *control* function, as it is known in the literature,[8] is meant to capture the term $E\left[Y(0) \mid X = x\right]$ in equation (1). In practice, the control function will include the number of immigrants in grades 4 to 6, the total number of children in grade 5, the squares of these two variables, the interaction between the two, and, in some specifications, additional individual and school characteristics entered linearly. Note that since the exact functional form of $E\left[Y(0) \mid X = x\right]$ is unknown, estimating equation (1) on the full sample imposes the specific functional form assumption globally, and may be misleading. Instead, by dividing the sample into narrow intervals on the basis of the number of immigrants in grades 4 to 6, we are less likely to induce bias in our estimates by our local approximation to $E[Y(0) \mid X = x]$.

Summing up, within each interval we run the following regression to explain the high school outcome of native Israeli student *i* who attended elementary school *j*:

$$
\begin{aligned}
(\text{High School Outcome})_{ij} = \lambda_0 + \beta\,&(\text{Percent Immigrants in 5th Grade})_j \\
&+ \lambda_1 (\text{Family Background})_i \\
&+ f\,(\text{\# of immigrants in grades 4 to 6, \# of children in grade 5})_j \\
&+ u_{ij},
\end{aligned}
$$

where the treatment parameter, β, is identified by the conditional random assignment assumption described above. Since the percent of immigrants in 5th grade does not vary at the individual level, the standard errors are adjusted for clustering at the elementary school level. The average treatment effect can then be calculated by simply taking the weighted average of the coefficients within each interval. Alternatively, one can pool all intervals together, and then regress the outcome variable on the treatment effect, a dummy variable for each one of the intervals, and the interactions of this dummy with each one of the conditioning variables. This specification is equivalent to running separate regressions within each sub-sample, but constraining the coefficient on the treatment variable to be the same across sub-samples. This allows us to pool together the data from the whole sample and obtain one single estimate of the treatment effect. In the tables that follow, we refer to this model as the "fully-interacted" model.

Balancing Tests

To determine whether the data supports our identification strategy, we now test whether there is a significant relationship between immigrant concentration in grade 5 and the

[8] Heckman and Robb (1985).

observable characteristics of native Israeli children in the various sub-samples of schools defined by the total number of immigrant children in grades 4 to 6. This is done by regressing various measures of the native student's background or elementary school environment on the percent of immigrant students in the school's 5th grade for the eight sub-samples. If the assignment of immigrant students across the 4th, 5th and 6th grades within the same school is indeed random once we condition on the narrow range of total immigrants in the school and on the control function, then we should find no such significant relationship.

This type of balancing test does not necessarily provide a proof of random assignment, as the assumption requires there to be no correlation between the percentage immigrants in 5th grade and both observable and *unobservable* background characteristics. However, the lack of a significant relationship between the percentage of immigrants in 5th grade and observable characteristics suggests that unobservable characteristics are also unlikely to be correlated with the percentage of immigrants in 5th grade. The results of these tests are presented in the first eight columns of Table 4.

Each coefficient in Table 4 represents the estimate from a single regression for each of the various measures of the native student's background and elementary school environment on the percent of immigrants in 5th grade.[9] Out of the 64 coefficients, only six are significant at the ten percent significance level or lower. This is almost exactly the rejection rate that we would observe under the null hypothesis that the covariates are indeed balanced. The other 58 coefficients point to no systematic relationship between observable characteristics and the percent of immigrants in 5th grade. Furthermore, the six coefficients which do turn out to be statistically significant do not tell a consistent story about whether students are positively or negatively selected into schools with larger percentages of immigrants in the 5th grade (holding constant the range of immigrant students in grades 4 to 6). For example, the second column reveals that higher proportions of immigrants in 5th grade are significantly associated with larger families and lower class sizes, which leads to contradicting inferences about negative or positive selection. By contrast, a higher proportion of immigrants is significantly related to smaller families and larger class sizes in the third column. These coefficients not only have contradictory implications for the direction of selection within a given sub-sample, but they also yield contradictory implications for the direction of selection for a given variable across sub-samples. The bottom row of the table shows that, within cells, the standard deviation of percentage immigrants in 5th grade is fairly large, implying that our failure to find significant coefficients is not due to insufficient variation in the explanatory variable.

The next column in the table reports the average of the eight coefficients for each row, weighted by the inverse of the variance of the coefficients,[10] and its associated t-statistic. This column confirms that there is no consistent or significant relationship between the observable characteristics of native students and the percentage of immigrants in 5th grade, once the number of immigrants in grades 4 to 6 is limited to a narrow range. The last column in the table reports the "naïve" OLS coefficient from a simple regression of each variable on the percentage of immigrants in grade 5 in the entire sample. Contrary to the coefficient estimates within each sub-sample, these coefficients are large and statistically significant, and point to a strong negative relationship between immigrant concentration and the background characteristics of natives and schools. The contrast between the "naïve" OLS coefficients and the coefficients within the narrow cells illustrates the extent to which our identification strategy can reduce bias stemming from the non-random selection of immigrants across schools.

[9] The regressions are at the individual student level, with standard errors adjusted for clustering at the school level.
[10] This is essentially a minimum distance estimator, where each one of the eight coefficients is a separate estimate of the common parameter of interest.

The OLS-Stratified Regression Results

Table 5 presents the estimates of the long-run effects of immigrants on the high school achievements of native Israeli students, using regressions within each stratified sub-sample as described above. Each entry in the table represents the coefficient estimate from a separate regression on the treatment variable (the percent of students in 5th grade who are recent immigrants) for the two high school outcomes of interest: the probability of dropping out before completing 12th grade, and the probability of receiving a matriculation diploma at the end of high school. The estimates reported in the upper panel of the table are from regressions that include only the treatment variable and the flexible control function (the number of immigrants in grades 4 to 6 and its square, the number of children in grade 5 and its square, and the interaction between these two variables). The estimates in the bottom panel come from regressions that include additional school-level variables (test scores and socio-economic index of the students in 1991, a dummy for being a religious school, and class size in 1994), as well as personal characteristics of each student (father's and mother's schooling, number of siblings, and ethnic origin). The first eight columns in the table correspond to the eight samples used in Table 4 for the balancing tests.[11]

Most of the estimates in the top panel of Table 5 are small with large estimated standard errors, and not statistically different from zero. When the additional controls are included in the lower panel of the table, the results are similar. The fact that the coefficients do not change very much after adding personal and school-level characteristics into the specification may be interpreted as additional evidence that the treatment and control samples are relatively balanced in terms of these observable measures within each sub-sample.

Although most of the coefficients in each sub-sample are insignificant, the general pattern of the signs of the coefficients points in the direction of an adverse effect of immigrants on the high school outcomes of natives. In the last two columns of Table 5, we present two summary estimates of the treatment effect and their associated t-statistics. The ninth column is simply the weighted average of the eight within-interval treatment effects, and the last column presents the treatment effect obtained from the fully interacted model discussed above. These last two columns indicate that immigrant concentration in elementary school has a marginally significant negative effect on high school matriculation rates, and an insignificant effect on the dropout rate. The point estimates imply that an increase of 10 percentage points in the immigrant concentration in grade 5 lowers the individual matriculation rate by 1.2-1.6 percentage points.

5. The Endogenous Distribution of Immigrants across Schools and Grades

The analysis in the previous section is based on the assumption that the endogenous placement of immigrants in the 5th grade across schools is eliminated once we control for the total number of immigrants in the school from grade 4 to 6. That is, given that a school has a particular number of total immigrants in those three grades, the number that winds up in each grade is as good as random. This assumption is supported by the balancing tests in Table 4, which showed no correlation between the observable characteristics of natives and 5th grade immigrant concentration within each stratified sample. It appears that the stratification corrects for the endogeneity problem across schools, whereby more immigrants are found in poorer areas with more disadvantaged natives. However, given that a school has a particular number of immigrants in grades 4 to 6, there is reason to be concerned that the distribution of immigrants across grades within the same school may not be entirely random. This concern is based on the high rate of holding students back a grade among immigrants.

For example, among the immigrant children who should have attended 5th grade based on their date of birth and the enrollment rules in place for that cohort, 55 percent were in fact

[11] The sample size in each of the columns includes only native Israeli pupils, and therefore, is identical to the sample size reported in the respective column of Table 4.

enrolled in grade 5, while 38 percent are in grade 4.[12] The remainder are scattered between grade 6 (children who have been pushed up a grade, about 3 percent), and grades 1, 2 and 3 (4 percent).[13] This would not be a cause for concern if we could safely assume that a school's propensity to hold back immigrants is uncorrelated with differences in characteristics of native students across grades within the same school. If, however, the school tends to hold immigrant students back so that more of them are placed in the grade with better or worse native students compared to the adjacent grade, then the results in the previous section could be biased. The balancing tests indicate that there is no such endogenous placement based on observable characteristics, but the high rate of holding immigrant students back could be correlated with unobservable characteristics, such as behavioral problems of students in one grade versus the other. If true, then the previous results would be biased towards zero if immigrant students are endogenously held back in order to be with better native students, while an upward bias (in absolute value) would result from being placed with weaker native students. Measurement error in the grade enrollment information of immigrants would also bias the results in the previous section towards zero.[14]

An Instrumental Variable Strategy

To account for the potential endogeneity of immigrant placement within schools, we adapt our identification strategy by predicting how many immigrants are supposed to be in grade 5 within a school based on their date of birth and using this prediction to instrument for the actual immigrant concentration in grade 5 within a school. This strategy assumes that the

[12] Source: authors' calculations from the Ministry of Education data.

[13] We cannot report analogous statistics for native children, as we have no information on natives in grades 4 and 6 in 1993/94. However we can compare the distribution of natives and immigrants who should have attended second grade: 86 percent of natives were indeed enrolled in second grade, compared to 72 percent of immigrants.

[14] Even with the best administrative data, some students' grades may be recorded incorrectly, or there may be movements across grades in the middle of the school year. If measurement error is of the classical type, this would lead to attenuation bias. Moreover, attenuation bias could be exacerbated by controlling for variables such as the number of immigrants in grades 4 to 6, since the standard deviation in this latter variable is probably larger than the standard deviation in the number of grade 5 immigrants; hence, much of the true signal in the explanatory variable would be swept away by the control variable, leaving mostly noise.

predicted percentage of immigrants in grade 5 (i.e., the percentage of immigrant children who should have been in grade 5 based on their dates of birth) is as good as random, conditional on the *predicted* number of immigrants in grades 4 to 6, and on the total *predicted* number of children in grade 5. In other words, conditional on the total number of immigrant children born between December 1981 and November 1984 in a given school, the number of immigrant children born between December 1982 and November 1983 is as good as random. This strategy abstracts from endogenous holding back decisions by parents and schools, and assumes only that the distribution of immigrant children's birth dates is random, conditional on the total number of immigrants in a school.

Formally, we define *Z* as the predicted percentage of immigrant children in grade 5,[15] and we replace Assumption 1 with the following:

Assumption 1' (Conditional random assignment of the instrument):

$$\text{Y(t) independent of Z} \mid \widetilde{X}, \quad \text{for all t} \in [0,100).$$

Note that the conditioning variable is $\widetilde{X}$, the *predicted* number of immigrants in grades 4 to 6. Because of the constant linear treatment effect assumption (Assumption 2), we can write the potential outcome as:

$$Y_i(t) = Y_i(0) + \beta t = \mu_0 + \beta t + v_{i0},$$

where μ_0 is the mean of *Y(0)* in the population, and v_{i0} is its stochastic component. Now, the observed outcome can be written as:

$$\begin{aligned} y_i^{obs} &= \mu_0 + \beta t + v_{i0} = \mu_0 + \beta t + E\left(v_{io} \mid \widetilde{X}\right) + \left[v_{i0} - E\left(v_{i0} \mid \widetilde{X}\right)\right] \\ &= \mu_0 + \beta t + E\left(v_{i0} \mid \widetilde{X}\right) + \xi_{io}. \end{aligned}$$

[15] Since we do not have data on native children who attend grades 4 and 6, the *predicted* percentage of immigrants is calculated as:

$$z = 100 \times \frac{\text{Predicted number of immigrants in grade 5}}{\text{Predicted no. of immigrants in grade 5} + \text{Actual no. of natives in grade 5}}.$$

Similarly, the *predicted* number of children in grade 5 is calculated as the sum of the *predicted* number of immigrants plus the *actual* number of natives in grade 5.

The error term in this equation, ξ_{i0}, is potentially correlated with the treatment variable. However, because Z is randomly assigned conditional on $\widetilde{X}$, it implies that $E\left(v_0 \mid Z, \widetilde{X}\right) = E\left(v_0 \mid \widetilde{X}\right)$. Hence, Z is uncorrelated with ξ_{i0} and can be used as an instrument in estimating the above equation, for a given value of $\widetilde{X}$. In practice, we will stratify the sample by the *predicted* number of immigrants in grades 4 to 6, and estimate the equation by IV within each narrow cell, always controlling for a flexible function of the key conditioning variables.[16]

<u>Balancing Tests for the Instrument</u>

Table 6 presents tests for whether our instrument, the predicted percentage of immigrants in grade 5, is balanced with respect to the background characteristics of native students and schools. The table is analogous in structure to Table 4 and it shows that the predicted percentage of immigrants is not systematically correlated with any of the observable characteristics of natives or their schools. As in Table 4, the most striking result is the sharp contrast between the insignificant coefficients for the row averages (the second to last column) and the highly significant coefficients from the naïve OLS estimation (the last column). Overall, Table 6 shows that once we control for the predicted number of immigrants in grade 4 to 6 and the predicted number of children in grade 5, the predicted percentage immigrants in grade 5 is not correlated with any of the observable school or peer characteristics.

[16] The IV estimation strategy is not directly comparable to the stratified OLS strategy for two reasons: first, the conditioning variables in the IV strategy are the *predicted* number of immigrants in grades 4 to 6 and the *predicted* number of children in grade 5, as opposed to the *actual* numbers of the OLS strategy. Second, we limit ourselves to schools where the *predicted* number of immigrants in grades 4 to 6 is between 1 and 40. This results in a slightly different sample of schools relative to the OLS strategy, where we included all schools where the *actual* number of immigrants was between 1 and 40. An alternative is to use exactly the same sample and stratification as in the OLS strategy, treat the key conditioning variables also as endogenous, and instrument for them using their predicted values. The results are very similar to the ones reported below, and are available from the authors upon request.

Instrumental Variable Results

The upper panel in Table 7 reports the IV estimates from regressions with no individual and school controls while the lower panel includes these controls. In general, relative to the stratified OLS results, the IV estimates point to a larger adverse effect of immigration on native students' outcomes. The individual cell coefficients are mostly insignificant, but they tend to point to an adverse effect of immigration on both matriculation rates (as in Table 5) and on dropout rates (in contrast to Table 5). The weighted average of the cell coefficients and the interacted model coefficients are similar to one another, and they both suggest that immigrants have a significant adverse effect on the dropout rate and the matriculation rate of their native peers. Furthermore, the size of the estimated effects is not negligible**.** The specification with all the control variables included (bottom panel) indicates that a 10 point increase in the percentage immigrants in 5th grade raises the dropout rate of native students by 1.4 percentage points and lowers the individual matriculation rate by 2.7-3.2 percentage points. For comparison purposes, one additional year of father's education raises matriculation rates by 1.5-1.9 percentage points; a one standard deviation change in the elementary school's socio-economic index raises matriculation rates by 3.4 percentage points; and a one standard deviation change in 1991 verbal test scores raises matriculation rates by 4.0 percentage points.

The finding that the IV results are larger and more significant than OLS suggests that the latter estimates may have been biased towards zero because of the endogenous holding-back of immigrants within a school and/or because of measurement error in the treatment variable. In particular, the IV results suggest that schools tend to place more immigrants in the grade that has stronger native students, thus causing OLS to be biased towards zero.[17]

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[17] In the previous version of this paper (Gould, Lavy, and Paserman (2004b)), we reported weak effects of immigrants on native outcomes using the OLS stratification, but stronger adverse effects on disadvantaged native students. The previous version did not use the IV strategy described above. Using the IV strategy, we find much stronger effect on the whole population as shown in this section, but we did not find differential effects on native students according to their socio-economic background.

6. Robustness Checks

We explore the robustness of our results to alternative specifications in Table 8. For ease of exposition, we present only the coefficient on the treatment variable in the fully interacted model, for both the stratified OLS and stratified IV specifications; the weighted average estimates are similar and are available upon request. The first two columns use a finer stratification based on 10 sub-samples, two more than the previous set of results. The results are essentially unchanged in magnitude and significance for both outcome variables, and for both the OLS and IV specifications. The second two columns stratify the samples along two dimensions: the total number of immigrants in grades 4 to 6 (4 intervals) and the total number of children in grade 5 (3 intervals), for a total of 12 different cells. The stratification along two dimensions aims to approximate more accurately the control function, but it necessitates a coarser stratification along the dimension of the number of immigrants in grades 4 to 6, meaning that within the cells there is greater variance in the propensity of schools to absorb immigrants. With this stratification, the coefficients tend to be slightly larger in magnitude and significance for the dropout rate, and significantly larger for the matriculation rate.

Finally, the last set of estimates uses the *number* of immigrants in grade 5 as the explanatory variable, rather than the percentage. The results are similar to those using the percentage immigrants, although the implied effects are a bit larger – six additional immigrants in grade 5[18] lower the matriculation rate by up to 1.8 percentage points according to the OLS specification, and by 5.8 percentage points according to the IV specification. Overall, the robustness checks confirm the previous findings: once we account for the selectivity of immigrants both across schools and between grades within a given school, we find that native outcomes are negatively affected by the presence of immigrants.

As an additional test of our identification strategy, we run a series of "placebo" regressions, where the treatment variable is the percentage of immigrants in grades 1 and 2 in

[18] Roughly a 10 percent increase in the percentage immigrants; see Table 4.

the 1993/1994 school year, rather than the percentage of immigrants in grade 5. If the results in Tables 5 and 7 were due to unobserved school characteristics, we would expect that the long-term outcomes of fifth graders would be equally affected by the percentage of immigrants in the earlier grades as by the percentage of immigrants in their own grade. This scenario is not supported by the results in Table 9: the percentage of immigrants in either first or second grade has no effect on the high school outcomes of fifth grade children, in either the stratified OLS or the IV specification. We interpret this as evidence that our results are not spuriously driven by unobserved school characteristics.

7. Other Results

Gender Specific Effects

In Table 10, we investigate whether immigrant boys and immigrant girls have a differential effect on the outcomes of native boys and girls. We estimate the fully interacted model as in Tables 5 and 7, but we now focus on two right hand side variables: the percentage of immigrant boys in 5th grade (i.e., 100 times the number of immigrant boys divided by the total number of children in the grade), and the percentage of immigrant girls in 5th grade. Interestingly, we find that the matriculation rate of native boys is affected exclusively by the percentage of immigrant boys, while it is not affected by the presence of immigrant girls. This result holds in both the stratified OLS and stratified IV specifications. On the other hand, the male dropout rate is not significantly affected by the percentage of immigrants of either gender. For girls, the results are somewhat mixed: the OLS specification indicates that native girls are negatively affected primarily by the percentage of immigrant boys in 5th grade in both dropout rates (p-value of 0.103) and matriculation rates; the IV results suggest that the dropout rate of native girls is roughly equally affected by the presence of immigrant girls or boys (although neither coefficient is statistically significant), while the matriculation rate is affected primarily by the presence of immigrant girls in 5th grade. If the main mechanism explaining the adverse effect of immigration on native outcomes is peer influence, and if this influence operates primarily within gender, we would have expected that natives would have been affected mostly by the presence of immigrants of their same gender. We find some evidence for this hypothesis, especially for boys, but the mixed results for girls suggest that other factors, such as the crowding out of limited school resources, could also be important.

Decomposing the Effect of Immigrants on the Matriculation Rate of Natives

The results show that the presence of a significant proportion of immigrants during primary school has an adverse effect on the dropout rate and matriculation rate of natives. However, these outcome variables are clearly related to one another – it is not possible to pass the matriculation exams if one has dropped out of high school. Therefore, it is natural to ask how much of the adverse effect of immigration on matriculation rates is due to higher dropout rates, and how much of the effect is due to lower matriculation rates conditional on not dropping-out. Table 11 decomposes the overall effect into these two components.

In particular, we decompose the effect of increasing the immigrant percentage in 5th grade from zero to 10 percent. Using the stratified IV estimate from Table 7, the overall effect of this increase is to reduce the probability of passing the matriculation exams by 3.16 percentage points. Over 80 percent of this reduction (2.88 percentage points, to be precise) is due to the lower probability of passing the matriculation exams conditional on not dropping-out, while the rest (0.99 percentage points) is due to the higher dropout rate. Therefore, most of the effect of immigration on matriculation rates seems to be due to the lower rate of passing the matriculation exam, rather than the lower rate of staying in school long enough to take the matriculation exam.

8. Conclusion

This paper represents one of the first attempts to study the consequences of natives and immigrants interacting in the same social and learning environment, while paying particular attention to issues of identification and causality. In addition, this paper breaks from the existing literature on peer effects within schools by looking at the long-term effects on natives in high school, rather than focusing on contemporaneous effects. After accounting for the endogenous placement of immigrants, both across schools and between grades within schools, we find that the percentage of immigrants in 5^{th} grade has a substantial adverse effect on the dropout rate and high school achievements (matriculation rate) of native Israelis. These results stand in contrast with some of the recent estimates in the U.S. literature on desegregation, which found little or no impact of desegregation programs on the outcomes of white students (Angrist and Lang, 2004; Guryan 2004). One possible explanation for the larger estimated effects in our study is that the average number of immigrant children in each class in our sample is particularly large. Also, given that average class size in Israel is also relatively high, it is possible that the migration wave crowded out school resources that were already strained.

In many respects, these findings can be of general interest beyond the local Israeli context. The empirical episode studied here examines how immigrants from a relatively educated and skilled background from the former Soviet Union affect the human capital outcomes of natives. Despite the high levels of parental education, our analysis shows that the parents of these immigrants exhibit many troubling socio-economic patterns which could lead to a crowding out of school resources or generate other forms of negative peer effects on the outcomes of natives. The lessons learned from this analysis are becoming increasingly relevant to Western countries that are expected to absorb growing numbers of immigrants from Central and Eastern Europe (especially following the enlargement of the European

Union), and others who are debating whether to move toward a more skill-biased immigration policy.[19]

Our findings may also have implications for income inequality. The most important dividing line in schooling in the US is high school completion. In Israel and in many European countries, the crucial threshold is passing the matriculation exams, which opens the door to a college education. Angrist and Lavy (2004) estimated the returns to a year of schooling in Israel at about 8.3 percent, while holders of the Israeli matriculation certificate earn a further 24 percent. The effect of immigrants on the matriculation outcomes of natives may therefore have severe implications for the income of disadvantaged natives as well as for income inequality.

[19] We should also keep in mind that the number of highly skilled immigrants in the West is already non-negligible. In the United States in the year 2000, the fraction of recent immigrants with a college degree is slightly larger than that of natives (22.0 percent versus 20.9 percent), and in some states it is substantially larger. In Ohio, the fraction of immigrants with a college degree is 37.4 percent, versus 17.6 percent among natives; in Michigan, it is 33.2 percent among immigrants versus 17.2 among natives; in Pennsylvania, it is 33.1 percent among immigrants versus 19.0 among natives. (Source: 2000 Census of Population).

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Table 1: Background Characteristics of Immigrants and Natives

A: Ministry of Education data:

	All Immigrants	Immigrants from:			Native Israelis
		Former Soviet Union	Ethiopia	Other	
Father's years of schooling	11.45 (5.49)	13.04 (3.62)	1.80 (3.30)	14.21 (4.46)	12.03 (3.56)
Mother's years of schooling	11.58 (5.31)	13.25 (3.26)	1.39 (2.90)	13.72 (3.70)	12.06 (3.41)
Number of siblings	1.54 (1.73)	1.11 (1.16)	4.28 (2.61)	1.98 (1.35)	2.41 (1.57)
Number of children	7,159	5,477	945	737	54,700

B: Census 1995 data

	All Immigrants	Immigrants from:			Native Israelis
		Former Soviet Union	Ethiopia	Other	
Father's years of schooling	12.69 (4.50)	13.51 (3.20)	2.57 (4.61)	13.50 (4.32)	11.67 (3.83)
Mother's years of schooling	12.53 (4.52)	13.45 (3.09)	1.92 (4.20)	13.40 (3.95)	11.59 (3.98)
Female headed household	0.176 (0.381)	0.179 (0.383)	0.248 (0.432)	0.093 (0.290)	0.085 (0.278)
Home ownership	0.533 (0.499)	0.561 (0.496)	0.482 (0.500)	0.338 (0.473)	0.693 (0.461)
Household head unemployed	0.071 (0.256)	0.067 (0.252)	0.096 (0.295)	0.086 (0.281)	0.037 (0.189)
Household monthly income	3895.85 (3911.46)	4258.76 (3738.91)	751.01 (1184.43)	3497.91 (5371.44)	5899.36 (6963.88)
Number of households	8,910	7,321	737	852	68,725

Notes: The Ministry of Education sample includes the universe of children enrolled in 5th grade during the 1993-1994 school year. The Census data includes all households with at least one child between the ages of 8 and 12 in 1995.

Table 2: Summary Statistics

	Number of immigrants in grades 4 through 6				
	1-10	11-20	21-30	31-40	All (1-40)
Number of schools	271	244	164	124	803
Total number of children in grade 5	12,593	13,175	9,573	7,005	42,346
Average number of children in grade 5	50.49	62.02	69.96	71.44	61.21
Average percentage immigrants in grade 5	5.39	10.22	13.97	19.48	10.79
Peer characteristics (non-immigrants)					
Father's years of schooling	12.62 (3.66)	12.24 (3.53)	11.97 (3.32)	11.91 (3.34)	12.24 (3.50)
Mother's years of schooling	12.65 (3.47)	12.23 (3.37)	12.04 (3.13)	11.83 (3.29)	12.25 (3.35)
Number of siblings	2.43 (1.69)	2.45 (1.54)	2.37 (1.52)	2.31 (1.42)	2.40 (1.57)
Fraction Asia-Africa	0.240 (0.427)	0.290 (0.454)	0.317 (0.465)	0.312 (0.463)	0.285 (0.451)
Class Size	30.08 (6.04)	31.09 (5.74)	32.49 (5.16)	33.04 (5.36)	31.43 (5.74)
School characteristics					
Standardized Math Score in 1991	0.347 (0.772)	0.164 (0.837)	0.115 (0.802)	0.102 (0.814)	0.197 (0.813)
Standardized Verbal Score in 1991	0.410 (0.769)	0.144 (0.799)	0.023 (0.739)	0.011 (0.808)	0.174 (0.795)
Socioeconomic Index in 1991	0.397 (0.749)	0.194 (0.817)	0.155 (0.811)	0.129 (0.814)	0.235 (0.803)
Outcome Variables					
Fraction dropping out before completing 12th grade	0.047 (0.211)	0.055 (0.229)	0.059 (0.236)	0.058 (0.233)	0.054 (0.226)
Fraction eligible for high school matriculation	0.642 (0.480)	0.611 (0.488)	0.605 (0.489)	0.584 (0.493)	0.614 (0.487)

Notes: Authors' calculations based on Ministry of Education data

Table 3: Naïve OLS estimates of the effect of immigration on natives' long-term educational outcomes

	(1)	(2)	(3)
Dependent variable:			
Dropped out before completing 12^{th} grade	0.0013 [6.07]	0.0004 [1.71]	0.0003 [0.95]
Passed HS matriculation exam	-0.0057 [-8.75]	-0.0013 [-2.68]	-0.0014 [-2.03]
Controls for individual and school characteristics	No	Yes	Yes
Controls for number of immigrants in grades 4 to 6 and number of children in grade 5	No	No	Yes

Note: Entries in the table represent the coefficients from separate regressions of the relevant dependent variable on the percentage immigrants in 5^{th} grade. Individual controls: mother's years of schooling, father's years of schooling, dummy indicators for whether parents' schooling is missing, number of siblings, a dummy for whether the number of siblings is missing, age in 1994, a gender dummy. School controls: a dummy for whether the elementary school is religious, class size in 1994, math scores in 1991, verbal scores in 1991, and school socioeconomic index in 1991. Robust t-statistics (adjusted for clustering at the school level) in brackets.

Table 4: Balancing Tests for the Actual Percentage of Immigrants

	Number of immigrants in grades 4-6									
	1-5	**6-10**	**11-15**	**16-20**	**21-25**	**26-30**	**31-35**	**36-40**	**Row average**	**Naïve OLS**
Dependent Variable:										
Mother's years of schooling	0.0625	0.0054	0.0118	0.0115	0.0293	-0.0244	-0.0299	-0.0357	-0.0001	-0.0526
	[1.34]	[0.16]	[0.42]	[0.36]	[1.13]	[-1.05]	[-0.92]	[-1.06]	[-0.01]	[-7.79]
Father's years of schooling	0.0640	0.0085	0.0193	0.0040	0.0276	-0.0364	-0.0222	-0.0587	-0.0026	-0.0540
	[1.27]	[0.24]	[0.70]	[0.13]	[1.08]	[-1.50]	[-0.60]	[-1.66]	[-0.24]	[-7.81]
Number of Siblings	-0.0057	0.0374	-0.0314	0.0197	-0.0130	0.0224	0.0133	0.0050	0.0066	0.0125
	[-0.18]	[2.17]	[-2.08]	[1.22]	[-0.66]	[1.47]	[0.59]	[0.28]	[1.04]	[3.01]
Fraction Asia-Africa	-0.0014	-0.0001	-0.0028	0.0034	-0.0001	0.0021	0.0001	-0.0075	-0.0006	0.0053
	[-0.33]	[-0.04]	[-0.92]	[1.05]	[-0.02]	[0.68]	[0.03]	[-2.15]	[-0.54]	[7.65]
Standardized math score in 1991	0.0077	-0.0147	-0.0217	-0.0151	0.0117	-0.0024	-0.0180	0.0069	-0.0064	-0.0317
	[0.37]	[-0.74]	[-1.14]	[-1.08]	[0.68]	[-0.13]	[-0.96]	[0.36]	[-1.00]	[-7.93]
Standardized verbal score in 1991	0.0133	-0.0095	0.0048	-0.0196	0.0037	-0.0034	-0.0168	0.0127	-0.0042	-0.0348
	[0.60]	[-0.51]	[0.24]	[-1.45]	[0.22]	[-0.21]	[-0.95]	[0.70]	[-0.68]	[-9.03]
Socioeconomic index in 1991	0.0012	-0.0200	0.0157	-0.0219	-0.0187	-0.0181	0.0003	0.0162	-0.0068	-0.0379
	[0.05]	[-1.04]	[0.88]	[-1.54]	[-0.99]	[-0.93]	[0.02]	[0.84]	[-1.06]	[-10.14]
Class size in 1994	-0.1709	-0.2134	0.2698	0.0244	0.0838	0.0778	0.0186	0.0689	0.0222	-0.0657
	[-1.33]	[-2.37]	[2.70]	[0.25]	[0.83]	[0.75]	[0.23]	[0.73]	[0.64]	[-2.48]
Number of schools	123	148	153	91	99	65	72	52	803	
Number of children	4,872	7,721	8,053	5,122	5,610	3,963	3,959	3,046	42,346	
Average number of children in 5^{th} grade	42.20	57.38	59.46	66.34	67.85	73.17	69.50	74.13	61.20	
Average number of immigrants in 5^{th} grade	1.04	3.06	4.39	6.56	7.59	9.65	11.14	12.40	5.82	
Average Percentage Immigrants in 5^{th} grade	3.48	6.97	9.05	12.19	13.35	14.93	19.04	20.09	10.79	
Standard deviation of % Immigrants in 5^{th} grade	4.73	5.84	5.98	9.70	7.89	7.75	9.88	11.18	9.03	

Notes: Entries in the table represent the coefficients from separate regressions of the relevant dependent variable on the percentage immigrants in 5^{th} grade. All regressions control for number of immigrants in grades 4 to 6, number of children in grade 5, their squares, and the interaction between the two. Robust t-statistics, adjusted for clustering at the school level, in brackets.

Table 5: Stratified OLS Regressions for the Outcomes for Native Israelis – Accounting for Endogenous Sorting Across Schools

Treatment variable: Percentage immigrants in grade 5

Estimation method: Stratification based on number of immigrants in grades 4 to 6, and OLS within cells

	Number of immigrants in grades 4 to 6:								**Weighted**	**Interacted**
	1-5	**6-10**	**11-15**	**16-20**	**21-25**	**26-30**	**31-35**	**36-40**	**Average**	**Model**
A: No Individual Controls										
Dropped out before completing 12th grade	-0.0022 [-1.23]	0.0012 [1.33]	-0.0003 [-0.29]	0.0013 [1.84]	0.0003 [0.31]	0.0009 [0.98]	0.0016 [1.66]	-0.0017 [-1.25]	**0.0006** **[1.84]**	**0.0003** **[0.88]**
Passed HS matriculation exam	0.0031 [0.66]	-0.0016 [-0.49]	-0.0024 [-0.80]	-0.0018 [-0.66]	0.0006 [0.23]	-0.0039 [-1.31]	-0.0063 [-1.97]	0.0020 [0.63]	**-0.0015** **[-1.37]**	**-0.0016** **[-1.46]**
B: With Individual Controls										
Dropped out before completing 12th grade	-0.0023 [-1.45]	0.0011 [1.37]	0.0003 [0.31]	0.0010 [1.40]	0.0003 [0.37]	0.0003 [0.43]	0.0008 [0.91]	-0.0012 [-0.96]	**0.0004** **[1.35]**	**0.0003** **[0.77]**
Passed HS matriculation exam	0.0002 [0.04]	-0.0016 [-0.64]	-0.0040 [-1.60]	-0.0007 [-0.37]	-0.0012 [-0.70]	-0.0019 [-0.77]	-0.0033 [-1.89]	0.0015 [0.76]	**-0.0014** **[-1.88]**	**-0.0015** **[-1.88]**
Number of schools	123	148	153	91	99	65	72	52	803	
Number of children	4,872	7,721	8,053	5,122	5,610	3,963	3,959	3,046	42,346	

Notes: Entries in the first eight columns represent the estimated effect of percentage immigrants in 5th grade on the relevant dependent variable, within narrow cells defined by the actual number of immigrants in grades 4 to 6. The last two columns represent summary measures of the estimated effect (see text for details). All regressions control for the number of children in grade 5, the number of immigrants in grades 4 to 6, their squares, and the interaction between the two. Individual controls are mother's years of schooling, father's years of schooling, dummy indicators for whether parents' schooling is missing, number of siblings, a dummy for whether the number of siblings is missing, age in 1994, a gender dummy, a dummy for whether the elementary school is religious, class size in 1994, math scores in 1991, verbal scores in 1991, and school socioeconomic index in 1991. Robust t-statistics (adjusted for clustering at the school level) in brackets.

Table 6: Balancing Tests for the Predicted Percentage of Immigrants

	Predicted number of immigrants in grades 4 to 6									
	1-5	**6-10**	**11-15**	**16-20**	**21-25**	**26-30**	**31-35**	**36-40**	**Row average**	**Naïve OLS**
Mother's years of schooling	-0.0245 [-0.54]	0.0101 [0.24]	0.0270 [0.90]	0.0236 [0.65]	-0.0066 [-0.23]	-0.0052 [-0.20]	0.0107 [0.40]	-0.0835 [-1.71]	0.0008 [0.07]	-0.0559 [-8.45]
Father's years of schooling	-0.0066 [-0.13]	0.0209 [0.45]	0.0155 [0.51]	0.0347 [0.96]	-0.0097 [-0.35]	-0.0037 [-0.14]	0.0346 [1.29]	-0.1025 [-2.10]	0.0052 [0.44]	-0.0553 [-8.17]
Number of Siblings	0.0746 [2.64]	0.0291 [1.47]	-0.0156 [-0.71]	0.0149 [0.95]	-0.0023 [-0.10]	-0.0034 [-0.22]	0.0287 [1.55]	-0.0070 [-0.36]	0.0112 [1.65]	0.0109 [2.82]
Fraction Asia-Africa	0.0112 [2.41]	0.0012 [0.33]	-0.0013 [-0.35]	-0.0016 [-0.44]	-0.0044 [-1.14]	0.0007 [0.22]	0.0001 [0.04]	0.0024 [0.52]	0.0005 [0.39]	0.0057 [8.04]
Standardized math score in 1991	0.0093 [0.38]	-0.0059 [-0.26]	-0.0287 [-1.69]	-0.0084 [-0.55]	0.0007 [0.04]	-0.0005 [-0.03]	0.0000 [0.00]	-0.0563 [-2.65]	-0.0115 [-1.72]	-0.0318 [-7.81]
Standardized verbal score in 1991	-0.0059 [-0.25]	-0.0115 [-0.49]	-0.0113 [-0.64]	-0.0086 [-0.64]	0.0040 [0.19]	0.0034 [0.19]	-0.0047 [-0.30]	-0.0518 [-2.52]	-0.0098 [-1.53]	-0.0354 [-9.28]
Socioeconomic index in 1991	-0.0419 [-1.61]	-0.0181 [-0.84]	-0.0125 [-0.73]	-0.0061 [-0.33]	-0.0125 [-0.56]	-0.0117 [-0.59]	0.0043 [0.27]	-0.0031 [-0.13]	-0.0102 [-1.44]	-0.0379 [-10.28]
Class size in 1994	0.0052 [0.04]	-0.1488 [-1.34]	0.0140 [0.12]	0.1972 [2.21]	-0.1415 [-1.45]	-0.0438 [-0.52]	-0.1894 [-1.65]	-0.1002 [-0.86]	-0.0395 [-1.07]	-0.0868 [-3.34]
Number of schools	113	158	136	93	89	86	64	61	800	
Number of children	4,461	8,209	7,170	5,058	5,186	4,701	3,859	3,687	42,331	
Average number of children in 5^{th} grade	42.17	57.12	59.58	63.77	68.54	67.15	74.84	75.39	61.36	
Average number of immigrants in 5^{th} grade	1.09	3.01	4.50	6.06	7.13	9.08	11.17	11.97	5.80	
Average Percentage Immigrants in 5^{th} grade	3.64	6.96	9.13	11.69	12.67	15.41	18.14	17.79	10.67	
Standard deviation of % Immigrants in 5^{th} grade	5.03	6.10	6.59	10.00	8.32	7.72	11.15	8.35	8.95	

Notes: Entries in the table represent the coefficients from separate regressions of the relevant dependent variable on the predicted percentage immigrants in 5^{th} grade. The predicted percentage is the percentage of immigrants that should have been in 5^{th} grade based on their dates of birth. All regressions control for predicted number of immigrants in grades 4 to 6, predicted number of children in grade 5, their squares, and their interaction. Robust t-statistics, adjusted for clustering at the school level, in brackets.

Table 7: Stratified IV Regressions for Outcomes for Native Israelis – Accounting for Endogeneity in Grade Placement

Treatment variable: Percentage immigrants in grade 5

Estimation method: Stratification based on number of immigrants in grades 4 to 6, and IV within cells

(instrument: predicted percentage immigrants in grade 5)

	Predicted number of immigrants in grades 4 to 6:								**Weighted**	**Interacted**
	1-5	**6-10**	**11-15**	**16-20**	**21-25**	**26-30**	**31-35**	**36-40**	**Average**	**Model**
A: No individual or school controls										
Dropped out before completing 12th grade	0.0035 [1.37]	0.0012 [0.63]	0.0017 [1.15]	0.0021 [1.92]	0.0001 [0.03]	0.0023 [1.08]	0.0013 [0.68]	0.0013 [0.72]	**0.0017** **[2.84]**	**0.0016** **[2.59]**
Passed HS matriculation exam	0.0065 [0.78]	-0.0049 [-0.77]	-0.0028 [-0.55]	0.0010 [0.29]	-0.0098 [-1.27]	0.0005 [0.08]	-0.0001 [-0.02]	-0.0156 [-2.69]	**-0.0024** **[-1.27]**	**-0.0032** **[-1.62]**
B: With individual and school controls										
Dropped out before completing 12th grade	0.0028 [1.18]	0.0012 [0.67]	0.0021 [1.48]	0.0022 [1.72]	-0.0006 [-0.27]	0.0027 [1.62]	0.0010 [0.57]	-0.0014 [-0.66]	**0.0015** **[2.51]**	**0.0014** **[2.23]**
Passed HS matriculation exam	0.0078 [1.10]	-0.0056 [-1.26]	-0.0041 [-1.17]	-0.0014 [-0.51]	-0.0106 [-1.89]	-0.0001 [-0.03]	-0.0007 [-0.19]	-0.0074 [-1.66]	**-0.0028** **[-2.00]**	**-0.0032** **[-2.12]**
Number of schools	113	158	136	93	89	86	64	61	800	
Number of children	4461	8209	7170	5058	5186	4701	3859	3687	42,331	

Notes: Entries in the first eight columns represent the estimated effect of percentage immigrants in 5th grade on the relevant dependent variable, within narrow cells defined by the predicted number of immigrants in grades 4 to 6. The last two columns represent summary measures of the estimated effect (see text for details). All regressions control for the predicted number of children in grade 5, the predicted number of immigrants in grades 4 to 6, their squares, and the interaction between the two. Individual controls are mother's years of schooling, father's years of schooling, dummy indicators for whether parents' schooling is missing, number of siblings, a dummy for whether the number of siblings is missing, age in 1994, a gender dummy, a dummy for whether the elementary school is religious, class size in 1994, math scores in 1991, verbal scores in 1991, and school socioeconomic index in 1991. Robust t-statistics (adjusted for clustering at the school level) in brackets.

Table 8: Robustness Tests

	(1) Finer Stratification		(2) Two-dimensional stratification		(3) Number of immigrants	
	Treatment variable: Percentage immigrants in grade 5 Stratification: Actual/Predicted number of immigrants in grades 4 to 6 (10 cells) Instrument: Predicted percentage immigrants in grade 5 (IV only)		Treatment variable: Percentage immigrants in grade 5 Stratification: Actual/Predicted number of immigrants in grades 4 to 6 **and** actual/predicted number of children in grade 5 (12 cells) Instrument: Predicted percentage immigrants in grade 5 (IV only)		Treatment variable: Number of immigrants in grade 5. Stratification: Actual/predicted number of immigrants in grades 4 to 6 (8 cells) Instrument: Predicted number of immigrants in grade 5 (IV only)	
	Stratified OLS	**Stratified IV**	**Stratified OLS**	**Stratified IV**	**Stratified OLS**	**Stratified IV**
Dependent variable:						
Dropped out before completing 12^{th} grade	0.0002 [0.57]	0.0014 [2.50]	0.0002 [0.54]	0.0018 [2.60]	0.0009 [1.40]	0.0022 [1.65]
Passed HS matriculation exam	-0.0014 [-1.77]	-0.0028 [-1.97]	-0.0023 [-2.67]	-0.0052 [-2.93]	-0.0032 [-2.37]	-0.0098 [-2.94]

Note: Entries in the table represent the estimated effect of the treatment variable on the relevant dependent variable from separate models. The effect is calculated based on the fully interacted model. All regressions control for predicted number of children in grade5, the predicted number of immigrants in grades 4 to 6, their squares, and the interaction between the two. Individual controls are mother's years of schooling, father's years of schooling, dummy indicators for whether parents' schooling is missing, number of siblings, a dummy for whether the number of siblings is missing, age in 1994, a gender dummy, a dummy for whether the elementary school is religious, class size in 1994, math scores in 1991, verbal scores in 1991, and school socioeconomic index in 1991. Robust t-statistics (adjusted for clustering at the school level) in brackets.

Table 9: Placebo Regressions

	Stratified OLS – Fully interacted model Estimation method: Stratification based on number of immigrants in grades 4 to 6, and OLS within cells			**Stratified IV – Fully interacted Model** Estimation method: Stratification based on number of immigrants in grades 4 to 6, and IV within cells (instrument: predicted percentage immigrants in grade 5)		
	Treatment variable:			Treatment variable:		
	True effect: % immigrants in 5^{th} grade	"Placebo": % immigrants in 1^{st} grade	Placebo: % immigrants in 2^{nd} grade	True effect: % immigrants in 5^{th} grade	"Placebo": % immigrants in 1^{st} grade	Placebo: % immigrants in 2^{nd} grade
Dropped out before completing 12^{th} grade	0.0003 [0.77]	-0.0000 [-0.27]	-0.0000 [-0.74]	0.0014 [2.23]	-0.0000 [-0.35]	-0.0001 [-1.50]
Passed HS matriculation exam	-0.0015 [-1.88]	0.0000 [0.27]	0.0001 [0.92]	-0.0032 [-2.12]	0.0001 [0.65]	0.0002 [1.20]

Notes: Entries in the table represent the coefficients on the treatment variable in the fully interacted model (see text for details). All regressions control for the predicted number of children in grade 5, the predicted number of immigrants in grades 4 to 6, their squares, and the interaction between the two. Individual controls are mother's years of schooling, father's years of schooling, dummy indicators for whether parents' schooling is missing, number of siblings, a dummy for whether the number of siblings is missing, age in 1994, a gender dummy, a dummy for whether the elementary school is religious, class size in 1994, math scores in 1991, verbal scores in 1991, and school socioeconomic index in 1991. Robust t-statistics (adjusted for clustering at the school level) in brackets.

Table 10: Gender Specific Effects

		Males		Females	
		Stratified OLS	**Stratified IV**	**Stratified OLS**	**Stratified IV**
	Treatment variable:				
Dependent variable: Dropped out before completing 12th grade	Percentage Immigrant Males	-0.0003 [-0.37]	0.0017 [1.23]	0.0009 [1.63]	0.0015 [1.50]
	Percentage Immigrant Females	-0.0002 [-0.25]	0.0002 [0.16]	0.0006 [0.98]	0.0011 [1.26]
Dependent variable: Passed HS matriculation exam	Percentage Immigrant Males	-0.0030 [-2.01]	-0.0057 [-2.05]	-0.0032 [-2.15]	-0.0026 [-0.93]
	Percentage Immigrant Females	0.0011 [0.82]	-0.0009 [-0.37]	-0.0006 [-0.40]	-0.0039 [-1.76]
Number of schools		779	777	776	775
Number of children		20,766	20,790	20,867	20,842

Notes: Entries in the table represent the coefficients on the treatment variable in the fully interacted model (see text for details). All regressions control for the predicted number of children in grade 5, the predicted number of immigrants in grades 4 to 6, their squares, and the interaction between the two. Individual controls are mother's years of schooling, father's years of schooling, dummy indicators for whether parents' schooling is missing, number of siblings, a dummy for whether the number of siblings is missing, age in 1994, a gender dummy, a dummy for whether the elementary school is religious, class size in 1994, math scores in 1991, verbal scores in 1991, and school socioeconomic index in 1991. Robust t-statistics (adjusted for clustering at the school level) in brackets.

Table 11: Decomposing the IV Effect of Immigrants on the Matriculation Rate of Natives

(1)	(2)	Difference between Columns (1) and (2)	
Prob (HS matriculation | pct. Immigrants = 0%)	Prob (HS matriculation | pct. Immigrants = 10%)		
0.5766	0.5451	0.0316	
Prob (HS matriculation | not dropping out, pct. Immigrants = 0%)	Prob (HS matriculation | not dropping out, pct. Immigrants = 10%)		
0.6185	0.5897	0.0288	% of total diff. explained by diff. in conditional matriculation rate: **81.6%**
Prob (not dropping out | pct. Immigrants = 0%)	Prob (not dropping out | pct. Immigrants = 10%)		
0.9276	0.9177	0.0099	% of total diff. explained by diff. in probability of not dropping out: **18.4%**

Notes: The conditional probabilities are calculated for a male native student who attended a non-religious elementary school with 16 immigrants in grades 4 to 6 and 73 5th grade children altogether, whose fifth grade class had 32 children, whose parents were born in Israel and both completed 12 years of education, who has two siblings, who was 11 years old when he attended 5th grade, and whose elementary school had the national average in terms of 1991 socioeconomic status, math test scores and reading test scores.