Immigrants' peer effects in vocational schools[1]

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Abstract

This paper provides new evidence on the effect of immigrant peers on native students' achievement. The analysis is based on a large longitudinal administrative dataset covering two cohorts of students enrolled in vocational training in the largest Italian region. Vocational training institutions provide the ideal setting to study immigrant peer effects because they attract the lowest ability native students, and a disproportionately high share of immigrants. We adopt a value added model, and exploit within school variation, both within and across cohorts, for identification. Our results show small negative average effects on math's test scores, which are larger for low ability native students. We also show that the effects are strongly non-linear, and they only arise in classes with a high (top 20%) immigrant concentration. Results are driven by classes with a high average linguistic distance between immigrants and natives, while ethnic diversity plays no role.

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1. Introduction

Recent years have witnessed a substantial increase in immigration in most OECD countries, even in many European countries where immigration levels had been historically low. In 2015, 11.1% (43.9 million) of residents of EU-15 countries were foreign born, according to the EU Labour Force Survey, up from 6.3% in 1990, and 8.2% in 2000 (Source: UN Population Division). The surge has been especially high in Southern European countries like Spain and Italy, where the immigrants' share of population has increased by 7 percentage points and 5.5 percentage points, to 12% and 9%, respectively. While an extensive literature has investigated the labour market (e.g. Borjas, 2003; Card, 2001 and 2005; Dustmann, et al., 2013; Ottaviano and Peri, 2012) and fiscal (Auerbach and Oreopoulos, 1999; Storesletten, 2003; Dustmann and Frattini, 2014; Preston, 2014) effects of immigration in receiving countries, less is known about the impact of such flows on the education system. Indeed, the share of students with immigrant backgrounds is rising in most advanced countries: between 2003 and 2012, it has grown by between 4 and 6 percentage points in countries like Ireland, Italy and Spain, and in 2012 12% of 15-year-old students across OECD countries had an immigrant background (OECD, 2015). At the same time, the children of immigrants exhibit in many countries, especially in Europe, significant gaps in school performance relative to the children of natives (Schnepf, 2007; Dustmann et al. 2012). This educational disadvantage has sparked fears that the presence of immigrant students in the classroom may be detrimental to the learning achievements of native students. These concerns often motivate native students to move out of schools with a high concentration of immigrants (the so-called "*native flight*"), leading to immigrant school segregation, which has been documented both in the US and in the European context (e.g. Betts and Fairlie, 2003; Cascio and Lewis, 2012; Farrè *et al*., 2015). From the theoretical point of view, the worries that a large concentration of immigrant students may harm the educational attainment of natives may be rationalised within the Lazear's (2001) education production function model, according to which classroom teaching is a public good where congestion effects are potentially important. Immigrant students may be more likely to require specific attention and potentially create negative externalities for two main reasons. First, they often come from families with a poor socio-economic background and thus they tend to have lower performance compared to natives. Therefore, they are likely to be concentrated at the bottom of the academic ability distribution, where peer effects are strongest (Lavy, Silva, and Weinhardt, 2012). Moreover, immigrants tend to have lower command of the host country's language than natives, and may hence attract more than proportional teachers' attention, because of poorer language skills, thus diverting teaching resources away from other students. Teachers may also slow the pace of instruction in order to accommodate migrants (Hunt, 2012).

Whether these concerns are justified or not, however, is empirically less clear. This paper provides insights into this thorny issue for a recent immigration country, like Italy. We study the effect of immigrants' concentration in the class on native students' outcomes, taking advantage of a unique administrative dataset on the universe of students in vocational institutions in the largest Italian region. Vocational tracks are the ideal setting to study immigrants' peer effects, both because they attract particularly high shares of immigrants and because they are usually attended by the lowest achieving natives, who are typically most affected by peer effects (see Angrist and Lang, 2002). Our results show that the presence of immigrant students in the classroom has no effects on natives' language achievements, while it holds slightly back their math's test scores. These effects are quantitatively small on average, and larger for low ability native students. We also show that the effects are strongly non-linear, and they only arise in classes with a high (top 20%) immigrant concentration. As regards the mechanisms through which these effects operate, we investigate the role of ethnic diversity, finding that it plays no role, while the results are driven by classes with a high average linguistic distance between immigrants and natives.

Our paper is related to the large literature on peer effects in education (see Sacerdote, 2011 for a review), and in particular to the more recent literature that focuses on immigrant peer effects. This literature is still rather scant, and displays mixed results: papers differ in the identification strategy adopted, in the type of data used, as well as in the age groups considered and in their geographical focus (Jensen, 2015, and Brunello and De Paola, 2017, provide useful reviews).

Brunello and Rocco (2011) and Jensen and Rasmussen (2011) using PISA data and exploiting, respectively, cross-country or cross-region geographic variation, find small but significant negative effects of immigrants on natives' performance in secondary school, an effect which is limited to math in the case of Jensen and Rasmussen (2011).

Using an alternative and tighter identification strategy, which relies on within-school variation in the share of immigrant students, other papers tend to find zero or weakly negative peer effects. For example, Ohinata and van Ours (2013) using PIRLS and TIMMS data for the Netherlands and Geay, McNally and Telhaj (2013) using administrative data from the British National Pupil Database, do not find any evidence of spill-over effects from the presence of immigrant children (defined as non-English speakers in the latter paper) on test scores of native students in primary schools. In contrast, focusing on other outcomes and exploiting the mass migration of Russian immigrants to Israel in 1990, Gould, Lavy and Paserman. (2009) find that immigrant concentration in primary school has adverse effects on the dropout rate of native Israelis and on their chances of passing the high school exam necessary to attend college.

Three recent papers have instead specifically focused on the Italian experience, using administrative data from the standardised INVALSI test and looking at primary and lower-secondary schools. Contini (2013) and Tonello (2016), relying on within-school variation in immigrant concentration, find that the proportion of immigrant students has a negative weak effect on child learning outcomes, and show that this effect is slightly larger for children from low socio-economic background (Contini, 2013) and highly non-linear (Tonello, 2016). Ballatore, Fort and Ichino (2015), take a different perspective and exploit rules of class formation to identify the causal effect of increasing the number of immigrants in a classroom on natives' test scores, keeping class size and the quality of students constant. They find sizable negative effects on native performance in both language and math at age 7 and 10, and argue that conventional estimates of immigrants' peer effects are usually smaller because they are confounded by endogenous class size adjustments implemented by principals when confronted with immigrant and native inflows.

Our paper contributes to this literature along different dimensions. First, we focus on vocational schools, which were overlooked by previous studies despite attracting a disproportionate share of immigrant students, and the most disadvantaged segment of the native student population. Second, we have administrative data on the whole student population in vocational tracks, which reduces the measurement error in peer variables based on surveys that do not sample all the students in a class or schools (Micklewright et al., 2013) and overcomes the problem of under-representation of immigrant share typical in survey data (Aydemir and Borjas, 2011). Third, our data contain also information on test scores of students tested at the beginning of the first year. This feature allows us to perform several balancing test to validate our identification strategy, as well as to implement value added models that help reducing the omitted variable bias in the modelling of the education production function (Todd and Wolpin, 2003). Finally, we allow for nonlinearity in the estimation of peer effects and we investigate the underlying channels at work, testing whether ethnic diversity or linguistic distance matter in shaping the effect. To the best our knowledge, we are the first paper looking at the role of diversity within the immigrant peer groups and studying compositional effects.

The rest of the paper is structured as follows: in section 2 we present the data and provide some descriptive statistics. Our empirical approach and identification strategy are illustrated in section 3, while section 4 discusses several possible threats to identification and reports various tests of the validity of our identifying assumptions. Section 5 presents the results, and section 6 concludes and discusses some possible policy implications.

2. Data and descriptive statistics

Our analysis is based on an administrative dataset recording information on all students that attained a three-years vocational qualification certificate in 2012 and in 2013, in any vocational secondary institution in Lombardy that falls under the competence of the regional authority. Lombardy is the largest Italian region with 9.8 million inhabitants in 2013, accounting for 16.4% of the Italian population and for 15% of the total school population, but for 24% of the immigrant school population. This indicates that immigrants are disproportionally located in Lombardy, which is in fact also the region with the highest share of immigrants in school population (14%, against a national average of 8%).[2]

In Italy, education is compulsory from age 6 to age 16. After completing lower secondary schools at age 14, students can choose between three different tracks: academic, technical, and vocational[3]. The vocational track involves two types of institutions: vocational schools (*Istituti Professionali*) and vocational training institutions (*Formazione professionale regionale*). The former lasts five years and it gives direct access to university. The latter is organized at regional level and it lasts three or four years, with the possibility of attending a fifth additional years for students who want to gain access to higher education. Vocational training under the competence of Regional authorities is part of the national education and training system and is organized in two basic pathways: three-year courses, leading to the award of "*Attestato di qualifica di operatore professionale*" (vocational qualification certificate)[4] and four-year courses, leading to a "*Diploma professionale di tecnico*" (Professional technician diploma).[5] These schools aim especially to develop basic, transversal and technical-occupational skills, but they have to keep a balance between general and vocational subjects, each accounting for about 50% of total school time (about one thousand hours a year like in all upper secondary institutions, see Cedefop 2014).

Schools in the vocational track area characterized by a disproportionate share of immigrant students, both in Italy as a whole, and in Lombardy, where almost 20% of students are foreign born, compared to 10% in the technical track and less than 5% in the academic track (Figure 2.1).

[2] Source: ISTAT Geodemo, www.demo.istat.it and I.Stat based on "Rilevazione sulle scuole secondarie di secondo grado" run by MIUR (year 2012) (http://dati.istat.it/Index.aspx?DataSetCode=DCIS_SCUOLESECOND2#).

[3] See Braga, Checchi and Meschi (2013) for more details on institutional features of school systems in an international perspective.

[4] This corresponds to Level 3 in the European Qualifications Framework (EQF) (see http://ec.europa.eu/ploteus/search/site?f[0]=im_field_entity_type%3A97#)

[5] The qualifications released under the regional system are recognised at national level and within Europe. A National Register of qualifications awarded in the VET system was created in 2011. Students holding a Professional technician diploma can continue into the Higher Technical Education and Training system (IFTS- ITS) or Higher Education, on completion of an additional year and after passing a State exam (see ISFOL, 2008, for more details on vocational education in Italy).

[Figure 2.1]

Moreover, the performance of students in vocational tracks is significantly lower than that of students in academic or technical tracks, as highlighted by Figure 2.2 which reports the share of students that attain different achievement levels in PISA test, by tracks. Remarkably, 50% of students in vocational training institutions in Lombardy fall in the lowest achievement class, despite the average good performance of students in in Lombardy (14% are in the lowest class, against 25% in Italy and 26% across OECD countries).

[Figure 2.2]

Our dataset includes information on two cohorts of students that entered the regional vocational training system in Lombardy in 2009 and 2010 and that attained a vocational qualification certificate, after a three-year course, in 2012 and in 2013 respectively. All students are tested in Italian language and math at the beginning of the first year and at the end of the third year, as part of the final exam. The tests are standardized and externally marked. We standardize test scores for each subject and each cohort to have mean of zero and standard deviation of one. Besides test scores, the data contains information on students' gender, age and country of birth. We define immigrant status based on country of birth and define as immigrants all foreign-born students. A key advantage of our data is that we can identify immigrants' country of birth, which allows studying the composition of the immigrant group in each class and testing whether the diversity of the group plays a role. On the other hand, we are not able to identify second-generation immigrants. Another important advantage of our dataset is that we have the *universe* of the students attending vocational courses in Lombardy and we are able to match students with their classmates. This reduces the measurement error in construction of the peer variable that often characterises studies based on survey data, such as PISA or PIRLS (see Ammermueller and Pischke, 2009 and Micklewright *et al*., 2013). In addition, while the dataset provides little information on individual characteristics and family background, we have test scores at the beginning of the first school year and we use this variable to capture all unobserved individual, school and family characteristics that affected school performance before entering the vocational track under study.

Our sample is composed of all the students who completed the course and attained a qualification at the end of the third year. This leaves us with about 14,700 students (6,329 in the first and 8,365 in the second cohort, respectively).

Table 2.1 provides some summary statistics of the main variables included in the analysis. About 18 percent of students are foreign born,[6] which is consistent with the share reported in MIUR aggregate data in figure 2.1 and confirms the high share of immigrants in vocational education institutions. Most students are between 17 and 18 years old, as expected (deviations from this age are possible due to grade repetition), and 44% of all students are female. Overall, students are allocated between 1308 classes, and there are on average 3.5 classes per school, with the mean class size being 17.3, and the mean share of immigrants per class being 19%.

[Table 2.1]

The bottom lines of Table 2.1 report, separately for immigrants and natives, summary statistics for the initial test scores in Math and Literacy, where scores have been standardized to have mean zero and standard deviation 1 in each wave. Natives have higher test scores than immigrants in both subjects, although the difference is largest for literacy. Interestingly, there is not only a clear difference in means, but the native and immigrant score distributions appear to be different, especially for literacy: the 25th percentile of the native literacy distribution is -0.41, higher than the mean value for immigrants (-0.56); likewise, the 75th percentile of the immigrants' score distribution (0.13) is lower than the native mean (0.16).

3. Identification of Peer Effects and Empirical Strategy

The main challenge in the estimation of peer effects resides in the fact that peers are hardly randomly assigned to schools. In fact, similar students tend to choose similar schools. For example, students from advantaged backgrounds with higher levels of ability and better access to information typically choose better schools. Thus, peer group is likely to be self-selected. This is especially true in the case of immigrants that tend to sort into disadvantaged schools (due to lower access to information and residential segregation), where also low-ability natives are likely to be concentrated. If immigrants are not randomly allocated to schools, then the impact of class composition can be easily confounded with school-specific unobservable effects, leading to biased estimates of peer effects.

The existing literature has adopted different strategies to deal with the endogenous sorting of students across schools and to identify causal effects.

[6] Albania, Morocco, Romania, India, Ecuador, Pakistan, Senegal, Peru, Moldova, Ukraine are the leading countries of origin for immigrants, accounting for almost 70 percent of all countries of origin of immigrants in our sample

One method is to rely upon some form of *exogenous variation* in the assignment of students to schools, or classrooms. Duflo et al., (2011) exploit variation in peer composition generated by actual randomization. Angrist and Lang (2004) take advantage of the substantial increase in the number of disadvantaged black or other minority students in schools of the affluent suburbs of Boston induced by a desegregation program run by the Metropolitan Council for Educational Opportunity (Metco). Gould et al. (2009) rely on the variation in the number of immigrant students induced by the exogenous early '90s immigration waves in Israel. Ballatore, et al. (2015) use the exogenous variation in the number of natives and immigrants induced by the rule, in place in Italian primary schools, imposing a cap of 25 students per class. Other papers instead overcome the issue of endogenous sorting of students between schools by aggregating the data at city, state or country level (see, for example, Card and Rothstein, 2007, Brunello and Rocco, 2011, Jensen and Rasmussen, 2011, and Hunt, 2012). Another common method is to use models with school fixed effects in an effort to control for the unavoidable self-selection of students into schools. The papers using this approach identify the effects of peers by exploiting the idiosyncratic within-school variation in peer characteristics across adjacent cohorts (e.g. Hoxby, 2000; Lavy and Schlosser, 2011; Lavy, Paserman and Schlosser, 2011; Burke and Sass, 2013; Geay, McNally and Telhaj, 2013) or across classes in the same cohort (e.g. Ammermueller and Pischke, 2009; Ohinata and van Ours, 2013).

Our identification strategy relies on random variation of students both across classes and across adjacent cohorts within schools. In particular, we estimate the following model, derived from a reduced form of an education production function:

$$Y_{icst} = \rho Y_{icst}^{0} + X_{icst}'\alpha + C'_{cst}\beta + \gamma IMMSHARE_{cst} + \delta_s + \vartheta_t + \varepsilon_{icst} \qquad (1)$$

Where Y_{icst} is the outcome (standardised test scores in either math or language) of native student *i* in classroom *c*, school s in cohort *t*; Y^0 is the student's outcome at the beginning of the first school year, X_{icst} are students' characteristics (gender, age), C_{cst} class characteristics (class size, share of females), IMMSHARE is the share of immigrants in each class, δ_s are school fixed effects, ϑ_t are cohort dummies and ε_{icst} is the error term.

If students are randomly allocated across classes and cohorts within schools, school fixed effects allow controlling for systematic cross-school variation in school or student quality and thus overcome the issue of endogenous selection of students. In this case, the parameter γ identifies the *causal* effect of immigrant share on natives' performance. Therefore, our main identification assumption is that, once school specific unobserved characteristics are controlled for, students are allocated to each class randomly within a school. We will test this assumption as explained below (see section 4).

When studying peer effects in education, a second - and often neglected - identification issue lies in modelling the education production function. The student's academic achievement at a given point in time is a function of both current and past inputs. Since it is clearly difficult to have data on all past and present family and school inputs, the estimation of the impact of the observed inputs is likely to suffer from an omitted variable bias (see, for example, Todd and Wolpin, 2003 and 2007). Our strategy to overcome the lack of data on historical input measures is to adopt a value added specification, which assumes that "a previous test score can serve as a sufficient statistic for the influence of all historical inputs" (Todd and Wolpin, 2007).[7]

4. Threats to identification

As discussed in Section 3, our key identifying assumption is that once school specific unobserved characteristics are controlled for, students are allocated to each class randomly within a school. In this section, we provide several alternative tests of the validity of our identification strategy.

First, we test whether, in each cross- section, the observed distribution of immigrant students across classes is compatible with random assignment. Second, we check whether the concentration of immigrants in each class is systematically correlated with initial natives' (or immigrants') ability. Third, we assess whether the share of immigrants in a classroom affects the likelihood that native students drop out of school. Lastly, we check for possible non-random variation of immigrant share across adjacent cohorts, due to *natives' flight*, i.e. the possibility that higher immigrant concentration may lead native students to choose different schools in subsequent years.

4.1: Random assignment test

Within each cohort of students, we can test random assignment of immigrants (across classes within schools) performing the following Pearson X^2 test (see also Ammermueller and Pischke, 2009). Random allocation implies independence between immigration status and the class the student is assigned to. The Pearson X^2 test probes whether the number of immigrants in a particular class is consistent with independence, given the number of students in the school. Formally, we can write the test statistic for each school as follows:

[7] To the best of our knowledge the only other papers that study immigrants' peer effects adopting a value added approach are Friesen and Krauth (2011) and Geay et al., 2013.

$$P = \sum_{C} \frac{(n_{cIMM} - \widehat{n_{cIMM}})^2}{\widehat{n_{cIMM}}} + \sum_{C} \frac{(n_{cNAT} - \widehat{n_{cNAT}})^2}{\widehat{n_{cNAT}}}$$

where $n_{cIMM\,(NAT)}$ is the number of immigrant (native) students in classroom c= 1, …, C_s . $n_{c\widehat{IMM(NAT)}}$ is the predicted number of immigrant (native) students when immigrant status and classroom are independent, which are given by:

$$\widehat{n_{c,IMM}} = \frac{N_c * N_{sch}^{IMM}}{N_{sch}} \qquad \widehat{n_{c,NAT}} = \frac{N_c * N_{sch}^{NAT}}{N_{sch}}$$

where N_{sch}^{IMM} is the total number of immigrant students in the school, N_c is the total number of students in class c and N_{sch} is the total number of students in the school. This means that the total number of immigrant students in the school is allocated to each class c, according to the proportion of overall students in class c. This implies that the joint probability that a randomly chosen student from a given school has a migrant background and is assigned to class c is equal to the overall proportion of migrants in the school times the proportion of students in class c. Under the null hypothesis of independence, $P \sim X^2$ with (C_s -1) degrees of freedom. We performed these tests for every single school in each cohort. Table 4.1 shows the distribution of *p*-values in the two cohorts. In about 90 percent of the schools, we cannot reject the random assignment of students across classes at the 5% significance level (that is, the *p*-values are above the 5% level in about 90 percent of the schools in both cohorts).

[Table 4.1]

4.2: Non-random sorting of immigrants and natives between classes within schools in each cohort

A potential concern could be that schoolmasters concentrate more immigrants in classes with "better" or "worse" native students. We show that this is not the case in Table 4.2, where we report results from regressions of native test scores at entrance (i.e. before any peer effects have taken place) on the share of immigrants in the classroom and additional control variables (gender, age, class size, share of females, cohort dummies). Columns 1 and 2 report the results for language and math test scores, respectively, when we do not include school fixed effects, and show clear evidence of negative sorting between schools. However, once we include school dummies, in columns 3 and 4, there is no indication of any systematic correlation between the share of immigrant students in each class and the initial language or math proficiency of native students. This finding is an additional indication that our identifying assumption is likely to hold.

[Table 4.2]

We may still be worried that the allocation of immigrants across classes is not random with respect to their own ability (if for example schoolmasters decide to allocate more immigrants to a class if they have relatively good past school performance). Since we have data on immigrants' initial test scores, we test this hypothesis, by regressing immigrants' entrance test scores in math and language on the share of immigrants in the class, without (columns 1 and 2) and with (columns 3 and 4) school fixed effects. Again, the results show that, once we control for school fixed effects, there is no systematic correlation between immigrant share and immigrants' ability.

4.3: Natives' drop-out

An additional concern on the identification strategy regards the possibility that higher shares of immigrants in the class increase the probability of natives' drop out. For instance, if a higher initial concentration of immigrants leads the best native students to drop out of school, then we would observe a spurious negative correlation between immigrant share and natives' test scores, due to their unfavourable selection. As a result, our estimates of the causal effect of immigration on natives' achievement would be downward biased. Dropout rates are particularly high in vocational schools (about 40% in our sample), so this concern may be particularly relevant in our context. Our data provide information on classes' composition at the beginning of the first school year and we can thus test whether the initial share of immigrants in the class affects the probability of natives' dropout. In particular, we estimate a linear probability model where we regress, for each native student, the probability of dropping out on the initial share of immigrants in the class and on the usual set of additional control variables (initial test score, age, gender, class size, share of females in the class, cohort dummies), with and without school fixed effects. The results, reported in table 4.3, indicate that, in general, natives are more likely to drop out of school in classes with a higher share of immigrants (column 1). However, this is due to the clustering of immigrants in schools characterized by higher dropout rates. In fact, once we control for sorting across schools by means of school fixed effects, the correlation becomes negative but much smaller in magnitude and not statistically significant at conventional levels (column 2). Even though these results are comforting, we may still be worried that the absence of an average effect on dropout rates originates from opposite effects on high and low ability native students. To investigate this possibility, we run separate regressions for native students with high and low initial ability. We measure ability as the mean of the math and language test score at entrance, and define as “high” or “low” ability students those with a score above or, respectively, below the median value. Reassuringly, our results reported in columns (3)-(6) indicate the absence of any impact on dropout rates for both groups.

4.4: “Natives’ flight”

Another potential threat to identification is related to the *natives’ flight* phenomenon (see Betts and Fairlie, 2003):[8] we may be concerned that the best native students change schools as a result of high immigrants’ concentration, so that the variation in the quality of the schools over time would not be random. In this case, estimates of negative impacts of immigrant concentration on native students may simply reflect selective school enrollment of both immigrant and native children. To test this, we run school-level regressions of the change in mean initial ability of natives between the second (entrance in 2010) and the first cohort (entrance in 2009) on the share of immigrants in the school population in the first cohort, as well as on the usual set of additional control variables. The results indicate that the variation in the average quality of incoming students over time is not affected by past share of immigrants in the school.[9]

5. Results

The results of our main specification are reported in table 5.1. The table shows the effect of immigrant share on language (col.1-3) and math (col. 4-6) test scores of native students. Columns 1 and 4 report the results for the whole sample, while in columns 2-3 and 4-6 we divide the sample by students’ initial test scores in order to test whether peer effects are homogenous or not along the distribution of ability. In particular, students are defined as “high” or “low” ability if the mean of their initial scores in math and language is above or, respectively, below the median value.[10]

[Table 5.1]

The results indicate that, once we control for non-random sorting of students across schools, the share of immigrants does not significantly affect natives’ language test scores. We find instead a

[8] Betts and Fairlie (2003) coined the expression ‘native flight’ to denote the tendency of native-born Americans to leave public schools for private alternatives following an increase in immigration shares in their home communities.
[9] The coefficient and standard error are respectively -0.22 and 0.175.
[10] As robustness check, we also tried alternative definitions of high and low ability, such as above and below the *mean* or above 75th percentile and below 25th percentile and the results are qualitatively similar.

small negative effect of immigrants' share on natives' math test scores. In particular, the magnitude of the effect is such that a ten percentage point increase in immigrant share (70% of a standard deviation) leads to about 4.6% s.d. decrease in test scores. The lack of any adverse effect on literacy test scores, and the fact that immigrant peers have instead a negative impact on math's achievements of natives may seem counterintuitive at first. However, the educational literature has emphasized that language is essential for mathematical learning (Ríordáin and O'Donoghue, 2009) and has demonstrated that students underachieve in mathematics when the school language is different from their home language. In multilingual settings, math teachers have to deal with language practices that learners bring to school, and "discontinuities in understanding new words and new meanings can turn into a wide variety of cultural conflicts and disruptions of the learning process" (Gorgoriò and Planas, 2001). Moreover, the stronger impact on math may be due to the fact that while performance in literacy test depends mainly on language proficiency (in turn related to family background and to competences acquired in primary and lower secondary schools), the attainment in math is more influenced by learning environment in school and peer effects.

Table 5.1 also shows that the impact of immigrant concentration is not homogenous along the natives' distribution of ability. In fact, when we run separate regressions by natives' ability (columns 5 and 6), we find that the average negative effect discussed above is entirely due to the impact on low ability native students. For this group the effect is larger: a ten percentage point increase in immigrant share leads to a decrease in the test scores of low ability natives of about 7.5% of a standard deviation. This result is consistent with previous literature on peer effects that typically found that disadvantaged children are more likely to be adversely affected by a higher immigrant concentration in the class or school (e.g. Gould et al., 2009; Angrist and Lang, 2004). On the other hand, the effect on high ability natives is smaller in magnitude, and very imprecisely measured. A test for the equality of the coefficients for low and high ability allows rejecting the null hypothesis of no difference with a p-value of 0.09.

There is considerable variation in immigrant shares across classes in our sample. While the mean value of the share of foreign born students per class is 19%, moving from the 25th to the 75th percentile of the distribution implies a jump from 8% to almost 30%, as we have shown in Table 2.1. Such a variation may indicate that constraining the effect to be linear may be too restrictive. It may in fact be that the effect of immigrant share in a class arises only when the concentration of immigrants is above a certain threshold. For this reason, in Table 5.2 we test for non-linearities in the effects of immigrant concentration in the class, by using as regressors dummies for each quintile of the distribution of immigrant share in each cohort.

[Table 5.2]

Our results confirm the lack of any immigrant peer effects on language test scores, even for classes with very high share of immigrant students, while they show strong evidence of non-linearities of the effect for mathematics. Students in classrooms where the share of immigrants is in the second, third or fourth quintile of the distribution have no significant differences in their test scores with respect to students in classes where the share of immigrants is in the first quintile. Conversely, being in the fifth quintile implies a reduction of 23 percent of a s.d. in math test scores relative to being in the first quintile.[11] This suggests that it is only with high proportion of immigrants that negative peer effects arise (see also Hardoy and Schøne, 2013; Tonello, 2016).[12] If we split the estimates by natives' ability, again we find that the effect of high concentration of immigrants is stronger and larger for low ability natives. Moreover, the results for low ability natives reported in column (5) suggest that the critical threshold is lower for this group, for which negative effects arise already in classes in the fourth quintile of the immigrant share distribution.

Summing up, our estimates reveal that immigrants' peer effects on natives' math scores are rather small on average. They are larger for low ability native students and they are driven by the classes where the concentration of immigrants is particularly large, meaning that as long as the immigrant share in a class is sufficiently low, non-native students' presence is not able to generate negative peer effects on native outcomes.

In the next section, we explore other dimensions of heterogeneity that should help shedding some light on the channels at work.

5.1: The role of diversity and language distance

Our results so far suggest that high share of immigrants in the class slightly harms the academic performance of natives. In this section, we investigate what drives this effect. In particular, we ask whether the degree of diversity of the immigrant group or the linguistic distance of their own languages from Italian play a role.

[11] Moving from the first to the fifth quintile implies increasing the share of immigrants by about 40 percentage points, from a mean of 0.02% to 41%.

[12] As an alternative way to test for non-linearity, we also introduced a linear spline functional form in IMMSHARE, with a threshold set at the median in order to allow the marginal effects to be different below and above the median. We found significant and negative marginal effects only for high levels of immigrant share in the class (above the median). In particular, above median (0.15) a 10pp increase in IMMSHARE leads to a reduction of 10.4% of sd (results available upon request)

Several papers investigated the role of ethnic diversity on economic and social outcomes with mixed results. Ethnic diversity was found to have a negative effect on trust and solidarity (Putnam, 2007) and on the provision of public goods (Alesina and La Ferrara, 2005), but a positive effect on the productivity of natives (Ottaviano and Peri, 2006). In the context of schools, ethnic diversity may worsen the social interaction of students and make the job of teachers more difficult, but it may also enrich the school environment and lead to faster assimilation of immigrant students. In fact, small ethnic minority groups have a bigger incentive to adopt the majority culture and language as a mean for interaction (Lazear, 1999; Maestri, 2016). To the best of our knowledge, there are almost no empirical works on the effect of ethnic diversity in the class on natives' school performance. One exception is Maestri (2016)[13] who finds that ethnic diversity does not have a significant impact on language test scores of native students, but it increases the language performance of immigrant students, even after controlling for ethnic compositional and peers' effects.

We construct a measure of ethnic diversity across the foreign born population in each class, considering the country of birth of students as defining their cultural and ethnic identity (see Ottaviano and Peri, 2006; Constant et al., 2009). Specifically, we calculate an index of diversity based on the Hirschman–Herfindahl Index (Hirschman, 1964) as follows:

$$DIV = 1 - \sum_i s_i^2$$

where s_i denotes the share of students born in country i out of the total number of foreign born students in each class. This index can be interpreted as the likelihood that two randomly drawn immigrant students were not born in the same country. The index is bounded between a minimum of 0 for contexts with only one category and a maximum of 1, which is reached when the population is divided into an infinitive amount of categories.

DIV is a measure of fractionalization that considers all groups as the same, regardless of the characteristics of students' countries of origin. However, we know that students whose native language is significantly different from Italian may have lower school achievements and might also require a higher proportion of teacher's time. For this reason, we want to explore the role of linguistic distance between the native language of immigrants and Italian. We therefore created a language dissimilarity index (LDI) calculated as the mean for each class of a measure of linguistic

[13] On this topic, see also Dronkers and van der Velden (2012) and Braster and Dronkers (2013). These papers, however, do not attempt to identify any causal effect.

distance, the *Levenshtein linguistic distance index*, drawn from Adsera and Pytlikova (2015). The index is produced by the Max Planck Institute for Evolutionary Anthropology, and relies on phonetic dissimilarity of words in two languages. The continuous index increases with the distance between languages. Linguists chose a core set of the 40 more common words across languages describing everyday life and items; then, expressed them in a phonetic transcription called ASJP code and finally computed the number of steps needed to move from one word expressed in one language to that same word expressed in the other language. For a detailed description of the method, see Bakker et al., (2009). In our sample, the index ranges from 58 (for Spanish) and 102 (for Rwanda).

The two indices (DIV and LDI) measure different dimension of diversity, and they are not necessarily correlated with the share of immigrants in the classroom. As an illustration, consider two classes with twenty students, twenty of which are foreign born. However, in the first class all foreign-born students were born in Romania, whereas in the second class all foreign-born students were born in China. In both classes, the share of immigrants is 25 percent and the DIV is zero. However, since a neo-Latin language like Romanian is more similar to Italian than Mandarin, the LDI will be 61 in the first class, and 100 in the second. Note that both DIV and LDI would be the same if the number of immigrants, but not their composition, changed. Likewise, if instead of being from Romania, the five immigrant students in the first class were from Ecuador, Peru, El Salvador, Venezuela and Spain, then the DIV would jump up to 0.84, since ethnic diversity increases, yet the LDI would even decrease to 58, as Spanish is closer to Italian than Romanian.

[Figure 5.1]

Figure 5.1 and 5.2 illustrate empirically the points illustrated above. Figure 5.1 plots for each class the LDI against the DIV, and shows visually the lack of correlation between the two measures. The linear correlation between LDI and DIV is 0.0802 with a p-value of 0.0103:

[Figure 5.2]

Figure 5.2 plots instead the DIV (left figure) and the LDI (right figure) against the share of immigrants in the class, showing empirically that DIV and LDI capture different attributes of a class than the immigrant concentration.

We then define as high (low) diversity those classes where the DIV is above (below) the median, and similarly define classrooms as characterized by low or high language dissimilarity if their value of LDI is below or above the median. We then run separate regressions for classes characterized by high and low diversity (top panel of table 5.3) and high and low linguistic dissimilarity (bottom panel of table 5.3).

[Table 5.3]

The results again confirm that lack of any significant impact of immigrant concentration on language test score even in classes with high language dissimilarity and high cultural diversity (columns 1 and 2 – top and bottom panel). More interestingly, our estimates show that while there are no significant differences between classes with high and low diversity (columns 3 and 4 – top panel), the share of immigrants has a negative effect on math test score of natives only in classes characterized by a high language dissimilarity (columns 3 and 4 – bottom panel). The difference between the two coefficients of immigrant share in classes with high and low linguistic dissimilarity is statistically significant, as we can reject the null hypothesis of equality with a p-value of 0.008. This finding suggests that the linguistic distance between foreign born and native students plays a key role in explaining immigrants' peer effects. Thus, one potential channel through which immigrant students could adversely affect natives is an externality from their limited language proficiency that becomes difficult to cope for teachers when the immigrant group is diverse and linguistically distant. To investigate this result more deeply, we further split our sample by students' levels of ability (see Table 5.4) and we find that only low ability natives in classes with high linguistic dissimilarity are negatively affected by the share of immigrants (see column 3). High ability natives are instead not affected by immigrants even if their linguistic distance is high (see column 4).

[Table 5.4]

In panel B of Table 5.4, we test whether the difference in immigrant peer effects between classes with high and low LDI holds in a nonlinear setting. We therefore include as regressors dummies for each quintile of the distribution of immigrant share in each cohort, as we did in table 5.2. The results confirm that the negative effect of immigrants comes entirely from the classes with high linguistic distance and a very high concentration of immigrants (top 20%) and that it is larger for low ability natives.

6. Conclusions

In this paper, we provide new evidence on the effect of immigrant peers on native students' achievement. We are the first to investigate the impact of immigrant concentration in vocational schools, which are an interesting setting since they are typically attended by a disproportionate share of immigrant students, and by the most disadvantaged segment of the native student population. We take advantage of a unique administrative dataset on the universe of students in vocational institutions in the largest Italian region, which contains information on test scores in literacy and math for each students at the beginning and at the end of the three-year vocational course. This allows us implementing value added models that help reducing the omitted variable bias in the modelling of the education production function. Our identification strategy relies on random variation of students across classes and across adjacent cohorts within schools and we provide several tests supporting the validity of our identifying assumptions.

We find that the presence of immigrant students in the classroom has no effects on natives' language achievements, while it negatively affects math's test scores. These effects are quantitatively small on average and larger for low ability native students. Our results also indicate that the effects are strongly non-linear, and they only arise in classes with a high (top 20%) immigrant concentration. As regards the mechanisms through which these effects operate, we investigate the role of ethnic diversity, finding that it plays no role, while we show that the results are driven by classes characterised by a high average linguistic distance between immigrants and natives.

Overall, our estimates indicate that the impact of immigrant concentration in the class is negligible, even in this generally disadvantaged context. This suggests that the widespread perception that the increasing number of immigrant students in schools creates negative peer effects on native-born students may not be empirically grounded. However, we show that potential problems may arise in those cases where the share of immigrants is particularly large and the linguistic distance is high. Therefore our findings seem to suggest that native students may benefit from a more even distribution of foreign born students across schools, operated, for example, through residential desegregation polices. In addition, given that our estimates indicate that linguistic distance matters, investing more resources for linguistic support to immigrant students could help mitigating the possible disruptive effects of high immigrants' concentration in schools.

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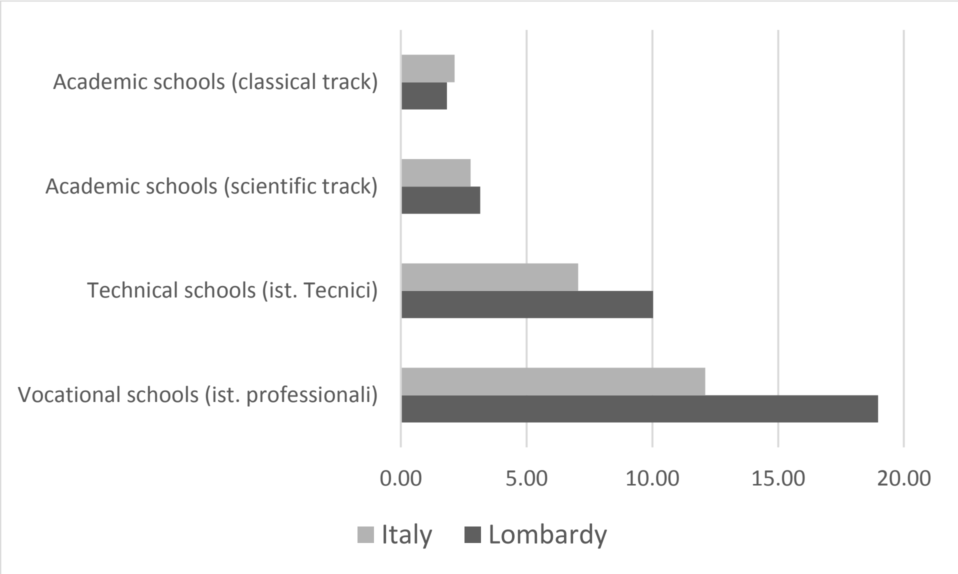
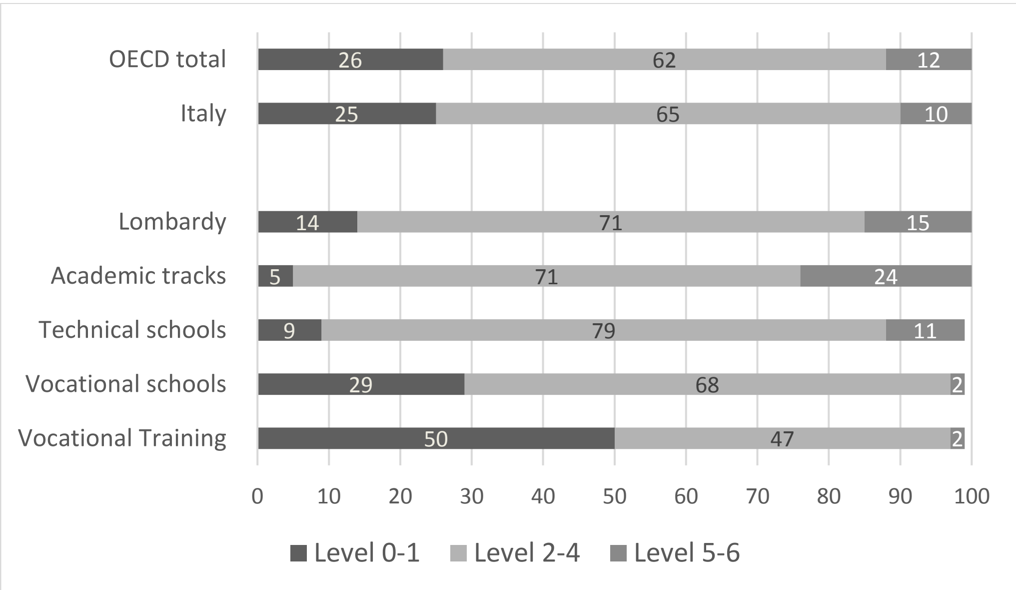
Figures

Figure 2.1: Share of foreign-born students, by school track - Lombardy and Italy



Source: *Open Data, MIUR, 2012*

Figure 2.2: Share of students in PISA levels, by track



Source: *PISA, 2012*

Figure 5.1: Correlation between Language Dissimilarity Index (LDI) and Ethnic Diversity Index (DIV)

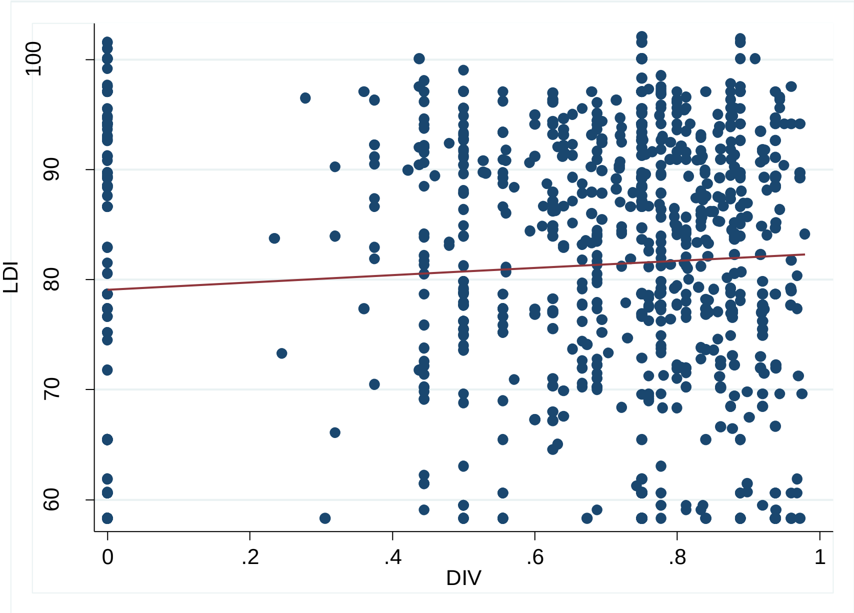
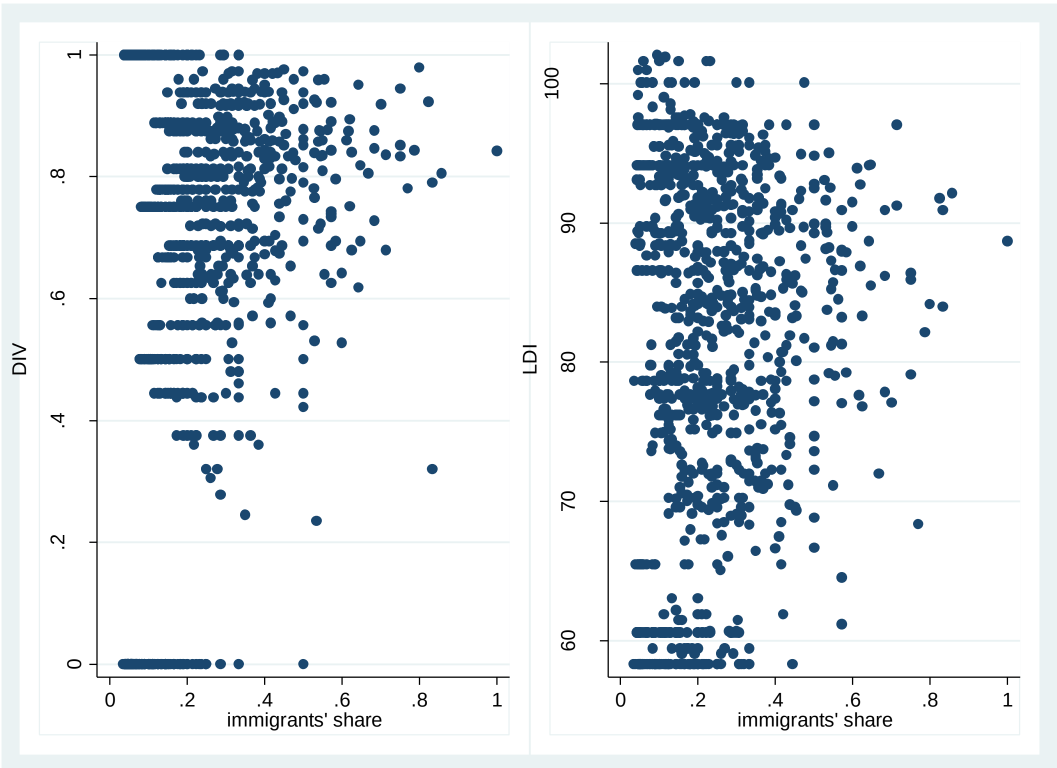


Figure 5.2: Correlation between immigrants' share in the class (horizontal axis) and Ethnic Diversity Index - DIV (left panel) and Language Dissimilarity Index – LDI (right panel)



Tables

Table 2.1: Summary statistics

	N	Mean	sd	p25	p75
Foreign born	14708	0.18	0.38	0	0
Female	14708	0.44	0.50	0	1
Age	14708	17.76	0.96	17	18
Share of foreign born per class	1308	0.19	0.16	0.08	0.29
N. of classes per school	1308	3.51	2.14	2	4
Class size	1308	17.32	4.97	14	21
Share of female per class	1308	0.40	0.39	0	0.8
Std (entrance) test score in Math - Natives	11995	0.07	1.00	-0.68	0.75
Std (entrance) test score in Math – Immigrants	2660	-0.18	0.96	-0.92	0.37
Std (entrance) test score in Literacy - Natives	12047	0.16	0.93	-0.41	0.81
Std (entrance) test score in Literacy –	2661	-0.56	0.99	-1.28	0.13

Table 4.1: *p*-value for Pearson X^2 tests of independence between immigrant background and classroom assignment within each school

	5%	10%	25%	50%	75%	90%	95%
2009	0.025	0.036	0.171	0.521	0.737	0.916	0.963
2010	0.016	0.052	0.140	0.425	0.738	0.912	0.970

Table 4.2: Sorting into schools and classes

	Panel A: Dep. Var.: Test scores of natives (at entrance)			
	(1)	(2)	(3)	(4)
	Language	**Math**	**Language**	**Math**
Immigrant share	-0.382***	-0.253***	-0.066	-0.156
	(0.068)	(0.073)	(0.116)	(0.121)
Controls for Age, gender, class size, share of females, cohort	*Yes*	*Yes*	*Yes*	*Yes*
School fixed effects	*No*	*No*	*Yes*	*Yes*
Observations	12,636	12,620	12,636	12,620
	Panel B Dep. Var.: Test scores of immigrant (at entrance)			
	Language	**Math**	**Language**	**Math**
Immigrant share	-0.460***	-0.089	-0.116	0.056
	(0.111)	(0.110)	(0.209)	(0.199)
Control for Age, gender, class size, share of females, cohort	*Yes*	*Yes*	*Yes*	*Yes*
School fixed effects	*No*	*No*	*Yes*	*Yes*
Observations	2,661	2,648	2,661	2,648

Robust standard errors (clustered by class) in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 4.3: Effect of immigrant share on the probability of natives' dropout

	Dependent variable: dropout					
	Whole sample		*Low ability*		*High ability*	
	(1)	(2)	(3)	(4)	(5)	(6)
Immigrant share	0.127***	-0.082	0.080*	-0.082	0.169***	-0.042
	(0.043)	(0.050)	(0.048)	(0.066)	(0.058)	(0.070)
School fixed effects	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
Observations	20,958	20,958	10,425	10,425	10,533	10,533

Robust standard errors (clustered by class) in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 5.1: Effect of immigrant share on language and math test scores, overall and by ability

	Language			Math		
	whole sample	*low ability*	*high ability*	*whole sample*	*low ability*	*high ability*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Immigrant share	0.131	0.052	0.167	-0.463**	-0.754***	-0.392
	(0.164)	(0.219)	(0.209)	(0.203)	(0.235)	(0.257)
Entrance test	0.372***	0.289***	0.364***	0.210***	0.107***	0.219***
	(0.010)	(0.018)	(0.019)	(0.012)	(0.018)	(0.018)
Female	-0.063**	-0.123***	0.026	-0.082***	-0.104***	-0.050
	(0.027)	(0.040)	(0.037)	(0.026)	(0.037)	(0.035)
Class size	0.006	0.008	0.007	-0.001	0.005	-0.004
	(0.004)	(0.006)	(0.005)	(0.006)	(0.006)	(0.007)
Share of females	-0.065	-0.009	-0.183	0.164	0.195	0.173
	(0.148)	(0.222)	(0.168)	(0.200)	(0.261)	(0.219)
Age	0.071***	0.092***	0.047***	0.039***	0.017	0.042***
	(0.010)	(0.017)	(0.014)	(0.010)	(0.015)	(0.014)
Cohort 2	-0.035	-0.093***	0.020	0.055	0.025	0.082*
	(0.025)	(0.035)	(0.030)	(0.036)	(0.042)	(0.044)
School FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	12,047	5,869	6,178	12,026	5,868	6,158

Robust standard errors (clustered by class) in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 5.2: Non-linear effect of immigrant share – quintiles of IMMSHARE

	Language			Math		
	whole	*low ability*	*high ability*	*whole*	*low ability*	*high ability*
	(1)	(2)	(3)	(4)	(5)	(6)
2nd quint of imm. share	-0.015	-0.062	0.014	-0.024	-0.100	0.007
	(0.046)	(0.060)	(0.054)	(0.061)	(0.069)	(0.069)
3rd quint of imm. share	0.005	0.001	0.019	0.014	-0.051	0.029
	(0.046)	(0.065)	(0.052)	(0.062)	(0.071)	(0.070)
4th quint of imm. share	-0.039	-0.059	-0.018	-0.068	-0.198**	-0.005
	(0.056)	(0.075)	(0.070)	(0.076)	(0.083)	(0.097)
5th quint of imm. share	0.080	0.022	0.109	-0.235***	-0.312***	-0.227**
	(0.062)	(0.083)	(0.077)	(0.084)	(0.095)	(0.107)
Control for Age, gender, class size, share of females, cohort, school fixed effect	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	12,047	5,869	6,178	12,026	5,868	6,158

Robust standard errors (clustered by class) in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 5.3: Results by HHI or by language distance

	Language		Math	
	(1)	(2)	(3)	(4)
	By cultural diversity (DIV)			
	Low	*High*	*Low*	*High*
IMMSHARE	0.090	0.311	-0.327	-0.105
	(0.298)	(0.273)	(0.319)	(0.369)
Observations	6,516	5,531	6,497	5,529
	By linguistic distance (LDI)			
	Low	*High*	*Low*	*High*
IMMSHARE	0.427	-0.083	-0.069	-0.806**
	(0.299)	(0.291)	(0.340)	(0.357)
Observations	5,498	5,204	5,487	5,194

Robust standard errors (clustered by class) in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 5.4: Results by linguistic distance and ability, linear (panel A) and nonlinear (panel B) effects

	Low LDI		High LDI	
	Low ability	*High ability*	*Low ability*	*High ability*
	(1)	(2)	(3)	(4)
	Panel A			
IMMSHARE	-0.110	-0.398	-1.292***	-0.387
	(0.376)	(0.386)	(0.454)	(0.432)
Observations	2,746	2,741	2,530	2,664
	Panel B			
2nd quint of imm. share	-0.283**	-0.034	-0.110	-0.114
	(0.118)	(0.109)	(0.194)	(0.140)
3rd quint of imm. share	-0.008	-0.052	-0.084	-0.045
	(0.115)	(0.100)	(0.176)	(0.136)
4th quint of imm. share	-0.134	-0.015	-0.205	-0.152
	(0.118)	(0.093)	(0.184)	(0.140)
5th quint of imm. share	-0.053	-0.147	-0.514***	-0.311*
	(0.134)	(0.168)	(0.199)	(0.179)
Observations	2,746	2,741	2,530	2,664

Robust standard errors (clustered by class) in parentheses, *** p<0.01, ** p<0.05, * p<0.1