Immigrant background peer effects in Italian schools

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Abstract

This article provides an empirical assessment of the effect of immigrant concentration on student learning in Italian primary and lower secondary schools, using the data of a standardized learning assessment administered in 2010 to the entire student population of selected grades at the national level. Identification is accomplished by exploiting the within-school random variability observed in the share of immigrant students across classes. I estimate peer effects allowing for heterogeneous effects between native and immigrant background children, and among natives, between children of different socio-economic status. The main finding is that the proportion of immigrant students has a negative weak effect on child learning outcomes. This effect is somewhat larger for children from disadvantaged backgrounds (immigrants and low socio-economic status) and negligible or even positive for high status native children.

Keywords: educational inequality, learning outcomes, immigrant children, peer effects

JEL code: I24

1. Introduction

The rapid growth of immigrant flows which has occurred over the last decade in Italy, much like in other European countries, has sparked a growing concern within large sectors of the public opinion over the assimilability of newcomers and the demographic and cultural transformations of the Italian society. A key element of the integration process is the educational system, which is now confronted with the challenge of the inclusion of numerous immigrant children of diverse origins. Overall, at the national level, the share of students from an immigrant background in primary and lower secondary school has increased from 3 to 9% in ten years (with peaks of 20% in some Northern cities). This growth has contributed to raise the fear that immigrant students are detrimental to the learning opportunities of native children. However, whether this is true or not, is still an open empirical question.

Evidence of large performance gaps between native and immigrant students is provided by OECD (2006), Schnepf (2007) and Dustmann *et al*. (2011). Many reasons may be lying behind this disadvantage: the lower socio-economic background of immigrants, language problems, cultural factors, the features of origin and host countries' educational systems (de Heus and Dronkers, 2010). There is a considerable cross-country heterogeneity in the magnitude of these gaps: in traditional immigration countries like USA, Australia and Canada immigrant children perform much better relative to natives as compared to most European countries, where immigration is a recent phenomenon. Major differences are also observed within Europe, as in English-speaking countries the disadvantage is much smaller. Native-immigrant differentials are attenuated once conditioning on parental background, but in most countries gaps do not disappear.

Understanding how peer effects function is crucial to analyzing a variety of educational policies (Hoxby, 2006). The existing literature mainly focuses on socio-economic status, gender and ethnic differences, while little effort has been directed to the estimation of peer effects related to immigrant background. Findings from previous studies on ethnic composition of schools may not be relevant for the more recent immigrants. On the one hand, new immigrants have higher motivations and aspirations than ethnic minorities (Ogbu, 1991; Portes and Rumbaut, 2001); on the other hand, they have to adapt to a new (often hostile) environment, facing a new language, new social networks, different working conditions and living arrangements.

The sociological literature offers a number of papers on selected European countries and different levels of schooling. Cebolla-Boado (2007) focuses on French lower secondary school, and finds non-significant effects of the share of foreigners on various educational outcomes. Van der Silk *et al.* (2006) and Dumay (2008) focus on the effect on achievement in the Netherlands. While the first reports only small negative effects on language proficiency, and not always significant, the second finds stronger effects, especially in 4th grade. Agirdag *et al* (2011) study compositional effects of social background and minority status in Flemish Belgium on the achievement of lower secondary school pupils, finding non-significant effects. Cebolla-Boado and Medina (2011) report no significant effects of the share of immigrants in Spanish primary education. Fekjaer and Birkelund (2007) focus on upper secondary graduates, and examine the effect of migrant school composition on grades and on the probability of university enrollment in Norway; they find small positive effects on both outcomes for native students and second generation immigrants, negative effects on first generation immigrants on grades. In the educational economics literature, exploiting aggregate data at the country level, Brunello and Rocco (2011) use international PISA data to analyse how immigrant pupils affect the school performance of natives at age 15, finding evidence of small but significant negative effects, increasing with the level of segregation of immigrants. Gould *et al*. (2009) focus on the immigrant concentration in 5th grade on later educational outcomes in Israel; their results suggest that the overall presence of immigrants has large adverse effects on the dropout rate and on the chances of passing the high school exam necessary to attend college.

Although findings from all these empirical studies are not always consistent, peer effects related to immigrant background are generally negative, but small and sometimes not statistically significant. On the other hand, substantial effects related to socio-economic school composition are often reported.

In this paper I provide an empirical assessment of the impact of immigrant concentration on student learning in Italian primary and lower secondary education. To date, there are no such studies on Italy. I contribute to the existing literature by investigating peer effects on a very recent immigration country, where the majority of immigrant children are born abroad and there is no institutionalized body of policies aimed at their integration. I estimate peer effects allowing for heterogeneous effects of immigrant concentration between native and immigrant background children, and among natives, between children of different socio-economic status.

I assume that peer effects act at the class level. The main empirical issue is self-selection into schools, which makes the proportion of immigrant students highly endogenous. Schools with a high share of immigrant students often host low socio-economic status native children; for this reason I include social origin, native students' repetitions and gender class composition variables as controls. Most importantly, if children from advantaged backgrounds, having higher aspirations and better access to information, choose better schools and/or school attendance rules select students with respect to ability related factors, the impact of class composition can be easily confounded with school-specific unobservable effects, leading to biased estimates of peer effects. However, if children are randomly assigned to classes, it is possible to exploit the within-school random variability observed between classes in the peer variables (Ammermueller, Pischke; 2009). Under class random assignment, school fixed-effect models provide consistent estimates of the causal effects of class composition.

I use the data of the standardized learning assessment administered in 2010 by the Italian National Evaluation Institute (INVALSI) to the entire student population of 5th (end of primary school) and 6th graders (lower secondary school). Although the assumption of random allocation of students into classes with respect to immigrant background is rejected at the system-level, when performing school level tests, random assignment is rejected only for a minority of institutions. Schools not passing this test are discarded.

In the main body of the paper I follow the common practice of estimating the impact of class composition effects without trying to separate the effects due to peer achievement from other effects related to peer characteristics. As demonstrated by Manski (1993), disentangling them is a very difficult task. Moreover, since both effects are due to social interaction, it is their joint action that is of interest for public policy (Moffitt, 2001). In the last section however, building on the idea developed by Hoxby (2000) to exploit multiple peer variables, I attempt to investigate the different channels by which peer effects operate.

The paper is structured as follows. In Section 2 I presents the model and the identification issues, review and discuss empirical strategies employed in the literature. Section 3 is dedicated to a brief description of the Italian schooling system and of the data. Sections 4 and 5 provide background descriptive evidence on the concentration of immigrant children in schools and achievement gaps. Section 6 is devoted to the empirical issue of random class allocation of immigrant children. Section 7 turns to the analysis of data and to the presentation of the results. Conclusions follow.

2. Theoretical background

2.1 Structural and reduced form model

Since learning in schools takes place in a group setting, the composition of the group may affect individual outcomes. First, *achievement* effects could operate. Students performing poorly might influence others' learning because teachers adjust performance targets and keep the level of the instruction low. Individuals' achievement could also be directly influenced by the achievement of peers: good students may contribute to establish positive competition, while low motivated children may negatively influence others, to the detriment of everybody's learning. Children with an immigrant background are on average lower performing than native students: peer achievement effects operate if they affect the learning of natives (and possibly that of other immigrants) *because* they perform more poorly.

Second, learning could be affected by predetermined *characteristics* of peers. If children from disadvantaged backgrounds receive lower family support as compared to better off children, they may develop negative feelings about schooling, influencing the overall class climate; on the other hand, if recently arrived immigrant families have high aspirations for their children's future, their presence may even be beneficial. In this sense, we could regard parental socio-economic, ethnic and immigrant background composition of classmates as possible proxies for attitudes and behavioral patterns influencing learning that are not captured by performance scores (Hanushek *et al.* 2003).

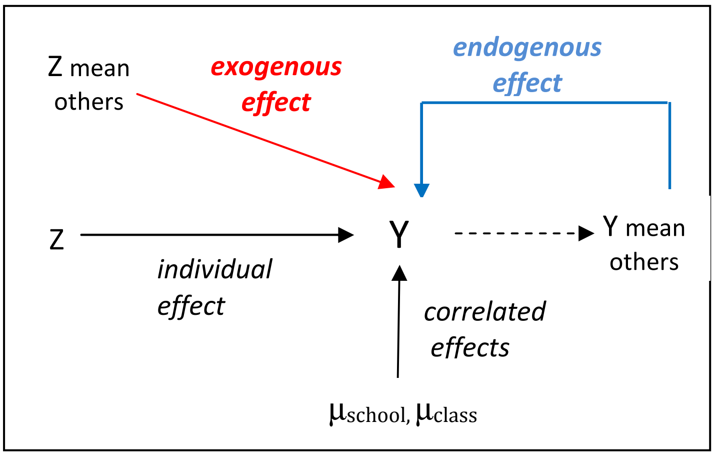
Assume that peer effects act at the class level. Since individuals are nested into classes and classes are nested into schools, the typical theoretical model for individual achievement is:

$$y_{ics} = \alpha + \beta \bar{y}_{(-i)cs} + \gamma \bar{z}_{(-i)cs} + \tau z_{ics} + \mu_s + \mu_{cs} + \varepsilon_{ics} \quad (1)$$

where z denotes individual characteristics. Subscript i represents the individual, c the class and s the school, $\bar{y}_{(-i)cs}$ denotes class average achievement and $\bar{z}_{(-i)cs}$ class average characteristics, all taken excluding individual i. The error term includes a component ε_{ics} capturing individual shocks and components representing unobservables at the class and school levels. Unobserved school-specific effects μ_s are related to organizational features, effectiveness of the principal, school resources. Class-specific effects μ_{cs} capture class teachers' quality.

In the language of the seminal work of Manski (1993), the influence of peer achievement β is the *endogenous effect*; the influence of peer characteristics γ are *exogenous effects*; the effect of being exposed to the same environment, captured by μ_s and μ_{cs}, are *correlated effects*. These mechanisms are depicted in Figure 1.

Figure 1. The structural peer effects model



The effect of peer achievement is endogenous because peer achievement influences the achievement of individual i, but is itself influenced by i's achievement. The existence of feedback effects implies that a change in individual achievement generates a social multiplier, thereby group average achievement changes by a larger amount than that corresponding to the original change. Due to this simultaneity that cannot be solved in standard ways (the "reflection problem"), unless strong restrictions are posited, model (1) is unidentified (Manski, 1993). Thus, disentangling endogenous and exogenous effects is very difficult: however, their joint effect still retains an intrinsic interest because they are both induced by social interaction. Correlated effects, on the other hand, are spurious. In this perspective, empirical work is often based on "reduced form models", where peer characteristics – but not peer achievement – are included as explanatory variables:

$$y_{ics} = \alpha + \tau^* z_{ics} + \gamma^* \bar{z}_{(-i)cs} + \mu_s + \mu_{cs} + \varepsilon_{ics} \tag{2}$$

The parameter of interest is γ^*, which measures class composition effects and captures both endogenous and exogenous effects.[1] Richer versions of the model would include available school characteristics.

2.2 Multilevel modeling

Multilevel analyses are recommended for models that aim at exploring how micro-level variables are affected by micro-level and macro-level variables (Goldstein, 1997; Snijders and Bosker, 1999). Allowing to handle explanatory variables at the student, class and school levels, they are now widely employed in educational research. The effect of immigrant concentration in schools has been the object of a large number of recent papers from the sociological literature using multilevel models (Driessen, 2002; Fekjaer and Birkelund, 2007; Dumay and Dupriwz, 2008; Brannstrom, 2008; Cebolla-Boado and Medina, 2011; Argidarg *et al.*, 2011). However, multilevel models *by themselves* do not address the main empirical problem in the estimation of the effect of school characteristics, including peer effects: how children are allocated to schools.

The error term in model (2) has a school-specific component, a class-specific component and an individual component. This complex structure implies that errors of children in the same class or school are not completely independent. Standard statistical tests leaning on the assumption of

[1] Some technical and rather tedious issues regarding the derivation of the reduced form (2) from the structural model (1) which are apparently neglected in the literature, are discussed in the Appendix. I will make the following points: a) γ^* and τ^* are function of class size, so the reduced form (2) is only an approximation of *true* reduced form if classes have different numerosity; b) a given structural model yields to different reduced form parameters γ^* and τ^* depending on the number of children in the class; c) reduced form coefficients γ^* and τ^* are function of *all* structural coefficients: γ^*captures exogenous and endogenous peer effects, but its magnitude depends also on individual effects; τ^* also differs from the corresponding structural parameter and if endogenous effects are large, the difference between τ^* and τcan be substantial.

independence lead to the underestimation of standard errors; as a consequence many significant results are spurious. Multilevel models tackle this problem by allowing multiple error components embedded in a hierarchical structure. However, these models assume that each component is *uncorrelated* to explanatory variables. But when the allocation of children to schools and classes is not random they yield – just like OLS – to biased estimates. Let us discuss the issue of school allocation (which is more severe), postponing that of class assignment for a later section.

Allocation of children to schools is hardly ever random. In some countries children are required to enroll into the school of the area of residence; in others there is freedom of choice. In the former case, neighborhoods generally differ with respect to residents' social background, immigrant status and so on. If parents are allowed to choose their offspring's school, other effects may add on. Children of the most advantaged backgrounds, having higher aspirations, might favor institutions that ensure better peers (natives, high socio-economic status), and having access to more information, might select higher quality institutions. Hence, school choices are driven by families' observable features (socio-economic status, native or immigrant background) and by unobservable factors (aspirations, attitudes towards immigrants, child innate ability). In addition, especially in those countries with a well developed private sector, school boards may sort students by applying enrolment fees and setting ability related attendance rules.

Multilevel estimation of (2) yields to consistent estimates of peer effects if only features that are observed by the analyst drive the selection process (i.e. only observed characteristics of children and observed characteristics of schools matter). The following conditions must hold:

(a) *There is no relation between the unobserved components of school quality and observable features of the student-body* (μ_s is independent of z and $\bar{z}$)

This condition applies if, regardless of their background, families have no information on school quality or if preferences for school quality do not vary with family background. Note that even if researchers had access to data on organizational aspects of schools, they would generally have no information on teacher quality; instead, this information is usually available to (well informed) parents. Information on school quality is likely to matter even with no freedom of school choice, because families choose the neighborhood to live. Another restriction is that high quality teachers and resources should have no incentive to move towards schools attended by more advantaged (or disadvantaged) children.

(b) *Parents of high innate ability children have the same preferences for peer characteristics of parents of low innate ability children* (ε independent of $\bar{z}$)

If high social origin parents might prefer peers with similar family background no matter how their children perform, disadvantaged origin parents of high innate ability children may be more selective that those of low innate ability: if this is the case, the assumption is not valid.

Summing up, multilevel models tackle the issue of correlated errors (which lead to biased estimates of standard errors), but assuming that school-quality is exogenous, do not help solving the school-selection problem, which leads to biased estimates of peer effects and of the coefficients of the other explanatory variables.

2.3 Accounting for school endogeneity

If children are not randomly allocated to schools, school (and class) characteristics – including the characteristics of peers – cannot be considered exogenous. In the peer effects literature, Rangvid (2007) and Fekjaer and Birkelund (2007) assume that only observables enter the selection process and includes several individual and school variables. Cebolla-Boado (2007) attempts to solve the problem using aggregate levels of deprivation in the area of reference as instrumental variables. To remove school selection issues, Brunello, Rocco (2011) exploit PISA data aggregated at the country level: since immigrants sort across countries and the more developed countries usually host a higher share, they control for between-country immigration flows by conditioning on country fixed effects and on the stock of immigrants in a given country at a given time. Schneeweis, Winter-Ebmer (2005) examining Austrian upper secondary school students, argue that self-selection is mainly driven by the segregation of students in different school-types and employ a school-type fixed effects model.

Other scholars attempt to render school composition an exogenous effect with different identification strategies. Hoxby (2000) controls for selection by exploiting idiosyncratic within-school variation in peer characteristics between adjacent cohorts in given grades. Ammermueller, Pischke (2009) and Lugo (2011) rely instead on differences in the compositions of individual classes within a school. Gould *et al*. (2009) and Black *et al.* (2010) investigate long-term effects of school peers. Gould *et al*. (2009) focus on the immigrant concentration in grade 5 on later educational outcomes in Israel, and account for the endogenous sorting of immigrants across schools by exploiting random variation in the number of immigrants in grade 5, conditional on the total number of immigrants in grades 4-6. Black *et al.* (2010) study post-school and labor-market outcomes, exploiting random variation in cohort composition within schools. Their analyses are not affected by simultaneity issues because the dependent variables are later outcomes and not contemporaneous performance, allowing a clear-cut identification of peer achievement effects.

Hanushek *et al*. (2003) use panel data to estimate peer effects on test score gains over time using student and school-by-grade fixed effects in a value-added specification. Identification is achieved by exploiting the fact that students move from one school to another. They aim to control for endogenous school selection, but also to account for omitted past school and family inputs, which, if neglected, are likely to lead to upward biased estimates of peer effects. The analyses also address the reflection problem, by using past performance as the measure of peer achievement.

In this paper I follow the identification strategy suggested by Ammermueller and Pischke (2009). If children are randomly assigned to *classes*, it is possible to exploit the within-school random variability observed across classes in the peer characteristics variables.[2] Within-school differences are given by:

$$y_{ics} - \bar{y}_s = \tau^*(z_{ics} - \bar{z}_s) + \gamma^*\left(\bar{z}_{(-i)cs} - \bar{z}_s\right) + \left(\mu_{cs} - \bar{\mu}_{cs(s)}\right) + \left(\varepsilon_{ics} - \bar{\varepsilon}_{ics(s)}\right) \qquad (3)$$

Model (3) has the advantage that (observed and unobserved) school variables are removed, overcoming the issue of school-selection. Random assignment ensures that class-specific effects are independent of the characteristics of children and their families. Moreover, this assumption ensures that also the individual error component is independent of peer characteristics, in that even if school choices were related to innate ability, class assignment is not. Unfortunately, as described in section 5, I reject the assumption that random assignment is applied at the system-level, i.e. by *all* schools. However, when carrying out school-level tests, the random assignment hypothesis is accepted for the majority of the institutions; for this reason the analyses are carried out on this subset of schools (see section 6 for a discussion on this strategy).

The class-specific error term is assumed to be a random effect, normally distributed and independent of individual error terms. I also include peer effect related to other variables and allow for heterogeneous immigrant origin peer effects across children of different backgrounds: immigrant or natives and of different socio-economic status.

3. Italian school system and data

3.1 The school system

Formal education starts at age 6. Children follow eight years of comprehensive schooling, divided in two cycles: five years of primary education and three years of lower secondary education. Excluding grade failures and a limited mobility of children across schools, children remain with the same

[2] I can estimate school fixed-effect models because, with the exception of few very small schools, the majority of institutions host multiple classes per grade. Note that from this perspective this paper relies on better data than Ammermueller, Pischke (2009) who use PIRLS, where one, maximum two classes per school are sampled. Since only schools with at least two classes are needed to estimate model (3), this significantly limits their sample size.

classmates and often with the same teachers for each entire cycle. In primary school one to three main teachers are usually in charge of the class. More teachers are involved in lower secondary education. Lower secondary school ends with a nationally-based examination at age 14, after which students choose between a variety of upper secondary educational programs, broadly classified into academic, technical and vocational tracks. There are no ability-related admission restrictions. Education is compulsory up to age 16.

The Italian schooling system is mainly public: in primary and lower secondary school, private institutions host only about 7 and 4% of the student body respectively (MIUR, 2008). There is freedom of school choice; children have the right to attend the neighbourhood's public school, but they may also apply to a different public or private institution. Admission in public schools is normally conditional on the availability of places, and ability restrictions are uncommon, even in private institutions. In practice, the majority of students attend their neighborhood public school; due to urban segregation, schools located in disadvantaged areas mainly recruit students from the lowest family backgrounds, thereby the ethnic and socio-economic composition varies considerably across schools. Classes are formed by school-boards; there are broad national recommendations to ensure within class heterogeneity with respect to students' characteristics and to distribute disadvantaged children evenly across classes, but these recommendations are not binding.

The Italian educational system is inclusive: immigrant students are always placed in regular classes (and not in special classes, as occurs in some countries). However, first generation immigrants are frequently held back to the previous grade, and repetitions are much more common among immigrants that among natives. Italy lacks of an institutionalized body of policies aimed at the integration of migrant background children. Interventions – tackling the reduction of achievement gaps between native and immigrant children, programs of language support addressed to first generation immigrants, training for second language teaching, measures promoting parental and community involvement in schools – are fragmented, and conducted on a voluntary basis by schools and teachers searching for private or local government funds. Notwithstanding the lack of active interventions designed at the national level, the Migrant Integration Policy Index[3] for education for Italy is considered "halfway favorable", and ranks near the European average.

3.2 Immigrant population

Italy has witnessed a sharp rise of the number of immigrants over the last decade. About 2.7% in 2002, at the end of 2010 the share of foreign citizens reached 7.5% of the resident population. These figures include foreigners' children who were born in Italy (the acquisition of the Italian citizenship

[3] www.mipex.eu, produced by the British Council and the Migration Policy Group.

follows the *ius sanguinis*). Yet, despite this increasing trend, the share of immigrant background people is still considerably lower than that of Central European and Anglo-Saxon countries having a longer history of immigration. The large majority of the foreign born (87%) lives in the North and in the Centre, although the share living in the Southern part of the country is now increasing. The largest foreign communities are those from Romania (21%), Albania (11%), Morocco (10%), China (4.5%), Ukraine (4.4%) and Philippines (3%). Altogether these nationalities represent more than half of the foreign population. If older immigrant flows were mainly driven by economic reasons, the number of new permits of stay for family reunion has recently exceeded that of work-related permits, while the number of refugees is still very low. Like other Mediterranean countries, Italy tends to attract immigrants with lower qualifications (EUROSTAT, 2011); however, given the low average educational attainment of Italians, their formal educational level is similar to that of natives (Dustmann et al, 2011).[4]

In the same period, the share of immigrant background children – children with both parents born abroad – has also more than tripled, reaching 8.7% in primary school, 8.5% in lower secondary education and 5.3% in upper secondary education in 2010. The lower share of students in upper secondary school is one of the indicators of their relative disadvantage: drop-out and non-continuation rates among immigrants are much higher than among natives, and a much larger percentage of children entering upper secondary education opt for academically less demanding vocational schools.

3.3 Data

The survey *Indagine sugli Apprendimenti* is a standardized learning assessment conducted by the National Evaluation Institute (INVALSI) on children attending 2nd, 5th and 6th grade.[5] For the first time in 2010 the assessment was administered to the entire populations of children, consisting of approximately 500.000 individuals per grade. Tests cover the domains of Italian (reading comprehension, knowledge of the language, grammar) and math, and have been designed following the experience of international assessments. Similarly to TIMSS and PISA, INVALSI submits to 5th and 6th grade students a questionnaire recording information on living customs, main activities and time use, attitudes towards school and learning, persons living with the child, home possessions. School administrations provide information on parental background characteristics (migrant background, working condition, educational level). School teachers are normally in charge

[4] Italy is a country with a very low share of individual with tertiary education.

[5] A standardized assessment is administered also to eighth grade students, as part of the final lower secondary examination. However, family background information is not collected, so these data cannot be exploited to estimate peer effects.

of test administration. However, in order to keep cheating behavior under control, a random sample of classes (consisting of about 30,000 students) have taken the tests under the supervision of personnel external to the school. These results represent a benchmark to evaluate and correct potential bias in performance scores. Scores are measured by the proportion of correct answers, hence vary between 0 and 1. In line with many other papers in the research field, as a measure of socio-economic status (SES) I use the number of books and the composite index ESCS – Economic, Social and Cultural Status – provided by INVALSI. The relevant information is recorded in the student questionnaire, which is not administered to children attending 2nd grade; for this reason, in this paper I focus on 5th and 6th grade.

4. Immigrant children in Italian schools

Table 1 reports the percentages of first and second generation immigrants in 5th and 6th grades, according to the INVALSI survey data. The country average share is 9-10%, although immigrants are mainly concentrated in the North and Centre, where they represent 11-15% of the student population, more than half of which are of first generation.[6]

Table 1. Student population by immigrant status and macro-area.

	5TH GRADE (PRIMARY SCHOOL)				6TH GRADE (LOWER SECONDARY SCHOOL)			
AREA	Natives	2G	1G	Mis[1]	Natives	2G	1G	Mis[1]
North-West	86.8	5.9	7.3	1.5	85.6	5.1	9.3	1.1
North-East	86.2	5.9	7.9	1.4	84.6	5.3	10.1	1.2
Centre	88.8	4.7	6.5	2.2	87.5	4.2	8.3	1.7
South	97.0	1.4	1.6	2.7	96.7	1.3	2.0	1.9
Islands	96.7	1.6	1.7	3.4	96.3	1.6	2.1	2.8
Total	**91.0**	**4.0**	**5.0**	**2.2**	**90.0**	**3.5**	**6.5**	**1.7**

Elaboration of INVALSI data.

[1] Students with missing immigrant status.

Immigrant children are not evenly distributed across schools (Table 2). They represent less than 25% of the student body in the majority of the schools. Yet, in some institutions the percentage of immigrants is below 10%; in others, most of which located in the North-West, the share goes beyond 40%. This situation reflects the territorial distribution of immigrant background families, housing choices, explicit school preferences on part of the families, but may also involve school board practices. For example, Luciano *et al.* (2009) report that some institutions set significant

[6] These shares are close to the official figures reported by the National Statistical Institute for 2010, according to which the percentage of immigrant origin students is 13.6/13.8 in the North-West (all grades together in primary/lower secondary school), 13.8/13.8 in the North-West, 11.4/11.4 in the Centre, 2.5-2.7 in the South, 2.4-2.6 in the Islands.

barriers to entry to immigrant background students by denying proper information to parents and any form of support to children.

Table 2. School percentage of immigrant background students, by macro-area

	5TH GRADE					6TH GRADE				
% immigrants	NW	NE	Centre	South	Islands	NW	NE	Centre	South	Islands
0	10.7	9.2	13.3	37.3	37.3	7.3	4.7	5.1	23.3	27.9
<10	42.7	34.7	44.9	56.1	56.2	38.8	31.8	41.7	70.2	66.5
10-25	40.7	50.5	38.1	6.1	5.7	44.8	53.6	48.5	6.1	5.2
25-40	4.8	5.3	3.2	0.3	0.8	7.5	9.3	4.3	0.4	0.3
>40	1.1	0.3	0.4	0.1	0.1	1.6	0.7	0.4	0.1	0.1
school mean	10.8	11.5	9.4	3.0	3.1	12.3	13.6	11.4	3.5	3.3
s.d. of school	8.7	7.6	7.4	4.8	4.4	9.6	8.4	7.4	4.3	3.9
overall	13.1	13.7	11.1	3.0	3.3	14.4	15.4	12.5	3.2	3.7
n° schools	1697	1136	1400	1774	1535	1416	982	1031	1221	1175

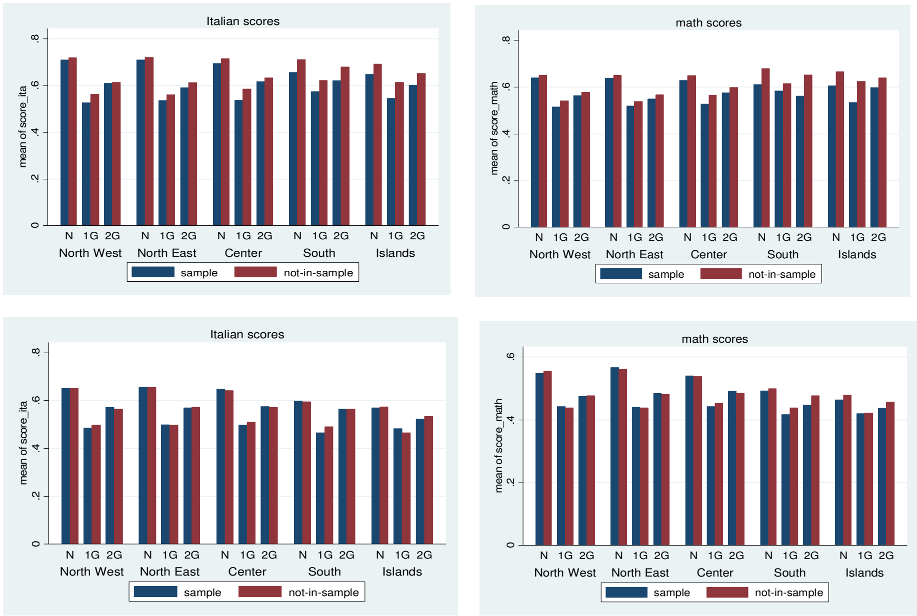
Elaboration of INVALSI data.

Figure 2 shows mean performance scores of native and migrant background students. Sample statistics can be thought as ‘true’ values, while differences between sample and population means reflect cheating. Populations means are generally higher than the benchmark samples. Differences are marked in 5th grade, in particular in the Southern part of the country (but also in the North for first generation migrants), suggesting that teachers may help disadvantage students. Smaller discrepancies between sample and population scores are observed in 6th grade.

Average sample scores of natives and immigrants differ substantially, in particular for first generation immigrants on Italian tests; however, differences are also marked on math tests. Second generation immigrants perform better than first generation ones. Among natives, the scores of students from the South and Islands are substantially lower than the scores of children from the North and Centre, confirming the serious North-South divide already observed in international assessments.

Due to the small number of immigrants living in the South and Islands, I restrict the empirical analyses of the effect of immigrant background class composition to the North and the Centre of the country. This choice is also related to the lower quality of test scores data observed in the South: while cheating is a minor problem in the North (although some adjustments will still be made in the empirical analyses), it is a very relevant issue in the South. Note that it is not possible to rely only on the data of the benchmark sample, of better quality, because the sample does not include more than one class per school, so within-school estimates cannot be obtained.

Figure 2. Mean scores by immigrant background and macro-area.



NOTE. 5th grade upper panel. 6th grade lower panel. Italian left panel. Math right panel

5. Achievement and immigrant concentration: *prima facie* evidence

On average, children attending schools with many immigrants perform more poorly. The correlation coefficients between the percentage of immigrant children and the mean scores of natives, first generation and second generation immigrants are negative and quite large in size (Table 3). These relations are stronger for Italian tests and in 6th grade; stronger for natives than immigrants in the North-West and in the Centre for Italian scores, weaker in the North-East and in the Centre for math scores.

Table 3. School-level correlations between the share of immigrants and mean scores[1]

		5TH GRADE		6TH GRADE	
AREA	MEAN SCORES OF	ITALIAN	MATH	ITALIAN	MATH
North-West	N	-0.14	-0.08	-0.32	-0.26
	2G	-0.11	-0.06	-0.20	-0.15
	1G	-0.12	-0.06	-0.21	-0.13
North-East	N	-0.14	-0.08	-0.15	-0.13
	2G	-0.08	-0.05(ns)	-0.20	-0.15
	1G	-0.15	-0.11	-0.20	-0.20
Centre	N	-0.15	-0.16	-0.04(ns)	-0.00(ns)
	2G	-0.09	-0.08	-0.13	-0.05(ns)
	1G	-0.11	-0.07	-0.20	-0.13

Elaboration of INVALSI data.
NOTE. All correlations but those marked with (ns) are significant at level<0.01.

This *prima facie* evidence is consistent with the hypothesis that high concentrations of immigrants are detrimental to the learning of both natives and immigrant children. However, this is not the only possible story. Institutions hosting many immigrant children on average attract lower social background students, and also social background affects performance. School-level correlations between the share of immigrants and the average SES (as measured by the number of books) of both native and immigrant students are large and negative (Table 4)[7]. These negative associations could be due to the segregation of disadvantaged segments of the society in particular neighborhoods or/and to explicit school choices on part of the families. Distinguishing between these two explanations is not the object of this paper; moreover, this distinction could be meaningless if families made their residential choices by taking school locations into account. Note, however, that for immigrants correlations are considerably higher in 6th grade, approaching the values of natives. Since a strong residential mobility between 5th and 6th grade is highly unlikely, this result suggests that at least in lower secondary school better off immigrant families, like natives, prefer institutions with lower concentrations of foreign students.

Table 4. School level correlations between the % of immigrants and SES

	5TH GRADE		6TH GRADE	
Area	*Natives' Books*	*Immigrants' books*	*Natives' Books*	*Immigrants' books*
North_West	-0.17	-0.11	-0.25	-0.17
North_East	-0.24	-0.11	-0.24	-0.20
Centre	-0.18	-0.11	-0.16	-0.16

Elaboration of INVALSI data.
NOTE. All correlations are significant at level<0.001.

6. Class allocation

Although families are sometimes allowed to express preferences for a particular class, leeway for parental choice is limited. In this sense, we should not expect family choices to represent a major issue at this stage. However, despite broad indications to form classes by maximizing within-class heterogeneity and minimizing between-class differences, there are no binding rules, so some school-boards may allocate children according to different criteria. The assumption of random assignment with respect to immigrant background was tested both at the school-level and at the system-level. Random allocation implies independence between immigrant status and class. At the school-level, the null hypothesis is therefore:

[7] Having computed correlations separately for native and migrant students rules out that the negative figures are merely the result of compositional effects entailed by the lower average SES of immigrants.

$$H_0: p_{mig,c|s} = p_{mig|s} \cdot p_{c|s}$$

where $p_{mig,c|s}$ is the joint probability that a randomly chosen child from a given school s has a migrant background and is assigned to class c, $p_{mig|s}$ is the overall proportion of migrants in the school, and $p_{c|s}$ is the proportion of children in class c. Instead of the classical Pearson X^2 test, due to the limited number of immigrant children in some schools, to avoid problems related to small expected frequencies I use Fisher's exact test.[8] Considering a prudential significance level α=0.10, the null hypothesis is rejected in 20% of the schools for 5th grade and in 22% of the schools for 6th grade. These institutions do not differ with respect to mean SES, but host on average more immigrants than those for which random assignment is accepted.[9]

The null hypothesis of the system-level test is that random assignment regulates class allocation of immigrant children in *all* schools; due to sampling variability some institutions may exhibit substantial deviations from random allocation. Disregarding the problem of small expected frequencies, the test-statistics is the sum of each school X^2 over all schools; under the null hypothesis it follows a χ^2 distribution with $\sum_s(k_s - 1)$ degrees of freedom, where k_s is the number of classes in school s. Random assignment is rejected at the significance level α=0.001, suggesting that at least some schools actually distribute children to classes according to different criteria.[10]

Identification of peer effects rests on the assumption of class random assignment, therefore, similarly to Hoxby (2000) and Lugo (2011), I discard non-random allocating institutions and estimate model (3) only on the schools passing the single-school test. The hypothesis underlying this strategy is that the allocation criterion should not depend on predictions of how peer effects would operate within the specific group of children enrolled in the school.[11]

Let us go back to single-school tests. A significant level α=0.10 means that we have a 10% probability to reject the null hypothesis when it is true, but the probability of accepting the null hypothesis under near alternatives could be large. In other words, the consequence of adopting commonly used low thresholds is to keep in schools that are not really adopting a random allocation criterion, but deviate mildly from it. As a robustness check I run regressions on the set of schools

[8] *p*-values of Fisher's exact test are computed by summing the probabilities under the null hypothesis of all contingency tables having a smaller or equal probability of the observed table.

[9] The average percentage of immigrants in 5th grade is 16.1% in non-random allocating schools and 12.1% in the random-allocating ones; 16.7% vs 13.7% in 6th grade.

[10] The value of the test-statistic is 28.072 and the corresponding chi-square has 19.783 degrees of freedom. Note that, on the contrary, the hypothesis of random assignment with respect to gender is not rejected (test-statistics=16.941).

[11] Assume that there are two sets of immigrant children: the "good" and the "bad", and that if a school is attended mainly by the "good" ones, children are allocated randomly in the classes, while if they are attended mainly by the "bad" ones the sorting is non-random. If the "good" immigrant children do not influence natives' performance while the "bad" ones do, by discarding the latter we would end up underestimating average peer effects.

passing the test at different significance levels, up to α=0.50, but substantive results do not change much and no clear pattern is appreciable.

Besides immigrant status – which is the focus of this paper – we may also consider allocation along the SES dimension. Random allocation implies that at the school level expected average SES is the same in all classes. Approximately 30% of the schools do not pass the ANOVA F-test with respect to the ESCS index[12] at the significant level α=0.10. In this light, I also analyze the subset of schools passing both the immigrant status and the SES random allocation tests.[13]

What if non-random allocating schools were not completely eliminated? In principle, neglecting the departure from random assignment could affect peer estimates in any direction: (a) there would be *no bias* if despite the sorting, teachers were randomly assigned to classes; (b) we would *overestimate* peer effects if higher quality teachers were allocated to the "better" classes (in this case we would erroneously ascribe the effect of better teachers to peers); (c) we would *underestimate* peer effects if higher quality teachers were allocated to the "worse" classes.

The way students and teachers are actually allocated should be a topic of empirical educational research, because little is known about it and it is a relevant issue from the perspective of fostering social cohesion and ensuring equality of opportunity to all children. Notwithstanding the lack of empirical studies, case (c) can be considered highly unlikely in Italy. The rationale for non-random sorting and higher quality teachers allocated to the "worse" classes could be to apply ability streaming (which could drive uneven immigrant status and SES distributions) and assign better resources to those more in need; however, streaming is not a popular pedagogical practice in the Italian compulsory school system. On the other hand: i) some not-explicit sorting by ability could occur; ii) the more informed parents of advantaged backgrounds could manage to place their children with better teachers; iii) better teachers often prefer better students. In this light, if some residual non-randomness was left, it would probably lead to the overestimation of peer effects.

7. Peer effects estimation

7.1 Dependent variables

As dependent variables I use the scores of both Italian and math tests, measured by the proportion of correct answers.[14] Mean scores lay in the range 0.54-0.70 and standard deviations between 0.15

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[12] Since ESCS is approximately normally distributed, it is better suited than the number of books for an ANOVA F-test.

[13] Nearly 60% of the schools pass both random allocation tests at the significance level 0.10.

[14] INVALSI also supplies performance scores computed with Rash analysis, thereby taking into consideration the difficulty of each item (correlation with raw scores is 0.99). Moreover, for 5th grade (not for 6th grade because cheating was limited) the Institute releases scores adjusted for cheating with various statistical techniques (Quintano *et al*, 2009). Even if alternative measures are available, I use raw scores because their significance is clearer and analyses with

and 0.18, depending on the test and the grade. Mean scores are somewhat higher for Italian tests and in 5th grade, while math test scores display a slightly larger variability.

7.2 Explanatory variables

Following the literature, I consider gender, SES and immigrant background as individual determinants of school performance. Gender is included in order to account for the well-established international evidence reporting significant differentials between girls and boys, that vary between mathematics (more favorable for boys) and reading comprehension (more favorable for girls). I use two indicators of the family socio-economic status. The first is the number of books at home, which is regarded in the literature to be a better predictor of student performance than other indicators of family background (Hanushek, Woessmann, 2011). The second is the composite index ESCS – Economic, Social and Cultural Status – provided by INVALSI (Campodifiori *et al.*, 2010), which is based on parental education, occupational status and a number of home possessions.[15] I differentiate between first generation immigrants (children born abroad from two foreign-born parents) and second generation immigrants (children born in Italy from two foreign-born parents); as we have seen in Figure 2 and in line with the international literature, scores differ substantially between them.

I add a variable indicating children repeating a grade (identified as those who are older than the regular age), as these children are usually particularly low performing. This variable includes only natives; immigrant children are not considered here because many of them are older than their classmates – first generation migrants are often held back in earlier grades (Gavosto, 2010) and the share of immigrant background students failing to pass to the school-year is larger than that of natives – and since the focus of the empirical analysis is to estimate the effect of immigrant concentration, their inclusion would capture part of the effect of interest.

To control for cheating, I include a binary variable distinguishing children in the benchmark sample, who took the tests under the supervision of personnel external to the school. This variable has also been interacted with dummy variables indicating first and second generation immigrant children, to account for the evidence that immigrant children could be given more (or less) help than natives.

As regards peer variables, I consider variables accounting for gender, SES, repeating grade and immigrant background class composition. Peer gender effects have been addressed by Lavy and

adjusted scores yield to odd results on peer effects. I take cheating under control with the simpler and more transparent way of including dummy variables distinguishing sample and population children.

[15] ESCS follows the lines of PISA's homologous index; it is based on a similar methodology and variables. It is obviously correlated with the number of books (also because the number of books is one of the variables entering ESCS); correlation coefficient is 0.48.

Schlosser (2007), who find that an increase in the proportion of girls leads to a significant improvement in students' cognitive outcomes. Similar results are reported by Hoxby (2000). The importance of peer effects related to the socio-economic background has been documented by many studies in the peer effects literature.

To account for the effect of immigrant students I consider the proportion of first and second immigrants. Since first generation immigrants may have language problems and get consistently lower scores than those of second generation, I allow these two groups to have different effects. In addition, I test the assumption of heterogeneous effects of immigrant concentration on children of different backgrounds, by including variables interacting each of the immigrant background peer variables with native status (to distinguish between the effect of immigrant concentration on immigrants and natives), and with both native status and individual SES (to allow for different effects on natives, according to their SES). Individual and peer variables included in the regressions are summarized in Table 5.

Table 5. Description of dependent and explanatory variables.

DEPENDENT VARIABLES					
VARIABLE	DESCRIPTION	MEAN 5TH	S.D. 5TH	MEAN 6TH	S.D. 6TH
Score Italian	Percentage correct answers Italian test	0.70	0.17	0.63	0.15
Score math	Percentage correct answers math test	0.64	0.18	0.54	0.18
EXPLANATORY VARIABLES					
Individual characteristics					
VARIABLE	DESCRIPTION	MEAN 5TH	S.D. 5TH	MEAN 6TH	S.D. 6TH
Female	Gender	0.49		0.48	
SES-Books	N° of books at home [1]	2.06	1.18	2.10	1.20
SES-ESCS	Economic Social and Cultural Status	0.11	0.96	0.14	0.97
Repeat	Native repeating grade	0.006		0.032	
1G	First generation migrant	0.073		0.093	
2G	Second generation migrant	0.056		0.049	
Sample	Child in sampled class	0.075		0.080	
1G*sample	First gen. migrant child in sampled class	0.005		0.007	
2G*sample	Second gen. migrant child in sampled class	0.004		0.004	
Class peer characteristics					
VARIABLE	DESCRIPTION	MEAN 5TH	S.D. 5TH	MEAN 6TH	S.D. 6TH
p. Female	Proportion of females	0.49	0.11	0.48	0.11
Mean Books	Mean n° of books at home[1]	2.06	0.45	2.10	0.43
Mean ESCS	Mean ESCS	0.11	0.46	0.14	0.47
p. Repeat	Proportion of natives-repeating grade	0.006	0.020	0.032	0.045
p. 1G	Proportion of first gen. migrants	0.073	0.081	0.093	0.092
p. 2G	Proportion of first gen. migrants	0.056	0.070	0.049	0.062
p. 1G*nat	Native child * prop. first generation migrants	0.059	0.075	0.074	0.085
p. 2G*nat	Native child * prop. second gen. migrants	0.045	0.064	0.039	0.055
p. 1G*nat*SES	Native child * n° books * prop. first gen. mig.	0.123	0.190	0.159	0.224
p. 2G*nat*SES	Native child * n° books * prop. sec. gen. mig.	0.097	0.162	0.086	0.144

NOTE. Standard deviation is not reported for binary variables
[1] 0=0-10 books; 1=11-25 books; 2=26-100 books; 3=101-200 books;4=>200 books

7.3 Results

Maximum likelihood estimates of within-school models (3) including schools passing both random class allocation tests with respect to immigrant background and ESCS at the level α=0.10 are reported in Table 6.

Table 6. Determinants of individual performance.

	5th grade Italian	*5th grade Math*	*6th grade Italian*	*6th grade Math*
Individual variables				
Female	0.012***	-0.039***	0.012***	-0.026***
Books	0.021***	0.020***	0.019***	0.020***
ESCS	0.031***	0.030***	0.028***	0.030***
1gen mig (ref native)	-0.120***	-0.074***	-0.103***	-0.070***
2gen mig (ref native)	-0.067***	-0.044***	-0.056***	-0.057***
Repeat grade *native	-0.134***	-0.141***	-0.091***	-0.120***
Sampled class	-0.005	-0.004	0.002	0.002
Sampled class *1gen mig	-0.020**	-0.009	-0.005	-0.001
Sampled class *2gen mig	-0.005	-0.002	-0.005	-0.008
Peer variables at class level				
% Females	-0.002	0.004	-0.001	0.005
Mean ESCS	0.006	0.005	0.001	0.005
% native repeating grade	0.025	-0.003	-0.016	-0.022
% 1gen mig	-0.087***	-0.054**	-0.034**	-0.003
%1gen mig * native	0.054**	-0.003	0.025	-0.015
% 1gen mig *native*books	-0.003	0.006	0.005	0.006
% 2gen mig	-0.097***	-0.009	-0.059***	-0.022
% 2gen mig * native	0.032	-0.060**	0.027	-0.047
% 2gen mig *native*books	0.018**	0.030***	0.015**	0.033***
VAR(BETW CLASSES)/VAR(TOT)	0.051***	0.087***	0.010***	0.021***
N° CHILDREN	122244	126187	141390	141487
N° CLASSES	7232	7305	7428	7425
N° SCHOOLS	1756	1756	1780	1780

NOTE. Estimates are based on the subset of schools passing the immigrant and SES allocation tests at the level α=0.10. Classes with at least 10 children without missing values on all explanatory variables, schools with at least 20 children and 2 classes, and less than 20% of children with unknown native/immigrant origin.

* p-value<0.05, **p-value<0.01, ***p-value<0.001

Individual characteristics strongly affect achievement. In line with international results, females perform significantly better in Italian and worse in math. Children of the highest SES levels obtain much better scores than those belonging to the lowest strata, and the coefficients of both indicators, the number of books and ESCS, are large and highly significant. The achievement of native students repeating the grade is much lower than that of regular students. Immigrant children perform more poorly that natives; first generation immigrants are particularly disadvantaged, as the percentage of questions answered correctly is 7-12 points below that of natives. Not surprisingly, gaps are larger for Italian tests.

Moving to peer variables, we observe that the share of females is never statistically significant. SES is also not significant[16]: however, this result is not robust to specification changes and to the set of schools excluded from the analysis (see next section). Similar finding hold for the share of native children repeating the grade.

The effects of immigrant concentration – linear combinations of the coefficients of main effects and interaction effects – are shown in Table 7. The share of immigrant background children does appear to influence achievement. Yet, effects are heterogeneous and generally small. Immigrant children's achievement is negatively affected as regards Italian, in particular in 5th grade, while it is not for math (with the exception of first generation immigrants in 5th grade). Low SES native children's scores are negatively affected in particular by the share of second generation students. On the other hand, high SES native children even seem to benefit from the presence of second generation immigrant peers.

In addition to statistical significance, we should focus on the magnitude of these effects. The highest figure reported in Table 7 is -0.085, the effect of the proportion of first generation immigrants on the Italian test scores of immigrant students in 5th grade. Since this share varies in principle between 0 (no immigrants) and 1 (all immigrants), what this number says is that a 10 percentage point increase in the proportion of first generation immigrants lowers the average percentage of correct answers by less than 1 point, approximately 1/20th of the population standard deviation. Although not negligible, this is indeed a *weak* effect.

Table 7. Effects of immigrant background class composition

	5TH GRADE ITALIAN	5TH GRADE MATH	6TH GRADE ITALIAN	6TH GRADE MATH
Effect of first gen. immigrants on:				
Immigrants	-0.085***	-0.045***	-0.035**	-0.005
natives Books=0	-0.037**	-0.045***	+0.002	-0.005
natives Books =2	-0.037**	-0.045***	+0.002	-0.005
natives Books =4	-0.037**	-0.045***	+0.002	-0.005
Effect of second gen. immigrants on:				
Immigrants	-0.075***	-0.009	-0.046***	-0.021
natives Books =0	-0.075***	-0.071***	-0.046***	-0.072***
natives Books =2	-0.029*	-0.009	-0.005	-0.002
natives Books =4	+0.017	+0.053**	+0.036**	+0.067***

NOTE. According to point estimates of the models with only significant immigrant background peer effects.

[16] I report only results relative to mean ESCS because, although not significant, has a smaller *p*-values than the mean number of books. On the other hand, only the number of books is significant when interacted with immigrant background peer effects.

7.4 Robustness checks

Results summarized in the previous section refer to the subset of schools passing both random allocation tests – with respect to immigrant status and ESCS, at the significance level α=0.10. In order to evaluate the extent to which results are dependent on the subset of schools employed for the analyses, I make a number of the robustness checks. First, focusing on the immigrant random allocation test only, I analyze various sets of schools passing the test at different significance levels (up to α=0.50). Second, I raise the threshold of both the immigrant status and the ESCS test. Although some changes regarding immigrant background peer effects – the focus of this paper – are found, no clear pattern is appreciable and the substantive conclusions remain the same.[17]

On the other hand, average class SES coefficients are subject to relevant changes: they are positive and significant if we choose the schools to analyze on the basis of the immigrant background random allocation test, but lose significance (as appears in Table 6) when we restrict to schools passing the SES random allocation test as well. Consider however that SES is likely to be affected by measurement error, and in this case SES peer effects are underestimated (Ammermueller, Pischke, 2009). In this perspective, the potential bias due to including SES non-random allocating schools (presumably conducting to the overestimation of SES peer effects, see the discussion in section 6) might in fact counterbalance the bias due to measurement error. With this in mind, caution should be applied when interpreting the estimates of SES peer effects.

I also estimate different specifications of the model allowing for non-linear effects of the class share of immigrant origin children, but since the results are not particularly insightful, I do not show them here.

7.5 Achievement or characteristics of peers?

Despite the well known difficulties due to the reflection problem (Manski, 1993), some scholars attempt to disentangle the effects due to peer achievement and peer characteristics. Sacerdote (2000) examines peer effects of college roommates in a very simple setting, with random assignment and only two-people peer groups and shows that in this case the effects are identified. Entorf and Lauk (2008) start from a pure endogenous effects model and derive "social multipliers", summarising the overall impact of exogenous changes in individual or school characteristics.[18]

Exploiting multiple peer exogenous variables, Hoxby (2000) translates the estimates of reduced form coefficients into what she calls "a common basis for achievement effects", i.e. the

[17] These results are not shown here but are available upon request. Also note that when restricting the analyses to schools *not* passing the immigrant background test, immigrant background peer effects are often larger.
[18] The authors acknowledge that estimates of the pure endogenous model are biased because of the reflection problem.

implied estimates of the endogenous effect from each peer variable, under the assumption that only endogenous effects are at work. Since these implied estimates vary substantially, she concludes that not only achievement effects operate, but also peer characteristics. I draw this idea from her, and follow her line of reasoning quite closely. However, I develop a more explicit formalization of the method, obtaining an interpretation of the results that conflicts with hers.

The functions linking the coefficients of the reduced form to structural parameters have been derived in Appendix B. It can be easily demonstrated that in a model with *k*=1..*K* peer variables, (setting class size to its average value) each couple of parameters (γ_k, τ_k) takes the form:

$$\gamma_k^* = \frac{\gamma_k+\beta\tau_k}{1-\beta}\frac{n_{cs}-1}{n_{cs}-1+\beta} \tag{4}$$

$$\tau_k^* = \tau_k\frac{(n_{cs}-1)}{(n_{cs}-1+\beta)} + \frac{(\gamma_k+\tau_k)\beta}{(1-\beta)(n_{cs}-1+\beta)} \tag{5}$$

Note that achievement effects are governed by one single parameter β. This means that if achievement effects operate, they must be the same no matter if a given change in test-scores is induced by, say, an increase in the share of females or in the share of immigrants.

Under the assumption that exogenous effects γ_k are nil, β and τ_k are identified. From equations (4) and (5) we derive that:

$$\breve{\beta}_k = \frac{\gamma_k^*(n_{cs}-1)}{(n_{cs}-2)\gamma_k^*+(n_{cs}-1)\tau_k^*} \cong \frac{\gamma_k^*}{\gamma_k^*+\tau_k^*} \tag{6}$$

For β to be meaningful it must be non-negative and smaller than 1, so we should expect estimated reduced form effects to be either both positive or both negative. In this case, the larger γ^* with respect to τ^*, the larger the implied β. Moderate individual level gaps may have a large impact on peers if achievement effects are strong; on the other hand, if β is small even large individual gaps could have small effects on peers.[19]

Consider the following question:

Since $\breve{\beta}_k$ are the implied values of β if only endogenous effects operated, (disregarding sampling variability) could we take them as upper bounds for β?

[19] To my understanding, Hoxby derives the "common basis for achievement effects" by dividing γ_k^* by τ_k^*, and interprets the result as if it was an estimate of β. However, according to equation (6), $1/\breve{\beta}_k \cong 1+\frac{\tau_k^*}{\gamma_k^*}$, hence $\frac{\gamma_k^*}{\tau_k^*} \cong \frac{\breve{\beta}_k}{1-\breve{\beta}_k}$. This would explain some of her findings, that she had interpreted as odd results. Values far larger than 1 are suspect if we interpret them as $\breve{\beta}_k$, but they are no longer anomalous if they represent $\frac{\breve{\beta}_k}{1-\breve{\beta}_k}$ (for example, Hoxby finds $\frac{\gamma_k^*}{\tau_k^*}$=6 for some variable; this implies that $\breve{\beta}_k$ is approximately 0.86, which is a large but acceptable value).

First note that the empirical result $(\gamma_k^* \geq 0, \tau_k^* \geq 0)$ does not imply $(\gamma_k \geq 0, \tau_k \geq 0)$, and $(\gamma_k^* \leq 0, \tau_k^* \leq 0)$ does not imply $(\gamma_k \leq 0, \tau_k \leq 0)$.[20] However, if structural parameters γ_k and τ_k are either both negative or positive, then also γ_k^* and τ_k^* have the same sign and $0 \leq \beta \leq \check{\beta}_k$.[21] This result does not hold if structural parameters have opposite signs.

What is the meaning of $(\gamma_k \geq 0, \tau_k \leq 0)$? Individuals with larger z_k perform more poorly, while a group of peers with large z_k has a positive influence on learning. As suggested in some empirical research, this might be the case of gender effects on math scores; although females score lower than males, a peer group with many females may foster learning. In this case peer effects related to the share of females cannot be entirely driven by achievement effects: if females are lower performing than males, they should *negatively* affect others' learning. In this case γ_k cannot be nil and $\check{\beta}_k$ does not represent un upper bound for β. Note that although the empirical finding that γ_k^* and τ_k^* have opposite signs does not imply the corresponding result for structural parameters, it obviously does suggest that the case is possible.

In conclusion, focusing on the explanatory variables for which we may assume that individual and peer effects operate in the same direction, $\check{\beta}_k$ represent *upper* bounds for β. As a consequence, we are able to find *lower* bounds for the exogenous effects.

To simplify the illustration, I use the estimates of a school fixed-effect model with no interactions involving peer variables. For the reason exposed above, I do not consider gender peer effects. Instead, for immigrant and socio-economic background I assume that individual and peer exogenous effects have the same sign, disregarding the possibility that immigrants could actually foster instead of hamper average learning. Since with this simple specification there are no significant immigrant background peer effects in 6th grade, I limit the example to 5th grade.[22]

Refer to Table 8, and take Italian scores in the upper panel as an example. Columns (1) and (2) report reduced form estimated values. In column (3) we show the implied values of β under the assumption that γ_k=0, according to equation (6). In column (4) I report the smallest $\check{\beta}_k$. Since β must be smaller than *all* the estimated upper bounds, $\beta \leq$0.145. If 0.145 were the "true" value of β, an exogenous increase in peer scores of 1 percentage points would lead to an increase of almost 0.15 percentage points in individuals scores. We conclude that if endogenous effects operate, they must be weak.

[20] All these results are trivial consequences of equation (A.6) in Appendix B.

[21] Similarly, if $\beta = 0$ then $\gamma_k = \gamma_k^*$. So, if γ_k and τ_k have the same sign, either $0 \leq \gamma_k \leq \gamma_k^*$ or $\gamma_k^* \leq \gamma_k \leq 0$; if they have different signs, these limits do not hold.

[22] An in Section 7, the analysis has been done on schools passing both immigrant background and SES random allocation tests at the significance level α =0.10.

Table 8. Results on endogenous and exogenous effects.

SCORES	PEER VARIABLE	(1) τ_k^*	(2) γ_k^*	(3) $\breve{\beta}_k$	(4) $\breve{\beta}$	(5) $\breve{\tau}_k$	(6) $\breve{\gamma}_k$
5th grade – Italian	1G	-0.1433	-0.0453	0.243	0.145	-0.1430	-0.0182
	2G	-0.0832	-0.0379	0.318		-0.0829	-0.0206
	ESCS	0.0439	0.0074	0.145		0.0438	0
5th grade – Math	1G	-0.0892	-0.0451	0.342	0.152	-0.0888	-0.0250
	2G	-0.0520	-0.0010	0.163		-0.0519	-0.0006
	ESCS	0.0427	0.0076	0.152		0.0426	0

NOTE. Average class size is approximately 20 in 5th grade and 22 in 6th grade.

As a final exercise, I take for good this value of β (an *upper* bound), and use it to evaluate *lower* bounds for γ_k. Column (5) reports implied individual effects (in this case almost identical to the corresponding reduced form estimates). Column (6) shows the implied estimates of exogenous peer effects. $\breve{\gamma}_k$ is set to 0 for the variable displaying the smallest $\breve{\beta}_k$. Hence, we are considering what would happen if peer effects of this variable were entirely driven by achievement differentials. The other $\breve{\gamma}_k$ have the same sign of the corresponding reduced form parameter, and their absolute value falls between 0 and that of γ_k^*; their values reflect the larger size of these exogenous effects with respect to the benchmark variable.

For Italian scores, assuming an endogenous effect of 0.145, a 10 percentage points increase in the share of first generation immigrants – on top of the effects that could be ascribed to their lower performance – would yield to a reduction of 0.18 points in test scores. A similar effect regards the share of second generation students. For math scores, the proportion of first generation immigrants seems to have larger adverse exogenous effects than average class ESCS, while that of children of second generation does not.

Conclusions and discussion

The considerable growth of the share of immigrant students which has occurred over the last decade has contributed to raise the concern within large sectors of the public opinion that immigrant children would have a negative influence on the school performance of natives. However, this concern does not seem to be empirically well founded. The analyses carried out in this paper point to the existence of *negative* effects of the concentration of immigrant students on peer performance; yet, these effects are *small* and *heterogeneous*. As regards Italian tests, the concentration of first generation immigrant students appears to influence immigrants more than natives. Among natives, while low SES children may suffer somewhat from a large share of immigrant background classmates, high SES children do not; on the contrary, in some cases they even seem to benefit from the presence of immigrants.

The identification strategy adopted in this paper rests on the assumption of random class assignment: consequences of a possible residual non-randomness are discussed in section 6 and point to the overestimation of family background peer effects. I can think of two additional potential sources of bias: omitted variables and measurement error. Regarding the first, Hanushek *et al.* (2003) demonstrate that peer effects are overestimated when historical family and school inputs are neglected. As for the second, Ammermueller and Pischke (2009) show that measurement error in the family background variables leads to the underestimation of the corresponding peer effects; yet, they focus on the number of books at home, which have a large likelihood of incorrect reporting. Although the complexity of the model does not allow to make precise predictions, if the immigrant origin is not subject to measurement error, the underestimation of peer effects related to the number of books should yield to the overestimation of peer effects related to immigrant background. In this light, my overall conclusion is that the estimates obtained in this paper are likely to represent upper bounds of immigrant origin peer effects.

Two major conclusions can be drawn: (i) the concentration of immigrant children in schools should not be an issue of major concern as there is little evidence of substantial detrimental effects on students' learning; (ii) still, since disadvantaged children (immigrants or low SES) are somewhat affected, children should be allocated into schools and classes according to the principle of maximum family background heterogeneity.

On the other hand, the relative disadvantage of immigrant children at the individual level is large and needs to be urgently addressed with adequate integration policies – severely lacking in Italy – aimed at ensuring equality of opportunity to all children and at fostering social cohesion.

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APPENDIX A. Children investigated by the INVALSI survey

Table A1. Population and sample size by immigrant background and macro-area

5TH GRADE						
	BENCHMARK SAMPLE[1]			TOTAL POPULATION		
AREA	Natives	Mig 2°	Mig 1°	Natives	Mig 2°	Mig 1°
North_West	5951	353	443	109002	7435	9174
North_East	6590	492	576	76956	5305	7006
Centre	6746	397	496	81932	4366	5955
South	7101	149	158	110918	1643	1862
Islands	6117	97	108	78867	1288	1423
TOTAL	32505	1492	1781	457675	20035	25420
6TH GRADE						
	BENCHMARK SAMPLE[1]			TOTAL POPULATION		
AREA	Natives	Mig 2°	Mig 1°	Natives	Mig 2°	Mig 1°
North_West	6596	422	720	110035	6563	11997
North_East	7120	495	872	77314	4868	9207
Centre	7594	387	757	83395	3976	7928
South	8453	135	186	111509	1467	2265
Islands	7337	121	150	80137	1298	1778
TOTAL	37100	1560	2685	462390	18172	33175

NOTE. Excluding students with missing immigrant status .

[1]. Students belonging to classes with external supervision during test administration.

APPENDIX B. Derivation of the reduced form from the structural model

In this section I derive the reduced form from the structural model (1). I present two results:

(i) The commonly employed reduced form is just an approximation of the "true" reduced form;

(ii) For explanatory variables entering the model with both individual and peer effects, the reduced form coefficient of individual effects is not equal to that of the structural form.

From the structural model:

$$y_{ics} = \alpha + \beta \bar{y}_{(-i)cs} + \gamma \bar{z}_{(-i)cs} + \tau z_{ics} + \mu_s + \mu_{cs} + \varepsilon_{ics} \tag{A.1}$$

we obtain the school mean score:

$$\bar{y}_s = \alpha + \beta \bar{y}_s + \gamma \bar{z}_s + \tau \bar{z}_s + \mu_s + \bar{\mu}_{cs(s)} + \bar{\varepsilon}_{ics(s)} \tag{A.2}$$

where $\bar{\mu}_{cs(s)}$ is the average of class effects in school *s*, and $\bar{\varepsilon}_{ics(s)}$ is the average of individual effects in school *s*, Equation (A.2) implies that:

$$\bar{y}_s = \frac{\alpha}{(1-\beta)} + \frac{\gamma+\tau}{(1-\beta)} \bar{z}_s + \frac{1}{(1-\beta)} \left(\mu_s + \bar{\mu}_{cs(s)} + \bar{\varepsilon}_{ics(s)} \right) \tag{A.3}$$

Similarly, the class mean score is given by:

$$\bar{y}_{cs} = \frac{\alpha}{(1-\beta)} + \frac{\gamma+\tau}{(1-\beta)} \bar{z}_{cs} + \frac{1}{(1-\beta)} \left(\mu_s + \mu_{cs} + \bar{\varepsilon}_{ics(cs)} \right) \tag{A.4}$$

where $\bar{\varepsilon}_{ics(cs)}$ is the average of individual effects in class *c*, school *s*.

The term $\bar{y}_{(-i)cs}$ in the structural model can be written as follows:

$$\bar{y}_{(-i)cs} = \frac{\bar{y}_{cs} n_{cs} - y_{ics}}{n_{cs}-1} \tag{A.5}$$

where n_{cs} is the class size, and similar formulas hold for $\bar{z}_{(-i)cs}$,

Including (A.4) in (A.5), and then the resulting expression in the structural model, we obtain the following:

$$y_{ics} = \frac{\alpha}{1-\beta} + \frac{\gamma+\beta\tau}{1-\beta} \frac{n_{cs}-1}{n_{cs}-1+\beta} \bar{z}_{(-i)cs} + \left[\tau \frac{(n_{cs}-1)}{(n_{cs}-1+\beta)} + \frac{(\gamma+\tau)\beta}{(1-\beta)(n_{cs}-1+\beta)} \right] z_{ics} + \frac{1}{1-\beta} \mu_s + \frac{1}{1-\beta} \mu_{cs} + \frac{\beta}{1-\beta} \frac{n_{cs}}{n_{cs}-1+\beta} \bar{\varepsilon}_{ics(cs)} + \frac{n_{cs}-1}{n_{cs}-1+\beta} \varepsilon_{ics} \tag{A.6}$$

which is formally equivalent to:

$$y_{ics} = \alpha^* + \gamma^*(n_{cs}) \bar{z}_{(-i)cs} + \tau^*(n_{cs}) z_{ics} + \mu_s^* + \mu_{cs}^*(n_{cs}) + \varepsilon_{ics}^*(n_{cs})$$

The equivalence of (A.6) with the typical reduced form holds only if class size is constant, otherwise regression coefficients vary (deterministically) over individuals. Also note that the class effect μ_{cs}^* is a function of the structural class effect and of the class average of individual error terms. Due to this last component, the resulting reduced form class-specific effect is not nil even with no structural class effect; it is independent of other explanatory variables, and can be handled with conventional random effect models.

The reduced-form coefficient of peer characteristics γ^* depends on the structural effects of peer ability β and of peer characteristics γ, but also on the structural effect of individual characteristics τ. This a well established result. On the other hand, a result that to my knowledge has not been highlighted in the literature is that the reduced-form coefficient of individual characteristics τ^* is not equal to the corresponding structural coefficient τ. The first term of τ^* approaches τ, but the second can be non-negligible if β and either γ or τ are large (the upper bound as β approaches 1 is $\frac{\gamma+\tau}{n_{cs}}$), and in this case it can vary substantially with class size.[23] Why is it so? While the structural τ captures only the direct effect of individual characteristics z (taking mean peer ability and characteristics as given), the reduced form τ^* also captures an indirect effect triggered by endogenous effects. As z directly affects student i's own performance, in the model for student j peer performance will also change (because i is among j's peers). Consequently j's performance will be affected, yielding to a further change in i's performance (Figure 2).

To conclude, the commonly employed reduced form is just an approximation of the true reduced form (A.6), because it does not acknowledge that parameters vary with class size. What happens if we ignore this variability? I have explored the consequences in a heterogeneous class size environment with a small simulation study. For the range of parameters I considered – suggested by the actual estimates of model (3) – consequences are small, but in order to come up with more general results this issue should be investigated more in depth.[24]

[23] On the other hand γ^* varies little with class size, as the multiplicative factor $\frac{n_{cs}-1}{n_{cs}-1+\beta}$ is close to 1 for reasonable n_{cs}.

[24] These results are not shown in the paper but are available from the author upon request.