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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 weak effect on child learning outcomes, and that this effect is somewhat larger for children from disadvantaged backgrounds (immigrants and low socio-economic status).

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. Not surprisingly, 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: Fekjaer and Birkelund (2007) on Norwegian upper secondary education, Cebolla-Boado (2007) on French lower secondary school, Cebolla-Boado and Medina (2011) on Spanish primary education, Brannstrom (2008) on Swedish upper secondary education. Lugo (2011), instead, focuses on

primary and secondary school in Argentina. Overall, they all report small negative peer effects related to immigrant background and substantive social background effects. 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, finding evidence of small but significant negative effects, increasing with the level of segregation of immigrants.

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 χ^2 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:

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

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 model*
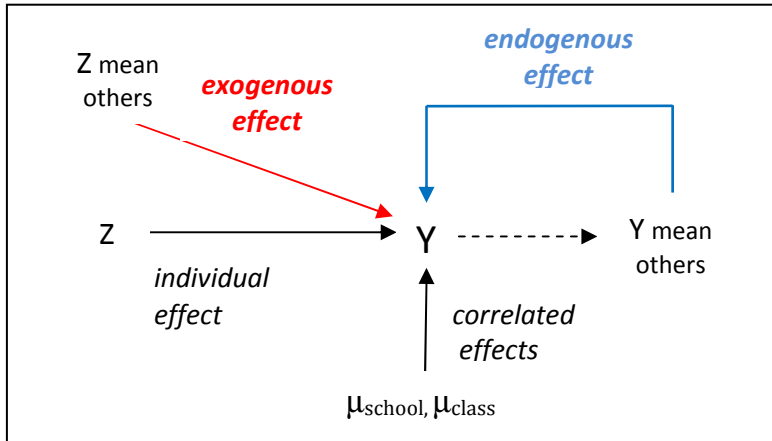
The effect of peer achievement is endogenous because peer achievement influences the achievement of individual i, but is itself influenced by i's achievement. The existence of feedback effects implies that a change in individual achievement generates a social multiplier, thereby group average achievement 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 number of recent papers from the sociological literature using multilevel models (Fekjaer, Birkelund, 2007; Brannstrom, 2008; Cebolla-Boado, Medina, 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 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'

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[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 τ can be substantial.

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) assumes that only observables enter the selection process and includes several individual and school variables, while Cebolla-Boado (2007) employs instrumental variables estimation. 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

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[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.

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 with respect to immigrant background 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 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 immigrants represented 7.5% of the resident population. The large majority of them (87%) lives in the North and in the Centre, and the main countries of origin are Romania, Albania, Morocco, China and Philippines. Despite this increasing trend, the share of immigrant background people is still considerably lower than that of Central European and Anglo-Saxon countries, which have a longer history of immigration. Immigrants living in Italy are on average less qualified than in the rest of Europe; however, 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 has also more than tripled, reaching in 2010 8.7% in primary school, 8.5% in lower secondary education and 5.3% in upper secondary education. 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

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

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 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 they vary between 0 and 1.[6] In line with many other papers in the research field, I use the number of books as a measure of socio-economic status (SES). This information is recorded in the student questionnaire, which is not submitted to children attending 2nd grade; for this reason, in this paper I focus on 5th and 6th grade.

4. Immigrant children in 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.[7]

Table 1. Student migrant background, by macro-area.

	5TH GRADE (PRIMARY SCHOOL)				6TH GRADE (LOWER SECONDARY SCHOOL)			
AREA	Natives	Mig 2°	Mig 1°	*Mis*	Natives	Mig 2°	Mig 1°	*Mis*
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**

* The "*Mis*" column refers to students who have taken the tests, but whose migrant status is not available.

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[6] INVALSI also supplies performance scores computed with Rash analysis, thereby taking into consideration the difficulty of each item (correlation with raw scores 0.99). Moreover, for 5th grade computes scores adjusted to account for cheating, not for 6th grade because cheating was limited (Quintano *et al*, 2009). I use raw scores because their significance is clearer and analyses with adjusted scores yield to odd results on peer effects. See also footnote 14.

[7] 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.

Immigrant children are not evenly distributed across schools (Table 2). In the majority of the schools they represent less than 25% of the student body. 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 barriers to entry to immigrant background students by denying proper information to parents and any form of support to children.

Table 2. School concentration of migrant background students, by macro-area

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

Tables 3 and 4 report 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 their sample counterparts: 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 help disadvantage students. Only small discrepancies between sample and population scores are observed instead in 6th grade. Standard deviations (not reported here) vary between 0.15 and 0.20, depending on the test, the grade, and the area (instead, they do not vary between sample and population).

Table 3. Mean scores by migrant background and macro-area. 5th grade

ITALIAN						
	SAMPLE			POPULATION		
AREA	Natives	Mig 2°	Mig 1°	Natives	Mig 2°	Mig 1°
North_West	0.72	0.61	0.52	0.72	0.61	0.56
North_East	0.71	0.59	0.54	0.72	0.61	0.56
Centre	0.69	0.62	0.54	0.71	0.63	0.58
South	0.63	0.55	0.55	0.71	0.67	0.62
Islands	0.63	0.57	0.51	0.69	0.65	0.61
MATHEMATICS						
	SAMPLE			POPULATION		
AREA	Natives	Mig 2°	Mig 1°	Natives	Mig 2°	Mig 1°
North_West	0.65	0.57	0.51	0.65	0.58	0.54
North_East	0.63	0.55	0.52	0.65	0.57	0.54
Centre	0.63	0.58	0.53	0.65	0.60	0.56
South	0.61	0.51	0.58	0.67	0.64	0.61
Islands	0.58	0.56	0.50	0.66	0.64	0.62

Table 4. Mean scores by migrant background and macro-area. 6th grade

ITALIAN						
	SAMPLE			POPULATION		
AREA	Natives	Mig 2°	Mig 1°	Natives	Mig 2°	Mig 1°
North_West	0.65	0.56	0.49	0.65	0.57	0.50
North_East	0.66	0.56	0.50	0.66	0.57	0.50
Centre	0.64	0.57	0.50	0.64	0.57	0.51
South	0.59	0.55	0.48	0.60	0.56	0.49
Islands	0.55	0.50	0.48	0.57	0.53	0.47
MATHEMATICS						
	SAMPLE			POPULATION		
AREA	Natives	Mig 2°	Mig 1°	Natives	Mig 2°	Mig 1°
North_West	0.55	0.46	0.45	0.55	0.48	0.44
North_East	0.57	0.48	0.45	0.56	0.48	0.44
Centre	0.53	0.48	0.44	0.54	0.49	0.45
South	0.49	0.44	0.43	0.50	0.47	0.44
Islands	0.45	0.41	0.42	0.48	0.45	0.42

Average sample scores of natives and immigrants differ substantially, in particular for first generation migrants on Italian tests; however, differences are also marked on math tests. Second generation migrants 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 in children's learning 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.

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 5). 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 5. School-level correlations between the % of migrants and mean scores

AREA	MEAN SCORES OF	5TH GRADE		6TH GRADE	
		ITALIAN	MATH	ITALIAN	MATH
North_West	Natives	-0.14	-0.08	-0.32	-0.26
	mig 2°	-0.11	-0.06	-0.20	-0.15
	mig 1°	-0.12	-0.06	-0.21	-0.13
North_East	Natives	-0.14	-0.08	-0.15	-0.13
	mig 2°	-0.08	-0.05	-0.20	-0.15
	mig 1°	-0.15	-0.11	-0.20	-0.20
Centre	Natives	-0.15	-0.16	-0.04	-0.00
	mig 2°	-0.09	-0.08	-0.13	-0.05
	mig 1°	-0.11	-0.07	-0.20	-0.13

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 average SES of both native and immigrant students are large and negative (Table 6)[8]. 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 the correlations are considerably higher in 6th grade, approaching the values of natives. Since a strong

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[8] 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.

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 6. School level correlations between the % of migrants and SES

	5TH GRADE		6TH GRADE	
Area	*Natives' Books*	*Migrants' books*	*Natives' Books*	*Migrants' 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

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 too much of an 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 differently.

The assumption of random assignment with respect to immigrant background was tested both at the school-level and at the system-level. School-level tests evaluate random assignment in a given school and show that only a minority of schools deviate from it. The null hypothesis is:

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

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. Using a classical Pearson X^2 test and considering a prudential significance level $\alpha = 0.10$, the null hypothesis is rejected in 23% of the schools for 5th grade and in 20% 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. 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

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[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.

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 underlying hypothesis is that peer effects are independent of *how* children are sorted into groups (the effects should be the same no matter whether a particular class composition was generated by randomness or by somebody's decisions). Moreover, the allocation criterion should not depend on the predictions of how peer effects would operate within the specific group of children enrolled in the school.[11]

Let us go back to the 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 that deviate mildly from it. As a robustness check, I run regressions on the set of schools who pass the test at different significance levels, up to α=0.40, but results do not change substantially and no clear pattern is appreciable.

What if, despite this strategy, I did not totally accomplish the aim of eliminating non-random allocating schools? In principle, neglecting the departure from random assignment could affect peer estimates in any direction: (i) there would be no bias if despite the sorting, teachers were randomly assigned to classes; (ii) 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); (iii) we would underestimate peer effects if higher quality teachers were allocated to the "worse" classes, under the belief that ability streaming is beneficial to all and better resources should be assigned to those more in need. The way students and teachers are actually allocated into classes should be a topic of empirical educational research, because little is known about it and it is a relevant issue from the perspective of ensuring equality of opportunity to all children. Notwithstanding the absence of empirical studies for Italy, my feeling is that case (ii) would be by far more common than case (iii). This is because in some cases parents of advantaged backgrounds do manage to put their children with better teachers, and better teachers often prefer better

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[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.

students.[12] In this light, if some residual non-randomness was left, it would probably operate in the direction of overestimating peer effects.

7. Peer effects estimation

7.1 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 number of books at home as an indicator of the family socio-economic status (SES), which is regarded in the literature to be a better predictor of student performance than other indicators of family background such as parental education or occupational status (Hanushek, Woessmann, 2011). 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 Tables 3 and 4 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 that distinguishes children in the benchmark sample (who took the tests under the supervision of personnel external to the school) from those who did not. This variable has also been interacted with dummy variables indicating first and second generation migrant children, to account for the evidence that immigrant children could be given more help than the others.[13]

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[12] This belief is not supported by scientific evidence (as I said, to my knowledge there is no scientific evidence on this topic), but is based on my personal experience as a mother and teacher and on informal talks with other stakeholders.

[13] For 5th grade INVALSI provides adjusted test scores, corrected from cheating with various statistical techniques. I run regressions using these adjusted scores; however, the results on peer effects were quite odd and not consistent with those obtained for 6th grade, for which adjustments were applied. In this light, I find it safer not to use them and to take cheating under control with the simpler and more transparent way of including dummy variables distinguishing sample and population children.

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 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 most of the research in the peer effects literature; consistently with the individual variables, I take a measure of the average number of books (considered as an ordinal variable with 5 categories).

To account for the effect of immigrant students I consider the proportion of first and second immigrants. Since first generation immigrants have on average more 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 that interact 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). The list of individual and peer variables included in the regressions is summarized in Table 7.

Table 7. Explanatory variables

Individual variables		
LABEL	DESCRIPTION	VALUES
Female	Gender	{0,1}
SES	N° of books at home*	0-4
Repeat	Native repeating grade	{0,1}
1 gen mig	First generation migrant	{0,1}
2 gen mig	Second generation migrant	{0,1}
Sample	Child in sampled class	{0,1}
1 gen*sample	First generation migrant child in sampled class	{0,1}
2 gen*sample	Second generation migrant child in sampled class	{0,1}
Peer variables		
LABEL	DESCRIPTION	RANGE
% Female	Proportion of females	0-1
Mean SES	Mean of n° of books at home*	0-4
% Repeating	Proportion of natives-repeating grade	0-1
% 1 gen mig	Proportion of first gen. migrants	0-1
% 2 gen mig	Proportion of first gen. migrants	0-1
%1 gen mig*native	Interaction: native child * % first generation migrants	0-1
%2 gen mig*native	Interaction: native child * % second generation migrants	0-1
% 1gen mig*native*SES	Interaction: native child * % first generation migrants * n° books	0-4
% 2 gen mig*native*SES	Interaction: native child * % second generation migrants * n° books	0-4

*0=0-10 books; 1=11-25 books; 2=26-100 books; 3=101-200 books; 4=>200 books

7.2 Results

Maximum likelihood estimates of within-school models (3) including schools passing the test for random class allocation of immigrant background students at the level α=0.10 are reported in Table 8. As mentioned above, results change little when a larger significance threshold is used.

Table 8. Determinants of individual performance

	5th grade Italian	*5th grade Math*	*6th grade Italian*	*6th grade Math*
Individual variables				
Female	0.0117***	-0.0383***	0.0106***	-0.0270***
	(0.0007)	(0.0007)	(0.0006)	(0.0007)
SES	0.0308***	0.0297***	0.0279***	0.0302***
	(0.0005)	(0.0005)	(0.0004)	(0.0005)
1gen mig (ref native)	-0.1264***	-0.0787***	-0.1160***	-0.0806***
	(0.0054)	(0.0055)	(0.0039)	(0.0050)
2gen mig (ref native)	-0.0687***	-0.0549***	-0.0659***	-0.0602***
	(0.0057)	(0.0058)	(0.0048)	(0.0062)
Repeat grade *native	-0.1422***	-0.1426***	-0.1035***	-0.1302***
	(0.0048)	(0.0049)	(0.0017)	(0.0021)
Sampled class	-0.0098***	-0.0104***	-0.0005	-0.0005
	(0.0022)	(0.0027)	(0.0015)	(0.0020)
Sampled class *1gen mig	-0.0224***	-0.0106*	-0.0105**	-0.0031
	(0.0053)	(0.0054)	(0.0038)	(0.0048)
Sampled class *2gen mig	-0.0021	-0.0083	-0.0083	-0.0039
	(0.0057)	(0.0058)	(0.0049)	(0.0062)
Peer variables at class level				
% Females	-0.0045	0.0000	0.0003	0.0045
	(0.0050)	(0.0059)	(0.0035)	(0.0048)
Mean SES	0.0027	0.0059***	0.0052***	0.0098***
	(0.0016)	(0.0018)	(0.0011)	(0.0015)
% native repeating grade	0.0011	-0.0262	-0.0302***	-0.0317**
	(0.0289)	(0.0338)	(0.0091)	(0.0124)
% 1gen mig	-0.0915***	-0.0497**	-0.0561***	-0.0249
	(0.0152)	(0.0164)	(0.0100)	(0.0130)
%1gen mig * native	0.0426*	-0.0101	0.0251*	-0.0293*
	(0.0170)	(0.0174)	(0.0116)	(0.0148)
% 1gen mig *native*SES	0.0053	0.0107*	0.0090**	0.0141***
	(0.0045)	(0.0046)	(0.0031)	(0.0040)
% 2gen mig	-0.0961***	-0.0277	-0.0720***	-0.0271
	(0.0168)	(0.0182)	(0.0137)	(0.0178)
% 2gen mig * native	0.0356	-0.0524**	0.0314	-0.0404
	(0.0188)	(0.0192)	(0.0169)	(0.0215)
% 2gen mig *native*SES	0.0215***	0.0336***	0.0159***	0.0302***
	(0.0051)	(0.0052)	(0.0047)	(0.0059)
N° CHILDREN	180153	186076	207198	207377
N° CLASSES	10699	10805	10789	10786
N° SCHOOLS	6029	6059	4750	4750
VAR(BETW CLASSES)/VAR(TOT)	0.055***	0.091***	0.016***	0.027***

 * sig<0.05, **sig<0.01, ***sig<0.001

Estimates are based on classes with at least 10 children with no missing values on each explanatory variable and less than 20% of children with unknown native/immigrant origin.

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 level obtain on average a score which is 12 percentage points higher than that of children belonging to the lowest stratum (recall that scores are computed as the proportion of correct answers). Native students repeating the grade get much lower scores than regular students (10-14 points less). Immigrant children perform more poorly that natives; first generation immigrants are particularly disadvantaged (7-12 points less), not surprisingly, especially for Italian tests.

Moving to peer variables, we observe that the share of females is never statistically significant. On the other hand, SES is significant in most cases, but has a small effect: scores increase of 1-4 points when we move from the case where all peers belong to the lowest SES level to the case where all peers belong to the highest. The effect is largest for math scores and in 6th grade. The number of native children repeating the grade seems to affect performance only in lower secondary school. As regards the effects of immigrant concentration, given the complexity of the variables involved, I summarize results in Table 9.

Table 9. 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	-0.091	-0.050	-0.056	-0.025
natives SES=0	-0.049	-0.060	-0.031	-0.054
natives SES=2	-0.038	-0.038	-0.013	-0.026
natives SES=4	-0.028	-0.017	0.005	0.002
Effect of second gen. immigrants on:				
immigrants	-0.096	-0.028	-0.072	-0.027
natives SES=0	-0.060	-0.080	-0.041	-0.067
natives SES=2	-0.018	-0.013	-0.009	-0.007
natives SES=4	0.026	0.054	0.023	0.053

According to point estimates reported in Table 7.

The concentration of immigrant background children does affect achievement. Yet, effects are heterogeneous, and generally small. As regards Italian scores, immigrant children are more affected than natives. On the other hand, the negative effect on native children of low SES is larger than the effect on immigrants for math. In all cases, the effect on medium-high SES is negligible, and in some cases it is positive, implying that these students could even benefit from the presence of immigrant children. The highest figure reported in Table 7 is -0.096 (referred to the effect of second generation immigrants on immigrants' achievement in Italian, in 5th grade). Since the peer variable varies between 0 (no immigrants) and 1 (all immigrants), this figure implies that a 10 percentage

point increase in the class share of first generation immigrants would lower the percentage of correct answers by less than 1 point. Although not negligible, this should be considered a weak effect. I also estimate a different specification of the model allowing for non-linear effects in the class share of immigrant origin children, but since the results are not particularly insightful, I do not shown them here.

7.3 Robustness checks

I made a number of robustness checks in order to evaluate the extent to which results are dependent on particular assumptions.[14] First, I run additional regressions using an alternative SES measure provided by INVALSI (Campodifiori *et al.*, 2010), a composite index based on numerous variables, similar to PISA's ESCS. Both the magnitude and the significance of immigrant background peer effects are close to those reported in Table 8. Average class ESCS coefficients are consistent with the estimates relative to the number of books, although they are not strictly comparable because the scale of the two measures is different.

Second, I estimate model (3) on different subsets of schools. As anticipated in section 6, I consider the set of schools who pass the test of random allocation to classes with respect to immigrant background at different significance levels (up to α=0.40). Results do not change substantially and no clear pattern is appreciable.

Moreover, since a substantial number of schools do not pass random allocation tests with respect to SES[15], I restrict the analyses to the schools that pass the tests with respect to both immigrant background and SES (as measured by the n° of books at home) at the significance level 0.10. Immigrant origin peer effects, the focus of this paper, change little. I find a slight increase of the effect of the share of immigrant background students on immigrant students in 5th grade Italian, of the share of first generation immigrants on immigrants in 5th grade math, of the share of second generation immigrants on all students in 6th grade Italian, but the substantive conclusions remain the same.

On the other hand, the coefficients of average class SES diminish substantially and in some cases even loose significance. Consider however that SES is likely to be affected by measurement error, and in this case SES peer effects would be underestimated (Ammermueller, Pischke, 2009). In this perspective the 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

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[14] Results are not shown here but are available upon request.
[15] Approximately 38% of schools do not pass single school tests of random allocation with respect to SES (based on Anova F-statistics).

counterbalance the bias due to measurement error. With this in mind, caution should be applied when interpreting the estimates of SES peer effects.

7.4 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.[16]

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 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 the Appendix. 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 are 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.

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

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

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

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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.[17]

Since $\check{\beta}_k$ represent the value of β if only endogenous effects operated, (disregarding sampling variability) could we think of $\check{\beta}_k$ as upper bounds for β? First note that the empirical result ($\gamma_k^* > 0, \tau_k^* > 0$) does not imply ($\gamma_k > 0, \tau_k > 0$), and ($\gamma_k^* < 0, \tau_k^* < 0$) does not imply ($\gamma_k < 0, \tau_k < 0$).[18] However, if structural parameters γ_k and τ_k are both positive or negative, then also γ_k^* and τ_k^* have the same sign and $\check{\beta}_k \geq \beta \geq 0$.[19]

The exception occurs when structural parameters have opposite signs. What is the substantive meaning of ($\gamma_k > 0, \tau_k < 0$)? As z_k grows the individual is less performing, while a group of peers with large z_k positively affects performance. As suggested in some empirical research, this might be the case of gender effects on math scores: females score lower than males, however a peer group with many females may foster learning. In this case peer effects related to the share of females cannot all be driven by achievement effects: if females are less performing than males, they should *negatively* affect others' learning. In this case, depending on the values of the true γ_k e and τ_k, we could even find values of γ_k^* and τ_k^* leading to $\check{\beta}_k < 0$ or $\check{\beta}_k > 1$. So, if we suspect that γ_k and τ_k have different signs – which can be suggested (but not demonstrated) by the different signs of the corresponding reduced form parameters – we should not calculate the implied values of $\check{\beta}_k$: they are meaningless, because γ_k cannot be 0.

Comparing the estimates of $\check{\beta}_k$ derived from the explanatory variables we that believe have either both positive or both negative structural parameters, gives us some insights on the relative magnitude of exogenous and endogenous effect. To simplify the exposition, I use the estimates of a school fixed-effect model with no interactions involving peer variables and no heterogeneous effects. For the reason exposed above, I do not consider gender peer effects here.

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[17] 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), $1/\check{\beta}_k \cong 1 + \frac{\tau_k^*}{\gamma_k^*}$, hence $\frac{\gamma_k^*}{\tau_k^*} \cong \frac{\check{\beta}_k}{1-\check{\beta}_k}$. 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 $\check{\beta}_k$, but they are no longer anomalous if they represent $\frac{\check{\beta}_k}{1-\check{\beta}_k}$ (for example, Hoxby finds $\frac{\gamma_k^*}{\tau_k^*}$=6 for some variable; this implies that $\check{\beta}_k$ is approximately 0.86, which is a large but acceptable value).

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

[19] 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.

Table 10. Endogenous and exogenous effects.

SCORES	PEER VARIABLE	(1) τ_k^*	(2) γ_k^*	(3) $\check{\beta}_k$	(4) $\check{\beta}$	(5) $\check{\tau}_k$	(6) $\check{\gamma}_k$
5th grade – Italian	1 gen migr	-0.1351	-0.0450	0.253		-0.1349	-0.0312
	2 gen migr	-0.0779	-0.0264	0.257	0.078	-0.0778	-0.0184
	SES	0.0322	0.0027	0.078		0.0322	0.0000
5th grade – math	1 gen migr	-0.0789	-0.0391	0.337		-0.0787	-0.0295
	2 gen migr	-0.0560	-0.0050	0.082	0.082	-0.0559	0.0000
	SES	0.0319	0.0060	0.159		0.0319	0.0029
6th grade – Italian	1 gen migr	-0.1235	-0.0181	0.129		-0.1234	0.0000
	2 gen migr	-0.0736	-0.0161	0.181	0.129	-0.0735	-0.0047
	SES	0.0293	0.0052	0.152		0.0293	0.0008
6th grade – math	1 gen migr	-0.0801	-0.0240	0.233		-0.0800	-0.0150
	2 gen migr	-0.0607	-0.0058	0.088	0.088	-0.0607	0.0000
	SES	0.0326	0.0097	0.232		0.0326	0.0060

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

To illustrate Table 10, take 5th grade – Italian scores (uppermost panel). Columns (1) and (2) report reduced form estimated values. In column (3) we find implied values of β under the assumption $\gamma_k=0$, according to equation (6). If we assume that individual and peer exogenous effects for immigrant backgrounds have the same sign, and the same we do for SES, we can infer that β must not exceed the smallest of these implied values, so $\beta \leq 0.078$. The reason is that β must be smaller than *all* the estimated upper bounds. In column (4) I report the smallest value of $\check{\beta}_k$. If this was the "true" value of β, an exogenous increase in peer scores of 10 percentage points would lead to an increase of less than 0.8 percent of correct answers. The other values in column (4) are also small (between 0.082 and 0.129); hence we can conclude that if endogenous effects operate, they must be weak.

As a final exercise, I take for good this β, and use it to estimate τ_k and γ_k. Column (5) reports the implied individual effects (which in this case are almost identical to the corresponding reduced form estimates). Colum (6) shows the implied estimates for exogenous peer effects. Note that these estimates are trivially 0 for the variable displaying the smallest threshold $\check{\beta}_k$. So, going back to the example of 5th grade – Italian scores, $\check{\gamma}_k = 0$ for SES, because we are assuming that all peer effects related to SES are driven by achievement differentials. By assumption, the other estimates of $\check{\gamma}_k$ have the same sign of the corresponding reduced form parameter and their absolute value falls between 0 and that of γ_k^*. Under the hypotheses made above, a 10 percent increase in the share of first generation immigrants would lead to an increase of 0.31 in the percentage of correct answers.

Interesting conclusions can be drawn by comparing the $\check{\gamma}_k$ across explanatory variables: as regards 5th grade –Italian tests, the share of first generation immigrants is likely to affect learning also through exogenous effects, and these exogenous effects are likely to be larger than those

related to the share of second generation immigrants and to average SES. This also holds for 5th grade-math tests. Second generation exogenous effects slightly prevail for 6th grade-Italian tests and SES for 6th grade-math tests (recall that SES varies between 0 and 4, so a change from all peers with SES=0 to all peers with SES=4 induces an increase in individual scores of approximately 2.4 percentage points).

8. 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 immigrant students (either of first or second generation) 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.

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.[20] 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).

Figure 2. *The reduced-form model*
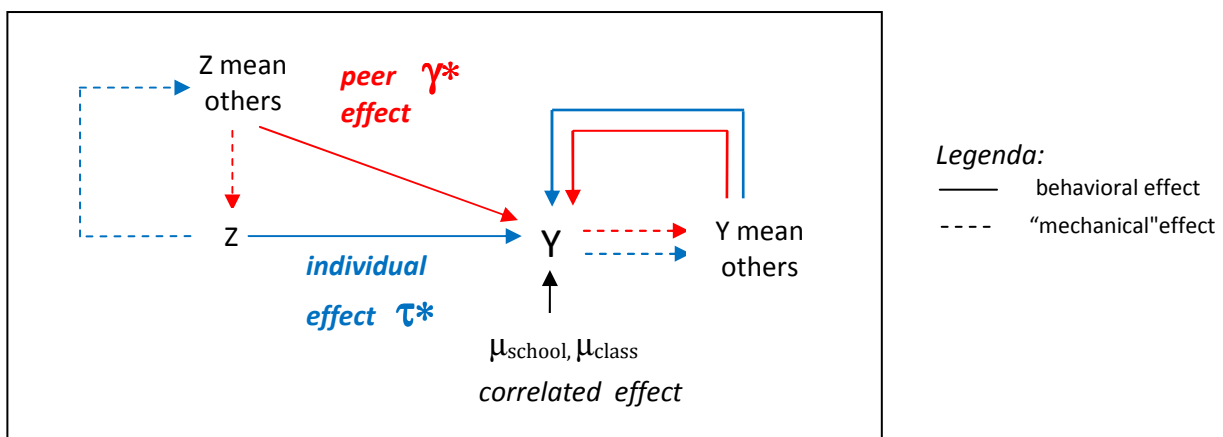
To conclude, the commonly employed reduced form is just an approximation of the true reduced form (A.6), because it does not acknowledge that parameters vary with class size. What

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[20] 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}.

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.[21]

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