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MECHANISMS OF PEER INTERACTIONS BETWEEN NATIVE AND NON-NATIVE STUDENTS: REJECTION OR INTEGRATION? *

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ABSTRACT: This paper focuses on mechanisms of "peer interactions" among native and non-native students. We present a theoretical framework based on Lazear (2001) education production model and on the "sub-cultural" sociological theory and we test the theoretical predictions exploiting a dataset of Italian junior high school. Results show that non-native school share has small and negative impacts on Language test scores of natives' peers, while it does not significantly affect Math test scores. The negative effects to natives' attainment are concentrated in schools characterized by low levels of non-natives' isolation or where non-natives' school share is above 10%.

JEL Codes: J15, I21, I28

Keywords: Peer effects, native and non-native students, social interactions

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1. Introduction

In the last two decades, a lot of Western countries have experienced massive immigration waves. Despite the growing relevance of this phenomenon in Europe, and the well-established (de)segregation literature in the U.S., studies investigating the peer interaction between native and non-native students in European schools are just a few. Although it is widely accepted that non-native students typically face more problems at school and have lower scores in standardized tests, causes, consequences and possible policy implications are still unclear (OECD, 2010). Moreover, while there is a vast literature on the effects of immigration on native labour market outcomes, economic literature on the effects of non-native students on native peers' achievement is quite limited, and the specific question of whether non-native peers affect natives' educational outcomes has received relatively little attention and presents mixed evidence (Brunello and Rocco, 2011; Gould *et al.*, 2009[2]). For instance, Jensen and Rasmussen (2008) find a negative effect of school ethnic concentration on cognitive outcomes for Danish native students[3]. Brunello and Rocco (2011) provide cross-country evidence of a negative but small effect of the share of immigrants on natives' educational attainment exploiting PISA data for a sample of 27 countries (mainly from Europe and the Anglo-Saxon world).

This paper focuses on 'social interactions'[4] among pupils of different ethnic origins attending the same class or the same school. In the existing literature social interactions among schoolmates are commonly referred to as 'peer effects' or 'peer-groups effects'[5]. Peer influence in general, but also in the specific case of the interactions between native and non-native students, studied different outcomes such as achievement levels (as measured by test scores), teen pregnancy, delinquency, smoke and drug use, high school attrition and drop-outs, college choice (Hanushek *et al.*, 2003), and it may have an effect in the accumulation and development of both cognitive and non-cognitive skills (Neidell and Waldfogel, 2010). Indeed, there is not clear evidence on possible consequences of social interactions between natives and non-natives in educational settings, and it

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[2] Gould *et al.* (2009), "*[...] the effect of immigration on the local labour market has received considerable attention in the literature, but little is known about the impact of immigration on the school system".*

[3] Evidence on school composition and immigrant lower test scores for Denmark and Switzerland is also provided by Schindler (2007) and Meunier (2010), respectively.

[4] We define 'social interactions' all forms of interdependencies among individuals in which preferences, beliefs and constraints faced by one socioeconomic actor are *directly* influenced by the characteristics and choices of others (Durlauf and Ioannides, 2009). Peer group influence is a particular form of social interaction. It refers to contemporaneous, and usually reciprocal, behavioural influences within a reference group so that the propensity of an agent to behave in some way varies positively with the prevalence of this behaviour in the group (Durlauf, 2004, and Manski, 2000). These interactions usually produce the well documented empirical regularity that "[...] agents belonging to the same group tend to behave similarly" (Manski, 2000).

[5] This terms usually indicates social interactions of children or young adults with people of similar age, in order to make a distinction from the broader 'neighbourhood effects' stemming from interactions with superiors, family or teachers (Gibbons and Telhaj, 2006).

might happen that such interactions (if they exist) could tend either to increase or decrease the existing attainment gaps. On top of that, even less is known on the possible underlying mechanisms that such peer interactions may follow (De Giorgi and Pellizzari, 2011).

The aim of the paper is twofold. On the one hand, we propose a theoretical framework to stylize the possible mechanisms of peer interactions that could lead to an ‘integration’ or a ‘rejection’ of non-native peers, depending on the degree of isolation experienced by non-native students in each school. On the other hand, we test the theoretical predictions identifying the causal link between non-natives’ school concentration and the educational outcomes of native students[6]. Which kind of social mechanism may work in peer interaction between native and non-native students? Do different levels of non-native school share have different impacts on natives’ attainments? We use as outcome measure attainment levels proxied by standardized test scores exploiting a unique dataset combining *INVALSI First Cycle Exams* (test scores of all 8th grade students enrolled in Italian junior high schools[7]) with census and administrative records on schools characteristics and socio-economic environment.

The theoretical framework is based on Lazear (2001) model of education production and on the ‘subculture model’ proposed by sociological literature in the U.S. (Fordham and Ogbu, 1986). The basic intuition is the following. Non-native and native students are characterized by different levels of propensity to ‘disrupt’: non-native students are more disruptive because they typically need more help from teachers. Thus, in mixed schools, the presence of non-native students tends to have a negative impact on natives’ attainment levels. However, if non-native students are relatively isolated, peer interactions with natives may be helpful for them so that their propensity to disrupt decreases, and so does their negative impact on natives’ attainments. Disruption is mitigated through an ‘integration mechanism’ that can be at work only for sufficiently low levels of non-native school concentration. On the other hand, if non-native students are enough to create some ‘critical mass’ (i.e. they are not isolated), they tend to cluster and do not interact with native, so that they continue to be, on average, more disruptive and thus cause negative impacts on natives’ attainment. This ‘rejection mechanism’ may be due to different reasons, for instance, natives may be willing to make sufficient effort to include a few minority members but unwilling to make the effort to include numerous non-native schoolmates.

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[6] Data from Italian Ministry of Education generally only distinguish between *Italian* and *non-Italian* students, thus referring to a pure citizenship criterion. In the reminder of the paper we define to as ‘non-native’ student an individual enrolled in the Italian school system and having both parents without Italian citizenship. This definition coincides with the definition of the Italian Ministry of Education Statistical Service (MIUR 2009a) of ‘non-Italian students’. Notice that if a student has one of the parents who is Italian, he automatically gains the Italian citizenship (because of the *ius sanguinis* rule) and so he is defined as ‘native student’ independently from the country of birth.

[7] 8th grade students, i.e. students finishing their third year of the Italian middle grade comprehensive school. The Italian ‘Junior High School Diploma’ corresponds to ISCED level 2.

From the empirical point of view, solving serious problems of sorting and omitted variables bias is crucial in the correct identification of the effect. Our identification strategy is based on school-level averages in order to sidestep the non-random allocation of non-native students across classes and school fixed effects, and exploits the within school idiosyncratic variation in non-native share between adjacent cohorts (Hoxby, 2000; Gould *et al.* 2009; Brunello and Rocco, 2011).

This paper contributes to the existing literature in a number of ways. It provides additional evidence on peer effects between native and non-native students in the Italian junior-high schools contexts. It overcomes problem of under-representation of immigrant shares typical of survey data exploiting a rich administrative dataset containing information on all 8th grade students enrolled in Italian junior high schools. Finally, it links the empirical evidence to the theoretical framework of peer interaction based on 'integration' and 'rejection' mechanisms to shed light on the interpretation of the results. Our results show that non-native school share has small and negative impacts on Language test scores of natives' peers, while it does not significantly affect Math test scores. The stylized predictions of the theoretical framework are confirmed in the analysis of heterogeneous and non-linear effects. In particular, heterogeneous effects show that the negative effects to natives' attainment are concentrated only in schools characterized by low levels of non-natives' isolation (or, alternatively, high exposure), while they are not statistically different from zero when isolation is high. Non-linearity analysis shows that non-natives' school share below 10% for Language, and 20% for Math, does not significantly affect natives' attainment. To give a numerical intuition for these results, we calculate that, on average, a non-native school share of 10% corresponds to 9 non-native students in the school or, equivalently, 1 or 2 non-native students in each class. Below these critical mass average values, the 'integration' mechanism is at work, and non-native students are assimilated with native peers. The opposite is true for the 'rejection mechanism', which is working above the critical mass value found.

The rest of the paper is organized as follows: Section 2 presents a review of the literature; Section 3 explains the theoretical framework, Section 4 describes the econometric model and identification strategy designed to test the stylized predictions of the theoretical framework; Section 6 discusses the main characteristics of the dataset and provides general descriptive evidence; Section 5 and Section 6 discuss the results and conduct sensitivity checks. Section 7 concludes and provides some policy implications.

2. Literature

The empirical analysis of the effects of non-native students' on native peers educational outcomes stems from the 'desegregation' literature[8], which examines the effect of minority students on the achievements of the other students in the U.S. schools (Gould, Lavy and Paserman, 2009). Early desegregation literature proposes a variety of analyses on the relationship between ethnic origins and achievement (among the others: Armor, 1995; Cook, 1984; Crain *et al.* 1978), but does not consider social interactions between native and non-native students as a potential educational input to explain the persistent attainment gap. The first study mentioning the contribution that the class and school ethnic composition has on the individual achievement is the 'Coleman Report' (Coleman, 1966)[9]. Starting from Coleman (1966), scholars in the sociology of education have long argued that, apart from students' ability and background, peers influence is an important determinant of students' achievement (Kramarz *et al.*, 2008). Economic literature on peer effects among native and non-native students only appears in the Nineties, while interest on the economic analysis of social interactions was flourishing. Although the great variety of studies on social interactions in educational outcomes, empirical evidence and theoretical models on peer effects between native and immigrant students still presents mixed findings and just a few analysis on possible channels and mechanisms at work (Sacerdote, 2010).

Empirical literature in the U.S. traditionally focused on achievement gaps between black (or other minority students) and white students, and only in the last decade peer interaction has started to be seen as one of the possible causes of many observed different behaviours between white and black students (Heckman, 2011). Early contributions were given by Evans, Oates and Schwab (1992) and Cutler and Glaeser (1997), while Hoxby (2000), Hanushek *et al.* (2009) and Hanushek and Rivkin (2009) are the first to define 'racial peer effects' as a particular group of social interactions taking place between students belonging to different ethnic groups. Hoxby (2000) exploits idiosyncratic variation in the racial and gender composition of adjacent cohorts within the same grade and within the same school to estimate the effects of exposure to minority school share on achievement of white and minority students. Her results show that immigrant school share has

[8] For decades economists and sociologists studied the effects of desegregation plans imposed by U.S. Courts, starting from *Brown vs. Board of Education*, 347 U.S. 483 (1954). The ruling in *Brown v. Board of Education* (1954) held that 'separate but equal', while not inherently unconstitutional in all areas, was unconstitutional in the case of education because separate education for blacks and whites could not be equal. This ruling led to dramatic changes in schools throughout the country (Hanushek *et al.*, 2009).

[9] "[...] those inputs characteristics of schools that are most alike for Negroes and whites have least effect on their achievement. The magnitudes of differences between schools attended by Negroes and those attended by whites were as follows: least, facilities and curriculum; next, teacher quality; and greatest, educational backgrounds of fellow students. The order of importance of these inputs on the achievement of Negro students is precisely the same: facilities and curriculum least, teacher quality next, and backgrounds of fellow students, most", Coleman (1966).

weak effects on students' achievement, but these effects are generally higher among students of the same ethnic group than among students belonging to different ethnic groups. Hanushek *et al.* (2009) and Hanushek and Rivkin (2009) base the estimation strategy on individual fixed effects retrieved tracking the same students and cohorts over time: the estimation of peer group effects relies therefore on cohort differences in the changes in racial composition as students' progress through school. They find that black students test scores are strongly decreasing in the black school share: their estimates imply that excess exposure of black students to black grade mates causes the black-white test score gap to grow by 0.07 standard deviations with each year in school, but no effects on white students.

The general result that 'intra-race group' peer effects are stronger compared to 'extra-race group' effects is also found by Angrist and Lang (2004) who exploit the quasi-natural variation in the fraction of minority students provided by one of the most important desegregation plan implemented in Boston school districts (i.e. the Metco program), moving low-achievers black students to preeminently white schools in the rich Boston suburbs. A new strand of literature interprets this general result under the light of the 'acting-white' theory that says that black students may underachieve in order to fit with their peers' behaviour. Fryer and Torelli (2010) provide the first empirical evidence using the National Longitudinal Study of Adolescent Health (*AddHealth*) and estimating the effects on achievement of an 'index of social status' based on the individuals' contacts with same-race friends within the school. They show that this 'acting white' proxy variable varies a lot with respect to school characteristics and individual achievement. The effect is concentrated in schools with more interracial contact: their coefficient for 'acting-white' variable is twice as large in schools that are above the median in terms of segregation, whereas is significantly lower where black students are more isolated.

Outside the U.S., empirical evidence is still quite limited and generally points to a negative effects of non-native school shares on native students attainments. In order to identify the causal link of the immigrant concentration on the outcomes of natives, Gould *et al.* (2009) exploit the variation in the number of immigrants in 5th grade conditional on the total number of immigrant students in grades 4 to 6. IV results point to a strong adverse effect of immigrant concentration on native outcomes, but the estimates are not statistically different from the OLS coefficients. Their approach is interesting and new under two main aspects: first, they use a quasi-experimental evidence claiming that early '90 immigration waves in Israeli can be considered as an exogenous variation in immigrants' flows; second, they focus on long-term outcomes (rather than contemporaneous peers' outcomes effects). Jensen and Rasmussen (2008) analyse the effect of ethnic concentration in schools on the cognitive outcomes of children. They use a rich dataset for

Danish ninth-grade students, based on PISA test scores matched with administrative and census information. In order to correct for the endogeneity in school ethnic concentration authors apply school fixed-effects and IV, using as instrumental variable the ethnic concentration in a larger geographical area where school is located. Results show that there is a negative effect of ethnic concentration on students' outcomes, and that this is significant only for the native Danish children. Brunello and Rocco (2011) study whether a higher share of immigrant pupils affects the school performance of natives using aggregate multi-country data from PISA, and find a negative but small effect. The analysis is conducted exploiting aggregation at the country level to avoid sorting problems of immigrant students within each country, while fixed effects and country socio-economic indicators are used to solve the problem of across country sorting and time trends in immigrants residential choices. They also find evidence that, conditional on the average share of immigrant pupils, a reduction of the dispersion of this share between schools would have small positive effects on the test scores of natives. Finally, Maestri (2011) investigates how the heterogeneity of the ethnic minority composition within schools affects natives' and non-natives' attainment. She exploits the within school cohort-to-cohort variation in ethnic diversity of a rich dataset about primary education in the Netherlands and finds that ethnic diversity has a positive impact on the test scores of minority students, in particular for language skills. She also finds some evidence of a negative relationship between ethnic diversity index and a measure of school social interactions among pupils.

3. Theoretical framework

The theoretical framework combines Lazear (2001) 'bad apple model' for the education production process and the 'subcultural' model of interaction between white and blacks students proposed in the sociological literature (Fordham and Ogbu, 1986; Steele and Aronson, 1998). On the one hand, the 'bad apple model' is based on the driving idea that peer effects are important determinants of all educational processes in the classroom and proposes the 'disruption mechanism' as one of the possible explanation of peer pressure in educational settings. On the other hand, the subculture model identifies the 'integration vs. rejection mechanism' as a possible explanation of the 'social mechanisms' causing the black-white gaps in attainments levels. From a general perspective, in the 'subculture model' the native student (majority type) remains supportive of non-native students (minority type) as long as the latter is relatively isolated (Hoxby and Weinghart, 2006). When, however, minority students become prevalent enough to form a critical mass, the majority type rejects them. The 'subculture' model can also explain the evidence of 'acting-white' behaviours found in U.S. schools (Austen-Smith and Fryer, 2005; Fryer and Torelli, 2010).

3.1 The disruption model with native and non-native students

Following Lazear (2001), define p as the probability that any student is not hurting his own learning or other's learning at any moment in the time spent at school, and $(1 - p)$ as the probability that any given student initiates a 'disruption'. Given a class size of n, the probability that disruption occurs at any moment in time t is $(1 - p^n)$. Define V as the value of a unit of learning, which is influenced by the likelihood that a student is not engaged in a disruptive behaviour in the given instant t, and Z the total number of student in the school. Then, total output for each school (Y) and per student output (y) are given by:

$$Y=ZVp^n$$

$$y=Vp^n$$

Disruption can actually follow many channels. It could be thought as students' need of help causing the teacher to slow the activity of the class, as well as students' propensity to disturb/interrupt or even to make questions to the teachers. The basic assumption made is that one child's disruption hurts the learning process in that moment of all students (including the disruptive one). Therefore, disruption is a possible mechanism of peer interaction that directly influences the learning process and the attainment levels through externalities caused by peers' behaviour[10]. Non-native students are lower achieving students (on average, as shown by descriptive statistics), and usually need more help and attention from teachers. Moreover, non-native students are always less numerous with respect to native peers. Therefore, we assume that non-native students ($j=NN$) causes more disruption (on average) with respect to native peers ($j=N$), and define as $(1\text{-}\theta)$ the proportion of native students in each school. Then, we can identify to types of students ($j=N$, NN) with different values of p_j: $p_N \geq p_{NN}$.

Lazear (2001) demonstrates that total output is maximized when students are segregated by type. To see this, suppose without loss of generality that $V=1$ and consider the output per student assuming class size equals n, the optimum class size with mixed classes. Per student output in mixed (y_{mix}) and perfectly segregated schools (y_{seg}) will be equal to:

$$y_{mix} = p_N^{(1-\theta)n} p_{NN}^{\theta n}$$

$$y_{seg} = (1-\theta) p_N^n + \theta p_{NN}^n$$

[10] De Giorgi and Pellizzari (2011), Epple and Romano (2011) and Sacerdote (2010) point to the Lazear (2011) model as one of the potential model of peer interaction in the classroom, as well as Hoxby and Weinghart (2006) include the 'Bad Apple model' and the 'subculture model' in their analysis of possible model of peer interaction in the classroom.

Then:

$$(y_{seg} - y_{mix})\Big|_{p_N = p_{NN}} = 0$$

$$\frac{\partial(y_{seg} - y_{mix})}{\partial p_N}\Bigg|_{p_N > p_{NN}} = (1-\theta)np_N^{n-1}\left[1 - \left(\frac{p_{NN}}{p_N}\right)^{\theta n}\right] > 0$$

Which implies that segregation induces a higher per student output with class size optimal for mixed classes whenever $p_N > p_{NN}$. Letting class size be optimal when schools or classes are segregated reinforces the results (Lazear 2001, Epple and Romano 2011).

3.2 The integration-rejection model of peer interaction

Segregation leads to the maximum output if only if $p_N > p_{NN}$. Peer interactions could intervene to reduce non-natives disruption probability $(1 - p_{NN})$ as far as native students' behaviour could exert positive spillovers on non-natives through an 'integration mechanism'. Native students' behaviour (i.e. less disruptive types' behaviour) could have a positive impact on non-native peers and, as a consequence of the integration process, $p_{NN} \rightarrow p_N$. Integration, however, has some cost which we assume to be the effort made by native students to integrate non-native peers. Intuitively, if non-native students are relatively isolated, then the integration mechanism is less costly for native students, whereas anytime non-native students become prevalent enough to form a critical mass, the native type rejects them because the effort of integration becomes too high[11]. Conversely, whenever non-native students are relatively more isolated, they are somehow 'forced' to interact with native peers. Therefore, p_{NN} depends on the integration effort made by native students (e) which, in turn, is an increasing function of the proportion of non-native students, θ. Without loss of generality, we set to zero the effort cost in the case θ is lower than a certain critical value ($\tilde{\theta}$) and positive otherwise, and assume that native students do not exert integration effort unless it is zero (or, close enough to zero). Formally:

$$e(\theta) = \begin{cases} 0 \text{ if } \theta \le \tilde{\theta} \\ > 0 \text{ if } \theta > \tilde{\theta} \end{cases}$$

[11] The basic intuition for this formalization of the 'integration' vs. 'rejection' mechanism based on non-natives relative isolation is already present in Lazear (2001) model: "[…] It is necessary that *B*'s can be *transformed* into *A*'s by being around them. If this effect is strong enough, then integrated classes are efficient. For example, if *B*'s were immediately transformed into *A*'s when integrated with them, and if this imposed no cost on *A*'s, then efficiency would be enhanced by mixing *B*'s with *A*'s. As a practical matter, transformation of *B*'s into *A*'s is most likely to occur when the ratio of *A*'s to *B*'s is large. If a school of 100 had 99 *B*'s and 1 *A*, it is unlikely that the one *A* student would change the behavior of all of the other *B* students", p. 791, original enphasis. The same intuition can be also found in the 'cultural assimilation model' by Lazear (1999).

$$p_{NN} = \begin{cases} p_N \text{ if } \theta \leq \tilde{\theta} \\ < p_N \text{ if } \theta > \tilde{\theta} \end{cases}$$

The formalization of the integration mechanism makes per student output depend on θ: if $\theta \leq \tilde{\theta}$, the integration mechanism prevails, $p_N = p_{NN}$ and $(y_{seg} - y_{mix}) = 0$; if $\theta > \tilde{\theta}$ the rejection mechanism prevails because integration is too costly and less likely to take place, therefore the previous results for mixed classes hold. Actually, the rejection may be due to different reasons: natives may be willing to make sufficient effort to include a few minority members but unwilling to make the effort to include numerous non-native schoolmates and but also unwilling to include some non-native students while rejecting others (Hoxby and Weingarth, 2005).

Focusing on the effects on non-native students, and given that (perfect) segregation cannot be observed in our data, if θ is sufficiently high, then the rejection mechanism is at work and disruption in a mixed class leads to lower levels of per student output for natives. On the other hand, if θ is sufficiently low, then the integration mechanism is at work, and per student natives' output is not hurt by non-natives disruption. We can stylize the predictions of the model in the following way:

i) $\frac{\partial y_N}{\partial \theta} < 0$ if $\theta > \tilde{\theta}$ $\rightarrow$ the rejection mechanism prevails

ii) $\frac{\partial y_N}{\partial \theta} > 0$ if $\theta \leq \tilde{\theta}$ $\rightarrow$ the integration mechanism prevails

4. Empirical strategy

As widely recognized in the literature, the vast majority of cross-sectional variation in students' peers is generated by selection: students self-select into schools based on their family background and income, parents' job locations, residential preferences, school rules, educational preferences and even ability (Hoxby, 2000). In the specific case of the estimation of peer effects between native and non-native students, first of all, one must account for the endogenous placement of immigrants into some geographical areas that are usually more likely to be populated also by lower-achieving native students, regardless of the local level of immigrant concentration (Gould *et al*., 2009). As a consequence, non-natives' concentration in the schools may be endogenous because of parents' housing decisions: individuals sort into neighbourhoods because they want - or do not want, or they are forced - to live in a 'ghetto' area, or in areas where an occupation is more likely to be found, or in areas where renting houses is less expensive, and so on. Second, the peer group can be the result of individual choices: for example, given the residential choice of the household, individuals living in a given area choose a certain school on the basis of some (perceived) school quality. Third, given the school choice, the allocation of non-native students among the classes within a certain school is usually not random. Ammermueller and Pischke (2009) provide evidence of the non-random assignment of non-natives students within school and within classes in some European country. Finally, it is worthy to underline that non-random allocation of non-native students is not only the results of endogenous individual choice, but also depends on school staff, municipalities choices and law regulation[12]. Besides self-selection issues, the estimation of a reduced form model retrieving the peer effect parameters is also hard because of the problems arising from the presence of the correlated effects that will give rise to a bias if they are correlated with peer group composition (Manski, 1993).

4.1 Baseline empirical model

The sorting processes described and the difficulty to control for all possible correlated effects may lead to a negative spurious correlation between attainments levels of native students and non-native school share, independently from the fact that non-native students actually cause some bad or good externalities on natives' (Brunello and Rocco, 2011). Our estimation strategy relies on the basic assumption that changes in non-natives school shares between subsequent cohorts within the same school are not correlated with pupils' unobservable characteristics that may be relevant in the educational production process. We solve sorting of non-native students across classes within the same school using school level averages and we identify the effect of non-native school share on natives' attainment by exploiting school by time variations in the data, using the following empirical specification:

$$y_{st}^{N} = \beta P_{st}^{NN} + X_{st}^{N}\alpha + \varphi_s + \varphi_t + \eta_{st}^{N} \quad [1]$$

where y_{st}^{N} represents the school mean test score of all 8th grade native (j=N) students in school s and year t, P_{st}^{NN} is non-native school share in school s and year t, X_{st}^{N} is a vector containing mean characteristics of native students in school s and year t, φ_s are school fixed-effects and the term φ_t includes time and territorial fixed-effects[13]. A nice feature of this specification is that it can be easily reconducted to a reduced-form linear-in-means model for peer effects estimation (Card and Rothstein, 2009) where both endogenous and exogenous effects arising from exposure to non-native peers are incorporated in β (Manski, 1993). However, we cannot distinguish

[12] In Italy, Heads, School Boards and Municipalities must collaborate to allocate non-Italian students within schools and within classes in such a way to avoid segregation problems.
[13] Territorial fixed-effects include five territorial dummies (North West, North East, Centre, South, Islands) interacted with year dummies for the three Invalsi IC waves.

whether β reflects the exogenous effects of student's peers characteristics or the endogenous effects operating through student's peers achievement (i.e. the well-known 'Reflection Problem'). Anyway, finding evidence of the 'social effects' (i.e. both endogenous and exogenous) is still of substantial policy interest (Ammermüller and Pischke, 2009; Hoxby, 2000) and still hard in practice because of endogenous sorting, selection issues and omitted variables bias (Hanushek *et al.* 2003).

Conducting our analysis at the school level solves the sorting of non-natives across classes in the same school, while school fixed effects solve possible omitted variable bias in individual mean characteristics and school mean characteristics which may influence native attainments (i.e. the correlated effects). The important issue that must be addressed in our empirical model is across schools sorting of non-native students. Indeed, school fixed-effects and geographical area fixed effects should already capture this sorting. However, we exploit the original features of our dataset and add to the specification in eq. [1] a set of school by year variables (W_{st}) which capture the socio-economic characteristics of each school catchment-area. The socio-economic variables are chosen in order to select characteristics of the catchment-area that could have attracted immigrant families in the past, and thus influence the actual non-native school shares. For example, we include male and female occupation rate, population density, indicators for poor housing conditions. We also include the number of non-Italian residents in each school catchment area in 2001 (i.e. at the beginning of the sharp increase in the Italian immigration trend) which can be shown to be a strong predictor of the actual non-natives school shares and thus control for non-natives' sorting across schools.

A final concern may arise if we observe that even the variation of non-natives shares across subsequent school years can be endogenous because of some sort of 'native flight' or underlying time trends (Betts and Fairlie 2003, Hoxby 2000 among others). To solve this issue we apply the same strategy used by Gould *et al.* (2009) and Brunello and Rocco (2011) conditioning on the total stock of non-native students in the school and on the total school size (S_{st}). Therefore, conditioning on these variables, the share of non-native students who are attending the 8th grade in each school can be considered as good as random, while any residual correlation between non-native shares and school characteristics is captured by the school fixed-effects. Thus, we estimate the following equation:

$$y_{st}^{N} = \beta P_{st}^{NN} + X_{st}^{N}\alpha + W_{st}\delta + S_{st}\gamma + \varphi_s + \varphi_t + \eta_{st}^{N} \quad [2]$$

Table 4 contains the complete list and description of the variables included in the X_{st}, W_{st} and S_{st} vectors.

4.2 Heterogeneous and non-linear and effects

The estimation of β in eq. [2] allows a causal interpretation of the effect of non-native school share on natives' attainment which we interpret as non-natives' peer effects on natives' attainment. However, to test the predictions of the theoretical model it is necessary to introduce heterogeneity and/or non-linearity in the estimation of the parameter of interest.

We allow for heterogeneity in the effects dividing the sample according to different levels of the 'isolation' experienced by non-natives: the more they are isolated the less costly is for natives to integrate them. In particular, we calculate the exposure index ($E^{j}_{N/NN}$) as a measure of the exposure of native students to non-native students in each school district j (i.e. province): the higher is the exposure of native students to non-native peers, the lower is the isolation of non-natives and the higher is the effort cost for assimilation. The exposure index takes the following form:

$$E^{j}_{N/NN} = \frac{\left[\sum_{i=1}^{K_j} N_{ij}\left(\frac{NN_{ij}}{NN_{ij}+N_{ij}}\right)\right]}{\left[\sum_{i=1}^{K_j} N_{ij}\right]}$$

where N_{ij} represents the sum of native students in school i of school district j, and similarly NN_{ij} represents the sum of non-native students in school i of school district j; K_j is the total number of junior high school in school-district j. This measure is a refinement of the simple non-native school share and is generally interpreted as the percentage of non-native students enrolled with the average native student (Clotfelter, 1999; Freyer, 2011). Then, we create a dummy variable (d_s) taking value 1 if a school is located in a school district characterized by a low level of non-natives' isolation (i.e. an exposure index above the median). Heterogeneous effects are estimated splitting the whole schools population into subsamples according to d_s:

$$\begin{cases} y^{N}_{st} = \beta_1 P^{NN}_{st} + X^{N}_{st}\alpha_1 + W_{st}\delta_1 + S_{st}\gamma_1 + \varphi_s + \varphi_t + \eta^{N}_{st} & \text{if } d_s = 0 \\ y^{N}_{st} = \beta_2 P^{NN}_{st} + X^{N}_{st}\alpha_2 + W_{st}\delta_2 + S_{st}\gamma_2 + \varphi_s + \varphi_t + \eta^{N}_{st} & \text{if } d_s = 1 \end{cases} \quad [3]$$

The specification of Equation [2] also allows testing the basic results of the theoretical framework presented in Section 3 introducing non-linearity in an easy and flexible way. In fact, the theoretical framework predicts that the effects of non-native shares are non-linear, but rather vary with respect to different levels of P^{NN}_{st}. We introduce a linear spline functional form in the non-native school share dividing the percentage range [0; 1] into two intervals with boundaries $\underline{\theta}$, $\tilde{\theta}$ and $\bar{\theta}$, where $\underline{\theta}$ and $\bar{\theta}$ correspond, respectively, to 0 and 1.

$$y_{st}^{N} = \beta_1 P_{st}^{1} + \beta_2 P_{st}^{2} + X_{st}^{N}\alpha + W_{st}\delta + S_{st}\gamma + \varphi_s + \varphi_t + \eta_{st}^{N}$$

where: [4]

$$P_{st}^{i} = \begin{cases} \theta_{st} & \text{if } 0 \le \theta_{st} < \tilde{\theta} \\ 1-\theta_{st} & \text{if } \tilde{\theta} \le \theta_{st} < 1 \end{cases}$$

Equations [3] and [4] can be used to test the predictions of the theoretical framework, testing that $\beta_1 \geq 0$ and $\beta_2 < 0$. More precisely:

i) If $\frac{\partial y_N}{\partial P_{st}^{1}} = \beta_1 \ge 0$ when $\theta \le \tilde{\theta}$ or $d_s = 0 \Rightarrow$ the 'integration mechanism' prevails

ii) If $\frac{\partial y_N}{\partial P_{st}^{2}} = \beta_2 < 0$ when $\theta > \tilde{\theta}$ or $d_s = 1 \Rightarrow$ the 'rejection mechanism' prevails

5. Data and descriptive statistics

Data requirements to estimate peer effects differ a lot according to the identification strategy. One generally needs information on students, students' family background and schoolmates, school characteristics and environment. Using cross-sectional datasets, the definition of the peer group is crucial. If data allows to define an individual specific peer group (for example, using individual specific friends network information), then it is conceptually possible to overcome the 'reflection problem' and estimate separately endogenous and exogenous peer effects (Bramoullé *et al.* 2009, and De Giorgi *et al.* 2010). On the other hand, if the peer group is exogenously defined as the school or classmates group, a separate identification of the two effects is usually not possible, and researchers generally estimate a reduced form model aimed at disentangling the peer effects (endogenous and exogenous) from the correlated effects (Gibbons and Telhaj, 2006; Ammermueller and Pischke, 2009, among others). In addition, as outlined above, in the specific case of the estimation of 'ethnic peer effects' data from sample surveys may suffer from underrepresentation of the immigrants population and errors in immigrant shares measures can lead to substantially underestimate the effects of non-native school share on test scores (Aydemir and Borjas, 2010).

We exploit a unique dataset that combines the *Invalsi First Cycle Final Exam* data[14], administrative records from *Ministry of Education Statistical Office,* and the *Italian Population*

[14] INVALSI (*Istituto Nazionale per la Valutazione del Sistema Educativo di Istruzione e di Formazione*) is the independent public institute carring out the evaluation of Italian school system and test students' attainment levels.

Census Survey 2001[15]. *Invalsi First Cycle Exam* (from now on 'First Cycle' or 'Invalsi IC') data are the first experience of testing attainment levels of all students enrolled in Italian junior high schools. The census dimension of Invalsi IC tests allows us to overcome problems of underrepresentation of immigrant individuals and measurement errors in sample surveys. Additional information about socio-economic family background are obtained as school-level averages of Census variables linked to each school using an original matching technique that identifies for each junior high school its 'catchment area'. To our knowledge, this is the first time that a dataset with such a variety of information and covering the universe of 8th graders students is made available for the Italian school system.

In detail, Invalsi IC dataset contains school level information, Math and Language test scores results and individual information for each 8th grade student enrolled in a public or private Italian junior high school[16]. Three waves are available, corresponding to 2007-08, 2008-09 and 2009-10 school years final exams (about 500,000 students per wave). Individual information covers year of birth, gender, citizenship (Italian, non-Italian), place of birth; how long the student is in Italy if born abroad (from primary school, for 1-3 years, less than 1 year); mother's and father's place of birth (Italy, EU, European but non-EU, other non-European country), grade retention (if the student is 'regular' i.e. if he/she is 14 years old at the end of the school year; 'in advance' i.e. younger than 'regular' students, or 'retained' i.e. older than 'regular' students), school and class identifier. Administrative records from Ministry of Education Statistical Office provide general information about school characteristics (i.e. type of school, public vs. private, number of students enrolled and number of teachers, average class size) matched to Invalsi First Cycle data through an anonymous school identifier. Finally, Census 2001 contains information about resident population in Italy in 2001. Each school is matched to a group of census divisions through an original matching technique designed to associate to each junior high school a group of census cells constituting its 'catchment area' (Barbieri, Rossetti and Sestito, 2010)[17]. This procedure allows matching to each junior high school more than two hundreds variables from 2001 Italian Population Census Survey covering a great variety of demographic and socio-economic information on resident population (gender, age, ethnic origins, education, labour force participation, occupation, households' composition and houses characteristics).

5.1 Descriptive statistics

[15] Many people collaborate to make available the dataset used. We thank: Claudio Rossetti (Luiss), Patrizia Falzetti (Invalsi) and Marco Mignani (Invalsi) for their work in merging Census and Miur data with the Invalsi IC datasets; Paolo Sestito (Bank of Italy), Piero Cipollone (Invalsi and Bank of Italy) for fruitful discussions and data support.

[16] Test scores range from 0 to 100 and refer to the fraction of right answers for each of the two subjects.

[17] See Appendix B for a detailed description of data and matching techniques used.

We exploit the panel dimension of the dataset constituted by 5771 junior high schools[18] (*s=1…5771*) and three school years (*t=2008, 2009, 2010*). Mean test scores and mean individual characteristics are obtained from all 8th grade students enrolled in all Italian junior high schools[19] in 2007-08, 2008-09 and 2009-10 (1,504,286 individuals), while school characteristics are matched from Census and administrative school records as explained above.

Table 1 describes general characteristics of the junior high schools in the dataset (percentage of public schools, non-native school share, average school size, average class size) with respect to macro-area and Invalsi IC wave, while Figure 2 shows the average percentage of non-native students per school (i.e. 'school shares'). The distribution of non-native students across Italian territory is highly not homogenous: Northern and Centre regions experience the highest average school share of non-native students (10.01% and 9.18% in 2010), while it dramatically falls in the South (1.97%), while school characteristics, such as average school and class size are generally equally distributed. Table 2 shows school average and standard deviation of test scores results according to the native/non-native partition of each school population: gaps between mean test scores for natives and non-natives are large and statistically significant. Descriptive evidence confirms general results common in the European literature: first, non-native students perform worse than their native peers; second, gaps are greater in Language and lower for Math. The distribution of school mean test scores is also different: non-native students' test score distribution is more similar to a normal distribution, and shows a higher variance.

We focus on the test score gap between native and non-native students at the individual level in Table 3 where we report the coefficient of the dummy variable '*being non-native*' obtained running descriptive (pooled) OLS regressions on the whole sample of IC 2009 and 2010 students. We first show the row coefficient, i.e. the unconditioned attainment gap: non-native students have test score results lower than native peers by 21.21% in Language, and 15.08% in Math. Then, we progressively add controls for individual characteristics (gender, retention, parents' origins, time spent in the host country since birth), school characteristics (ownership, type, size, average class size, pupil-teacher ratio, support teacher-pupil ratio) and territorial dummies[20]. The conditioned gaps turn out to be smaller than the unconditioned one, but still significantly different from zero: *coeteris paribus*, being non-native implies a 7% lower test score in Language and 4% in Math.

Apart from the sharp differences in non-native students school shares across regions and areas, two main results may be drawn from these general descriptive evidence. First, there exists a

[18] From the original population of 6290 schools, almost 5% are dropped because they appear in only one wave.
[19] We exclude all individuals who did not sit either Maths or Italian Language test (0.73% of the total students population).
[20] For the complete list and explanation of control variables used see Table 4.

sizable gap in test scores results between native and non-native students, and this attainment gap seems to be more critical in Language rather than in Math skills. Second, even after taking into account individual characteristics, parental background, school characteristics and territorial differences, the attainment gap is reduced but still persists. Therefore, descriptive evidence supports the basic assumption of the theoretical framework: *coeteris paribus*, non-native students may cause more disruption in the classroom compared to native peers ($p_{NN}>p_N$) because of lower attainments levels and poorer language skills. Moreover, given that the gap does not disappear controlling for usual school and family background inputs, it is plausible to think that 'social' inputs and peers' externalities may play a crucial role in explaining these gaps (Akerlof and Kranton 2000, 2002; Zenou and Patacchini 2006; Heckman 2011; Freyer 2011, among others).

6. Results

In this section, we first present the baseline model results and then we test for heterogeneous and non-linear effects in order to find evidence to support or reject the theoretical model we propose in Section 3. As already outlined, the variable of interest is the non-native school share (P_{st}^{NN}) and the parameter β captures both the exogenous effects operating through student's peers characteristics and the (pure) endogenous effects operating through students' peers achievement.

6.1 Baseline model results

Table 5 contains the results for the estimation of the parameter of interest from eq. [1] and [2]. The dependent variable is the log of the Invalsi IC school mean test score for native students, and we conduct our analysis separating the Language from the Math test score. The rationale for doing it being that we expect peer effects to have greater impact on Language tests as long as language skills are directly influenced by the use of Italian language with native peers in the classroom. We progressively add school variables controls (S_{st}) in columns (II), and catchment-area socio economic variables (W_{st}) in columns (III)[21]. Thus, the coefficients estimated in columns (I) correspond to eq. [1], while the ones estimated in columns (III) to eq. [2]. Adding school and catchment-area controls significantly influences the estimates, improving the school fixed-effects basic framework and limiting the possible biases due to across school sorting. In fact, focusing on the estimates of β from eq. [2] (columns III) we have small and negative effects, statistically different from zero only for Language test score. Thus, increasing non-native school share by 1%

[21] See Table 4 for the complete list of variables and description.

determines a decrease of 6.5% in native peers' Language test score, and no significant effects in Math.

These results are in line with the limited evidence from European literature on peer effects between immigrants and native students (see Brunello and Rocco, 2011), and with evidence from U.S. (Angrist and Lang, 2004; Hoxby, 2000; Hanushek and Rivkin, 2009). However, to test the implications of the theoretical framework proposed, we have to allow for heterogeneous effects and non-linear effects in non-natives school share.

6.2 Non-linear and heterogeneous effects: which underlying mechanism is at work?

To sum up the basic intuition of the theoretical model, if the integration process is at work, or worked throughout the whole school year, there is not difference in the disruption attitudes between the two types ($p_{NN} \rightarrow p_N$), thus non-native share should not have negative and significant impacts on natives' attainment. On the contrary, if integration mechanisms is not at work, non-native and native students tend to isolate themselves into separate peer groups and the disruption parameter of non-native students would not converge to the one of native peers ($p_{NN} > p_N$) and, consequently, natives attainment will be hurt by non-natives. The transition from the 'integration' to the 'rejection' mechanism is driven by the isolation of non-native students. Integration is more likely to happen when non-native students are relatively more isolated, and therefore it is not (or, at least, less) costly for native peers integrate them. In order to retrieve the estimates of β_1 and β_2, heterogeneous effects split the whole school population into two groups according to different levels of isolation experienced by non-native students ($d_s=1$ low isolation; $d_s=0$ high isolation), while non-linear effects seek for structural break into the distribution of the non-native school share variable, so that the estimated parameters are different below and above a given threshold ($\tilde{\theta}$). Thus, equations [3] and [4] can be both consistently used to test:

i) $\frac{\partial y_N}{\partial P_{st}^1} = \beta_1 \geq 0 \rightarrow$ the integration mechanism prevails

ii) $\frac{\partial y_N}{\partial P_{st}^2} = \beta_2 < 0 \rightarrow$ the rejection mechanism prevails

<u>Heterogeneous effects</u>

Following the definition provided in §4.2 we split the sample according to a high (above the median) and low (below the median) level of the exposure index. Exposure is commonly used as a measure of isolation: a high exposure of non-native to native peers implies a low isolation, and vice versa (Freyer, 2011). Table 6 shows the results from the estimation of eq. [3]: the effects on natives' attainment due to non-native peers are negative and significant only in schools characterized by high levels of exposure (β_2<0), while they are not statistically different from zero when exposure is low (β_1=0). Interestingly, we find negative effects both in Math and Language, contrary to the non-heterogeneous baseline model. Therefore, these results support the theoretical predictions: when non-natives are more isolated the integration process is working, while when isolation is reduced non-natives' school share determines negative effects on natives' attainment (increasing of 1% the non-native school share, determines a decrease in natives' test scores by 8% in Language and 7% in Math). In the sensitivity analysis we test the robustness of these results using different measures of isolation to split the sample, showing that results do not depend on the particular measure chosen.

Non-linear effects

We test for non-linarites in the effects of non-native school share on natives' test scores estimating β_1 and β_2 using a spline linear function with one break point ($T=\tilde{\theta}$), eq. [4]. To seek for structural changes in the effect we use different values of the break and report the results in Table 7. This allows to show in a flexible way how effects are different above and below any given threshold, and if they are statistically significant. First, we notice that effects are highly non-linear (we always reject the null that $\beta_1 - \beta_2 = 0$). For instance, setting the threshold at the mean of the non-native school share distribution (T=0.065) we have that increasing by 1% the non-native share has not significant effects if the non-native school share is below 6.5% (T), while it decreases natives' language test scores by 7.6% if the share is above 6.5%. Thus, both in Language and Math, the general pattern of the results confirms that the increase of non-native share has negative and significant effects only for sufficiently large values of $\tilde{\theta}$. To be more precise, we cannot reject the null that β_1=0 and β_2<0 for T<0.20, while if T>0.20 β_1 and β_2 are both negative and significant for Language test.

We go deeper into the analysis introducing a spline function with two break points, where the first one is fixed at 10% (T_1=0.10) and we set different values for the second (T_2). The rationale is the following: with one break point we exclude that the structural break ($\tilde{\theta}$) is greater than the threshold of 10%, indeed the effects above 10% are still unclear. Table 8 shows the results for three possible break points for T_2=0.20; 0.25; 0.30[22]. Results for Math test score do not show clear patterns, while for Language we always find negative and significant effects between 10% and 20, 25 and 30% levels of non-native school share.

Summing up and putting together the results from Table 7 and Table 8, we have that non-linear effects are stronger for Language test score, while less clear for Math. In details: for the Language test, we cannot reject the null that β_1=0 and β_2<0 for $\tilde{\theta}$ < 0.10, while for Math the same

[22] Additional robustness checks for other thresholds between 0.20 and 0.30 are always consistent with these results.

result holds for $\tilde{\theta} < 0.20$. To give a numerical intuition, we calculate that, on average, a non-native school share of 10% corresponds to 9 non-native students in the school, or, equivalently, 1 or 2 non-natives students in each class. If in each class there are one or two non-native students, the 'integration' process works and their presence do not significantly affect natives' attainment. Interestingly, this result is stronger for language skills.

7. Sensitivity analysis

We test the robustness of our results under three dimensions. First, we test the robustness with respect to missing values in school and catchment-area variables due to the dataset construction. Then we test for possible concerns due to the main source of endogeneity (across school sorting). Finally we try different specifications of the 'isolation measure' used for the estimation of eq. [3].

7.1 Missing values in school and catchment-area variables

Missing values in school and catchment-area variable are due to the construction of the dataset. This fact causes the number of schools in the regression estimates to shrink from 6,289 in the estimation of eq. [1] to 4827 in the estimation of eq. [2] (Table 5). We test the robustness of the baseline results controlling for missing values and correcting missing values with some approximations, where possible, to verify that the results are not driven by any kind of sample selection. Indeed, a preliminary analysis with OLS regressions excludes any particular pattern of missing values due to geographical school location.

The variable containing the information about the 'stock of non-native students' in the school is missing for 16% of schools due to school register data missing. We correct this variable in two possible ways: *(i)* setting missing values to zero and creating an indicator variable taking value 1 when this information was corrected; *(ii)* we replace the missing values with the total stock of 8th graders non-natives students in the three available school years. Catchment-area variables are missing because the matching procedure between the school identifier and the census cells failed due to some non-perfect overlapping between the school identifier in the Invalsi data and the one in the Census data. However, for more than half of them we can replace the missing values of the socio-economic variables of the school catchment-area with the average value of the same variables taken from the schools which are located in the same municipality. This correction procedure shrinks missing data on catchment-area variables from 6.3% to 4.6% of schools. Table 9 shows that implementing the correction procedures allows keeping all the observations but does not modify previous results, which, in turn, are not due to some selection pattern in the missing data.

7.2 Possible concerns from across school sorting

The identification strategy implemented by eq. [2] is designed to control for across school sorting through school fixed-effects, territorial by year fixed effects and school specific catchment-area socio economic variables. To test that the identification strategy is suitable to capture this main source of endogeneity, we split the sample of schools into two groups according to school location in big or small municipalities. We define to as 'big municipalities' those having three or more junior high schools in their territory, while 'small municipalities' have one or two junior high schools. The enrolment rules are based on residency criteria, therefore students have to attend the junior high school in the same municipality where they live with their family. If there is more than one school, then families usually have to enrol the child to the school of the area where they reside, otherwise they are allowed to enrol the child to another junior high school of the municipality, if free slots are available.

Thus, the enrolment institutional framework limits per se across school sorting, however this is still possible and more likely to happen in big municipalities, where there is a sufficiently large number of junior high schools and families have some degree of 'choice'. Moreover, 'big municipalities' are the ones located in more urbanized areas, which benefit from higher public transportation means that could favour, to some extent, the commuting process from the residency place to a distant junior high school, alternative to the one nearby home. Thus, we run separately eq. [2] on the subsample of small and big municipalities. If across school sorting is at work, the estimations should differ substantially in the two groups of schools inducing a negative spurious correlation between natives' mean test scores and non-native shares, and downward bias in the estimation of β. Given that across school sorting is more likely to happen in urban areas (i.e. big municipalities group), concerns for across school sorting would then arise if we systematically find that $\left|\beta_{big\_municip}\right| > \left|\beta_{small\_municip}\right|$. Estimations in Table 10 reject this hypothesis: effects are similar in the two subsamples, though slightly larger, in absolute terms, in small municipalities.

An additional sensitivity check was carried out using instead of the five areas territorial dummies (North East, North West, Centre, South, Islands), 103 territorial dummies corresponding to junior-high school districts (which also correspond to Italian Provinces, NUT5). School districts by year fixed-effects and school fixed-effects would capture any kind of across-school sorting within each school district. Table 11 shows that the effects do not change with respect to the baseline specification. Thus we can conclude that across schools sorting is adequately captured in our empirical model.

7.3 Alternative 'isolation measure'

We build an alternative measure of isolation to show that the heterogeneity in the effects is not driven by the use of the exposure index. Following Lazear (2001) we define an isolation index as the ratio between non-native and native students, i.e. the ratio between the numbers of the two types in each school. Thus, we build the isolation index (I_s) as the ratio between non-native students in school s (as the sum of non-native students in each class of the school s, C_s) and native students:

$$I_s = \frac{\left[\sum_{j=1}^{C_s} NN_{js}\right]}{\left[\sum_{j=1}^{C_s} N_{js}\right]}$$

I_s gets larger for lower levels of isolation. Thus, we estimate eq. [3] defining $d_s=1$ if the school has an isolation index below the median, and, vice-versa, $d_s=0$ if the school has an isolation index above the median. Table 12 confirm robustness of previous results based on the exposure index. Focusing on Language, β_1 is non-significantly different from zero (though higher with respect to the analysis with the exposure index) and $\beta_2<0$. Both the effects are non-statistically different from zero for Math test. The main intuition of the model seems to be confirmed especially in Language skills.

8. Conclusions

This paper shed light on peer interaction between native and non-native students contributing to the existing literature in three main aspects. First, we provide a theoretical framework to interpret possible underlying social mechanisms that work through peer interactions; second, we estimate the effect of non-native school share on natives' attainments identifying the social interaction parameter (β); third, allowing for heterogeneous and non-linear effects, we provide empirical evidence to support the stylized predictions of the theoretical framework.

The estimation results are of substantial interest *per se*, given the limited evidence in European setting of peer effects between natives and immigrants, and given the growing relevance of the immigration phenomenon and its impacts, not only on the labour markets, but also on the school system. Increasing non-native school share by 1% determines a decrease of 6.5% in native peers' Language test score, and no significant effects in Math. These results are in line with the limited evidence from European literature on peer effects between immigrants and native students (see Brunello and Rocco, 2011), and with evidence from U.S. (Angrist and Lang, 2004; Hoxby, 2000; Hanushek and Rivkin, 2009; Sacerdote, 2010) which finds limited evidence of negative 'between-groups' effects. Moreover, the empirical analysis confirms the theoretical framework proposed and points to 'isolation' of non-native students as a possible transition device that determines whether 'integration' or 'rejection' mechanism is at work.

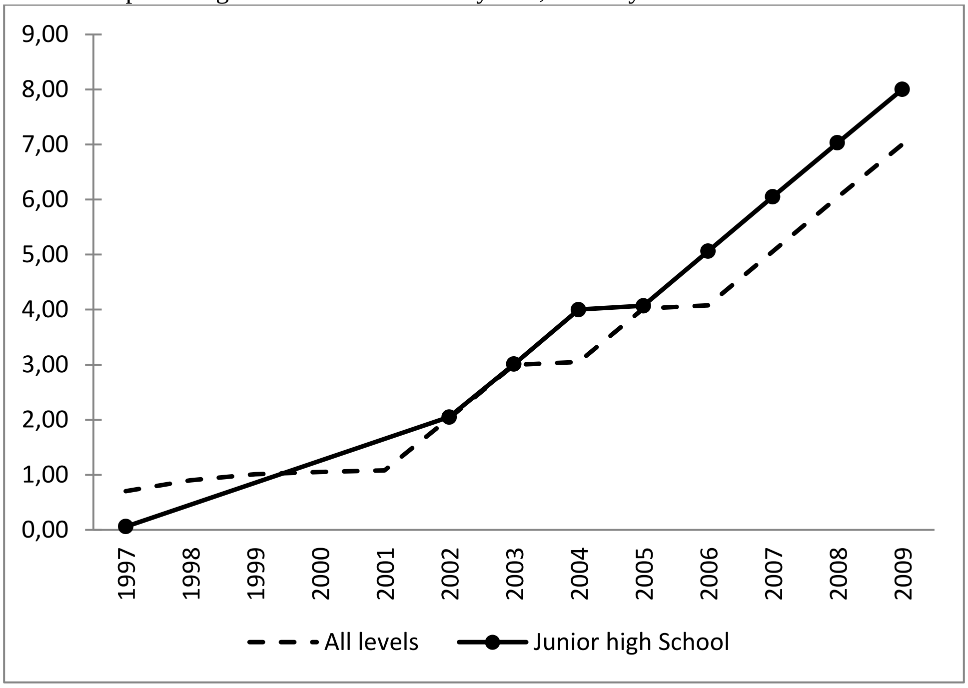
The negative effects are concentrated in schools where non-native students experience limited isolation to non-native peers and are highly non-linear with respect to non-native school share. We calculate that if in each class there are one or two non-native students (or, equivalently, the non-native share is below 10%), the 'integration' process works and their presence do not significantly affect natives' attainment. Interestingly, all the results are stronger for Language test scores, confirming that language skills are more influenced by peer interaction between native and non-natives, rather than Mathematical competences. Indeed, more research has to be undertaken to study peer effects within the peer group of non-natives students, to understand to which extent their concentration in the school or in the class could harm themselves and induce their clustering.

Our work also suggests important policy implications concerning allocation rules of non-native students across classes and across schools. The driving idea of any allocation rule should be to avoid any concentration on non-native students in the same class or school, and rather to distribute them equally. In general, our results show that a relative isolation of non-native students from other non-native peers is beneficial for natives as it forces the integration mechanism between the two peer groups. A non-native school share below 10% in each school would ensure this mechanism to be at work. For instance, a recent regulation act from the Italian Ministry of Education imposes a cap threshold of 30% to non-native share in each class[23]. According to our findings, this threshold would be inefficiently high and may not have any effect to the educational production in the classroom.

[23] "*Indicazioni e raccomandazioni per l'integrazione di alunni con cittadinanza non italiana*", MIUR, Circolare Ministeriale No. 2/2010 (C.M. 8/1/2010, n. 2).

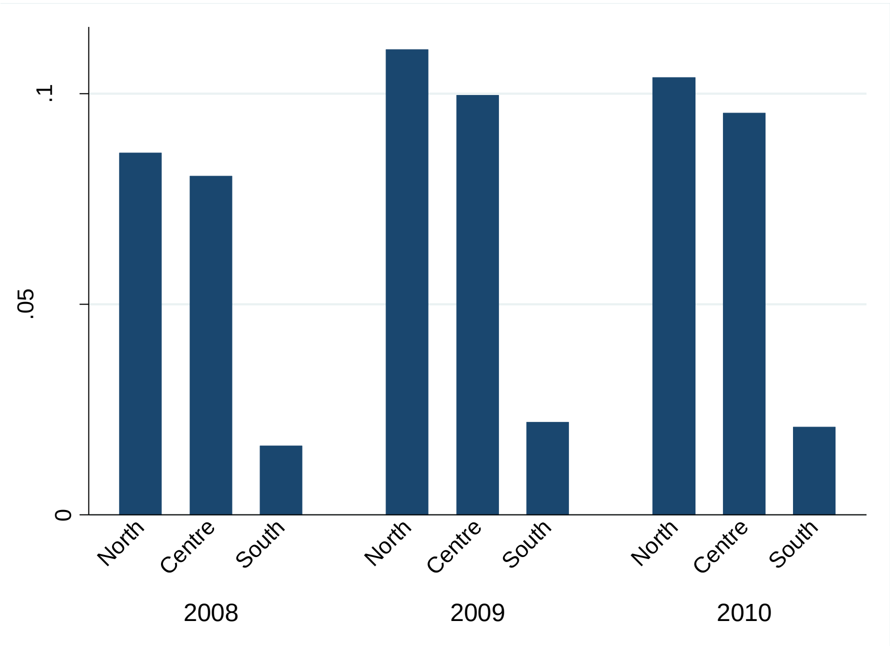
Figures

Figure 1. Non-native students percentage in the Italian school system, from s.y. 1996-07 to 2008-09.



Source: own elaboration on MIUR (2009) data.

Figure 2. Average percentage of non-native student per school (i.e. 'school share') by macro-area and school year (Invalsi IC data).



Tables

Table 1. School level descriptive statistics.

Wave Invalsi IC	*Area*	*No. Students*	*No. Schools*	*% Public Schools*	*% Non-native students*	*Avg. No. Students per School*	*Avg. No. Students per Class*	*% Schools linked to Catchment Area Info.*
2007-08	North	201,650	2313	83.48	9.49	295.32	21.08	95.72
	Centre	89,870	998	85.77	8.07	301.62	20.68	94.99
	South	204,339	2388	95.27	1.48	287.48	19.36	93.34
	Tot.	*495,859*	*5699*	*88.82*	*5.95*	*293.11*	*20.28*	*94.59*
2008-09	North	211,567	2359	83.59	11.20	341.94	21.20	94.82
	Centre	93,440	1017	86.52	8.97	342.03	20.83	94.00
	South	205,856	2427	95.09	1.83	298.58	19.75	93.08
	Tot.	*510,863*	*5803*	*88.91*	*7.04*	*322.21*	*20.48*	*93.95*
2009-10	North	206,530	2368	83.78	11.24	306.61	21.30	93.12
	Centre	91,629	1009	86.72	9.28	315.50	21.00	92.86
	South	199,405	2356	95.33	1.84	291.01	20.08	91.85
	Tot.	*497,564*	*5733*	*89.04*	*7.12*	*301.72*	*20.74*	*92.55*

Table 2. Mean and standard deviation of Invalsi IC average school test scores for native and non-native students.

MEAN						
	Language			Math		
Wave Invalsi IC	Natives	Non-natives	Delta	Natives	Non-natives	Delta
2007-08	68.73	59.44	9.29***	54.15	47.65	6.50***
2008-09	67.01	53.39	13.62***	66.11	56.28	9.83***
2009-10	65.24	55.80	9.44***	55.74	49.82	5.92***
SD						
	Language			Math		
Wave Invalsi IC	Natives	Non-natives	Ratio	Natives	Non-natives	Ratio
2007-08	6.36	11.77	0.54***	8.79	12.55	0.70***
2008-09	8.22	15.01	0.55***	9.43	15.51	0.61***
2009-10	7.05	11.11	0.63***	8.16	11.07	0.74***

Notes. Test scores range from 0 to 100 (percentage of right answers). Delta indicates the difference between test score means of (native – non-native); *** indicates whether Delta>0 (ttest, p.val≤0.001); Ratio indicates the ratio between test score sd of (native / non-native); *** indicates whether Ratio<1 (p.val≤0.001).

Table 3. Gap in individual test scores between native and non-native students (Invalsi IC 2009-2010).

	Non-native students attainment gap (Pooled OLS)			
	Italian Language test score			
Non-native (dummy)	-0.2121***	-0.0685***	-0.0773***	-0.0716***
	(0.0025)	(0.0039)	(0.0039)	(0.0038)
	Math test score			
Non-native (dummy)	-0.1508***	-0.0455***	-0.0483***	-0.0405***
	(0.0025)	(0.0047)	(0.0042)	(0.0040)
Clusters	6215	6190	5513	5513
N	994593	852099	794896	794896
Controls:				
Year Dummies	*X*	*X*	*X*	*X*
Individual Characteristics		*X*	*X*	*X*
School Characteristics			*X*	*X*
Province FE				*X*

*(Robust Std. Errors in parenthesis, Clustered at the School level). Significance level: * p<0.1, ** p<0.05, *** p<0.01*

Notes. Coefficients are obtained from the dummy variable 'being non-native' through pooled OLS regressions performed at the individual level. For detailed description of control variables included in the individual and school characteristics see Appendix B, Table B.1. Province fixed-effects (*Province FE*) include 103 territorial dummies.

Table 4. Control variables used, description and source.

Type	*Name*	*Description*	*Source*
Individual (X)	*female*	Fraction of group j females in school s	Invalsi / MIUR
	late	Fraction of group j retained students in school s	
	father place of birth	Fraction of group j students in school s with father born abroad	
	always_italy	Fraction of group j students in school s in Italy since birth	
	mother place of birth	Fraction of group j students in school s with mother born abroad	
School (S)	*nonnatives_stock*	Total number of non native students in the school	
	School_size	School size, given by the total number of students in the school	
	High_cheating_dummy (subject specific)	Dummy equal 1 if the school is in the 9^{th} decile of the school cheating coefficient distribution	
Catchment Area (W)	*lpop*	Log of total resident population	Census 2001
	illiterate	Fraction of illiterate pop.	
	university_edu	Fraction of pop. with university level education	
	m_occup_rate	Male occupation rate	
	f_occup_rate	Female occupation rate	
	foreign_citizens	No. of non-Italian residents	
	agri_oc	Fraction of workers occupied in agriculture	
	self_empl	Fraction workers self-employed	
	commuter	Fraction of resident commuting every day for school or working reasons	
	avg_family_members	Average number of family members	
	house_poor	Fraction of houses without clean water	
	house_new	Fraction of houses built after 1980	
	avg_rooms	Average number of rooms per house	

Table 5. Baseline model results with school fixed-effects.

	Dep. Var.: School Mean Log Score for NATIVE students					
	Language			Math		
	(I)	(II)	(III)	(I)	(II)	(III)
Non-native SS	-0.0867***	-0.0584**	-0.0653**	-0.0866**	-0.0428	-0.0499
	(0.0230)	(0.0249)	(0.0253)	(0.0345)	(0.0336)	(0.0344)
R sq.	0.172	0.289	0.302	0.462	0.587	0.589
Adj.R sq.	0.171	0.288	0.300	0.461	0.587	0.587
Clusters	6289	5115	4827	6290	5115	4827
N	17198	14368	13851	17199	14368	13851
Controls						
Individual Charact. (X), school and year FE	X	X	X	X	X	X
School variables		X	X		X	X
Catchment Area*Year FE			X			X

(Robust Std. Errors in parenthesis, Clustered at the School level). Sig. level: * p<0.1, ** p<0.05, *** p<0.01

Table 6. Heterogeneous effects: high vs. low exposure.

	Dep. Var.: School Mean Log Score for NATIVE students			
	Language		Math	
	High Exposure (β_2)	Low Exposure (β_1)	High Exposure (β_2)	Low Exposure (β_1)
Non-native SS	-0.0810***	-0.0703	-0.0710**	-0.0440
	(0.0253)	(0.0763)	(0.0361)	(0.0941)
R sq.	0.109	0.368	0.646	0.581
Adj.R sq.	0.103	0.364	0.643	0.579
Clusters	2514	2959	2514	2959
N	6303	7136	6303	7136
All Controls	X	X	X	X

(Robust Std. Errors in parenthesis, Clustered at the School level). Sig. level: * p<0.1, ** p<0.05, *** p<0.01

Table 7. Non-linear effects: spline linear functions with one structural break (T)

	Dep. Var.: School Mean Log Score for NATIVE students				
	Language				
	T=0.04 (med)	T=0.065 (mean)	T=0.10 (P75)	T=0.20 (P95)	T=0.25 (>P95)
Share < T (β_1)	-0.0648	-0.0239	-0.0185	-0.0469*	-0.0478*
	(0.1080)	(0.0686)	(0.0452)	(0.0278)	(0.0259)
Share > T (β_2)	-0.0628**	-0.0764**	-0.0954**	-0.1199*	-0.1615*
	(0.0291)	(0.0319)	(0.0379)	(0.0728)	(0.0949)
R sq.	0.300	0.300	0.300	0.300	0.300
Adj.R sq.	0.298	0.298	0.298	0.298	0.298
Clusters	4825	4825	4825	4825	4825
N	13576	13576	13576	13576	13576
	Math				
	T=0.04 (med)	T=0.065 (mean)	T=0.10 (P75)	T=0.20 (P95)	T=0.25 (>P95)
Share < T (β_1)	-0.0844	-0.0047	0.0234	-0.0078	-0.0147
	(0.1448)	(0.0939)	(0.0620)	(0.0381)	(0.0350)
Share > T (β_2)	-0.0405	-0.0612	-0.0977*	-0.1840*	-0.2536*
	(0.0397)	(0.0440)	(0.0528)	(0.1084)	(0.1417)
R sq.	0.589	0.589	0.589	0.589	0.589
Adj.R sq.	0.588	0.588	0.588	0.588	0.588
Clusters	4825	4825	4825	4825	4825
N	13576	13576	13576	13576	13576
All Controls	X		X	X	X

(Robust Std. Errors in parenthesis, Clustered at the School level). Significance level: * p<0.1, ** p<0.05, *** p<0.01

Table 8. Non-linear effects: spline linear functions with two break points (T1=0.10 and T2=0.2, 0.25, 0.3).

	Dep. Var.: School Mean Log Score for NATIVE students					
	Language			Math		
	T1=0.10 T2=0.20	T1=0.10 T2=0.25	T1=0.10 T2=0.30	T1=0.10 T2=0.20	T1=0.10 T2=0.25	T1=0.10 T2=0.30
Share < T1 (β_1)	-0.0215	-0.0241	-0.0249	0.0087	0.0095	0.0136
	(0.0465)	(0.0459)	(0.0456)	(0.0630)	(0.0624)	(0.0622)
T1<Share<T2 (β_2)	-0.0820*	-0.0723*	-0.0697*	-0.0304	-0.0397	-0.0577
	(0.0471)	(0.0397)	(0.0388)	(0.0639)	(0.0528)	(0.0514)
Share > T2	-0.1114	-0.1544	-0.2133*	-0.1785	-0.2463*	-0.2812
	(0.0741)	(0.0952)	(0.1222)	(0.1104)	(0.1418)	(0.1785)
R sq.	0.300	0.300	0.301	0.589	0.589	0.589
Adj.R sq.	0.298	0.298	0.298	0.588	0.588	0.588
Clusters	4825	4825	4825	4825	4825	4825
N	13576	13576	13576	13576	13576	13576
All Controls	X		X		X	X

(Robust Std. Errors in parenthesis, Clustered at the School level). Significance level: * p<0.1, ** p<0.05, *** p<0.01

Table 9. Sensitivity analysis to missing variables.

	Dep. Var.: School Mean Log Score for NATIVE students			
	Language			
	(I)	(II)	(III)	(IV)
Non-native SS	-0.0639**	-0.0616**	-0.0718***	-0.0732***
	(0.0253)	(0.0250)	(0.0237)	(0.0229)
R sq.	0.302	0.300	0.276	0.272
Adj.R sq.	0.299	0.298	0.274	0.270
Clusters	4827	4992	5557	6150
N	13855	14142	16114	16919
	Math			
	(I)	(II)	(III)	(IV)
Non-native SS	-0.0512	-0.0450	-0.0534	-0.0581*
	(0.0344)	(0.0340)	(0.0329)	(0.0330)
R sq.	0.589	0.589	0.573	0.574
Adj.R sq.	0.587	0.588	0.571	0.573
Clusters	4827	4992	5557	6151
N	13855	14142	16114	16920
All Controls	X	X	X	X
Catchment Area Missing Correction		X		X
School Variable Missing Correction			X	X
(Robust Std. Errors in parenthesis, Clustered at the School level). Sig. level: * p<0.1, ** p<0.05, *** p<0.01				

Table 10. Sensitivity analysis: big vs. small municipalities.

	Dep. Var.: School Mean Log Score for NATIVE students			
	Language			
	Big Municipalities	Small Municipalities	Big Municipalities	Small Municipalities
Non-native SS	-0.0561*	-0.0808	-0.0559**	-0.0983**
	(0.0288)	(0.0522)	(0.0281)	(0.0403)
R sq.	0.305	0.316	0.301	0.242
Adj.R sq.	0.302	0.306	0.298	0.236
Clusters	3757	1070	3949	2201
N	10873	2984	11342	5577
	Math			
	Big Municipalities	Small Municipalities	Big Municipalities	Small Municipalities
Non-native SS	-0.0386	-0.0938	-0.0344	-0.1049
	(0.0375)	(0.0796)	(0.0366)	(0.0656)
R sq.	0.595	0.579	0.596	0.538
Adj.R sq.	0.593	0.572	0.594	0.534
Clusters	3757	1070	3949	2202
N	10873	2984	11342	5578
All Controls	X	X	X	X
Corrected Missing			X	X
(Robust Std. Errors in parenthesis, Clustered at the School level). Significance level: * p<0.1, ** p<0.05, *** p<0.01				

Table 11. Sensitivity analysis: school district fixed-effects (province * Year).

	Dep. Var.: School Mean Log Score for NATIVE students			
	Language		Math	
Non-native SS	-0.0492*	-0.0636***	-0.0410	-0.0533
	(0.0252)	(0.0228)	(0.0344)	(0.0332)
R sq.	0.340	0.311	0.604	0.588
Adj.R sq.	0.328	0.301	0.597	0.582
Clusters	4827	6150	4827	6151
N	13857	16917	13857	16918
All Controls	X	X	X	X
Province * Year FE	X	X	X	X
Corrected Missing		X		X

(Robust Std. Errors in parenthesis, Clustered at the School level). Significance level: * p<0.1, ** p<0.05, *** p<0.01

Table 12. Sensitivity analysis on heterogeneous effects: isolation index.

	Dep. Var.: School Mean Log Score for NATIVE students			
	Language		Math	
	Low Isolation (β_2)	High Isolation (β_1)	Low Isolation (β_2)	High Isolation (β_1)
Non-native SS	-0.0606**	-0.2543	-0.0444	-0.2735
	(0.0282)	(0.1769)	(0.0422)	(0.2261)
R sq.	0.159	0.371	0.608	0.597
Adj.R sq.	0.154	0.367	0.606	0.594
Clusters	3167	2888	3167	2888
N	7366	6488	7366	6488
All Controls	X	X	X	X

(Robust Std. Errors in parenthesis, Clustered at the School level). Sig. level: * p<0.1, ** p<0.05, *** p<0.01

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