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School Heterogeneity, Parental Background and Tracking: Evidence from PISA 2006

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School Heterogeneity, Parental Background and Tracking: Evidence from PISA 2006

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Abstract: So far the empirical literature using large international students' assessments neglects the role of school composition variables in order not to incur in a misidentification of peer effects. However, this leads to an error of higher logical type since the learning environment crucially depends on peers' family background and on school heterogeneity. In this paper, using PISA 2006, we show how school heterogeneity is a key determinant of student attainment and of opportunity equalization. Interestingly, the effect of school compositional variables differs depending on the country tracking policy. School heterogeneity reduces efficiency in comprehensive schooling systems whereas it has a non-linear impact in early-tracking ones. In turn, linear peer effects are higher in early-tracking systems. Besides, higher heterogeneity tends to equalize student differences related to family background. Results remain robust in school- and student-level regressions suggesting that the impact of heterogeneity is correctly identified. Results are also robust when we add school-level dummies, school compositional variables and several controls correlated with the school choice to alleviate the selectivity bias of linear peer effects.

Key words: school heterogeneity, peer effects, schooling tracking, educational production function, equality of opportunities.

1. Introduction

The quality of the educational system is recognized to have a remarkable impact on growth and on the equalization of student outcomes. Large international assessment programs constitute a valid tool for analysing how differences in educational policies translate into different student outcomes, circumventing problems of skill comparability. Existing studies using these surveys attempt to reconcile the observed lack of correlation between resource invested and educational outcomes accounting for the institutional features of the educational process, such as the ones associated to the degree of autonomy of the school and of the accountability (e.g. Woessmann et al. 2010). Much

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less attention has been devoted to analyse the effect of the school (or class[1]) composition by background and/or abilities —i.e. the so-called 'peer effect'.

By fostering or hindering skill formation, peer effects and social interactions have provided to be a fundamental source of both efficiency—if peer effects are non-linear (Benabou 1996)—and intergenerational inequality (Durlauf 2004). Moreover, especially up to the secondary level of education, the influence of the social background at school has been shown to be critical for the development of cognitive skills and therefore of labour market success (Carneiro and Heckman 2003; Fernandez and Rogerson 1996; Hanushek and Woessmannn 2010). In spite of the well-recognized importance of these effects, the limitation to the cross-sectional dimension and the lack of initial information on student ability makes it difficult to identify linear 'peer effects' using these surveys. In particular, the fact that the assignment of individual to classes and schools of different quality is endogenous severely distorts the estimation of peer effects[2].

However, neglecting to account for the characteristics of the social interactions and, more in general, of the 'external environment' at school can also raise serious biases in the estimates; at least as large as the ones that would emerge from not considering endogeneity issues in the estimates of peer effects[3]. So far, except for few recent cases[4], the mainstream empirical strategy in studies using large international assessment programs attempted to minimize the first-type of bias associated to an improper identification of peer effects. This, however, brings to a second-type of error, *of higher logical level*, associated to a misspecification of the true educational production function.

The 'error' made by not explicitly including "class- or school-compositional variables" is of great policy concern when the characteristics of the school environment are strongly linked with

[1] Note that peer effects at the class and the school level capture two different ways in which social interactions affect student outcomes: whereas the former is more correlated with direct effects on the learning environment, the second include a broader set of interactions.

[2] For instance, since the school composition is endogenous both to educational policies, such as the admission procedures and the age of tracking, and to the characteristics of the neighbourhood of residence, the choice of the school would be strongly influenced by unobservable individual, parental and urban characteristics. An important caveat is required here. If the selection problem is not perfectly solved and hence identification of the peer is not transparent, the estimated coefficient on peer variable turns out measures both the school and the community peer effects (Toma and Zimmer 2000). The distinction of the two effects is crucial for targeting policies at the national level, whereas in international comparisons the exact estimates of the production function externalities generated at school, net of the community externality due the social interaction out-of-school, is far less important as urban and schooling policies are intrinsically indistinguishable.

[3] Another well-known problem is the one of reflexivity, namely each student outcome is affected by the average mean of the other student and at the same time affect the outcome of all other students, (Maskin 1993). Due to the reflexivity problem, the data requirements for unbiased estimates of peer are almost impossible to meet. For policy purpose what is relevant is to quantify the peer effect, not to identify the source of it. Hence, many studies ignore the reflexivity problem.

[4]Vanderberghe (2002), Ranvid (2007), Schneeweis and Winter-Ebmer (2007), Entrof and Lauk (2006), Ammermueller and Pischke (2006). For the PIRLS survey, a reliable identification strategy of the peer effects is available as long as within-school variation can be exploited due to the detailed class-level information of the peer variables (see Ammermueller and Pischke 2006).

other policies that are found to affect the student choice. Of particular interest is the interaction between school compositional variables and tracking policies as long as the latter influence the school choice of student from different background and ability (e.g. Dustmann 2004; Checchi and Flabbi 2005). In turn, even in comprehensive systems –i.e. Anglo-Saxon and Scandinavian where school tracking is absent – there might be a strong tendency to self-select students by ability and background through several other factors such as residential segregation, admission procedures, use of private sector and within-school ability tracking (Waldinger 2006). Unfortunately, large international student assessments do not contain information of student and school residential locations, hence good instruments for the school composition are not available especially for this type of schooling system. Information on admission procedures and within-school sorting by ability—included in the PISA dataset used in this work—allow to partially attenuate the estimation bias of the 'school-composition effect'.

Using PISA-2006 dataset, the aim of this paper is to fill the existing gap in the literature on international comparison of educational systems by analysing how heterogeneity in the school environment, proxied by the standard deviation of the student backgrounds, affects both school efficiency and equity in reducing the impact of parental background on educational attainments. In particular, starting from a widely accepted specification of the schooling production function (see Hanushek 1986, 2003; Fuchs and Woessmann 2007), the paper seeks to investigate whether the impact of school-composition variables changes in system with different tracking policies. To partially address the selectivity bias, as a first step we carry on regressions at the schooling level in order to reduce unobservable student variability. Secondly, we move to student-level regressions (i.e. controlling for individual and parental characteristics) that allow to assess the effect of school heterogeneity in different quintiles of the test score distribution. In this case, we argue that the bias in the estimation of the school compositional variables turns out to be attenuated by including a school dummy for each quintile of the country distribution of the average parental background in the school. Moreover, as will be clearer below, the comparison of individual- and school-level estimates represents a reliable way to identify the effect of school heterogeneity on outcomes. Finally, the impact of school heterogeneity and tracking policies on the equality of opportunity is assessed taking into account confounding factors such as the duration of pre-primary education and the share of private schools (see Schuetz et al. 2008).

Our empirical analysis strongly confirms that the impact of school-composition variables is strong, very significant and the single most important determinant of student performance. Unlike previous studies[5], often focussing on a single country, heterogeneity has a significant impact on

[5] See Hanushek et al. (2003), Zimmer and Toma (2000), Rangvid (2007), Schneeweis and Winter-Ebmer (2007).

student outcomes, but the pattern followed by countries with different age of tracking widely differ. On the one hand, school heterogeneity by background reduces attainments in comprehensive schooling systems but this result is largely driven by pupils attending vocationally-oriented programs. On the other hand, in early tracking systems, there exists an optimal degree of heterogeneity that maximizes attainments. Consistently with the theoretical literature (e.g. Brunello et al. 2007), linear peer effects are found to be stronger under the early-tracking regime. Besides, as expected, higher school heterogeneity reduces the socio-economic gradient both in early and late tracking systems.

The paper is organized as follows. Next section briefly summarizes the literature to which our work is connected to and the empirical strategy adopted. Section 3 describes the data, provides some preliminary evidence supporting our way of measuring background and qualifies the main issues of the paper. In section 4 (resp. 5), we present the results of school- (resp. student-) level regressions. Section 6 analyses the effect of heterogeneity on the socio-economic gradient, whereas section 7 concludes.

2. Related literature and Empirical Strategy

The observed weak correlation between the educational inputs and student outcomes represented the main puzzle for the literature attempting to explain the determinants of educational quality (Hanushek 2003). A simple principal-agent approach to educational production claims that accounting for the institutional design of the educational sector is crucial to explain this puzzle (Bishop and Woessmann 2004). In this framework, institutions enhancing school competition, autonomy and accountability are expected to increase the pressure towards higher standards, to enable the full exploitation of local knowledge regarding students' characteristics and to reduce the risk of opportunistic behaviour that would emerge in absence of appropriate monitoring practices (Woessmannn et al. 2010). Recent empirical studies using international assessment programs supported this view of educational production and highlighted possible complementarities among different institutions; in particular, between accountability practices and the degree of school autonomy (Woessmann 2003; Fuchs and Woessmann 2007; Woessmannn et al. 2010).

Whereas at the empirical level these institutions seem to explain part of the missing correlation between resources and educational quality, background variables still represent the ones with the larger explanatory power in all the works using international surveys (e.g. Fuchs and Woessmann 2007). Hence, the puzzle of the missing resource-quality link can be explained from a theoretical perspective that explicitly includes the school composition variables as an input of educational production (De Bartolome 1992, Benabou 1996, Fernandez and Rogerson 1996). The learning environment can not, in fact, be reduced to a vector of school characteristics as long as the abilities and the home background of school- and class-mates determine the learning standards for the class as a whole and the out-of-school social context.

From a theoretical standpoint, the influence of class heterogeneity on student outcomes is ambiguous as forces going in opposite direction tend to offset each other. On the one hand, more homogeneous classes imply similar initial cognitive levels, so less teaching efforts devoted to equalize students skills. On the other hand, in heterogeneous classes various types of externalities might arise: disruptive due to the presence of students with a particular bad attitude (Laezar 2001), or positive knowledge spillovers from good students to average and/or bad ones (e.g. Durlauf 2004). Which one tends to prevail depends on the shape of the educational production function. More precisely, if school-composition effects enter linearly in the educational production function, efficiency is unaffected by the reallocation of students to schools and classes; the opposite occurs in the non-linear case (Benabou 1996). These considerations turn out to have important policy implications as long as the matching process of students to schools might depend in a substantial way from factors beyond the sphere of educational policies. For instance, the rich can successfully isolate themselves by approving residential restrictions to school admission. More in general, the assignment of students to schools of different quality that maximizes aggregate human capital is very unlikely to emerge as a market outcome because several structural constraints shape schooling choices: admission procedures, physical distance, early tracking policies, within-school ability tracking, etc. (e.g. de Bartolome 1992).

Recent theoretical and empirical contributions underlie the role of early tracking policies in affecting schooling decisions of individuals from different backgrounds (Epple and Romano 2002, Dustnmann 2004, Brunello et al. 2007). Because parental background matters more at the beginning of the student life, an early student streaming increases the probability that students from worse backgrounds end up in the vocational streaming, which offers less promising learning perspectives in terms of teacher quality, resources invested and course content. As a result, students from disadvantaged socio-economic backgrounds that decide to be enrolled in a gymnasium are likely to be more motivated and/or particularly able. Finally, educational systems with an early tracking age often puts vocational and specific training at the centre of their development strategy (Hall and Soskice 2001, Krueger and Kumar 2004), hence vocational schools might attract also students with background above the average. With these premises in mind, one would expect that heterogeneity in unobservable student characteristics within the school end up being substantially lower in systems tracking earlier with respect to comprehensive ones. Thus, the effect of peers' heterogeneity on student outcomes should vary in systems with different tracking policies (Brunello et al. 2007) and empirical assessments are required to quantify this difference. To the best of our knowledge, existing empirical works do not assess whether the impact of school heterogeneity on student attainments varies in schooling system with different tracking policies. This represents a main contribution of the present work.

Not only student outcomes, but also the distribution of educational opportunities depends on school heterogeneity and early tracking policies. Theoretical works predict that highly segregated schools and an earlier streaming age both widen educational opportunities (e.g. Brunello et al. 2007). The latter effect is well documented in the empirical literature (e.g. Ammermueller 2005, Hanushek and Woessmann 2006, Schuetz et el. 2008), although more recent works using difference-in-difference estimation at the individual level, or more reliable measures of tracking, seem to discard the hypothesis that an earlier tracking age increases the inequality of opportunities (Waldinger 2006, Brunello and Checchi 2007). The former effect is less analysed using a direct measure of school heterogeneity as we do here. However, a positive and significant peer effect mechanically leads to levelling opportunities. Still, it is not clear whether the levelling of educational outcomes is stronger for low ability students, as it appeared in earlier works (Zimmer and Toma 2000; Vandenberghe 2002; Hanushek et al. 2003; Rangvid 2007; Schneeweiss and Winter-Ebmer 2007), or from high to average ability students, as more recent researches for the UK have demonstrated (Gibbons and Telhaj 2008; Lavy et al. 2009).

The paper is related to the literature on peer effects using large international assessment surveys. The more rigorous attempt to identify peer effects in this literature is the paper Ammermueller and Pischke (2006). Similarly to Hoxby (2000), Hanushek et al. (2003), McEvan (2003), they use within-school variations in the class composition to solve the identification problem associated to a non-random student assignment. Under the assumption that the within-school allocation of student and resources is random, all the distortions due to non-random assignment are associated to variations between-schools. Hence, class variations in the peer composition within the school enable to disentangle the pure peer effect from an endogenous school selectivity bias. Using the PIRLS dataset, they found modestly large peer effect even when controlling for measurement errors. Moreover, accounting for selection bias only slightly reduces the peer effect obtained in a standard OLS specification.

Unfortunately, a similar identification strategy is not available in the PISA survey that does not provide detailed classroom information. Conversely, for the scope of this paper, the main advantage of the PISA dataset is that it allows to uncover cross-country variations in tracking policies and offers information on several policies affecting the school composition, i.e. admission procedures, ability grouping.

In the few papers attempting to assess peer effects using PISA, the identification strategy has been based upon the claim that the omitted variable bias is the most important source of selectivity problems. Therefore, the selectivity problem is reduced by having a large set of controls, both at school- and at individual-level, which are likely to affect the assignment of students to schools (Rangvid 2007; Schneeweiss and Winter-Ebmer 2007). For instance, Rangvid (2007) measures peer quality with the average education of the mother and uses variables of parental care, encouragement and time spend with their children to reduce the omitted variable bias. By using quintile regressions techniques, she conditions the effect of peers to the ability distribution and found stronger peer effects for low ability students in Denmark. In turn, a higher heterogeneity has an insignificant impact on student achievement along the entire distribution of test scores. Schneeweiss and Winter-Ebmer (2007) found a similar differential impact of peer quality along the ability distribution for Austria, whereas background heterogeneity appears to have a slightly negative and significant effect on outcomes. In a paper with a logic similar to our but with a different focus, Entrof and Lauer (2006) attempts to distinguish the peer effect of immigrants and native in different tracking systems and found stronger peer effects in countries with an earlier tracking. Another possibility, followed by Fertig (2003), lies in instrumenting school heterogeneity with proxies of the caring behaviour of parents at home and of admission procedures. For the U.S., he found a strongly negative effect of heterogeneity, measured with the coefficient of variation in the achievement of schoolmates, on performance in reading. However, both the measure of schooling heterogeneity—the coefficient of variation—and the instruments chosen appear rather weak. In particular, the coefficient of variation is such that a lower mean in the test leads to an increase in the coefficient of variation. As a result, since a lower mean of the schoolmates negatively affects the final outcome, this impact appears the mechanical consequence of a higher heterogeneity rather than the one of a lower mean[6]. An opposite, negative and significant effect of class heterogeneity, measured with the standard deviation of a composite index of family background, on student attainments in science and math is found by Vandenberghe (2002) in a cross-country study using the TIMMS dataset. However, he introduces several non-linear terms in the class composition variables that makes the effect of heterogeneity difficult to isolate.

Using the international student assessment PISA 2006, the purpose of this paper is to estimate the impact of school-composition variables, in particular of school heterogeneity, on efficiency and

[6]Moreover, the instruments used become not anymore valid if one includes other contextual variables such as the share of parents working in the peer group. More in general, it seems difficult to find convincing instruments in cross-sectional regressions without having crucial information on the characteristics of the neighbourhood of residence.

equity, and how this impact varies in systems with different tracking policies; hence we are implicitly testing the validity of a linear and of a 'pooled' specification of the peer influence. In order to minimize biases in the estimates of school composition variables, the core of our empirical strategy is to compare school- and student-level regressions adding several controls that are correlated with the sorting of individuals to schools of different quality, i.e. admission procedures, dummies for school competition, ability grouping.

The main advantage of school-level estimates is that they allow to attenuate the bias associated to unobservable individual characteristics; at least under the plausible assumption that the mean of these unobservable characteristics – i.e. the average individual selected by the school – are fully captured by national policies (e.g. age of tracking), compositional variables (e.g. share of immigrants) and certain schooling characteristics (e.g. school type or admission procedure). This advantage should be balanced against the cost that in school-level regressions linear peer effects are indistinguishable from the average background effect. However, this effect of heterogeneity should be correctly identified if once moving from school to individual estimates, the sign, the size and the significance of the coefficient of school heterogeneity remain substantially unchanged.

In student-level regressions, we perform several robust checks with the purpose of improving the reliability of the estimated impact of heterogeneity and, at the same time, to adopt updated empirical strategies to reduce the bias in the estimation of the linear peer effect. Concerning the latter issue, as in Ammermueller and Pischke (2006), we use school level dummies in order to attenuate the selectivity bias. Recall that this strategy is valid under the assumption that the correlation between school and individual unobservable characteristics is mainly dependent on school characteristics. According to Ammermueller and Pischke (2006), this necessary condition for identification is less likely to be satisfied in secondary schools where within-school sorting by ability matters, especially in certain countries. Here, however, this argument does not hold since, due to data limitations, we can not identify classroom peer effect. Also, controlling for variables of within-school tracking policy enables us to mitigate this 'confounding effect'.

To be more precise, consider the following specification of the schooling production function that is basically the one proposed by Fuchs and Woessmann (2007):

$$A_{isc} = \alpha + \beta X_i + \chi X_s + \delta X_c + \gamma BACK_i + \mu \overline{BACK}_s + \sigma Var_s(BACK_i) + u_c + u_i + u_{is} + u_{isc} \qquad \text{(eq.1)},$$

where, for sake of space, we do not include the imputation dummies for missing variable (see section 5). The student achievement A in school s and country c is the resultant of a vector of individual, school, country controls plus individual background and school compositional factors. The error is decomposed here in a country effect u_c, an individual effect u_i, a school effect u_s and a correlated school-individual effect u_{is} plus the standard independent error term u_{isc}. The school-individual interaction and the individual effect are the ones that are likely to be correlated with both the school composition variables and to the student achievement. This is because individuals are selected by schools upon certain unobservable variables and procedures. Averaging away by school individual effects, eq.1 becomes:

$$\overline{A}_{sc} = \alpha + \beta \overline{X}_s + \chi X_s + \delta X_c + (\gamma + \mu)\overline{BACK}_s + \sigma Var_s(BACK) + u_c + \overline{u}_s + u_{isc} \qquad \text{(eq.2)}$$

Under the plausible assumption that the average unobservable individual characteristics boils down onto the school composition variables and school characteristics, the impact of heterogeneity is correctly identified using ‘school-level clustering-robust’ linear regressions. At the individual level, instead, the way of reducing the selectivity problem rests on the assumption that individual and correlated school-individual effects are fully captured by observable and unobservable schooling characteristics. Unobservable schooling characteristics are proxied including both school composition variables (e.g. the share of immigrants and of females), and a dummy equal 1 for the quintile of the country-specific distribution of the average parental background at which the school belongs to. Hence we estimate the following function:

$$A_{isc} = \alpha_{sc} + \beta X_i + \chi X_s + \delta X_c + \gamma BACK_i + \upsilon \overline{X}_i + \mu \overline{BACK}_s + \sigma Var_s(BACK_i) + u_c + u_s + u_{isc} \qquad \text{(eq.1'),}$$

where $\upsilon \overline{X}_i$ is the school-mates composition (net of individual) and α_{sc} is the school quintile fixed effect. Equation 1’ leads to unbiased estimates of both the linear and the heterogeneous peer effect if the correlated and the idiosyncratic individual term are fully absorbed in the new covariates. Throughout the paper, the fact that equation 1’ is often estimated separated by the type of tracking policy (see below) and always using standard errors clustered by school further reduces the endogeneity bias especially in countries that track students earlier—where the within-school variation in the unobservable individual characteristics is expected to be lower.

Concerning the estimation of the effect of heterogeneity and tracking on equity, we follow the specification of Schuetz et al. (2008) and Brunello and Checchi (2007) where a full set of interaction dummies between a measure of background and several factors that might affect the size of the socio-economic gradient are included. Among these factors, we add heterogeneity in background in a reduced-form model where school characteristics are excluded since, differently from estimation of the standard production function, the impact of student background on performance should be depurated by any effect that might act through families’ differential access to schools of different quality (Schuetz et al. 2008). In section 6 we estimate the following relationship:

$$A_{isc} = \alpha + \beta X_i + \gamma BACK_i + \eta (X_c * BACK_i) + ...$$
$$+ \vartheta (T_c * BACK_i) + \varphi (BACK_i * Var_s(BACK_i)) + \lambda (BACK_i * \overline{BACK_s})) + u_{isc} \qquad \text{(eq.4)}$$

The first interaction is between the factors – i.e. duration of pre-primary school, share of public schools, student/teacher ratio – that might disturb the relationship between background and or variables of interest—i.e. tracking and school compositional variables—which are captured by the other interaction terms. Next section briefly describes the PISA dataset and provides a descriptive glance of the different impact of heterogeneity in countries with different tracking policies.

3. Descriptive Statistics and Preliminary Analysis.

In the empirical analysis, we use the 2006 PISA survey that so far has not been used yet to assess the impact of school compositional variables on student outcomes. PISA's target population is 15-year-old students in each country, regardless of the grade they currently attend. Differently from other internationally comparable surveys such as PIRLS and TIMMS programs, the PISA dataset presents the additional desirable feature of being more oriented on problem solving capacities (know-how) rather than on curricula skill (know-what). The importance of problem solving and cognitive skills is not only recognized by the micro-econometric literature on the determinants of earning (e.g. Murnarne et al. 1995), but also at the macro level the observed strong correlation between the average PISA score and the growth rate guarantees the validity of our dependent variable (Hanushek and Woessmann 2007).

PISA dataset contains detailed information on student's home background, school resources and a wide range of institutional variables capturing the degree of school autonomy, accountability practices and variables affecting the student choice (Woessmann et al. 2010). Individual controls such as sex, age, grade, etc. are also available, whereas policy variables at the national level are usually integrated by other dataset (Oecd, UNESCO, etc.). Since PISA 2006 is focussed on science, we consider only the outcome in science as the dependent variable.

Some variables used in the econometric analysis of next sections are indexes built by PISA experts in order to summarize various school or individual characteristics related to each other. For instance, the degree of autonomy in managing resources at the school level is captured either by a vector of dummies (autonomy in within-school allocation, in hiring and firing teacher, etc.) that are highly dependent to each other or by a synthetic indicator built on these dummies (see tab. A1 for details). The same holds for indexes of school resources and background[7]. Of particular interest for our work is the variable of background built by the Oecd—called the Economic Social Cultural Status 'escs'—that mixes the information provided by the widely used measures of parental background: highest parental years of education, the highest occupational level quantified with the index of occupational status (Ganzeboom et al. 1992), the number of books at home and the resources available at home to study, i.e. homepos. The 'escs' variable is chosen here as our baseline measure of background since it encompasses in a synthetic way the various and multidimensional aspects shaping the impact of family characteristics on the student's attainment.

Table 1 shows by countries the descriptive statistics of the main variables on which we focus on to explain student performance in science: age and grade of first tracking, mean results in science and mean and standard deviation of family background indexes, etc., whereas a full description of the variables used is provided in table A1 in the appendix. Table 2 focuses on the 'escs' variable by showing the strong correlation of this composite index with each of its components, which follows by the construction. However, a much lower correlation with the variable books-at-home suggests to include such variable together with either 'escs' index (or its components) as individual background controls in the empirical specification. School compositional variables are built according to our choice of the background variable. In particular, the average level of the 'escs', net of the individual one, is our favourite measure of linear peer effect, whereas the standard deviation of the 'escs' account for school heterogeneity.

A key issue for our paper is to find a reliable measure of background heterogeneity at the school level. Similarly to Rangvid (2007), we measure it using the standard deviation of a quantitative measure of parental background. Moreover, in order to partially account for cross-country differences in the allocation process of students of different background to schools of different quality, we normalize the standard deviation at the school level with the one at the country level[8]. In fact, a high heterogeneity at the school level can be due to a high heterogeneity in the country rather than to a random sorting of students to schools, hence the desired level of heterogeneity at the school level is bounded by the overall background heterogeneity at the country level.

Simple scatter plots adjusted for school weights highlight a pronounced non-linearity in the relationship between school heterogeneity and performance resulting as the balance of the positive and negative externalities triggered by the interaction of individuals from different backgrounds

[7] See OECD 2009 and the PISA 2006 Technical Report for a detailed explanation about how these indexes have been computed

[8] However, all results are robust to the inclusion of the 'no-normalized' background standard deviation at the school level. Results using these further measures of heterogeneity are available upon request by the authors.

(fig. 1). As it appears clear from figure 2 and tab. 3, this relationship is largely driven by countries tracking students earlier. In turn, in comprehensive systems, higher heterogeneity negatively influences outcomes along the entire school distribution (fig. 3). Since background variables are highly correlated with test outcomes, figure 2 suggests that this pattern is somehow driven by the one between the mean and the variance of the 'escs' at the school level. Between the two school systems, a closer inspection of tab.4 (lines 4-8) shows that differences in the school composition by background are not as large as one would expect. Looking at tab.4, what substantially differs between the two groups of countries is the quota of persons doing vocational programs (significantly higher in the early tracking system), the sorting within school by ability (significantly higher in comprehensive systems) and the admission procedures (relatively more based on student records and residence in comprehensive systems). Besides, the two systems seem to have a different degree of school dispersion in terms of unobservable individual characteristics: the variance in the average attainment at the school level is much larger in the early tracking with respect to the comprehensive system (tab. 4, line 1-2). This reinforces our claim that differences in tracking age mainly translate into differences in sorting by unobservable.

However, also within the two systems, a large cross-country variation still subsists. In the early-tracking system, Germany, Switzerland and Austria display a strong inversed U-shaped relationship between scores and heterogeneity, whereas in the Netherlands and in Belgium a higher heterogeneity is associated to a lower performance at the school level. In the comprehensive system, the relationship is much steeper and negative in Anglo-Saxon countries with respect to Scandinavian ones (fig. 4). The latter differences can be attributed to the effect of segregation in elite, high quality, schools in Anglo-Saxon systems as suggested by the strong negative pattern between average and variance in background (fig. 5).

By and large, the descriptive analysis presented here confirms that different patterns emerge between countries with or without an early student tracking. This evidence motivates the inclusion of a non-linear and asymmetric specification of the effect of heterogeneity on outcomes. In the pooled specification, this is obtained with an interaction dummy between heterogeneity and early tracking. In the separated regressions, the square of the standard deviation is also included to account for non-linear effects of heterogeneity. Next sections present the results.

4. School-level regressions

The sample of countries used in this paper consists in OECD ones but France where school variables have not been recorded in the PISA 2006 survey. As in Woessmann et al. (2010), Mexico and Turkey are excluded because they have an average 'escs' that is a full standard deviation below the OECD average. Also following many studies using PISA surveys, we excluded from the sample those very few students enrolled in grades lower than 8 or higher than 11. Finally, as we intend to analyze the effect of social interactions at school, we restricted the sample to students attending schools for which PISA 2006 provided data for at least 15 students, i.e. we dropped schools with less than 15 interviewed students[9]. Our final sample includes 202.817 students clustered in 6.728 schools.

As discussed in section 2, the first stage of our analysis focuses on regressions at the schooling level in order to reduce unobservable student variability. Several control variables identified at schooling level are included. The first type of controls are compositional variables that proxy certain basic features of the demographic, social and cultural environment at school: the mean students' age and the share of females, of immigrants, of students speaking a foreign language at home and of students enrolled in a vocational programme (see table A1). In turn, as stated before, the average student index 'escs' and its standard deviation at the school-level (normalized by the country standard deviation) are our measures of linear peer and of school-mates heterogeneity respectively.

As further control variables we add a set of variables concerning school resources and institutions, class size, school location and country level controls (see table A1). Among country controls, we included institutional variables that are provided to be important determinants of student attainment (Fuchs and Woessmann 2007); in particular, the share of students subjected to external evaluation and/or standard test in science[10], the age of tracking between different kinds of programmes (general or vocational, OECD educational dataset) and the quota of pupils attending pre-primary education (UNESCO educational dataset).

Among school-level institutional variables, we include quantitative PISA indexes about school responsibility for allocating resources and for curriculum and assessment[11], school-type dummies (built interacting the public or private school management and main source of financing) and two dummies for the admission procedures followed by the school (i.e. signalling if residence or students' ability are a high priority or a prerequisite for being enrolled in that school). The latter dummies seem particularly well suited in order to reduce the selectivity bias due to a non-random assignment of students to schools.

[9] The same sample restriction for similar purposes has been applied in Rangvid (2007).
[10] This information on external exit exams and general accountability practices across countries has been collected first by John Bishop and then refined by Woessmann and its associated (see Woessmann et al. 2010)
[11] Among controls about school institutions, in all regressions shown in this paper, we included the 'respres' and 'respcurr' indexes (see table A1 and OECD 2009) instead of the dummies about the single components of school autonomy and responsibility about resources and curricula, due to the several missing values characterizing each dummy. Replacing these dummies with the two OECD indexes, which by construction have much less missing values, does not alter at all regressions' results.

In table 5, we show the results of school-level regressions on science performance for the pooled sample of countries. For sake of space, in what follows we present estimated coefficients of variables of interest, i.e. average and standard dev. of 'escs'. Results available upon request show that, in both regressions at school- and at student-level (see §5), other variables display the expected signs and significance consistently with the empirical literature on students' performances using international assessment programmes (see Fuchs and Woessmann 2007; Hanushek and Woessmann 2010), in particular school resources seem to exert a much lower influence than school and country institutional features.

Model SC-1 in table 5 highlights the large positive effect played by the average parental background; in fact, a change in one standard deviation of the 'escs' index turns out to explain 41 out of the 100 points of the standard deviation in the student attainments. This is not surprisingly as long as, in school-level regressions, the average 'escs' identifies both the peer and the individual parental background effect, which has been found to be the larger explanatory factor of student outcomes (e.g. Hanushek and Woessmann 2010). Unlike linear peer effects, the impact of heterogeneity is correctly identified in school-level regressions under the plausible assumption that the average unobservable individual characteristics boils down onto the school composition variables and school characteristics. Background heterogeneity exerts a negative and significant impact on the average performance, even if the magnitude of this impact is rather small: a one standard deviation increase in the degree of heterogeneity leads to a 1.8 point decrease in the average science score. This result in favour of school segregation appears nuanced when we allow for non-linear effects of heterogeneity. The inclusion of the 'escs' variance, so as suggested by the preliminary analysis in section 3, makes the relationship between heterogeneity and performance inversely U-shaped, being now positive and significant the linear term while negative and significant is the coefficient of the quadratic term (see model SC-3 tab. 5).

In models so far discussed we included, as controls of the link between school composition and performances, variables recording resources and institutional aspects at the school-level. However, educational inputs can be related to student background; hence estimates of background variables can be plagued by endogeneity since pupils from better families attain schools with more resources and better institutions. Since this source of endogeneity stems from a more or less distributed allocation of resources and institutions within the country, aggregating school-level variables of resources and institutions at the country-level allows circumventing these endogeneity problems, then providing unbiased estimates (see Woessmann 2003). Accordingly, the robustness of model SC-3 can be checked replacing school resources and institutional variables with their country average[12], which largely confirms previous result (see SC-4, tab. 5). Interestingly, with respect to model SC-3 the estimated joint impact of background and peer increases by only 1.9 points of a full standard deviation in the PISA score suggesting that the distortion induced by this source of endogeneity is negligible.

Coherently with the focus of the paper and with the preliminary analysis of section 3, the next step is to consider the joint influence of tracking and school heterogeneity on achievements. Recall that tracking can occur within-school or between different types of schools. The former is based on ability grouping and prevails in Anglo-Saxon countries; the latter implies the streaming into completely different segments of the education process, generally offering general or vocational programmes such as in Germany and in many central European countries (Brunello and Checchi, 2007). Here we focus on the schooling tracking to split[13] countries according to the age when students have to choose between programs[14].

A first way to differentiate the effect of heterogeneity by tracking systems is to introduce an interaction term between school heterogeneity and a dummy classifying OECD countries into early or late tracking ones (model SC-5, table 5). In this case, the negative size and the significance of the 'escs' standard deviation increases, but at the same time the interaction term is also positive and significant showing a large positive effect of school heterogeneity in countries where the choice among different tracks occurs before the age of 13.

As a next step in order to better assess differences between the early-tracking and the comprehensive system, we run school-level regressions separated by the two groups of countries (table 6). Replicating model SC-2 enables to better disentangle the large difference in the impact of school heterogeneity between early tracking and comprehensive systems. On the one hand, in countries tracking students after the age of 13, heterogeneity exerts a negative influence on student outcomes. On the other hand, the sign reverts in early tracking countries, but the positive effect is significant only at the cut-off level of 85%. Note that the opposite influence of school heterogeneity according to tracking systems is confirmed even when we split countries following the method proposed by Waldinger (2006), as shown in table 6 by model SC-2A. Finally, the size of the impact of heterogeneity on student outcomes increases when separated regressions are carried on with the

[12] Country average have been computed using PISA data since Woessmann et al. (2010) show the robustness of considering PISA means instead of data provided by other data sources. In model SC-4 also the share of students enrolled in vocation courses is considered as a country average.

[13] Literature provides several measures of tracking systems: Hanushek and Woessmann (2006) uses the age of the first tracking choice, Ammermueller (2005) the number of tracks experienced by the student before enrolling in upper secondary education, Waldinger (2006) the minimum school grade where a significant share of students is allocated in different tracks. In model SC-5, in line with the Hanushek and Woessmann (2006), we consider as early-trackers countries where students have to choose before they are 13 years old.

[14] To account for within-school tracking, in model SC-7 in table 6, we willalso control for information on ability grouping within the school.

impact of one std. dev. increase ranging from +2.0 (resp. -4.1) to +2.4 (resp. -4.3) std. dev. increase in the early tracking (resp. comprehensive) system.

When including also non-linear effect of school heterogeneity, differences between the two groups widen. In comprehensive systems both linear and quadratic terms become not significant, whereas in early-tracking ones both terms appear highly significant, showing an inverted U-shaped relationship between background heterogeneity and average performances (model SC-3, tab. 6). Moreover, this relationship remains robust either to the inclusion of country fixed effects (model SC-6, tab. 6) or – although at a much lower significance level – when country averages instead of school level resources and institutional variables are considered (model SC-4, tab. 6)[15]. It is worth to notice that, using the coefficients estimated in table 2, the optimal degree of heterogeneity that maximizes attainments in early-tracking systems is located near to the median level of the 'escs' standard deviation.

Finally, in order to account for across countries differences in ability tracking within the school (a widely used policy particularly in Anglo-Saxon countries), we run another model (SC-7, tab.6) with additional school-level dummies capturing the procedure followed within the school for grouping students by ability and also, following Woessmann et al. (2010), accountability practices internal to the school (see tab. A1). The inclusion of these additional controls, which in principle should distort the impact of heterogeneity, does not change the results rather the positive influence of school heterogeneity in early tracking systems becomes stronger and more significant.

As stated in previous sections, the main advantage of school-level regressions presented so far is that they allow to attenuate the bias associated to unobservable individual characteristics. This advantage should be balanced against the cost that in school-level regressions linear peer effects are indistinguishable from the background effect. With the aim of identifying also this effect, we now move to student-level estimations.

5. Student-level regressions

Pooled student-level regressions lead to a substantial increase in the number of observations and hence allow controlling for several additional factors. First of all, when running regressions using students as the unity of observations individual characteristics (age, sex, grade etc…) are included (see table A1). Second, the multifaceted and complex mechanisms that drive the transmission of parental characteristics to children can be considered by unpacking the individual background effect

[15] These differences between early and late tracking countries emerge also when proxies of school heterogeneity based on different parental background variables are computed (e.g. highest parental occupational status and educational attainment). Detailed results are available upon request by the authors.

in the several components of the 'escs' index: the highest parental education (in years) and occupational status, the OECD variable summarising in a quantitative index the family "home possessions" (OECD 2009), dummies on the 'number of books at home'. Thirdly, compared to school-level regressions, 'peer composition' variables are net of the individual ones and consist in six students' characteristics: sex, age, immigrant and 'foreign language' status, type of school programme (general or vocational) and the 'escs' index. Finally, in an extended model, we also include additional controls proxying the effort devoted in studying science (see table A1).

Student-level regressions might lead to biased estimates in so far as missing values on certain individual characteristics are not randomly distributed, but turn out to be related to background and ability. As a result, dropping students with missing information for some variables could engender a sample selection bias. In order to copy with this issue, we impute individual missing values regarding family background (escs, pared, hisei and homepos variables, see tab. A1) and some individual characteristics (immigrant and foreign language) according to the usual methodology followed in the literature (Woessmann 2004). Thereafter, we regress each variable subjected to the imputation procedure with some basic controls available for nearly all students (age, sex, grade, dummies 'vocational' and 'isced 3', two country-level controls – GDP and expenditure on education per capita – and the number of books at home) and replace missing values with predicted ones. Once having replaced missing values with imputed ones, in all student-level regressions carried on we correct for the measurement error that could arise in the imputation procedure by allowing the observations with missing data on each variable to have their own intercepts and slopes (Woessmann 2004)[16]. As an additional methodological caveat, the 'school-level clustering-robust' linear regression method is always used in student-level regressions to estimate standard errors that recognize the schools as the basic unit of sampling in the survey (Woessmann 2004).

Table 7 shows OLS estimations for the pooled sample of OECD countries[17]. With respect to school-level estimates, the impact of heterogeneity is also negative but at a significant level around the cut-off level of 15% (ST-1, tab.7), whereas it is not significant at all when non-linear effects are included (ST-2 and ST-3, tab.7). The size of the heterogeneity effect only slightly decreases from around 1.9 to around 1.3 points of a full standard deviation in the test scores. In turn, linear peer effects are significant and very large with a change in one standard deviation of the 'escs' accounting for more than a 20% change in the standard deviation of science test (ST-1, tab.7).

[16] In particolar, we include a dummy that takes the value 1 for an imputed data and 0 for observations with original data and an interaction term between this imputation dummy and the respective variable subjected to the imputation procedure

[17] To obtain representative coefficient estimates from the stratified survey data – as in section 4, where regressions were run using schools' sample weights provided in the PISA dataset – in all estimations of sections 5-6 students' sample weights are used.

Whereas the first result is somehow in line with the one of the previous literature finding small (but insignificant!) effects of heterogeneity on student performance (e.g. Hanushek et al. 2003; Rangvid 2007), the estimated impact of the linear peer effect is larger than the bulk ones founded in the literature (see Ammermueller and Pischke 2006). However, when we adopt a more precise identification strategy to isolate the linear peer effect (see §2 and ST-0, tab.7; i.e. including school fixed effects and school- and student-level additional controls and excluding heterogeneity terms), the estimated effect decreases to 19%, closer to the impact found by other studies using PISA surveys (e.g. Rangvid 2007; Schneeweis and Winter-Ebmer 2007).

Note that the R^2 reduces compared to the very high level shown in school-level regressions (over then 60%). This is expected since a large part of the performance variation across students has to be attributed to unobserved variables (e.g. their innate ability or learning motivation). However, its level, around 34%, is in line with the one of the two studies using a large set of controls to reduce the omitted variable bias in the estimation of peer effects (Rangvid 2007; Schneeweis and Winter-Ebmer 2007).

As in school-level analysis, the picture substantially changes when the interaction between the heterogeneity and the tracking system is added (model ST-4, tab. 7). Again, this interaction is positive and significant suggesting that in early-tracking countries heterogeneity can foster students' performances, even once controlling for other school composition aspects. School-level results are also confirmed in separate regressions with a higher heterogeneity being significant with opposite signs in systems with early-tracking (+) and comprehensive (-) schools (ST-1, tab.8). Looking at table 7, results remain robust to different classification of the countries by tracking (ST-1A) and to the inclusion of country fixed effects (ST-6). Moreover, the difference between the two tracking systems is further more evident when the quadratic heterogeneity term is also included (model ST-5): with respect to school regressions both the inverted U-shaped relationship – again increasing up to median level of the 'escs' standard deviation – for early-tracking countries and the insignificance of the polynomial function for comprehensive ones are confirmed at student-level. It has to be emphasized that moving from school- to student-level regressions estimated signs and sizes of the heterogeneity terms remains the same, hence the impact of heterogeneity should be correctly identified. Finally, consistently with the theoretical literature (e.g. Brunello et al. 2007) and with Entrof and Lauer (2006) – but with a focus on the effect of immigrant peers – separate regressions display a larger (linear) peer effect in early tracking systems (tab.8).

Interestingly, in early-tracking countries the relationship between heterogeneity and performances is not driven by the share of students enrolled in vocational programmes (ST-5, tab.8). In contrast, in comprehensive school systems, the interaction term between heterogeneity and the country-level share of students enrolled in vocational programs displays a negative and significant coefficient suggesting that the negative impact of heterogeneity on student performance is largely concentrated in schools offering vocational programs (model ST-5, tab. 8). All in all, this finding has a strong policy implication in so far as, also in the comprehensive system, the impact of higher background heterogeneity appears to be negative only in a minority of schools oriented towards training[18].

Adding further school and student controls (i.e. admission procedures, school accountability and proxies of individual efforts, see table A1) corrects for the omitted variable bias in the estimation of school composition variables. In this case, the significance of the two opposite effects of the 'escs standard deviation' slightly decreases but still emerges, whereas, as expected, the impact of the linear peer effect is mitigated (model ST-7, tab. 9).

However, correcting for the omitted variable bias might not be sufficient to attenuate the selectivity bias in the estimation of peer effects if unobservable schooling characteristics are still present (see section 3). In order to attempt a better identification of the linear peer effect, following Ammermueller and Pischke (2006)[19] and according to the empirical strategy described in section 2, we add school-level fixed effects, identified, for each country, by the quintile of the average parental 'escs' distribution to which the school belongs to (models ST-8 – ST-9, tab. 9). The linear peer effect reduces in size but only in countries tracking earlier, whereas its size remains unchanged in countries with comprehensive school. This implies that the identification strategy of linear peer effect suggested by Ammermueller and Pischke (2006) is particularly suitable for early-tracking systems where the early selection process might create more homogeneous but 'less observable' school types.

Concerning the impact of heterogeneity, in comprehensive systems a strong difference emerges comparing models ST-1, ST-7 and ST-8 (tables 8 and 9); indeed, when variables about students' time of work and school sorting are added, the significance of the negative heterogeneity effect strongly reduces and it disappears when school fixed effects are included too. In turn, in the most complete model (ST-9) where both types of additional controls and the quadratic heterogeneity term are included, the inverted U-shaped relationship between heterogeneity and student's competences in science is confirmed for early-tracking countries.

So far, using OLS techniques, we have focused on average peer and heterogeneity effects. This standard methodology may miss how school composition affects achievement differently at

[18] However, the share of students enrolled in vocational programs is zero in several countries considered, hence leading to measurement errors of this effect. Using the share of students enrolled in schools that mainly offer training, a higher share of students attending schools who offer training also leads to a significantly negative effect of heterogeneity in comprehensive systems.

[19] Also Schneeweiss and Winter-Ebmer (2007) include a school fixed effect in their analysis of peer effects in Austria.

different points of the conditional test score distribution and hence might lead to misleading policy implications. For instance, while the school heterogeneity may not be significant for average test scores, it is useful to know whether this effect is not significant in all quintiles of the conditional test score distribution, or whether it masks significant effects at some points of the distribution or even effects of opposite sign (Rangvid 2007).

In order to answer this question, we use quintile regressions to estimate model ST-1 separated for early and late tracking countries. Quintile regressions confirm that the average peer effect is higher in early tracking countries all along the conditional test score distribution. Moreover, the peer effect is largely positive everywhere and, consistently with previous studies limited to Austria (Schneeweiss and Winter-Ebmer 2007) and Denmark (Rangvid 2007), it is slightly larger in lower deciles. As expected, main differences between the two groups of countries emerges with respect to the influence of school heterogeneity. In all deciles, the 'escs' standard deviation is always statistical significant at the 99% level. However, its sign is largely positive and slightly U-shaped along the entire test score distribution in early tracking systems, while it remains always negative in comprehensive systems where the size of the negative effect is only slightly lower in upper deciles.

In sum, quintile regressions reinforce the previous finding in terms of a small efficiency-enhancing effect of mixing students in early tracking systems, where individuals are probably more homogeneous in their unobservable features. Conversely, the picture in comprehensive systems is nuanced: on the one hand, stronger peer effects at the bottom of the ability distribution would lead to support policies aimed at increasing background heterogeneity[20]; on the other hand, a too high heterogeneity turns out to offset the efficiency-enhancing effect of mixing background. For policy considerations, the effect of school composition variables on efficiency should be seen together with the one on equity; this is the objective of next section.

6. School heterogeneity and equality of opportunity

So far, we have analyzed the "efficiency" effect of school composition and heterogeneity. Our focus now moves to the equity effect; in particular, we want to analyse the extent to which the theoretical prediction that a heterogeneous school environment tends to level opportunities of pupils from different backgrounds (e.g. Benabou 1996) is empirically warranted. A way to answer this question empirically consists in assessing whether the family background effect, i.e. i.e. the link

[20] However, a caveat is required here. The effect of regrouping students by background should be balanced against the associated regrouping of students by ability. It might be that the regrouping would bring about peer effects due to interactions of individuals of different abilities that offset or amplify the ones due to the interactions of individuals of different background. Since we can not disentangle ability peer effect from background ones, policy implications are less clear cut.

between individual performances and family background, is linked to peers' average and heterogeneity. The existing literature (Woessmann 2007; Schuetz et al. 2008; Brunello and Checchi 2007) suggests using the coefficient of a synthetic variable of parental background as a proper measure of educational inequality of opportunities in reduced form regressions of the determinants of student performance[21].

Following this literature, in this section we run a reduced form regressions where only individual characteristics are included among the control variables (see section 2), whereas the family background is summarized in a single variable – the student's parental escs index[22]. In particular, in order to analyse the differential impact of school compositional variables and tracking on family background, we interact the individual family background effect with the early-tracking dummy, the average and the standard deviation of school parental escs respectively. Also consistently with the existing literature, we interact the individual escs with possible confounding factors in order to isolate the pure effect of heterogeneity and tracking on background. These confounding factors are four country-level features: the duration of pre-primary school, the share of public schools, the average students/teachers ratio and the per capita spending on education[23]. Besides, following Schuetz et al. (2008), we run two different sets of regressions, respectively including or excluding country fixed effects.

Without including country fixed effects (table 11), the usual result that an earlier tracking widen the opportunity gap between student from different background is strongly confirmed even if all the caveat due to the incorrect identification of the true effect of tracking in cross-sections should be kept in mind here (Hanushek and Woessmann 2006; Ammermueller 2005; Waldinger 2006). More to the point, the 'escs' coefficient is twice as large in early tracking with respect to comprehensive countries in separated regressions, whereas the interaction term between tracking and 'escs' is large, positive and significant in pooled ones. However, it is worth to emphasize that such significance disappears when interactions between 'escs' and school composition are also added. This finding adds new insights to the growing literature on tracking and equality of opportunities (see Brunello and Checchi 2007) since the effect of tracking appears as spurious and largely driven by school compositional variables.

Looking to the effect of school compositional variables per se, in all the empirical specifications considered (tab. 11) a higher peer average significantly increases the impact of background, while

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[21] Interestingly, in two recent studies, Woessmann (2004 and 2007) found that there is no trade off between equity in educational outcomes and efficiency.

[22] Results presented in tables 6 and 7 are robust to the use of different background variables instead or the escs (e.g. parental highest occupational status or educational attainment). Detailed results are available upon request by authors.

[23] The interaction with the share of students enrolled in pre-primary school (a further potential confounding factor highlighted by Schuetz et al. 2008 and Brunello and Checchi 2007) has not been included, since we did not found reliable data for Korea and Ireland.

the opposite happens regarding the impact of school-mates heterogeneity: i.e. a higher heterogeneity offsets the impact of family background. Interestingly, this reduction is higher in early tracking countries where the impact of individual 'escs' is much higher.

When country fixes effects are included (table 12), school compositional variables keep the same sign and high significance, but the size of the escs*(escs standard deviation) interaction is now similar in the two tracking systems. More puzzling is the inversion in the size of the family background effect that turns out to be higher in comprehensive schooling systems.

All in all, school compositional variables affect equity in the way expected by the theory (e.g. Benabou 1996). In turn, including these variables make the negative impact of early tracking on opportunity equalization less limpid suggesting that school compositional variables should be included in future, more detailed analyses. Finally, the effect of mixing student by background appears socially desirable both in terms of equity and efficiency in early tracking systems.

7. Concluding remarks

The main efforts of this paper have been devoted to study the impact of school heterogeneity in different tracking regimes. It has been shown that school heterogeneity does have an impact on both efficiency and equity. Whereas a higher heterogeneity leads to a substantial levelling of the educational opportunities, the impact of heterogeneity on efficiency is opposite in schooling systems with different school tracking policies. In early-tracking systems, school heterogeneity has a positive but non-linear impact on student outcomes. In the comprehensive ones, instead, heterogeneity negatively affects student outcomes but this result is largely driven by pupils attending vocationally-oriented programs. This result holds both in school- and in individual-level regression leading us to conclude that the effect of heterogeneity is correctly identified. In turn, the linear impact of peers is far larger in early tracking systems and seems correctly identified either by adding controls correlated with the school selection process or by using school-level fixed effects.

All these findings point, as a possible explanation, to a different way in which the tracking age affects the sorting of students by unobservable characteristics. For instance, in order to avoid the vocational streaming, better students might put more efforts to signal their higher abilities and motivations sooner in early tracking systems. If this is the case, the unobservable degree of heterogeneity should be lower in early tracking systems and, hence, policies attempting to enhance the opportunities of disadvantaged students should intervene before tracking occurs. Further empirical researches should investigate more carefully the effect of early tracking and school admittance policies on student sorting by both ability and background.

A final caveat is required to use these results for policy purposes. The significant impact of school heterogeneity on student performance is rather small both in comprehensive and in early-tracking systems, hence favouring student mobility and the mixing of background might have a cost well-above the benefits in terms of efficiency. Also, the large variation in the factors affecting the selection of student by schools of different quality, both within- and between-country, would require further analyses to obtain more limpid policy implications regarding the scope of policies aimed at mixing students of different backgrounds.

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Tab. 1: Descriptive statistics of PISA 2006 selected variables

		Age Track	Grade Track	Mean Score Science	Mean Parent. Edu. (Pared)	Mean ESCS	Pared Std. Dev. (mean by school)	ESCS Std. Dev. (mean by school)	Share Immigr.	Share Foreign. Lang.	Share of Public	Share of no ability track	Share of stud Vocat.	Share admitted by stud. record	Share admitted by residence
Early Tracking	Austria	10	4	518.4	13.8	0.23	2.05	0.69	0.12	0.10	0.87	0.60	0.44	0.69	0.22
	Czech Rep.	11	5	520.5	13.5	0.07	1.86	0.66	0.02	0.02	0.91	0.35	0.43	0.45	0.20
	Germany	10	4	523.1	14.2	0.32	2.86	0.78	0.17	0.13	0.89	0.59	0.00	0.40	0.65
	Hungary	11	4	514. 1	12.8	-0.01	2.13	0.71	0.02	0.01	0.81	0.31	0.62	0.70	0.01
	Slovak Rep.	11	4	493.7	13.3	-0.10	2.19	0.74	0.00	0.14	0.88	0.25	0.46	0.50	0.17
	Belgium	12	6	512.5	13.8	0.18	2.67	0.79	0.13	0.19	0.37	0.56	0.47	0.26	0.02
	Netherlands	12	6	525.8	13.7	0.25	2.61	0.78	0.11	0.07	0.32	0.19	0.30	0.66	0.10
	Luxembourg	13	6	486.3	13.1	0.10	3.91	0.96	0.37	0.91	0.85	0.27	0.13	0.41	0.42
	Switzerland	12	6	515.1	13.4	0.10	2.94	0.79	0.23	0.19	0.93	0.24	0.07	0.54	0.82
Late Tracking	Italy	14	8	479.0	12.5	-0.05	3.06	0.82	0.04	0.13	0.92	0.54	0.57	0.07	0.11
	Korea	14	9	522. 9	13.2	-0.01	2.30	0.70	0.00	0.00	0.55	0.12	0.24	0.60	0.22
	Greece	15	9	481.3	13.4	-0.09	2.92	0.80	0.05	0.03	0.95	0.89	0.15	0.05	0.71
	Ireland	15	6	508.6	12.9	-0.01	2.21	0.74	0.08	0.06	0.40	0.02	0.02	0.02	0.42
	Japan	15	9	532.0	14.0	-0.01	1.72	0.60	0.00	0.00	0.74	0.44	0.24	0.87	0.20
	Portugal	15	9	488.7	9.9	-0.52	4.19	1.01	0.05	0.02	0.89	0.48	0.14	0.06	0.56
	Australia	16	10	527.3	13.2	0.21	1.83	0.68	0.22	0.09	0.00	0.05	0.10	0.09	0.42
	Canada	16	8	536.3	14.7	0.37	2.28	0.71	0.23	0.15	0.85	0.08	0.00	0.11	0.78
	Denmark	16	9	495.0	14.0	0.30	2.46	0.82	0.08	0.07	0.63	0.16	0.00	0.03	0.55
	Finland	16	9	564.0	14.4	0.25	2.34	0.75	0.02	0.02	0.96	0.49	0.00	0.04	0.75
	Icelend	16	10	489.7	15.1	0.82	2.74	0.81	0.03	0.03	0.96	0.15	0.00	0.01	0.94
	Norway	16	10	485.6	13.8	0.43	1.73	0.68	0.08	0.08	0.96	0.58	0.00	0.00	0.78
	New Zealand	16	6	529.3	12.8	0.09	2.10	0.72	0.21	0.09	0.93	0.03	0.00	0.10	0.50
	Poland	16	9	497.7	12.2	-0.31	1.75	0.75	0.00	0.01	0.97	0.53	0.00	0.13	0.83
	Spain	16	10	488.9	11.1	-0.31	3.53	0.88	0.07	0.16	0.53	0.29	0.00	0.03	0.68
	Sweden	16	9	502.9	13.8	0.23	2.11	0.70	0.11	0.09	0.88	0.24	0.00	0.01	0.58
	UK	16	12	514.6	13.7	0.19	1.97	0.69	0.10	0.06	0.74	0.00	0.00	0.10	0.61
	US	16	12	491.0	13.6	0.15	1.96	0.75	0.17	0.12	0.87	0.12	0.00	0.08	0.81

Source: elaborations on PISA 2006 data

Tab. 2: Correlation matrix between Background Measures

	escs	pared	hisei	homepos	books at home
escs	1				
pared	0.769	1			
hisei	0.796	0.461	1		
homepos	0.708	0.321	0.339	1	
books at home[1]	0.499	0.323	0.327	0.524	1

[1] The variable books at home has been linearized. Source: elaborations on PISA 2006 data

Tab. 3: Correlation between escs mean and. escs std. dev.

	Escs standard deviation	Escs standard deviation related to country escs S.D.
Early tracking	-0.005	-0.024
Comprehensive	-0.222	-0.151
All countries	-0.171	-0.119

[1] The variable books at home has been linearized. Source: elaborations on PISA 2006 data

Tab. 4: Descriptive statistics of PISA 2006 selected school-level variables

	Early Tracking	Early2: grade track<6	Comprehensive
Mean SCIE	502.6	502.9	498.8
Std. Dev. SCIE	73.6	73.3	54.5
Std. Dev. Escs average	0.49	0.49	0.51
Std. Dev. Pared average	1.3	1.3	1.5
Average escs std. dev.	0.75	0.75	0.75
Average pared std. dev.	2.6	2.6	2.2
Share of students attending vocational	0.21 (0.39)	0.20 (0.39)	0.1 (0.30)
Share of immigrants	0.14 (0.19)	0.14 (0.19)	0.07 (0.14)
Share of students speaking foreign languages	0.13 (0.17)	0.13 (0.17)	0.07 (0.14)
Share of school no sorting students by ability	0.44 (0.50)	0.43 (0.49)	0.29 (0.45)
Share of schools that admit according to students records	0.43 (0.49)	0.42 (0.49)	0.19 (0.39)
Share of schools that admit according to residence	0.46 (0.50)	0.45 (0.50)	0.53 (0.50)

Source: elaborations on PISA 2006 data

Tab. 5: School average performances in science in OECD countries[1]. OLS regressions[2, 3].

	SC-1	SC-2	SC-3	SC-4	SC-5
Escs average	81.25	81.07	81.04	83.61	80.40
	(2.74)	(2.76)	(2.73)	(2.44)	(2.66)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Escs standard deviation		-13.38	102.39	106.39	-26.76
		(7.15)	(57.47)	(50.20)	(7.00)
		0.061	*0.075*	*0.034*	*0.000*
Escs standard deviation^2			-66.85	-68.25	
			(31.04)	(27.55)	
			0.031	*0.013*	
Early track* Escs standard deviation					39.05
					(15.07)
					0.010
Groups of Control Variables					
School Location and Class Size	yes	yes	yes	yes	yes
School Composition	yes	yes	yes	yes[4]	yes
School Resources	yes	yes	yes	country average	yes
School Institutions	yes	yes	yes	country average	yes
Country level controls	yes	yes	yes	yes	yes
Number of observations	5,831	5,831	5,831	6,482	5,831
F	87.6	85.7	84.0	85.0	83.7
Prob.>F	0.000	0.000	0.000	0.000	0.000
R^2	0.6235	0.6245	0.6259	0.6195	0.6297

[1] Mexico, Turkey and France are not included. [2] Regressions are run using school sample weights provided in PISA database. [3] For each variable, the first row refers to the estimated coefficient, the second to the robust standard error and the third to the P value. [4]The share of students enrolled in vocational programme is considered as country average. Source: elaborations on PISA 2006 data

Tab. 6: School average performances in science in OECD countries[1] by early and no early tracking countries[2]. OLS regressions[3, 4].

	SC-2		SC-2A[5]		SC-3		SC-4		SC-6		SC-7	
	No early track	Early track	No early track	Early track	No early track	Early track	No early track	Early track	No early track	Early track	No early track	Early track
Escs average	74.75 (3.21) *0.000*	81.17 (4.29) *0.000*	75.62 (3.25) *0.000*	84.36 (4.22) *0.000*	74.74 (3.20) *0.000*	80.47 (4.30) *0.000*	77.16 (2.84) *0.000*	95.27 (3.56) *0.000*	72.09 (3.40) *0.000*	80.35 (4.33) *0.000*	76.30 (3.36) *0.000*	78.18 (5.01) *0.000*
Escs standard Deviation	-31.22 (6.71) *0.000*	11.70 (8.15) *0.151*	-31.00 (6.77) *0.000*	12.63 (8.15) *0.121*	-39.63 (56.40) *0.482*	136.28 (57.55) *0.018*	-11.52 (54.93) *0.834*	99.54 (60.00) *0.097*	-37.04 (56.85) *0.515*	135.78 (57.47) *0.018*	-32.64 (6.89) *0.000*	15.34 (8.89) *0.084*
Escs standard deviation^2					4.80 (31.10) *0.877*	-72.88 (32.67) *0.026*	-9.95 (30.34) *0.743*	-53.24 (34.57) *0.124*	1.08 (31.42) *0.973*	-72.81 (32.59) *0.026*		
Groups of Control Variables												
Sc. Loc & Size	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Sc. Comp.	yes	yes	yes	yes	yes	yes	yes[6]	yes[6]	yes	yes	yes	yes
Sc. Resources	yes	yes	yes	yes	yes	yes	cnt average	cnt average	yes	yes	yes	yes
Sc. Institutions	yes	yes	yes	yes	yes	yes	cnt average	cnt average	yes	yes	yes	yes
Country level controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Sc. additional controls											yes	yes
Country F.E.									yes	yes		
Number of obs.	4,319	1,512	4,153	1,678	4,319	1,512	4,835	1,647	4,319	1,512	3,846	1,252
F	54.5	98.0	54.8	97.4	54.1	94.7	70.4	122.6	65.9	94.4	42.8	78.4
Prob.>F	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
R^2	0.5766	0.8046	0.5795	0.7957	0.5766	0.8059	0.5695	0.7863	0.6079	0.806	0.5804	0.8121

[1] Mexico, Turkey and France are not included. [2] Early track countries are considered those countries where the age of first school tracking is before 13. [3] Regressions are run using school sample weights provided in PISA database. [4] For each variable, the first row refers to the estimated coefficient, the second to the robust standard error and the third to the P value. [5] In model SC-2A the split between early and no early track countries is the one proposed by Waldinger (2006), coded in variable early_track2 (see tab. A1). [6] The share of students enrolled in vocational programme is considered as country average. Source: elaborations on PISA 2006 data

Tab. 7: Students performances in science in OECD countries[1]. OLS regressions[2, 3].

	ST-1	ST-2	ST-3	ST-4	ST-0
Peer Escs average	49.57	49.63	48.95	50.10	38.67
	(2.54)	(2.50)	(2.71)	(2.54)	(6.64)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Escs standard Deviation	-9.53	53.35	23.97	-16.62	
	(6.54)	(55.23)	(50.01)	(8.08)	
	0.145	*0.334*	*0.632*	*0.040*	
Escs standard Deviation^2		-36.02	-21.22		
		(30.82)	(28.06)		
		0.243	*0.450*		
Early track* Escs standard deviation				24.68	
				(12.53)	
				0.049	
Groups of Control Variables[4]					
Individual Characteristics	yes	yes	yes	yes	yes
Family background	yes	yes	yes	yes	yes
Peer composition	yes	yes	yes	yes	yes
School Location and size	yes	yes	yes	yes	yes
School Resources	yes	yes	country average	yes	yes
School Institutions	yes	yes	country average	yes	yes
Country level controls	yes	yes	yes	yes	yes
School fixed effects[5]					yes
School additional controls					yes
Students additional controls					yes
Number of observations	174,921	174,921	193,467	177,795	126,949
F	175.5	173.9	208.1	174.1	212.6
Prob.>F	0.000	0.000	0.000	0.000	0.000
R^2	0.3399	0.3400	0.3477	0.3393	0.4183

[1] Mexico, Turkey and France are not included. [2] Regressions are run using students sample weights provided in PISA database. [3] For each variable, the first row refers to the estimated coefficient, the second to the robust standard error – adjusted for clustering at the school level - and the third to the P value. [4] Imputation dummies for missing data are included in all regressions. [5] School fixed effects are identified, for each country, according to the quintile of escs average of every school. Source: elaborations on PISA 2006 data

Tab. 8: Students performances in science in OECD countries[1] by early and no early tracking countries[2]. OLS regressions[3, 4].

	ST-1		ST-1A[5]		ST-2		ST-5		ST-6	
	No early track	Early track	No early track	Early track	No early track	Early track	No early track	Early track	No early track	Early track
Peer Escs average	44.96 (2.92) *0.000*	60.36 (3.71) *0.000*	45.28 (2.94) *0.000*	60.12 (3.51) *0.000*	45.00 (2.89) *0.000*	60.07 (3.70) *0.000*	45.35 (2.87) *0.000*	60.44 (3.71) *0.000*	41.66 (3.13) *0.000*	60.09 (3.71) *0.000*
Escs standard Deviation	-17.68 (7.57) *0.020*	13.58 (8.57) *0.113*	-16.79 (7.64) *0.028*	13.25 (8.27) *0.109*	7.51 (71.23) *0.916*	110.59 (52.95) *0.037*	-10.16 (8.05) *0.207*	14.66 (9.80) *0.135*	-22.84 (7.59) *0.003*	14.40 (8.65) *0.096*
Escs Standard Deviation^2					-14.36 (39.37) *0.715*	-56.32 (29.48) *0.056*				
Vocational*Escs standard deviation							-68.25 (20.55) *0.001*	-6.68 (15.24) *0.661*		
Groups of Control Variables[4]										
Individual characteristics	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Family Background	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Peer composition	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
School Location and Size	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
School Resources	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
School Institutions	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Country level controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Country fixed effects									yes	yes
Number of observations	132,104	42,817	124,046	50,875	132,104	42,817	132,104	42,817	132,104	42,817
F	121.9	149.9	119.8	149.2	121.3	146.9	122.6	148.0	133.6	148.2
Prob.>F	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
R^2	0.314	0.5455	0.3143	0.5323	0.314	0.5459	0.3147	0.5455	0.3245	0.5459

[1] Mexico, Turkey and France are not included. [2] Regressions are run using students sample weights provided in PISA database. [3] For each variable, the first row refers to the estimated coefficient, the second to the robust standard error – adjusted for clustering at the school level - and the third to the P value. [4] Imputation dummies for missing data are included in all regressions. [5] In model ST-1A the split between early and no early track countries is the one proposed by Waldinger (2006), coded in variable early_track2 (see tab. A1). Source: elaborations on PISA 2006 data

Tab. 9: Students performances in science in OECD countries[1] by early and no early tracking countries[2]. OLS regressions[3, 4], including additional school and student controls and quintile of school escs average by country fixed effects

	ST-7		ST-8		ST-9	
	No early track	Early track	No early track	Early track	No early track	Early track
Peer Escs average	38.13 (3.11) *0.000*	53.76 (3.71) *0.000*	37.70 (7.59) *0.000*	48.05 (8.37) *0.000*	37.40 (7.48) *0.000*	48.08 (8.37) *0.000*
Escs standard deviation	-10.48 (7.26) *0.149*	12.00 (8.18) *0.143*	-6.73 (7.16) *0.348*	11.33 (8.49) *0.182*	-40.83 (49.49) *0.409*	127.95 (51.49) *0.013*
Escs Standard Deviation^2					19.23 (27.27) *0.481*	-67.50 (28.19) *0.017*
Groups of Control Variables[4]						
Individual characteristics	yes	yes	yes	yes	yes	yes
Family Background	yes	yes	yes	yes	yes	yes
Peer composition	yes	yes	yes	yes	yes	yes
Sc. Loc & Size	yes	yes	yes	yes	yes	yes
Sc. Resources	yes	yes	yes	yes	yes	yes
Sc. Institutions	yes	yes	yes	yes	yes	yes
Country level controls	yes	yes	yes	yes	yes	yes
School fixed effects[5]			yes	yes	yes	yes
School additional controls	yes	yes	yes	yes	yes	yes
Students additional controls	yes	yes	yes	yes	yes	yes
Number of observations	94,656	32,293	94,656	32,293	94,656	32,293
F	113.0	119.9	93.2	108.3	93.0	106.6
Prob.>F	0.000	0.000	0.000	0.000	0.000	0.000
R^2	0.3797	0.5778	0.3981	0.5824	0.3982	0.5831

[1] Mexico, Turkey and France are not included. [2] Regressions are run using students sample weights provided in PISA database. [3] For each variable, the first row refers to the estimated coefficient, the second to the robust standard error – adjusted for clustering at the school level - and the third to the P value. [4] Imputation dummies for missing data are included in all regressions. [5] School fixed effects are identified, for each country, according to the quintile of escs average of every school. Source: elaborations on PISA 2006 data

Tab. 10: Students performances in science in OECD countries[1] by early and no early tracking countries. Estimated coefficients by model ST-1 of "Peer escs average" and "Escs standard deviation". Quantile regressions[2].

	Peer Escs average		Escs standard deviation	
Percentile	No early track countries	Early track countries	No early track countries	Early track countries
10	46.38***	63.75***	-22.38***	16.18***
20	45.36***	61.40***	-22.60***	15.38***
30	45.89***	60.94***	-18.40***	8.39***
40	46.39***	58.28***	-23.56***	12.06***
50	45.29***	60.57***	-24.54***	11.75***
60	44.12***	63.20***	-21.35***	7.44***
70	45.01***	60.97***	-15.79***	12.52***
80	42.11***	59.29***	-16.43***	11.27***
90	41.08***	57.26***	-12.42***	22.05***

[1] Mexico, Turkey and France are not included. [2] Regressions are run using students sample weights provided in PISA database. Robust standard error – adjusted for clustering at the school level have been computed. Imputation dummies for missing data are included in all regressions. *** 99% significance level. Source: elaborations on PISA 2006 data

Tab. 11: School composition, educational policies and inequality of opportunity: interactions with student level family background effects. OLS regressions. No country fixed effects.

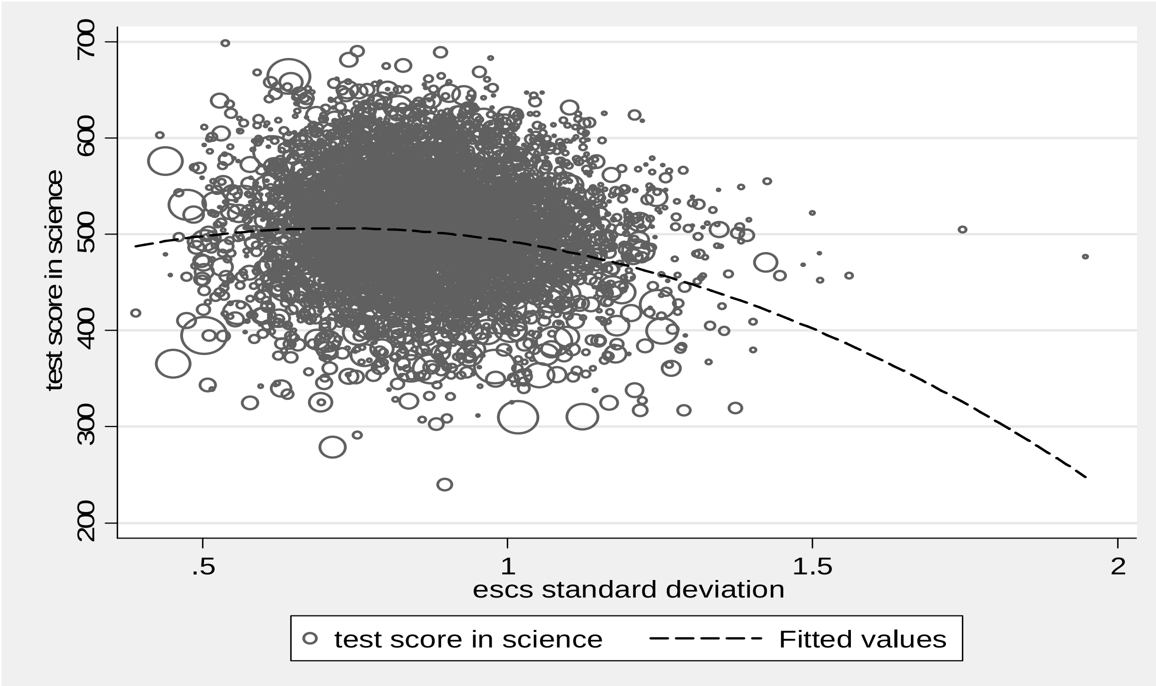
	Pooled		No early track countries	Early track countries
Escs	26.32***	50.64***	40.13***	105.26***
escs*early track	2.87*	1.18		
escs*peer escs average		9.93***	10.10***	11.09***
escs*escs standard deviation		-21.09***	-17.72***	-29.40***
Controls				
Individual characteristics	Yes	Yes	Yes	Yes
Interactions with counfounding factors				
Escs*Dur_preprimary	-2.50***	-1.94***	-2.27***	-8.80***
Escs*Public_cnt	1.14	0.14	1.96	16.50***
Escs*Stratio_cnt	1.29***	0.99***	1.46***	-1.16**
Escs*Educ_spending	0.00	0.00*	0.00	0.00***
Country Fixed effects	No	No	No	No
Number of observations	202,804	202,804	154,719	48,085
F	447.5	393.4	322.2	245.9
Prob.>F	0.000	0.000	0.000	0.000
R^2	0.1839	0.1879	0.1746	0.3168

[1] Mexico, Turkey and France are not included. [2] Regressions are run using students sample weights provided in PISA database. Robust standard error – adjusted for clustering at the school level have been computed. Imputation dummies for missing data are included in all regressions. *** 99% significance level; ** 95% significance level. * 90% significance level. Source: elaborations on PISA 2006 data

Tab. 12: School composition, educational policies and inequality of opportunity: interactions with student level family background effects. OLS regressions. Country fixed effects.

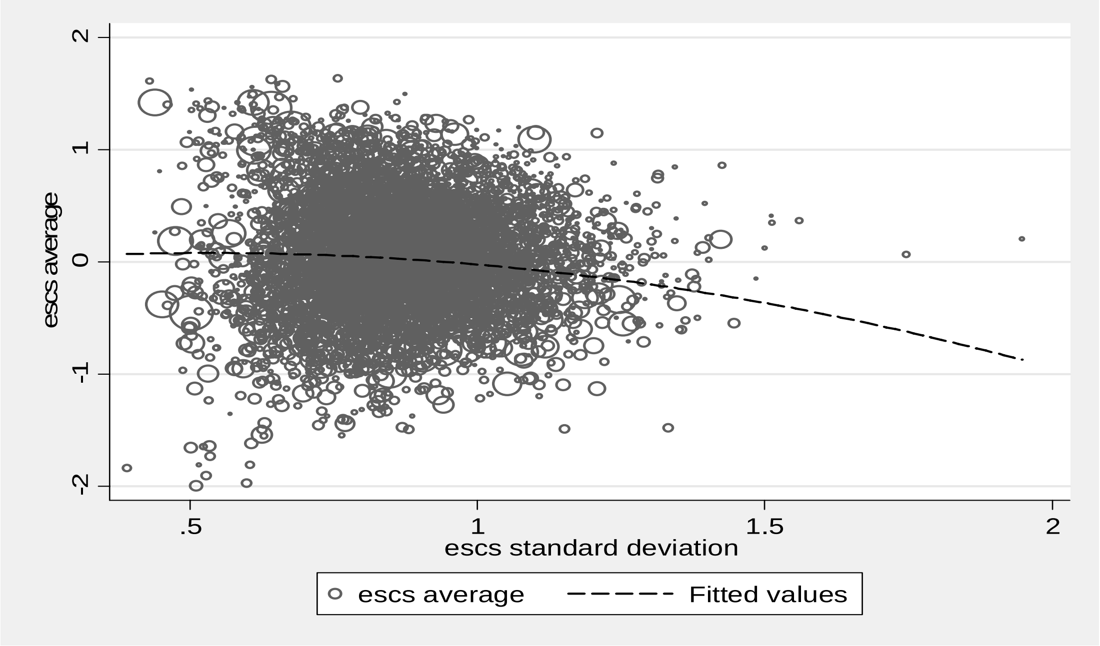
	Pooled		No early track countries	Early track countries
Escs	25.57***	55.42***	44.02***	94.90***
escs*early track	-3.91***	-5.91***		
escs*peer escs average		11.66***	11.73***	10.99***
escs*escs standard deviation		-26.66***	-25.33***	-28.23***
Controls				
Individual characteristics	Yes	Yes	Yes	Yes
Interactions with counfounding factors				
Escs*Dur_preprimary	-2.56***	-1.93***	-1.85***	-5.42**
Escs*Public_cnt	4.67**	3.64*	7.69***	12.11***
Escs*Stratio_cnt	1.18***	0.85***	1.51***	-1.24**
Escs*Educ_spending	0.00	0.00**	0.00***	0.00***
Country Fixed effects	Yes	Yes	Yes	Yes
Number of observations	202,804	202,804	154,719	48,085
F	232.7	222.6	237.5	220.5
Prob.>F	0.000	0.000	0.000	0.000
R^2	0.2328	0.2385	0.2217	0.3361

[1] Mexico, Turkey and France are not included. [2] Regressions are run using students sample weights provided in PISA database. Robust standard error – adjusted for clustering at the school level have been computed. Imputation dummies for missing data are included in all regressions. *** 99% significance level; ** 95% significance level. * 90% significance level. Source: elaborations on PISA 2006 data



700
600
500
400
300
200
test score in science
.5
1
1.5
2
escs standard deviation
test score in science
Fitted values

Figura 1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1
0
-1
-2
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1
1.5
2
escs standard deviation
escs average
Fitted values

Figura 3

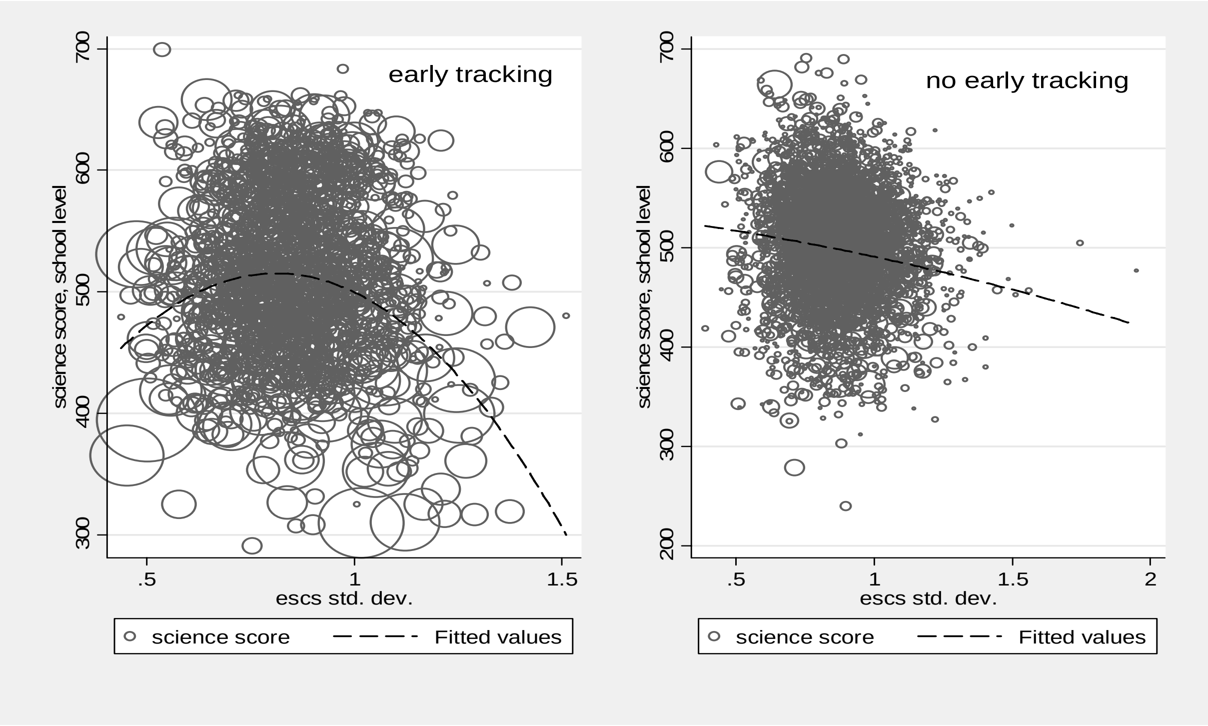

early tracking
no early tracking
science score, school level
escs std. dev.
science score
Fitted values
700
600
500
400
300
200
.5
1
1.5
2

Figura 2

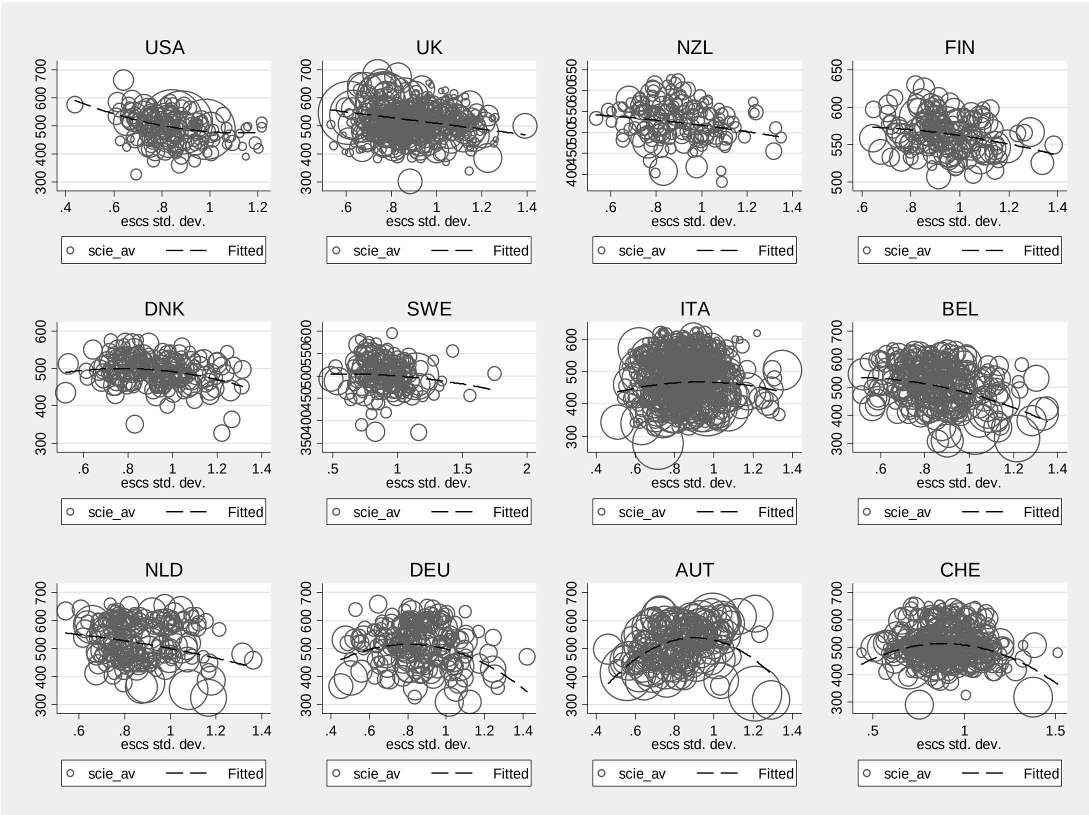

USA
UK
NZL
FIN
DNK
SWE
ITA
BEL
NLD
DEU
AUT
CHE
escs std. dev.
scie_av
Fitted

Figura 2

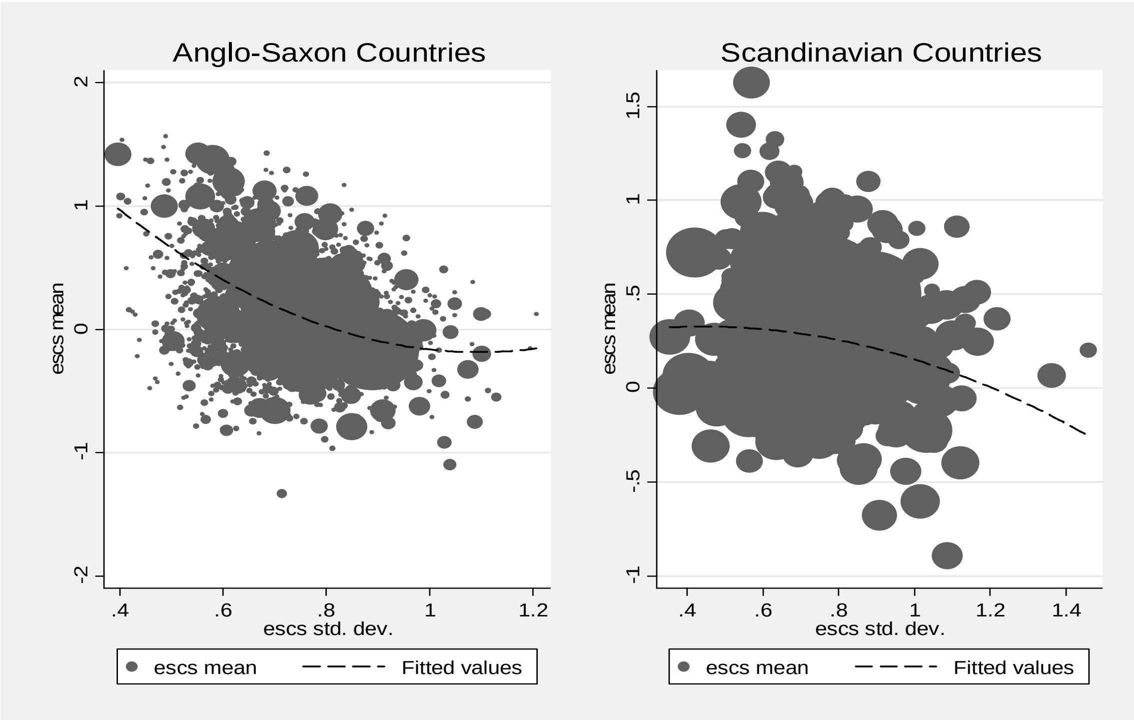

Anglo-Saxon Countries
escs mean
2
1
0
-1
-2
.4
.6
.8
1
1.2
escs std. dev.
escs mean
Fitted values
Scandinavian Countries
escs mean
1.5
1
.5
0
-.5
-1
.4
.6
.8
1
1.2
1.4
escs std. dev.
escs mean
Fitted values

Figura 3

Fig. 6: Quantile regressions on students performances in science in OECD countries. Estimated coefficients of "Peer Escs average". Source: elaborations on PISA 2006 data

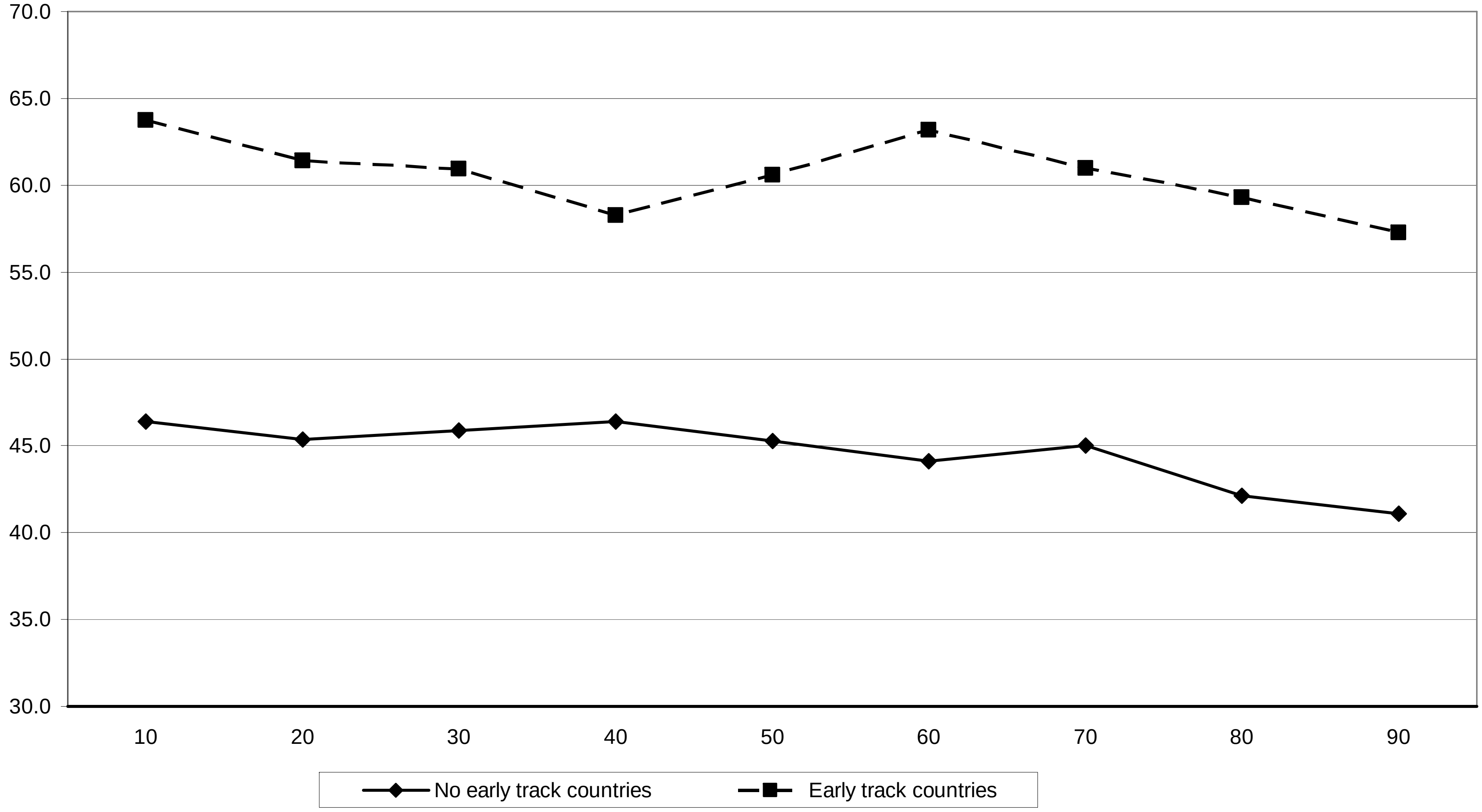
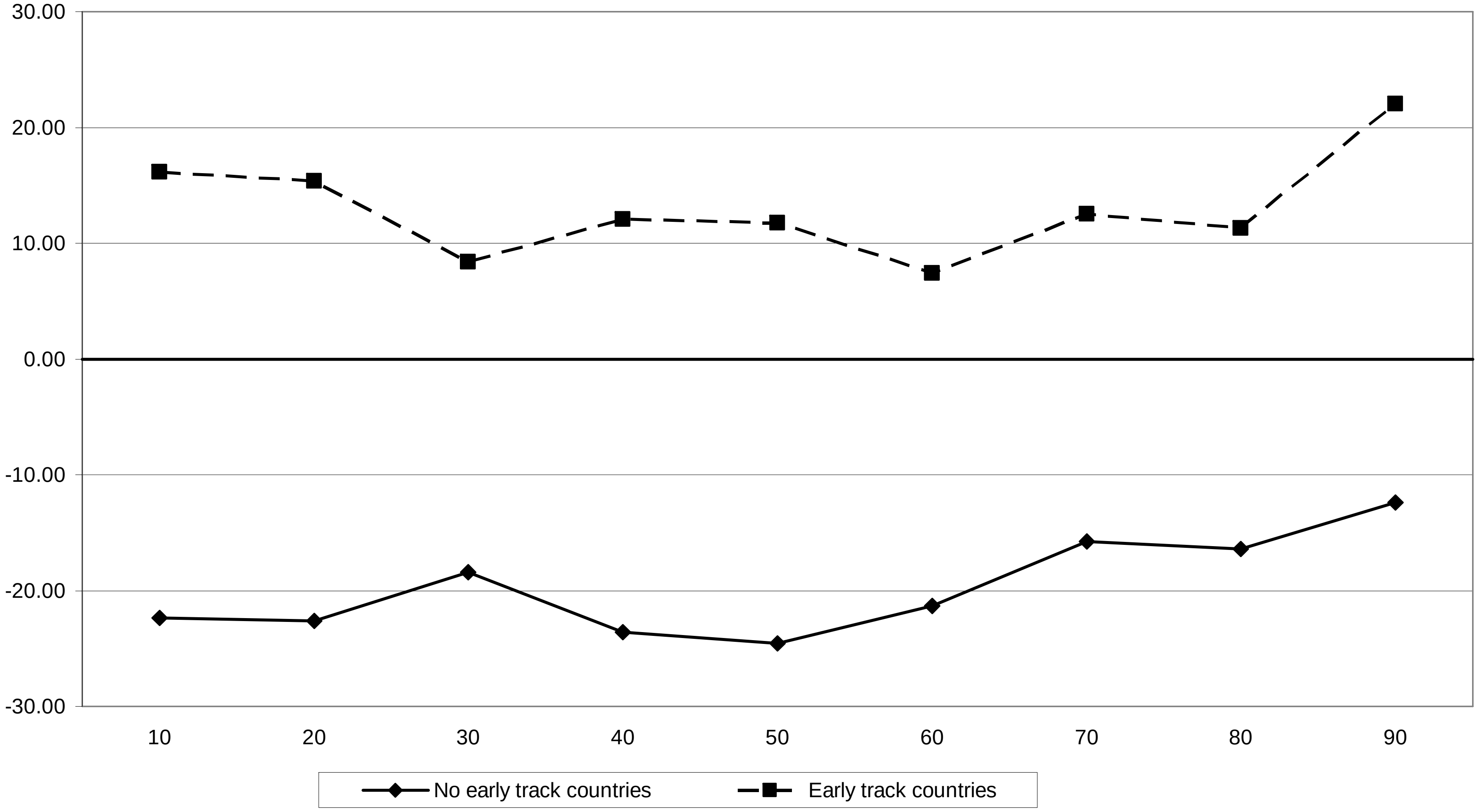


Fig. 7: Quantile regressions on students performances in science in OECD countries. Estimated coefficients of "Escs standard deviation". Source: elaborations on PISA 2006 data



Tab. A1: Control variables used in regressions

School level controls	
School Location and Class Sizes	
School Location	4 dummies built on the 5 modalities of the scq07 question of the Pisa school questionnaire
Class sizes	5 dummies built on the modalities of the scq06 question of the Pisa school questionnaire
School resources	
Ratcomp	Ratio of computers to school size
Compweb	Proportion of computers connected to web
Stratio	Student-Teacher ratio
Scmatedu	Quantitative index provided in PISA 2006 dataset about "Quality of educational resources"
Tcshort	Quantitative index provided in PISA 2006 dataset about "Teacher shortage" (on a negative scale)
School Institutions	
Respres	Quantitative index provided in PISA 2006 dataset about "Responsibility for resource allocation index"
Respcurr	Quantitative index provided in PISA 2006 dataset about "Responsibility for curriculum & assessment"
School type	3 dummies built on the modalities of the schltype variable provided in PISA 2006 dataset (i.e. "public", "private dependent", "private independent" and "missing schooltype" due to the several missing values of the schltype variable)
Residence	Dummy variable showing if residence in a particular area is a prerequisite or a high priority for being admitted to the school
Student record	Dummy variable showing if previous academic record (or a specific test) is a prerequisite or a high priority for being admitted to the school
N.B. the dummies showing the single components of school autonomy and responsibility about resource and curricula (the modalities of the scq11 school Pisa questionnaire) have not been included because of the several missing values. Replacing these variables with the respres and respcurr indexes (with much less missing values) does not alter regression results.	
School Additional Controls	
Sorting by ability	Two dummies from the 3 modalities of the Abgroup variable provided in the PISA 2006 dataset showing, respectively, if students are grouped according to their abilities within schools for all subjects of for some subjects
School competition	Two dummies from the 3 modalities of the scq10 question of the Pisa school questionnaire showing, respectively, if there is one (or more than one) other school in the area that competes for students
Principal evaluation	Dummy variable showing if achievement data are used in the evaluation of the principal's performance
Teacher evaluation	Dummy variable showing if achievement data are used in the evaluation of teachers' performance
Allocation evaluation	Dummy variable showing if achievement data are used in decisions about instructional resource allocation to the school
Over time evaluation	Dummy variable showing if achievement data are tracked over time by an administrative authority
School composition	
Average age	Average age of interviewed students at school level
Share of females	Share of females among interviewed students at school level

Share of immigrants	Share of immigrants among interviewed students at school level
Share of "foreign languages"	Share of interviewed students at school level which speak a foreign language at home.
Share of vocational students	Share of interviewed students at school level enrolled in a vocational programme.
Escs average	Average level of the escs index of interviewed students at school level
Escs standard deviation	Standard deviation (corrected for the country escs standard deviation) of the escs index of interviewed students at school level
Escs variance	Square of the escs standard deviation
N.B. in regressions at student level these variables (apart from escs standard deviation and variance) are considered net of the individual responses.	
Country level controls	
Gdp per capita	
Spending in education per capita	
Age of first track	
Early_track	Dummy variable showing if the decision about which school track to attend happens before age 13
Early_track2	Dummy variable showing if the decision about which school track to attend happens before grade 7
Duration of pre-primary schools	In years
External exam	Quantitative variable showing the share of students that is subjected to an external evaluation in science
Standard test	Quantitative variable showing the share of students that is subjected to standard evaluation tests in science
Student level controls	
Individual characteristics	
Age	
Sex	
Grade	Students below grade 8 and beyond grade 11 are excluded from the sample; then, grade is considered through 3 dummies
Vocational	Dummy variable showing if the student is enrolled in a vocational programme
Isced 3	Dummy variable showing if the student is enrolled in an upper secondary course
Immigrant	Dummy variable showing if the student was not born in the country of test
Foreign Language	Dummy variable showing if the student speaks a foreign language at home
Family background	
Hisei	Quantitative index provided in PISA 2006 dataset showing "the highest parental occupational status"
Pared	Quantitative index provided in PISA 2006 dataset showing (in years) "the highest parental educational level"
Homepos	Quantitative index provided in PISA 2006 dataset about "home possessions"
Books at home	Five dummies built of the six modalities of stq15 Pisa student questionnaire
Escs	Quantitative index provided in PISA 2006 dataset showing the "Family economic, social and cultural status"

Student Additional Controls	
Time science at school	Two dummies showing, respectively, if the student spends at school per week 2-4 or more than 4 hours studying science
Time science at home	Two dummies showing, respectively, if the student spends at home per week 2-4 or more than 4 hours studying science
Type of out-of-school-time lessons	Four dummies built on stq32 student Pisa questionnaire showing the kind of out of school time lessons in science attended by the student
Imputation mummie	
Intercept dummies	One dummy for each imputed variable showing if the value has been imputed.
Slope dummies	One dummy for each imputed variable showing the interaction between the intercept imputation dummy and the value of the imputed variable.