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The Role of Family Background on Secondary School Choices *

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Abstract

The aim of our work is to measure the impact of social origins on the choice of the academic track in order to allow for consistent cross-country comparisons. We analyze Italy, the Netherlands, and Germany, employing the data from PISA 2003. A substantive problem is that a good measure of individual ability before tracking occurs is not available, thus ability cannot be adequately kept under control. A simple model for school choice is proposed, but the model is not identified with cross-section data. The consequences of unobserved ability are assessed; in the absence of a measure of ability at time the track, the logit regression coefficient of social background is an estimate of the *total* effect of social background, given by the sum of direct and indirect effects. This is a measure of substantive interest because it represents the total *causal* effect of social origins on school track. Yet, given that regression coefficients in logit models with independent unobserved heterogeneity are biased towards zero, comparison across countries are difficult; the average sample derivative of the response probability is employed, and it is shown to be a valid alternative measure of the effect of explanatory variables in this context. Our main substantive finding is that the total effect of social background on the choice of the academic track is weaker in the Netherlands and stronger in Germany, with Italy somewhere in between, although, as the German case reveals, when access is regulated by formal restrictions based on ability tests the role of parental background is significantly reduced.

Keywords: equality of opportunity, intergenerational mobility, school systems, PISA, PIRLS

(Preliminary draft)

1. Introduction

Equality of opportunities in education is a widespread goal. In spite of this, the *Programme for International Student Assessment (PISA)*, carried out by OECD in order to evaluate how far students near the end of compulsory education have acquired some of the knowledge and skills that are essential for full participation in society (OECD, 2005)¹, highlights that in most countries performance is still greatly dependent on family background. Given that competencies also depend on the education programme and that preferences are related to social origin, understanding how school choices are undertaken is an important issue in the study of intergenerational mobility. The matter is particularly relevant in tracked school systems. "...Educational attainment is the major mediating factor in class mobility,

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1 Every PISA survey tests reading, mathematical and scientific literacy in terms of general competencies, that is, how well students can apply the knowledge and skills they have learned at school to real-life challenges. PISA does not test how well a student has mastered a school's specific curriculum.

although this is more apparent when education is measured by highest level of qualification achieved (academic or vocational) rather than by the number of years of education completed” Erikson and Goldthorpe (2002, pg. 37).

Tracks differ in the curriculum offered and in the average skills of the students. Choices are thus affected by individual ability and by family background. The aim of our work is to measure the impact of social origins on the choice of the track² in order to allow for consistent cross-country comparisons. We analyze Italy, the Netherlands, and Germany, employing data from PISA 2003³.

The age of tracking varies from 10 in Germany to 14 in Italy. Germany is an interesting reference point as its very early tracking system has been sharply criticized because it is believed to maximize the influence of parents’ social background on future educational attainment (Dustmann, 2001, Sinn, 2006). This argument is not supported by Checchi and Flabbi (2007), whose claiming is that the way students are sorted into tracks is less strongly related to parental background and more to individual ability in Germany compared to Italy⁴.

There are few recent papers investigating the determinant of enrolment into the different tracks in Italy. Cappellari (2004) analyses the distinction between general and technical schools together with the public/private dimension, allowing for correlation of the unobservables in the two equations. Checchi and Flabbi (2007) study the determinants of the choice of different tracks with PISA, comparing Italy and Germany. Dustmann (2001) focuses on Germany. All the results indicate that social background still plays a substantial role in shaping educational decisions. There is also an ongoing project of the MPIB (Max-Planck-Institut für Bildungsforschung, Berlin) aimed at investigating transition from primary to secondary school in Germany (MPIB, 2006), but the results are not yet available.

The main problem we face with PISA is that a good measure of the competencies acquired before tracking occurs is not available, thus we cannot adequately keep ability under control. A simple model for the process of “ability building” is proposed (Fig. 1), but the model is not identified with cross-section data⁵. In the absence of a measure of ability at time the track is chosen, under the assumption that initial ability is independent from individual and family characteristics, the regression coefficient of social background over school track is an estimate of the total effect of social background⁶. Although we should seek to disentangle the direct and indirect effects, and thus identify primary and secondary effects (see Section 4), the estimated effect is still a measure of substantive interest because it represents the total *causal* effect of social background on school track.

Checchi and Flabbi (2007), who also employ PISA data, adopt a different strategy: they include PISA performance scores to proxy ability. Our argument against this option is that

2 Other features of secondary schools relevant also in comprehensive school systems are affected by social background. The most important is the choice between private and public schools. Contini, Scagni, Riehl (2007) attempt to evaluate for Italy whether decisions are related to the school social composition.

3 Clearly, the purpose of PISA is not the study of secondary school choices. Nevertheless, it is a good starting point for cross-country comparisons, because of common variable definitions and sampling scheme. This feature is particularly important for public policy evaluation, whenever the aim is to assess the impact of specific institutional features on educational outcomes.

4 As explained in the sequel, we will question this result. Although we do not specifically address the issue in this paper, we do find some evidence on this topic. A speculative interpretation of the statistical evidence is presented in the concluding section.

5 Our model is coherent to the model underlying the counterfactual approach employed in a few recent contributions (see for example Erikson *et al.*, 2005) aiming at assessing the relative contribution of primary and secondary effects in educational attainment. However, lacking a clear measure of ability before tracking, this approach cannot be employed with PISA data.

6 *Nurture* and *nature* effects are indistinguishable if the independence assumption doesn’t hold.

these scores are endogenous, because they refer to a time well after that of school choice. Employing the PISA score can give rise to severe bias, as highlighted by the results of a set of simulations (see the Appendix).

Comparing the results across such a limited number of countries does not allow us to ascribe the differences to specific institutional features (Brunello and Checchi, 2007, for example, employ a much larger set of countries in order to evaluate the impact of school tracking on equality of opportunity). In spite of this, preliminary analyses on the effect of access restrictions to the higher level tracks can be carried out by exploiting the institutional differences across the German Länder (states). While in some states families are essentially free to choose the track, in other Länder decisions depend on a formal assessment testing the student's ability. Assuming that restricted access is an exogenous policy, by comparing the social background effect in the states with formal restrictions with the states with no constraints, we can assess the impact of these rules on equality of opportunity in secondary school transitions. By enhancing sorting by ability, the social background effect should be weakened, as suggested by Checchi and Flabbi (2007). On the other hand, if primary effects are predominant (i.e. higher status children are on average more able than lower status students), access restrictions could in the end reinforce inequality of opportunity. The net effect of access restrictions on school track enrolment is thus theoretically undetermined.

School designs of the countries under study differ in many other aspects. Given these institutional differences, it is difficult to find a comparable classification involving the distinction among academic, technical and vocational schools. Moreover in Germany the education system is Länder-based so that within the country students do not face the same set of available options (see Table 1). For this reason, we focus here on the more clear-cut distinction between the academic track (*lyceum*) and the other tracks. This option is common in the literature (Erikson *et al.*, 2005) and retains a close relation to the student's decision on further (tertiary) education.

Our main finding is that the total effect of social background on the choice of the academic track is weaker in the Netherlands and stronger in Germany, with Italy somewhere in between. When access is regulated by formal restrictions based on ability tests the role of parental background is significantly reduced, as results from the estimation of the German model. In spite of this, the overall effect of social origin appears to be stronger in Germany than in Italy and the Netherlands even in the areas of the country where these rules apply, suggesting that these rules alone do not necessarily counterbalance the negative effects on equality of opportunity due to other features of the school design.

In this paper we also address statistical issues. Logit models regression coefficients are estimable up to arbitrary identification restrictions on the error variance and with independent unobserved heterogeneity are biased towards zero (Cramer, 2005). Comparison across countries are difficult in this context. For this reason the average sample derivative of the response probability is sometimes employed as an alternative measure of the effect of explanatory variables. By extending the simulation study of Cramer using our model and calibrating the parameters on the relevant observed distributions, we verify that this alternative measure is not biased when ability is unobserved (see the Appendix).

The paper is organized as follows. In Section 2 we describe the school systems of the countries under study. In Section 3 we present some descriptive evidence on selected outcomes of the decision process. Section 4 introduces the theoretical model and the estimable model when ability is not observed. In Section 5 we discuss the statistical problems arising when the effects of explanatory variables are compared across samples. Section 6 is devoted to the empirical analysis and the data. The simulation study is described in the Appendix.

2. Educational Systems

In this paper we compare three countries with a tracked school system: Italy, the Netherlands and Germany. Tracking generally occurs at 10 years old in Germany⁷, at 12 in the Netherlands, at 14 in Italy. In this section we briefly review the main features of the education systems in each of these countries.

2.1 Italy

Compulsory education starts at the age of six, with the five year cycle of primary school, continues with three years of lower secondary education (*scuola secondaria di primo grado*), still comprehensive, and, ending at the age of fifteen⁸, usually lasts until the first year of upper secondary school (*scuola secondaria di secondo grado*). At age 14 students choose their upper secondary school between a variety of different programmes.

The academic track lasts five years and contains different types of lyceum (*liceo*), classical, scientific, linguistic or artistic. The *socio-pedagogic lyceum*, the former *istituto magistrale*, prepares students for the profession of primary teacher, for which only recently a further course at university has become necessary. Furthermore there are technical (*istituti tecnici*) and vocational (*istituti professionali*) upper secondary schools, the first lasting five and the second three years. These two lead directly to a professional qualification.

There are no special admission requirements, such as ability tests or marks, for access to the different upper secondary school types. All tracks permit access to university, provided that the final certificate is obtained after five years of schooling, with two integrative years for the *istituti professionali*. (Eurydice, 2006b).

2.2 Netherlands

Primary education takes place in an 8 year cycle from the age of four (facultative) or five (compulsive) until about 12 (in fact most children start school at the age of four). Compulsory education lasts 12 years full time, at least until age 16, followed by another year of at least part-time schooling (Eurydice, 2006c). At twelve, pupils are divided into three main tracks.

The academic *VWO* prepares students for university in six years, the *HAVO* provides higher general education for five years after which students can access higher professional education and the *VMBO* is a school of vocational education, divided into different sectors and pathways, lasting four years and giving access to apprenticeship.

For admission to the different tracks, student's suitability is assessed by a primary school leavers attainment test; parents may express a preference, but the final decision is taken by the secondary school board.

There is a maximum time of five years (extended to six in special cases) to complete the lower secondary level, which are the first three years of *VWO* and *HAVO* and whole four year *VMBO* course. If a student fails twice in the same grade he must change to another type of school. The leaving certificates of one school (*VMBO* or *HAVO*) give access to a higher level school (*HAVO* or *VWO*) only if the curriculum meets certain requirements.

Most schools are combined schools offering more than one track. A feature of the Dutch education policy is the freedom to set up private schools, which are publicly financed and attended by over 70% of all students (Eurydice, 2003).

7 In two German states, Berlin and Brandenburg, tracking takes place after 6 years of primary school at the age of twelve (Woessmann 2007).

8 With effect from school year 2009/2010, the end of compulsory education shall be raised to the age of 16 (Andrews, Brown, Sargent 2007), so that it will last ten instead of nine years.

2.3 Germany

In Germany the *Länder* (states) have the responsibility over educational issues, making the German school system rather heterogeneous. Institutional differences regard the type of schools students are tracked into, and the admission requirements to the different tracks.

Primary school lasts four years (from age 6 to 10). Pupils are subject to compulsory education from 6 to 18, with at least 9 years of full-time schooling. Primary school are generally requested to give a recommendation for the transition to secondary school, but while in some *Länder* families are anyhow free to choose⁹, in others this choice is conditional on a formal assessment testing the student's suitability for the selected track. In any case, once the student has begun secondary school, each grade can be repeated only once and the new failure in the same or the following grade leads to a change of school to a lower level.

In the most common system there are three distinct tracks, on different ability levels: *Gymnasium* (academic), which lasts nine years now being reduced to eight for a total 12 years of schooling, *Realschule* (professional), with six grades, and *Hauptschule* (vocational), with five or six years of schooling. The first gives access to university, the other two to different professional or vocational education and apprenticeship, usually organised in the so-called dual-system, i.e. a combination of job training and school lessons. After the *Hauptschule*, the *Realschule* leaving certificate can be obtained at the vocational school as well, if the student reaches a certain performance level. Similarly, the transition from *Realschule* to *Gymnasium* is in principle allowed, but is conditional on achievement level.

In some of the *Länder* there are only two distinct tracks, because *Realschule* and *Hauptschule* are combined in only one school¹⁰ but the school leaving certificates are equivalent to those in the three-track-system (Eurydice, 2006a). In most states there is also the alternative of a comprehensive school (*Integrierte Gesamtschule* or *Kooperative Gesamtschule*) combining all three schools. In some of the *Länder* more students are enrolled to this kind of institution than to the more traditional school types. So every Land has its very own combination of school types and its own rules for admission.

Table 1 illustrates the main secondary school options in the 16 states. The numbers refer to the percentage of students reported in the German "PISA 2003 extension study" (Prenzel *et al.*, 2005). The last column indicates the *Länder* with most restricting rules for admission to *Realschule* or *Gymnasium* at that time (KMK, 2006).

9 Or, in some cases, a first period at the new school is considered as a trial period. About one third of German students attend school systems that involve significant restrictions on track choice.

10 Different names are given to these schools: *Oberschulen*, *Sekundarschulen*, *Erweiterte Realschulen*, *Mittelschulen*, *Regelschulen*.

Table 1. *Frequencies of 15 years-old students in main school types by Länder in Germany*

Land	Gymnasium	Realschule	Hauptschule	Integrierte Gesamtschule comprehensive	mixed Hauptschule Realschule	ACCESS
Baden-Württemberg	27.8%	30.3%	27.9%			restricted
Bayern	26.3%	27.2%	32.2%			restricted
Berlin	34.5%	21.6%	11.2%	27.3%		free
Brandenburg	30.8%	15.7%		50.1%		free
Bremen	30.6%	26.7%	21.7%	15.5%		free
Hamburg	33.4%	14.9%	10.6%	25.4%	5.1%	free
Hessen	31.7%	27.0%	15.6%	16.6%		free
Mecklenburg-Vorpommern	25.8%	9.6%			53.2%	free
Niedersachsen	26.6%	33.5%	28.3%			free
Nordrhein-Westfalen	28.8%	24.7%	26.6%	16.2%		free
Rheinland-Pfalz	25.8%	22.2%	22.9%		12.8%	free
Saarland	25.7%			13.5%	45.7%	restricted
Sachsen	32.0%				61.8%	restricted
Sachsen-Anhalt	30.6%				60.9%	free
Schleswig-Holstein	25.2%	31.4%	29.3%	6.5%		free
Thüringen	30.5%				57.6%	restricted

3. Some outcomes of the selection process. Descriptive evidence

Measuring how strongly social origins influence the secondary school decision process is the issue tackled in present work. In this section we examine some descriptive evidence on potential outcomes of this process: social stratification in secondary schools and the extent to which PISA scores vary across schools and school tracks.

A few words on the *Programme for International Student Assessment*. PISA is an international survey of the knowledge and skills of 15 year olds promoted every three years by OECD (first survey in year 2000). Each survey covers different skills: mathematics, reading comprehension, science and problem solving. The main assessment for PISA 2003 is on mathematics: tests evaluate how well students can recognize, formulate and tackle mathematical problems in real life contexts. The questionnaire contains detailed information on the family background of students. A specific index is provided, the so-called *ESCS (Index of Economic, Social and Cultural Status)*, see section 6.1).

PISA reports reveal wide differences in countries' skill profiles. Average scores in the math test are reported in Table 3, column (4): Italy is placed at the very lowest ranks in the OECD list¹¹, Germany is around the average, while the Netherlands is considered a well performing country (OECD, 2004).

A simple measure of social stratification in schools is given by the ratio of the variance between schools and the total variance of the *ESCS*. This analysis can be carried out with PISA data because around 30 students are randomly chosen within each selected school. The measure is reported in Table 2 column (1) for selected countries. Generally speaking, tracked systems rank in the top half of the list (for example Hungary, Austria, Italy, Germany, France), the only exception being the Netherlands, where the variance ratio is much smaller,

¹¹ Regional differences in Italy are very marked: average scores of students from the Northern part of the country are much higher than those from the South, the former being at the level of Denmark (higher than OECD's average), the latter being at the level of Turkey, the worse performing country of the area.

close to that encountered in comprehensive school systems. The percentage of variance between tracks (as opposed to single schools) is reported in column (2). Similar results are described in Jenkins *et al.* (2006).

When turning to the analysis of math PISA scores (Table 3), we find the Netherlands at the first place in the OECD area, followed by Germany. In both countries the score variance between single schools is more than 60% of the total variance and the one between school tracks is over 50%. In Italy the value is smaller but still rather high if we look at the single school level, but when we consider tracks it drops to less than 25%.

The differences encountered across schools and school types are to be interpreted with caution, because PISA scores represent ability at the time of the survey, *not* before school choice is undertaken. Thus, the data does not tell us how well the education systems divide students according to their ability, because skills keep on developing during secondary school, and the “rate of growth” may be different across individuals and tracks. Cross-country comparisons are difficult also because tracking does not occur everywhere at the same age, as described in section 6.

Nevertheless, these numbers do suggest that:

- in the Netherlands students are well divided by ability into tracks and these differences only to a small extent reflect social differences;
- in Germany school sorting also seems to be highly related to ability (although tracking here occurs earlier than elsewhere, so the issue of endogeneity is more severe), however social stratification is much stronger than in the Netherlands;
- in Italy there are also deep social differences among schools, while performance variability is large across schools but much less across tracks, implying that a large fraction of the differences among students of different schools are not related to the differences in the curricula.

Table 2. *Social stratification in schools. Analysis of variance between schools and between school types of the Index of Economic, Social and Cultural Status (ESCS). Selected countries*

Country	(1) Between Schools η^2	(2) Between School-types η^2	(3) Total Variance	(4) Mean	(5) Std. Deviation
Hungary	0,449		0,784	-0,068	0,885
Mexico	0,443		1,451	-1,129	1,205
Turkey	0,403		1,203	-0,980	1,097
Austria	0,343		0,723	0,061	0,851
Italy	0,343	0,216	1,048	-0,111	1,024
Germany	0,332	0,232	0,978	0,160	0,989
Belgium	0,331		0,888	0,152	0,942
France	0,315		0,870	-0,078	0,933
Spain	0,305		1,011	-0,297	1,005
United States	0,272		0,835	0,296	0,914
Netherlands	0,257	0,159	0,733	0,098	0,856
Poland	0,253		0,675	-0,201	0,822
Finland	0,141		0,688	0,247	0,830

η^2 is the ratio of the variance between schools or school-types over the total variance of ESCS

Table 3. *PISA mathematic scores: Analysis of variance between schools and between school types. Selected countries*

Country	(1) Between Schools η^2	(2) Between School-types η^2	(3) Total Variance	(4) Mean	(5) Std. Deviation
Netherlands	0,659	0,556	8082	538	89,9
Germany	0,629	0,511	9911	503	99,6
Turkey	0,614		10161	423	100,8
Hungary	0,602		8050	490	89,7
Belgium	0,575		11375	529	106,7
Italy	0,569	0,246	8465	466	92,0
Austria	0,563		8085	506	89,9
Mexico	0,542		6484	385	80,5
France	0,520		7758	511	88,1
United States	0,310		8487	483	92,1
Spain	0,272		7145	485	84,5
Poland	0,168		7482	490	86,5
Finland	0,083		6365	544	79,8

η^2 is the ratio of the variance between schools or school-types over the total variance of the math PISA scores

4. The model

“Human capital accumulation is a dynamic process. The skills acquired in one stage of the life cycle affect both the initial conditions and the technology of learning at the next stage. ... A major determinant of successful school is successful families. School operate more efficiently if parents reinforce them by supporting and encouraging children.” (Carneiro and Heckman, 2003, pg. 6).

We analyze how social background affects secondary school choice (see Figure 1), assuming that:

- i) there is a latent unobservable individual “initial ability” not correlated with individual’s social background SB^{12} ;
- ii) the individual ability before school choice is undertaken (we will call it “previous ability”) depends on initial ability and SB ;
- iii) the choice of secondary school ST depends on previous ability and SB .

The effect of social background on school-choice is twofold.

- *Direct effect:* given the level of ability, individuals from higher social background are more likely to enrol in the academic track (as they generally have higher aspirations, lower opportunity costs...);
- *Indirect effect:* higher status children reach on average higher levels of ability at the end of primary or lower secondary school (they may be exposed to more intellectual stimulation, receive more parental motivation and support for schoolwork... the so-called nurture effects). Being more skilled, they are more motivated to choose the academic track.

12 Assumption i) is rather strong: if intelligence was at least partially inherited, an intergenerational mechanism of social selection could be at work, giving rise to a correlation between “initial ability” and social status (Woessmann, 2004). In this case it would not be possible to separate nurture effects from nature effects.

The intensity of the indirect effect depends on how strongly decisions are affected by ability, but also on the way ability is influenced by social background. Direct and indirect effects can be related to the conceptual distinction between primary and secondary effects. Primary effects, “those ... that create class differentials in demonstrated ability early in children’s educational careers...” (Erikson and Goldthorpe, 2002, pg. 41), are represented by the association between social origins and ability, the first arrow of the indirect effect. Secondary effects, “those that later operate through the choices that children make among the options they have available”, correspond to the direct effect.

Policy implications of a high primary effect vs. a high secondary effect are rather different. In the latter case interventions should be directed towards enhancing the performance of low status children at primary school level, in order to reduce the ability gap. In the latter case the focus should be on endorsing the enrolment of lower status children into the academic track.

In order to assess direct and indirect effects, data on student's ability before secondary school choice is needed. As Breen *et al.* (2005) point out, the different effects of social origin on educational attainment are impossible to disentangle without adequate longitudinal data. Under the assumptions made above, when we model school choice without controlling for ability the regression coefficient of *SB* will represent the *total* effect of social origins on school track, given by the sum of the direct and the indirect effect. This is however a quantity of substantive interest because it is altogether a *causal* effect.

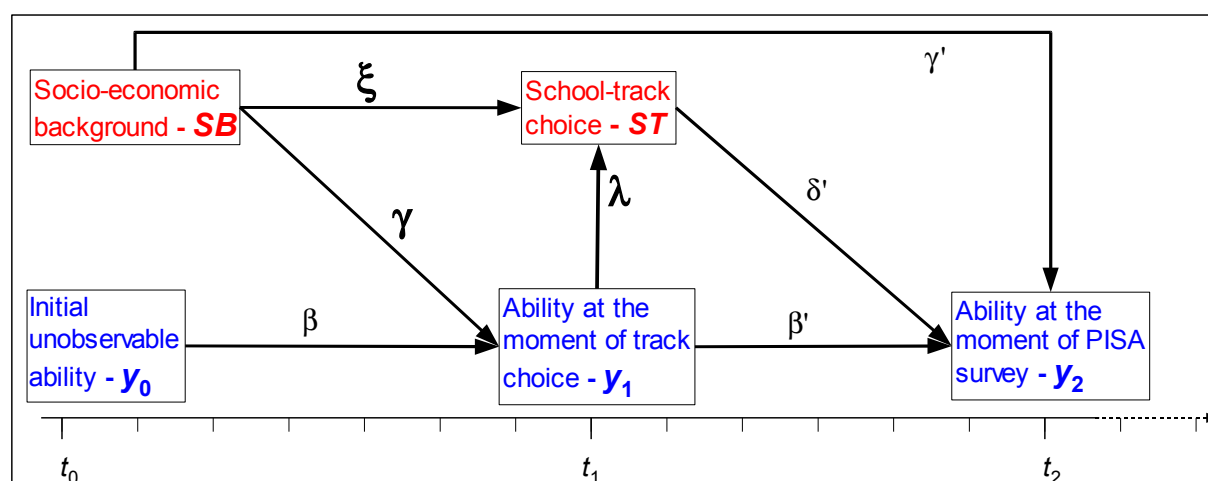


Figure 1. Modeling secondary school choice and PISA scores

Let y_t represent ability at time t . Consider the time of birth $t=0$, a time $t=1$ before secondary school choice and the time of the PISA survey $t=2$. Thus:

- y_0 is the unobservable initial ability;
- y_1 is the so-called “previous ability” (potentially observable, but here unobserved);
- y_2 is ability at $t=2$, measured by the PISA score.

Let *SB* be a continuous measure of social background, while *ST* is a binary variable taking the value 1 if the academic track is chosen and 0 otherwise. Translating into formal terms the model depicted in Fig.1 we assume that individual ability develops as follows:

$$y_{1i} = \alpha + \beta y_{0i} + \gamma SB_i + u_i \quad (1)$$

$$ST_i^* = \mu + \lambda y_{1i} + \xi SB_i + \varepsilon_i \quad (2a)$$

$$ST_i = \begin{cases} 1 & \text{if } ST_i^* > 0 \\ 0 & \text{if } ST_i^* \leq 0 \end{cases} \quad (2b)$$

$$y_{2i} = \alpha' + \beta' y_{1i} + \gamma' SB_i + \delta' ST_i + v_i \quad (3)$$

Notice that all the regression coefficients in the model are likely to be positive.

Following the usual notation for binary choice models, ST^* is a latent variable related to the utility of enrolling in the academic track, and ST is its observed counterpart. The errors for each equation are mutually independent and independent from the explanatory variables included in their own equation. Initial ability is regarded as a random variable independent from all observed and unobserved individual characteristics: $\rho(y_0, SB) = 0$ and $\rho(y_0, u) = 0$.

If the error ε_i has a standard logistic distribution, equations (2a)–(2b) give rise to the well known expression for the logit model:

$$\ln \left(\frac{\Pr[ST_i = 1]}{1 - \Pr[ST_i = 1]} \right) = \mu + \lambda y_{1i} + \xi SB_i \quad (2c)$$

and the exponential of regression coefficients can be interpreted as odds-ratios. For example:

$$e^\xi = \frac{\frac{\Pr(ST = 1 | SB = x + 1)}{1 - \Pr(ST = 1 | SB = x + 1)}}{\frac{\Pr(ST = 1 | SB = x)}{1 - \Pr(ST = 1 | SB = x)}}$$

represents the relative change in the odds of enrolling in the academic track following to a one unit rise of SB .

Equation (3), the model for the PISA score, is based on the assumption that scores are directly affected by previous ability, school-type and social background. This equation will be employed in the appendix with the purpose of evaluating the strategy of using the PISA score as a proxy for previous ability.

Given that y_1 is not observed, when ignoring ability the equation for latent ST^* becomes:

$$\begin{aligned} ST_i^* &= \mu + \lambda (\alpha + \beta y_{0i} + \gamma SB_i + u_i) + \xi SB_i + \varepsilon_i \\ &= (\mu + \lambda \alpha) + (\lambda \gamma + \xi) SB_i + (\lambda \beta y_{0i} + \lambda u_i + \varepsilon_i) \end{aligned} \quad (4)$$

With respect to the effect of SB on school choice, we observe that:

- the coefficient of SB is given by $(\lambda \gamma + \xi)$; it represents the total effect of social background on the probability to enrol in higher track schools¹³ and is given by sum of the direct effect ξ and the indirect effect $\lambda \gamma$;

¹³ If the assumption of independence between social background and “initial ability” did not hold, a third component would enter the estimated total effect, representing the so-called nature effects.

- the direct effects of ability and social background λ and ξ are not identified. ξ represents the net effect of social background given ability and λ is the net effect of ability given social background;
- ξ also represents *secondary effects* of social origin on secondary school choices; is not an exhaustive measure of the influence of SB on school choice, because indirect effects are potentially relevant too;
- γ , a component of the indirect effect, represents the *primary effect* of SB.

Note that γ cannot be estimated within the PISA database, although, as we will see, it is possible to approximately evaluate it by employing the data of the comparative education survey PIRLS¹⁴, submitted to fourth grade students.

The error term in equation (4) ($\lambda\beta y_{0i} + \lambda u_i + \varepsilon_i$) is larger than the original error ε_i , and is still independent from explanatory variable SB . Moreover, its distribution is generally no longer a logistic.

When omitting ability, the estimated logit model is:

$$\ln\left(\frac{\Pr[ST_i=1]}{1-\Pr[ST_i=1]}\right) = \mu' + (\lambda\gamma + \xi)SB_i \quad (5)$$

Consequences of the omission of ability on the estimate of the total effect are discussed in the following section.

5. Comparing the effect of social background across countries

Assessing the effect of explanatory variables on the response variable is more complicated in binary response models than in linear models, in particular with neglected heterogeneity. This circumstance is relevant in our context because previous ability is omitted.

5.1 General discussion

First, logit (and probit) models are non-linear: the effect on the probability $\Pr(Y=1|\mathbf{x})$ varies with the value of the independent variables: where the probability curve is almost flat the response probability varies little; where the curve is steep the response probability exhibits a much larger change. When we describe the effect of the variable by means of the regression coefficient β we miss this point. Notice that for given x , the slope of the probability curve varies also with the value of the constant and of the other explanatory variables (see Figure 2).

For this reason, alternative measures of the impact of the regressors on the response variable have been proposed in the econometric literature (see for example Long, 1997; Wooldridge, 2002) and are now widely employed. These measures are based on the slope of the probability curve, i.e., the partial derivative of $\Pr(Y=1|\mathbf{x})$ with respect to each explanatory variable x_k .

In particular, let y be a binary response variable, \mathbf{x} a vector of explanatory variables and β the vector of associated coefficients:

¹⁴ PIRLS is an international survey aimed at evaluating reading comprehension of fourth grade students across a number of countries. For further details see section 6.4.

$$ASE = \text{sample mean} \frac{\partial \hat{\Pr}(y=1|x)}{\partial x_k} = \frac{1}{n} \sum_{i=1}^n \frac{\partial \hat{\Pr}(y=1|x)}{\partial x_k}$$

is the average slope over all the sample units; following Cramer (2005) we call this measure *ASE* (*average sample effect*). Note that for a given β its value changes according to the actual location of the explanatory variables in the sample. Suppose we wish to compare the effect of x_k across countries. Consider countries A and B, with the *same* value of β_k ; if explanatory variables in country A are positioned where the curve is almost flat while in country B they are in the steep part of the curve then, when x_k changes, the response probability changes more in B than in A. Coherently, $ASE_A < ASE_B$.

Another measure is given by the slope of the response probability at the average value of the explanatory variables \bar{x} , which can be thought as a “representative” individual. We call it the *effect at the sample average* (*ESA*):

$$ESA = \frac{\partial \hat{\Pr}(y=1|\bar{x})}{\partial x_k}$$

For the logit model, the *ASE* and *ESA* associated to variable x_k turn out to be:

$$ASE = \sum_{i=1}^n \hat{\Pr}(y=1|x) (1 - \hat{\Pr}(y=1|x)) \hat{\beta}_k$$

$$ESA = \hat{\Pr}(y=1|\bar{x}) (1 - \hat{\Pr}(y=1|\bar{x})) \hat{\beta}_k$$

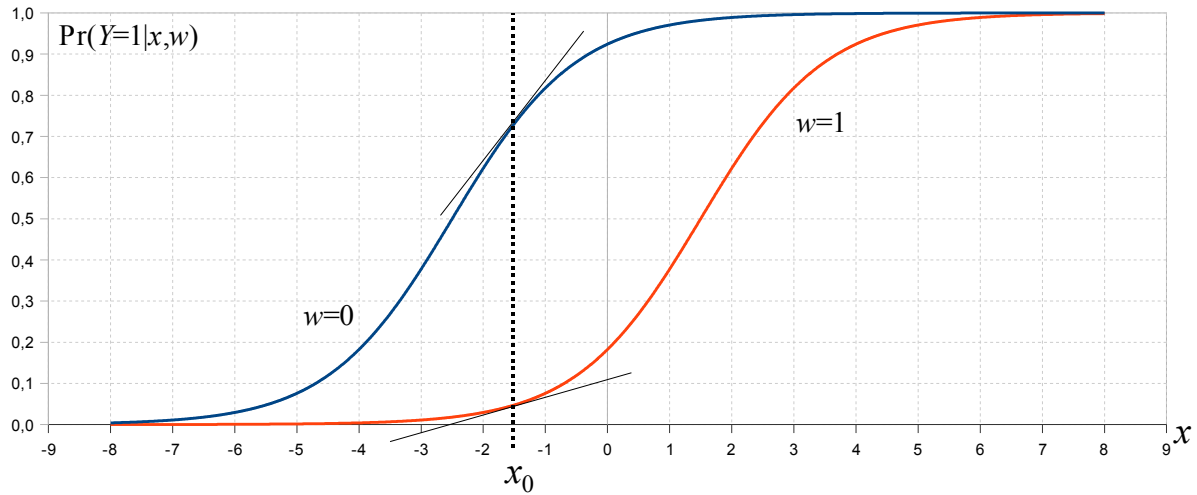


Figure 2. Slope of the logistic function at different values of explanatory variables

The interpretation problems of β become more severe with neglected heterogeneity. The omission of an orthogonal regressor (i.e., uncorrelated with the included explanatory variables) does not affect β OLS estimates in linear models. On the other hand, standard methods’ estimates for binary choice models are not unbiased: the omission of an orthogonal regressor will bias coefficients towards zero (Cramer, 2005; Wooldridge, 2002). This behaviour is related to the fact that arbitrary assumptions on the error variance are necessary to identify regression coefficients. When an orthogonal explanatory variable w is omitted the parameter being estimated is no longer the original β_k , but a smaller value, given by:

$$\beta_k \left(\frac{\sigma_\varepsilon^2}{\sigma_\varepsilon^2 + (\beta_w)^2 \text{Var}(w)} \right)$$

If the variance or the coefficient of the omitted variable change greatly across countries, the amount of the bias would be substantially different, thus comparing the estimated β could be meaningless. Suppose you find $\hat{\beta}_{kA} < \hat{\beta}_{kB}$. Does this relation hold because the effect of the independent variable is stronger in country B, or is it because the coefficient is more heavily underestimated in country A?

Luckily, *ASE* is not affected by independent neglected heterogeneity. Wooldridge (2002, pg. 471) proves the result for the probit model, in the case where the omitted variable has a normal distribution. The behaviour of *ASE* is more difficult to derive analytically for the logit model. Cramer (2005) develops a simple simulation study, and finds that *ASE* is hardly affected also in logit models. No such results are available for *ESA*.

A first intuition behind this result is that, although the estimate of β declines with uncorrelated unobserved heterogeneity, this neglected source of variation – by underestimating the variability among the $\text{Pr}(Y=1|\mathbf{x})$ – pushes them closer to the average value. Since $\text{Pr}(Y=1|\mathbf{x})[1 - \text{Pr}(Y=1|\mathbf{x})]$ reaches its minimum value when $\text{Pr}(Y=1|\mathbf{x})=0.5$, the consequence is that with unobserved heterogeneity $\text{Pr}(Y=1|\mathbf{x})[1 - \text{Pr}(Y=1|\mathbf{x})]$ is overestimated. The two effects compensate each other.

The intuitive explanation provided by Wooldridge (2002) is also insightful. If \mathbf{x} is the observed vector and w is the omitted variable, partial effects of $\text{Pr}(Y=1|\mathbf{x})$ can be employed because they are always the average of the partial effects of the $\text{Pr}(Y=1|\mathbf{x},w)$ over the distribution of w .

5.2 Back to the specific model

We will now turn to how these general issues apply to the specific model represented by equations (1)-(4). First notice that omitted previous ability y_1 is not uncorrelated with social background *SB*. Nevertheless, because of the particular structure imposed by equation (1), we can still refer to the framework described above. The relevant assumption is here given by the independence of initial ability y_0 and social background *SB*.

The omission of previous ability y_1 implies that, instead of dealing with the original full equation (2c), we estimate the reduced equation (5). Thus, the coefficient of *SB* turns into $(\lambda\gamma + \xi)$, the *total* effect of *SB* on school choice, and the error term (see equation 4) is given by $(\lambda\beta y_{0i} + \lambda u_i + \varepsilon_i)$, it is therefore larger than the original error and still independent from *SB*.

With a linear model the omission of y_1 does not affect the estimate of $(\lambda\gamma + \xi)$. On the other hand, the estimates for binary choice models are no longer unbiased: neglected heterogeneity will bias coefficients towards zero. The total effect of *SB* on school choices is therefore underestimated. Moreover, since the error term in equation (4) depends on λ , i.e. on the way previous ability affects school choices, if in some countries individual ability plays a greater role in shaping school choices than in others, these countries will suffer from a *larger relative bias*. In this light, the direct comparison of $(\lambda\gamma + \xi)$ across countries could lead to ambiguous results.

Since the simulation exercise developed by Cramer (2005) is based on a very simple model, we have carried out an extensive simulation study based on model (1)-(4), in order to test the behaviour of *ASE* and provide a guidance for the magnitude of the bias of regression

coefficients in the specific context under study. The results, largely confirming that *ASE* adequately captures the total effect of *SB* on school choices, are reported in the Appendix. The same simulations were also carried out for *ESA*, which instead appears to be sensitive to neglected heterogeneity. Thus, *ASE* only will be shown in the empirical results.

The simulations also have the purpose of showing that employing the PISA scores as a proxy for ability at the time of the choice is not a sound strategy. Referring to a time well after that of school choice (one year later for Italy, 5 years later for Germany), the score is endogenous. With this strategy both the direct effect of social background on school track ξ and the ability coefficient can be severely biased.

6. The empirical analysis

The *reduced* model for school track choice *ST* basically defined in (5) was estimated for the three countries examined.

6.1 Description of the variables

The dependent variable *ST* is dichotomous and distinguishes *Liceo* for Italy¹⁵, *Gymnasium* for Germany and *VWO* for the Netherlands from all other school types.

The social background *SB* is here represented by the **ESCS Index of economic, social and cultural status** as defined by PISA analysts. *ESCS* is a second level index provided by PISA based on three first level indexes, regarding parent's professional status, their educational background and household possessions related to culture (e.g. books) and technology (e.g. PCs). The score is given by the first principal component obtained by the analysis of the three lower level indexes and standardized with respect to OECD average.

Although the main interest lies in studying the *SB* total effect on track choice, a set of control dummy variables was added, with some country-specific items for Germany and Italy:

FEMALE	<i>female</i>
FOREIGN	<i>non-native or first-generation students</i>
FAMDUM	<i>standard family: father and mother living together with student</i>
AREA	<i>(Italy only): geographical area (North West, North East, Center, South and Isles)</i>
EAST	<i>(Germany only): former Eastern German Democratic Republic state¹⁶</i>
RIGID	<i>(Germany only): states with specific rules for transition from primary to secondary school restricting access to Gymnasium and Realschule¹⁷</i>

15 In the PISA dataset for Italy *licei* are not distinguished from *istituti magistrali*, now called *socio-pedagogic lyceum*. Because of the specific features of this school type, they have been separated by identifying as *socio-pedagogic-lyceums* schools with over 80% female students (this is the value for which the survey share of students in each type of school is closest to the proportion reported by the official statistics at a national level). *Istituti magistrali* are not considered lyceums in the analysis.

16 Following Woessmann (2007) the state of Berlin has been classified as an Eastern German Länder.

17 In the PISA 2003 dataset the German Länder are not explicitly identified. We have sent a request for this information to the IQB (Institute for Educational Progress, Berlin). In the meantime, a first identification is possible because the states have been used as a stratification variable (OECD 2005), with 18 strata included in the data file (variable STRATUM). Excluding the two of them which refer to special education and vocational education, the remaining 16 strata have been attributed to the 16 German states as follows. The German states have been ordered by the number of students as reported in official statistics, and then compared to strata size. A cross tabulation of the original variables STRATUM and PROGN has been analysed in order to see the combination of different school types existing in each stratum and the percentage

Two additional control variables, defined for Germany only and capturing part of the complexity of the schooling system in that country, were tested, but their effect was not significant:

- the indicator of states with only two main tracks (not counting comprehensive school), combining *Hauptschule* and *Realschule* together;
- the indicator of states with comprehensive schools available for students.

While of course these institutional characteristics do influence track choices by modifying the choice set itself, when tracking is studied with respect to the simple alternative between lyceums-type school and all others, these differences are no longer relevant.

The other area where availability of further data would be obviously useful is that of student's ability assessment. Two items recorded by PISA 2003 have some relevance here, but for some reasons were not included in the model:

- A rough proxy of pre-track ability is provided by grade repetitions. As a rule, we should also expect *SB* to be negatively correlated with it, causing its coefficient to decrease when this variable is included. However, in Italy grade repetitions up to lower secondary school are quite uncommon (around 1,7% in the sample): the variable indicates the presence of extreme situations, and thus does not capture a consistent amount of variation in previous scores. So, the coefficient of *SB* will change only slightly when this variable is introduced. Even if in Germany and the Netherlands grade repetitions are less rare, the variable is clearly an unsuitable measure of ability, as it has no sensibility at all for all situations except the really critical cases.
- The math mark in the last school report is also requested in the student's questionnaire. However this information, besides being measured on a scale that can differ even between single schools, is not really pre-track, especially for countries with early age tracking. Therefore it is generally only slightly less endogenous than PISA score.

6.2 Sampling scheme and estimation methods

As common in educational surveys, PISA uses a two stage sampling procedure, where schools are the primary sampling units, chosen with probability proportional to their size (in students). Within each selected school, 35 fifteen years old students are randomly chosen. Appropriately weighting the students selected from each school, each student would have the same selection probability. However, weights still need to be adjusted due to the oversampling of some population strata, school and student non-response and inaccuracies in the school size records.

The Italian sample covers 11.639 students, including oversampling in some regions¹⁸ following specific requests of local educational authorities. The available German sample is smaller (at 4.660 students) when compared with population size, since all units belonging to local oversampling schemes were not included in it. Finally, Dutch schools are represented by 3.992 units.

The two-stage sampling procedure affects the distribution of sampling estimates. Standard inference - ignoring the correlation among observations - underestimates standard errors, causing significance tests to reject the null hypothesis of single coefficients being equal to 0 much too often. Standard errors can be consistently estimated with resampling methods: PISA

of students enrolled to them. Combining this information with the figures of the PISA-E 2003 report (Prenzel et al. 2005), strata and states can be reasonably put together. The relatively small sample size (4660 students in Germany) does not allow comparisons between single states. But a division in two groups, for example by the binary variables EAST or RIGID, is possible.

¹⁸ Among them Piedmont, Tuscany, Veneto, Trentino-Alto Adige.

analysts suggest using the modified Balanced Repeated Replication (BRR) approach proposed by Fay. BRR is derived from the well-known Jackknife method but uses a more complex scheme of sample unit removing and re-weighting; further stability to the procedure is added by Fay modification, that avoids the complete removal of any sample unit and builds each replication only using weights.

In the present work estimation is also based on Fay's method; to achieve optimal performance and extend the approach to additional statistical techniques, however, relevant SPSS code made available by PISA was updated and further developed.

6.3 Results from PISA

Parameter estimates for the *ST* model on each country are given in Table 4, 5 and 6 respectively for Italy, Germany and the Netherlands, together with ASE_{ESCS} values and Wald tests for the hypothesis of global model significance.

The models for Italy and Germany also contain an interaction term. In the German model the negative interaction between ESCS and RIGID suggests that the influence of social origins on school choice is weaker in the states where restrictions to track choice are at work. In this case the ASE_{ESCS} is actually replaced by two values: ASE_{ESCS} , estimated on data for “nonrigid” states only, and $ASE_{ESCS+ESCS*RIGID}$, estimated on data for “rigid” states.

While parameter estimates can be severely biased as a consequence of the omission of y_1 on the right-hand side of the model, (as confirmed by the simulation studies performed for this study, see Appendix), ASE values are not. It is then convenient to better evaluate the distribution of these statistics, based on the Fay-BRR replication method.

Table 4. Estimation results. Modelling *ST* for Italy with and without *ESCS-FEMALE* interaction

	β (σ_β)	e^β	<i>P-value</i> ¹⁹	β (σ_β)	e^β	<i>P-value</i>
Constant	-1,531 (0,151)	0,216	0,000	-1,47 (0,145)	0,230	0,000
ESCS	1,304 (0,078)	3,685	0,000	1,118 (0,075)	3,058	0,000
FEMALE	0,551 (0,134)	1,734	0,000	0,473 (0,135)	1,604	0,001
ESCS * FEMALE	-0,322 (0,114)	0,724	0,005			
FOREIGN	-0,682 (0,324)	0,506	0,035	-0,691 (0,324)	0,501	0,033
FAMDUM	0,219 (0,101)	1,245	0,029	0,224 (0,101)	1,252	0,026
NorthEast	-0,42 (0,211)	0,657	0,047	-0,423 (0,218)	0,655	0,052
Center	0,328 (0,14)	1,389	0,019	0,322 (0,139)	1,381	0,020
South & Isles	0,344 (0,229)	1,411	0,132	0,348 (0,229)	1,416	0,129
ASE_{ESCS} (male) = 0,1947 ASE_{ESCS} (female) = 0,1824		Wald test (complete model) = 480,42		ASE_{ESCS} = 0,1884		Wald test (complete model) = 389,1

19 Based on the hypothesis of approximated normality of parameter estimates.

Table 5. Estimation results. Modelling ST for Germany with and without FOREIGN dummy

	β (σ_β)	e^β	<i>P</i> -value	β (σ_β)	e^β	<i>P</i> -value
Constant	-1.756 (0.167)	0.1727	0.0000	-1.794 (0.167)	0.1663	0.0000
ESCS	1.413 (0.088)	4.1079	0.0000	1.431 (0.087)	4.1827	0.0000
ESCS*RIGID	-0.193 (0.133)	0.8247	0.1481	-0.249 (0.13)	0.7793	0.0557
FEMALE	0.477 (0.104)	1.6119	0.0000	0.469 (0.102)	1.5988	0.0000
FOREIGN	-0.151 (0.204)	0.8596	0.4582			
FAMDUM	0.17 (0.11)	1.1854	0.1220	0.188 (0.109)	1.2074	0.0843
EASTDUM	0.351 (0.246)	1.4202	0.1541	0.369 (0.246)	1.4468	0.1325
RIGID	0.104 (0.183)	1.1098	0.5691	0.137 (0.182)	1.1467	0.4530
<i>ASE</i> _{ESCS} (RIGID) =0.2143 <i>ASE</i> _{ESCS} (NO rigid)=0.2282		<i>Wald test</i> (complete model)=456.54		<i>ASE</i> _{ESCS} (RIGID) =0.2091 <i>ASE</i> _{ESCS} (NO rigid)=0.228		<i>Wald test</i> (compl.model) =441.76

Table 6. Estimation results. Modelling ST for Netherlands

	β (σ_β)	e^β	<i>P</i> -value
Constant	-2.404 (0.141)	0.090	0.000
ESCS	1.176 (0.086)	3.243	0.000
FEMALE	0.191 (0.104)	1.211	0.066
FOREIGN	-0.004 (0.23)	0.996	0.987
FAMDUM	0.715 (0.125)	2.045	0.000
<i>ASE</i> _{ESCS} =0.1656		<i>Wald test</i> (complete model)=223.00	

Figure 3 illustrates the approximate sampling distribution of ASE_{ESCS} (and $ASE_{ESCS+ESCS*RIGID}$) for each country²⁰. These distributions display a very limited amount of overlapping, suggesting that differences are significant; the least total *SB* effect is in the Netherlands, followed by Italy and then Germany (with the “rigid” states first).

Access restrictions seem to weaken inequality of opportunity in track enrolment. Two considerations are to be made. First, this result does not arise from a well defined impact evaluation design, thus it should be validated with further studies²¹. Second, the result applies for Germany, and should not be generalised to other countries, as it is likely that this policy

20 Since only 80 replicated observations are available for each density estimation, the histogram shows very rough B-spline smoothing approximations.

21 The study of the effect of specific institutional features on performance scores carried out by Woessman (2007) – based on a regression model where the slope of the socio-economic gradient of each state is related to the features of the educational system – does not include access restrictions among the characteristics under investigation.

will interact with other features of the educational system, potentially giving rise to different effects in different contexts.

Note that the associated parameter estimates shown in Tables 4-6 display an ordering which is inconsistent with that of *ASE*: by comparing Italy and the Netherlands, the *ASE* is larger for Italy, whereas the *ESCS* coefficient is larger for the Netherlands. As pointed out in section 5.1, logit regression parameters may have an ambiguous meaning with unobserved heterogeneity.

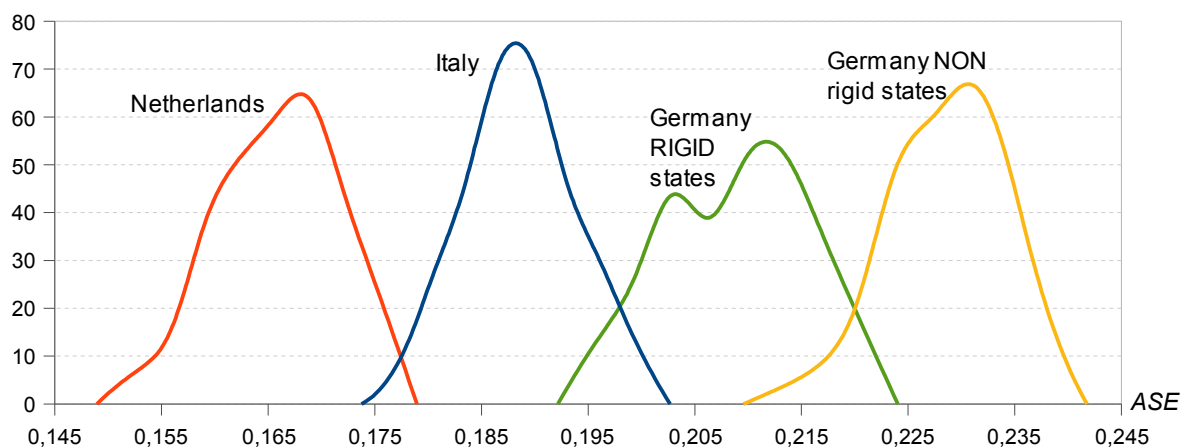


Figure 3. *SB* total effect *ASE* - estimated distribution (via Fay-BRR) in each country

6.4 Results from PIRLS

PIRLS stands for *Progress in International reading literacy study*, an assessment of students' reading achievement at fourth grade conducted every five years by IEA - the *International Association for the Evaluation of Educational Achievement*²². 2001 PIRLS data are analyzed here.

While PIRLS survey data are not the main focus of interest here, they can help to assess y_1 , since PIRLS performance scores²³ are pre-track for all countries, while being reasonably closer to the tracking time than to pupils' time of entry into the schooling system.

It is thus possible to obtain a rough estimate of γ by modeling PIRLS test scores on some item pertaining to the family socio-economic background recorded in the same survey.

Since information in this area is more limited in PIRLS than in PISA, defining a composite *SB* index similar to *ESCS* proves difficult here. The variable of choice was then the *highest educational level attained by parents*, measured with 2 dummies for secondary and tertiary as opposed to primary educational level²⁴.

The PIRLS survey has sampling features analogous to the PISA context, and model estimation on it was performed using a similar replication-based approach.

Evidenced offered by Table 7 suggests a stronger *SB* effect on y_1 in Germany, even when

22 *PIRLS 2001 International Report: IEA's Study of reading literacy achievement in primary schools*, Mullis et al., 2003, Boston College

23 Taken here as a mean of the results obtained on a set of different reading and comprehension test items.

24 The choice is motivated by the closeness with *ESCS* the latter shows on PISA data. If the highest parental educational status is measured, somewhat improperly, on a simple integer scale (1 primary, 2 secondary, 3 tertiary), correlation is high ($\rho > 0,8$), and a linear regression of *ESCS* on this item gives a coefficient which is very close to 1.

taking into account the presence of non natives (and offspring of non native parents). Coefficients for Italy and the Netherlands are instead lower and quite close, with a slight prevalence for Italy when higher education is involved.

Table 7. *Parameter estimates for linear regression models for PIRLS 2001 scores*

	ITALY ^a		GERMANY		NETHERLANDS	
Constant	515.0 (3.47)	516.6 (3.53)	489.6 (6.31)	506.3 (5.86)	537.5 (3.45)	541.7 (3.31)
Female	7.25 (2.59)	7.42 (2.58)	11.38 (3.16)	10.54 (3.06)	11.83 (3.08)	11.19 (3.20)
Parent. higher educ. Secondary	31.07 (3.54)	30.50 (3.57)	49.73 (6.09)	41.13 (5.67)	33.03 (3.17)	31.18 (3.20)
Parent. higher educ. Tertiary	50.84 (4.08)	51.25 (4.19)	81.02 (6.34)	70.72 (6.05)	47.60 (5.00)	46.55 (5.00)
Native from foreign parents		-35.67 (11.52)		-25.92 (4.92)		-47.04 (9.75)
Born abroad from foreign parents		-41.02 (8.59)		-55.05 (5.18)		-36.86 (8.89)
F test <i>p-value</i>	0.000	0.000	0.000	0.000	0.000	0.000

Robust standard errors in parenthesis. All the coefficients are highly statistically significant.

a. Geographical area (North West, North East, Centre, South and Isles) is also taken under control

7. Conclusions

Summarizing the results of our work, we find that the *total* effect of social background on the choice of the academic track is weaker in the Netherlands and stronger in Germany, with Italy somewhere in between. Moreover, by exploiting the institutional variability across the German states with respect to access restrictions into the different tracks, we provide an assessment of the impact of admission rules on the parental background effect. Access restrictions seem to reduce the effect of social origin on school choices.

The overall effect appears to be stronger in Germany than in Italy (where access is free) and the Netherlands (where access is also regulated) even in those Länder where these restrictions apply, suggesting that these rules alone do not necessarily counterbalance the negative effects on equality of opportunity due to other features of the school design.

We may try to gather the empirical evidence from PIRLS and PISA in order to make some conjectures on the size of primary and secondary effects of social background. The line of reasoning is summarized in Table 8.

Table 8 should be interpreted as follows. The terms “low”, “medium” and “high” are not to be interpreted as absolute assessments, they are just rough comparative evaluations within the set of countries under study. Column (1) and (5) refer to the “strong” empirical evidence described in the previous sections. In column (1) we evaluate primary effects as resulting from the analyses of PIRLS data, while column (5) refers to the *ASE* of *ESCS*, estimated in Section 6.2. The content of the other columns is instead largely speculative: the question marks next to the attributes are meant to stress this point.

Start from column (3). What can we say about the relation between ability and school type? We assume that, given the existence of formal assessments of ability, λ should be large in the German-rigid case and for the Netherlands (as suggested also by the descriptive evidence reported in Section 3). Since we have little direct information for Italy and German-

not rigid states, although these effects are likely to be lower here, we leave the corresponding cells empty.

Now move to column (4). Given γ and λ we can also assess the indirect effect. The indirect effect should be medium or high for the Netherlands and high for German-rigid states. The last step is column (2). The direct effect is assessed as the “difference” between the total effect and the indirect effect. According to this speculative reasoning, we should conclude that secondary effects are likely to be very low in the Dutch system and higher in the German-rigid states. Both states have an early-tracking system and restrictions on secondary school choice.

Table 8. *A speculative assessment of the different components of the social background effect*

	(1)	(2)	(3)	(4)	(5)
	γ PRIMARY EFFECT	ξ SECONDARY EFFECT (DIRECT)	λ ABILITY \rightarrow <i>ST</i>	$\gamma\lambda$ INDIRECT EFFECT	$\gamma\lambda + \xi$ TOTAL CAUSAL EFFECT
ITALY	medium	?	?	?	medium
NETHERLAND	medium	very low?	high?	medium? high?	low
GERMANY-RIGID	high	medium?	high?	high?	medium-high
GERMANY-NOT RIGID	high	?	?	?	high

If we wish to move from conjectures to more reliable conclusions longitudinal data is called for. Following Breen *et al* (2005, pg. 7) “.. primary and secondary effects are impossible to disentangle without adequate longitudinal data. Studies like those of PISA would urgently need a longitudinal design to improve our understanding of cross-national differences in primary and secondary effects of social class.”.

Appendix - The simulation study

The aim of the simulation study is threefold:

- to investigate the downward bias in parameter estimation in the logistic regression model for track choice when ability at the moment of choice is altogether omitted;
- to assess *ASE*, rather than *ESA*, as the main tool to interpret the effects of explanatory variables in the above context;
- to evaluate the consequences of including PISA performance scores as a proxy of students' unobserved ability at the moment of school choice.

Given the complexity of the context and aiming at mimicking as closely as possible the structure of the actual choice mechanisms, the simulation environment was built with direct reference to real data²⁵ whenever feasible. Simulated data included track choice outcome *ST*, ability at the moment of choice y_1 , PISA scores y_2 , and ESCS levels *SB* as defined in section 3.

A.1 - The simulation model

For each simulated context, 35 independent samples of size $n = 1000$ are created.

The track choice outcome *ST* is derived from a linear utility model where expected utility U_{ji} of choosing track j for unit i depends on *SB* and on y_1 :

$$U_{0i} = \mu_0 + \lambda_0 SB_i + \xi_0 y_{1i} + \varepsilon_{0i};$$

$$U_{1i} = \mu_1 + \lambda_1 SB_i + \xi_1 y_{1i} + \varepsilon_{1i};$$

$$ST_i = \begin{cases} 1 & \text{if } U_{1i} > U_{0i} \\ 0 & \text{if } U_{1i} \leq U_{0i} \end{cases}$$

Error terms ε_{0i} and ε_{1i} are independent and both *extreme-value* distributed.

Parameters of (2a) are thus derived as $\mu = \mu_1 - \mu_0$; $\lambda = \lambda_1 - \lambda_0$; $\xi = \xi_1 - \xi_0$. For any set $(\lambda; \xi)$, μ is adjusted to get a $\Pr(ST_i = 1)$ reasonably close to the marginal observed probability.

ESCS is obtained from a normal distribution with the observed mean and variance (it is an internationally standardized index, but its values can differ somewhat at the national level). This is a slight simplification, since the real distribution is a bit differently skewed for Italy and Germany. Moreover, ESCS is strictly never larger than 2.4; the simulated normal distributions were thus similarly truncated.

Ability y_1 is unobserved on a per-unit basis, but its distribution can be approximated using scores obtained in PIRLS tests at age 9 (pre-track for both countries). The observed distribution of such scores is apparently normal, and the scores are measured on a scale similar to that used in PISA.

y_1 depends also on parental socio-economic status: to take this into account an estimate of γ as in (1) is obtained from a linear regression on highest parental educational status estimated for PIRLS test scores. This value is then employed for generating y_1 , assuming that it represents the true coefficient of the continuous measure of social background ESCS level, which more fully describes socio-economic status in PISA and is closely related to parental educational status (see note 8).

Finally, a generic PISA score y_2 is obtained based on its observed distribution (also close

25 With reference to PISA and PIRLS data for the Italian and German cases.

to normal) first two moments, while being linearly related to SB , y_1 and ST , as in (3). Values of α' , γ' and δ' are based on the estimation of model (3) on actual data omitting unobserved y_1 , but γ' and δ' are reasonably deflated to compensate for the omission. β' is instead assumed = 0,5, so that a Δy_1 increase in previous ability implies half the increase on PISA score.

On each of the 35 simulated samples, logistic regression models for ST are then estimated with the following explanatory variables:

- both y_1 and SB (*full model*), to check how simulated samples mirror the simulation scheme;
- SB only (*reduced model*), to study how this affects estimation of the total effect $\xi + \lambda\gamma$ of socio-economic family status on track choice;
- SB and y_2 , to assess how (and how much) the inclusion of PISA scores can cause an incorrect evaluation of the above total effect.

Besides parameter estimates, ASE and ESA associated to $\xi + \lambda\gamma$ are computed for every model both in the *full* and the *reduced* case, to check their invariance in the present situation (more complex than that addressed by Cramer, 2005) and better understand their behaviour and its relation to parameter estimates.

This framework is applied to different combinations of ξ and λ , both impossible to estimate directly, considering also some differing values of γ , since its value reflects the correlation between the observed and the omitted variable in the reduced model.

A2. Parameters estimation bias and ASE

The tendency to show downward bias in parameter estimates in Logit models with omitted variables (even if uncorrelated with included regressors, as shown in Cramer, 2005) is indeed striking, making the former often unsuitable to evaluate the effect of observed variables on the examined probability.

Table 4 shows this through the results for the reduced model applied to different sets of simulated data, all producing estimates of $\xi + \lambda\gamma$ that are similar *and* very close to estimated value for Italy (approximately 1.12), while having a true underlying $\xi + \lambda\gamma$ that can strongly differ.

In the table $E(\widehat{\xi + \lambda\gamma})$ and $\sigma(\widehat{\xi + \lambda\gamma})$ are respectively the *mean* and *standard error* of the estimates of $\xi + \lambda\gamma$ on the 35 simulated samples based on the same set of parameters.

In all cases in the higher half of the table $E(\widehat{\xi + \lambda\gamma})$ is similar to 1.128, but the set values of $\xi + \lambda\gamma$ required to obtain such estimate are close to 1.128 only if λ is near zero, i.e. in unlikely contexts where student's ability at the moment of track choice is irrelevant.

The downward bias is persistent also when the omitted variable y_1 is uncorrelated with the observed one ST ($\gamma=0$), as in the lower part of Table 4. In this context primary effects are not at play; here $\xi + \lambda\gamma$ has been fixed to 1.128 but $E(\widehat{\xi + \lambda\gamma})$ moves further away from it as secondary effects strengthen (λ increases).

Both alternative measures ASE and ESA were then studied on the same sets of simulations. However, whereas ESA exhibits unpredictable variation when passing from the full to the reduced model, ASE displays almost complete invariance for all parameter combinations. Remarkably, this result holds also when the omitted variable y_1 is far from uncorrelated to SB , as in the higher half of Table 4, which shows ASE values associated with the total effect $\xi + \lambda\gamma$ for the estimated full models and - as in Cramer (2005) - the ratio of ASE values in the reduced w.r. to the full models (equivalent ratios for ESA are also reported, showing significant instability). As before, mean values over the 35 simulated samples are presented, together with their standard errors.

Such results strongly suggest that the use of *ASE*, as opposed to regression coefficients, can give clearer and more reliable indications on the data structure examined with logistic regressions.

Table 9. *Estimated, true total effect parameters, ASE, ASE and ESA ratios for reduced models*

λ	ξ	γ	$\xi+\lambda\gamma$	$E(\widehat{\xi+\lambda\gamma})$	$\sigma(\widehat{\xi+\lambda\gamma})$	$E(ASE_{ESCS}^F)$	$\sigma(ASE_{ESCS}^F)$	$E\left(\frac{ASE_{ESCS}^R}{ASE_{ESCS}^F}\right)$	$\sigma\left(\frac{ASE_{ESCS}^R}{ASE_{ESCS}^F}\right)$	$E\left(\frac{ESA_{ESCS}^R}{ESA_{ESCS}^F}\right)$
0.064	1.400	25	3.000	1.188	0.084	0.198	0.011	1.017	0.068	1.195
0.044	1.000	25	2.000	1.092	0.072	0.197	0.018	0.989	0.068	0.909
0.034	0.950	25	1.650	1.102	0.071	0.175	0.008	0.998	0.048	0.945
0.024	0.900	25	1.500	1.084	0.091	0.197	0.010	0.988	0.036	0.833
0.014	0.900	25	1.400	1.123	0.093	0.198	0.010	1.002	0.028	0.960
0.004	1.050	25	1.200	1.130	0.077	0.202	0.013	1.002	0.008	1.008
0.014	1.128	0	1.128	0.962	0.088	0.176	0.013	0.999	0.025	0.950
0.024	1.128	0	1.128	0.783	0.087	0.156	0.009	0.977	0.049	0.805
0.034	1.128	0	1.128	0.666	0.071	0.124	0.010	0.997	0.069	0.908
0.044	1.128	0	1.128	0.538	0.068	0.106	0.011	1.008	0.085	0.872
0.064	1.128	0	1.128	0.436	0.074	0.086	0.008	1.018	0.131	1.162
0.084	1.128	0	1.128	0.312	0.065	0.064	0.010	1.046	0.179	1.162

A3. Endogeneity of Pisa scores and its effects

Evaluation of the consequences of including PISA score y_2 in the model for *ST* is complex since they depend markedly on other parameters values. No attempt to reach systematic conclusions is made here, focusing instead on just presenting some examples where this can lead to inappropriate results in different directions.

In this way it is easy to simulate cases where parameters estimates for *SB* and y_2 (used as an observed proxy for y_1) in the logistic model:

$$\ln \left[\frac{\Pr(ST_i = 1)}{1 - \Pr(ST_i = 1)} \right] = \mu + \lambda y_{2i} + \xi SB_i$$

are both downward biased, both upward biased or else biased in opposite directions.

In some cases the estimate of ξ can even be negative, as in third column. It is interesting to notice that even in cases where δ' is smaller, the track choice *ST* not having a large influence on y_2 (for example where the timespan between PISA survey and track choice is small), results can be definitely misleading.

Table 10. Different bias configurations when using PISA scores y_2 in logistic regression models for ST

		$\hat{\xi}, \hat{\lambda}$ both biased downward	$\hat{\xi}, \hat{\lambda}$ both biased upward	$\hat{\xi}$ biased downward $\hat{\lambda}$ biased upward	$\hat{\lambda}$ biased downward $\hat{\xi}$ biased upward
Simulated model parameters	ξ	0,399	0,764	0,764	0,028
	λ	0,028	0,028	0,028	0,044
	γ	37	25	25	25
	β'	0,5	0,5	0,5	0,5
	γ'	23	0	50	20
	δ'	82	200	200	45
Estimates	$E(\hat{\xi})$	0,355	0,843	-0,846	0,260
	$\sigma(\hat{\xi})$	0,108	0,173	0,222	0,074
	$E(\hat{\lambda})$	0,020	0,034	0,034	0,013
	$\sigma(\hat{\lambda})$	0,001	0,003	0,003	0,001

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