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Persistent Classmates: How Familiarity with Peers Protects from Disruptive School Transitions*

Son Thierry Ly†　　Arnaud Riegert‡

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Abstract

Students' social networks are deeply disrupted during school transitions and students start in a classroom environment where almost all their peers are new. In this study, we investigate the consequences of keeping partly the same classmates during the transition to high school. To overcome the issue of endogenous selection across classes, we exploit rare natural experiment settings in which students are plausibly randomly allocated to classes within high schools. Two estimation strategies are presented and provide the same results. We find that each classmate who was already in a student's class in the last grade of middle school reduces substantially the risk of grade retention in 10th grade, but also in following grades. For low-ability students, the effect amounts to minus 1 percentage point per "persistent classmate", without increasing the risk of dropping out. A number of robustness checks are provided. By analyzing the distribution of the effect, we show that it is the strongest for students who are the most likely to experience a difficult transition, *i.e.* low-ability, low-SES students from low-quality middle schools. The underlying mechanisms are examined. Our results suggest that grouping students who already know each other during school transitions would constitute an efficient, no-cost policy lever to improve overall achievement and equality in high schools.

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1 Introduction

In most countries, school transitions yield great discontinuities in peer groups, since schoolmates do not necessarily enroll in the same middle or high school. As suggested by many scholars (Wentzel, 1998; Sacerdote, 2001; Crosnoe *et al.*, 2003; Cullen *et al.*, 2006; Nelson & DeBacker, 2008; Véronneau *et al.*, 2008; Lavy & Sand, 2012), this strong disruption in students' social networks may partly explain the decline in educational achievement observed in the literature in the beginning of middle or high school (Rockoff & Lockwood, 2010; Schwerdt & West, 2013). In particular, Lavy & Sand (2012) present some evidence of the detrimental effect of losing reciprocal friends during the transition from elementary to middle school. This effect could be even more dramatic during the following transition from middle to high school: as it marks the end of mandatory schooling in many countries, difficult transitions could result in substantial dropout rates[1].

However, previous papers have overlooked the fact that the disruption goes much beyond the loss of friends: school transitions imply that students may arrive at a new school in which *they basically know nobody or almost*, since most high school mates come from different middle schools and classrooms. In France for example, only 5 % (resp. 20 %) of 10th grade classmates are former classmates[2] (resp. middle school mates), as shown in Figure I, which means that French students start HS in a classroom with 80 to 95 % classmates who are complete strangers[3]. This huge discontinuity in the peer group is very specific to school transitions: as a comparison, around 30 % of a students' classmates were persisting from one year to another during the middle school years (more in section 2.3.2). Contrary to friends, we know nothing about the importance of this more broadly defined group of familiar peers and about the consequences of unfamiliarity with new peers during school transitions.

The consequences of getting (almost) only new peers as they arrive in new schools on students are yet unclear. On the one hand, since former classmates may be friends, keeping more former classmates may simply increase the probability to keep friends, which has been shown to yield positive effects on students' well-being and achievement (Lavy & Sand, 2012). But *even if they were not friends ex ante*, former classmates are peers to whom it may be easier to talk to during the first weeks, to sit next to in the classroom or to ask for help and cooperation, thus making it easier to adapt to higher academic expectations from teachers. Even without bonds of friendship, familiarity within the classroom could therefore yield reduced anxiety, prevent social isolation and

[1] Besides, note that many papers indicate that social support from peers might be even more important in high school than in middle school (Hertzog *et al.*, 1996; Zeedyk *et al.*, 2003).

[2] 10th grade is the first grade of high school; we call "former classmates" only those students who were in the same class *the previous year*, *i.e.* in 9th grade in this case.

[3] Throughout this paper, we presume that students necessarily know their former classmates, may or may not know school mates who were in other classes the previous year, and do not know students coming from other middle schools.

foster students' sense of belonging in their new school and classroom. On the other hand, former classmates could prevent students to socialize with new peers, or favor bad behavior in the classroom if formerly disruptive students stay together. Besides, former classmates are not necessarily (potential) reciprocal friends. They could even be the exact opposite, *i.e.* enemies or "non-reciprocal" friends (peers that consider you as a friend while you do not, or the other way around), thus producing negative effects on students as suggested once again by Lavy & Sand (2012)[4]. Overall, the consequences of peer persistency remain an open question that has to be settled empirically, so as to provide clear guidelines on whether we should group students who already know each other, or separate them.

In the present study, we investigate this issue by comparing students *coming from the same middle school and 9th grade class, arriving at the same high school* but *assigned to different classes* where they end up with more or less of their former classmates. We exploit a natural setting that allows us to overcome endogenous selection across classes. When students enter high school in 10th grade, principals, who are in charge of assigning them to classes, do not know them yet. They have thus no other information than a finite set of formal information they may observe on the students' registration files – unlike in other grades where they may know some of the students personally. Thanks to an exhaustive, administrative dataset of the French Ministry for Education, we are able to observe all the main variables used by principals to allocate students across classes. We can therefore limit ourselves to the rare natural experiment setting during which a principal has to *allocate in different classes two (or more) students who share the same observable characteristics* (named "similar-file" or SF students throughout the paper): for instance, they chose the same courses and share the same gender, age, socioeconomic status and 9th grade scores. They may come from the same 9th grade class (SF-S students) or from distinct but similar classes within the same middle school (SF-D students). Since the principal has no further significant information to distinguish these SF students, we argue that in this case, they will decide randomly whether they assign the student 1 to class X and the student 2 to class Y, or the other way around. More formally, we use the fact that the assignment to classes in 10th grade can be credibly assumed random conditional to this whole set of personal, observable characteristics that is observed by both the principal and the econometrician. Strong empirical evidence to support this assumption is provided.

In order to estimate the causal effect of former classmates, we develop two empirical strategies that exploit this conditional random assignment of SF students. In the first strategy, we use the fact that SF-S students

[4]In the same spirit, Mora & Oreopoulos (2011) show that non-reciprocal friends seem to have no effect on dropout intentions in high school.

themselves are randomly assigned to classrooms that differ in several characteristics, including the number of their former classmates that have been assigned therein. By comparing students only with their SF-S mate(s), we are thus able to identify the causal effect of virtually any measurable classroom characteristic we might be interested in, as long as its variance is large enough between SF-S students' 10th grade classes. In particular, this allows us to identify the causal effect of the number of "persistent classmates" (PC), *i.e.* the number of former 9th grade classmates that a SF-S student gets in his 10th grade classroom, controlling precisely for other classroom characteristics.

This first estimation strategy shows a strikingly positive effect of persistent classmates on academic achievement during high school. Each additional persistent classmate that a SF-S student has in her 10th grade class reduces her risk of repeating 10th grade by about one percentage point and yields higher scores at the *Baccalauréat* examination (end of 12th grade). Many robustness checks are provided: we demonstrate in particular that the result is very stable across cohorts and robust to specification choices. Furthermore, we find that persistent classmates do not have an effect on all kinds of students. The estimates are very large and statistically significant only for students who may face the most difficulties during the transition to high school, *i.e* low-SES, low-ability students whose high school is attended by many more high-SES students than their middle school was. These results indicate that the historical familiarity with classmates protects precisely those students experiencing a strong social and cultural shock in their environment during the transition to high school. Persistent classmates also have a slightly higher securing effect on boys than on girls.

The estimated effect of persistent classmates is basically unchanged by controlling for the effect of other classroom characteristics (such as classmates' average ability of female share) that may differ between SF-S students' classrooms, which shows that this particular characteristic is likely to be independent of other classroom characteristics that matter for outcomes. Yet, the issue remains that the estimates could still reflect omitted classroom variables that we are not able to account for and that might be correlated with the number of persistent classmates (*e.g.* teacher quality or any classmates' unobserved characteristics). To tackle this issue, we develop a second estimation strategy that looks at similar-file students who come from distinct but very similar 9th grade classes instead of the same class (SF-D students). Once again, we provide convincing evidence that SF-D students could also be considered undistinguished by HS principals during the allocation to 10th grade classes. Since they do not come from the same 9th grade class, the result of their random allocation to one of two 10th grade classes can be used as an instrumental variable that provokes an exogenous change in the number of persistent classmates *of the students from these two 10th grade classes who come from the same*

9th grade classes as the SF-D students. Instead of estimating the causal effect directly on SF-D students, this second strategy thus derives it from the indirect impact of SF-D students' random allocation on their former classmates.

Although its method differs significantly from the first strategy and is based on another pool of students, this second estimation strategy yields very similar estimates, which is another proof of the consistency of our estimates and their external validity. This also has two main implications. First, because the SF-D students are similar across all individual characteristics except for their class of origin (including gender, ability or age), the way they are assigned across 10th grade classes only changes the number of persistent classmates of the students that have been allocated in these 10th grade classes, and not any other classroom characteristics. We argue that this rules out the possibility that our estimates reflect omitted classroom characteristics that could be correlated with the number of PC. Secondly, adding a 10th grade class fixed effect in the regression equation allows us to compare only groups of students who are assigned in the same 10th grade class but who do not come from the same 9th grade class, using the fact that only one group of students in the class is affected by the exogenous allocation of a former classmate. The estimated PC effect on academic outcomes remains basically unchanged, showing that potential mechanisms that would work through changes in the global classroom context and that would affect everyone in the classroom (through teachers or the global atmosphere within the classroom) are not credible. Obviously, the positive effect of peer continuity works directly through the students that are grouped (*e.g.* through better cooperation, reduced anxiety or easier socialization)

These results have strong policy implications. Since we find no clear evidence that the effect of the number of persistent classmates might be non-linear (at least within the 0 to 10 PC range studied here), grouping students with their 10th grade classroom may have a tremendous impact on overall achievement and school equality at absolutely no cost. This policy lever might thus be much more interesting than class size reduction, which has been extensively studied but implies very high costs. Second, moving students around to group them by class of origin is not a zero-sum game. If a student is isolated in their 10th grade class, they can be moved to another class where their former classmates are, without impairing the students of their initial class. To give an opposite example, moving girls across classes may not be as efficient, since boys and girls may benefit equally from more girls in the classroom (Lavy & Schlosser, 2011). Finally, there is important room for maneuver using this lever, for we find that principals do not tend to group students by class of origin and scatter these groups across classes.

From a more academic perspective, this paper gives an interesting insight into the more general debate on peer effects and in particular on the issue of which peers do matter. We show that accounting for basic familiarity, without any knowledge on precise relationships between former classmates, is enough to find strong, positive peer effects. This may be surprising, as Lavy & Sand (2012) show that friendships yields very different effects regarding the precise type of bond (reciprocal or non-reciprocal). Following Foster (2006), our results call into question the common idea that agents should be more influenced by their friends than by other peers (see also Halliday & Kwak, 2012).

This work can finally be related to the literature on school mobility and school choice. Policies that enhance school choice or expand students' access to high-performing schools have been unexpectedly inefficient to improve students' educational outcomes, especially for males (see for example Cullen *et al.*, 2006; Kling *et al.*, 2007). This might be a direct consequence of the disruption generated on students' social network and environment by such programs, as suggested by Lavy & Sand (2012). Our findings support this interpretation, as they stress the crucial role of classmates students already known at the beginning of high school.

2 Institutional context and data

2.1 The French high school curriculum

2.1.1 The transition to high school

After elementary school, French students go into middle schools from grade 6 to grade 9. Then at the end of middle school, students decide to apply for vocational or academic studies[5], with the approval of their middle school teachers. In the latter case, students can apply to academic high schools in their district.

Rules of admission then differ with school districts and years[6], but they usually depend upon the students' home address and school performance (9th grade scores). The allocation is over in the end of June and high school administrations hold the registration files of their future 10th graders in the first week of July.

Simultaneously, 9th grade students take national anonymous exams in the end of June in three core subjects: mathematics, French and history-geography. Their results are combined with in-school scores, *i.e.* scores obtained in 9th grade class in all courses, to compute a total score that determines whether they obtain the *Diplôme national du brevet* (DNB hereinafter)[7] or not. The anonymous scores are only available by mid July, after students' registration in their high school, so that the latter's administration does not know these scores[8]. We will rest on these measures of students' ability to provide empirical evidence of the conditionally random assignment of 10th grade students among classes (see section 3).

2.1.2 Tracking in high schools

The 10th grade is known in France as a complicated year for students. At the end of the year, they will have to choose a major that will determine their 11th and 12th grade courses, their Baccalaureate degree, as well as the university studies they will be able to apply for at the end of high school. Students first have to opt for the academic or the technological track, the former being historically more renowned with more academic and difficult courses. Within the academic track, they have to choose between science, humanities and social science

[5] Around 2/3 of 9th grade students enroll in academic high schools. Vocational high schools are excluded from this study because the choice of the major occurs in grade 10 and only in rare cases are there several classes for a given major in a given high school.

[6] In 2009, a major reform has implemented an automatic procedure (*Affelnet*) to allocate students among academic high schools.

[7] Students do not need to obtain the diploma to pursue in high school.

[8] These scores are not even sent to the high school through administration processes. Some students do inform the high school of their results at the anonymous exams after getting them (even if it is not required), but according to informal discussions we had with some high school administrations, this happens only rarely. In any case, principals do not know these scores for all students, so that they are very unlikely to use them for the class allocation process.

for their major[9]. Within the technological track, students major predominantly in industry, administration or healthcare[10].

In the first stage, at the end of 10th grade, students may enroll in a given major only if teachers agree with regard to their scores in the major's main courses (*e.g.* mathematics, physics-chemistry and biology for the science major). If students are not accepted in any of the majors they applied for, and do not want to pursue in an alternative major proposed by teachers, they can choose to repeat 10th grade and try again the next year[11]. Therefore, students' outcomes at the end of 10th grade year depend on students' achievement and preferences. A positive effect of persistent classmates on observed students' outcomes could thus operate through a change in students' preferences rather than better achievement. We will address this issue in this paper by investigating students' outcomes in following grades. Overall, students' enrollment at the end of 10th grade year (choice of major, or grade retention) is shown to provide a relevant outcome measure for our study.

2.2 The class-assignment mechanism

In France, students are assigned to the same class for the entire school year and for all courses. Classmates have therefore even more influence on students' outcomes, as they will spend most of the day together during one full year. Because students are not assigned randomly to classes, econometricians need to overcome the issue of selection on unobservables to retrieve unbiased estimates of peer effects within the classroom. This paper exploits the specific class assignment mechanism for 10th grade students, which is now described in detail.

In practice, the class assignment is made in July, after students have completed their registration to high school, and two months before the beginning of the school year in September. It is completely done by hand, not by any computer algorithm. 10th grade students are not assigned randomly among classes, but contrary to other grades, high school administrations do not know personally the students when they do the allocation, which is the key feature used in this paper for identification. As a consequence, high school principals only use the set of formal information on students that is available in their registration files and observable in our dataset. They first consider specific courses chosen by students. Apart from common-core courses that are identical for all students (*e.g.* mathematics or French), there are indeed some choice-specific courses for which

[9] The scientific major is the most prestigious among them, as it allows the students to enroll in virtually any field of studies after high school, including humanities.

[10] There are actually quite a few other majors, but they represent a very small share of students.

[11] They also have a right of appeal and might thus "force" their enrollment in a major. During the meeting with the student and their family however, the high school principal strongly discourages them to enroll in a major if they did not achieve high enough in 10th grade. Only a few students actually use this right of appeal in the end.

students have more freedom: for example, they have the right to choose the foreign languages they want to study (*e.g.* English or Spanish) and might also take additional optional courses (*e.g.* ancient languages such as Latin or ancient Greek). Students who take the same optional courses are often grouped in the same class, for convenience with regard to classes' timetables (see footnote 15).

Conditional to students' specific courses, school principals generally (but not necessarily) try to equilibrate classes' environments in terms of gender and ability composition[12]. They can rely on formal information contained in students' personal registration file: grades obtained in 9th grade courses (between 0 and 20), 9th grade teachers' comments or personal information on the student and their family (mainly gender, age and parents' occupations). Contrary to other grades for which principals know their students, they cannot take into account personal knowledge about them such as motivation, mental strength or well-being[13].

There are good reasons to think that principals do not use all the detailed formal information they have on students during class assignment. We have been able to witness several sessions of class allocation that revealed the very high complexity of this process for principals. They have to do it by hand and to take into account a multiplicity of constraints, which already takes a lot of time while they have many other tasks to complete to prepare the new school year. Obviously, two 10th grade students do not need to be exactly identical on the paper to be undistinguished during the process: if their files look vaguely similar, principals are not going to spend more time to investigate further their characteristics and find some small detail to distinguish them.

2.3 Data

2.3.1 Data sets

The empirical analysis is based on two administrative datasets from the French Ministry of Education.

Administrative registration records: For all students who are enrolled in French public and publicly-funded

[12] There is no legal requirement to do so, but the 1975 Haby law that implemented middle school comprehensiveness established a tacit rule urging schools to favor class heterogeneity. Besides, principals may not want to group all low-achieving students together in a class that will more likely get out of the teachers' hands.

[13] In these cases, they might for example separate two students who are disturbing classes, or allocate a fragile student with their friends to ensure them emotional support. However, one could argue that this situation might appear even in 10th grade in schools that combine both middle school and high school curriculums in the same place, which represent about 16 % of students in France. For these specific (mostly private) schools, principals might know 10th grade students who come from their own middle school. However, we find that the exogeneity tests (see section 3.1.2) still work on these schools, which suggest that similar-file students are randomly assigned even in this case. This is not surprising, as the middle school and the high school still have separate principals who do not coordinate during the class assignment of 10th grade students. In addition, removing these schools from the sample only makes our results stronger.

private middle and high schools from 2001 to 2012, this dataset contains personal information on students' identity (*e.g.* date and region of birth, gender, parents' occupation) and schooling: in particular grade, school and class attended, specific courses, grade and school attended in $t-1$ (but not the class attended in $t-1$).

Examination records: For all students from 2004 to 2011, this dataset contains personal information and informal scores obtained at the 9th grade DNB (both at the anonymous test and the in-school scores) and the 12th grade *Baccalauréat* exams.

Unfortunately, there is no unique identification number that allows us to track each student through the different datasets. Yet, for each 10th grade student, we need to know at least which class they attended in 9th grade, as well as the grade attended and major chosen in $t+1$ (repeating 10th grade, or moving to the 11th grade). We also have to match the administrative and the examination records.

For this purpose, we use a matching procedure based on students' personal information contained in each dataset. The procedure is based mainly upon the date and region of birth, the gender, and the grade and school attended in years t and $t-1$. We manage to match all needed information for 80 % of new 10th grade students. In the rest of the paper, all regressions include controls for the share of unmatched students within the classroom, although they do not change the estimates.

Our identification will rely on the set of information on students we observe in our dataset with regard to the information observed by principals in their registration files at the time of classroom allocation. Therefore, it is useful at this stage to summarize what variable is observed by whom:

X_i **covariates observed by both the principal and the econometrician:** Date of birth, city of residence, gender, parents' occupation, foreign languages and optional courses chosen, 9th grade in-school scores in all subjects, middle school and 9th grade classroom.

U_i **covariates observed by the principal but not the econometrician:** Students' first and last name (from which, in particular, ethnicity could be inferred), precise home address, 9th grade teachers' written comments on students (which may especially signal disruptive behaviors).

A_i **covariates observed by the econometrician but not by the principal:** Anonymous DNB test scores.

Section 3.1.2 will show how the A_i variables offer us a generic solution to test whether principals only use the X_i variables to sort students across 10th grade classes, so that not observing U_i covariates does not compromise our identification strategy.

2.3.2 Descriptive statistics

Descriptive statistics are presented in Table I, for the whole 10th grade student population (column I) and for low- and high-ability students separately (columns II and III). These two categories are defined by students' relative position to the median score of their middle school of origin, and will be used throughout this paper. The population contains 2.9 million individuals[14], of which 55 % are girls and 31 % are high-SES. The DNB anonymous exam is graded over 40 points and has mean 23.9; the high-ability students' average is 26.8 while the low-ability students' average is 21.0, which corresponds to a one-standard-deviation gap. The grade retention rate is 15 % overall, a figure mostly driven by low-ability students for which this rate rises to 27 % versus only 3 % for high-ability students. Conversely, 86 % of high-ability students enroll in academic majors while only 38 % of low-ability students do.

To measure the familiarity of students' with their classmates, we count for each 10th grade student the number of their classmates who were already in their class in the previous year (in 9th grade). These students are denoted persistent classmates (PC). Table II provides descriptive statistics on the number of PC for the whole population (column I), and for low- and high-ability students separately (columns II and III). We note that the average student has only 1.7 PC in their class; this figure is slightly higher for high-ability students, most likely because of their choice-specific courses[15]. They have 8.3 former classmates in total in the whole high school[16], meaning that 6.6 of them are in other 10th grade classes; the ratio of the number of former classmates in the high school to the number of PC is roughly equal to 5 which is the average number of classes in high schools: this suggests that principals do not consider students' class of origin when allocating them among 10th grade classes. Table II also reports the number of PC of each nature, *i.e.* whether they are of the same sex or not and whether they are high- or low-ability.

[14] Over 8 cohorts.

[15] Foreign languages and optional courses are often used as a disguised method for ability tracking. Parents who want their children to be assigned in a high-level class can incite them to take "elite" courses like German as first foreign language or Latin. This student will most likely be allocated with other students who did the same choice. To avoid *de facto* ability tracking, some principals however decide to distribute students across classes unconditional to students' specific courses. But it remains very rare, as they need to synchronize all classes' time schedules for this purpose (all classes must have these choice-specific courses at the same moment in the weekly schedule).

[16] We call "former classmates" students who were in the same class in 9th grade; persistent classmates are former classmates who are still in the same class in 10th grade.

The number of persistent classmates is very small in grade 10 compared to all other years of secondary education. Figure I shows the classroom composition of a typical student[17] in each grade. The share of persistent classmates remains fairly constant across middle school at around 30 %[18]. This means for instance that students in 9th grade have in average 30 % of their current classmates who were also in their class in 8th grade. However, the number of classmates whom students are familiar with drops dramatically in 10th grade. Only 5 % of their classmates come from the same class and 20 % from the same middle school. This suggests that students do not know at least 80 % of their classmates at the beginning of the year (assuming that they very rarely know students coming from other middle schools from other activities). This figure then rises up in following grades and amounts to 55 % in grade 12 due to the partial conservation of major-specific classes from grade 11. Overall, Figure I makes it clear that grade 10 represents a very intense disruption of students' social environment.

[17]In this figure, we do not consider students who are retained in their current grade.

[18]In grade 6, we are only able to identify students coming from the same elementary school, as we don't have any information on the classes in grade 5.

3 Empirical strategy

3.1 Identification

We consider in this study the number of "persistent classmates" (PC) that a student has in their 10th grade class, *i.e.* the number of classmates who were already in their class in 9th grade. This number can be viewed as a measure of the degree of familiarity of a student with their classmates: the smaller the number of persistent classmates is, the more anonymous a student may feel in their class and the stronger the social disruption experienced by the student is. To our knowledge, the degree of familiarity of a student with their peers has been overlooked in the literature on peer effects.

Because students are usually not assigned randomly to classes, the number of persistent classmates is endogenous to students' potential outcomes. For example, well-informed students may anticipate the difficulty of the transition to high school and choose the same optional courses than their former classmates. As a consequence, they are more likely to be assigned to the same class and the naïve regression of 10th grade outcomes on the number of persistent classmates would yield biased estimates of the PC effect.

3.1.1 Similar-file students

Our approach rests on the rare quasi-experimental setting that arises when principals encounter the case of two (or more) students who are exactly or very similar on paper ("similar-file" or SF students): they chose the same specific courses, share the same personal characteristics (gender, age, social class) and have exactly or approximately the same scores in 9th grade. In this section, we also require them to come from the same 9th grade class and we will refer to them as SF-S students.

These groups of students are defined with regard to the set of variables observed by both the principal and the econometrician, which define the X_i vector. The specification of the vector of covariates X_i is crucial to our strategy as it defines the minimum degree of similarity for which we assume students are undistinguished by high school principals during class assignment. SF-S groups are defined as students who:

- come from the same 9th grade class in middle school;
- come to the same high school in the same year;

- selected the same choice-specific courses;
- share the same gender, school age (has been held back at least once in previous grades, or is at least in time) and social background (low- or high-SES) based on father's occupation;
- belong to the same quintile of in-school 9th grade average score in scientific subjects (mathematics, physics-chemistry, biology);
- belong to the same quintile of in-school 9th grade average score in humanity subjects (French, history and foreign languages)[19];
- belong to the same decile of the in-school 9th grade average score of all subjects enumerated above.

This specification leaves us with 13,723 natural experiments of quasi-random class assignment. The sample is composed of 28,140 students having at least one "similar-file" mate who does not end up in the same class as them. Starting from an initial sample of 2,888,258 10th grade students over 8 cohorts[20], it implies that 1 % of them come to high school with at least one other student who shares the same values for the set of characteristics defining X_i (only one for 93 % of these students), while ending up in different classes. In the rest of the paper, "the SF-S sample" denotes the 28,140 students that have at least one similar-file mate ("SF mate").

In 10th grade, SF-S students may either be grouped in the same class or split in different classes. For instance, if two SF-S students chose a rare foreign language (*e.g.* Chinese) and only a few other students did so, then the school principal will probably group them in one class to simplify the making of the time schedules. But in other cases, they may be separated: if Chinese-learning students can be allocated to any of two (or more) classes, the school principal may want to equilibrate classes as much as possible by separating these students that have similar characteristics. The empirical strategies developed in this paper are based on the assumption that principals, *if they have to separate for any reason such similar students across different classes*, will decide randomly which student they assign to which class. We argue that this assumption is credible because these SF-S students are so close in terms of observed characteristics (X_i) that they cannot or do not want to spend time to distinguish them. Formally, this assumption writes

$$X_{ic}|X_i \perp U_i \qquad i.e. \qquad \mathrm{Cov}(X_{ic}, U_i|X_i) = 0 \tag{1}$$

[19] The foreign languages score is the weighted average of student's main foreign language (weight = 2/3) and second foreign language (weight = 1/3). Using different weights does not change the results of the paper. Besides, the in-school History score is missing for 5.4 % of observations. For these students, the average humanity score is the average of the French and foreign languages scores only.

[20] Excluding 10th grade repeaters, but also newcomers for whom data on 9th grade exam scores is missing.

where U_i is the vector of covariates observed only by the principal and X_{ic} is the vector of 10th grade classroom characteristics. However, this condition is unnecessary if, conditional on X_i, the U_i covariates are not correlated with potential outcomes or, equivalently, with unobserved factors of achievement in high school ϵ_{ic}. This means that we need that either condition (1) holds or

$$U_i|X_i \perp \epsilon_{ic} \qquad i.e. \qquad \mathrm{Cov}(\epsilon_{ic}, U_i|X_i) = 0 \tag{2}$$

In literal terms, the random assignment assumption cannot be used to identify the causal effect of PC on school achievement if, conditional to X_i, SF-S students are separated across classes regarding U_i covariates (such as teachers' written comments) *and* if U_i covariates are correlated with potential outcomes in high school (again, conditional on X_i).

3.1.2 Exogeneity test

We are able to implement a generic test, so as to test our core assumption that omitted variables U_i are not taken into account by principals when separating SF-S students across classes, or that they are not a relevant signal for potential outcomes conditional on X_i covariates, *i.e.* that condition (1) *or* condition (2) holds. This is possible thanks to the availability of the variables from vector A_i, *i.e.* students' anonymous DNB scores that we do observe while principals do not, as described in section 2.3.1.

This test rests on the hypothesis that any correlation between the U_i covariates and ϵ_{ic} should be captured by a correlation between U_i and A_i, *i.e.* that the DNB test score is a measure of ability that is relevant for high school outcomes for a given value of X_i. We argue that this hypothesis is credible and we discuss it later on. This assumption implies that if condition (2) does not hold (and it does probably not), then U_i and A_i are correlated. In that case, if condition (1) does also not hold, then $X_{ic}|X_i \not\perp A_i$, *i.e.* students will be sorted in classes according to their DNB score even though it is not observed by principals. This can be tested by estimating the following regression equation:

$$X_{ic} = \alpha_{X_i} + \beta \cdot A_i + u_{ic} \tag{3}$$

Table III (panel A) displays the β parameters estimated on the SF-S sample for many X_{ic} background characteristics of their classroom. β should be equal to zero if condition (1) *or* condition (2) holds. Column I measures the raw sample correlations between ability and classroom characteristics, *i.e.* without adding the α_{X_i} fixed effect. In the SF-S sample, more able students are assigned to larger classes and with more persistent classmates; their classmates are besides higher-achieving students, less often female and more often high-SES. Yet, the correlations are dramatically smaller in magnitude and precision within SF-S groups, *i.e.* as soon as we include the SF-S fixed effect (panel A, column II). In other words, differences in ability are not correlated with differences between classrooms characteristics, for students who were similar regarding all X_i covariates at the time of the class assignment.

Since we will examine the effect of the number of persistent classmates for low- and high-ability SF-S students separately, we check further that the assignment of SF-S students is random for each of these categories. Table III, panel B provides the estimated correlations between SF-S students' anonymous score A_i and PC for low- and high-ability SF-S students separately. Again, restricting to comparisons within SF-S groups makes all correlation between ability and PC disappear (line 1, columns V and VI versus columns II and III). The same pattern is found for all kinds of persistent classmates. Within each SF-S group, better students are neither assigned with more same-sex persistent classmates, nor with opposite-sex, low- or high-ability PC (lines 2 to 5). None of the estimates is statistically significant even at the ten-percent level, and the correlations are all very small anyway.

These tests clearly show that there is no correlation between anonymous scores and classroom characteristics for students with similar observed X_i characteristics (SF-S students). The most credible interpretation of these results is that conditional to X_i covariates, principals do not use the U_i information to distinguish between two SF-S mates and thus separate them in a random way. An alternative storyline is that principals do take U_i into account to assign SF-S students to 10th grade classes, but that U_i is not correlated to potential outcomes. While this scenario is not a problem for our identification, it does not seem very credible, since ethnicity (signaled by names) or behavior (signaled by teachers' written comments) is most probably correlated to potential outcomes.

A third possible storyline is that principals do consider U_i variables and that the latter are indeed correlated to potential outcomes in high school, but *without being correlated to DNB test scores.* This is yet very unlikely. Consider for instance 9th grade teachers' written comments on students. If teachers see a student as disruptive enough to signal it by written comments, then they probably underscored his performance in class. Therefore, disruptive students should exhibit in average higher anonymous scores than their SF-S mate(s) with no behavioral issues, since SF-S students have very close in-school scores by construction[21]. If SF-S students differed by behavioral characteristics and if high school principals assigned them to classes accordingly, their anonymous score should thus be correlated to their classroom characteristics. Results presented on Table III allow us to rule out convincingly this potential bias.

Overall, Table III's results support strongly our identification assumption that high school principals assign randomly separated SF-S students to classes. Students sharing the same characteristics as defined in section 3.1.1 thus appear undistinguished by principals during the class assignment. This could be surprising as principals observe more detailed information on students for which we could also control for, such as the exact in-school score in each subject or the precise occupation of parents. This probably explains why we do not need to control for the most precise level of detail on students' characteristics to observe exogenous class assignment.

3.2 Estimation strategy

Throughout this paper, we use the separation of similar-file students across 10th grade classes as a quasi-experimental setting, whatever the reason for separating them might be. In the first strategy, we estimate the effect of classmates' persistency directly on SF-S students, comparing classes in which they were randomly assigned. Within a SF-S group, the assignment is assumed random as in Rubin (1977) or Sacerdote (2001): differences in peer characteristics are assumed orthogonal to differences in individual unobservable characteristics because school principals could not distinguish SF students at the time of the class assignment.

Therefore, the random assignment of students in each SF-S group can be considered as separate natural experiments that allow identification of the causal effect of classroom characteristics. While this study focuses only on the effect of persistent classmates, this strategy may be implemented to identify the causal effect of any classroom characteristics (*e.g.* the average ability of classmates, or their female share) provided that their variance is large enough within SF-S groups. This derives from the fact that the random assignment of students within each given SF-S group is a separate instrument for identification. Theoretically, we are thus able to identify as many classroom effects as there are similar-file groups, although in practice we also need the set of quasi-experiments to cover a large share of the support of the peer effects of interest. Going into the details of the effect of other classroom characteristics would require a lengthy analysis that goes beyond the objective of this study. Here, we will limit ourselves to the analysis of the effect of the number of PC. A deeper analysis of

[21] Recall that in-school scores are precisely scores given by 9th grade teachers in class.

the other peer effects will be proposed in future papers using the same empirical strategy.

One limitation of this estimation strategy comes from the fact that SF-S students are separated across classes that vary in many characteristics simultaneously. As a consequence, the estimated effect of persistent classmates could reflect correlated omitted classroom variables that we did not account for. To tackle this issue, we control in our regression for linear or quadratic functions of several other classroom characteristics. As our results will show, the estimates of interest are left unchanged by adding these controls, which is a first piece of evidence that correlated unobserved classroom variables are not an issue. However, it remains an incomplete answer, but the second estimation strategy presented in section 5 will definitely solve this problem by examining pure variation in the number of persistent classmates, without any change in other classroom characteristics.

Formally, we estimate the following model by OLS[22]:

$$y_{ic} = \alpha_{X_i} + \beta \cdot \mathrm{PC}_{ic} + \sum_m f_m(\bar{X}_c^m) + \epsilon_{ic} \tag{4}$$

where :

- y_{ic} denotes the outcome of student i assigned to 10th grade class c.
- α_{X_i} is a fixed effect that is specific to each possible combination of the vector of covariates X_i observed by the principal. This "similar-file" fixed effect restricts the analysis to comparisons within groups of SF-S students, as defined in section 3.1.1.
- PC_{ic} is the number of persistent classmates that student i has in their class c. β is the parameter of interest; it captures the causal effect of being assigned with one additional persistent classmate.
- The $f_m(\bar{X}_c^m)$ are linear or quadratic functions of classroom characteristics that might vary with PC_{ic}: the average classmates' ability, which is computed as the classmates' mean average score at the DNB anonymous exams[23], the classmates' female share, the share of high-SES classmates (as measured by father's occupation[24]) and the class size (the total number of classmates). Again, the estimations of β presented in the paper are however unaffected by adding or not these controls, or by using different

[22] Although our estimation strategy is similar in spirit to exact-matching methods, we chose not to use matching estimation as the number of persistent classmates examined in this paper is not a binary treatment. As far as we know, the literature is very poor on the estimation of average causal effects of multi-valued treatments through propensity score or exact matching methods (see Imbens, 2000, in this direction).

[23] Because this data is missing for all retained students (around 10 % classmates) and for another 20 % classmates (not matched, see section 2.3.1), we also control quadratically for the shares of retained students and missing data.

[24] The SES field in our database actually contains the father's occupation if available, the mother's or legal guardian's otherwise.

functional forms.

- ϵ_{ic} captures individual unobserved heterogeneity and is assumed orthogonal to observable characteristics. Note that $\mathbb{E}(\epsilon_{ic}|\text{PC}_{ic}, X_c^m) \neq 0$ but under our identification assumption, $\mathbb{E}(\epsilon_{ic}|\text{PC}_{ic}, X_i, X_c^m) = 0$.

3.3 About the external validity of the SF-S sample

One could worry about the specificity of the sample of students with similar registration files, raising legitimate concerns regarding the external validity of any conclusions we could draw from out analysis.

Descriptive statistics on the SF-S sample compared to the initial population are presented on columns IV to VI of Table I and Table II. Students in the SF-S sample are slightly higher achievers: they repeated less before high school, obtained better scores at the 9th grade national exams, enroll more often in the academic track and major more often in science in 11th grade. Interestingly, they take less high-quality choice-specific courses (German, ancient languages, European section[25]). This comes from the fact that students who take these rare courses are often assigned to the same class and are thus not included in the sample. Last but not least, sample students have more former classmates in their high school (12.7 versus 8.3) and in their class (2.0 versus 1.7). This is true for all types of former classmates (same- and opposite-sex, high- and low-ability).

Therefore, the SF-S sample appears slightly different from the whole population of French 10th graders. However, the differences are not always large in magnitude, though statistically significant. For example, students' average DNB test score is 25.1 (sd = 5.6) in the SF-S sample compared to 23.9 (sd = 5.1) in the whole population. 15.0 % of the SF-S sample repeat 10th grade while 15.3 % of the whole population does, a difference that is again very small.

In addition, just because the sample is a small fraction of the population does not mean that it does not encompass a variety of situations. All levels of ability, SES are largely represented in the SF-S sample. This allows us to detail quite precisely the distribution of our results over the characteristics of the individuals. In terms of schools attained, SF-S students are found in 1,851 high schools out of 2,679, *i.e.* 69 % of all high schools. The high schools that do not get SF-S students are mostly very small schools: they have in average 66 tenth grade students, while the high schools that do have SF-S students have 259 tenth grade students in average. Overall, these high schools account for 91 % of all 10th grade students.

[25]The European section consists in having some of the core courses taught in a foreign language (usually English, German or Spanish).

Besides, many intermediate results throughout the paper show that our conclusions are not specific to our selected SF-S sample. On Table IV for example, we find peer effects that are consistent with the existing literature, especially regarding the effect of the female share within the classroom(Hoxby, 2000; Lavy & Schlosser, 2011). We also show that the estimated PC effect is robust to changes in the specification of SF-S groups, either in a more restrictive or a less restrictive way (section 4.3), or when implementing our second estimation strategy based on SF students' classmates (section 5). It implies that the effect of classmates' persistency can be found in several different samples, which is reassuring in terms of external validity.

4 Results

4.1 10th grade school outcomes

Table IV reports the OLS estimates of the β parameter in the model presented in section 3.1 (equation 4). The effect of the number of persistent classmates is estimated for all students (column I) and for low- and high-ability students separately (columns II and III). All regressions include the SF-S fixed effect, which ensures that students are compared exclusively to their similar-file mate(s). Panel A reports the effects on students' propensity to be retained in grade 10 at the end of the year, while panels B and C display respectively the estimated effects on enrolling in an academic or a technological major in grade 11.

The results suggest that assigning a student in a class with more persistent classmates has a substantial positive effect on their academic achievement. In average, one additional PC reduces the risk of grade retention by 0.3 percentage point (pp.) (standard error (se) = 0.1 pp.) and increases students' enrollment in academic majors by 0.3 pp. (se = 0.1 pp.) (Table IV, column I). The effect on technological majors enrollment is very small and not precisely estimated (+0.1 pp., se = 0.1 pp.). The distribution of these effects regarding ability is in fact very heterogeneous. There is almost no effect on high-ability students (Table IV, column III), while the number of persistent classmates is obviously very beneficial for low-ability students: the effects amount to −0.9 pp. in grade retention per PC (se = 0.3 pp.), +0.5 pp. in academic major (se = 0.3 pp.) and +0.5 pp. in technological major enrollment (se = 0.3 pp.) (Table IV, column II). It should be noted, however, that high-ability students have very extreme initial values for these three outcomes: only 3 % of them repeat grade 10 and 86 % choose an academic major (Table I).

The magnitude of the PC effect on the grade retention risk of low-ability students is huge. To put this into perspective, note that 27 % of the low-ability students are retained at the end of the year, so that each additional persistent classmate decreases a low-ability student's risk to be retained by 3 %. Based on the descriptive statistics in Table II, assigning a low-ability student to the same class as all of their former classmates that are in the same high school (7.8 versus 1.5 in their actual class) could reduce low-ability students' retention rate by 5.7 pp. (*i.e.* around 21 % of their current retention risk), assuming that the effect is linear (we come back to this point in section 4.5). Also, note that the sum of the effects on academic and technological majors enrollment is roughly equal, in absolute value, to the effect on grade retention, indicating that the decrease in the retention rate does not hide an increase in the dropout rate[26].

In columns IV to VI, we reproduce the results controlling for other characteristics of the 10th grade class, *i.e.* including the $f_m(\bar{X}_c^m)$ in equation (4). Adding these controls does not change the estimated effect of PC. This shows that there is almost no correlation between the number of persistent classmates and other class characteristics, which supports our causal interpretation of the effect of the number of persistent classmates. Looking now at the effect of these other characteristics on 10th grade outcomes, we first find that classmates' average ability increases the risk of retention. This negative peer effect makes sense considering that retention does not depend only on students' level but also on their relative position within the class. A student assigned with better classmates will thus appear weaker in comparison, which may increase their risk of being retained in 10th grade. Conversely, the share of female classmates has a positive effect on students outcomes, which is consistent with previous results found in the literature on this topic (Hoxby, 2000; Lavy & Schlosser, 2011). We find no effect of class size or of the share of high-SES classmates, most likely due to the small variance of these variables between the classrooms of a given high school. Overall, the consistency of our estimations of the exogenous peer effects (in the sense of Manski, 1993) that are traditionally studied in the existing literature, *i.e.* peer average ability and gender composition, is reassuring with respect to the external validity of our result on the effect of persistent classmates. In the rest of this paper, we will only show the estimates of the PC effect while controlling for quadratic functions of other classroom characteristics[27]. As in Table IV, removing these controls or using linear functional forms does not change the results.

We investigate further the PC effect on the precise major chosen by students within the academic and technological majors. Results for low-ability students only are reported in Table V. Persistent classmates do not seem to push students towards one particular major rather than the others, although the positive PC effect on technological major enrollment seems slightly more driven by the administration major rather than the industry major. This indicates that students with all kinds of academic profiles (regarding their favorite subjects for example) may benefit from the presence of persistent classmates. It also suggests that the latter do not raise students' achievement in one specific subject, which would lead them to enroll more often in one particular major.

In Table VII, we examine the effect of other middle school mates, *i.e.* classmates who attended the same

[26]Students that drop out or that are not matched across the 10th to 11th grade transition for another reason are not distinguished in our dataset. These students are *not* removed from the sample, and the effect of PC on being matched, estimated using equation (4) with "being matched in 11th grade" as an outcome, is small and non-significant.

[27]A detailed investigation of these other peer effects will be proposed in future papers

middle school but not the same 9th grade class. Interestingly, the latter have no impact on students' achievement in 10th grade. This important result suggests that the effect of classmates' persistency does not operate through channels that former middle school mates should also trigger, such as shared neighborhood. The effect of familiarity seems to operate rather through mechanisms such as sharing working habits or increasing sense of belonging in class, since former classmates are much more likely to foster than middle school mates from other classes.

4.2 Long-term high school outcomes

The positive short-term impact of students' familiarity with their classmates on 11th grade enrollment might be hiding longer-term negative consequences for at least two reasons. First, persistent classmates could isolate students from their new environment if they prevent them from interacting with other students. Therefore, students would be less integrated to their new high school and would possibly undergo a negative experience if they are separated in 11th grade, for instance if they choose different majors. Second, enrollment in academic or technological majors is determined by students' achievement but also preferences, as explained in section 2.1.2. If, for instance, persistent classmates do not mitigate grade retention by helping you to achieve better, but only make you more reluctant to be retained (*e.g.* because your friends are accepted in a given 11th grade major and you do not want to be left alone in 10th grade), you may force teachers to accept your enrollment in 11th grade (see footnote 11) and pay for it later, because courses are too difficult. If the PC effect operates only through this "over-enrollment" mechanism, then the number of persistent classmates should be negatively correlated with 11th and 12th grade outcomes.

To address these issues, we exploit a dataset based on the *Baccalauréat* examination records, *i.e.* the national exam for high school graduation. This exam takes place at the end of grade 12 both in the academic and the technological majors, and it is based almost entirely on anonymous tests graded by teachers outside students' high school. We substitute *Baccalauréat* outcomes to 10th grade outcomes in our regression to estimate the effect of persistent classmates on these longer-term achievement, focusing now only on low-ability students.

Table VI shows the estimated PC effect on three outcomes. We start by examining whether students take the exam "in time", *i.e.* three year after entering high school, meaning that they did not repeat grade 10 or grade 11 and that they made it through grade 12 without dropping out (column I). Persistent classmates in 10th grade class increase strongly SF-S students' likelihood to take the exam in time (+1.4 pp. per additional

PC, se = 0.5 pp.) or to pass the exam (column II, +1.1 pp. pe PC, se = 0.4 pp.). We also look at students' average score at the exam (column III), conditional on taking it in time. The effect is still positive, though small in magnitude and non significant. We thus find no evidence that students would perform less well in average after grade 10. They even achieve slightly better at the *Baccalauréat* exam.

To conclude, students perform considerably better in high school when assigned in 10th grade with classmates that were already in their class in 9th grade. They are less retained at the end of the year, enroll more in 11th grade and perform at least as well in 11th and 12th grades.

4.3 Robustness checks

In this section, we provide several proofs of the robustness of the estimated PC effect presented on Table IV.

Our main identification assumption is that students who are assigned with more persistent classmates than their similar-file mate(s) do not have individual unobservable characteristics associated with better outcomes. This assumption is violated if, for instance, they have very motivated parents who ask explicitly for their child to be put in the same class as their friends, by contacting directly the principal. As long as such parents behaved similarly during previous transitions, we can test for this scenario using the number of classmates kept by students between their 8th and 9th grade classes (named "PC-8to9"). This variable could differ between students of the same SF group, since they come from the same 9th grade class but not necessarily from the same 8th grade class. Table VIII first shows the absence of statistically significant correlation between PC-8to9 and PC, the number of classmates kept during the current transition to high school (panel A). We then implement a pseudo-placebo test, substituting PC-8to9 to PC in our main regressions (panel B). This is not a pure placebo test, as the number of classmates kept during the previous transition could have a long-term effect on students, hence on 10th grade outcomes. However, Table VIII, panel B reveals that this is not the case. The number of classmates kept by a student through their 8th to 9th grade transition is not correlated with any outcome in 10th grade. This result reinforces our conviction that the PC effect is not driven by any unobservable characteristics correlated to the number of classmates they keep at each grade transition.

In Table IX, we examine whether our estimations are driven by a spurious correlation in a particular cohort. We run the basic regression separately for each cohort and present the results for low-ability students. Overall, results are very stable across cohorts. The estimated effect of persistent classmates on grade retention is negative in all cohorts, with a magnitude comprised between -0.4 and -1.8 pp. The effect is even higher (in absolute terms) than 0.8 pp. in 5 out of 8 cohorts. The effect is a little bit less stable for academic or technological major enrollment: in four cohorts out of eight, both coefficients are positive, but in two cohorts (2005, 2009) the coefficient of academic major is negative and in two other cohorts (2010, 2011) the coefficient of technological major is negative. However, the sum of the coefficients for enrollment in each major is always roughly equal to the opposite of the coefficient on grade retention, which confirms that the effect on grade retention is not hiding a rise in dropout. Overall, the main effect on grade retention is quite stable over cohorts.

Finally in Table X, we show that the estimated effect of persistent classmates on low-ability students is not strictly dependent on the specification of the vector of characteristics X_i described in section 3.1.1. Other specifications that are less restrictive (columns I, II and III) or more restrictive (column V) on the required degree of similarity of students' files lead to comparable estimates of the effect of persistent classmates than the specification used in this paper (reproduced in column IV for the purpose of comparison). In Column III, we remove 9th grade class of origin from the X_i vector, so that SF students may come from different classes (in the same middle school), but that are similar across several characteristics (SF-D students): classes that offer "elite" courses, with the same average ability (same quintile of the average DNB score) and that sent approximately the same number of students in the high school (again, same quintile). The measured effect of PC is still positive, though only significant at the 5 % level. Column II restricts, again, to same-ninth-grade-class students but removes gender and SES from the X_i vector, implying that students from opposite gender or SES may be considered as SF students if they are similar on the remaining X_i characteristics. This specification elicits a similar estimated PC effect of -0.7 pp. on grade retention, that is also more precise (se $=$ 0.2 pp.). In column I, we are considerably less restrictive as we remove quintiles of humanity and science average scores, leading to a much larger sample of 241,601 low-ability students compared to the 11,409 low-ability students studied in this section. This specification also yields a positive and significant effect of persistent classmates, though smaller in magnitude (-0.4 pp. on grade retention). Conversely, column V displays the results for a specification that is more restrictive than the original one: we use deciles of humanity and science average scores instead of quintiles. Once again, the results are confirmed, as each persistent classmate has a mitigating effect of -1.3 pp. on grade retention, significant at the 5 % level. Overall, the evidence confirms that the results presented on the paper are not strictly dependent on specification choices[28].

[28]We chose to present more extensively in this section the results obtained with the specification in column IV of Table X, because results of the exogeneity test for most other specifications did not support the assumption of SF students' random assignment. This was only the case for the specification in columns IV (as showed in Table III) and III. Even though the estimations of the PC effect displayed in columns I, II and V are thus probably not identified, the fact that their signs and magnitudes are similar is reassuring with regard to the robustness of our results. Specification III with SF-D students instead of SF-S students will be used

4.4 Distributional pattern

In Table XI, we analyze the distributional pattern of the effect of persistent classmates across genders and SES. We report only the estimates for low-ability students, since those for high-ability students were systematically small and non-significant, regardless of their gender or SES. First, panel A presents the distribution of the effect across gender only. The reduction in the grade retention risk is slightly larger for boys (-1.0 pp. versus -0.7 pp.) and more precisely estimated. The discrepancy is higher for 11th grade choice of major. While the decrease in grade retention translates for girls into a significant rise in technological major enrollment only ($+0.6$ pp., se $= 0.4$ pp., versus $+0.2$ pp., se $= 0.5$ pp. in academic major enrollment), persistent classmates seem to make boys turn towards academic rather than technological majors ($+0.8$ pp., se $= 0.5$ pp., versus $+0.4$ pp., se $= 0.4$ pp.). All in all, persistent classmates generate some kind of "catching up" effect by driving students of a given gender towards the major in which they are under-represented.

We then reproduce this analysis on low- (panel B) and high-SES (panel C) students separately. Noteworthy is the very unequal distribution of the PC effect across SES. Persistent classmates have virtually no effect on high-SES students, but a very strong, positive and significant impact on low-SES students (-1.4 pp. on grade retention, $+0.8$ pp. on academic and $+0.7$ pp. in technological major enrollment). The gender distribution of the effect on low-SES students reveals a similar pattern as previously. In particular, we find a tremendous effect on low-SES boys: each additional PC diminishes their grade retention risk by 1.8 pp. (se $= 0.7$ pp.) and increases their probability to choose an academic major by 1.4 pp. (se $= 0.6$ pp.). The effect for girls is smaller but still high on grade retention (-1.0 pp., se $= 0.6$ pp.) and on technological major enrollment ($+0.8$ pp., se $= 0.5$ pp.). By contrast, the total absence of any effect for high-SES student – even within low-achievers and/or boys – is remarkable and is discussed in section 4.6.

In Table XII, we look at the distribution of the effect over the intensity of the disruption in terms of social environment during the transition to high school. For each student, we compute the difference in the share of high-SES students between grade 9 and grade 10. Looking only at low-ability students, on whom the effect is mostly concentrated, we can split the sample into three equally populated categories: those for whom this difference is negative (column II), those for whom it is positive and under 11 % (column III), and those for whom it is positive and above 11 % (column IV). Column I gives the average, and is identical to column II in Table IV. We find that the effect grows stronger as the difference increases, meaning that those coming from a

again in section 5 to implement our second identification strategy.

middle school with a more deprived background and going to a high school with much more high-SES students are those who benefit the most from the presence of persistent classmates. In the last category for instance, the effect on grade retention is as high as -2.2 pp. (se $=1.1$ pp.) per persistent classmate.

We analyzed further the distribution of the effect with regard to the middle and high school contexts. However, we did not find any other interesting pattern[29]. For example, the effect does not seem to vary significantly with school sizes or the share of middle school classmates attending the high school. We also investigated whether the effect of classmates' persistency depends on the 10th grade classroom context. We checked in particular whether the degree to which your new classmates are grouped with their former classmates increased your need to be with yours. Yet, we found no result in this direction, which has important policy implications: indeed, it suggests that grouping former classmates would not drive negative spillovers on their other classmates, who did not necessarily have many former classmates in the high school. Though, we think that such a conclusion needs to be confirmed by a more precise investigation on an experimental setting in which we would examine more directly all sorts of externalities within the classroom.

Overall, our results indicate that assigning low-ability 10th grade students with more classmates from their 9th grade class may help strongly those facing the most difficulties during the transition to academic high school. This effect operates particularly well on low-SES, low-ability students, and seems slightly more important on boys than on girls. This is an important result since the risk of grade retention is especially high for male, low-achieving, low-SES students. Furthermore, the effect is very large for students who experience a violent disruption in the socio-economic composition of their schoolmates that may induce a difficult cultural shock. Our findings thus imply that allocating students with more classmates they already know could be an efficient policy lever to help them catching up with other students regarding educational achievement and, consequently, to mitigate school inequalities.

4.5 Non-linearities

Non-linearities in peer effects are a crucial feature to identify in a policy perspective. In our particular case, if each persistent classmate until the fifth has a very strong effect but there is no additional gain above this threshold, there will be no need to group all 8 former classmates that the average student has in his high school into one class. In columns I to V, Table XIII tests whether the effect rises suddenly when PC exceeds a certain

[29] All detailed results on the distribution of the effect are available on demand.

threshold and/or whether the slope of the PC effect varies before and after the threshold. We limit the sample to low-ability students, on which the effect is the strongest. Testing different values for the threshold (1 to 5), there is no clear evidence of non-linearities in the effect of the number of persistent classmates. Estimates of a threshold effect (line 2) or a trend change (line 3) are often small and hardly significant. However, columns IV and V display larger estimates for a threshold effect. In particular, column V indicates that the effect could be non-linear around 5 PC. For grade retention, the regression suggests a linear effect until 4 PC (-0.9 pp., se $=$ 0.5 pp.), a huge "jump" of -7.7 pp. at 5 PC (se $=$ 5.7 pp., non significant), and then no gain from additional PC (change in trend by $+1.1$ pp., se $=$ 0.9 pp.). Regarding academic major enrollment, the results indicate no linear effect, but a large and significant increase for students with more than 5 PC ($+11.2$ pp., se $=$ 6.1). But we find no similar evidence of non-linearity for technological major enrollment. In Column VI, we provide the results for another test of non-linearities using simply a quadratic function of PC. The quadratic coefficient is very small and insignificant for all outcomes, suggesting that the effect is rather a linear function of the number of persistent classmates. To sum up, we probably lack statistical power to draw any clear conclusion on the linearity of the PC effect. What appears from our data is that the effect seems rather linear, except maybe for a threshold around 5 persistent classmates.

4.6 Mechanisms

Why would students be better off with more persistent classmates in their new 10th grade class? Several answers could be provided, that we divide into two categories.

First, persistent classmates make the environment more familiar and could thus decrease the anxiety of the transition to high school, whether you interact with them directly or not. We refer to such an effect as the *indirect familiarity channel*. Knowing many faces in the class reduces the uncertainty of the environment, since students' expectations about classmates' ability and behavior is then more accurate. It may also raise your sense of belonging in the high school. This could be salient if one thinks of oneself as an outcast in academic high school. For example, low-achieving students from low SES may feel like they are in the wrong place because most of their friends pursued vocational studies after middle school, and because their parents may not have studied in academic high schools. Conversely, high-SES students may have anticipated the transition to academic high school for many years, even if they did not perform really well in middle school, for instance if vocational studies were not even an option regarding their parents' expectations. This could explain why persistent classmates do not change anything for high-SES students, although their presence helps low-SES students in a psychological way: after all, if my former classmates enrolled in academic high school, there is no reason why I should not feel like I am in the right place.

Second, the persistent classmates effect may rather operate through a *direct interaction channel*, as students may interact more easily with persistent classmates at the beginning of the school year, or might even be friends with them since middle school. During the transition to high school, friends might be valuable for many reasons. Having friends to interact with is probably very important for students to feel protected and surrounded. Besides, teenagers' needs for recognition by peers might be fulfilled if they kept their friends during the transition. Otherwise, students might be more inclined to behave in a disruptive way, so as to assert themselves in the classroom. Friends might also be beneficial in an academic perspective, since friends may work together and cooperate more efficiently during teamwork. They might thus help to face the sharp increase in teachers' expectations during the transition to high school. Finally, even though students are not necessarily friends in an absolute sense with their persistent classmates, the latter remain people with whom it is easier to interact directly during the first weeks of the year. Students can easily talk to their persistent classmates during breaks and classes, should it be to share their high school's experience, to ask questions about courses, or simply to ask for directions and not lose themselves in the new and larger premises of the high school. This channel could also explain why persistent classmates do affect low-SES students but not high-SES ones (Table XI). Firstly, high-SES parents are more likely to help their children (directly or by affording private tuitions). Secondly, low-SES persistent classmates may be more likely persistent friends, if for example low-SES students are less likely to make new friends in their class. More generally, low-SES students might be more influenced by former classmates because they may interact more with their school peers in general, since they participate in fewer out-of-school activities.

We now try to clear up the extent to which persistent classmates have a positive impact through direct interactions, rather than helping indirectly by making the classroom environment more familiar. To do so, we investigate whether students are more affected by persistent classmates who share similar characteristics. We know indeed from the literature that social networks are homophilic, *i.e.* that individuals tend to interact more with similar peers (Currarini *et al.*, 2009). So if persistent classmates had a positive effect only through direct interactions, we would expect students to benefit much more from same-characteristics persistent classmates.

This is however not what the data shows. In Table XIV, we investigate the specific role of persistent classmates with respect to their gender and ability. We report the estimated effect for low-ability students only, since the estimates for high-ability students are as in previous tables very small and mostly non-significant. The results do not confirm the previous interpretation. In column I, we run regressions of students' 10th grade outcomes on the number of same- and different-gender separately. Students seem to benefit from both types of persistent classmates and the effect is even stronger and more significant for opposite-gender PC than for same-gender PC: -1.2 pp. (se $= 0.5$ pp.) versus -0.6 pp. (se $= 0.5$ pp.) in grade retention rate and $+0.6$ pp. (se $= 0.5$ pp.) versus $+0.4$ pp. (se $= 0.5$ pp.) in academic major enrollment (Table XIV, column I). In column II, we examine whether low-ability students are more impacted by persistent classmates who exhibit similar achievement (*i.e.* who are themselves low in ability) and with whom they are thus more likely to interact. Again, this is not what we find: both kind of persistent classmates have a similar impact on students. Although same-ability persistent classmates are slightly more beneficial than different-ability – that is high-achieving PC – with regard to grade retention (-1.0 pp., se $= 0.6$ pp., versus -0.8 pp., se $= 0.5$ pp.) or academic major enrollment ($+0.7$ pp., se $= 0.5$ pp., versus $+0.3$ pp., se $= 0.5$ pp.), the effect of different-ability PC is higher than low-achieving PC on technological major enrollment ($+0.7$ pp., se $= 0.4$ pp., versus $+0.4$ pp., se $= 0.5$ pp.). On the whole however, there is no clear evidence that students benefit more from persistent classmates with whom they are more likely to interact directly, *i.e.* same-gender or same-ability ones. Therefore, we can at least conclude that persistent classmates are not only beneficial through direct interactions and friendships, but probably also operate by increasing indirectly students' familiarity with their new environment.

5 Alternative estimation on SF students' classmates

In the previous section, we provided unbiased estimates of the causal effect of classroom characteristics based directly on SF-S students who were randomly assigned to different classes. In doing so, we emphasized a strong and positive effect of being assigned in a class with more former classmates. However, such classes might be specific in many other dimensions that may be correlated to classmates' persistency. Although we showed that controlling for several other classroom characteristics did not affect the estimates, we cannot completely rule out at this stage that the estimated PC effect captures in fact the effect of some omitted correlated classroom variables (*e.g.* teachers' characteristics). To deal with this particular issue and, more generally, to confirm our results, we develop an alternative approach based on an instrumental variable strategy. This second strategy exploits the idea that the random allocation of some students may induce an exogenous variation of the classroom composition *for the other students who have been assigned (even endogenously) to these classes.*

5.1 Identification approach based on the indirect effect of SF students' random assignment

Previously, we considered only similar-file students coming from the same 9th grade class (SF-S students), who were presumably randomly scattered across two (or more) distinct 10th grade classes denoted classes X and Y on Figure II. Note that in this case, the resulting allocation does not change anything for their former classmates that have been assigned in these classes. Figure II makes it clear that A3 has two former classmates in both cases: A4 plus A1 or A4 plus A2.

Now, consider two students who share very similar individual characteristics, come from the same middle school *but from different 9th grade classes* though similar in characteristics (SF-D students). We already defined these groups in specification III used on Table X (see section 4.3 for the detailed specification). Section 5.2 will show that SF-D students may be credibly assumed undistinguished by school principals during the 10th grade class assignment, as for SF-S students. Figure III illustrates how the random assignment of SF-D students (denoted A1 and B1) may be used as a convincing instrument for the number of persistent classmates *of their former classmates* assigned to 10th grade classes X and Y (denoted A2, A3, A4 and B2, B3, B4). If A1 is assigned to X, then A2 and A3 will have two persistent classmates and A4 will not have any. But if A1 is assigned to Y, A2, A3 and A4 will all have 1 persistent classmate. The same exogenous variation of the number of persistent classmates happens for B2, B3 and B4. Therefore, since A1 and B1 do not come from the same 9th grade class, their random assignment to 10th grade classes X and Y may be used as an instrument for the total number of PC of students A2 to A4 and B2 to B4.

Formally, we estimate the following model:

$$Y_{ic} = a + b \cdot \mathrm{PC}_{ic} + \lambda_{\mathrm{highschool}_i} + \epsilon_{ic} \tag{5}$$

We instrument PC_{ic} in the first stage with Z_{ic}, the number of PC they got, but who belong to a SF-D group and have thus been presumably randomly assigned to i's class:

$$\mathrm{PC}_{ic} = c \cdot Z_{ic} + \lambda_{\mathrm{highschool}_i} + u_{ic} \tag{6}$$

The regression includes fixed effects for high school and excludes the SF-D students themselves, so that the comparison is restricted within groups of *former classmates of SF-D students*, taking only those who are assigned to a classroom in which the SF-D student could have been himself randomly assigned. This sample – the peers of SF-D students – is denoted $\mathcal{P}$(SF-D) in the rest of this paper. In the example presented on Figure III, the regression is restricted within A2, A3 and A4, excluding A1 but also A5 and A6 who were assigned to class Z in which A1 could not have been assigned.

5.2 IV exogeneity test

Within this sample, we argue that Z_{ic} is a credible exogenous instrument (*i.e.* uncorrelated to unobservable characteristics that affect PC_{ic}) as it results from a random draw.

To support this assumption, we implemented an exogeneity test, which consists in checking whether the value of the instrument Z_{ic} is correlated to individual characteristics of students from the $\mathcal{P}$(SF-D) sample. This usual exogeneity test for IV strategies is necessary to ensure students who get a former classmate from the random split of a SF-D group do not share significantly different observable characteristics than other $\mathcal{P}$(SF-D) students. Formally, we run the following regression:

$$Z_{ic} = \beta \cdot X_i + \lambda_{\mathrm{highschool}_i} + u_{ic} \tag{7}$$

where X_i is the vector of observable characteristics tested. Table XV presents the estimated β vector without (column 1) and with the high school fixed effect (column 2). The results clearly show that within a given high school, having repeated in the past, being a girl, a high-SES status, having picked elite optional courses or having got higher scores at the DNB exam does not increase the probability to receive an additional former classmate from the random allocation of SF-D students. This result provides evidence of the exogeneity of the instrumental variable Z_{ic}.

5.3 IV exclusion restriction

Even if exogenous, Z_{ic} is a viable instrument to estimate the causal effect of PC_{ic} on academic achievement only if it does affect outcomes through its effect on PC_{ic} solely. Note that in our case, this exclusion restriction assumption is very credible. By construction, SF-D students *only differ with regard to their 9th grade classroom of origin* since we constrained them to have similar characteristics in all other dimensions: they chose the same optional courses, share the same gender, the same social background and have very close abilities. Provided that such similar SF-D mates do not differ in any unobserved PC-correlated individual characteristic, the result of their random assignment does thus not impact any classroom characteristic other than PC. Figure III makes it clear that for student A2, getting A1 or B1 in her class impacts the number of PC she has, but does not make any difference with regard to the average ability of her classmates, their gender or social background composition, or the size of her class. Therefore, the effect of Z_{ic} on Y_{ic} operates plausibly only through PC_{ic}.

Therefore, this second estimation strategy allows us to identify a pure effect of persistent classmates, as other classroom characteristics are credibly held constant by construction. This is a substantial improvement with regard to the previous strategy based on SF-S students, since the latter were themselves randomly assigned to classes that differ in many other ways than PC (see again section 3.2). While it enabled the identification of several peer effects, the PC effect itself could be driven by other correlated peer characteristics that even the quadratic functions we used did not sufficiently control for – although this does not seem likely. On the other hand, the current estimate of the PC effect on $\mathcal{P}$(SF-D) students, *i.e.* SF-D students' former classmates themselves is more accurate, because other peer characteristics do not change with the result of the random assignment of SF-D students.

5.4 Results

Table XVI (column I) displays the effect of PC on SF-D students' former classmates, estimated in two stages as presented in equations (5) and (6). Standard errors are clustered by group of students who come from the same 9th grade class and are assigned to the same 10th grade class (students A2 and A3 for example on Figure III). Overall, the results are close to those obtained with the first strategy implemented in section 4. Being assigned in 10th grade with one additional former classmates has a positive and significant effect on students' outcome at the end of the year: students are less subject to grade retention (-0.9 pp., se $= 0.2$ pp.), they enroll less in technological majors (-0.4 pp, se $= 0.2$ pp, non-significant) but much more in academic majors ($+1.2$ pp., se $= 0.03$ pp.). The longer-term effects are less precisely estimated, but the pattern is overall similar to previous results presented on Table VI: each additional persistent classmate in one's 10th grade class raises the probability to take the *Baccalauréat* exam in time by 1.1 pp. (se $= 0.4$ pp.) and to pass the exam by 1.4 pp (se $= 0.4$ pp.). Overall, Table XVI results provide strong evidence that the main estimated effects presented on section 4 are very robust, since we estimate very similar effects with two very different estimation strategies on two different samples. Furthermore, they strongly suggest that the estimates provided with the first strategy based on SF-S students were not driven by omitted classroom variables, for the reasons stated above (section 5.3). Besides, $\mathcal{P}$(SF-D) students within the same high school come from the same middle school by construction, because they are former classmates of SF-D students who come themselves from the same middle school, though not from the same 9th grade class). Thus, these results also confirm that students are much more (if not only) affected by persistent classmates rather than by persistent middle school mates from other classes (as already suggested on Table VII).

One question that we did not answer yet is whether the positive effect of persistent classmates operates through improvement in the global classroom context. For instance, one could expect it would be more comfortable to teach to a class if more students already know each other in the classroom, because it would be easier to organize teamwork for instance. This beneficial impact on teachers could then affect all students in the class, even those who are not directly concerned by having more former classmates. Fortunately, we are able to test for this potential mechanism with this second strategy. In column III, we replaced high school fixed effects on equations (5) and (6) by 10th grade classroom fixed effects. This allows us to compare only $\mathcal{P}$(SF-D) students who are assigned in the same class, that is to compare A2 and A3 only to B2, not to A4 or B3 and B4 on Figure III. Even though we lose statistical power in this case, the estimates remain very close in magnitude and are still statistically significant for 10th grade outcomes. This result makes it clear that the beneficial effect of persistent classmates does not solely operate through improvement in the global classroom context, since we should not find in this case any difference between students of the same 10th grade class. 10th grade students thus seem to be directly affected by their former classmates, through channels that operate at the individual level such as higher sense of belonging or social and academic support.

6 Discussion and conclusion

During transition to high school, most students experience a dramatic disruption in their social network that goes much further than being separated from their friends. Since students are scattered across high schools, students usually arrive in a school where they have basically not met most of their peers. While it is unclear whether getting almost only new peers is good or not for students, this paper is the first to investigate empirically this issue.

To this purpose, we develop an identification strategy based on the conditional random assignment of 10th grade students to classes who share very similar registration files. We provide convincing evidence that the key assumption behind this strategy is credible. Then two different estimation approaches on distinct samples are implemented, examining the consequences of within high-school exogenous variation in the number of "persistent classmates", *i.e.* 10th grade classmates who were already in one's class in 9th grade. Both approaches show a tremendous effect of a student's familiarity with their 10th grade classmates, not only on 10th grade achievement, but also on educational outcomes in 11th and 12th grade. Furthermore, the diversity of contexts in which the natural experiment we use occurs allows us to study precisely the distributional pattern. We find that the strong and positive effect of familiarity with classmates is concentrated on students who are likely to face more difficulties during the transition to high school: low-ability students with a low SES who arrive in a high school that is much higher in quality than their middle school of origin.

Obviously, former classmates help precisely these students who may suffer from a hard disruption in their social and cultural environment and from a strong raise in academic expectations. Being surrounded by classmates they already know might help to feel more secure psychologically, thus fostering their self-confidence and sense of belonging in their new high school. As a consequence, students may be inclined to devote themselves more to academic work, and less to the reconstruction of their social network. It might also be easier for them to find friends to work with during courses. In this way, grouping students who come from the same class may be an efficient tool to help teachers to develop cooperative learning within the classroom, as they might rely on existing friendships and social links between students right from the start of the year. Further research is needed to understand why students benefit that much from higher familiarity with their peers, by investigating its implication on students' social interactions and integration in class and school. In any case, grouping students who already know each other may be a powerful lever to mitigate school inequalities and to increase overall educational achievement in high school.

Finally, the present paper puts into question the relevance of the structural transition between middle and high school during secondary education grades in many countries. The results suggest that comprehensive schools that include all secondary education grades may be less detrimental to low-ability students and may foster their completion of secondary schooling. Note that 30 % of private middle schools in France also include high school grades, against only 5 % of public middle schools. This is one potential explanation of the relative attractiveness and performance of private schools compared to public schools, while the existing literature on this subject has mainly focused on school resources and the social composition of the students' body. More empirical investigations on the role played by grade structures in school systems are thus needed. In particular, it would be interesting to study whether the short term difficulties of the transition between middle and high school might have positive long-term effects, by preparing students to the following transition to higher education.

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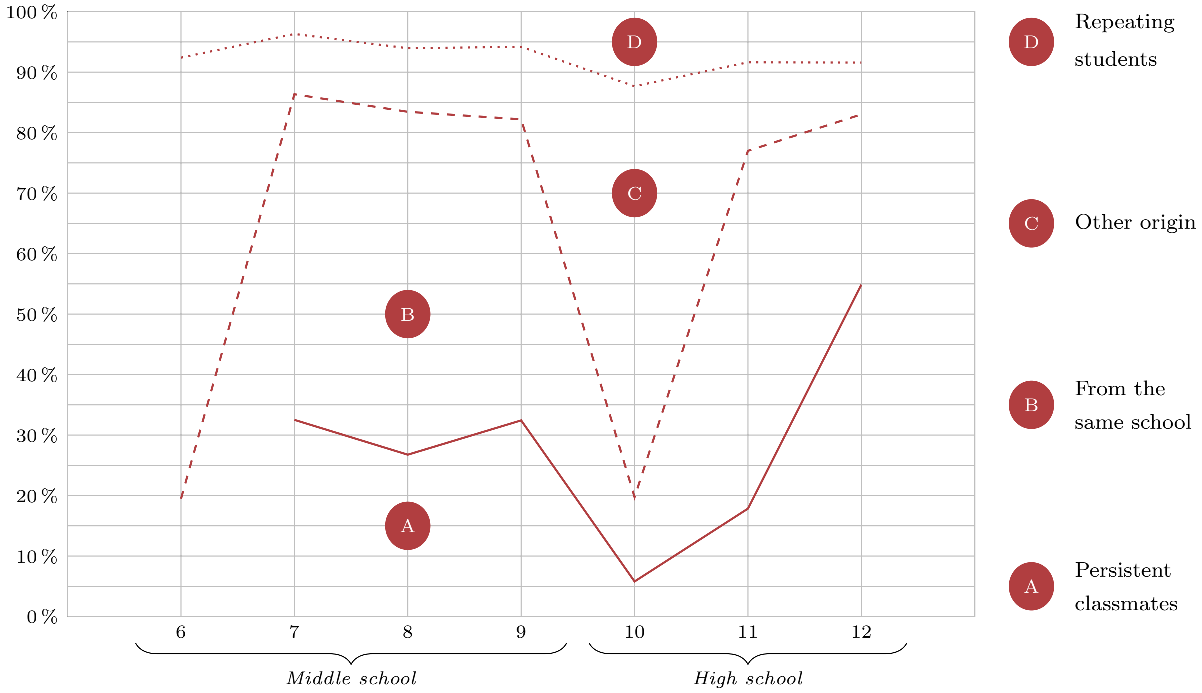
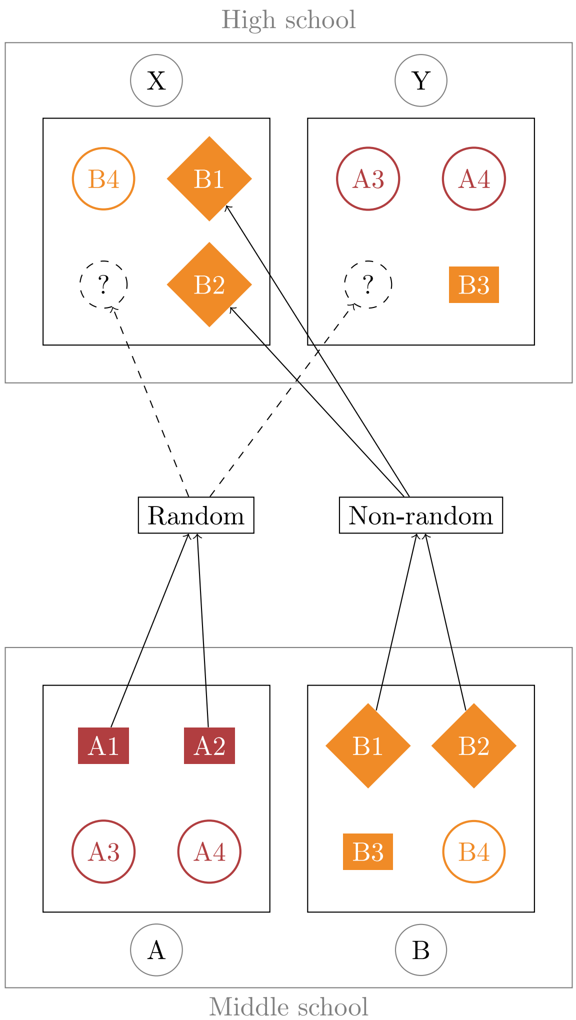
FIGURE I: COMPOSITION OF THE TYPICAL CLASSROOM FROM A NON-RETAINED STUDENT'S POINT OF VIEW



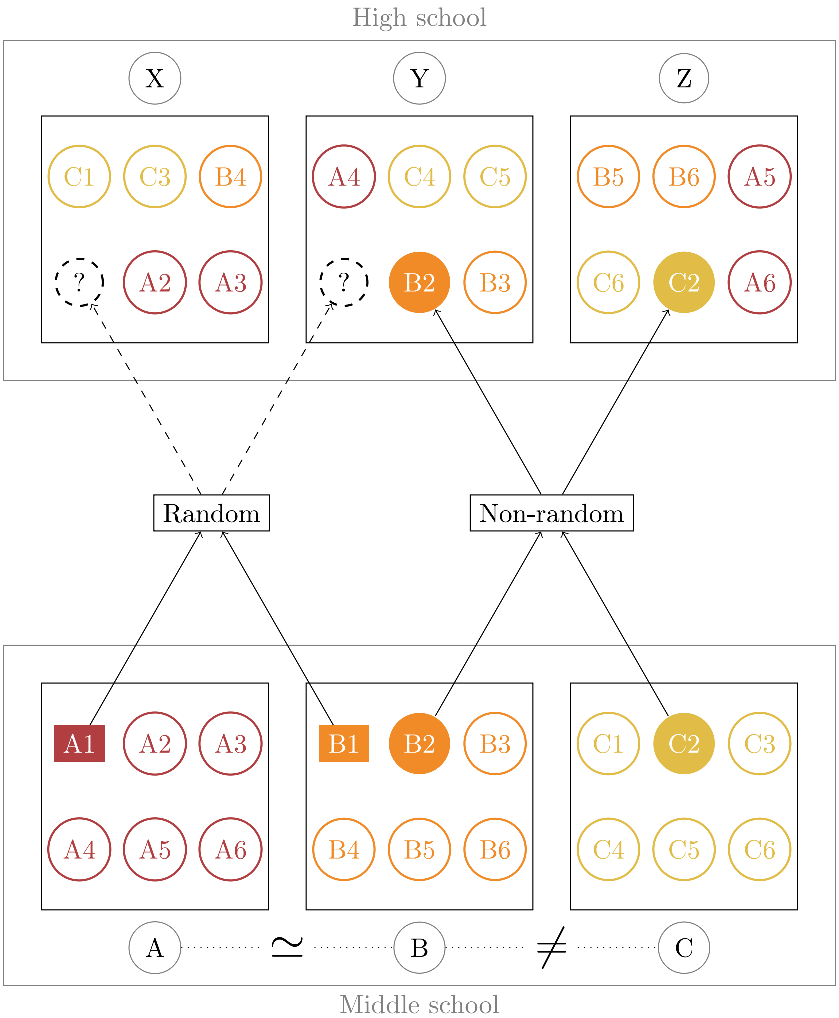
Sample for grade g consists of all students entering grade g between years $1994 + g$ and $2001 + g$. First year missing for grades 6 and 7; last year missing for grade 12.

Figure II: The assigment of SF-S students



Students represented by blank circles have generic characteristics. A1 and A2 have similar characteristics, come from the same 9th grade, as do B1 and B2. The principal chose to allocate B1 and B2 in the same class whereas they split A1 and A2 for some unknown reason: for the couple of split students, the allocation is assumed random. The allocation of B3, which is similar to A1 and A2 but comes from another class, is not assumed to be random.

Figure III: The assigment of SF-D students



Students represented by blank circles have generic characteristics. A1 and B1 have similar characteristics and come from similar 9th grade classes: they are randomly assigned between 10th grade classes X and Y. B2 and C2 also have similar characteristics but come from different classes (*e.g.* one is proposing a "high-quality" option and not the other, or one has significantly better scores): we don't assume that their allocation between classes Y and Z is random.

Table I: Descriptive statistics on students' characteristics

	Population			SF sample		
	All	Low-ab.	High-ab.	All	Low-ab.	High-ab.
	(I)	(II)	(III)	(IV)	(V)	(VI)
Girl	0.547	0.523	0.574	0.618	0.567	0.652
	(0.498)	(0.499)	(0.494)	(0.486)	(0.496)	(0.476)
Upperclass	0.306	0.251	0.360	0.301	0.211	0.363
	(0.461)	(0.434)	(0.480)	(0.459)	(0.408)	(0.481)
High quality optional course	0.150	0.087	0.212	0.093	0.028	0.138
	(0.357)	(0.281)	(0.409)	(0.291)	(0.164)	(0.345)
DNB national exam score	23.935	20.972	26.834	25.136	20.755	28.132
	(5.077)	(3.901)	(4.372)	(5.554)	(4.046)	(4.318)
In advance	0.055	0.027	0.083	0.065	0.030	0.089
	(0.228)	(0.163)	(0.275)	(0.246)	(0.170)	(0.285)
Held back	0.102	0.175	0.029	0.037	0.089	0.002
	(0.302)	(0.380)	(0.169)	(0.190)	(0.284)	(0.048)
Repeats 10th grade	0.153	0.274	0.033	0.150	0.338	0.020
	(0.360)	(0.446)	(0.179)	(0.357)	(0.473)	(0.141)
Academic major in gr. 11	0.620	0.377	0.859	0.693	0.361	0.920
	(0.485)	(0.485)	(0.348)	(0.461)	(0.480)	(0.271)
Science major	0.343	0.120	0.561	0.446	0.112	0.674
	(0.475)	(0.325)	(0.496)	(0.497)	(0.316)	(0.469)
N	2,888,258	1,425,929	1,462,329	28,140	11,436	16,704

Standard deviations are reported in parentheses.

Table II: Descriptive statistics on the number of former classmates

	Population			SF sample		
	All	Low-ab.	High-ab.	All	Low-ab.	High-ab.
	(I)	(II)	(III)	(IV)	(V)	(VI)
Persistent classmates (PC)	1.721	1.465	1.977	1.999	1.740	2.177
	(2.523)	(2.218)	(2.772)	(2.258)	(2.024)	(2.389)
Same sex PC	0.998	0.848	1.147	1.163	0.988	1.283
	(1.606)	(1.430)	(1.751)	(1.513)	(1.338)	(1.610)
Opposite sex PC	0.724	0.618	0.831	0.837	0.752	0.895
	(1.370)	(1.219)	(1.498)	(1.278)	(1.190)	(1.331)
High ability PC	0.976	0.812	1.137	1.120	0.911	1.263
	(1.733)	(1.535)	(1.896)	(1.585)	(1.366)	(1.704)
Low-ability PC	0.746	0.653	0.840	0.879	0.828	0.914
	(1.238)	(1.090)	(1.362)	(1.212)	(1.122)	(1.268)
Former classmates (FC)	8.325	7.766	8.895	12.679	12.052	13.108
in high school (HS)	(6.431)	(6.258)	(6.554)	(5.925)	(5.754)	(6.001)
Same sex FC in HS	4.376	4.048	4.706	7.257	6.804	7.567
	(3.725)	(3.587)	(3.832)	(3.830)	(3.620)	(3.937)
Opposite sex FC in HS	3.949	3.718	4.189	5.422	5.248	5.541
	(3.596)	(3.518)	(3.658)	(3.544)	(3.517)	(3.558)
High ability FC in HS	4.435	4.458	4.419	6.767	6.102	7.222
	(4.057)	(4.172)	(3.940)	(4.116)	(3.956)	(4.162)
Low-ability FC in HS	3.890	3.308	4.475	5.911	5.949	5.885
	(3.332)	(2.907)	(3.602)	(3.254)	(3.023)	(3.403)
PC between grades 8 and 9	9.205	8.443	9.944	9.699	8.273	10.653
(PC-8to9)	(6.626)	(6.419)	(6.744)	(7.096)	(6.566)	(7.275)
N	2,888,258	1,425,929	1,462,329	28,140	11,436	16,704

Standard deviations are reported in parentheses.

Table III: Student's classroom characteristics regressed on own anonymous exam score: Evidence of the random assignment of similar-file students

(A) Class composition

	All	All
Dependent variable	(I)	(II)
Persistent classmates (PC)	0.062***	0.000
	(0.002)	(0.005)
Normalized DNB score	0.040***	0.001
	(0.001)	(0.001)
Share of girls	-0.001***	-0.000
	(0.000)	(0.000)
Share of upperclass students	0.009***	0.000
	(0.000)	(0.000)
Class size	0.067***	0.002
	(0.004)	(0.006)
N	28,095	28,095
SF fixed effect	No	Yes

(B) Number of former classmates of each type

	All	Low-ab.	High-ab.	All	Low-ab.	High-ab.
Dependent variable	(I)	(II)	(III)	(IV)	(V)	(VI)
Any PC	0.062***	0.083***	0.074***	0.000	0.003	-0.001
	(0.002)	(0.005)	(0.004)	(0.005)	(0.008)	(0.008)
Same sex PC	0.037***	0.045***	0.042***	0.004	0.003	0.004
	(0.002)	(0.003)	(0.003)	(0.004)	(0.006)	(0.005)
Opposite sex PC	0.025***	0.039***	0.032***	-0.003	-0.001	-0.005
	(0.001)	(0.003)	(0.002)	(0.003)	(0.005)	(0.005)
Low-ability PC	0.012***	0.026***	0.010***	-0.004	0.001	-0.007
	(0.001)	(0.003)	(0.002)	(0.003)	(0.005)	(0.004)
High-ability PC	0.050***	0.058***	0.063***	0.005	0.002	0.005
	(0.002)	(0.003)	(0.003)	(0.004)	(0.006)	(0.006)
N	28,095	11,409	16,686	28,095	11,409	16,686
SF-S fixed effect	No	No	No	Yes	Yes	Yes

The normalization is done over the whole population; the sample's mean is 0.245.
Each cell is from a separate regression of the classroom characteristic of interest on the student's standardized average anonymous score at the DNB exam. All regressions include quadratic controls for the share of retained students and of missing DNB scores. Robust standard errors are reported in parentheses.

Table IV: Effect of the number of Persistent Classmates and other classroom characteristics on 10th grade outcomes

	Full sample	Low-ab.	High-ab.	Full sample	Low-ab.	High-ab.
Dependent variable	(I)	(II)	(III)	(IV)	(V)	(VI)
(A) Outcome: Repeats 10th grade						
PC	-0.003***	-0.009***	-0.000	-0.003**	-0.009***	-0.000
	(0.001)	(0.003)	(0.001)	(0.001)	(0.003)	(0.001)
Average DNB score				0.040***	0.064***	0.020***
				(0.010)	(0.022)	(0.006)
Share of girls				-0.046**	-0.127**	0.010
				(0.023)	(0.053)	(0.014)
Share of upperclass students				-0.013	-0.012	-0.013
				(0.029)	(0.070)	(0.016)
Class size				-0.001	-0.003	-0.000
				(0.001)	(0.003)	(0.001)
(B) Outcome: Academic major						
PC	0.003**	0.005*	0.001	0.003**	0.005	0.001
	(0.001)	(0.003)	(0.001)	(0.001)	(0.003)	(0.001)
Average DNB score				-0.023**	-0.047**	-0.003
				(0.011)	(0.020)	(0.011)
Share of girls				0.017	0.049	-0.009
				(0.025)	(0.049)	(0.026)
Share of upperclass students				0.001	-0.031	0.021
				(0.032)	(0.068)	(0.030)
Class size				0.001	0.001	0.000
				(0.001)	(0.002)	(0.001)
(C) Outcome: Technological major						
PC	0.001	0.005**	-0.001	0.001	0.005**	-0.001
	(0.001)	(0.003)	(0.001)	(0.001)	(0.003)	(0.001)
Average DNB score				-0.005	-0.006	-0.004
				(0.010)	(0.019)	(0.008)
Share of girls				0.010	0.033	-0.003
				(0.023)	(0.050)	(0.019)
Share of upperclass students				-0.031	-0.020	-0.036*
				(0.028)	(0.064)	(0.022)
Class size				-0.000	-0.001	0.000
				(0.001)	(0.002)	(0.001)
Control for other peer effects	No	No	No	Yes	Yes	Yes
N	28,140	11,436	16,704	28,095	11,409	16,686

Each column in a panel is from a separate regression of students' outcomes on their classroom characteristics. All regressions include similar-file fixed effects and controls for the share of retained students and of missing DNB scores. Robust standard errors are reported in parentheses.

Table V: Effect of class characteristics on the choice of major.

	Academic major				Technological major				
	Any	Sci.	Soc.	Hum.	Any	Ind.	Adm.	Health	Other
Independent variable	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)
PC	0.005	0.001	0.002	0.001	0.006**	0.001	0.004*	-0.001	0.001
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.001)	(0.003)	(0.001)	(0.001)
N	11,409	11,409	11,409	11,409	11,409	11,409	11,409	11,409	11,409

Sample: Low-ability students only.
Each column is from a separate regression. All regressions include similar-file fixed effects and quadratic controls for class characteristics. Robust standard errors are reported in parentheses.

Table VI: Effect of the number of Persistent Classmates on end of high school outcomes

	Takes *Bac* in time	*Bac* score	*Bac* graduate
Independent variable	(I)	(II)	(III)
PC	0.014***	0.011**	0.007
	(0.005)	(0.004)	(0.014)
N	8,002[1]	8,002[1]	3,822[2]

[1] Sample: low-ability students from 6 first cohorts out of 8 (no *Bac* data for the last two).

[2] Effect on *Bac* score measured only on those taking the exam three years after entering school. The matching procedure generated some attrition, which is linked to administrative procedures and considered exogenous.

Each cell is from a separate regression of students' Bac outcomes on their number of PC. All regressions include similar-file fixed effects and quadratic controls for class characteristics. Robust standard errors are reported in parentheses.

Table VII: Effect of PC and of middle school mates from other classes on 10th grade outcomes

	Full sample	Low-ab.	High-ab.
Independent variable	(I)	(II)	(III)
(A) Outcome: repeats 10th grade			
PC	-0.003***	-0.009***	-0.000
	(0.001)	(0.003)	(0.001)
Former mates from other classes	-0.001	-0.002	-0.000
	(0.001)	(0.002)	(0.001)
(B) Outcome: academic major			
PC	0.003*	0.005	0.001
	(0.001)	(0.003)	(0.001)
Former mates from other classes	-0.000	0.001	-0.001
	(0.001)	(0.002)	(0.001)
(C) Outcome: technological major			
PC	0.001	0.005*	-0.000
	(0.001)	(0.003)	(0.001)
Former mates from other classes	0.000	-0.001	0.001
	(0.001)	(0.002)	(0.001)
N	28,095	11,409	16,686

Each column in each panel is from a separate regression of students' outcomes on former mates of each type. All regressions include similar-file fixed effects and quadratic controls for class characteristics. Robust standard errors are reported in parentheses.

Table VIII: Robustness check: effect of the number of friends kept between grades 8 and 9

	All	Low-ab.	High-ab.
Dependent variable	(I)	(II)	(III)
(A) Correlation with PC			
PC	0.003	-0.002	0.006
	(0.004)	(0.006)	(0.006)
(B) Academic outcomes			
Repeats 10th grade	-0.000	0.000	-0.001
	(0.001)	(0.002)	(0.001)
Academic major	0.000	-0.000	0.001
	(0.001)	(0.001)	(0.001)
Technological major	-0.000	0.000	-0.000
	(0.001)	(0.001)	(0.001)
N	27,496	11,014	16,482

Each cell is from a separate regression of PC or student's outcomes on the number of persistent classmates between grades 8 and 9 (PC-8to9). All regressions include similar-file fixed effects and quadratic controls for class characteristics. Robust standard errors are reported in parentheses.

Table IX: Robustness check: effect of PC on low-ability students' 10th grade outcomes by cohort

	Cohort							
	2004	2005	2006	2007	2008	2009	2010	2011
Dependent variable	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Repeats 10th grade	-0.008	-0.015	-0.018*	-0.009	-0.005	-0.005	-0.013	-0.004
	(0.009)	(0.010)	(0.011)	(0.008)	(0.010)	(0.007)	(0.013)	(0.012)
Academic major	0.012	-0.007	0.010	0.010	0.005	-0.008	0.023*	0.010
	(0.009)	(0.010)	(0.010)	(0.008)	(0.009)	(0.008)	(0.013)	(0.011)
Technological major	0.005	0.022***	0.007	0.004	0.006	0.007	-0.009	-0.008
	(0.007)	(0.008)	(0.008)	(0.007)	(0.008)	(0.006)	(0.011)	(0.011)
N	1,714	1,582	1,661	1,567	1,478	1,607	836[1]	964[1]

Sample: low-ability students only.
The year indicated in the first row is the year of entry in grade 10.
Each cell is from a separate regression. All regressions include observable-characteristics fixed effects. Robust standard errors are reported in parentheses.

[1] The sample size is quite smaller for the last two cohorts because of a reform of high school that increased the number of choice-specific courses, thus decreasing the number of SF students.

Table X: Robustness check: effect of PC on low-ability students' retention rate using different specification of the SF fixed effect

	Specifications				
Independent variable	(I)[1]	(II)[2]	(III)[3]	(IV)[4]	(V)[5]
PC	-0.004***	-0.007***	-0.007**	-0.009***	-0.013**
	(0.001)	(0.002)	(0.005)	(0.003)	(0.005)
N	241,601	28,121	15,395	11,409	4,112

Sample: low-ability students only.
Each cell is from a separate regression of grade retention on PC. All regressions include similar-file fixed effects (different in eachc column) and quadratic controls for class characteristics. Robust standard errors are reported in parentheses.
Column III is the original specification, columns I-II are less restrictive and columns IV-V are more restrictive.

[1] X = (Decile of in-school score, Student is repeating, Set of options, Middle school attended).

[2] X = (Quintile of in-school score in science, Quintile of in-school score in humanities, Decile of in-school score, Student is repeating, Set of options, Middle school attended, 9th grade class attended).

[3] X = (Quintile of in-school score in science, Quintile of in-school score in humanities, Decile of in-school score, Student is repeating, Gender, Two-category social origin, Set of options, Middle school attended,).

[4] Original specification as described in section 3.1.1, *i.e.* X = (Quintile of in-school score in science, Quintile of in-school score in humanities, Decile of in-school score, Student is repeating, Gender, Two-category social origin, Set of options, Middle school attended, 9th grade class attended).

[5] X = (Decile of in-school score in science, Decile of in-school score in humanities, Decile of in-school score, Student is repeating, Gender, Two-category social origin, Set of options, Middle school attended, 9th grade class attended).

TABLE XI: EFFECT OF PC ON LOW-ABILITY STUDENTS' 10TH GRADE OUTCOMES BY GENDER AND SES

	All	**Male**	**Female**
Dependent variable	(I)	(II)	(III)
(A) All low-ability students			
Repeats 10th grade	-0.009***	-0.010**	-0.007
	(0.003)	(0.005)	(0.005)
Academic major	0.005	0.008*	0.002
	(0.003)	(0.005)	(0.005)
Technological track	0.006**	0.004	0.006*
	(0.003)	(0.004)	(0.004)
N	11,409	4,938	6,471
(B) Low-SES students			
Repeats 10th grade	-0.014***	-0.018***	-0.010*
	(0.004)	(0.007)	(0.006)
Academic major	0.008**	0.014**	0.004
	(0.004)	(0.006)	(0.005)
Technological track	0.007*	0.004	0.008*
	(0.004)	(0.005)	(0.005)
N	9,004	3,519	5,485
(C) High-SES students			
Repeats 10th grade	0.002	0.003	0.001
	(0.005)	(0.007)	(0.007)
Academic major	-0.003	-0.001	-0.003
	(0.005)	(0.007)	(0.008)
Technological track	0.003	0.005	-0.000
	(0.003)	(0.005)	(0.004)
N	2,405	1,419	986

Sample: low-ability students only.
Each column is from a separate regression of students' outcomes on PC. All regressions include similar-file fixed effects and quadratic controls for class characteristics. Robust standard errors are reported in parentheses.

Table XII: Effect of PC on low-ability students as a function of the gap between middle school and high school share of high-SES students

	All	$\Delta p \leq 0$	$0 < \Delta p \leq 11\,\%$	$\Delta p > 11\,\%$
Dependent variable	(I)	(II)	(III)	(IV)
Repeats 10th grade	-0.009***	-0.003	-0.014	-0.022**
	(0.003)	(0.010)	(0.011)	(0.011)
Academic major	0.005	0.002	0.008	0.006
	(0.003)	(0.010)	(0.011)	(0.011)
Technological major	0.006**	0.008	-0.002	0.012
	(0.003)	(0.008)	(0.008)	(0.009)
N	11,409	3,945	3,852	3,612

Sample: low-ability students only.
Δp is the difference between the middle school and high school shares of high-SES students. Each cell is from a separate regression of students' outcomes on PC. All regressions include observable-characteristics fixed effects. Robust standard errors are reported in parentheses.

Table XIII: Effect of PC on low-ability students' 10th grade outcomes using non-linear specifications

	Threshold t					Quad.
	$t = 1$	$t = 2$	$t = 3$	$t = 4$	$t = 5$	
Independent variable	(I)	(II)	(III)	(IV)	(V)	(VI)
(A) Outcome: repeats 10th grade						
PC	-0.009**	-0.004	-0.011	-0.008	-0.009*	-.011*
	(0.004)	(0.015)	(0.009)	(0.007)	(0.005)	(.006)
PC $> t$	0.002	-0.008	-0.009	-0.039	-0.077	
	(0.016)	(0.020)	(0.028)	(0.038)	(0.057)	
PC $\times$ (PC $> t$)		-0.003	0.004	0.005	0.011	
		(0.016)	(0.010)	(0.009)	(0.009)	
PC^2						.000
						(.001)
(B) Outcome: academic major						
PC	0.006*	-0.004	-0.003	-0.001	0.003	.006
	(0.004)	(0.014)	(0.008)	(0.006)	(0.005)	.006
PC $> t$	-0.011	-0.014	0.003	0.060	0.112*	
	(0.014)	(0.019)	(0.028)	(0.039)	(0.061)	
PC $\times$ (PC $> t$)		0.011	0.007	-0.002	-0.013	
		(0.014)	(0.010)	(0.009)	(0.010)	
PC^2						-.000
						(.001)
(C) Outcome: technological major						
PC	0.003	0.021	0.016*	0.009	0.008*	.009
	(0.003)	(0.014)	(0.008)	(0.006)	(0.005)	(.005)
PC $> t$	0.019	0.021	0.006	0.017	-0.002	
	(0.014)	(0.018)	(0.022)	(0.028)	(0.041)	
PC $\times$ (PC $> t$)		-0.018	-0.011	-0.006	-0.004	
		(0.015)	(0.009)	(0.007)	(0.007)	
PC^2						-.000
						(.000)
N	11,409	11,409	11,409	11,409	11,409	11,409

Sample: low-ability students only.
Each column in each panel is from a separate regression. All regressions include similar-file fixed effects and quadratic controls for class characteristics. Robust standard errors are reported in parentheses.

Table XIV: Effect of PC decomposed by PC's gender or ability on low-ability students' 10th grade outcomes

	Gender	Ability
Independent variable	(I)	(II)
(A) Outcome: repeats 10th grade		
Same	-0.006	-0.010*
	(0.005)	(0.006)
Different	-0.012**	-0.008*
	(0.005)	(0.005)
(B) Outcome: academic major		
Same	0.004	0.007
	(0.005)	(0.005)
Different	0.006	0.003
	(0.005)	(0.005)
(C) Outcome: technological major		
Same	0.006	0.004
	(0.004)	(0.005)
Different	0.006	0.007*
	(0.004)	(0.004)
N	11,409	11,409

Each column in each panel is from a separate regression of students' outcomes on PC of each type. All regressions include similar-file fixed effects and quadratic controls for class characteristics. Robust standard errors are reported in parentheses.

Table XV: IV exogeneity test

	All	All
Independent variable	(I)	(II)
Held back	-0.004	-0.008
	(0.016)	(0.016)
Girl	-0.012	0.003
	(0.008)	(0.009)
Upperclass	0.065***	0.009
	(0.009)	(0.009)
High quality optional course	0.043***	0.002
	(0.012)	(0.022)
DNB national exam score	0.006***	0.002
	(0.001)	(0.001)
N	28,664	28,664
SF fixed effect	No	Yes

Each column is from a separate regression of the instrument Z on students' characteristics. All regressions include high-school fixed effects. Robust standard errors are reported in parentheses.

Table XVI: Effect of PC on short-term and long-term outcomes using the IV strategy

Dependent variable	(I)	(II)	(III)
Repeats 10th grade	-0.009***	-0.006**	-0.007***
	(0.002)	(0.003)	(0.002)
Academic major	0.012***	0.008**	0.010***
	(0.003)	(0.003)	(0.003)
Technological major	-0.004**	-0.004**	-0.003
	(0.002)	(0.002)	(0.002)
Takes *Bac* in time[1]	0.011**	0.005	0.010**
	(0.004)	(0.004)	(0.004)
Bac graduate[1]	0.014***	0.009*	0.011**
	(0.004)	(0.005)	(0.004)
Fixed effect	High-school	HS × 9th grade class	10th grade class
N	33,663	33,663	33,663

Each cell is from a separate regression of students' outcomes on former mates of each type. All regressions include similar-file fixed effects and quadratic controls for class characteristics. Robust standard errors are reported in parentheses.

[1] Estimated only over the first 6 cohorts (no *Bac* data for the last two), *i.e.* 22,384 students.