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Abstract

We use a reform that was recently implemented in Italy to investigate the effects on academic achievement of more stringent requirements for the admission to the next grade at upper secondary school. We study how such effects are mediated by changes in family and school inputs, and in the student commitment to learn all school subjects including those usually considered as marginal components of the curriculum. Geographical discontinuities in the implementation of the reform allow us to set out the comparison of similar students undergoing alternative progression rules, and to shed light on whether, and to what extent, the reform has worked as a tool to improve short-term achievement gains. We document differential effects across curricular tracks, picturing at best - depending of the data employed - a marginal improvement for students in academic schools. We instead find sharp negative effects of the reform in technical and vocational schools, where the students enrolled come from less privileged backgrounds. These findings are accompanied by a substantial increase in the number of activities out of the normal school hours in technical and vocational schools, but not in academic schools. Also, we find that the reform has left unchanged the various family inputs that we consider, and that parents did not provide extra economic support to students facing an increased threat of grade retention. However, in contrast with the documented effects on achievement, we find that schools reacted to the additional administrative burdens and costs imposed by the reform by admitting more students to the next grade. We thus conclude that the reform has had a negative effect on motivation and engagement of the most struggling students, thus exacerbating existing inequalities.

Keywords: policy evaluation, quasi experimental designs, remedial education

JEL code: C31, I24, I28

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1. Introduction

Increasing concerns about the quality of education in Europe and the United States have lead in the recent years to the implementation of accountability policies designed to hold administrators, teachers and students responsible for the level of academic achievement. This strategy reflects the belief that the promise of rewards or the threat of sanctions is needed to ensure change, setting clear standards and tools to promote educational change (Hamilton, 2003). This paper assesses the effectiveness of a remedial education reform that was recently introduced in Italian upper secondary schools with the aim of improving student achievement. The intervention considered shares with accountability policies the central assumption that sanctions may be an effective tool to enhance performance.

Starting from the school year 2007/08, students in upper secondary schools of the country who don't meet predefined performance levels must attend remedial summer courses, and their progression to next grade is conditional on passing a remedial exam before the beginning of the new year. Remedial education assumes a more important role in the formative plan, and is made compulsory during the school year for low performing students. This new progression rule replaced the old system, in which students who were not retained could be admitted to the next grade with 'educational debts' in one or more subjects to be cleared with no clear deadline. According to this rule, the practice of social promotion was effectively at work.

The policy question addressed in this paper is whether mandating remedial summer courses for those deemed in need of such courses, and testing students after the summer before admitting them to the next grade, makes a difference. In particular, we study the short-term effects on student achievement, and how such effects are mediated by changes in school and family inputs that are indirectly caused by the intervention. Our empirical strategy exploits the quasi experimental variation that results from *geographical discontinuity* in the implementation of the reform. Unlike the rest of the country, schools located in a well-defined area of Northern Italy, the province of Trento, were exempted from adopting the new progression system. We make use of this setting, and obtain counterfactual quantities that we employ to quantify the effects of the reform.

Remedial exams were introduced in Italian schools in 1923, and were abolished in upper secondary schools during the 1990s. The policy rationale for their reintroduction in 2007 resulted from a combination of scientific and political discussion on the evidence from the first three waves of PISA of low performance of Italian students. Variability in test scores across regions pictured a sharp North/South divide, with students in Northern areas, amongst which the province of Trento, performing well above the OECD average.1 On top of this geographical variability, marked differences emerged across curricular tracks even in high performing areas, students in vocational schools being the most problematic. The

[1] As we shall see, it was mostly this evidence that motivated the discontinuity in the geographic roll out of the reform, although Trento was the only autonomous province in Northern Italy not complying with the new system.

policy implemented was therefore intended largely to provide strong incentives to students, teachers and parents, thus reinforcing discipline and, though this, academic achievement.

Advocates of the reform believed that the threat of grade retention was the most effective device to control for low school performance. According to this interpretation, students should study more intensely and, indirectly, achieve higher levels of proficiency because individuals instinctively fair failure. From a theoretical point of view, this assumption echoes the reinforcement theory originally developed by the behaviourist school of psychology (see, for example, Staddon, 2003). However, the stimulus-response mechanism alone may not be sufficient to account for all outcomes observed in learning situations.[2] By adopting this point of view, reform opponents raised the concern that the threat of grade retention might undermine effort, motivation and engagement of struggling students, thus exacerbating existing inequalities.

As a matter of fact, the desirability of grade retention policies as a method for remediating poor performance is not uncontroversial. The recent push for educational accountability has brought this policy problem back to the forefront. Despite the large number of studies that have looked into this issue, evidence from quasi-experimental designs is relatively scarce (notable exceptions are Jacob and Lefgren, 2004 and 2009). If one considers only studies rigorously designed to control for selection bias, the available evidence fails to demonstrate that grade retention is more beneficial than grade promotion, for both academic and socio-emotional outcomes (see Jimerson, 2001, for a comprehensive review of empirical findings). This paper marks something of a departure from this literature, as we do not seek identification of the causal effects of retention on student outcomes. The quasi experimental comparison of outcomes for students undergoing different progression rules is revealing of the threat.[3]

Overlaid to the dimension represented by retention policies, another relevant stream of the literature that we touch upon is that investigating the effectiveness of remedial education. The reform introduced clear requirements on the school side about the organization of remedial courses for low achieving students, both during the school year and in the summer for those mandated to the remedial exam. In investigating the *reduced form* effects of the reform, we compare outcomes in areas implementing different progression rules without distinguishing the relative merits of remedial instruction time vis-à-vis the increased threat of grade retention. Disentangling the causal effects of these two channels calls for empirical evidence on remedial education for underperforming students. However, rigorous research in this direction is still scanty, and points to mixed results. Lavy and

[2] The empirical evidence available suggests that intrinsic motivation (Fortier et al., 1996; Pintrich, 2003), social origins (Shavit and Blossfeld, 1993; Breen and Goldthorpe 1997; Bowles and Gintis 2002), parents' behaviour and expectations (Englund et al., 2004), teachers' expectations (Rosenthal and Jacobson, 1968; Saracho, 1991; Rubie-Davies et al., 2006), and teachers' classroom assessment practices (McMillan, 2001) are inputs playing a pivotal role in the learning process, yielding heterogeneous reactions to punishment practices.

[3] To the best of our knowledge, the closest in spirit to our paper is the work by Belot and Vandenberghe (2011), who study the effects of the threat of grade retention introduced by a reform implemented for the French speaking community in Belgium finding no effects on achievement gains.

Schlosser (2005) quantify the effects of a remedial intervention for high school students in Israel, finding a significant increase in the school mean matriculation rate. Calcagno and Long (2008) look at the impact of post-secondary remediation programmes in Florida, finding that mathematics and reading courses have mixed benefits on college performance. Battistin and Meroni (2012) investigate the short term effects for low achieving students in Italian lower secondary schools, and document positive results for mathematics but not for reading.

Our analysis adds to the empirical findings documented in economics, sociology and psychology on the interplay between incentives faced by students and academic achievement. Effort, total time devoted to study and engagement at school were found to be important determinants of student learning (see, for example, the review by Bishop, 2004). Students choose which subject to focus on, and decide how much effort to put into each task. Depending on the incentives facing students, one may expect sizeable differences in decisions about effort. The available empirical evidence suggests that determinants of effort may vary a great deal across school tracks. For example, Carbonaro (2005) finds that students in higher curricular tracks exert substantially more effort than do students in lower tracks. This, of course, may simply reflect differences in effort explained by sorting of students into tracks. However, given the very rich set of background and school characteristics controlled for in the analysis, Carbonaro (2005) claims that the differences documented are suggestive of track specific effects on effort. This evidence is reinforced if one considers the work by Hastings *et al.* (2012), who provide quasi-experimental evidence that school choice has sizeable effects on motivation and academic performance for low income and minority students. It is well documented that setting higher standards in schools may induce heterogeneous effects on effort, having adverse consequences on students for whom standards move beyond their reach. For example, Betts and Grogger (2003) find that high standards have significantly larger returns on test scores at the top end of the ability distribution. This result may be mediated by differential effects on effort, as students at the bottom end of the distribution may perceive themselves as losing ground and give up.[4]

Gender differences in decisions about effort are a relevant dimension to consider. The attitude of female students is more supportive of academic learning than that of their male peers (see, for example, Carbonaro, 2005). This implies that there might be positive externalities in classes or schools with higher percentage of females. Lavy and Schlosser (2011) study the effects of classroom gender composition on academic achievement, finding that both male and female students tend to perform better in classes presenting higher percentages of females. They find heterogeneous effects depending on the socioeconomic background of students, with larger effects for the most disadvantaged groups. In documenting the channels for the existence of such gender peer effects, Lavy and Schlosser (2011) find that having more female students in the class has positive effects

[4] For example, Betts and Grogger (2003) document differential effects of setting high standards by ethnicity, with lower returns on achievement for blacks. This is consistent with findings by Carbonaro (2005), who documents, ceteris paribus, lower effort from black students.

on the learning climate and inter-student relationships, thus leading to a more efficient use of instructional time. This should affect positively non-cognitive factors like motivation and concentration and thus, indirectly, learning.

The above findings suggest that a sensible stratification to consider for the empirical analysis is by gender and curricular track. This is what we will do in documenting the main results. Technical and vocational schools in Italy are characterised by a much lower proportion of female students in the class when compared to academic schools. It is well documented in the literature that not only students perform better if their peers are high achievers, but peers can also act as a buffer by legitimising deviant behaviour. Thus, it is interesting to investigate how the increased threat of sanctions induced by the reform interacts with gender, and with differences in the learning climate. The stratification by gender is also motivated by studies that have documented gender differential in skills that may depend on the mode of assessment.[5]

Our empirical analysis is conducted using survey and administrative data from complementary sources of information that we were able to obtain for the purpose of this study. Test scores and socio-economic indicators come from a small scale survey that was commissioned purposively for the evaluation of the reform in selected schools either side of the administrative border of the province of Trento. This information is complemented with data from the PISA 2006 and 2009 surveys, as they refer to pre and post-policy periods. We were able to obtain from the Ministry of Education area identifiers for where the schools are located, not available in the public use files, so to reproduce fairly closely the same evaluation design considered for the main analysis. Finally, we use time series data coming from administrative information released by the Ministry of Education on retention rates for all schools in the areas considered for the evaluation.

The main findings of this paper can be summarized as follows. First, we find sharp differences depending on the type of school considered, and thus on the socio economic background of students. Consistently across data sources, we document negative effects of the reform on academic achievement in technical and vocational schools. As for academic schools, in the main analysis we find no statistically significant effect, which becomes positive and significant in some dimensions of learning once we employ PISA data as a sensitivity check. Because of the importance of distributional effects, we go beyond averages and assess how the intervention considered affects achievement across quantiles of the test score distributions. We find that much of the variability in the effect is captured through the stratification by curricular track, and document much lower within track differences across students. Most interestingly, we find more pronounced negative effects

[5] For example, Machin and McNally (2005) find lower achievement of female students resulting from the introduction of the National Curriculum in the United Kingdom, that set out the standards that should be achieved at different stages of the education sequence and, amongst other things, assigned more importance to continuous assessment by teachers. Gipps and Murphy (1994) report evidence that females do less well in timed examinations because of higher levels of anxiety. Powney (1996) reviews a number of studies documenting that the mode of assessment is a factor explaining the differential performance of male and female students. Pekkarinen (2012) provides evidence that the structure of the educational system affects male and female students differently, in particular with reference to tracking in secondary schools.

for females in technical and vocational tracks, where the proportion of male students is higher. The results are robust to sensitivity checks that we perform on the functional form adopted, as well as the source of identifying variability employed. Thus, consistently with the findings in Betts and Grogger (2003), our first contribution is to show that higher standards coming with the threat of sanctions contribute considerably to create inequality in the distribution of educational achievement, resulting in both winners and losers.

Second, we use PISA data to investigate the effects of the intervention on key inputs of the education production function, providing important insights on the possible mediating factors driving the results documented above. We find no significant effects of the reform on household spending for education, the bulk of which - given the public school system in Italy - consists in fees paid to individual teachers in the school or to other teachers for tutoring. According to our findings, households *ceteris paribus* did not react to the reform by providing extra support to students facing an increase in the threat of grade retention. In contrast, we find that the amount of extra time spent by students learning subjects outside of normal school hours increased after the reform. Of course this effect could be explained as a mechanical consequence of the intervention itself, being the provision of remedial classes for low achieving students compulsory on the school side. However, our results show that much of the action took place in technical and vocational schools, while in academic schools the provision of remedial classes is unaffected by the reform. From this evidence we conclude that the reform lowered the “safety” of the most struggling students, imposing substantial extra work loads only for those from low socio-economic backgrounds.

Third, we document the effects on the promotion and retention rates for the two groups of schools considered during the first three school years following the reform. We compare the status of students in June of each year in areas affected by the reform (i.e. “admitted to the next grade”, “retained” or “mandated to summer courses and the remedial exam in September”) to the status of students in areas that we use as controls (i.e. “admitted to the next grade”, “retained” or “admitted to the next grade with ‘educational debts’”). Despite the effects on achievement documented above, we find that the reform sensibly increased the percentage of students in technical and vocational schools admitted to the next grade in June of each year. The same conclusion holds for academic schools, although the results documented are only marginally significant. On the contrary, we find - consistently across curricular tracks - no effect on retention rates in June, thus concluding that schools reacted to the reform by admitting to the next grade students who, before, would have been given an ‘educational debt’. Since the mandatory organisation of remedial summer classes for low achieving students impacts importantly on school budgets, we interpret this result as adaptive behaviour that resulted in less stringent rules to pass students to the next grade. Thus we conclude that the effects documented on achievement are driven by important changes in school inputs. Our findings highlight the importance of providing schools with sufficient resources to support general reforms of the school system that are aimed at enhancing the competences of students. The relevance of this problem for policy making is particularly important for students and areas facing marked socio-economic deprivation, and thus being at risk of lagging behind in their development.

Finally, we perform back of the envelope calculations to infer the long term effects of the reform on graduation rates at upper secondary school. To this end, we exploit a different education reform that took place in the country during the 1990s and, curiously enough, represents the mirror image of the intervention considered in this paper. The reform was rolled out starting from the school year 1994/95, and introduced the practice of the 'educational debt' by abolishing the same remedial exam in September that was again introduced in 2007/08. We use data from the Bank of Italy Household Survey on Income and Wealth to set out the comparison of cohorts of individuals aged 14 during the 1990s, 14 being the normal age for completing compulsory schooling at the time. Bearing in mind some important differences between the two reforms that we discuss in what follows, the cohort study that we set out points to no effects of the threat of grade retention on attainment of the upper secondary school diploma.[6]

The remainder of the paper is organised as follows. The main features of the policy under evaluation are discussed in Section 2. Section 3 presents the data, while the evaluation design is illustrated in Section 4. Results are reported in Section 5. Conclusions and policy implication are discussed in Section 6.

2. *Background*

Until the school year 2006/07, students in upper secondary schools in Italy who were not retained could be admitted to the next grade with or without an 'educational debt' (*debito formativo*), that is a final mark signaling the lack of predefined performance level in one or more subjects. Such lack in achievement was given at the end of the school year (mid-June), and was to be cleared in the following years with *no clear* deadline. The system at work *de facto* resulted in the practice of social promotion. Official figures provided by the Italian Ministry of Education show that about 42 percent of students enrolled at high school in the country were given at least one educational debt, with just one out of four students recovering it by the end of the following year.

Table 1 presents descriptive statistics for the school year 2006/07 derived by the Ministry of Education, using all upper secondary schools in the areas considered for the main analysis. Separately for academic and technical/vocational schools, reported are the percentage of students retained, the percentage of students with at least one educational debt and the end of the school year, and a breakdown of debts assigned by subject. Results

[6] A similar result is found by Belot and Vandenberghe (2011). Differently from the most recent counter-reformation, remedial courses for low achieving students were not mandatorily included in the school formative plan during the 1990s. It thus follows that the cohort study that we set out reveals the long-term effects of 'educational debts' vis-à-vis the increased risk of grade retention represented by the remedial exam in September. We could in principle consider other variables that refer to labour market outcomes later in life, as well as university participation and attainment. We however show that the cohorts of students affected by the 1994/95 reform were also affected by an additional reform of the university system in the early 2000s, thus making it difficult to disentangle the effects of the various interventions on outcomes such as university degree and wages.

are presented by gender, as this dimension will prove particularly important in our empirical analysis. Numbers reported refer to the first two upper secondary school grades, as these define the relevant age band for students in our sample.

Table 1. *Descriptive statistics on performance of students for the school year 2006/07 (pre-policy).*

	Grade 1		**Grade 2**	
	Males	**Females**	**Males**	**Females**
Academic Schools				
% of retained students	8.9	6.4	6.8	5.4
% of students with debts	42.5	27.2	47.5	28.8
in Italian	11.1	12.4	8.3	8.5
in Mathematics	62.0	58.1	43.5	47.2
in Foreign Languages	32.9	41.6	24.0	41.2
Technical and Vocational Schools				
% of retained students	16.2	14.2	14.5	11.3
% of students with debts	40.5	32.7	42.6	35.4
in Italian	15.4	11.1	17.3	10.1
in Mathematics	46.8	45.9	51.9	47.6
in Foreign Languages	36.2	33.1	39.1	37.7

Notes. Administrative data from the Ministry of Education for all schools located in the areas considered. Figures are presented by gender, curricular type (academic vis-à-vis technical or vocational) and grade at school (only the first two upper secondary grades are reported). See Section 2 for details.

Retention rates in vocational schools are much higher than in academic schools, for both males and females. Gender differences are clearcut, with females performing sensibly better than males across schools and grades. The difference across schools almost vanishes when it comes to educational debts, gender remaining the most relevant dimension. Above 40 percent of males are given at least one educational debt, and this figure is roughly the same across school types. The percentage for females with educational debts is well below that for males, and females in vocational schools are marginally worse than their peers in academic schools. The breakdown by subject reveals that mathematics and foreign languages are, by far, the most problematic subjects, with no clear pattern by gender.

These numbers, which provide a representative picture of the situation in Italy in the early 2000s, casted doubts on the learning effectiveness of the upper secondary system, and the need for a general reform of the school curricula was brought to attention. The disappointing output documented by the first three PISA surveys created even more concern in the public opinion.[7]

A major intervention was therefore implemented starting from the school year 2007/08 to enforce the recovery of educational debts. There are *three* key factors that characterized this reform (which the media called the "Fioroni reform" from the name of the Minister of Education in place). First, under the new progression system, which is still in operation, students in upper secondary schools were compelled to recover all educational debts *before* the beginning of the new year (mid-September). Second, students with educational debts must attend remedial courses organized by the school during the summer, and take a remedial exam in early September (the assessment mode being decided by the school). On failing to pass the exam, retention would be deliberated by the school council. Finally, the reform introduced more stringent requirements on the school side about the organization of summer courses. Although we were not able to access administrative data on school budgets for a large enough number of cases, it is well known to researchers and policy makers that the additional burdens imposed by the reform were not compensated by an adjustment of financial resources transferred from the Ministry of Education to the schools. It is thus fair to conclude that schools complied with the requirements of the new progression system administrating the same financial resources employed in the past.

Contrary to what happened in the rest of the country, the local government of the province of Trento, an area of Northern Italy which enjoys some degree of autonomy in the implementation of education policies, did not comply with the reform. The decision was made moving from the available evidence on the achievement of students enrolled in local schools. Italy is characterized by substantial variability in PISA scores across areas, with students in Northern regions performing well above the national average and vocational schools lagging markedly behind. At the time of the reform, PISA scores for students living in the province of Trento were as good as those recorded by top-ranking countries.[8] In light of this evidence, local policy-makers decided that there was no need to comply with the national intervention. Furthermore, they supported the idea that remedial courses already offered to students by schools in the province were effective for the full recovery of educational debts, and that no remedial exam was needed to ensure the achievement of academic standards.

We exploit such geographic discontinuity to investigate the short-term effects of the reform on a variety of outcomes. The question is whether mandating remedial summer courses for low performing teenagers, and tight their promotion to the exam in September, makes a

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[7] According to PISA 2000 data, Italy ranked above Spain, Portugal and Greece but far behind the most advanced countries. The average score of Italian students was 100 points lower than that of top-ranking Korean students (OECD 2001). The public concern became widespread after the PISA 2003 results for mathematics, when the overall performance of Italian students dropped below that of Spain and Portugal with an average score of 86 points lower than that of their Finnish peers (OECD 2004). The overall picture was confirmed in the PISA 2006 survey (OECD 2007).

[8] For example, according to PISA 2006 data the average test score in mathematics is 462 in Italy, and 508 in the province of Trento. The same sharp difference remains if test scores in reading comprehension and scientific literacy are considered.

difference. In addition to the direct effect on achievement, which may be mediated by the effect on effort, there may be an indirect effect of the reform on the attitude of parents towards the education of their children. For example, reacting to the threat of grade retention, parents may decide to increase household spending for fees paid to teachers for tutoring. Similarly, there may be effects on the school side, as the organization of summer courses imposed by the reform may come at some cost, and this may vary depending on the resources available at the school. The aim of the next section is to describe the data that we employ to shed light on these aspects.

3. Data

3.1. Main sample

The data set combines school administrative databases (containing teachers' marks and information on promotion/retention) and unique data from *two* surveys purposively designed for this study. The first survey collects information on student proficiency through the administration of a standardized assessment test to all students in our sample. The second survey – administered to parents – collects information on parental social background such as education, job status, household composition and learning resources at home.

The assessment methodology was shaped around PISA, and adjusted to the specific purpose of our study. The test was developed from publicly released items from the first three PISA assessments available at the time that this research started (2000, 2003 and 2006). The test was constructed by experts at the Ministry of Education to guarantee comparability of items difficulty with the PISA scale, and was conducted at the beginning of the 2008/09 school year (October/November 2008).[9] Students were asked to provide information about education and occupation of their parents, and life-style at home. An additional survey was carried out on parents soon after the test. Respondents were asked to provide detailed information on educational and employment background, household composition and home learning resources.

The sampling frame for the survey was constructed by considering a selected number of towns sharing similar characteristics in terms of their demographic, economic and occupational structure, as well as of school-related infrastructures (see Figure A.3 of the Appendix). To ensure comparability, we considered towns near the administrative border of the province of Trento. The leading criteria followed to guide selection were (i) the presence of schools for each curricular track of the Italian upper secondary school system: *licei* (academic, or general education, track), *istituti tecnici* (technical track) and *istituti professionali* (vocational track); (ii) population size of town; and (iii) features of the

[9] The items were presented to students in three one-hour booklets, resulting in a three-hour session with 23 units for reading, 20 for mathematics and 19 for science. All students in our sample took the same tests, thus leaving us with the joint distribution of test scores for the three dimensions of learning considered (reading, mathematics and science). Following the OECD procedure test scores were obtained from item response theory, and standardised using mean and standard deviation of PISA 2006 scores in the province of Trento.

economic and occupational structure. A pair-wise matching comparison of towns was conducted, which was further refined by controlling for geographical proximity (less than seventy kilometers). As a result of this procedure, we ended up selecting three towns in the province of Trento and their most similar counterparts outside the administrative border. The population of the three town considered covers approximately one third of the total population of the province of Trento.

The target sample of students resulted from a two-stage procedure that selected schools in the first stage, and in the second stage cohorts of students defined from the year attended at the time of the test. We again followed a *one-to-one* matching procedure, selecting similar schools located in each pair of towns. The selection of schools was conducted by controlling for observable dimensions such as school track, school size as measured by trends in enrollment and school resources, as well as unobservable dimensions (such as reputation of the school) gathered from general knowledge of the socio-economic background in which they operate.

Across all schools, we focused on students attending the second and the third upper secondary grade during the school year 2008/09, thus aged between 15 and 16. For each school we randomly selected two classes in the second year (i.e. for the cohort of students enrolled for the first time in school year 2007/08) and two classes in the third year (i.e. for the cohort of students enrolled for the first time in school year 2006/07). We did so to ensure variability in the duration of enrollment at school across the different regimes defined by the reform. The cohort dimension proved statistically not important in the analysis, and will not be considered in what follows.

Information on student achievement was complemented with administrative data on teachers' marks on past years at school, as well as with the final grade students obtained at the state examination on completion of the lower secondary school (leaving certificate). Qualitative data elaborated from interviews conducted with all school principals and teachers of sampled classes completed the sources of information that will be used for our empirical exercise. The sample size of the working data, which in what follows we will refer to as "Main Sample", is reported in Table 2. The number of schools involved in the analysis is 22. The number of students is 916 and 942 inside and outside the province of Trento, respectively, evenly distributed in academic and technical/vocational tracks.[10]

3.2. Additional sources of information

Test scores for the main sample were collected in October/November 2008. It follows that the identification strategy employed to measure the effects on achievement may only use *post* reform data. We complemented this information with data for pre reform periods coming from the PISA 2006 and 2009 surveys, and used information from these two waves

[10] We investigated the possible sorting effects deriving from the choice of curricular track at high school across the areas considered in our analysis. We computed the average transition rates from lower secondary school to *licei* for the school year 2007/08 using official data from the Ministry of Education. This analysis pictures rather similar figures in the areas considered, with transition rates ranging between 29 and 34 percent.

to assess the sensitivity of our conclusions obtained from the main sample to the presence of selection bias.

The Ministry of Education granted us access to information which is not available in public use PISA files, allowing us to select (academic and technical/vocational) schools in narrowly defined areas that match closely the evaluation design described in the previous section.[11] The resulting sample, which in what follows we will refer to as "PISA Sample", contains test scores for students *before* (PISA 2006) and *after* (PISA 2009) the reform roll out. Given that the nature of the information collected, the set of demographics in the main sample coincide with those available in the PISA sample. Sample size for the latter dataset is reported in Table 2.

Table 2. *Sample size (students and schools).*

	Main Sample				**PISA 2006 Sample**				**PISA 2009 Sample**			
	Males		**Females**		**Males**		**Females**		**Males**		**Females**	
	off	**on**	**off**	**on**	**off**	**on**	**off**	**on**	**off**	**on**	**off**	**on**
Academic Schools												
Students	168	146	281	205	141	56	320	146	150	97	309	139
Schools	5	4	5	4	14	7	14	7	15	8	15	8
Technical and Vocational Schools												
Students	266	269	201	222	366	292	274	154	301	147	188	96
Schools	6	7	6	7	20	15	20	15	16	9	16	9

Notes. Sample size by gender, curricular type (academic vis-à-vis technical or vocational) and policy status of the area where the school is located (on and off). The left hand side panel – "Main Sample" – refers to the main data collected for the analysis (see Section 3.1 for details). The remaining panels – "PISA Samples" – refer to the samples from the PISA 2006 (pre-policy) and 2009 (post-policy) surveys, derived as explained in Section 3.2.

Finally, we were able to gather administrative data from local government agencies and the Ministry of Education on retention rates since the school year 2006/07 in the areas considered for the evaluation. We constructed longitudinal information for the *same* schools included in the main sample, thus picturing changes in retention rates from before to after the reform that come on top of school-specific fixed effects. We will use this information to document how schools reacted to the reform, and to relate this to the documented effects on achievement.

[11] Because of the design of the PISA survey, in which repeated cross sections of schools are sampled at each wave, we were not able to identify the same schools as in the main sample. Moreover, the finest area identifier that we were able to gather is the province where schools in the main sample are located - an Italian province being a territory administratively similar to a US county.

4. *Methods*

4.1 *Identification strategy*

The evaluation design sets up the comparison of outcomes for students in upper secondary schools in the province of Trento, to outcomes for students in similar schools in adjacent areas. The *causal* interpretation crucially rests upon a *ceteris paribus* condition about the composition of students and inputs in the two groups of schools. This amounts to assuming that the outcome for students enrolled in one group of schools can serve as an approximation to the *counterfactual* outcome for students enrolled in the other group of schools.

The general problem underlying the validity of this condition can be easily put across using standard arguments taken from the programme evaluation literature (see, for example, Heckman and Vytlacil, 2007). In the *potential outcomes* framework interest lies in the causal impact of a given "treatment" on an "outcome" of interest. Let Y_1 (Y_0) denote the *potential* outcome that would result from the remedial exam being (not being) in operation. The causal effect of the reform on school achievement is then defined as $Y_1 - Y_0$. This difference is by its very nature not observable, as geographical location of the school attended reveals only one of the two potential outcomes (Y_0 for students in the province of Trento, and Y_1 otherwise).

The *average* policy impact for students facing remedial exams (or the *average treatment effect of the reform on the treated*) is defined as:[12]

$$E_{Y_1|D}[Y_1|1] - E_{Y_0|D}[Y_0|1],$$

where D denotes a dummy variable for schools *outside* the province of Trento. Similarly, the τ-th quantile treatment effect for students facing the new progression rule is defined as:

$$F^{-1}_{Y_1|D}[\tau|1] - F^{-1}_{Y_0|D}[\tau|1].$$

The evaluation problem consists of dealing with the missing data problem that precludes direct estimation of $E_{Y_0|D}[Y_0|1]$ and $F^{-1}_{Y_0|D}[\tau|1]$. Data are only informative about (features of) the distribution of Y_0 for $D = 0$ schools, and about (features of) the distribution of Y_1 for $D = 1$ schools. This term refers to a *counterfactual* situation which is not observable in the data, requiring as it does knowledge of what the average achievement would have been in schools outside the province of Trento, had the reform not been implemented.

The key econometric difficulty results from the non-random selection of students into schools. Under the assumption that conditioning on an available set of covariates X pre-determined with respect to the implementation of the reform removes all systematic differential selection, one could retrieve the counterfactual term of interest. The extent to which this assumption undermines the assessment of the causal relationship addressed in this paper needs to be carefully discussed in light of the information available in the data.

[12] The notation $E_{A|B}[A|b]$ and $F_{A|B}[a|b]$ indicates the conditional expectation and distribution, respectively, of the random variable A given $B = b$.

To reduce the degree of compositional differences between school inside and outside the Trento province, a matched pilot/control design for schools involved in the analysis was implemented. We then limited our analysis to students in these two groups of schools, thus controlling for the extent of heterogeneity across students on the one hand, but admittedly paying in terms of *external validity* of our results on the other. The *internal validity* of the design is strengthened by its similarity with a *regression discontinuity* strategy, the discontinuity holding with respect to the administrative border of the province of Trento.

4.2 Estimation

Estimation using the "Main Sample" will assume throughout that, netting off the effect of observable variables, the comparison of students enrolled in schools either side of the administrative border identifies the causal effect of the reform on the outcome of interest. We report values of the average return obtained from the following *parametric* regression:

$$y_i = \beta_0 + \beta_1 d_i + \beta_2 x_i + \varepsilon_i, \tag{1}$$

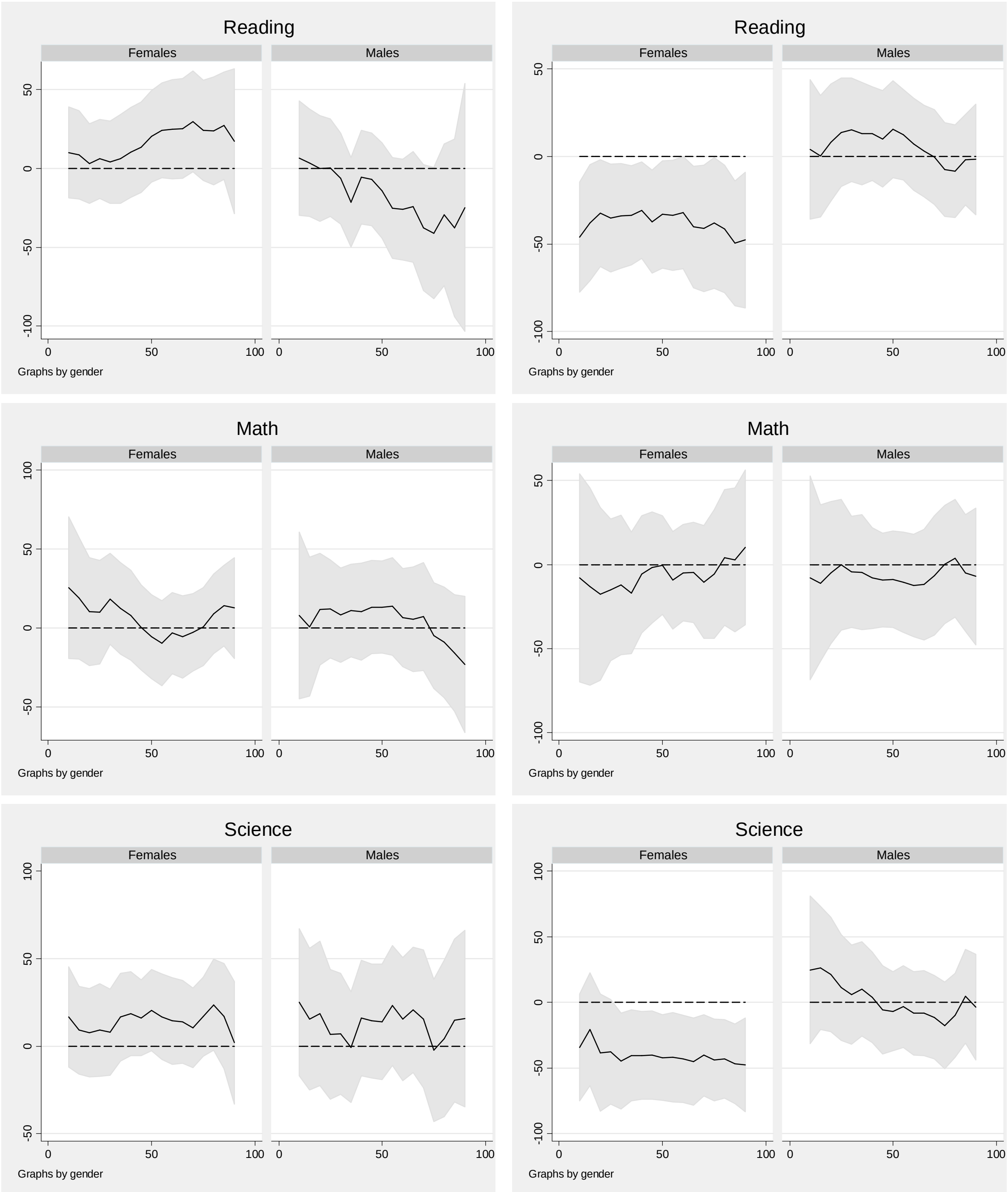
which is estimated separately for the various groups considered (gender and curricular track). Results from this specification will be reported in Table 5.

We considered semi-parametric alternatives to this specification, which we used to check the sensitivity of our conclusions to the estimation method employed. First, the average effect of interest was estimated through a *matching estimator*, contrasting outcomes across pairs of similar students in schools undergoing different progression rules. Matching was implemented using the *propensity score*, which was obtained from a parametric regression of the "treatment" status on the observables that will be described in the next section. Estimates of the propensity score are reported in columns (5) and (10) of Table 3. Second, we employed the same propensity score to estimate the average effect through a *weighting estimator* (see, for example, Imbens, 2004). Perhaps not surprisingly given the evaluation design adopted, the two groups of students contrasted were characterized by substantially identical distributions of the rich set of observables controlled for, thus ruling out any type of common support problem in the data. We however check the robustness of our results dropping from the sample observations that were extreme with respect to the propensity score metric. The various sensitivity checks considered yielded results equivalent to those obtained from the estimation of the parametric regression in (1), both in terms of point estimates and statistical significance. Because of this, we decided to report only parametric estimates while presenting the results in the following sections. Estimation results obtained using the weighting procedure described in Imbens (2004) are presented in Table A.1 of the Appendix.

In estimating quantile treatment effects from the "Main Sample", we decided to fit the standard quantile regression counterpart of equation (1). The results from this analysis will be presented in Figure 1. We checked the sensitivity of our findings to alternative estimation methods employed in the programme evaluation literature (see, for example, Firpo, 2010), but the results proved very similar to those presented in the main text (see Figure A.1 of the Appendix).

Figure 1. *Quantile treatment effects for achievement (Main Sample).*

Academic Schools **Technical and Vocational Schools**



Notes. Reported are estimates of quantile regressions for reading, mathematical and scientific literacy. Results were obtained from separate regressions by gender and curricular type (academic vis-à-vis technical or vocational). All regressions control for variables in Table 3, as explained in Section 4. Bootstrap standard errors based on 200 replications and clustered at the class level are used to compute 95% confidence intervals.

The analysis carried out for the "PISA Sample" makes use of repeated cross sections of students from the 2006 and 2009 survey waves. We implemented a difference in differences strategy and, separately by gender and curricular track, we estimated the average effect of the reform from the following regression:

$$y_{it} = \alpha_0 + \alpha_1 d_i + \alpha_2 p_t + \alpha_3 d_i p_t + \alpha_4 x_i + \xi_{it}, \quad (2)$$

where p_t is a dummy for observations coming from the post reform survey round. The socio-economic demographics controlled for in the analysis coincide with those considered in equation (1). As before, semi-parametric alternatives to (2) were considered as a sensitivity check (Abadie, 2005), which yielded informationally equivalent results and are not reported in the main text. The average effect of the policy, α_3, and the extent of pre-policy differences across areas, α_1, are the parameters of interest in (2). The former parameter is compared to β_1 estimated from equation (1). The latter parameter serves as a over-identification test for the validity of the conclusions drawn from the "Main Sample": should the evaluation design be properly conducted, no difference in test scores across areas in 2006 must be detected, after having netted off the effect of the observables X. The results from this analysis are presented in Table 6 and Table 7.

Finally, administrative data for schools were used to run the following regression:

$$y_{ijt} = \gamma_0 + \gamma_1 p_t + \gamma_2 d_i p_t + \gamma_j + \delta_i + \nu_{it}, \quad (3)$$

which models the outcome change (e.g. retention rates) at grade *j* for school *i* from before to after the implementation of the reform, controlling for both school (δ_i) and grade (γ_j) fixed effects. We will consider the results from the specification to look into the effects of the reform on school inputs, using micro data at the school level from the Ministry of Education. The results will be reported in Table 8.

Throughout the analysis, we will compute standard errors which are robust to heteroskedasticity and are clustered at the class level. When using PISA and administrative data, the cluster unit that we consider is the school.

5. *Results*

5.1 *Descriptive statistics*

Table 3 provides a picture of the degree of homogeneity for students in the two groups of schools along the key dimensions relevant for the analysis. Data from the "Main Sample" are considered for the following covariates: (i) *student demographics* (gender, age, dummy for foreign students, dummy for cohabitation with mother and father, proximity to school) (ii) *socio-economic background of the household* (father's age and education, mother's age and education, dummy for housewife mothers, dummy for unemployed mother or father), (iii) *household wealth and social-status*(occupational stratification scores, material deprivation index).[13]

[13] The socio-economic status is measured using an Italian occupational stratification scale to proxy social standing of different jobs (De Lillo and Schizzerotto, 1985). The life style deprivation index (Whelan *et al.*,

Means and standard deviations of these variables are reported, stratifying observations by curricular track and policy status. To test for the validity of our evaluation design, we estimated the *propensity score* from a regression of the dummy for being a student in schools outside the province of Trento on the covariates considered. Results from this regression are reported in columns (5) and (10) of the table, for academic and vocational tracks, respectively. Overall, the distribution of demographics is balanced across the two groups of areas, although some departure from the general pattern emerges for the education level of mothers. Regardless of the index considered, the difference in socio-economic backgrounds between students enrolled in technical/vocational vis-à-vis academic schools is worth noting.

The bottom panel of the table reports descriptive statistics for test scores in the three dimensions of learning considered by PISA. Taken at face value, the mean difference between the two groups of areas is positive for academic schools, and negative for vocational schools. Table 4 adds in the additional dimension represented by gender differences. The average test score for the three domains considered is considerably lower for students enrolled in technical and vocational schools compared to students in academic schools. Students in the former group of schools undergoing the new progression rule present levels of reading, mathematical and scientific literacy lower than those of their counterparts in the province of Trento. Simple tests for the significance of the outcome difference between policy on and policy off areas point to positive results for males in academic schools for the science test score, and negative results for females in technical and vocational schools for the reading and science test scores. The results are therefore suggestive of disparities in achievement between treated and control schools, with negative differences for females from lower socio-economic backgrounds.

For descriptive purposes, we used the "PISA Sample" to investigate the distribution of other key school inputs that may concur to determine test scores for students in the two areas. In particular, we considered the student to teacher ratio and the proportion of girls in the class, which produced roughly equivalent figures either side of the administrative border of the province of Trento and stable across survey waves. Academic schools present a teacher to pupil ratio equal to 8.65 and 8.01 inside and outside the province of Trento, respectively. The corresponding figures for technical and vocational schools are instead 6.49 and 7.27. As we have anticipated in the Introduction, the average proportion of girls in the latter curricular track is way below that in academic schools, being 44 percent in the province of Trento vis-à-vis 41 percent outside the province. These should be compared with the values 66 percent and 67 percent, respectively, for academic tracks. It is thus fair to conclude that the stratification by curricular track that we maintain throughout the analysis captures sensibly different environments for the class, both in terms of socio-economic background and climate learning.

2002) is an additive index based on the lack of 5 items in the household: TV, car, DVD player, computer, internet access. Each individual item is weighted by the proportion of households possessing that item in Italy. Weights were derived from the SILC 2006 survey for Italy.

Table 3. *Descriptive statistics and balancing tests for the covariates considered (Main Sample).*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Academic Schools					**Technical and Vocational Schools**				
	Policy On		**Policy Off**			**Policy On**		**Policy Off**		
VARIABLES	**Mean**	**SD**	**Mean**	**SD**	**Score**	**Mean**	**SD**	**Mean**	**SD**	**Score**
Student Demographics										
Age	15.837	0.707	15.755	0.671	0.031	16.005	0.807	15.963	0.788	0.018
Male	0.374	0.484	0.416	0.494	-0.055	0.570	0.496	0.548	0.498	0.020
Born abroad	0.547	0.498	0.443	0.498	0.121	0.124	0.330	0.122	0.328	-0.050
Cohabitating with mother and father	0.130	0.337	0.081	0.274	0.120	0.192	0.394	0.177	0.382	0.053
School in the place of residence	0.517	0.500	0.456	0.499	0.103*	0.261	0.440	0.265	0.442	0.001
Household Demographics										
Father's age	48.625	5.448	49.061	4.727	-0.006	47.963	5.879	47.896	5.639	0.004
Mother's age	45.404	4.586	46.145	4.371	-0.004	44.659	4.999	44.562	5.132	-0.003
Father's education: Secondary	0.530	0.500	0.530	0.500	-0.042	0.545	0.499	0.532	0.500	0.054
Father's education: Tertiary	0.280	0.450	0.332	0.472	0.007	0.083	0.276	0.097	0.296	-0.042
Mother's education: Secondary	0.581	0.494	0.540	0.499	-0.069	0.528	0.500	0.632	0.483	-0.123***
Mother's education: Tertiary	0.232	0.423	0.341	0.475	-0.168**	0.093	0.291	0.077	0.268	-0.043
Mother is housewife	0.160	0.367	0.171	0.377	-0.029	0.226	0.419	0.229	0.421	-0.031
Mother or father unemployed	0.018	0.135	0.012	0.109	0.025	0.030	0.196	0.026	0.160	0.032
Wealth and Social Status										
Life-stype deprivation index	0.300	1.083	0.259	1.006	0.013	0.535	1.433	0.583	1.478	-0.007
Occupational prestige scale	51.391	19.314	56.750	19.393	-0.002	40.221	17.803	39.524	17.159	0.000
Test Scores										
Reading literacy	513.420	95.962	510.988	101.368		409.682	82.465	431.599	89.989	
Mathematical literacy	514.283	89.798	505.468	91.809		452.067	118.681	454.618	111.246	
Scientific literacy	529.847	87.333	517.646	84.569		438.376	99.781	455.491	95.288	

Notes. Reported are descriptive statistics for test scores and the covariates used as controls in the analysis, by curricular type (academic vis-à-vis technical or vocational) and policy status of the area. The columns labeled with "Score" – columns (5) and (10) – report estimates from probit regressions of the policy status of the area on all covariates, clustering standard errors at the class level (see Section 5.1 for details). *** p<0.01, ** p<0.05, * p<0.1.

Table 4. *Descriptive statistics for test scores (Main Sample).*

		Males		Females	
		On	**Off**	**On**	**Off**
Academic Schools					
Reading	Mean	500.670	509.438	521.042	512.092
	SD	91.090	102.069	98.128	101.102
Math	Mean	540.312	523.952	498.814	492.304
	SD	79.690	91.804	91.999	89.734
Science	Mean	545.119	523.044	520.717	513.828
	SD	86.278	83.872	86.833	85.055
Technical and Vocational Schools					
Reading	Mean	411.271	418.770	407.558	447.144
	SD	80.488	85.151	85.194	93.376
Math	Mean	480.705	484.796	413.787	417.886
	SD	113.330	96.482	115.081	117.043
Science	Mean	453.240	460.934	418.407	448.835
	SD	104.739	101.633	89.128	86.668

Notes. Summary statistics by gender, curricular type (academic vis-à-vis technical or vocational) and policy status of the area where the school is located (on and off).

5.2 Effects on achievement

5.2.1 Evidence from the Main Sample

Table 5 reports the average policy effects obtained by estimating equation (1) from the "Main Sample". Results are presented separately for the three subjects, controlling for gender and curricular track. Leaving aside significance, it is striking to notice that point estimates for academic schools are characterised by *positive* signs, while estimates for technical and vocational schools point to the *opposite* direction. The effects for academic schools are, however, not statistically different from zero. On the contrary, we observe negative effects for female students in technical and vocational schools, for both reading (-39.59) and science (-38.55) test scores. The size of these effects is quite large, as it can be inferred by considering the standard deviations presented in Table 4. This sharp difference between curricular tracks proved robust to the estimation method employed. For example, Table A.1 of the Appendix reports results obtained by estimating average differences that are weighted using the propensity score in Table 3 (see Imbens, 2004). The pattern presented is identical to the one discussed here, with point estimates that – when significant – depict even more pronounced effects for female students in vocational schools.[14]

Table 5. *Average effects for achievement (Main Sample).*

	Reading		Mathematics		Science	
	(1)	(2)	(3)	(4)	(5)	(6)
	Males	**Females**	**Males**	**Females**	**Males**	**Females**
Academic Schools						
	-18.5915	17.4019	5.6371	6.2289	16.9269	11.0727
	(12.072)	(13.764)	(11.995)	(11.551)	(12.031)	(9.193)
Technical and Vocational Schools						
	2.3689	-39.5927	-7.956	-1.3815	-0.4246	-38.5546
	(10.961)	(11.561)	(14.854)	(12.718)	(12.957)	(11.688)

Notes. Results were obtained from separate regressions by gender and curricular type (academic vis-à-vis technical or vocational). All regressions include as controls the variables in Table 3. Robust standard errors clustered at the class level are reported in parentheses.

We checked the sensitivity of these findings to omitted variable bias by relying on *within student* variability in achievement. This idea is not novel, and was already employed in other studies (see, for example, Lavy *et al.*, 2012). We considered the various teacher marks available in the data as indicators of performance, pooled them with the three tests scores collected through the main survey (reading, mathematics and science), and derived a proxy for student "ability" according to the following procedure. First, for each student we considered final marks at the end of the two semesters of the first and second grade at school, for both mathematics and Italian language. We limited the analysis to these subjects as they are common across all curricular tracks. We also considered the final mark obtained at the end of the lower secondary school. This yielded a total of 8 to 12 indicators per students, depending of the cohort of enrollment in the original sampling frame (see

[14] We also investigated the heterogeneity of results allowing the policy effect to vary across groups of students that were plausibly more at risk of grade retention under the new progression system. We exploited the variability across cohorts of enrolment in the original evaluation design (see Section 4.1), and selected only students enrolled for the first time in the school year 2006/07 (pre-reform). These students completed the first year at school under the old regime. We then run the regression in (1) only for this cohort of students, adding a dummy identifying students at risk of grade retention and its interaction with the area identifier. We experimented with alternative definitions of "students at risk". First, we considered students who were admitted to the second grade with at least one educational debt. Second, we focussed on students having a debt in mathematics, as we know from Table 1 that this was, by far, the case most frequently encountered situation. Finally, we defined at risk those students who completed the lower secondary school with the minimum score. As expected, results from this set of regressions show that students more at risk of grade retention have actually lower test scores that their peers in the class. However, we rejected the hypothesis that the results documented in Table 5 vary with risk.

Section 3.1). Second, we stacked these indicators and run gender-specific fixed effect regressions controlling for subject dummies (mathematics or science, vis-à-vis reading or Italian language), nature of the indicator employed (test scores vis-à-vis administrative marks) and age when the indicator was measured. Third, we used these regressions to predict student level fixed effects, that we inserted into (1) to net off unobservables that can be related to ability of students, or unobserved family background characteristics.

Results from this specification are reported in Table A.2 of the Appendix, which aligns well with the pattern already documented in Table 5. In technical and vocational schools, only test scores for female students are affected by the reform. The negative effects documented for reading and science are still confirmed, although their magnitude is now somewhat attenuated. Differently from before, the effect for mathematics is now statistically significant, and positive. As for academic schools, most of the results in Table 5 are confirmed. The negative effect of reading for males, which was not statistically different from zero, is now more precisely estimated and significant at the conventional levels.

Consistently with other studies in the literature, we went beyond averages and tested whether the reform affected achievement across quantiles of the distribution of test scores. Figure 1 reports the values of the quantile treatment effects (QTEs) for the various groups considered, along with the corresponding 95 percent confidence intervals.[15] For academic schools all figures are not statistically different from zero, thus pointing to homogeneous effects of the reform. The pattern found for technical and vocational schools also supports the hypothesis of constant effects across students, but with a negative shift in reading and science test scores for female students. Overall, the evidence documented points to much lower within track variability in policy effects than the variability found between tracks. This result can partly be explained by noting that school tracking creates homogeneous classes with respect to ability and family background. Other studies in the literature (see, for example, Figlio and Lucas, 2006) have shown that high standards in the class have the largest effects on achievement for students mismatched with the average ability of their peers.[16]

5.2.2 Evidence from the PISA Sample

The aim of this part of the analysis is twofold. First, we replicate in Table 6 the same analysis carried out in the previous section, this time obtained by estimating equation (2) from the "PISA Sample". This serves as an additional sensitivity check for the conclusions drawn from the "Main Sample". Second, we assess whether the two groups of areas used

[15] Under the assumption of *rank invariance* of students across distributions of potential outcomes, that is if every student had the same rank across potential distributions, QTEs could be interpreted as the effects of the reform for a student at the τ-th quantile of the test score distribution.

[16] As for average effects, we checked the sensitivity of QTEs to the specification and the estimation method adopted. Figure A.1 of the Appendix is the analogue of Figure 1, but is obtained using the semi-parametric procedure suggested by Firpo (2010). It is clear that the informational content is equivalent to that of Figure 1. We additionally derive the analogue of Figure 1 when quantile regressions include student fixed effects, the latter being derived as explained in the section (see Figure A.2 of the Appendix).

for the evaluation design presented pre-reform differences in test scores. We therefore use the longitudinal dimension of PISA data to test the validity of causal conclusions from equation (1).

Table 6. *Average effects for achievement (PISA Sample).*

	Reading		**Mathematics**		**Science**	
	(1)	(2)	(3)	(4)	(5)	(6)
	Males	**Females**	**Males**	**Females**	**Males**	**Females**
Academic Schools						
Area	-9.1113	6.9984	-11.4768	-3.6890	-20.1786	-9.5158
	(23.505)	(17.591)	(12.121)	(17.079)	(12.197)	(15.835)
Time	-7.5086	-6.7866	-12.5551	3.1026	-18.8454	-4.6819
	(24.831)	(16.414)	(16.070)	(16.014)	(15.907)	(16.068)
Effect	29.1602	-1.6653	37.9587	2.2657	41.1284	-0.1197
	(26.718)	(18.862)	(18.893)	(19.792)	(19.135)	(19.528)
Constant	504.6415	581.9724	560.6949	564.0927	597.0172	588.0176
	(44.309)	(33.733)	(47.322)	(36.414)	(42.250)	(34.281)
Technical and Vocational Schools						
Area	-11.2102	7.2873	7.7600	7.1294	-7.2303	4.3575
	(10.002)	(11.789)	(10.616)	(10.562)	(9.668)	(12.380)
Time	1.7962	38.6940	8.5985	47.2009	-6.8917	37.0278
	(12.192)	(15.938)	(12.412)	(12.154)	(12.940)	(20.456)
Effect	-22.2331	-53.5597	-29.5479	-49.6937	-3.0306	-45.1361
	(14.684)	(19.704)	(17.070)	(16.255)	(16.890)	(24.988)
Constant	537.3787	572.8068	566.8904	564.8301	562.8183	603.9300
	(24.481)	(29.650)	(25.824)	(32.748)	(26.129)	(34.404)

Notes. Results were obtained from a difference in differences equation separately by gender, academic (top panel) and technical or vocational (bottom panel) schools. Baseline figures refer to reading, mathematical and scientific literacy in areas not affected by the reform in 2006. “Area” is a dummy for schools in areas affected by the reform; “Time” is a dummy for the post reform period (2009); “Effect” is the difference in differences estimate. Standard errors, in parentheses, are clustered at the school level.

Negative figures in Table 6 are noteworthy concentrated in vocational schools, and are now statistically significant at the conventional levels for nearly all the combinations considered. As for academic schools, this analysis confirms the pattern already documented in Table 5, and the results for males are now marginally significant for mathematics and science. Taken at face value, the results obtained from the two alternative samples depict sharp differences across curricular tracks, with negative average effects for students in technical and vocational schools and zero, or at most marginally positive effects, in academic schools. To a lesser extent, gender differences seem to emerge depending on the dataset employed. These findings points to a negative effect of the reform that exacerbates pre-existing inequalities in educational opportunities between school tracks.

As it was explained in Section 4.1, the key assumption required to rule out selection bias is that students in the two groups of schools would have presented the same average score had the reform not taken place. The coefficient labelled by “Area” in Table 6 measures the extent of such a difference in 2006, and is not statistically significant across all groups considered for the three scores. As the “PISA Sample” was constructed adopting the same selection criteria employed for the definition of the main evaluation sample, this piece of evidence corroborates the idea that the results presented in Table 5 depict causal relationships.

5.3 Effects on school and family inputs

The policy effects on family inputs are investigated by considering the “PISA Sample”, and maintaining the assumption that a difference in differences strategy that adjusts for the demographics as in Table 3 allows to retrieve causal relationships. We thus focussed on variables that are available in public use PISA files, and for which the wording in the questionnaire is unchanged between the 2006 and the 2009 survey waves. It turns out that the number of indicators that we could eventually employ, also taking missing data into consideration, is limited.

If parents perceive their children to be struggling at school, they may devote more attention to their children’s schoolwork. We started by considering an indicator of *education costs* borne by the household for the student in the last year, which refer to services by “educational providers”.[17] Given the Italian public school system in which there are practically no tuition fees paid to the school, these costs most likely cover extra instruction time in the form of private remedial classes. After controlling for the variables in Table 3, this represents a good proxy for household investment in education of the student. The mean value of this variable in the pre-reform period is 207EUR and 243EUR for students in technical/vocational and academic schools, respectively, with no detectable differences by gender and area. We also considered a series of variables measuring the perception of parents on the quality of the school. The dimensions analysed are competence of teachers,

[17]This is the wording for the question that refers to education expenses for the student interviewed, which comes from the parent questionnaire for both survey waves: “*In the last twelve months, about how much would you have paid to educational providers for services? In determining this, please include any tuition fees you pay to your child’s school, any other fees paid to individual teachers in the school or to other teachers for any tutoring your child receives, as well as any fees for cram school. Do not include the costs of goods like sports equipment, school uniforms, computers or textbooks if they are not included in a general fee (that is, if you have to buy these things separately)*”. The variable is coded by PISA using the following categories: less than 100EUR, between 100EUR and 200EUR, between 200EUR and 300EUR, between 300EUR and 400EUR, and more than 400EUR. For simplicity we decided to take as reference values for these categories 50EUR, 150EUR, 250EUR, 350EUR and 500EUR, and to treat the variable as continuous in all regressions.

standards of achievement, instructional methods, discipline and how progress of students is monitored.

Finally, we considered three indicators of *instructional time*, which we obtained from the self-reported number of weekly hours spent by the student attending out-of-school time lessons in Italian language, mathematics and other subjects. The latter subject category includes foreign languages, which – as it was documented in Table 1 – represents one of the most problematic dimensions of learning at the time of the reform. According to the PISA questionnaire, the activities considered are taken outside of normal school hours, refer subjects that are also learnt at school, and may be given at school, at home or somewhere else.[18] These indicators most likely comprise private remedial classes and extra classes organised by schools (which is exactly one of the school inputs affected by the reform). The inspection of mean values for this variable in 2006 reveals two interesting patterns. First, activities in mathematics and other subjects are more intense in academic schools than in technical and vocational schools. Second, students in technical and vocational schools located in the province of Trento are more engaged in extra-curricular activities than their peers in other areas; moreover, there are no differences across areas along this dimension when academic schools are considered. This finding is consistent with the claim made by the local government of the province, discussed in Section 2, about the number of remedial courses already in place at the time of the reform.

We estimated equation (2) separately for the various outcomes considered. The results reported in Table A.3 of the Appendix show that there are no detectable effects of the reform on the block of variables that refer to parents. Household spending is *not* affected, nor is the attitude of parents towards the role of or the learning environment at the school of their children. However, results for instructional time in Table 7 show that the number of out-of-school activities is significantly affected by the reform. The extra time spent by students learning subjects outside of normal school hours increases, which is a finding consistent with the requirements imposed by the reform on the school side. If instructional time increases but the cost for this is not covered by parents, then it must be that this effect is mediated by a change in school inputs. However, such an effect applies only to students in technical and vocational schools, and for those subjects (mathematics and foreign languages) that were the most problematic at the time of the reform.

This two pieces of evidence suggest that parents did not react by providing extra support to children as a result of the new progression rule. On the school side, much of the action was concentrated in technical and vocational schools, which is consistent with the hypothesis

[18] This is the wording for the question that refers to learning time for the student interviewed, which comes from the student questionnaire for both survey waves: "*How many hours do you typically spend per week attending out-of-school-time lessons in the following subjects (at school, at home or somewhere else)?*". The 2006 wave states explicitly to consider the time spent attending lessons "*at school, at home or somewhere else*". The 2009 wave is more explicit, and states that there "*are only lessons in subjects that you are also learning at school, that you spend learning extra time outside of normal school hours. The lessons may be given at your school, at your home or somewhere else.*" The variables considered in the analysis were obtained from raw categories collected by PISA, creating continuous indicators using the following definitions: "*no time*" (0 hours), "*less than 2 hours a week*" (1 hours), "*2 or more but less than 4 hours a week*" (3 hours), "*4 or more but less than 6 hours a week*" (5 hours), "*6 or more hours a week*" (6 hours).

Table 7. *Average effects for out-of-school-time lessons (PISA Sample).*

	AC (1)	TV (2)	AC (3)	TV (4)	AC (5)	TV (6)	AC (7)	TV (8)
	Italian		**Mathematics**		**Science**		**Other Subjects**	
Area	0.0162	-0.1462	0.0333	-0.3424	0.0330	-0.1295	0.1342	-0.1268
	(0.150)	(0.122)	(0.177)	(0.144)	(0.151)	(0.100)	(0.165)	(0.133)
Time	0.0312	-0.0398	0.0622	-0.1009	0.1181	0.0969	-0.0515	-0.0008
	(0.180)	(0.102)	(0.136)	(0.100)	(0.194)	(0.166)	(0.192)	(0.151)
Effect	-0.0487	0.2014	0.0368	0.7606	-0.1158	0.2037	0.0129	0.5840
	(0.194)	(0.143)	(0.163)	(0.237)	(0.192)	(0.210)	(0.214)	(0.203)
Constant	0.2030	0.5619	0.5482	0.9232	0.2918	0.3317	0.2095	0.7177
	(0.448)	(0.349)	(0.385)	(0.397)	(0.422)	(0.335)	(0.359)	(0.356)
Observations	942	1,108	954	1,116	941	1,103	923	1,087

Notes. Results were obtained from a difference in differences equation separately by Academic (AC) and Technical and Vocational (TV) schools, using information from the "Learning Time" section of the PISA student questionnaire. The variables considered are "Weekly hours spent attending out-of-school-time lessons" in: *Italian*, *Mathematics*, *Science* and *Other Subjects*. These were obtained from raw categories by creating continuous indicators using the following definitions: "no time" (0 hours), "less than 2 hours a week" (1 hours), "2 or more but less than 4 hours a week" (3 hours), "4 or more but less than 6 hours a week" (5 hours), "6 or more hours a week" (6 hours). Baseline figures refer to areas not affected by the reform in 2006. "Area" is a dummy for areas affected by the reform; "Time" is a dummy for the post reform period (2009); "Effect" is the difference in differences estimate. Standard errors, in parentheses, are clustered at the school level.

that the number of activities in academic schools already in place before the reform was sufficient to meet the student's needs and teachers' requirements.

Moving from this evidence, we use administrative school files released by the Ministry of Education to investigate the effect of the reform on retention rates. Results are presented in Table 8, considering micro data by school and grade up to three years after the reform rollout (the most recent figure available). We report separate results for academic, technical and vocational schools. We keep separate the former group as, after a state exam at the end of the third year, students can attain a formal qualification that enables to practice an occupation. Because of this, we consider data across curricular types for grades that are not characterised by having the state exam at the end (grades 1 to 4, excluding the third grade in vocational schools).

We first consider in columns (1), (4) and (7) students for whom the final status (retention or promotion to the next grade) is determined in June. All remaining students have either been given an educational debt (for schools in control areas), or been mandated to summer courses and the remedial exam in September (for schools affected by the reform). Results for promotion rates, as determined in June, are reported in columns (2), (5) and (8) of the table. Finally, columns (3), (6) and (9) report the overall retention rates at the end of the school year, computed by considering retention in June or after the remedial exam in

September. Figures reported in Table 8 are obtained from school fixed effects regressions run by curricular type, allowing for grade specific effects and controlling for grade dummies and enrollment rates.

For technical/vocational schools, consistently across grades, we find a significant increase in the percentage of students whose status is determined in June – see column (4) – which is driven by higher promotion rates – see column (5). The analysis for academic schools yields similar conclusions, but with policy effects in the first two grades that are only marginally significant – see columns (1) and (2). For the remaining curricular type, we are not able to detect any significant effect although, leaving significance aside, it is striking to note that the figures reported are consistent with higher retention rates – see column (8). Taken at face value, these results show that students admitted to the next grade in the post reform period are those who, before the reform, would have been given at least one educational debt. This phenomenon is more pronounced in vocational schools. We finally consider the effects on retention rates that results after the screening made by schools in June. The policy effects are positive, increasingly higher when we move to the left of the table, and strongly significant for vocational schools. Effects in columns (3) and (9) are, in some cases, only marginally significant at the conventional levels.[19]

Overall, the figures presented in Table 8 are at odds with the evidence documented for achievement. Despite the negative effects on test scores in technical and vocational schools, we observe in the latter group a marked increase in the number of students admitted to the next grade. Similar evidence, with lower statistical precision, is found in academic schools, where no effects on test scores are detected. These findings are consistent with changes in school inputs that result from adaptive behavior in the new regime. As clarified above, the reform introduced additional administrative burdens related to the organisation of remedial courses and the exam in September, leaving substantially unaffected school budgets. Schools reacted by admitting to the next grade those students who, with the practice of social promotion in place, would have obtained an educational debt. This is not the case for the worst students of this group, who are mandated to summer courses and have to sit the remedial exam in September. The risk of grade retention for this group is substantially higher with new progression system, and this impacts significantly retention rates in those schools with students from less advantaged backgrounds. We conclude that the behavior of schools may have induced an additional effect on effort on students that goes on top of the effect on curricular tracking that we reviewed in the Introduction.[20]

[19]We additionally check whether the effects documented in Table 8 reflect a temporary adaptation to the requirements imposed by the Ministry, or whether there are persistent over time. Starting from equation (3), we considered a specification that allows for year-specific effects for the three post-reform periods for which we have data (2007/08, 2008/09 and 2009/10). Results from this analysis confirm that the differences between areas remain fairly stable over time.

[20] The above interpretation was confirmed by considering results from qualitative analysis that we carried out from interviews conducted on teachers and principals of all schools in our "Main Sample".

Table 8. *Effects for retention and promotion rates (administrative data).*

	Academic Schools			Technical Schools			Vocational Schools		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	In June	**Admitted**	**Retained**	**In June**	**Admitted**	**Retained**	**In June**	**Admitted**	**Retained**
Effect Grade 1	0.0473	0.0526	0.0112	0.0657	0.0398	0.0506	0.0143	-0.0405	0.0899
	(0.030)	(0.034)	(0.012)	(0.020)	(0.019)	(0.017)	(0.065)	(0.079)	(0.047)
Effect Grade 2	0.0560	0.0566	0.0161	0.0680	0.0571	0.0381	0.0083	-0.0204	0.0625
	(0.029)	(0.033)	(0.010)	(0.021)	(0.020)	(0.017)	(0.068)	(0.081)	(0.049)
Effect Grade 3	0.0375	0.0400	0.0164	0.0507	0.0485	0.0333			
	(0.029)	(0.033)	(0.009)	(0.020)	(0.020)	(0.016)			
Effect Grade 4	0.0374	0.0387	0.0123	0.0448	0.0376	0.0294	0.0137	-0.0436	0.0962
	(0.030)	(0.032)	(0.008)	(0.020)	(0.017)	(0.015)	(0.068)	(0.083)	(0.049)
Grade 2	-0.0312	-0.0004	-0.0309	-0.0315	0.0132	-0.0449	-0.0357	0.0181	-0.0534
	(0.011)	(0.012)	(0.005)	(0.009)	(0.010)	(0.008)	(0.011)	(0.012)	(0.010)
Grade 3	-0.0116	0.0086	-0.0204	-0.0371	-0.0031	-0.0341			
	(0.011)	(0.014)	(0.007)	(0.010)	(0.012)	(0.011)			
Grade 4	-0.0017	0.0459	-0.0482	-0.0430	0.0330	-0.0763	-0.0503	0.0555	-0.1053
	(0.015)	(0.017)	(0.007)	(0.011)	(0.015)	(0.011)	(0.019)	(0.016)	(0.016)
Enrolment	0.0001	-0.0000	0.0001*	-0.0001	-0.0005	0.0004	-0.0000	-0.0003	0.0003
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.6876	0.6119	0.0774	0.6333	0.4867	0.1471	0.5742	0.3625	0.2083
	(0.021)	(0.023)	(0.010)	(0.014)	(0.022)	(0.016)	(0.020)	(0.020)	(0.022)
Observations	1,092	1,092	1,092	1,415	1,415	1,415	800	800	800

Notes. Reported are estimates of the ATE obtained from school fixed effects equations, by curricular type, using administrative data granted by the Italian Ministry of Education and local agencies. The available data refer to school grades, for the years 2006/07 (pre-reform), 2007/08, 2008/09 and 2019/10 (post-reform). Columns (1), (4) and (7) refer to students whose final status (retention or promotion to the next grade) is determined in June. Columns (2), (5) and (8) refer to students admitted to the next grade in June. Columns (3), (6) and (9) report the overall retention rates at the end of the school year, computed by considering retention in June or after the remedial exam in September. Standard errors, in parentheses, are clustered by school.

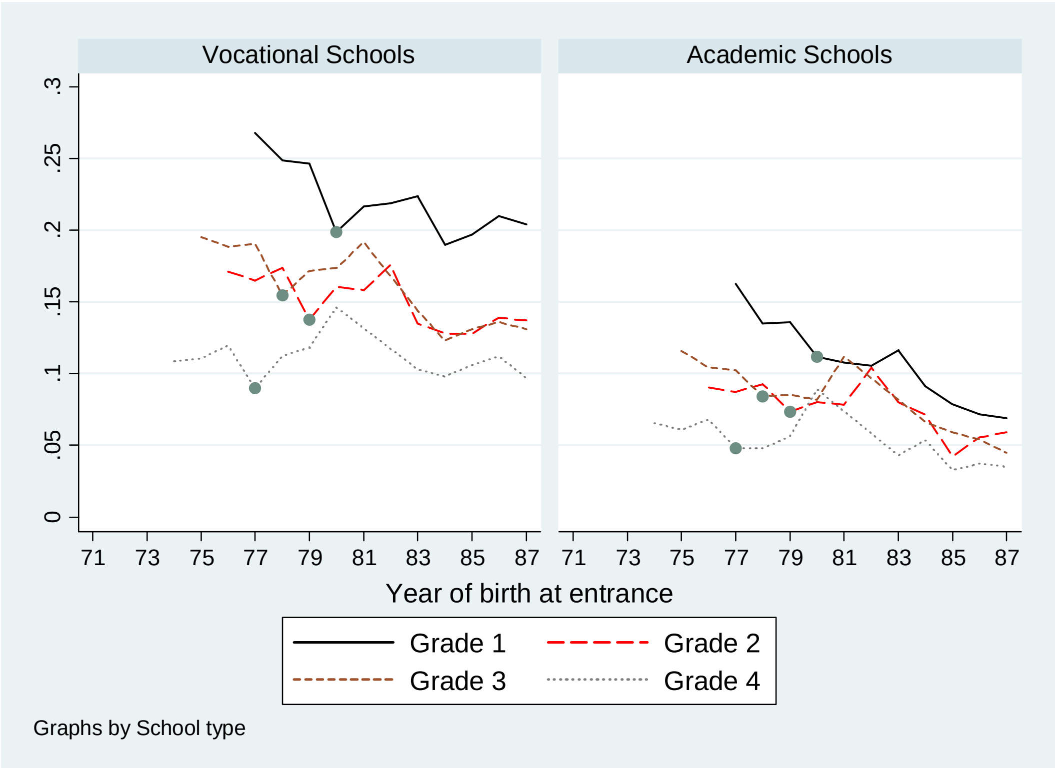
5.4 Long-term effects

In this section we present back of the envelope calculations on the long run effects of the reform. To this end, we use the variability introduced by a *different* reform that affected upper secondary schools of the country during the 1990s, and that shares many similarities with the nature of the intervention considered in this paper. As it was explained in the Introduction, remedial exams for low performing students were introduced for the first time in Italian schools in 1923. Starting from the school year 1994/95 they were abolished from upper secondary schools, and this intervention was universally applied in all areas of the country (also for cohorts of students already enrolled under the past progression system). It follows that the 1994/95 reform acted the opposite direction of the 2007/08 reform considered in this paper, and introduced the practice of educational debts explained in Section 2. The important dimension worth noting is that, contrary to the most recent reform, the former intervention did not introduce any condition on the inclusion of remedial courses in the school formative plan. Their inclusion became compulsory by law only starting from the school year 2004/05; before that time, the quantity and the quality of remedial classes depended on the resources invested by the school. Anecdotal evidence, for which we cannot provide empirical figures due to the lack of data for those years, suggests that most of the costs of remedial education were left to the household.

The last cohort of high school graduates before the 1994/05 reform comprises students born in 1976. For these students, remedial exams before the beginning of the new school year had substantially the same format as the exams introduced with the 2007/08 reform. In this sense, the former reform represents the mirror image of the latter. Again, anecdotal (but certainly uncontroversial) evidence suggests that the remedial exam represented a serious threat of grade retention, not just extra time that students had to spend studying during the summer. We can thus set out a comparison of cohorts of students born before and after 1976, and use the available longitudinal dimension to look at their outcomes for school attainment and later in life. The causal relationship addressed reveals just the effects of diminishing the risk of grade retention. This is a feature worth remembering in light of the results documented in Table 8, as the 1994/05 reform did not impose any clear burden on schools as it is the case for the most recent counter-reformation.

Figure 2 reports official figures for retention rates by school grade and curricular type published by the Italian National Bureau of Statistics (ISTAT). The reform year, which is marked with a dot in each profile, is associated with negative spikes at all grades, and this is more so for vocational schools. However, leaving aside temporary adjustments around the reform year, the time series does not seem to present any evident structural break arising from the change to the progression rule. As a result, the risk of grade retention does not impact on retention rates. Figure 3 maintains the same design, and reports profiles for the percentage of upper secondary school graduates around the reform year. Due to the lack of official figures, we decided to use data from the 2010 Survey of Household Income and Wealth run by the Bank of Italy, and we checked that the findings documented are consistently reproduced using information from the Italian Labour Force Survey. No clear

Figure 2. *Retention rates by cohort of birth (1994 reform).*

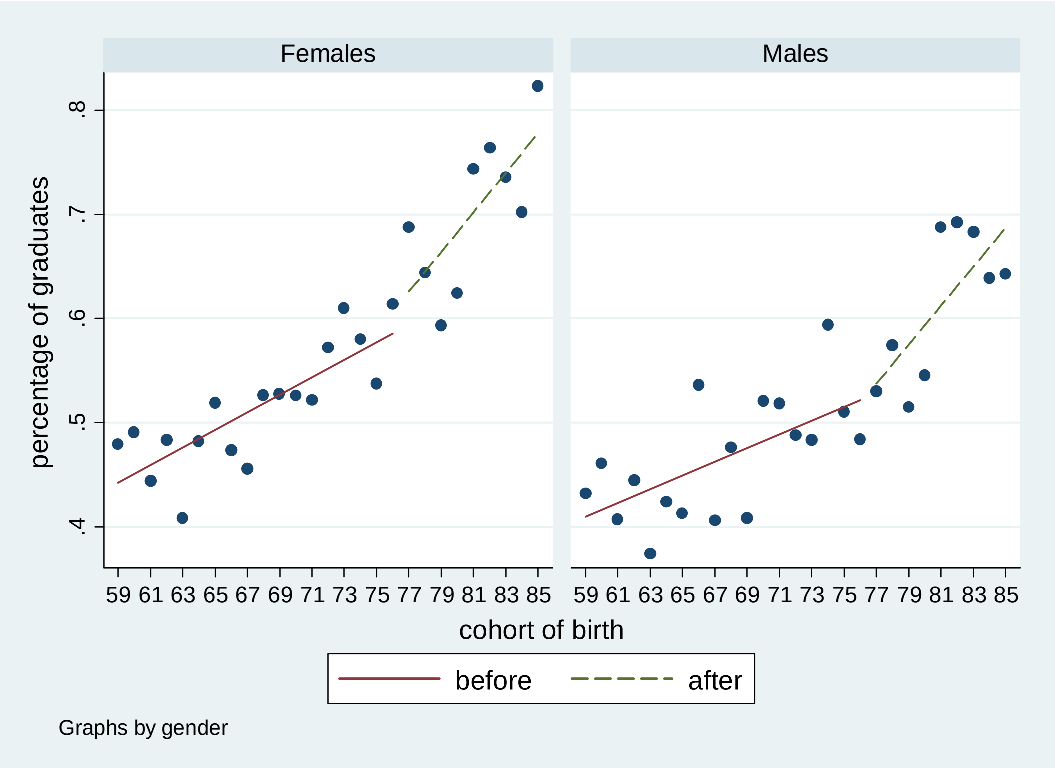


Notes. Reported are profiles for retention rates by cohort of enrollment at upper secondary school (e.g. 78 stands for "born in 1978" and enrolled at age 14), separately for the first four school grades. Only regions in the Northern/Central part of Italy are considered. The reform year is marked with the red dot. Source: ISTAT (Italian National Bureau of Statistics, before the school year 1997/98) and MIUR (Ministry of Education, from the school year 1998/99).

discontinuity emerges from the figure, for both male and female students. In principle, by adopting the same approach we could shed light of longer term effects on university completion and wages. However, cohorts around 1976 were also the most affected by a subsequent reform of the university system (see, for instance, Cappellari and Lucifora, 2009), that resulted for those cohorts into higher graduation rates and, therefore, indirect effects on labour market outcomes (see Figure A.4 of the Appendix).

The absence of clear effects of the 1994/95 reform, as opposed to the effects documented in Table 8, represents corroborative evidence in favour of the role played by changes in school inputs in the most recent reform.

Figure 3. *Percentage of Upper Secondary School Graduates by cohort of birth (1994 reform).*



Notes. Reported is the percentage of upper secondary school graduates by cohort of birth computed in 2010 from the Bank of Italy Household Survey on Income and Wealth. Cohorts born before 1976 are not affected by the 1994/95 reform of the upper secondary school system (see Section 5.4 for details).

6. Discussion and conclusions

Remedial programmes for compulsory secondary education have received relatively scant attention. We have presented evidence that such programmes have heterogeneous returns on student achievement. Our findings depict zero or mildly positive effects on performance for students enrolled in academic tracks, and negative effects for students in technical and vocational oriented schools.

Heterogeneous effects of the reform on effort could be one driver of this result. As in other advanced countries, a large proportion of Italian students enrolled in academic tracks comes from advantaged social backgrounds, while students in technical and vocational schools are from less privileged backgrounds. Disparities in educational achievement between curricular tracks can therefore be mediated by differential effects of the reform on effort that reflect the difference in composition of the classes. Through the impact that parents exert on their offspring (Boudon, 1974), children of advantaged social classes show more positive attitude towards learning and feel more confident in their school performance. This in turn affects the extent to which students identify with, and value, schooling outcomes participating in academic and non-academic activities.

Students choose to put effort and time in school to balance its social return with its opportunity cost. Breen and Goldthorpe (1997) argue that children aim to acquire a social position at least as advantageous as that of their parents. Thus the relative risk aversion, namely the concern of downward social mobility, varies between social classes and can affect schooling ambitions differently. Clearly, such risk is higher for children from less advantaged origins. It follows that they may be more reluctant to accept additional commitment to school with an uncertain payoff rather than minimising their effort to obtain a more certain, and possibly lower, payoff. This is suggestive of differential effects on effort and school engagement across curricular tracks caused by the reform. It follows that the intervention has reinforced, at least in the short run, preexisting inequalities in achievement among students from different school tracks.

We document in our main analysis that this negative effect is higher for female students, and more pronounced in reading and science than in mathematics. A possible explanation for this finding builds upon the evidence, also found in our data (see Table 4) and in PISA, that female students in technical and vocational schools perform, on average, well below their male peers. This is particularly true for mathematics, which is perceived by school teachers as the most important subject to learn when compared to Italian language and science. As girls are more risk-averse to school failures than boys (see, for example, Borghans *et al.*, 2009), it could be that female students in technical and vocational schools decided to react to the threat of grade retention by investing more in mathematics, and by lowering the effort devoted to other school subjects.

The result is exacerbated by secondary school tracking, that creates homogeneous classes according to ability and social background and thus reduces peer effects (see Zimmer, 2003, Hanushek and Wößmann, 2006). Not only students perform at a higher level if their peers are high achievers, but peers can also act as a buffer by legitimising deviant behaviour. The social stigmas may therefore be less important in schools where a non-negligible share of the population acts lowering effort. For example, Figlio and Lucas (2004) document sharp effects of grading standards for low-ability students when classroom ability is relatively high, supporting the idea that high standards improve the achievement of students mismatched with the typical ability level of their peers. While this is by no means a definitive explanation of our empirical findings, it is a plausible one.

Changes in school inputs caused by the reform represent an additional channel to explain results. We have seen that the reform imposed additional administrative burdens, leaving substantially equal the financial resources made available to schools. Despite the results documented on test scores, we found that the percentage of students admitted to the next grade increased. We have provided evidence that schools reacted by admitting to the next grade in June students who, in the pre-reform period, would have been given the 'educational debt'. If schools lowered the standards required to avoid the organization of summer courses and the remedial exam in September, the negative effect on test scores can be driven by adaptive behavior of students.

Students from less privileged backgrounds are likely to benefit more from reinforcement of positive attitudes towards learning as well as reinforcement of positive values about their schooling outcomes rather than punishment practices. For instance, a growing number of policies in both developing and developed countries provide cash transfers to students from disadvantaged backgrounds to help them stay in school (see, for example, Dearden *et al.*, 2009). Besides, merit-based scholarships have been recently found to raise not only school attendance but also test scores and boost classroom effort (Kremer *et al.*, 2009). Chevalier *et al.* (2012) find that positive incentives impact on students' willingness to exercise effort. In addition, recent comparative studies reveal that students perform significantly better in countries where large shares of schools use accountability measures to make decisions about students' retention or promotion (Wößmann *et al.*, 2007).[21]

Previous research has also demonstrated that teachers' expectations have a significant impact on student achievement. Teachers with high expectations are a positive predictor of students' goals and interests, while negative feedback is the most consistent negative predictor of academic performance (Wentzel, 2002). More generally, how teachers manage classroom culture (e.g. creating a supportive environment for low-achieving students) has a strong impact on student motivation and passing rates (Akerlof and Cranton, 2003). Given that teachers' expectations have been found to vary a great dael depending on socio-economic status of students (Rist, 1970; Bowles and Gintis, 1976; Speybroeck *et al.*, 2012), it seems reasonable to claim that expectations of teachers in academic schools are higher than those of teachers in technical and vocational schools where, conversely, students typically show a higher susceptibility to negative teacher expectations. The reform considered in this paper may have induced teachers to react differently across curricular tracks, thus contributing to some of the negative effects that we have documented.

[21] The idea that negative sanctions do not raise learning of low motivated and poorly motivated students learning efforts but, on the contrary, strengthen previously existing – or even generate *ex novo* – negative attitudes towards school and teachers, is supported, as far as Italy is concerned, by data regarding the distribution across school tracks of retention and drop-out rates. For instance, in the school year 2006/07 the average retention rate among first grade students of high school varied from 11 percent in academic schools, to 24 percent in technical school and 33 percent in vocational schools. The proportion of students who, in the school year 2007/08, repeated the grade displays an opposite trend (94, 88 and 85 percent in academic, technical and vocational schools, respectively). The remaining students dropped out. Hence, it can be claimed that poorly motivated and low performing students from low class origins are more inclined to leave school, rather than to try improve their performance, as a reaction to the negative sanction represented by retention.

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Appendix

Table A.1. *Average effects for achievement (sensitivity analysis - Main Sample).*

Academic Schools

	Reading		Mathematics		Science	
	Males	Females	Males	Females	Males	Females
Effect	-20.6359	19.2969	-0.1515	7.2619	14.5582	11.1516
	(15.5322)	(13.0739)	(14.5347)	(11.9800)	(16.4977)	(8.7610)
Baseline	521.3540	505.7525	532.5818	491.2104	528.2151	512.9147
	(12.6388)	(7.8504)	(11.3752)	(6.9062)	(11.7942)	(7.1733)

Technical and Vocational Schools

	Reading		Mathematics		Science	
	Males	Females	Males	Females	Males	Females
Effect	3.1842	-44.9988	-3.6869	-6.8148	-0.5680	-43.9076
	(14.4280)	(12.9602)	(18.9895)	(18.3495)	(17.5347)	(13.8951)
Baseline	415.6732	447.7098	481.5509	423.1282	459.6394	455.3818
	(10.8549)	(9.7333)	(16.4587)	(14.8501)	(13.6718)	(9.8758)

Notes. Results were obtained from separate groups defined by gender and curricular type (academic and technical or vocational), using the propensity score weighting estimator described in Imbens (2004). Baseline figures refer to reading, mathematical and scientific literacy in areas not affected by the reform. Standard errors, in parentheses, were computed via bootstrap using 200 replications, allowing for clustering at the class level.

Table A.2. *Average effects for achievement (sensitivity analysis - Main Sample).*

	Reading		**Mathematics**		**Science**	
	(1)	(2)	(3)	(4)	(5)	(6)
	Males	**Females**	**Males**	**Females**	**Males**	**Females**
Academic Schools						
	-26.0238	1.7277	2.5943	-3.7520	10.3944	-0.0456
	(7.2362)	(8.6828)	(9.0698)	(6.9193)	(7.8627)	(6.5460)
Technical and Vocational Schools						
	-0.1710	-22.6390	-11.8815	22.5778	-3.0561	-21.1343
	(7.7645)	(6.6710)	(9.2264)	(9.4210)	(8.8225)	(6.7858)

Notes. Results were obtained from separate regressions by gender and curricular type (academic vis-à-vis technical or vocational). All regressions include as controls the variables in Table 3, and a proxy for student's ability obtained as explained in Section 5.2. Robust standard errors clustered at the class level are reported in parentheses.

Table A.3. *Average effects for family inputs (PISA Sample).*

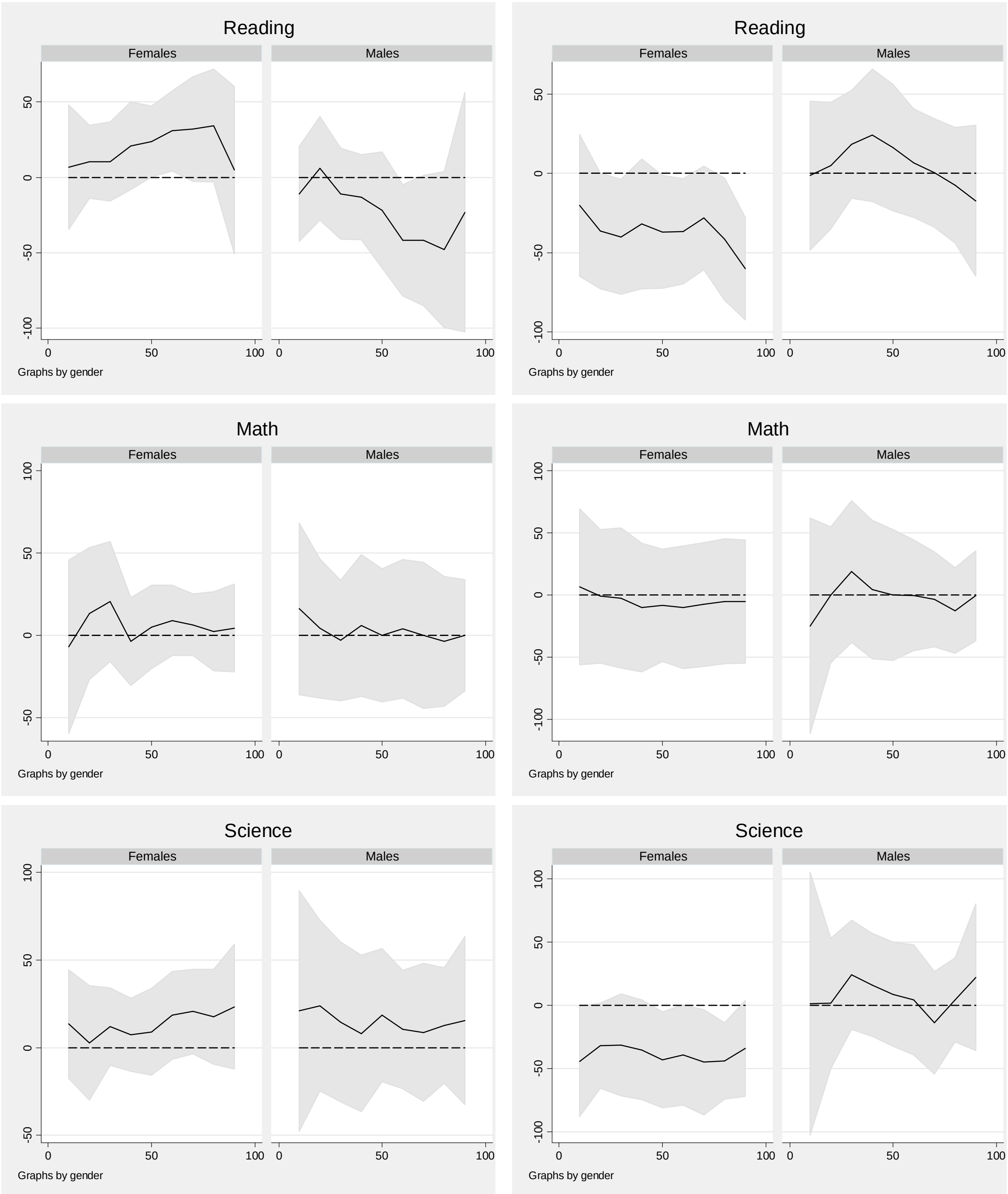
	AC (1)	TV (2)	AC (3)	TV (4)	AC (5)	TV (6)
	Methods		**Discipline**		**Education**	
Area	-0.0555	-0.0470	-0.0439	0.0528	0.0332	0.0477
	(0.058)	(0.035)	(0.032)	(0.036)	(0.052)	(0.038)
Time	-0.0431	-0.0875	-0.0815	-0.0021	-0.0064	-0.0514
	(0.052)	(0.030)	(0.031)	(0.039)	(0.047)	(0.037)
Effect	0.0397	0.0533	0.0817	-0.0473	0.0034	0.0280
	(0.058)	(0.042)	(0.039)	(0.044)	(0.058)	(0.043)
Constant	0.8927	0.9016	0.8930	0.8332	0.6909	0.8930
	(0.113)	(0.104)	(0.092)	(0.107)	(0.132)	(0.093)
Observations	1,008	1,186	1,017	1,191	1,006	1,189

	AC (7)	TV (8)	AC (9)	TV (10)
	Progress		**Spending**	
Area	-0.0343	0.0562	26.9392	-8.6224
	(0.063)	(0.055)	(24.338)	(10.650)
Time	0.0140	0.0023	-23.5837	-37.9330
	(0.042)	(0.068)	(22.939)	(16.156)
Effect	-0.0462	-0.0271	-18.5209	9.9809
	(0.052)	(0.077)	(26.098)	(21.791)
Constant	0.7886	0.7469	209.0503	202.209
	(0.116)	(0.117)	(55.470)	(40.018)
Observations	1,014	1,193	1,013	1,164

Notes. Results were obtained from a difference in differences equation, by Academic (AC) and Technical and Vocational (TV) schools, using information from the PISA parent questionnaire. The variables considered are: “I am happy with the content taught and the instructional methods used in my child’s school” (Methods, columns (1)-(2), dummy for agree or strongly agree); “I am satisfied with the disciplinary atmosphere in my child’s school” (Discipline, columns (3)-(4), dummy for agree or strongly agree); “My child’s school does a good job in educating students” (Education, columns (5)-(6), dummy for agree or strongly agree); “My child’s progress is carefully monitored by the school” (Progress, columns (7)-(8), dummy for agree or strongly agree); “Education costs” (Spending, columns (9)-(10); the variable is coded by PISA using the following categories: less than 100EUR, between 100EUR and 200EUR, between 200EUR and 300EUR, between 300EUR and 400EUR, and more than 400EUR. We take as reference values for these categories 50EUR, 150EUR, 250EUR, 350EUR and 500EUR, respectively). Standard errors, in parentheses, are clustered at the school level.

Figure A.1. *Quantile treatment effects for achievement (sensitivity analysis - Main Sample).*

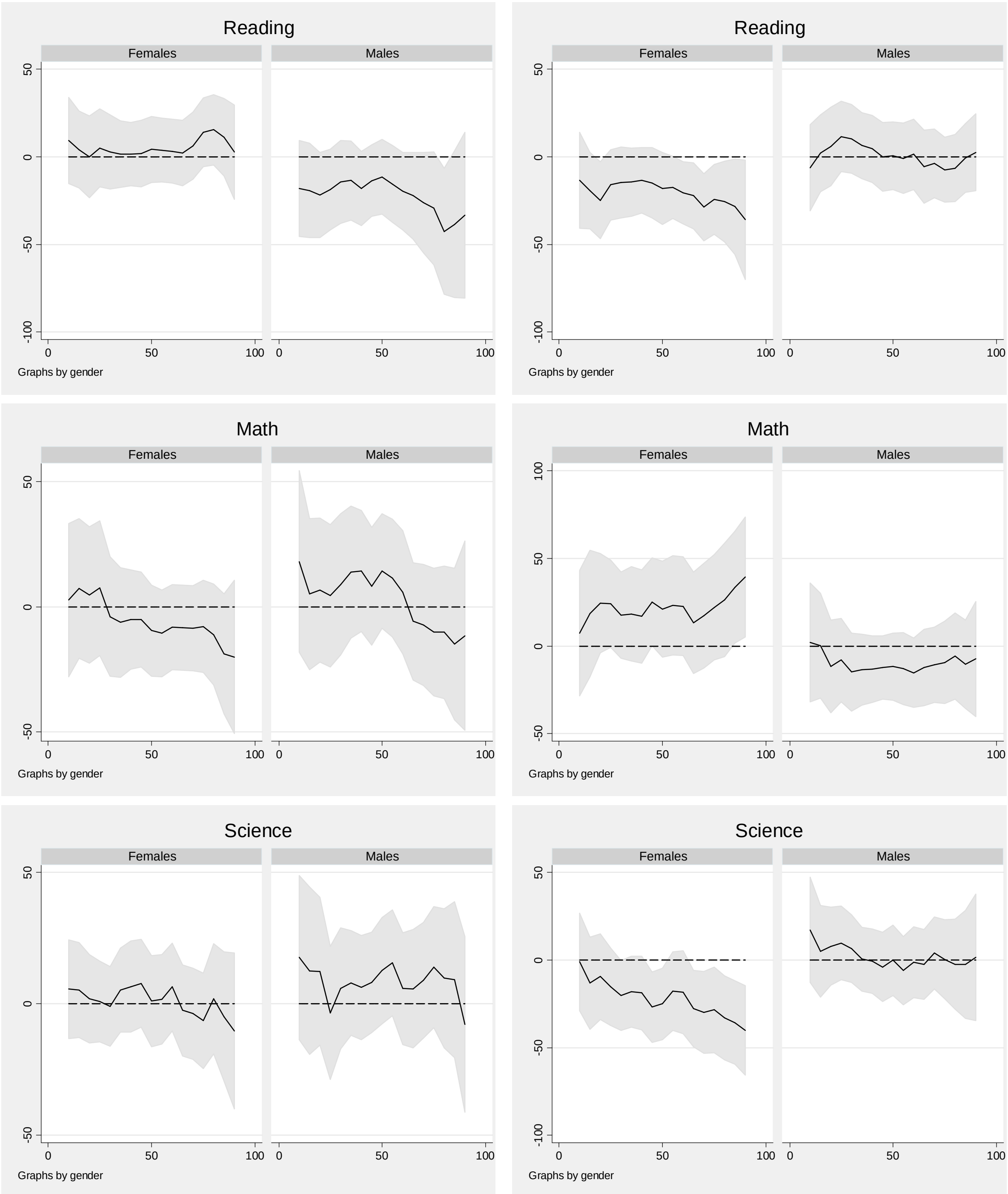
Academic Schools **Technical and Vocational Schools**



Notes. Reported are estimates of quantile treatment effects for reading, mathematical and scientific literacy. Results were obtained from separate regressions by gender and curricular type (academic vis-à-vis technical or vocational), using the method developed by Firpo (2010). Bootstrap standard errors based on 200 replications and clustered at the class level are used to compute 95% confidence intervals.

Figure A.2. *Quantile treatment effects for achievement (sensitivity analysis – Main Sample).*

Academic Schools | **Technical and Vocational Schools**

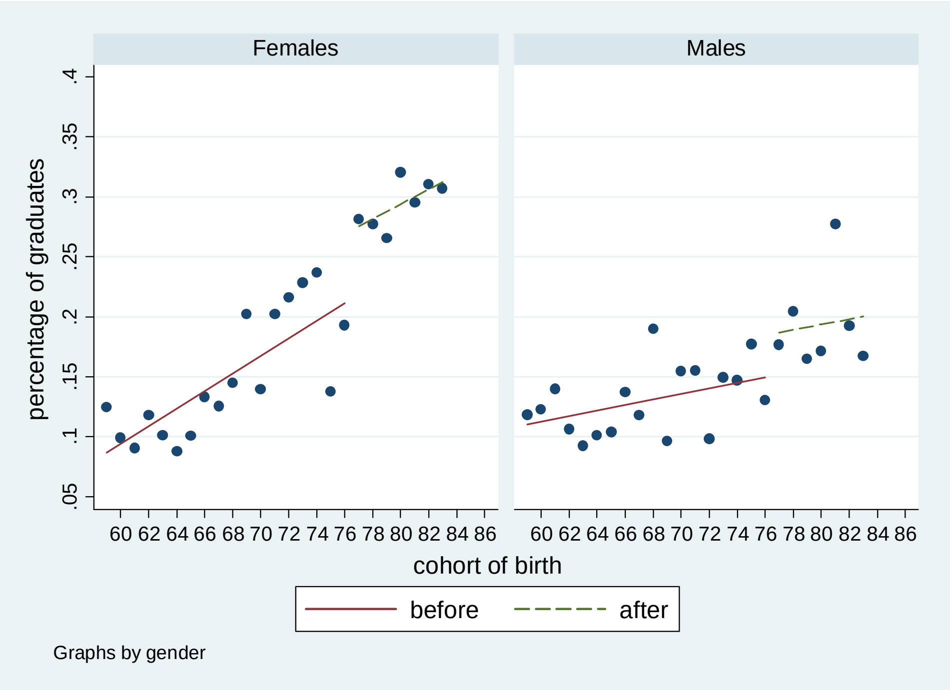


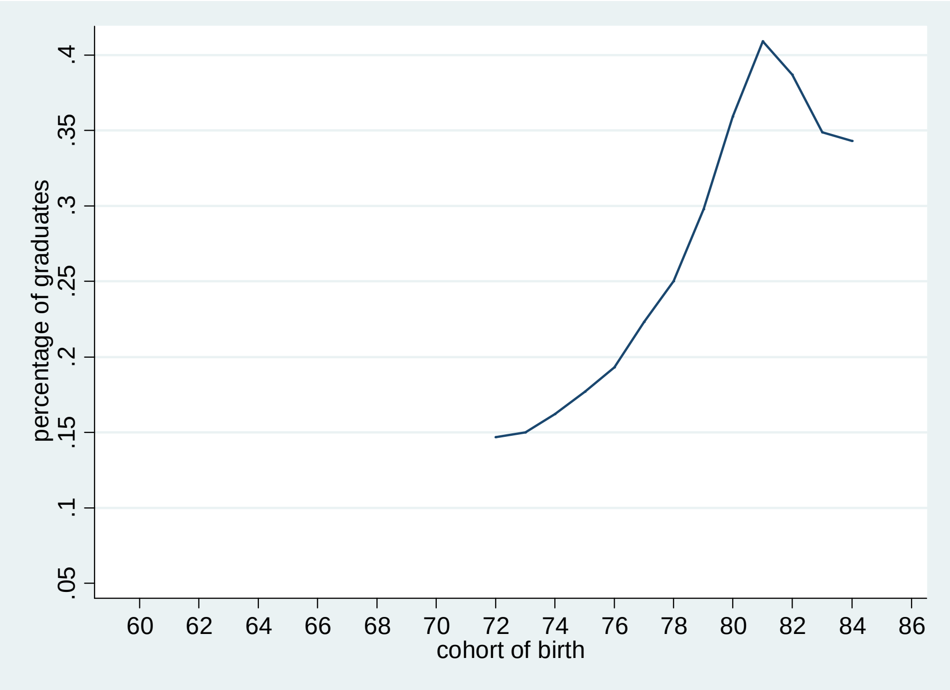
Notes. Reported are estimates of quantile regressions for reading, mathematical and scientific literacy. Results were obtained from separate regressions by gender and curricular type (academic vis-à-vis technical or vocational). All regressions include as controls the variables in Table 3, and a proxy for student's ability obtained as explained in Section 5.2. Bootstrap standard errors based on 200 replications and clustered at the class level are used to compute 95% confidence intervals.

Figure A.3. *Classification of Italian provinces depending on policy status and survey design.*

Notes. The province of Trento (Northern Italy, in light blue) is the only province not affected by the reform. An Italian province is a territory administratively similar to a US county. Provinces in dark blue are used as donors in the evaluation design to match schools in the province of Trento to similar schools outside its administrative border. All remaining provinces are discarded from the analysis. See Section 3 for additional details.

Figure A.4. *Percentage of University Graduates by cohort of birth (1994 reform).*





Notes. Reported is the percentage of university graduates by cohort of birth computed in 2010 from the Bank of Italy Household Survey on Income and Wealth (top panel), and using figures published from ISTAT (Italian National Bureau of Statistics: bottom panel). Cohorts born before 1976 are not affected by the 1994/95 reform of the upper secondary school system (see Section 5.4 for details).