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On the Causal Effects of Selective Admission Policies on Students' Performances. Evidence from a Quasi-experiment in a Large Italian University

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On the Causal Effects of Selective Admission Policies on Students' Performances. Evidence from a Quasi-experiment in a Large Italian University

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Abstract

Using a difference-in-differences approach, we exploit a quasi-experiment occurred in a large public university located in Southern Italy, to examine whether the introduction of a selective admission test affects students' performances. Our analysis on this unique data set shows that a change of regime to a restrictive admission policy reduces the dropout rate of first year students by about 14%, increasing their grade point average by 0.78 points. Interestingly, in addition to the increase in the average quality of admitted students, our results suggest that positive effects of a policy change to the testbased selective access largely operate through the impact of a better quality of peers on individual performances. Results are robust to various robustness checks. Compared with other studies using a similar approach, our own provides evidence that selective admission policies can induce different educational outcomes in different geographic areas of the same national system of higher education.

Keywords: Tertiary education, Selective test based admission policies; students' performances; peer effects; quasiexperiment.

JEL Classification: I21; I28; C21

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Table of contents

1. INTRODUCTION

To the extent that growth and development of nation economies hinge on human capital accumulation, college access and students' performances are important policy issues in tertiary education in both developed (Goldin and Katz, 2008) and developing countries (Hanushek and Woessmann, 2012a, 2012b). The organization of tertiary education systems world-wide is therefore under continuous scrutiny by the policymakers in the quest for the relaxation of the specific quantity-quality trade-off in this segment of education (D'Amato and Mookherjee, 2012). On the one hand, selectivity of the institution fosters excellence standards shifting the nation economy on the technological frontier; on the other hand, the extended participation by increasingly larger fractions of the population, fosters social and occupational mobility and brings up structural transformations of the economy. Our aim, in this paper, is to perform an evaluation of *local* admission policies, asking whether *selective* test based access improves students' performances as measured by the dropout rate and grade point average. Our analysis is based on a quasi-experiment occurred in a large public university located in the South of Italy. One of the key contribution of the paper is the understanding of the channels through which the selective admission policy operates.

Extensive literature has highlighted the national specificity of tertiary education systems and Italy is a particularly interesting case for our aim. During the 1990s and in reply to nationwide policy changes pushing the expansion of enrollment, "Italian universities started to expand their supply, both in terms of opening of new sites by established universities in neighboring cities and by offering a broader variety of degrees" (Bratti et al. 2008). The Italian context is a particularly interesting case in point for the analysis of local admission policies, since, as argued by Bratti et al. (2008), "Italian policy-makers did not pursue the route of stratifying the supply of tertiary education (between university and non-university or between public and private), but followed the principle of autonomy, encouraging local solutions while still retaining a regime of central approval". There is some evidence that the wave of reforms in Italy may have hit a particularly hard rock in their maneuvering through the quality-quantity trade-off mentioned above "as the greater availability of courses had a significantly positive impact only on the probability of university enrolment but not on that of obtaining a university degree" (once again in Bratti et al. 2008). The issue on whether increasing enrolment into higher education institutions, therefore, is a winning strategy in the "race between education and technology" is still open, if the observed effect of a better access entails students' careers that are too long or are interrupted (drop-out)[1].

Beyond Italy, the problem of interrupted careers (drop-out) in higher education has become an increasing concern in OECD countries. In a sample of 18 OECD countries, on average 31% of students entering tertiary education left without at least a first tertiary degree in 2008 (OECD, 2010). Even though the optimal dropout rate is not necessarily zero in general (see Manski, 1989, on schooling as experimentation), such a high dropout rate can be seen as the cost of policies aimed at extending access (OECD, 2009; OECD, 2010).

To relax the above stated trade-off and in order to improve students' performances, a range of solutions have been considered in the literature. A specific instrument is to make use of selective admission tests to target a minimum quality level for students entering the institution. Straightforward theoretical arguments (Stiglitz, 1975; Fernandez and Gali, 1999) can be used to argue for the effectiveness of admission entry tests, operating as a screening device on individual ability, in improving observed students' performances. In such a setting, it should be obvious that, to the extent that the test is correlated with inner individual ability, motivation and other conditions for good individual performances, a more selective policy leads to better educational outcomes.

Surprisingly, the empirical evidence on the effects of implementing test based selective admission schemes on student performances[2] is controversial, perhaps on the account that the main part of it is based on correlational studies. Overall

[1] See Brunello and Winter-Ebmer (2003) and Bound et al. (2012), among others, for further details on this issue.

[2] See Dale and Krueger (2002) and Hoekstra (2009) for evidence on the relationship between selectivity and future earnings.

this literature boils down to an attempt to tackle the question of whether the test, as a measure of individual ability, is a good predictor of student performance. Some papers found that higher education admission tests contribute substantially to the prediction of students' performances such as grade point average, graduation and persistence (Bridgeman et al. 2004; Park and Kerr, 1990; Noble et al. 2005). Some other papers suggest that selective tests are positively related to students' performances only if high-school grades are taken into account (Betts and Morrel, 1999; Cohn et al. 2004; Ragan et al. 2011; Burton and Ramist, 2001). In opposite direction there exists some evidence that once high school grades have been taken into account, the use of admission tests' scores is largely redundant (Crouse and Trusheim, 1988; Geiser and Santelices, 2007). Correlational studies, as it is well-known, suffer from self-selection bias. Sorting of individuals in faculties/universities may be driven by parental (cultural and economic) background, soft skills, preferences, motivations or any other unobservable factors also correlated with educational outcomes. These selection effects may hinder the information content of these correlational studies and overall, this literature does not allow us to retrieve robust evidence about the impact of test based admission policies on students' performances.

The analysis of the impact of a test based selective admission policy on students' performances, requires a thorough assessment of the possible causal chains and the exclusion of confounding factors. Although a formal model of the behavioral reaction by an heterogeneous population of students to the intentional introduction of a test based admission policy by a self-interested Faculty is outside the scope of this paper, we can easily describe the consequence of the introduction of a test in such a setting in words. A reform from an unrestricted access policy to a selective admission policy intentionally introduced by a tertiary education institution can affect observed students' performances through three channels: 1) the direct effect induced by the selection of students with better individual ability, the selection of students performing well enough in the test would turn out in a better average school performance (somewhat trivial, to the extent that the test reflects individual ability more efficiently than the score in the high school diploma), 2) an indirect effect driven by a change in expectations held by students about the quality of the institution, as in the case in which the announcement of the test is interpreted as a credible signal of a commitment by the educational institution to a better quality; in this latter case the introduction of a test based selective policy would be intentionally driven by reputational concerns by the Faculty in the market for education services and it may induce self-selection by perspective students; 3) an indirect effect of the test based selective policy driven by their post enrolment consequences: a smaller class size and/or a higher quality of peers. By abiding with the prevailing terminology we deem channels 1) and 3) as having a causal interpretation, whereas 2) as the main confounding factor due to expectations driven self-selection process. In our analysis we aim at providing an estimation of the causal effects of the introduction of a test based selective admission policy and an exploration of the causal mechanisms described in 1) and 3) taking care of the confounding factor arising because of 2). We will provide detailed arguments, based on the institutional environment in which it occurred, for why the change in policy can be safely considered as a natural experiment in that it cannot have affected candidate students' expectations.

Our analysis relies on a unique large data set from the university of Salerno, in Southern Italy. A difference-in-differences approach is used, exploiting a quasi-experiment consisting in the introduction of a test based *selective* admission policy in the Business and Economics curriculum at a different period, vis-à-vis other curricula. Under the assumption of common trend in students' performances across curricula, this approach should allow us to retrieve the causal effect of a test based selective admission policy on students' performances.

An important contribution of the paper is to investigate in some detail the causal chain triggered by the introduction of a selective test. In particular, we explore whether and to what extent the channels described above have been operative in driving the average treatment effect in our quasi-experiment setting. Details are provided in the section dedicated to the identification strategy; here it is sufficient to say that in our analysis we exploit the fact that, in the years before the introduction of the *selective* test, a *non-selective* test was required in order for students to get enrolled in the curriculum in Business and Economics at University of Salerno (our treatment group). This allows us to construct a counterfactual sample of students, by excluding those who would have been denied access if the *same selective rule* had been in place in the year of their enrolment. Hence, the observed performances by those students (whose actual career started under the previous non selective policy regime) that would have been selected under the new policy regime can be compared with the observed performances in the sample of students whose career initiated under the new (restricted) access regime. By comparing performances of students with similar ability as measured from the test but different post enrolment environment, we will explore the possible sources of the causal chain and the specific role of peer group quality on students' performances.

Indeed, the influence of peer group quality is an intense area of research by educational economists (see for instance Epple and Romano, 1998; Hoxby, 2000). Peer group can affect student achievement in various ways: "members of a group interact in learning, help each other in their studies, share important information, impose externalities on others by behaving well or badly (for example, a noisy student disrupts the study environment) or by allowing teachers to go deeper in subjects, contribute to the formation of values and aspirations, and so on" (see De Paola and Scoppa, 2010). The impact of peer quality is very hard to be properly estimated using observational data because of the well-known self-selection and reflection problems (Manski, 1993). The policy change occurred at the University of Salerno allows us to exploit an exogenous variation in the peer group quality which allows to overcome such endogeneity issues. Also other few papers (i.e. Sacerdote, 2001; Zimmerman, 2003; Foster, 2006) use quasi-experiments to estimate the impact of peer quality on undergraduate students' performances but, to the best of our knowledge, there are no works analyzing the effects of peer quality on students outcomes in European Universities in a quasi-experimental setting.

The following results emerge from our analysis: the estimated effect of increasing restrictedness in the admission policy is a reduction in the first year student's dropout rate by about 14% and an increase in their GPA by 0.78 points, with a significant effect that can be attributed to the consequences of the test based selection on the improved quality of the peers.

We also discuss issues of external validity by comparing our results to related studies within the same national institutional environment. Interestingly, the only study on the effects of selective procedures on students' performances in a quasi-experimental setting we are aware of, finds no significant effects of admission tests on the probability of obtaining the degree in a private university located in the North of Italy (Francesconi et al. 2011). In this study a quasi-experiment very similar to the one used in our analysis is used, taking advantage of the time variation in the policy consisting in the *removal* of the selective procedure across curricula. According to these authors, the removal of a selective procedure does not affect student's performances[3].

Our investigation focuses, instead, on the effectiveness of selective procedures in a completely different geographical and institutional setting, namely in a large public university located in the South of Italy in which students almost exclusively enroll from the regional market[4]. We believe that our analysis is interesting to assess the impact of a change in admission policies in a context where the pool of applicants (in terms of student's family background, proximity to the university, motivations, preferences, etc.) is likely to be very different from the one in a private university. Overall, we conclude that the same policy instrument (a reform of the admission policy regime) has dramatically different effects when used by private versus public universities and that positive effects of test based admission policies are largely driven by a different mechanism than the sheer selection of better individual abilities, mainly a peer quality effect.

[3] According to the authors, this result may be explained by the fact that "institutional selectivity by private universities, therefore, may never be successfully sustained in such an environment, where public institutions can offer a valuable outside option to students' enrolment decisions" and by the "expansion of the aggregate supply of university slots in Italy, which led to a considerable increase in uncertainty and competitive pressure felt by all incumbents, including the university under study".

[4] Notice that the Italian university system is rarely based on campus facilities. Residential costs for those students moving out of their parental home, therefore, are significantly larger than for those staying at home. In addition, rents and other living costs are substantially larger in the northern than in the southern regions.

The rest of the paper is organized as follows: section 2 provides specific institutional details on the policy experiment leading to a more restrictive access and illustrates the data used as well as the identification strategy, section 3 describes the results, section 4 provides robustness checks and sensitivity analysis. Finally, section 5 concludes.

2. INSTITUTIONAL SETTING, DATA AND IDENTIFICATION STRATEGY

2.1. Institutional setting

Until 1994, the Italian higher education system has been characterized by the presence of no limits in the universities' access policies (with the exception of few private universities and Medical Schools). Non-selective entry tests (meaning the result of the test does not prevent the enrollment in the curriculum) have been formally introduced by the Ministerial Decree 270/2004 (art. 6), aiming at evaluating students entering a curriculum. The University of Salerno, as many other Italian Universities, complied with this law by proposing a compulsory non-selective test, that is to say a test to which students have to participate, but whose results would not undermine student's entitlement to enroll. Under the non-selective entry test regime students are admitted regardless of the score, albeit subject to a penalty in the form of specific additional course credits to be acquired by those receiving a test score below a certain threshold. In the academic year 2010/2011 a local (i.e. not on a national level) selective admission test was introduced by the Faculty of Economics for students who had chosen to enter a curriculum in Business and Economics at the University of Salerno. The test worked in the following way: out of n applicants ($n=1200$ in the population considered) only k students were admitted ($k=800$) in the Business and Economics curricula, based on the rank obtained in the test. Importantly, no minimum quality standard (i.e. a minimum score in the test) has been used. The test consisted in a written class examination which tested candidates on logic, comprehension text and mathematics, following a nationwide format (other Business and Economics Faculties in a few other universities in Italy were adopting the same policy). Importantly the selective entry test was implemented in the faculty of Economics while all remaining faculties did not change their admission regime. This policy change will allow us to identify the casual effects of introducing a test based admission rule in a quasi-experimental setting, described in details below.

2.2. Data

The empirical investigation has been performed using a unique administrative dataset featuring information on students enrolled at the University of Salerno in a period spanning from academic year 2005/2006 to 2010/2011. The sample is made of 27771 entrants, enrolled in the following degrees: Economics, Engineering, Art and Philosophy, Languages, Educational Science, Math, Physics and Natural Science and Political Science. Other courses which have always been enforcing a selective entry test in the academic years covered by the data set (namely Cultural Studies, Biology, Communication Sciences, Education Studies) are excluded from the sample. The dataset contains information about individuals' characteristics (gender, age, residence), educational background and pre-enrollment characteristics (type of high school attended and high school diploma score), households' financial conditions (family self-reported income) and general information about the universities' careers and performances (degree, credits acquired and marks obtained).

On average, the number of male students is higher in the treatment group (53%) than in the control group (46%), while the average age is about the same in both groups (around 20 and 21 years respectively). Moreover the treatment group has, on average, a lower percentage of students (about 45% versus 52% in the control group) coming from a Lyceum (considering Scientific, Classic and Linguistic lyceum) while the average high school final score is about the same (around 78 points) in the two groups. On average, the ratio between students and professors is higher in the treatment group (10.8 versus 7.1 in the control group). In other words the Faculty of Economics exhibits a higher than average number of students per professor. Finally, students in the control group have a higher dropout rate (42%) and a higher GPA (25 points) than those in the treatment group (35% and 23 points respectively). See Table 1A in the Appendix for more details on the descriptive statistics.

2.2.1. Variables

We use two indicators for students' performances: the dropout and the grade point average. Both outcomes are measured at the end of the first year. This follows the Italian Ministry of Education, Universities and Research guidelines, according to which, universities are evaluated also on the base of indicators such as the drop-out rate after one year or the percentage of students who enroll in the second year, having acquired a certain amount of credits. In other words, the transition between the first and the second year has been considered by the Ministry of Education as the main checkpoint to evaluate the regularity of the educational path.

Our definition of a dropout student, in line with most of the literature, is broader than the one used for administrative purposes [5]. We consider a student as a dropout both when she officially withdraws after the 1st year from the university after presenting a formal request to the student office and when she does not renew the registration leaving the degree program (in our case after the 1st year) in which she had been enrolled. Notice that students who do not renew their registration but filled in a formal application to move to a different university have not been considered as dropout in our study. Moreover, students who do not renew their registration but, by appropriate procedure on the data set, are found to be enrolled in another Faculty of University of Salerno the following years are not considered dropout either[6].

Performances of students in the exams are measured by the grade point average. Grade point average (grades weighted by the corresponding credits after the 1st year) has been calculated in the following way:

$$Grade\ point\ average\ (GPA)_i = \frac{\sum_{i=1}^{n}\sum_{j=1}^{m} grades_{ij} * credits_{ij}}{\sum_{i=1}^{n}\sum_{j=1}^{n} credits_{ij}} \tag{1}$$

for a student i who passed the exam $j \in \{1, \dots n\}$. Exams (e.g. proficiency in the usage of computer packages, the so called “prova informatica”) to which only credits and no marks are assigned are, therefore, excluded.

We include several control variables to account for possible differences between treatment and control group which cannot be attributed to curriculum fixed effects. Specifically, a dummy variable for gender has been included (Mastekaasa and Smeby, 2008). Age has been modeled as a continuous variable. Based on the high school qualification, six levels of education have been considered: Scientific Lyceum, Classical Lyceum, Linguistic Lyceum, Professional school, Technical school and other institutions. Information about past educational history has also been included. Indeed, secondary school track represents a channel through which the family environment influences the level of education achievements inducing intergenerational correlation in educational attainment (Checchi et al. 2013; Carneiro and Heckman, 2005; Dustmann, 2004). High school final scores have been considered as a continuous variable, ranked on a scale from 60 to 100. These scores are considered as a proxy for proficiency and individual ability, indeed they have been shown to be an important predictor of students' outcomes (Di Pietro and Cutillo, 2007; Cappellari and Lucifora, 2009). In order to take into account the time (years) between the end of high school and the university enrollment, a dummy variable (1 if enrolled in the year of the diploma, 0 otherwise) has been included, supported by the evidence that the number of years between the secondary education diploma and the enrollment in the university might be correlated to the

[5] See, among others, Boero et al. (2005) and Belloc et al. (2009).

[6] This is to avoid to put together forms of leaving behavior different in their characteristics. According to Tinto (Tinto, 1975), failure to separate permanent dropout from temporary and transfer behaviors has often led institutional and state planners to overestimate substantially the extent of dropout from higher education.

students' performances (Belloc et al. 2009). Moreover, according to Holmlund et al. (2008), "postponement of higher education is associated with a persistent and non-trivial earnings penalty".

As proxy for education costs, a few control variables were considered: student's distance from campus (measuring local transportation costs) and student's family income (measuring wealth constraints)[7].

As for the distance variable, the relevant literature (Johnes and McNabb, 2004; Belloc et al. 2010) refers to the student's residence in the city, province or region where the university is located. Transportation costs are measured by the distance- in kilometers- between the student's residence and the university campus[8]. The use of such variable is meant to take into account both the cost of reaching the campus and the cost of renting a room which might influence students' motivations and is supported by the evidence showing that those who live at the parental address and off campus have a higher probability of dropping out (Smith and Naylor, 2001).

As for wealth constraints, we also control for the family's socioeconomic status (see Betts and Morell, 1999) using a measure of Equivalent Household Income (ISEE). This is a measure of student's economic status averaging household income, parental wealth and family size[9].

Finally, we control for the potential confounding effects of reduced class size on students' performances: a measure of student/professor ratio, at faculty level, has been included among the regressors, namely the proportion of students enrolled over of the total number of academic staff, for each academic year. This measure is included in order to control for the possibility that the impact of selective test on students' performances could run through a class size effect (see Bandiera et al. 2010; Kokkelenberg et al. 2008).

2.3. Identification strategy

As stated in the introduction the effects on educational outcomes of test based admission policies may arise from several channels. A channel is due to a reputational effect according to which students form better expectations about the quality of the institution who promoted the test, hence better quality and more motivated students *self-select* into a degree in response to the announcement that a selective test would be enforced by a faculty. This could operate through a signaling or reputational mechanism (the faculty sends a quality signal by affording the cost of a selective procedure and, hence, attracts better students) and it operates through a change in student's expectations induced by the announcement of the selective test. Since reputational concerns trigger change in expectations and hence self-selection of those applying for the test, we consider this as a confounding factor.

A second channel, is a standard compositional improvement induced by the enforcement of the test and the ensuing enrolment of better qualified candidates. The faculty intentionally sifts out students with higher *individual ability*; average performances improve as a direct consequence. The third channel - a peer effect- is related to the *average* quality changes in the pool of admitted students and operates through the effect of a better quality of peers on individual performances. In other words, additional to the increase in the average quality of admitted students, the effects of a policy change on

[7] Notice that university fees are quite low in Italy. For instance, in the academic year 2010/2011 students who paid the lowest fee, corresponding to € 362.00 per year for a full time student, are those which, according to the income tax returns, have an income up to € 4,500, while those with an income tax return higher than € 32,000.00 pay the highest fees, corresponding to € 1,179.00 per year for a full time student.

[8] In order to calculate that distance "Google map" has been used. Specifically the distance is considered as the best and fastest way, suggested by "Google map", to reach the university campus.

[9] The ISEE index is an instrument used to measure the actual property and income position of citizens that apply for social services under favourable terms and is determined by combining and evaluating three elements: income, assets and composition of the household. To calculate the ISEE index to the fiscal year at time t, gross income to all members of the household at time t as reported at time $t+1$ is used, along with the composition of the self-reported information about the value of household's assets in real estate at the end of t year, cadastral certificates or other documents regarding real property, etc.

individual performance can operate through a better post enrolment environment induced by the test based admission policy[10].

Self-selection arising from the reputational concerns by the promoting institution could be controlled for under the hypothesis that both reputational effects and training effects are fixed over time in the treated faculty. In support of this hypothesis we follow two routes. Firstly we rely on a detailed account of institutional aspects which we believe prevent such reputational effects to arise; secondly, we test in the data that the composition of the applicants does not change in response to the introduction of the test, according to a measure of ability. Indeed we have strong priors that, in the given institutional setting, this assumption can be credibly maintained for at least three reasons. The first reason is that the time span between the decision about the enforcement of the test, the announcement and the actual deadline for application, was too short to allow students to readjust long run plans[11] about their tertiary education and to invest in specific training activities. Specifically, the decision to enforce the test was taken at the end of April by the Economics Faculty, it was announced and campaigned in late July, the deadline for the application to enroll in the test was late August and the test was actually performed in September 10th. Finally enrollment took place in October. The second reason is that the Faculty of Economics had already in place a compulsory *non*-selective test devised in a way such that low performing students were admitted, conditional on the attendance of specific additional course credits, as discussed in section 2.1. Arguably, the (costly) provision of the test, in order to signal commitment to quality in teaching by the Economics Faculty, if present was already operating in the years before the selective test has been introduced. The third and decisive reason why the reputational effect cannot be expected to be at work in our setting is the fact that the adoption of the test was not a costly signal to the faculty: the test is paid by students to the University in the form of fees and these revenues are used to finance administrative costs associated to the test procedure. In other words, one of the assumptions under which reputational and signaling effects arise, i.e. the fact that the signal should be costly to the sender, cannot be maintained in the given institutional setting under analysis. Hence for all these reasons, we believe that it is strongly unlikely that the introduction of the selective test in Salerno was conceived and actually operated as a quality signal in the reference market.

We formally test our priors comparing the distribution of secondary high school grades obtained by applicants to the faculty of Economics in the year of the test and in the years before the test was implemented. This comparison (of a change in the sample mean and a change in the distribution) is useful to check whether the quality of applicants in terms of educational abilities changed in the year of the test (see table n. 1 and figure n. 1 below).

[Insert Table 1 around here]

[Insert Figure 1 around here]

Both table 1 and figure 1 confirm our priors: they provide strong support in favor of no self-selection and no emergence of a better pool of applicants in the year of the test. To support the graphical analysis, we also perform a non-parametric test for the following null hypotheses, H_0: the high school grade distribution for students enrolled in the year of the test is equal to that for students enrolled in the years before the test was implemented. The results show that H_0 cannot be rejected

[10] As noted in the introduction another potential consequence of selective test based admission policy can operate through a reduction of average class size. This effect is controlled for in our estimates through the inclusion of student/teacher ratio as a control variable, as discussed in the section 2.2.1.

[11] These plans involve financial planning on parental assets which are difficult, on average, to adjust on such a short horizon.

(see figure 1). Indeed, there is no significantly statistical difference in the data, in the year of the test and in the years before, *either* in average diploma grades *or* in the distribution of grades of applicants.

Our identification strategy of the causal effect of selective test operating through the admission of better qualified candidates relies on the fact that the introduction of the selective entry test was not implemented in all faculties in the same year. One of the groups (students enrolled in the Faculty of Economics) is exposed to a treatment (selective entry test) in the second period (academic year 2010/2011) but not in the first period (academic years before 2010/2011). The second group (students enrolled in the following faculties: Engineering, Art and Philosophy, Languages, Educational Science, Math, Physics and Natural Science and Political Science) is not exposed to the treatment during either periods. We compare the difference in outcome between treatment and control group in the second period with respect to the first period controlling for unobservable factors varying at faculty level and fixed over time.

To identify the effects of selective test operating through better peer quality, we exploit the fact that before the introduction of the selective test, a compulsory but not selective test was already present at the Faculty of Economics. Thus we compare the performances of the students admitted through the selective test at the Faculty of Economics to the performances of students enrolled in the years before through the non-selective test that would have been admitted if the selective rule had been applied. More precisely, under the rule of the selective test, 800 out of 1200 (around 65%) applicants have been admitted at the Faculty of Economics in academic year 2010/2011. Their performances are then compared to the performances of students whose test score was in the 65% top distribution of the non-selective test score in the previous academic years. This difference is compared to differences observed before and after the implementation of the selective tests in all control faculties. This comparison allows us to pín down the impact of peer group quality on the students performances because it compares the performances of students with similar ability but with a different quality of their peers.

To recover these effects, we estimate a linear probability model in a DID setting (Angrist and Pischke, 2008)[12]:

$$Y_{ijt} = \alpha + \gamma D_{ij} + \lambda T_{it} + \delta D_{ij} * T_{it} + \beta X'_{ijt} + \varepsilon_{ijt} \tag{2}$$

where Y_{ijt} is the outcome for a student i in period t enrolled in faculty j, D is a dummy which is equal to 1 if the observation is from the treatment group (Faculty of Economics), T is a time dummy which is equal to 1 for the observation after the introduction of the selective entry test and X' is a vector of control variables (see the Appendix for a detailed list of control variables included). The parameter δ is the treatment effect we are interested in, namely the impact of the selective entry test on educational outcomes.

3. RESULTS

Table 2 below presents the difference-in-differences estimates of the treatment effects based on separate linear probability models for both of the outcomes (Dropout and GPA) previously described. We pay particular attention to inference issues which can be problematic in the DID setting (see e.g. Angrist and Pischke, 2008 and Donald and Lang, 2007). Thus, for all the outcomes we report three estimates of standard errors. Columns 1a and 2a report standard errors robust to heteroscedasticity, while columns 3a and 4a report standard errors clustered at degree programs (DPs) and year level. Cluster-adjusted standard errors correct for the possible correlation in performances of students enrolled in the same degree program over time. DPs are the smallest organizational unit within the faculty and they might be effectively the

[12] We use a linear probability model in order to obtain a meaningful interpretation of the interaction effect of our interest, since, as suggested by Ai and Norton (2003), a simple summary measure of the interaction effect is problematic in non-linear models because the effect and the sign of the interaction effect actually changes for each single observation (being dependent on the different values of the covariates). As robustness, we also used a probit model. Results are qualitatively the same and available upon request.

main place where common shocks may occur. The asymptotic approximation relevant for clustered standard errors relies on a large number of clusters (see Donald and Lang, 2007). We have 162 clusters (6 years* 27 DPs) which should be enough to deal with this issue. However, as a matter of robustness, we also report non-parametric standard errors clustered at degree program and year level based on block bootstrap with 1000 replications (see Cameron et al. 2008). Non parametric standard errors are reported in columns 5a and 6a.

In table 2 only the variables more relevant in a diff-in-diff estimates are reported; namely the pre-post dummy (before and after the a.a. 2010/2011), the treatment dummy (Faculty of Economics vs other Faculties) and the interaction term. Estimates of the effects of all control variables are reported in table 2A (see Appendix).

As far as drop out is concerned, column 1a, 3a and 5a show that the treatment effect estimated (interaction term) is around 0.14. This means that the introduction of the test in the Faculty of Economics leads to a statistically significant reduction in the first year student's average dropout rate by almost 14%. Columns 2a, 4a and 6a present the treatment effect on the student's grade point average, conditional on having a GPA different from zero. We find an average increase in GPA, at the end of the first year, by 0.78 points. All in all, our estimates suggest that the introduction of the selective test lead to an increase of the average performances of students admitted at the Faculty of Economics in both educational outcomes. Such effects are significant in all the three models which assume a different structure of the error terms.

As for control variables, we find effects in line with previous literature. Namely, we find better performances on average for women and younger students and for students attending a non-vocational secondary high school (i.e. Classic lyceum). Students obtaining a high school diploma with larger grades also perform better at university, both in terms of dropout probability and GPA. Interestingly, there is no significant relation between student/teacher ratio and students performances, especially after correcting for intra-degree program correlation. This result is not new in the literature. Many other studies (Dobbelsteen et al., 2002; Betts and Morell, 1999) found similar results. More importantly, in a survey of the literature on the effects of teacher-pupil ratio on performances, Hanushek (1996) shows that only 15% of studies found a significant and positive effect of such a ratio on the performances. Despite its low impact, the inclusion of this variable in our regression is important to retrieve an effect of our variable interest purged by the effects of class size on performances.

[Insert Table 2 around here]

3.1 Peer quality and students' performances

In this section we further explore the causal chain driving the results reported in the previous section. As described in section 2.3, the causal effect could have been driven either by the direct effect of the access policy to individuals of better individual ability or by the better environmental conditions in which the students admitted in the new policy regime (treated group) operate. These conditions, after controlling for class size effects (student/teacher ratio), amount to a better average quality of the peer student. To this aim we compare our treatment group, i.e. the top 65% students in terms of test score exclusively enrolled after the reform, with similar students in the pre reform years, i.e. the best 65% students in terms of the test score enrolled along with around 400 additional students with lower performances (remember that the entry test was not selective in the years before the reform). This amounts to compare individual performance after the reform (same control group) with a group of the same individual ability on average operating in an environment with students with a lower performance in the test. Results from the same regression as in Table 2 featuring this different control group is reported in Table 3 below. As for the previous case, we report three models for each outcome, allowing for three different structures in the error term.

The estimates presented in columns 1b, 3b and 5b suggest a reduction of drop out by almost 12.5%. At the same time, we observe an increase in GPA by around 0.68 points (columns 2b, 4b and 6b) among students passing at least one exam. With some peculiarities, results are robust to different hypothesis on the error structure also in this case. Specifically, we detect a small reduction in significance when using cluster adjusted standard errors but still significant treatment effects are present at 5% conventional level. This result may indicate the importance of correcting for intra degree program correlation especially when comparing performances of more homogenous students in terms of ability.

Overall, we still find a statistically significant reduction in the first year student's average dropout rate. Differently from before this effect represents the treatment effect of a test based selective admission policy on individuals performances operating through a better quality of peers.

By comparing the magnitude of the effects shown in table 3 and table 2, we find that when restricting the analysis on better than average students, the estimated effect of the policy change is only a bit smaller. For instance, results in table 2 indicate that the dropout rate is reduced by almost 14% as a consequence of the policy. In table 3, an estimated reduction of 12.5% would be expected because of the change in the average quality of the peers. All these results are robust to the inclusion of the control variables described in the previous sections. The complete list of estimated coefficients is reported in the Appendix (Table 3A). All in all, these results provide evidence about the fact that a change from unrestricted access policy to a test based selection policy induces improvements in students performances that are not driven exclusively by the direct effect of selecting high individual ability students. Rather, by focusing on treatment and control group with similar quality but interacting with different peers, consistently with our identification strategy, results shown in this paragraph suggest that peer quality represents an important mechanism for the causal effect of the policy change .

[Insert Table 3 around here]

4. SENSITIVITY ANALYSIS

In this section we report two checks to verify the robustness of our results and to test the plausibility of the maintained common trend assumption. Firstly, we show graphically the trend similarity in the educational performances among treatment and control group (see Hasting, 2004). Secondly, we carry out a placebo difference-in-differences (i.e. a falsification test) to give more credit to our estimates (see Duflo, 2001; Card and Krueger, 1994).

The idea of using a relatively long time series for outcome measures relies on the fact that it "may allow the researcher to examine if the treatment and control groups tend to move in parallel" (Meyer, 1995) as we should expect in the case of absence of interactions between treatment and other (omitted) influences. Figure 2, below, displays the trend in the outcome variables analysed in this paper, by treatment and control group, in the five academic years before the introduction of the test based access policy, in order to motivate the common trend assumption.

[Insert Figure 2 around here]

We can see in figure 2 that the trend in both the dropout rate and the grade point average, in treatment and control group, is similar in absence of treatment, suggesting that the assumption of common trend can be rather confidently maintained. We also perform a placebo difference-in-differences (i.e. using a fake treatment group), namely we look for two groups whose performance cannot be affected by the policy under evaluation. Remember that the introduction of a test based restricted access policy was implemented by the Faculty of Economics in the academic year 2010/2011. Hence we apply the same difference-in-differences methodology using data on students enrolled in academic years before the policy, namely in 2005/2006, 2006/2007, 2008/2009 and 2009/2010. Under the assumption of common trend the effect of these placebo differences is expected not to be statistically different from zero. Regression results for two placebo regressions are reported in table 4 below. In the first line of table 4 the treatment effect of a placebo regression in which the (placebo) treated cohort is that of students enrolled in the Faculty of Business and Economics in the academic year 2009/10, whereas the control group is as before. Testing the null hypothesis of no treatment effect, we aim at falsifying the assumption of no common trend in the regression whose estimate was reported in Table 2. In the second line of Table 4 the (placebo) treated group and the control group is made of student in top 65% of their test score. Testing the null hypothesis of no treatment effect we aim at falsifying the assumption of common trend in the regression whose estimate is reported in Table 3. It turns out that the estimated treatment effect in both placebo regressions is indeed not statistically different from zero. Therefore, the falsification tests in table 4 provide additional evidence in support of the assumption on common trend.

[Insert Table 4 around here]

5. CONCLUSIONS

Our aim has been to provide evidence on the causal effects of a local reform in access policy to a tertiary education institution. We exploit a change of regime from unrestricted access to a test based restricted access occurred in a large size University located in Southern Italy to estimate the impact of selective policy on students' performances as measured by the dropout rate and grade point average of students. By estimating a difference-in-differences model on a unique administrative data set, we documented that the introduction of a selective entry test leads to a reduction of the dropout rates, at the end of the first year, by almost 14%. This is quite a strong effect as, on average, almost 20% of the tertiary education students in Italy dropped out from the university, from academic year 2002/2003 to 2008/2009 (CNVSU, 2011). Moreover, evidence is provided that more diligent students benefit from the policy change, since an improvement of the grade point average at the end of the first year, by almost 0.78 points is found among those students who have a grade point average different from zero.

Our analysis offers new insights on the effects of selective procedure on students' outcomes. Firstly, differently from previous literature largely based on correlational studies, our paper provides quasi experimental evidence that selective admission policies lead to an increase in the average performances of the students. Compared to other evidence from an analogous quasi experiment from a leading private university in Northern Italy (Francesconi et al. 2011), where no effects of the removal of test based restricted access were found, our study reveal that a change in access policies by local institutions may have different effects, possibly due to different selection effects at work. Our interpretation is that self-selection effects induced by transportation costs and tuition fees in the leading private Italian university in Northern Italy are strong enough that, once the selective test is removed, no worsening of student's performance shows up as empirically relevant. On the contrary, access costs both in terms of tuition fees and transportation expenses to public university in Southern Italy are so small (since the reference market is mainly regional) that a restrictive access policy turns out to be quite effective[13].

Secondly, specific causal chains have been investigated in our paper. Thanks to a specific feature of the environment in which our quasi-experiment had occurred, we are able to compare the performances of the students admitted through the selective test at the Faculty of Economics to the performances of students enrolled in the years before through a non-selective test, that would have been admitted under policy change, i.e. the test based restricted policy had been enforced. By comparing our treated group with a control group made only of individuals achieving the same results in the test score

[13] A leading private University in the North of Italy attracts students from all over Italy, whereas in the University of Salerno most of the students come from the region (Campania) where the campus is located (around 60% from the province of Salerno and around 30% from all other provinces of Campania region).

obtained by those in the treated group, we are able to compare students with similar individual abilities but operating in different post enrolment environment. Controlling for the class size effect, this comparison uncovers the causal effect of the average quality of peers on individual students performances. The estimated effect from this regression documents a reduction of dropout rates by almost 12.5%, similarly the GPA increases by around 0.68 points. In other words, a change from unrestricted access policy to a test based selection policy induces improvements in students performances that are not driven by the direct effect of selecting high individual ability students. Rather, by focusing on treatment and control group with similar quality but interacting with different peers, consistently with our identification strategy, we find that peer quality represents an important mechanism for the causal effect of the policy change. Our estimates would actually suggest that large part of the effect of the change in policy is driven by the effect due to the improvement in the peer quality.

We believe that two lessons can be learned from this analysis. Firstly, we realize that test scores, as measure of individual ability (although imperfect and possibly reflecting other conditions) seem to contain useful information- in our sample- in addition to that contained in the score obtained in the high school diploma. Secondly, we find that restricted access policies seem to have improved students' performances because of better post enrolment conditions, in particular because of the improvement of the average quality of the peers.

The overall conclusion of our study is that, restricting access is very likely to improve performances in tertiary education institutions with a local market, mainly because of improved conditions in post enrolment and not because of the direct effect driven by the sifting out of better individual abilities. Within the limit of external validity allowed by the quasi-experimental approach used in our analysis, we believe that our results are relevant for the assessment of public policies in tertiary education and the overall organization of this segment of education, especially in those institutions where access costs for perspective students in the reference market do not represent an empirically relevant source of self-selection mechanisms. Our study shows that test based admission policies can be an important instrument to university and governance structures (Chancellor, Executive Board and Senate) in order to control the balance between quantity and quality in this segment of education. However their usage should be carefully assessed, since their causal effect on students' performances does not seem to pass exclusively (or mainly, according to our estimates), through the channel- the sifting out of better individual abilities from the pool of the candidates- which is supposed to be the natural channel in test based selective approaches.

An interesting feature where further work is needed concerns the relative informational content of the admission test score vis a vis the score of high school diploma and, in particular, the reasons why admission test maintain their informative value in the design of restricted access policies to tertiary education institutions.

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TABLES AND FIGURES

Table 1 – Comparing the average diploma grade of applicants in the faculty of Economics in the year of the test and in the years before the test was implemented (Standard errors in parentheses)

	High school grade (mean)	
From academic year 2005/2006 to 2009/2010 (non-selective test)	79.1749 (0.1600)	
Academic year 2010/2011 (selective test)	78.4854 (0.4062)	
Difference	-0.3105 (0.4647)	Pr(|T| > |t|) = 0.5041

Table 2 - Difference-in-differences estimates – Effects of introduction of a test based restricted access policy on student's performances

	(1a) dropout between the I and II year	*(2a) GPA*	*(3a) dropout between the I and II year*	*(4a) GPA*	*(5a) dropout between the I and II year*	*(6a) GPA*
Pre-post	0.1721***	-0.1173**	0.1721***	-0.1173	0.1721***	-0.1173
	(0.0089)	(0.0593)	(0.0470)	(0.2598)	(0.0500)	(0.2593)
Treat	-0.0264***	-2.6985***	-0.0264	-2.6985***	-0.0264	-2.6985***
	(0.0086)	(0.0580)	(0.0266)	(0.2470)	(0.0298)	(0.2572)
Treat*pre-post	-0.1399***	0.7860***	-0.1399***	0.7860**	-0.1399**	0.7860**
	(0.0192)	(0.1206)	(0.0495)	(0.3660)	(0.0542)	(0.3661)
Obs.	27771	18999	27771	18999	27771	27771

(1a)-(2a) Standard errors robust to heteroscedasticity
(3a)-(4a) Standard errors clustered DP and year
(5a)-(6a) Standard errors clustered DP and year bootstrap (1000 replications)
** p<0.10, ** p<0.05, *** p<0.01*
Standard errors in parentheses
Notes: Other variables included in the regression are gender, age, residence, high school type and score, family income, whether the student enrolled immediately after high school and student/teacher ratio. Coefficients of control variables are displayed in the appendix.

Table 3 - Difference-in-differences estimates – Effects of introduction of a test based restricted access policy on student's performances, treatment and control group are the top 65% at the test score control

	(1b) dropout between the I and II year	*(2b) GPA*	*(3b) dropout between the I and II year*	*(4b) GPA*	*(5b) dropout between the I and II year*	*(6b) GPA*
Pre-post	0.1722***	-0.1164**	0.1722***	-0.1164	0.1722***	-0.1164
	(0.0089)	(0.0593)	(0.0470)	(0.2586)	(0.0500)	(0.2578)
Treat	-0.0423***	-2.5902***	-0.0423	-2.5902***	-0.0423	-2.5902***
	(0.0093)	(0.0626)	(0.0298)	(0.2702)	(0.0326)	(0.2836)
Treat*pre-post	-0.1246***	0.6853***	-0.1246**	0.6853*	-0.1246**	0.6853*
	(0.0195)	(0.1227)	(0.0513)	(0.3813)	(0.0547)	(0.3849)
Obs.	26135	18009	26135	18009	26135	18009

(1b)-(2b) Standard errors robust to heteroscedasticity
(3b)-(4b) Standard errors clustered DP and year
(5b)-(6b) Standard errors clustered DP and year bootstrap (1000 replications)
** p<0.10, ** p<0.05, *** p<0.01*
Standard errors in parentheses
Notes: Other variables included in the regression are gender, age, residence, high school type and score, family income, whether the student enrolled immediately after high school and student/teacher ratio. Coefficients of control variables are displayed in the appendix.

Table 4 - Difference-in-difference estimates – Placebo Regressions for the effects of the introduction of a test based access policy on student's performances, treated group: cohort 2009-10, control groups previous years

	(1) dropout between the I and II year	(2) GPA
Pre-post full sample	-0.0362	-0.2883
	(0.0282)	(0.4345)
N. obs.	23576	16052
Pre-post top 65% test score	-0.0539	-0.1317
	(0.0328)	(0.4516)
N. obs.	21940	15062

** p<0.10, ** p<0.05, *** p<0.01*
Note: Standard errors in parentheses.Other variables included in the regression are gender, age, residence, high school type and score, family income, whether the student enrolled immediately after high school and student/teacher ratio.

Figure 1 – Distribution of high school grades obtained by the applicants (Faculty of Economics) in 2010-2011 (year of implementation of the policy, dashed line) and in 2005-2006, 2006-2007, 2007-2008, 2008-2009 and 2009-2010 (before the implementation of the policy, solid line)

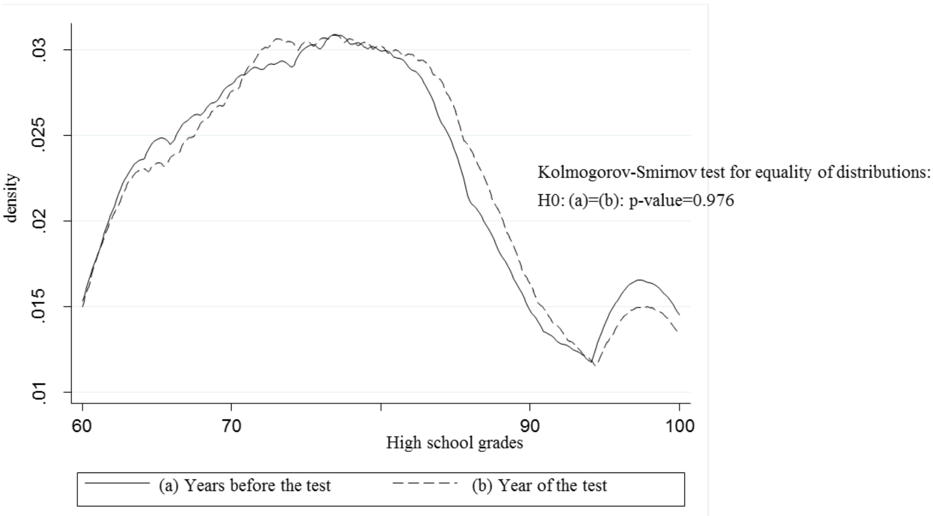
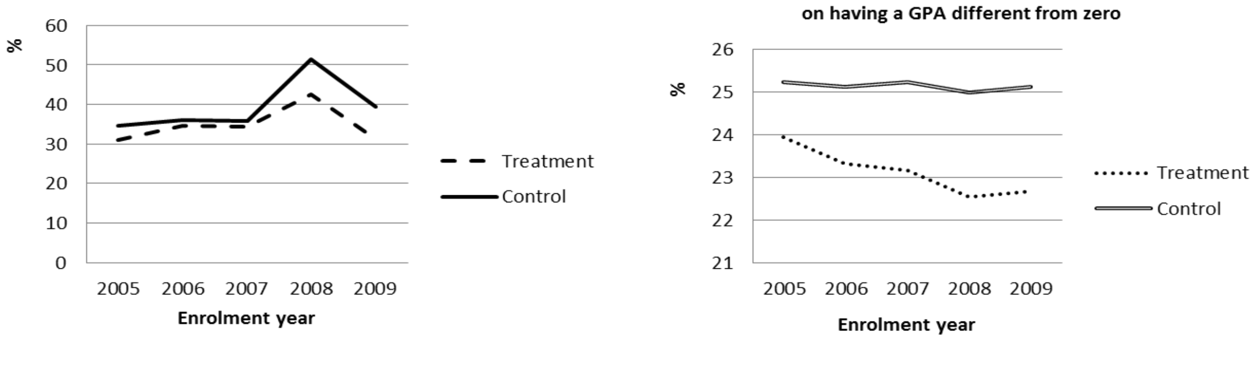


Figure 2 – Trend in educational performances. Dotted line refers to the treatment group and solid line to the control group
Treatment group consists of the students enrolled in the Faculty of Economics
Control group consists of the students enrolled in the Faculties of Engineering, Art and Philosophy, Languages, Educational Science, Math, Physics and Natural Science and Political Science



APPENDIX

Table 1A – Definition of variables and sample means (Standard errors in parentheses)

Variable name	*Variable definition*	*Treatment*	*Control*	*Difference*
Outcome variables				
Dropout	1 if drops out at the end of the 1st year; 0 otherwise	0.3517 (0.0057)	0.4228 (0.0034)	0.0710 (0.0068)
GPA	Grade point average at the end of the 1st year	23.1254 (0.0396)	25.1012 (0.0244)	1.9758 (0.0476)
Individual characteristics				
Males	1 if male; 0 otherwise	0.5381 (0.0060)	0.4638 (0.0034)	-0.0743 (0.0069)
Age	Age in years at the beginning of the enrolment year	20.2441 (0.0409)	21.0472 (0.0351)	0.8031 (0.0654)
Age^2	Age in years at the beginning of the enrolment year squared	421.4117 (2.2052)	468.7030 (2.2052)	47.2913 (4.0737)
KM	Residence distance from the University campus	36.3251 (0.5119)	41.5759 (0.3871)	5.2508 (0.7347)
KM^2	Residence distance from the University campus squared	3128.609 (266.925)	4856.036 (247.0442)	1727.427 (456.1465)
Type of high school				
Scientlyc	1 if attended Scientific lyceum; 0 otherwise	0.3783 (0.0058)	0.3622 (0.0033)	-0.0161 (0.0066)
Classlyc	1 if attended Classic lyceum; 0 otherwise	0.0683 (0.0030)	0.1002 (0.0020)	0.0319 (0.0040)
Linglyc	1 if attended Linguistic lyceum; 0 otherwise	0.0210 (0.0017)	0.0633 (0.0016)	0.0422 (0.0030)
Techninst	1 if attended Technical Institution; 0 otherwise	0.3970 (0.0058)	0.2544 (0.0030)	-0.1426 (0.0062)
Profinst	1 if attended Professional Institution; 0 otherwise	0.1066 (0.0037)	0.0751 (0.0018)	-0.0314 ()0.0038
Otherinst	1 attended Other Institutions; 0 otherwise	0.0285 (0.0020)	0.1446 (0.0024)	0.1160 (0.0043)
Diploma score				
Score	High school final exam score	79.2111 (0.1490)	78.8165 (0.0857)	-0.3946 (0.1719)
Family income				
Lowincome	1 if declared family income from € 0 to € 12,000.00; 0 otherwise	0.4741 (0.0060)	0.5001 (0.0034)	0.0260 (0.0069)
Mediumincome	1 if declared family income from € 12,000.01 to € 32,000.00; 0 otherwise	0.3653 (0.0057)	0.3657 (0.0033)	0.0004 (0.0066)
Highincome	1 if declared family income higher than € 32,000.01; 0 otherwise	0.1605 (0.0044)	0.1340 (0.0023)	-0.0264 (0.0048)
Other				
Gaptime	1 if enrolled in the year of the diploma; 0 otherwise	0.8730 (0.0040)	0.8118 (0.0027)	-0.0612 (0.0052)
Student/teacher ratio	Proportion of students enrolled out of the total number of academic staff at faculty level	10.8471 (0.0142)	7.1613 (0.0218)	-3.6857 (0.0388)

Table 2A - Difference-in-differences estimates – Effects of introduction of a test based restricted access policy on student's performances

	(1a)	*(2a)*	*(3a)*	*(4a)*	*(5a)*	*(6a)*
	dropout between the I and II year	*GPA*	*dropout between the I and II year*	*GPA*	*dropout between the I and II year*	*GPA*
Pre-post	0.1721***	-0.1173**	0.1721***	-0.1173	0.1721***	-0.1173
	(0.0089)	(0.0593)	(0.0470)	(0.2598)	(0.0500)	(0.2593)
Treat	-0.0264***	-2.6985***	-0.0264	-2.6985***	-0.0264	-2.6985***
	(0.0086)	(0.0580)	(0.0266)	(0.2470)	(0.0298)	(0.2572)
T*pre-post	-0.1399***	0.7860***	-0.1399***	0.7860**	-0.1399**	0.7860**
	(0.0192)	(0.1206)	(0.0495)	(0.3660)	(0.0542)	(0.3661)
Individual characteristics						
Males	0.0679***	-0.3462***	0.0679***	-0.3462***	0.0679***	-0.3462***
	(0.0062)	(0.0438)	(0.0104)	(0.0757)	(0.0098)	(0.0710)
Age	0.0826***	0.0791**	0.0826***	0.0791	0.0826***	0.0791
	(0.0047)	(0.0333)	(0.0063)	(0.0528)	(0.0063)	(0.0602)
Age^2	-0.0010***	-0.0001	-0.0010***	-0.0001	-0.0010***	-0.0001
	(0.00007)	(0.0005)	(0.0001)	(0.0008)	(0.0001)	(0.0009)
Km	-0.0001*	-0.0013**	-0.0001*	-0.0013*	-0.0001*	-0.0013**
	(0.00009)	(0.0006)	(0.00009)	(0.0007)	(0.0001)	(0.0006)
Km^2	3.94e-07**	2.16e-06**	3.94e-07***	2.16e-06	3.94e-07***	2-16e06*
	(1.55e-07)	(1.07e-06)	(1.44e-07)	(1.32-06)	(1.27e-07)	(1.18e-06)
Type of high school – Reference: Scientific Lyceum						
Classlyc	-0.0290***	0.9273***	-0.0290**	0.9273***	-0.0290***	0.9273***
	(0.0095)	(0.0656)	(0.0116)	(0.1159)	(0.0095)	(0.1157)
Linglyc	0.0333***	0.2637***	0.0333	0.2637**	0.0333	0.2637**
	(0.0126)	(0.0787)	(0.0223)	(0.1187)	(0.0224)	(0.1152)
Techinst	0.1206***	-0.6912***	0.1206***	-0.6912***	0.1206***	-0.6912***
	(0.0070)	(0.0498)	(0.0101)	(0.0741)	(0.0099)	(0.0724)
Profinst	0.1767***	-1.0003***	0.1767***	-1.0003***	0.1767***	-1.0003***
	(0.0111)	(0.0841)	(0.0115)	(0.1059)	(0.0099)	(0.1040)
Otherinst	0.0800***	-0.2687***	0.0800***	-0.2687***	0.0800***	-0.2687***
	(0.0105)	(0.0691)	(0.0146)	(0.0804)	(0.0137)	(0.0893)
Diploma score						
Score	-0.0082***	0.0864***	-0.0082***	0.0864***	-0.0082***	0.0864***
	(0.0002)	(0.0015)	(0.0003)	(0.0025)	(0.0003)	(0.0024)
Family income – reference: Lowincome						
Mediumincome	-0.0343***	-0.0505	-0.0343***	-0.0505	-0.0343***	-0.0505
	(0.0060)	(0.0411)	(0.0057)	(0.0387)	(0.0051)	(0.0402)
Highincome	-0.0002	-0.1225**	-0.0002	-0.1225**	-0.0002	-0.1225**
	(0.0083)	(0.0591)	(0.0084)	(0.0563)	(0.0085)	(0.0569)
Other						
Gaptime	-0.0218**	-0.2146***	-0.0218**	-0.2146**	-0.0218**	-0.2146**
	(0.0087)	(0.0650)	(0.0107)	(0.0875)	(0.0107)	(0.0865)
STR	-0.0051***	0.2234***	-0.0051	0.2234***	-0.0051	0.2234***
	(0.0011)	(0.0079)	(0.0043)	(0.0319)	(0.0049)	(0.0365)
Obs.	27771	18999	27771	18999	27771	18999

(1a)-(2a) Standard errors robust to heteroscedasticity
(3a)-(4a) Standard errors clustered DP and year
(5a)-(6a) Standard errors clustered DP and year bootstrap (1000 replications)
** p<0.10, ** p<0.05, *** p<0.01*
Standard errors in parentheses

Table 3A - Difference-in-differences estimates – Effects of introduction of a test based access policy on student's performances, treatment and control group are the top 65% at the test score control

	(1b)	(2b)	(3b)	(4b)	(5b)	(6b)
	dropout between the I and II year	*GPA*	*dropout between the I and II year*	*GPA*	*dropout between the I and II year*	*GPA*
Pre-post	0.1722***	-0.1164**	0.1722***	-0.1164	0.1722***	-0.1164
	(0.0089)	(0.0593)	(0.0470)	(0.2586)	(0.0500)	(0.2578)
Treat	-0.0423***	-2.5902***	-0.0423	-2.5902***	-0.0423	-2.5902***
	(0.0093)	(0.0626)	(0.0298)	(0.2702)	(0.0326)	(0.2836)
T*pre-post	-0.1246***	0.6853***	-0.1246**	0.6853*	-0.1246**	0.6853*
	(0.0195)	(0.1227)	(0.0513)	(0.3813)	(0.0547)	(0.3849)
Individual characteristics						
Males	0.0656***	-0.3695***	0.0656***	-0.3695***	0.0656***	-0.3695***
	(0.0064)	(0.0455)	(0.0111)	(0.0790)	(0.0109)	(0.0754)
Age	0.0830***	0.0728**	0.0830***	0.0728	0.0830***	0.0728
	(0.0048)	(0.0336)	(0.0064)	(0.0517)	(0.0063)	(0.0585)
Age^2	-0.0010***	-0.00006	-0.0010***	-0.00006	-0.0010***	-0.00006
	(0.00007)	(0.0005)	(0.0001)	(0.0007)	(0.0001)	(0.0008)
Km	-0.0001	-0.0015**	-0.0001	-0.0015**	-0.0001	-0.0015**
	(0.00009)	(0.0006)	(0.00009)	(0.0007)	(0.0001)	(0.0006)
Km^2	3.63e-07**	2.50e-06**	3.63e-07**	2.50e-06**	3.63e-07***	2.50e-06**
	(1.57e-07)	(1.09e-06)	(1.46e-07)	(1.28e-06)	(1.28e-07)	(1.13e-06)
Type of high school – Reference: Scientific Lyceum						
Classlyc	-0.0304***	0.9352***	-0.0304**	0.9352***	-0.0304***	0.9352***
	(0.0096)	(0.0665)	(0.0120)	(0.1174)	(0.0102)	(0.1176)
Linglyc	0.0280**	0.2852***	0.0280	0.2852**	0.0280	0.2852**
	(0.0128)	(0.0795)	(0.0229)	(0.1195)	(0.0235)	(0.1136)
Techinst	0.1180***	-0.6779***	0.1180***	-0.6779***	0.1180***	-0.6779***
	(0.0072)	(0.0517)	(0.0107)	(0.0764)	(0.0102)	(0.0761)
Profinst	0.1695***	-0.9779***	0.1695***	-0.9779***	0.1695***	-0.9779***
	(0.0118)	(0.0892)	(0.0129)	(0.1102)	(0.0113)	(0.1083)
Otherinst	0.0786***	-0.2387***	0.0786***	-0.2387**	0.0786***	-0.2387***
	(0.0107)	(0.0702)	(0.0151)	(0.0835)	(0.0144)	(0.0919)
Diploma score						
Score	-0.0080***	0.0861***	-0.0080***	0.0861***	-0.0080***	0.0861***
	(0.0002)	(0.0016)	(0.0003)	(0.0026)	(0.0003)	(0.0025)
Family income – reference: Lowincome						
Mediumincome	-0.0346***	-0.0524	-0.0346***	-0.0524	-0.0346***	-0.0524
	(0.0062)	(0.0423)	(0.0055)	(0.0401)	(0.0051)	(0.0422)
Highincome	0.0004	-0.1203**	0.0004	-0.1203**	0.0004	-0.1203**
	(0.0085)	(0.0607)	(0.0082)	(0.0592)	(0.0083)	(0.0589)
Other						
Gaptime	-0.0148*	-0.2106***	-0.0148	-0.2106**	-0.0148	-0.2106**
	(0.0089)	(0.0661)	(0.0109)	(0.0869)	(0.0106)	(0.0861)
STR	-0.0049***	0.2203***	-0.0049	0.2203***	-0.0049	0.2203***
	(0.0012)	(0.0079)	(0.0043)	(0.0319)	(0.0049)	(0.0365)
Obs.	26135	18009	26135	18009	26135	18009

(1b)-(2b) Standard errors robust to heteroscedasticity
(3b)-(4b) Standard errors clustered DP and year
(5b)-(6b) Standard errors clustered DP and year bootstrap (1000 replications)
** p<0.10, ** p<0.05, *** p<0.01*
Standard errors in parentheses