Essays on the Economics of Human Capital

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ABSTRACT

SOFOKLIS GOULAS: Essays on the Economics of Human Capital.
(Under the direction of Luca Flabbi)

In the first paper we use a natural experiment that relaxed class attendance requirements for one school year to explore students' marginal propensity to skip class, and to examine the effects of their absences on scholastic outcomes. We exploit exogenous variation resulting from a one-time policy Greece implemented allowing high school students to miss 30 percent more class hours without penalty during the 2009-10 academic year, a period when officials feared outbreaks of swine flu. Using a new dataset, we analyze which students missed more classes, and the effect of these absences on scholastic outcomes across the distribution of student ability, income, and peer quality. We find that while the swine flu itself did not affect the student population, the relaxed class attendance policy caused an increase in absences of roughly 10 hours per student, with more absences taken by those who had higher academic performance records, have academically weaker peers in their classes, or who live in poorer neighborhoods. End-of-year exam results show a positive effect of absences on grades across the ability distribution. The magnitude of the positive effect of absences on grades increases as we move to right of the ability distribution. Our results suggest that students who may have the resources or the human capital accumulation to learn outside the classroom may have lower performance when a strict attendance policy forces them to stay in class.

In the second chapter we study the effect of disclosing relative performance information (feedback) on students' performance in high-school, on subsequent university enrollment, and on expected subsequent earnings. We exploit a large-scale natural experiment in which students in some cohorts receive information about their relative performance within their schools and across the nation. Using unique primary data, we find an asymmetric response to feedback: high-achieving

students improve their final-year performance by 0.15 of a standard deviation, whereas the final-year performance of low-achieving students drops by 0.3 of a standard deviation. The results are more pronounced for females, indicating greater sensitivity to feedback. We also document the long-term effects of feedback: high-achieving students reduce their repetition rate for the national exams; they enroll into university departments that are more selective by 0.15 of a standard deviation and their expected annual earnings increase by 0.17 of a standard deviation. By contrast, the results for low-achieving students are negative. We provide suggestive evidence that feedback encourages students from low-income neighborhoods to enroll in university and to study in higher-quality programs, which may, in the long run, reduce income inequality.

In the third chapter we examine the extent to which college decisions among adolescents depend on the decisions of their peers. In particular, we ask whether individuals derive utility from conformity in college enrollment and academic mobility. We propose a new methodology in mitigating reflection and endogeneity issues in identifying social interactions. We use the proportion of females in a student's last year's reference group (school and neighborhood) as an instrumental variable. We investigate utility spillovers from the educational choices of students in consecutive cohorts. Spatial variation allows us to identify social interactions in groups of various sizes. We use a new data set that spans the universe of high school graduates. We find positive and significant externalities in the decision to enroll in college and the decision to migrate to a different city among peers that belong to the same social group. Results indicate that students who are in a school or neighborhood with 100% more peers who enrolled in college last year are 29% or 9.6% more likely to themselves attend college.

In the fourth chapter we consider how economic recessions alter the costs and expected returns of attaining college education in general and pursuing a specific college major. We examine how changes in the unemployment rate affect demand for college education, demand for different fields of university study and degrees' admission thresholds. We use panel data for applications submitted to the universe of undergraduate programs in Greece that span seven rounds of admission cohorts combined with a degree-specific job insecurity index, and time series on youth (ages 18-25) unemployment. We find that degree- and major-specific job insecurity turns applicants away

from degrees and majors that are associated with poor employment prospects. Results indicate that the steep increase in the unemployment rate that started in 2009 is associated with an increase in the number of college applicants. The effect is heterogeneous across fields, with an increase in the demand for degrees in Psychology as well as for entrance to Naval, Police and Military Academies, and a decrease in the demand for degrees in Business and Management. We also find that the business cycle changes degrees' admission thresholds by affecting their popularity.

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CHAPTER 1

THE EFFECT OF COMPULSORY CLASS ATTENDANCE ON ACADEMIC PERFORMANCE

1.1 Introduction

Most educational systems rely on lectures and class meetings as a means of instruction. This is even more prevalent when secondary or pre-tertiary education is considered. Lecture learning is based on group learning, which may not be the optimal learning style for everyone. In a classroom, students compete for the attention and time of the instructor. Thus, their consumption of education induces externalities on one another. As a result, many students decide to skip class when given the opportunity. Therefore, if the optimal level of class attendance is below perfect, is compulsory class attendance beneficial for every student?

In this paper, we investigate the causal relationship between compulsory class attendance and exam performance. In our setting, high school students in Greece are allowed to be absent from school up to a certain number of school periods without penalty. Exceeding the upper limit is punished by grade retention. In 2009, the Hellenic Ministry of Education received information about an increasing number of Swine Flu cases around Europe. Though high-school students in Greece did not constitute a population at risk, the ministry nevertheless decided to take action. This action went into effect several months later, when the European swine flu pandemic was over. The unique, one-time-only reform increased the number of periods a student could miss without penalty. The ministry specified that students did not need to provide a doctor's note, or to seek their parents' approval to take up the extra absences. Our paper exploits variation from this natural experiment that increased the absence allowance of high school students by 34 school periods or 30 percent. The treatment offers exogenous variation by relaxing the time budget constraint of students, who maximize utility by allocating time between school and leisure. Using an Instrumental Variable approach, we identify the causal effect of class attendance on exam performance. We control for individual-specific heterogeneity by using longitudinal data on the exam performance of students in consecutive grades. The Swine Flu Reform allows us to mitigate endogeneity stemming from unobserved common shocks that vary across grades.

Students may miss class both when they are sick and when they are not sick. In the latter case, student may miss class to enjoy leisure or study. Absence due to sickness may decrease performance. Absence for reasons other than sickness may have a non negative effect on performance depending on how the time outside the classroom is spent. We exploit a natural experiment that allowed students to miss more classes without actually being sick. The effect of absences on performance estimated comes from absences that are not related to sickness.

We divide students into three groups. The first group includes students who never skip class, and attend no matter whether they have the option to skip or not. The second group of students skips class regularly. Those students choose to skip regardless of whether the class attendance policy is strict or loose. The third group of students chooses to attend class only when the class attendance policy is strict. Students who have the resources or enough human capital accumulation to sufficiently substitute class work with out-of-class individual work or tutoring belong in the third group. Our instrumental variables approach estimates the effect of absences on exam performance that is due to a relaxed class attendance policy. Returns to absences are identified for students who can replicate class activities outside the classroom that is, the group of students who are most likely to exploit the relaxed class attendance policy.

The relaxed attendance policy makes some students to skip class. Our findings show that for students whose class activities are not too costly to replicate outside the classroom, school absences increase exam performance. We can view this the other way around: A strict class attendance would reduce school absences for those students who would skip class if given lax attendance rules, could cause exam performance to decrease. A decrease in exam performance due to a strict attendance policy could stem from two sources. First, class learning could be sub-optimal for some students who have the resources to acquire human capital through other pedagogical methods. Intuitively, the larger the class, the less efficient class learning becomes because the instructor's time is divided among more learners. Second, class peer effects could affect individual exam performance. We do not know a priori whether students capable of learning outside the classroom receive positive or negative class peer effects when they must adhere to a strict attendance policy. Be that as it may, we know that students are assigned to classes using alphabetical order based on their last names. Lexicographical class assignment allows for heterogeneous environments where students from different points of the ability distribution interact with each other. Such environments may be conducive to disruptive behavior or acting up that leads the learning experience to deteriorate. Thus, while estimating returns to absences due to a relaxed class attendance policy we are measuring the net externality the rest of the class imposes on students capable of learning outside the classroom.

The contribution of our paper is threefold. First, we exploit variation stemming from a natural experiment to mitigate two sources of endogeneity: time-invariant, individual- specific, unobserved heterogeneity, such as parental supervision or personality traits; and grade-varying, common shocks such as teacher or student-age effect. Second, we use new, hand-collected transcript data from a random sample of 114 schools in Greece in order to provide evidence relevant across the ability distribution. Thus, we can answer the question: Who benefits from the school more, the good or the bad students? The natural experiment utilized in our paper didn't force students into taking action but merely relaxed one constraint of their utility- maximization problem. In other words, the element of choice remains in both the strict and the relaxed class attendance policy regimes. Thus the third novelty of our paper is that we are able to estimate the differential response to the relaxed constraint as well as differential returns to absences that may be induced by student or school characteristics. Moreover, the natural experiment increased the upper limit of unexcused absences. Unexcused absences can be absences in the middle of the day, and they do not require a doctor's note or a parent's approval. That means students choose which period and subject to skip. Therefore, the students may choose to skip English but attend mathematics in the same day. The estimates of returns to absence identified by this unique reform pertain to the situation where the students strategically choose which school periods to attend and to miss.

Moreover, we delve into the reasons students choose to be absent from school. Because the exogenous policy change that we exploit for identification did not force students to skip class, and because not every student took advantage of the new policy to the same extent, we are able to explore the heterogeneous propensity to skip class across different observable student, school, and class characteristics.

The rest of the paper is organized as follows: Section 2 places in the literature, Section 3 provides a motivation in the context of economic theory, Section 4 provides a description of the institutional setting, Section 5 describes our data collection, Section 6 summarizes the data used, Section 7 describes the empirical strategy, and section 8 discusses our results. Section 9 contains robustness check for our results, and Section 10 concludes.

1.2 Our Place in The Literature

Educational interventions can be classified as taking one of two forms: those that improve the quality of the inputs of the production function and those that increase their quantity. Much of the work to date has focused on estimating the effects of interventions that targeted the quality of educational inputs, such as teacher quality, class size or classroom environment (Hanushek, Mayer, and Peterson 1999, Rivkin, Hanushek, and Kain 2005b, Hanushek 2003, Angrist and Lavy 1997, Krueger 2003) or measuring the effects of all school production factors on both the quantity and quality margins (Card and Krueger 1990, Loeb and Bound 1995, Betts 1995). Nevertheless, investment in resources are likely to happen concurrently. Historically, school districts with the longest term lengths were those with the highest paid teachers, making it difficult to disentangle the effects of interventions in quality from those in quantity. This work joins a strand of the literature focused on determining the returns to increasing the quantity of inputs, namely time in school, separately from changing their quality (Pischke 2007, Hansen 2011, Marcotte and Hansen 2010, Leuven, Lindahl, Oosterbeek, and Webbink 2010, Sims 2008)

The literature regarding class absenteeism is divided into two main categories: one refers to the reasons for students being absent from class (Levine 1992, Chong, Cheung, and Hui 2009) and the second one is concerned with the effect of students' absenteeism on their scholastic outcomes (Romer 1993, Caviglia 2006, Chen and Lin 2008, Arulampalam, Naylor, and Smith (2012), Latif and Miles (2013)). Most of these papers use college and field-specific class attendance data. In particular, most of these papers use data regarding Economics, Accounting or Management students. The majority of these papers find a negative or negligible relationship between students' absenteeism and academic performance, or, in one case, a negligible relationship (Caviglia 2006). Evidence from the existing literature suggests that class attendance improves educational outcomes. Romer (1993) claims that college students in three elite U.S. universities were found to perform better when attending classes and completing homework. Nevertheless, this claim may apply for only a small part of the right tail of the ability distribution in a given population. Chen and Lin (2006) using a sample of 129 college students in Taiwan find a 4 percent exam score improvement associated with higher class attendance. A subsequent study by the same authors Chen and Lin (2008) involved an experiment where different sections of the same college course were subject to random changes in the curriculum although everyone took the same exam at the end of the semester. The authors found that having the instructor cover all of the material improved score by as much as 18 percent. Latif and Miles (2013) used panel data of exam scores of Canadian college students to measure the effect of class attendance on exam performance. They find that when controlling for student heterogeneity, exam performance is positively related to class attendance. Similar results have been obtained when college classes on science (Moore 2006) or economics (Cohn and Johnson 2006) are considered. Arulampalam et al. (2012) use panel data to identify the causal relationship between class attendance and students' university performance. Focusing on economics students, they use quantile regression analysis and find that skipping classes leads to poorer performance. Interestingly, they highlight that the relationship between class attendance and students' performance may vary with student ability. Caviglia (2006) examines the impact of mandatory attendance of microeconomic classes on students' college performance. After accounting for students' motivation, he finds that class attendance did not impact grades. This is the only paper that finds a negligible effect between class attendance and students' academic outcomes. Despite the rich literature that involves college data, there is little evidence that the same results hold in a less-filtered context, such as high schools. Fitzpatrick, Grissmer, and Hastedt (2011) use quasi randomness in the timing of kindergarten assessment to examine the effect of time spent in school on student achievement. Their estimates suggest that a year of schooling increases math and reading test scores by about one standard deviation above normal developmental gains. Aucejo and Romano (2016) exploit a North Carolina state policy that resulted in variation in the length of the school year to jointly es- timate the effect of high school attendance and the length of the school calendar on performance while controlling for student and teacher characteristics. They also use local flu prevalence data to instru- ment for absences. They find that 10 days of school absence reduce math scores by 5.5 percent and reading scores by 2.9 percent. In the context of Aucejo and Romano (2016), students skip class because of the flu, and, thus, they have no choice over which periods to skip.

1.3 Theoretical Motivation

Following Arulampalam et al. (2012) we build a theoretical model to motivate both our hypotheses and our empirical strategy. Suppose the representative student faces the following additively separable utility function:

$$U = u(s(c, h, a), l) \tag{1.1}$$

which is a function of leisure, l and the following educational production function

$$s = s(c, h, a) \tag{1.2}$$

where s is a measure of a student's educational performance, c is the amount of time the student spends in class, h is the amount of time the student spends in out-of-class learning activities, a captures individual characteristics such as ability, motivation, and effort. Suppose for simplicity that the marginal utility of s is one and the marginal utility of leisure is constant for every unit of time outside the classroom[1].

The objective of the student is to maximize utility from performance and leisure given their

[1] The assumption of constant marginal utility of leisure is not crucial. Here is an example where we relax this assumption. Consider the following production function: $s = s(c, h, a)$. Suppose the utility function takes the following form: $U = u(s, l) = s(c, h, a) + \gamma\sqrt{l} = \alpha\sqrt{c} + \beta\sqrt{h} + \gamma\sqrt{l}$. Maximizing utility under the time constraint gives the following optimal time allocation: $\{c^*, h^*, l^*\} = \{\frac{\alpha^2}{\alpha^2+\beta^2+\gamma^2}, \frac{\beta^2}{\alpha^2+\beta^2+\gamma^2}, \frac{\gamma^2}{\alpha^2+\beta^2+\gamma^2}\}$

time constraint, which takes the following form:

$$c + h + l \leq T \tag{1.3}$$

where T is the maximum amount of available time in a given period. Assume initially that c and h in the production function are neither complements nor substitutes but independent. The student maximizes their utility by allocating their time efficiently between leisure, in class study, and out of class study. We assume there is no coordination among student in the decision of time allocation. Thus any peer effects are random and not the result of a collective behavior.

In reality, marginal products are likely to vary from person to person. A student faces the challenge of knowing whether in or out of class learning works best for them. In other words, students are supposed to know their relative marginal productivities of the inputs in their educational production function.

Assume that students have accurate information regarding the parameters in their own production function. This assumption may hold less for students in elementary school and more for high school students as the latter have had more learning experience. Assume also that the marginal products of study time in class and out of class are positive but exhibit diminishing returns and are independent of each other and of ability: $\frac{\partial s}{\partial c} := mpc > 0$, $\frac{\partial s}{\partial h} := mph > 0$, $\frac{\partial^2 s}{\partial c^2} < 0$, $\frac{\partial^2 s}{\partial h^2} < 0$, $\frac{\partial^2 s}{\partial c \partial h} = 0$, $\frac{\partial^2 s}{\partial c \partial a} = 0$, and $\frac{\partial^2 s}{\partial c \partial a} = 0$. We also assume $\frac{\partial s}{\partial a} > 0$. We will relax some of these assumptions later. Under these assumptions we can represent diagrammatically the solution to the problem of the utility-maximizing student.

In Figure 1.1a, we see that the utility-maximizing student will optimise at point A, choosing to attend c^* hours of class and engaging in $T - c^*$ hours of out of class study. Whether this involves absences from class will depend on the number of scheduled classes available to the student. In that sense, the school imposes a constraint on time. If there are significant external net benefits of attending class, then the number of classes supplied to the student, denoted by t_{cs}, is more likely to exceed the student's optimal number, thus $t_{cs} > c^*$, as in Figure 1.1a. If now $t_{cs} < c^*$ as in Figure 1.1c, then the outcome will be inefficient as $mpc > mph$.

In the case described in Figure 1.1a, the optimising student will choose to miss $t_{cs} - c^*$ hours of class. At the margin, if students were required to attend t_{cs} hours of class, their academic performance would decrease as $mph > mpc$ for the marginal classes. Suppose that class attendance is compulsory but that absence is not penalized. Then the propensity of students to miss at least some fraction of the suboptimal $t_{cs} - c^*$ classes will depend on their attitudes to compliance, which we suppose it's randomly distributed across the student population, but may change with age. Then, under the assumptions of the model, class absences in the range $t_{cs} - c^*$ will be associated with higher performance. This may seem as a depart from the standard hypothesis in the literature that more absences decrease performance, but our prediction emerges from an optimizing setting in which choices are made under perfect information. At the margin, class attendance improves performance, but only up to an optimizing point.

Suppose now that we relax our assumption that factor inputs are independent and allow the marginal product of attending class to be positively correlated with ability, $ceteris\,paribus$: $\frac{\partial^2 s}{\partial c \partial a} > 0$. This case is represented in Figure 1.1c, where the mpc for more able students, mpc_2, lies above that of the less able, mpc_1. We see that utility-optimizing makes the more able students to skip fewer classes in comparison to less able ones: $c_2^* > c_1^*$ in Figure 1.1c. In a framework where class attendance is optional, performance will be greater for the more able students and, thus, will be negatively associated with class absence.

Moreover, mph can also be positively correlated with characteristics captured in a. For example, students from economically more advantaged backgrounds may have better to access to private tutoring, books, faster internet and other resources, which thereby result in a higher mph.In that case, whether c_2^* will be less or greater than c_1^* will depend on comparative advantage, that is the relative correlation of a with mpc and with mph.

In an econometric estimation of the effects of absence on performance, correlation between a and either c or h in the education production function given by equation 1.2 could potentially generate endogeneity bias if a is not perfectly observed. If more able students are less likely to be absent from class $c_2^* > c_1^*$, as in Figure 1.1c then the estimated adverse effect of absence on performance will be biased upwards, in absolute terms, through endogenous selection and the resulting ability bias. The empirical investigation of the effects of absence from class on performance should be constructed so as to allow for heterogeneous effects of this sort. This observation lies behind the design of our later estimation strategy. In the case in which $c_1^* > c_2^*$, then the direction of endogeneity bias will be downward but, again, the effects will be heterogeneous.

As we have seen, ability differences across students can affect absences from class through their influence on the educational production function, equation 1.2. But suppose now that there are differences across students in marginal utility of leisure, mul . In Figure 1.1d, we consider the effects of an exogenous increase in the marginal utility of leisure. In this case, there will be an increase in the number of classes missed along with an associated drop in performance. In the model, the marginal utility of leisure, mul , is taken as exogenous. In reality, mul is likely to be influenced by various arguments. For example, individual, family or even city characteristics may account for differences in marginal utility of leisure. The marginal utility of leisure may also be related to student ability, and hence to a in equation 1.2. If, for instance, more able students undertake more non-curricular activities, then mul will be positively correlated with ability. In this case, more able students will be more likely to miss class. Note also from Figure 1.1d that the effect of missing class will be greater for more able students as $mpc_2^* > mpc_1^*$. Again, unobserved differences in ability across students will create a bias in the estimated effect of absence on performance as part of the association between absence and performance is being explained by a differential propensity of the more able to be absent from class.

In summary, we have seen that, in an optimizing framework, the theoretical effect of absence on performance is ambiguous. If class attendance is compulsory and students differ only in a randomly-distributed propensity toward compliance, then absence will have a positive association with performance as the less compliant will be more likely to adhere to the optimal number of classes. If, on the other hand, students are heterogeneous in ability then they will be likely to choose different optimal levels of class attendance: if ability is associated with a comparative advantage in class attendance as in Figure 1.1c then the more able will have a higher attendance rate and absence will be associated with poorer educational performance. Ability might also be correlated with the marginal utility of leisure: if more able students have a higher opportunity cost of studying, then it is likely that they will attend fewer classes. In this case, class absence will be likely to have a positive association with performance. Estimation of the effects of absence on performance will be biased if ability is not observed or accurately proxied and the direction of bias will depend on the relative dominance of factors of the type we have identified. The model predicts that the magnitude of any effects of absence on performance will vary with student ability: if, for example, ability is relatively highly correlated with productivity of class attendance then the negative effect of absence on performance will be greatest for the more able students. Nevertheless, out-of-class productivity is also positively associated with more classed missed. Ability may also be correlated with out-of-class productivity in the sense that more able students are better at learning on their own. If out-of-class-productivity exceeds in-class productivity, i.e. if there is a comparative advantage in out-of-class learning, lower class attendance may be associated with higher performance. These considerations inform our choice of empirical estimation strategy.

The model we have outlined so far assumes that students have sufficient information to be able to select their optimal level of class attendance. In reality, this is unlikely and students will make mistakes, attending either more or fewer classes than would be privately efficient. If students systematically under-estimate the marginal product of class attendance, then absence will tend to have an adverse effect on performance. This tendency might also be correlated with ability, so that less able students miss more classes and suffer a further reduced level of performance.

Informed by this theoretical motivation on the optimizing behavior of individuals, our empirical strategy will involve: first, an analysis of the effects of a relaxed class attendance policy on absences, second, an attempt to identify causal effects of absence on student performance using the exogenous variation from the relaxed attendance policy, third, an investigation of whether and how any effects of the relaxed attendance policy on absences and school performance vary systematically with student, class, and school characteristics.

1.4 Institutional Setting

1.4.1 Background

It is useful to provide some background on the design of the institutional setting in which our natural experiment takes place. Public high schools are the norm in Greece; only around 8 percent of students attend private high school[2]. Assignment to high school schools is based on geographical proximity, namely a school district system. Every high school offers the same curriculum, and funding is a linear function of the number of students. Teachers' quality characteristics, such as education and experience, are not taken into account for the allocation of teachers to schools. By law, students' assignment to classrooms is based on alphabetical order.

Students in the Greek educational system are allowed to be absent from class for a limited number of hours in a given school year. Class absence can be either excused or unexcused. Total absences are the sum of excused and unexcused hours of class absence. Excused absences are whole day absences that the student provided for a doctor's note or a custodian -usually a parent came to school to sign off their child's absence. Class absence for less than a whole day cannot be excused and therefore count towards a student's unexcused absences. For example, if a student goes to school late in the morning or if she decides to skip school midday, these absences cannot be excused. Whole day absence from school that is not excused counts towards a student's unexcused absences. Under the current class attendance regulation, every student could have 50 hours of unexcused and 64 hours of excused absence from class within a given year. The penalty for exceeding the number of allowed absences is severe, requiring that a student repeat the grade.[3]

It is worth mentioning that, by design, periods of the same subject are usually spread out within the weekly schedule of classes. This is important because one may worry that eligible students

[2] Descriptive statistics from a dataset that covers the universe of high school graduates between 2003 and 2011 show that 90% of students attend public schools, 2% attend public experimental (charter) schools and 8% attend private high schools. There are 1319 high schools in Greece, of which 112 are private and 23 are experimental.

[3] Near the end of the school year 2005-2006, a new bill was passed that included new, more lenient regulations regarding the number of allowed hours (periods) of absence from school. The new bill provided eligible students with 50 additional hours of excused absence. Eligibility was determined on the student's past Grade Point Average. In particular, every student who had received a Grade Point Average higher than 15/20 the previous school year (2004-05) was allowed more absences in the following school year (2005-06).

might skip classes of a particular subject. This strategic selection of classes is not entirely possible because only whole days of absence can be excused. Around 60 percent of school subjects are mandatory, and the remaining consist of electives and specialization courses. In Greece, unlike the situation typical of other educational systems, students remain in their assigned classrooms for the majority of school periods, instead of moving to different rooms depending on the subject being taught. This setting guarantees that a student's peer group remains the same for a series of courses, including Greek language and mathematics, subjects considered in our analysis.

All schools in Greece offer three academic tracks in the eleventh and twelfth grade. Each track offers different courses. The level of the track courses is comparable to the Advanced Placement (AP) courses found in the US educational system. There are three track courses in the eleventh grade, and four track courses in the twelfth grade. The Tracks are: Classics, Science and Information Technology(IT)[4]. All track courses are mandatory and available only for students within a track. Attending a particular track gives access to a set of college degrees relevant to the track attended. Exam scores in the track courses determine college admission. At the end of senior high school, students take national, standardized exams in the track courses in addition to Greek Language and one elective that matter for both high school graduation and university admission. The format of the national exams is the same as the one of the within-school exams in the previous grades, and they are externally marked and proctored.

1.4.2 The 2009 Swine Flu Pandemic

In late spring 2009, the first, sporadic cases of swine flu surfaced in Europe. The 2009 flu pandemic in Europe was part of a pandemic involving a new strain of influenza, subtype H1N1. H1N1 is commonly called swine flu. The pandemic infected at least 125,550 people in Europe. There were 458 confirmed deaths in Turkey, 438 confirmed deaths in Russia, and 299 confirmed deaths in the United Kingdom.

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[4]Students attending the Classics track take Ancient Greek, Latin, and Philosophy in the 11th grade, and Ancient Greek, Latin, Literature, and History in the 12th grade. Students attending the Information Technology track take Mathematics, Physics, and Programming in the 11th grade, and Mathematics, Physics, Computer Programming, and Business Administration in the 12th grade. Students attending the Science track take Mathematics, Physics, and Chemistry in the 11th grade, and Mathematics, Physics, Biology, and Chemistry in the 12th grade

Swine influenza was first proposed to be a disease related to human flu during the 1918 flu pandemic, when pigs became ill at the same time as humans. The first identification of an influenza virus as a cause of disease in pigs occurred later, in 1930. For the following 60 years, swine influenza strains were almost exclusively H1N1. Then, between 1997 and 2002, new strains of three different subtypes and five different genotypes emerged as causes of influenza among pigs in North America. The H1N1 form of swine flu is one of the descendants of the strain that caused the 1918 flu pandemic. As well as persisting in pigs, the descendants of the 1918 virus have also circulated in humans through the 20th century, contributing to the normal seasonal epidemics of influenza. However, direct transmission from pigs to humans is rare, with only 12 recorded cases in the United States since 2005.

According to the U.S. Centers for Disease Control and Prevention (CDC), in humans the symptoms of the 2009 swine flu H1N1 virus are similar to those of influenza and of influenza-like illnesses in general. Symptoms include fever; cough, sore throat, watery eyes, body aches, shortness of breath, headache, weight loss, chills, sneezing, runny nose, coughing, dizziness, abdominal pain, lack of appetite and fatigue. The 2009 outbreak evidenced an increased percentage of patients reporting diarrhea and vomiting, as well. "The 2009 H1N1 virus was not zoonotic swine flu, as was not transmitted from pigs to humans, but from person to person through airborne droplets", we read in the Hellenic Hellenic Center for Disease Control and Prevention (HCDCP) annual report from 2010.

Influenza spreads between humans when infected people cough or sneeze, then other people are exposed by breathing in the virus or touching something with the virus on it. Vaccines are available for different kinds of swine flu. If a person becomes sick with swine flu, antiviral drugs can make the illness milder and make the patient feel better faster. The most common cause of death is respiratory failure. Other causes of death are pneumonia (leading to sepsis), high fever (leading to neurological problems), dehydration (from excessive vomiting and diarrhea), electrolyte imbalance and kidney failure. Fatalities are more likely in young children and the elderly.

The HCDCP reports describe the chronicle of the swine flu outbreak: On May 19, 2009, authorities confirmed the first case of swine flu in Greece. The infected person was a 19-year-old

Greek student who studied in New York and who had flown to Greece a few days before becoming ill. He was hospitalized at Sismanogleion General Hospital of Athens, but was not considered to be gravely ill. The authorities contacted many of the passengers who sat near this patient on the plane, and examined them for suspicious symptoms. At that point in time Greece officials said they had enough anti-virals to cover 12 percent of the population. (European Union directives proposed that health officials have supplied on had to cover at least 10 percent of the population.). The 19-year-old was soon released, and none of the passengers in his flight were found to be infected. Looking back at newspaper articles from 2009 in conjunction with the HCDCP announcements we get an idea of how the swine flu spread in subsequent weeks after the first case: On June 14, 2009, the total number of cases in Greece had reached 20; on June 17, 25 total cases had been reported. On July 9, the total number of cases had reached 216, with 93 of these individuals having fully recovered; on July 14, the total number of cases had reached 323, with 200 having fully recovered. On September 16 the total number of cases had reached 2149.

Schools started on September 12, 2009. The number of new H1N1 cases started declining after October 2009 (Sypsa, Bonovas, Tsiodras, Baka, Efstathiou, Malliori, Panagiotopoulos, Nikolakopoulos, and Hatzakis 2011). The Hellenic Ministry of Education, indicating that it feared a recurrence of the outbreak, announced on April 12, 2010 a one-time-only increase in the number of hours of absence a student was allowed to make without penalty by 30 percent for the current academic year.

The upper limit of absences before and during the reform is given in Table 1.1. After the school year 2009-2010, the old attendance regulation was restored.

1.5 Data Collection

To study the effect of compulsory attendance on school performance we need information on class attendance and performance. Data on attendance are not centrally collected, and can only be found in the school archives. We have visited 134 schools and constructed a unique dataset on student transcripts from a large, randomized sample of high schools in Greece. For this study we focus on public schools (Full Sample: 110 schools, 51,666 students). We also use data from three experimental/charter schools (4,981 students) and five private schools (2,893 students) in our robustness checks. This novel dataset includes every student who attended one of the sampled schools between 2006 and 2012, and contains panel information from the following sources:

1. Administrative data from the high schools containing class identifier, class size [5], gender, year of birth, and year of graduation, information on the courses taken by a given student, and d exam results in each of the last three years of a given student's secondary education . For each student we also know how many hours were she was absent from class for each of the three years of high school (10th, 11th, and 12th grades). We know how many absence hours were excused by parents or with a doctor's note, and how many hours of students' absences remained unexcused. We do not have information on the schools students transfer from or to.

2. School-specific information, including the name of school, type of school (private, public[6], experimental[7])), and geographical location.

3. Average net income information for population within the postcode of the school (expressed in 2009 euros), provided by the Ministry of Finance.

4. Urban density information, provided by the Ministry of Internal Affairs. Urban areas are those with more than 20,000 inhabitants. We also match data on local population from the Hellenic Statistical Authority using the school postcode. Local population refers to city population or the population of the smallest unit of area obtained from the 2011 National Census.

5. Data on flu cases provided by the Hellenic Center of Disease Control and Prevention.

In our analysis we use administrative data provided by high schools in Greece. The map in Figure 1.2 shows the schools included in our data. We analyze course participation and grades at

[5] corr (class size, income)=0.149,corr (class size, experimental)=0.249, corr (class size, urban)=0.179

[6] Students are assigned to public schools according to a school district system

[7] Admission to experimental schools is based on a lottery

the student level for all three high school grades for the years between 2006 and 2012. High school students are assigned to classes by alphabetical order based on their last name. We know to which class each student is assigned, and we use this information to create peer-quality and peer-class-attendance measures. We also have some background characteristics, such as nationality, age and gender, which we use a check for the identification strategy, and to reduce the variance of the error term.

Measuring the causal effect of class attendance on grades requires data on attendance. For every class, attendance is registered by the instructor and two teacher-assistants. The assistants verify which students are present or absent, and gives the list of absentees to the instructor, who checks and signs it. All attendance lists are collected at the end of the day. The record is then digitized by a teacher who is responsible for notifying y parents about their child's attendance once per semester. Even if the administrative data contain no measurement error, the attendance rate is likely to have some inaccuracies for the following reason: The teacher responsible for keeping the attendance record may under-report the actual absences of a student who is extremely close to the cutoff. Going over the cutoff means that the student cannot be promoted to the next grade, and that the school must retain her. Nevertheless, this reason may not lead to an under- or over-estimation of the effect of absences on grades. To see this, note that the effect of absences on grades is estimated through an IV procedure, where a simple Wald-estimates equals: $\frac{E[G|D=1]-E[G|D=0]}{E[A|D=1]-E[A|D=0]}$, where G is the grade, A is number of absences in hours and D=1 if one is subject to the relaxed class attendance policy. First, underreporting the actual number of absences outside the year of treatment underestimates the hours of absence by students who were subject to the relaxed class attendance policy ($E[\widehat{A|D=0}] < E[A|D=0]$). Underreporting of absences outside the treatment years may happen because for some students the attendance policy is too strict for some students, and they find themselves too close to the grade retention cutoff. Second, underreporting the actual number of absences during the year of treatment underestimates the hours of absence by students who were subject to the relaxed class attendance policy ($E[\widehat{A|D=1}] < E[A|D=1]$). Underreporting of absences during the treatment years may happen because some students over-exploit the relaxed attendance policy, and when they find themselves too close to the grade retention cutoff; their teacher may underreport their absences to save them from the devastating charybdis of retention. We do not see any reason why the latter should be greater or smaller than the former. As long as underreporting remains stable over time, the estimated causal effect of absences on grades is unbiased.

1.6 Summary Statistics

Our initial panel consists of 57,380 individuals from 110 schools. Not all schools keep attendance records for every year. For 38,042 individuals from 102 schools we have full transcript information and attendance history. For 34,461 we have full transcript information and attendance history for at least two consecutive grades. For 21,514 individuals from 90 schools we have full transcript information and attendance history for three consecutive grades: 10th, 11th, and 12th grade. For 1,873 students there is missing age or gender information. Attrition can be either from transferring schools, dropping out, or grade retention. Grade retention can be either due to failure to obtain a grade point average of at least 9.5 out of 20, or due to missing more than 50 hours of class without a parent's approval or a doctor's note or 114 hours of class in total. Transcript information can be missing due to grade retention. When a student is grade retained we observe a note in the transcript that the student was retained along with the reason for his/her retention. If a student is retained because they went over the upper limit of absences they are not allowed to take the end-of-the-year exams and thus we observe missing values on the individual subject scores and the GPA for those students. Average grade retention is almost 4%. 1.3% of students is retained due to missing too many classes. We provide summary statistics for grade retention and grade retention due to absences in Tables A.1 and A.2 respectively. Grade retention due to absences is more common among lower performing students. Specifically 3.1% of students in the bottom quintile based on their midterm score in Mathematics end up being retained due to missing too many classes. Grade retention is more common among males than females: 5.1% in males and 2.7% in females. Grade retention has also higher incidence among students who are 18 or older: 19% compared to 3.3% for students younger than 18 years.

For our analysis, we use all students for whom we have full information in all three years (*N = 19,641*). Table 1.3 summarizes the data used in our analysis split by treatment status. Treatment group consists of observation in the year 2010, while control group consists of observations in the years 2006, 2007, 2008, 2009, 2011, and 2012.

Our theoretical motivation described a situation where the optimal level of attendance may be lower than the required level of attendance. If this holds in the educational system that we examine in our study, we would expect the students to exploit in full their absences allowance, that is the number of hours of class they can miss without penalty. To investigate whether students' absences constraint is indeed binding we plot the distributions of absences under the two attendance policy regimes. Since -as we have mentioned- excused absences require either a doctor's note or a parent's approval, they are more costly compared to unexcused absences. For the absences constraint to be binding we would expect a disproportionate amount of student to miss as many hours of class as he/she can getting very close to the upper limit of absences. We would expect this to be more apparent for the less costly type of absence, unexcused absences, as the cost of providing a doctor's note or bringing a parent to school may not allow the students to fully exploit the excused absences limit.

In figure 1.3 we plot the density distribution of total absences under the old class attendance regulation. We see that a portion of the distribution piles up close to 114 hours, the upper limit of total absences. In figure 1.4 we plot the density distribution of total absences under the new class attendance regulation. We see that the right tail of the absences distribution occurs to the right of the old limit and to the left of new class absences limit. In figure 1.5 we plot the density distribution of unexcused absences under the old class attendance regulation. We see that the distribution of unexcused absences piles up close to 50 hours, the upper limit of unexcused absences. In figure 1.6 we plot the density distribution of unexcused absences under the new class attendance regulation. We see that around half of the density is to the right of the old limit and to the left of new class absences limit. Since the hours of absences under the old regulation pile up close to the limit, especially so when we look at the less costly type of class absence, unexcused absence, we see that students experienced a binding absences constraint under the old regulation. When the new attendance regulation increased the upper limit of absences a great part of the absences distribution shifted to the right of the old limit, suggesting that students exploited the lax attendance policy.

1.7 Identification Strategy

In this section we present our empirical strategy for identifying the causal effect of compulsory class attendance on student's attainment. Using our dataset we estimate returns to absences among high school students. The standard approach when estimating the effect of absences on school performance consists of regressing an individual's performance on the number of classes missed. Estimating returns to absences using OLS will lead to biased estimates of β. The main problem is that Absences may be correlated with omitted variables that also affect student performance. In other words, the correlation of Absences with the error term would bias the OLS estimates. A student's own personality, ability, and motivation, as well as his/her class, school, and family environment can affect his/her joint decision of attendance and effort.

To control for unobserved characteristics of the students as well as past input history, we employ the following lagged value-added specification:

$$\begin{aligned} Score_{i,s,c,g,t} =& \beta_0 + \beta_1 Absences_{i,s,c,g,t} + \beta_2 Score_{i,g-1} + \beta_3 X_{i,s,c,g,t} \\ & + \beta_4 Class\ controls_c + Grade\ FE_g + School\ FE_s + \epsilon_{i,s,c,g,t} \end{aligned} \tag{1.4}$$

where $Score$ of student i, in grade g, school s, in classroom c, and in time t is a function of her hours of absence in grade g, school s and time t, some time-invariant factors that vary across grades, and grade varying student characteristics such as age. By including the lagged year performance in the regression model implies, according to the literature of teacher value added (Todd and Wolpin 2003), that we no longer need to incorporate additional measures of ability or previous years inputs. $Score$ can be final exam score in mathematics, Greek, physics or the grade point average (GPA). Scores are standardized by school and grade. We also include controls for class characteristics such as peer quality, measured as the average lagged GPA of the class peers, and class engagement, measured as the average lagged absences of the class peers. We include school fixed effects to control for school-varying characteristics. In section 9 we expand on our specification by allowing for a linear trend as well as school-specific linear time trends. Cluster-robust standard errors are obtained at the classroom level as peers in the same classroom in the same year are likely to share unobservables that may affect both performance and attendance.

When estimated via OLS the above specification give a biased estimate of the effect of absences on performance due to unobserved differences across the students in their relative productivity of time spent in and out of the classroom. Unobserved differences across students in the opportunity cost of learning in class will create a bias in the OLS-estimated effect of absences on performance as part of the association between absence and performance is being explained by a differential propensity to skip class of those who at the margin have a comparative advantage in learning outside the classroom (and an absolute advantage in learning in general). The opportunity cost of learning in class depends on the relative productivity of time spent in and out of class, and may be related to ability, age, family background, and endowed resources. Out-of-class productivity in terms of learning may be age-varying. Students' cognitive gains from one year to the next depend on their endowed ability (Kuh 1995, Bandura 1994, Winne and Hadwin 1998, Pintrich 2000, Zimmerman, Boekarts, Pintrich, and Zeidner 2000, Blomeyer, Coneus, Laucht, and Pfeiffer 2008, Schack et al. 1991). To mitigate the bias from unobserved age-varying opportunity cost of learning in class we exploit exogenous variation from a one-year-only reform that relaxed the cost of missing class. Our instrument changes the cost of class attendance and helps estimate the effect of absences on performance for students who were constrained by the higher cost of missing class.

Our instrument comes from a natural experiment that took place in the school year 2009-2010 in Greece. During that school year the Hellenic Ministry of Education implemented an one-time only reform that increased the unexcused absence allowance for all students in view of the rapid spread of the H1N1 virus in Eastern Europe. The swine flu-related one-time only reform increased the number of hours students could be absent from class by 30 percent. We exclude from our analysis students who enroll into private schools in order to eschew both potential selection issues and heterogeneity in terms of the implementation of the attendance policy regulation.

An instrumental variables approach can address biases due to selection, omitted variables, and measurement error. The bias from measurement error may be less of a threat when this error is time invariant but even measures of performance and attendance are less than perfect. Our identification uses the one-time flu-related absences reform as an instrument for the endogenous variable $Absences$.

The first stage regression equation for our lagged value-added estimator of the returns to absences is the following:

$$\begin{aligned} Absences_{i,s,c,g,t} =& \alpha_0 + \alpha_1 FluReform_t + \alpha_2 Score_{i,g-1} + \alpha_3 X_{i,s,c,g,t} \\ &+ \alpha_4 Class\ controls_c + Grade\ FE_g + School\ FE_s + \eta_{i,s,c,g,t} \end{aligned} \tag{1.5}$$

Where $FluReform_t = \mathbb{1}[schoolyear = 2009 - 2010]$. It's important to note that the Flu reform didn't increase the realized number of absences but only relaxed the students' time budget constraint allowing them to do more absences if they choose so. Thus, the coefficient δ can be viewed as the intention-to-treat effect on the treated (ITT). For all specification we cluster standard errors at the individual level to allow for nonzero covariance of the error term within each individual.

Students may miss class both when they are sick and when they are not sick. In the latter case, student may miss class to enjoy leisure or study. Absence due to sickness may decrease performance. Absence for reasons other than sickness may have a non negative effect on performance depending on how the time outside the classroom is spent. We exploit a natural experiment that allowed students to miss more classes without actually being sick. The effect of absences on performance estimated comes from absences that are not related to sickness.

The validity of our empirical strategy relies on the assumption that the counterfactual trend behavior of outcome variables in treatment and control groups is the same. In other words, we require that our outcome variables do not exhibit a time-varying trend, because in that case we wouldn't be able to disentangle this trend from the time-varying treatment effect. Tables 1.7 and 1.8 show mean gpa and individual subject exam scores over time along with a 99 percent margin of error. We see that the time series of the scores remain relatively steady over time, suggesting that any effects pertaining to 2010, the year of the treatment, are not the result of a time trend.

1.7.1 Validity of the H1V1 virus outbreak instrument

The validity of the Flu reform as an instrumental variable relies on the assumption that it has no direct effect on treated students' performance (exogeneity assumtion). To explore how the Flu affected the treated population we provide a graph[8] that shows the number of verified H1N1 cases and H1N1-related deaths for high-school age individuals during the school year 2009-2010. We see on Figure 1.9 that among 209,958 students attending high school at the school year of the reform, 301 were contracted with the H1N1 strain and 2 of them died. The very few H1N1 cases in the population of high school goers appease potential worries about a direct effect of the H1N1 virus on scholastic performance.

In figure 1.10 we provide visual evidence of the low geographical prevalence of swine flu in the locations we collected data from. Out of the 20 prefectures sampled, five had zero or one cases of swine flu, 10 had two to five cases, 2 had between six and 10 cases, while only the prefecture of Attica, that is home to more than five million people, had 145 cases of swine flu in high school students. We do not exclude affected areas from our sample as the symptoms and the recovery period of the swine flu are no different from the symptoms of the common flu (Smith, Vijaykrishna, Bahl, Lycett, Worobey, Pybus, Ma, Cheung, Raghwani, Bhatt, et al. 2009).

In addition, we provide evidence related to the timing of school absences to further appease potential concerns that the swine flu pandemic affected absences directly and not only through the relaxed class attendance policy. In figure 1.11 we draw mean absences per semester for the 2009-2010 school year. Although the number of new H1N1 cases in Greece started declining after October 2009 (Sypsa et al. 2011), we see that absences increased in the Spring semester of 2010 to 39 hours from 29 hours in the Fall semester. The relaxed class attendance policy was announced in April of 2010 and was put in place retroactively for both the Fall semester of 2009 and the Spring semester of 2010.

[8]The number of all high school students in Greece is an estimate constructed as follows: For 12 and 11 graders of school year 2009-2010 we use the number of students who participated in national exams for university admission, provided by the Hellenic Ministry of Education. For the number of 10th graders of 2009-2010 we use the number of 11th graders of 2009-10. The data on H1N1 verified cases and deaths come from the Hellenic Center for Disease Control & Prevention

Placebo Tests

Our identification strategy is based on the assumption that absences react to time shocks that are related to class-attendance reforms. One may be concerned that other time-specific shocks may obscure the direct effect of changes in class-attendance regulation on absences. After reviewing all the parliamentary and regulatory activities around the 2009-swine flu pandemic, we didn't find any other reform coinciding with the change in class attendance regulation during the 2009-2010 school year or any educational reform that could potentially impact school performance during the years included in the sample. Nevertheless, it is possible that macroeconomic variables may affect both school performance and class attendance. For the change in absences to be attributed solely to the one-time-only change in class attendance regulation, one may require that mean absences returned to their pre-reform levels once the reform was removed. To appease potential concerns regarding differential time trends of absences before and after the reform in class attendance regulation, we provide visual evidence in Figure 1.12. We find that mean absences followed the following trajectory: Mean absences were 66.6 hours in the 2008-09 school year; 76.4 hours in 2009-10 school year (with the more lenient attendance policy); and 66.4 hours in 2010-11 school year. Although our data expand only up to 2012, we see that for two consecutive school years after the reform in class attendance regulation was abolished, mean absences returned to their pre-2010 levels, suggesting that the time trend of absences remains the same before and after the reform, and that the single peak in the time pattern of absences coincides with the class attendance reform of 2009-10.

Moreover, one may be concerned that the change in the variation of absences over time is caused by time trends and not by swine flu-related reform in class attendance regulation. Exploiting the withinschool, across-time variation of absences, we run following specification and capture the coefficients of the year dummy variables.

$$
\begin{aligned}
Absences_{i,s,c,g,t} = &\gamma_0 + \gamma_2 Score_{i,g-1} + \gamma_3 X_{i,s,c,g,t} + \gamma_4 Class\ controls_c \\
&+ Year\ FE_t + Grade\ FE_g + School\ FE_s + \zeta_{i,s,c,g,t}
\end{aligned}
\tag{1.6}
$$

We model total hours of school absence of student i, in grade g, in school s, in year t as function of her own time-varying and time-invariant characteristics such as gender, age, age squared captured in vector $X_{i,s,c,g,t}$, and lagged Grade Point Average, grade fixed effects, school fixed effects, and year fixed effects.

Next, on Figure 1.13 we plot the coefficients of the dummy year variables obtained from the estimation of the above specification along with their 95 percent confidence interval obtained with clustering the standard errors at the class level. school year 2005-2006 is used a base year and is omitted from the model specification. We anticipate that the only coefficient significant in magnitude is that of the 2009-2010 year dummy variable.

The coefficients of the years 2007-2008, 2010-2011, 2011-2012[9] are not statistically significant. Although the standard errors of the 2006-2007 and 2008-2009 year dummy coefficients are quite small, the year dummy coefficients for 2006-2007, 2007-2008, 2008-2009, and 2010-2011 are roughly one fourth in magnitude of the 2009-2010 year dummy coefficient, suggesting an increase in mean absences between 9 and 13 hours in 2009-2010 in comparison to the school years spanning from 2005-2006 to 2010-2011, after controlling for student, grade, and school characteristics.

1.7.2 Assumptions for estimating LATE

In our study, the interpretation of LATE is the average treatment effect of those students who skipped class during the 2009-10 school year when a one-time-only relaxed attendance policy was in place, but who would not have skipped class otherwise. Nevertheless, LATE can only be

[9]Our data contain 860 observations for 2011-2012, while we have 19,101; 20,027; 21,567; 18,178; 14,120 observations for 2005-2006; 2006-2007; 2007-2008; 2008-2009; 2009-2010; 2010-2011 respectively. Small sample size for 2011-2012 may result in statistical power issues when it comes to the statistical significance of the 2012 year dummy.

identified when the instrument is exogenous, when we have a valid first stage, when the exclusion restriction is satisfied, and when the monotonicity assumption is met (Imbens and Angrist 1994). The exogeneity assumption is met because the students do not control their year of birth, and, consequently, they cannot control the timing of their school enrollment. The validity of the first stage and the rejection of the weak-instrument problem are shown in the next section. For the exclusion restriction to be met, we need to establish that the instrument (namely the new relaxed class attendance policy) does not affect grades directly other than through absences. The new attendance policy was introduced immediately after a swine flu pandemic in Europe. To address any concerns regarding the direct effect of the instrument on grades, we use data on flu cases to show that, in fact, very few students contracted flu during the 2009-10 school year. Next we explain why the monotonicity assumption is met. Following Imbens (2010, p.415), imagine a model where the grade of student i solely depends upon his absences as follows:

$$S_i = \alpha_o + \alpha_1 A_t + \epsilon_i \tag{1.7}$$

Absences are endogenous ($cov[A_t, \epsilon_i] \neq 0$), due to personality traits or parental monitoring correlating both with absences and scores. Now, one can think of an absence not as a binary random variable, but as a continuous latent variable (A_t^*) which describes the student's utility of skipping class. Next, this latent variable can be modeled as follows:

$$A_i^* = \beta_o + \beta_1 D_i + \upsilon_i \tag{1.8}$$

Where D_i reflects the assignment to the relaxed class-attendance policy. The continuous variable A_t^* is mapped into a binary variable by the following:

$$A_i = \begin{cases} 1, & \text{if } A_i^* \geq 0 \\ 0, & \text{if } A_i^* < 0 \end{cases} \tag{1.9}$$

The inclusion of D_i in the equation above reflects the benefit of being absent from class. That is, if $D_i = 1$, the utility of being absent from class is higher, since you are free to invest your time the way you want. Hence, a rational utility-maximizing agent would set β_1 higher than 0. Other characteristics such as lack of motivation remain in the error term υ_i. Unmotivated students won't go to class, even if $D_i = 0$: $\upsilon_i \geq -\beta_0$. They are the always takers. Very motivated students will go to class, even if $D_i = 1$: $\upsilon_i < -\beta_0 - \beta_1$. They are the never takers. The estimated results come from the compliers, who are defined as: $-\beta_0 > \upsilon_i \geq -\beta_0 - \beta_1$. This framework excludes the existence of defiers, since (i) if an individual is absent if $D_i = 0$, this implies they will also be absent if $D_i = 1$ (if $\upsilon_i \geq -\beta_0$, then $\upsilon_i \geq -\beta_0 - \beta_1$) and (ii) if an individual is present if $D_i = 1$, this implies they will also be present if $D_i = 0$ (if $\upsilon_i < -\beta_0 - \beta_1$, then $\upsilon_i < -\beta_0 - \beta_1$, then $\upsilon_i < -\beta_0$). Therefore, we are able to identify a well-defined local average treatment effect.

To see why the IV estimate of the second stage (α_1) does not equal ATE, we switch to a heterogeneous framework. This means that the parameters potentially differ by individual, so formally we have α_{1i} in the first-stage regression. If absences were exogenous in the first place, we would still be able to measure an ATE(T), since $ATE(T) = E[G_i|A_i = 1] - E[G_i|A_i = 0] = E[\alpha_{1i}] = \frac{1}{n}\sum_{i=1}^{n} \alpha_{1i} = \bar{\alpha_1}$. Since A_i is exogenous, this average can still be interpreted as an ATE(T). Now consider A_i as an endogenous variable and one used 2SLS in order to get a constant estimate. In a heterogeneous framework, the 2SLS estimator is as follows:

$$\alpha_{1,2sls} = \frac{cov[G_i, D_i]}{cov[A_i, D_i]} = \frac{\frac{1}{n} sum_{i=1}^{n} \alpha_{1i}\beta_{1i}}{\frac{1}{n} sum_{i=1}^{n} \beta_{1i}} \tag{1.10}$$

This boils down to ATE(T) if and only if $\alpha_{1i} = \alpha_1 \forall i$ and/or $\beta_{1i} = \beta_1 \forall i$. Therefore, in a heterogeneous framework the 2sls estimator equals a weighted average of individuals' treatment effects, with largest weight for whom the instrumental variable is most influential. Under the assumptions mentioned above the weighted average measures a LATE. This exercise makes clear that homogeneity in the first stage means LATE equals ATE. Thus, to characterize the LATE, we do the following. We rerun the first stage and include interaction effects between D_i and observables to find for which individuals i, β_{1i} is large or small. Equation 1.10 makes clear that individuals with a large β_{1i} contribute to the LATE estimator and individuals with a small β_{1i} do not contribute to the LATE estimator [10]. Whereas the monotonicity assumption is also fundamentally untestable, we would not want the total effect of D_i to become negative. Indeed, this would cause the explanation below equation 1.9 to break done, since β_{1i} is not positive for all individuals.

1.8 Main Results

1.8.1 Effect of Performance

Main results are reported on Table 1.4. The first column in Table 1.4 corresponds to the contemporaneous specification without lagged score and without school and year fixed effects. The unit of absences is in tens of hours. This shows that missing ten additional hours of class, a student's grade point average decreases by 6.5 percent of a standard deviation, ceteris paribus. In column (2) in Table 1.4 we expand on the contemporaneous specification by including the student's grade point average in the previous year. Controlling for lagged score allows us to capture both innate ability as well as the history of all inputs included in the educational production function up to the current period. When we control for lagged performance, we see that the effect of absences on performance becomes less negative, indicating that past performance explains some portion of the association between absences and performance. In other words, higher levels of past performance may be associated with both higher attendance and higher performance. We find that missing ten hours of class decreases one's performance by 2.2 percent of a standard deviation on average.

In column (3), which corresponds to specification 1.4, we include school fixed effect in our value-added specification to control for school-specific patterns of performance. We see that our estimates don't differ much from column (2): a ten-unit increase in the number of hours of class missed decreases a student's performance by 2 percent of a standard deviation.

Our estimates of column (3) are subject to an omitted-variable bias as unobserved grade-specific shocks may be associated with lower rates of attendance and poorer performance. For example, some students may be afflicted with sickness in certain years but not others. When a student is sick, it is probable that they exhibit lower performance as well as lower class attendance.

[10] In the extreme where $\beta_{1i} = 0$ the contribution of student i is zero

Moreover, not everyone is affected by such time-specific shocks in the same time periods. Nevertheless, when these time-specific shocks occur and are not observed, we cannot disentangle their effect on performance from the effect of absences on performance, as we do not observe who was subject to such a shock and who wasn't. There are additional grade-specific factors that could bias the effect of absences on performance. Students may become better at learning on their own as their grow up. It's important to note that our analysis looks at students who are at the end of their adolescence and near the beginning of their adulthood. Students who get closer to the age of 18 may have accumulated enough human capital, so as to substitute sufficiently class learning with self learning. The existing literature (see, Kuh 1995, Bandura 1994, Winne and Hadwin 1998) supports that college aged individuals are more able to learn on their own in comparison to younger students. Therefore, as students reach the end of their secondary education and prepare for their university entrance exams, they may find it more productive to learn on their own rather than in class. This effect is time-specific as it is more relevant to students in the junior or senior year of high school. However, the extent to which age brings upon a certain student the necessary self-discipline and maturity to learn on their own is largely idiosyncratic and differs from person to person. In that sense, our omitted-variable bias comes from a combination of grade and individual -specific unobservables.

To mitigate the omitted-variable bias from grade-student-specific shocks we exploit variation in the class attendance regulation. During the 2009-2010 school year students were allowed to miss 30 percent more classes without penalty. The relaxed attendance policy allowed students to miss class for reasons different from those related to sickness. As students could miss class without providing a doctor's note or their parent's approval, students could miss class to enjoy more leisure or study. The reform allows us to compare same grade individuals across years, while controlling for their past performance, to net out any unobserved grade-student-specific unobserved shocks. Our estimates are reported in column (4) of Table 1.4 and correspond to specification 1.4 estimated with IV. We find that missing ten hours of class increases the grade point average by 4.1 percent for those students who missed more classes due to the relaxed attendance policy. Since the new attendance policy didn't force students to miss class but rather simply gave the opportunity to miss class more frequently, only students who would be better off either in terms of leisure of studying at home would take advantage of the new policy. Exploiting the relaxed attendance policy to enjoy more leisure or to study at home would generate in principle different returns to absences. We do not know how students actually allocated their out-of-class time. However, we can interact students, class, and school characteristics with the variable of interest and estimate heterogeneous effects of absences. Our reduced form and first stage estimates are reported on Table 1.5. We find that the relaxing the class attendance policy results in a roughly 11 hour increase in the number of hours of class missed in a given year. Our reduced form results show that the reform that changed the attendance requirements induced a 4.6 percent of a standard deviation increase in the grade point average.

Robustness

One threat to identification is the existence of upward trend that could explain the positive estimated effects of absences on performance. To control for the existence of time trends in our outcome variables we perform the following robustness checks: We augment our specifications by adding a linear time trend or school-specific linear trends. In the case of a linear time trend our main specification 1.4 becomes:

$$\begin{aligned} Score_{i,s,c,g,t} = &\beta_0 + \beta_1 Absences_{i,s,c,g,t} + \beta_2 Score_{i,g-1} + \beta_3 X_{i,s,c,g,t} + \beta_4 t \\ &+ \beta_5 Class\ controls_c + Grade\ FE_g + School\ FE_s + \epsilon_{i,s,c,g,t} \end{aligned} \tag{1.11}$$

where the parameter β_4 captures the effect time t on our outcome variables. We go one step further by allowing for the existence of school-specific linear trends in our outcome variables. In that case, our main specification 1.4 becomes as follows:

$$\begin{aligned} Score_{i,s,c,g,t} = &\beta_0 + \beta_1 Absences_{i,s,c,g,t} + \beta_2 Score_{i,g-1} + \beta_3 X_{i,s,c,g,t} + \beta_4 t_s \\ &+ \beta_5 Class\ controls_c + Grade\ FE_g + School\ FE_s + \epsilon_{i,s,c,g,t} \end{aligned} \tag{1.12}$$

where the effect of time t_s on performance is now allowed to vary from one school to the next. Both specifications 1.11 and 1.12 are estimated via IV using exogenous variation from the introduction of a relaxed attendance policy.

Our results are shown in Table 1.6. We see that although our estimates of the returns to class absences decrease in magnitude compared to Table 1.4, the estimated standard errors remain almost half the size of the effects of absences both in odd-numbered columns that include a linear time trend as an additional control, and in even-numbered columns where school-specific time trends have been included to the list of explanatory variables. Our estimated effects are smaller compared to the specifications without the time trends in Table 1.4, suggesting the existence of some time trends. Nevertheless, when we control for even school-specific time trends, the effect of absences on performance is found positive and significant both in magnitude and statistically. Specifically, our preferred specification shown in column (8) of Table 1.6 shows that a ten-hour increase in the class absences leads to an increase of school performance by 2 percent of a standard deviation.

1.8.2 Heterogeneous propensity to skip class

1.8.3 By Ability

The H1N1-related reform relaxed for one school year only the class attendance policy. The reform allowed students to skip up to 34 more hours of school without penalty. Nevertheless, the decision to skip class when given the opportunity may not be identical for everyone. In fact, individual propensity to skip class may depend on individual, class or school characteristics. In this section we explore whether there is differential response to the relaxed time-budget constraint.

The first question we ask is: Does the effect of the flu shock on absences differ across the ability distribution? To answer this question, we employ the following regression model with interaction terms. The model below is an augmented version of regression equation (1) where the flu-shock variable is interacted with a prior-ability variable. To proxy cognitive ability we obtain the student's within-school rank based on the 10th grade GPA. The $Ability$ variable takes the values in [1,100] where the value 100 represents the top 1 percent of one's class. The following model is estimated for students attending 11th grade or 12th grade across years.

$$
\begin{aligned}
Absences_{i,s,c,g,t} =& \alpha_0 + \alpha_1 Flu_t + \alpha_2 Flu_t \times Ability_{i,10} + \alpha_3 Ability_{i,10} + \alpha_4 X_{i,s,c,g,t} \\
&+ \alpha_5 Class\ controls_c + Grade\ FE_g + School\ FE_s + \eta_{i,s,c,g,t} \qquad (1.13)
\end{aligned}
$$

We hypothesize that when the class attendance policy relaxes student who have the resources or the ability to learn outside the classroom may choose to skip class. Among those students we expect that students with higher human capital accumulation or ability may exploit the relaxed attendance regulation even more. Our findings are presented in table 1.8. We find that the higher the measure of prior cognitive ability the more the hours a student skips class. In particular we find that when controlling for variation across students and grade, being ranked 1 percent higher in your class increases the effect of a relaxed class attendance policy on your hours of absences by almost 2 hours. However, our estimated effect has large standard error. When we focus on different types of absences, we find that being ranked 1 percent higher in your class increases the estimated effect of a relaxed class attendance policy on your hours of excused absences by more than 2 hours, with a standard error almost half the size. The effect of the lax attendance policy on unexcused absences does not seem to vary with prior ability as the estimated coefficient of the interaction of interest is not significant quantitatively and statistically.

1.8.4 By Peer Quality

Next, we examine how the effect of the relaxed attendance policy on absences changes with the mean performance of classroom peers. Peer quality is defined as the average of lagged Grade Point Average of class peers[11]. Since we are employing a within-school estimation approach, the peer quality variable would pick up differences in the peer quality across classes and across years within the same school. We are using a logarithmic transformation of one's peer quality to normalize those differences and to estimate the effect of relative rather than absolute changes in the peer quality. We model differential effects of the flu shock on absences by peer quality using

---

[11] We follow the so-called leave-one-out approach in defining peer quality. For each student we calculate the average lagged GPA of the other students in the same classroom.

the following specification.

$$
\begin{aligned}
Absences_{i,s,c,g,t} =& \alpha_0 + \alpha_1 Flu_t + \alpha_2 Flu_t \times Log(PeerQuality)_c + \alpha_3 Log(PeerQuality)_c \\
&+ \alpha_4 Score_{i,g-1} + \alpha_5 X_{i,s,c,g,t} + \alpha_5 Class\ controls_c + Grade\ FE_g \\
&+ School\ FE_s + \eta_{i,s,c,g,t}
\end{aligned} \tag{1.14}
$$

Where $Class\ Engagement$ is the average lagged absences of class peers. Our OLS estimates of the equation 1.14 reported in table 1.9 show that on average peer quality matters significantly for the effect of the flu shock on absences. Specifically, a 10 percent decrease in peer quality leads to a one and a half-hour increase in the effect of the relaxed attendance policy on mean total absences. When we split absences in excused and unexcused we see that peer quality matters for the effect of the relaxed attendance policy only on excused absences, that is whole day absence for which the doctor's note or a parent's approval was provided. In particular, a 10 percent decrease in peer quality leads to a almost one and a half-hour increase in the effect of the relaxed attendance policy on mean excused absences, with a standard error of two fifths of that size. Peer quality doesn't seem to matter for the effect of the relaxed attendance policy on performance, as the estimated coefficient of the interaction of peer quality with the shock is not significant either in magnitude or statistically. Considering that the relaxed attendance policy is more likely to be exploited by those students who can learn outside the classroom, the estimated differential effect of the flu reform by peer quality measures the negative externality such a student incurs when they are forced to stay in a deteriorating class environment.

1.8.5 By Postcode Income

Next, we are interested in measuring potential differential effects of the flu-related reform on absences by socioeconomic status. Although we do not observe students' family income, we have a measure of mean family income at the postcode level. We are interested in the cross-sectional variation of income rather than the over-time variation for two reasons. The flu reform is a variable that changes over time, and in order to measure its heterogeneous effects in terms of cross-sectional characteristics, we can't have those characteristics changing over time as well; if this were the case, we wouldn't be able to completely exclude the possibility of that part of the variation in our cross- sectional characteristics could be explained by our instrument. Thus, as a measure of socioeconomic status, we use the mean family income at the postcode level expressed in euros in 2009, a year before the flu-related reform. We explore differential propensity to skip class in terms of rural setting using the following model:

$$\begin{aligned}
Absences_{i,s,c,g,t} =& \alpha_0 + \alpha_1 Flu_t + \alpha_2 Flu_t \times Log(Income)_s + \alpha_3 Log(Income)_s \\
& + \alpha_4 Score_{i,g-1} + \alpha_5 X_{i,s,c,g,t} + \alpha_5 Class\ controls_c + Grade\ FE_g \\
& + School\ FE_s + \eta_{i,s,c,g,t} \qquad (1.15)
\end{aligned}$$

According to table 1.10 a 10 percent decrease in family income increases absences by almost 10 hours on average when under the relaxed attendance policy. This evidence suggests that the better socioeconomic conditions negatively correlated with skipping class. We propose two hypotheses that may explain our evidence; these mechanisms are not mutually exclusive. First, students of higher family socioeconomic status (as proxied by the mean postcode income) may be less prone to the absenteeism not necessarily because it is not in their best interest to skip class but perhaps because social norms and behaviors they have been exposed to may deem such a behavior as immoral or unacceptable. Second, public schools in wealthier neighborhoods may provide higher-quality schooling in terms of either the educational inputs or the learning environment. Nevertheless, our results are robust to controlling for peer quality. As previously noted, the ministry of Education does not take into consideration any quality characteristics when allocating teachers to schools; however, self-selection cannot be excluded.

1.8.6 Heterogeneous Returns to Absences

1.8.7 By Ability

Next, we investigate whether and how returns to absences vary across the ability distribution. It is important to note that the estimated returns to absences that we have discussed so far are relevant to those who choose to miss class due to the newly introduced lax attendance policy. Assuming that only students who would benefit from class absence who choose to skip class, we anticipate an average return to class absence higher than zero, even though the magnitude of the return to absence may vary across the ability distribution. Using within-cohort ranking in the 10th grade to proxy cognitive ability, we estimate the effect of absences on performance due to the introduction of a relaxed attendance policy conditional on cognitive ability. We employ the following specification:

$$\begin{aligned} Score_{i,s,c,g,t} = & \alpha_0 + \alpha_1 Absences_{i,s,c,g,t} + \alpha_2 Absences_{i,s,c,g,t} \times Ability_{i,10} + \alpha_3 Ability_{i,10} \\ & + \alpha_4 X_{i,s,c,g,t} + \alpha_5 Class\ controls_c + Grade\ FE_g + School\ FE_s + \eta_{i,s,c,g,t} \end{aligned} \tag{1.16}$$

where $g \in 11, 12$. Specification 1.16 is estimated by IV. The two endogenous variables-absences and the interaction of absences and our proxy for cognitive ability-are instrumented by the flu shock and the interaction of the flu shock and our proxy for cognitive ability, respectively. Our results are shown in Table 1.11. We find that the higher a student's prior performance, the more positive the effect of absences of school performance. In particular, the performance of a student at the top one percent of his cohort improves by a net almost 0.2 percent of a standard deviation when he misses additional 10 hours of class in a given year. Our findings suggest that students who exhibit higher cognitive ability are worse off staying in class compared to less able students. Our results are opposite for student at the left end of the ability distribution. The performance of students at the bottom 1 percent of their cohort decreases by a net 0.2 percent of a standard deviation when they miss additional 10 hours of class in a given year.

Our evidence suggests that students of different cognitive ability either exploit differently the relaxed attendance policy or their out-of-class learning productivity is not homogeneous. The strong positive returns to absences for more able students suggest that better students may choose to skip class in order to study or avoid some class externality that could possibly disrupt their learning process. On the other hand, the negative returns to absences for weaker students implies that their out-of-class productivity is not higher than their in-class productivity either because weaker students may spend their out-of-class time in leisure or because they do not have enough human capital accumulation in order to harvest the same gains from self-study as better students do.

1.8.8 By Peer Quality

To see how returns to absences differ across the peer quality distribution we estimate the following model where we interact the effect of absences with the natural logarithm peer quality. Peer quality is calculated for each student as the average lagged GPA of other peers in the same classroom.

$$\begin{aligned} Score_{i,s,c,g,t} =& \alpha_0 + \alpha_1 Absences_{i,s,c,g,t} + \alpha_2 Absences_{i,s,c,g,t} \times Log(PeerQuality)_c \\ &+ \alpha_3 Log(PeerQuality)_c + \alpha_4 Score_{i,g-1} + \alpha_5 X_{i,s,c,g,t} + \alpha_5 Class\ controls_c \\ &+ Grade\ FE_g + School\ FE_s + \eta_{i,s,c,g,t} \end{aligned} \tag{1.17}$$

Our estimates are shown on Table 1.12. Overall, having class peers at the top quintile of the sample distribution increases your return to absence in terms of GPA, suggesting that high achieving peers may intimidate a student or the instructor neglects weaker students. However, the estimated standard error of the interaction term of interest is much larger than the coefficient, suggesting that that the effect of absences on performance does not vary statistically significantly by peer quality. It is important to note that the variation we observe in the peer quality in the classroom is not large. The difference in the mean peer quality between the top and bottom quintile is 16.58%, where peer quality is defined as the mean lagged GPA of one's class peers.

1.8.9 By Postcode Income

We explore differential effects of absences on performance by estimating model (1.18).

$$
\begin{aligned}
Score_{i,s,c,g,t} =& \alpha_0 + \alpha_1 Absences_{i,s,c,g,t} + \alpha_2 Absences_{i,s,c,g,t} \times Log(Income)_s \\
& + \alpha_3 Log(Income)_s + \alpha_4 Score_{i,g-1} + \alpha_5 X_{i,s,c,g,t} + \alpha_5 Class\ controls_c \\
& + Grade\ FE_g + School\ FE_s + \eta_{i,s,c,g,t} \qquad (1.18)
\end{aligned}
$$

Our results in Table 1.13 show postcode income does not seem to matter quantitatively or statistically for the effect of absences on performance.

1.8.10 By Subjects

In this section we explore how returns to absences differ among different subjects. We focus our analysis on Modern Greek, and Mathematics. Both subjects belong to the core curriculum and are mandatory courses for every high school student. It is important to note that we do not observe in our data how many hours of classes each student missed for every subject in a given school year. We rather observe an aggregate number of hours of absences for every student in a given school year. We investigate the effect of the total number of absences a student makes in a given school year on the end-of-the-year cumulative exam scores for Greek and Mathematics. Exams scores are not curved a follow a 100-unit scale. We standardize the exam scores by school and grade. Our specification is that described in equation 1.4 and is estimated by IV. The outcome variable $Score$ takes the values of standardized exam scores in Greek, and Mathematics. Our results are shown in Table 1.14. We find that the level of Absences does not have a statistically significant effect on the exam score in Greek. However, our estimates show that there are positive and statistically significant effects of Absences on the exam score for Mathematics, suggesting that missing more hours of class improve the Mathematics exam score. Specifically, missing additional 10 hours of class improves the Mathematics exam score by roughly 3 percent of a standard deviation. Our estimates are robust when we include school-specific linear time trends, shown in column (4) of Table 1.14.

1.8.11 By Track

In this section we explore how returns to absences vary across different specializations. We have already mentioned that students in the 11th and 12th grade choose a field of specialization (track): classics, information technology, or science. Attending a specific track allows students to apply for admission to university degree programs relevant to the chosen track. For instance, in order to apply to university degree program in History and Archaeology one must have attended the Classics track in high school. We use the subsample (18,943 individuals) of 11th and 12th grade students for whom we have full specialization information, final exam scores in the track courses, and attendance information to investigate heterogeneous returns to class absences for the three tracks available. We estimate the following specification via IV, where the endogenous variable absences is instrumented by the exogenous reform in the class attendance policy during the 2009-10 school year.

$$\begin{aligned} Score_{i,s,c,g,t} =& \beta_0 + \beta_1 Absences_{i,s,c,g,t} + \beta_2 Score_{i,g-1} + \beta_3 X_{i,s,c,g,t} \\ & + \beta_4 Class\ controls_c + School\ FE_s + \epsilon_{i,s,c,g,t} \end{aligned} \tag{1.19}$$

The difference between specification 1.19 and specification 1.4 is that in specification 1.19 we cannot include grade-specific fixed effects. Therefore specification 1.19 is estimated for 12th grade students using lagged score values from the 11th grade as a control variable.

Our results are shown in Table 1.15. We find that absences have a significant effect on performance both quantitatively and statistically for students in the Information Technology (IT) and Science Track. The effect of absences on performance for students attending the Classics is found to be negative, suggesting that missing more hours of class decreases the average score in the track courses for students specializing in Classics. On the other hand, missing more hours of class seems to improve the performance in terms of the average score in the track courses for students attending the IT or the Science Track, who missed more classes due to the relaxed attendance policy.

1.9 Conclusion

Our study uses new high school transcript data to address two questions. First, why does a student skip class? Second, what is the effect of absences on performance? Our identification strategy exploits a natural experiment that occurred when a European outbreak of swine flu led Greek officials to adopt regulations allowing students to miss 30 percent more class time without penalty during the 2009-20 school year. We provide evidence that very few students were directly affected by the swine flu; high school students were not a high-risk group, and most student absences took place well after the outbreak ended. Our institutional setting has the following features. First, students are assigned to classes according to the alphabetical order of their surnames, and class peers remain the same across subjects. Second, assignment to schools follows a school- district system based on geography. Third, attendance is diligently monitored, and the penalty for missing more than 114 class hours in a given year is severe: grade retention. The one-time-only reform allowed students to skip one period or a whole day of school. We use a within-student estimator to control for individual and age effects. Our outcomes include grade point average in the 10th, 11th, and 12th grades, and exam scores in specific subjects with minimal curricular variation across classes and schools.

We show that, when given the opportunity, students who are more likely to skip classes are those who have established records of higher prior performance, who have academically weaker peers in their classes, or who live in poorer neighborhoods. We find that students who choose to skip class when the attendance policy relaxes perform better, achieving an overall GPA that is 0.04 of standard deviation higher. We explore how the introduction of a relaxed attendance policy affected students in different parts of the ability distribution. We find that students of higher prior ability enjoy higher returns to absences, suggesting that more able students can do better under a less strict attendance policy. Students at the top one percent of the ability distribution proxied by the 10th grade GPA enjoy a net two percent of standard deviation increase in their end-of-the-year exam performance when they miss additional 10 hours of class. Our results are opposite for weaker students. The performance of students at the bottom one percent of their cohort, as measured in the

10th grade, decreases by two percent of a standard deviation when they miss additional 10 hours of class.

We also explore heterogeneous returns to absences across both different subjects in the core curriculum as well as across different specialization tracks in the 11th and 12th grade. We find negative returns to absences in Greek language and positive returns to absences in mathematics in the core curriculum. Our results are similar in the specialization track analysis, where we find negative and statistically insignificant returns to absences for students in the Classics Track, but positive returns for students in the Information Technology and Science Tracks, which both put emphasis on mathematics in the track curriculum. Our finding suggest that it is possible to gain from absence in certain fields of study but not in others.

Our estimated positive effects of absences show that a compulsory class attendance policy can hurt the performance of certain students. Allowing students to miss class has considerable effects on their performance. Our effect is of comparable magnitude to being taught by a teacher one standard deviation above the average (Chetty, Friedman, and Rockoff 2014b, Rivkin, Hanushek, and Kain 2005c). Moreover, our effect on test scores is of a similar magnitude to reducing the class size by 10-15 percent (Krueger 1997, Angrist and Lavy 1999b). These interventions are significantly more costly than a lax attendance policy and it could free up resources that could be used to boost the performance of those who rely more on school resources.

One limitation of our study is that we do not observe class attendance for specific subjects, and thus we cannot exclude the possibility that the observed differences in returns to absences may be due to the students' selecting to skip specific subjects but attend others. Our findings speak both to literature exploring the reasons for absenteeism as well as the literature investigating the quantity of inputs in the educational production function. A revealed preference argument leads us to claim that those who exploit a relaxed attendance policy do so because it makes them happier, either because they can enjoy more leisure or because they can learn on their own. Our estimated return to absence can be viewed as the externality compliers incur when they are forced to attend class. This externality may be related to class size, peer quality, and/or school characteristics. Our study supports the view that students of different characteristics have different input needs, and it highlights the trade-off between equality in an educational system and efficiency in terms of allocation of educational inputs.

Figure 1.1: A Model of Time Allocation

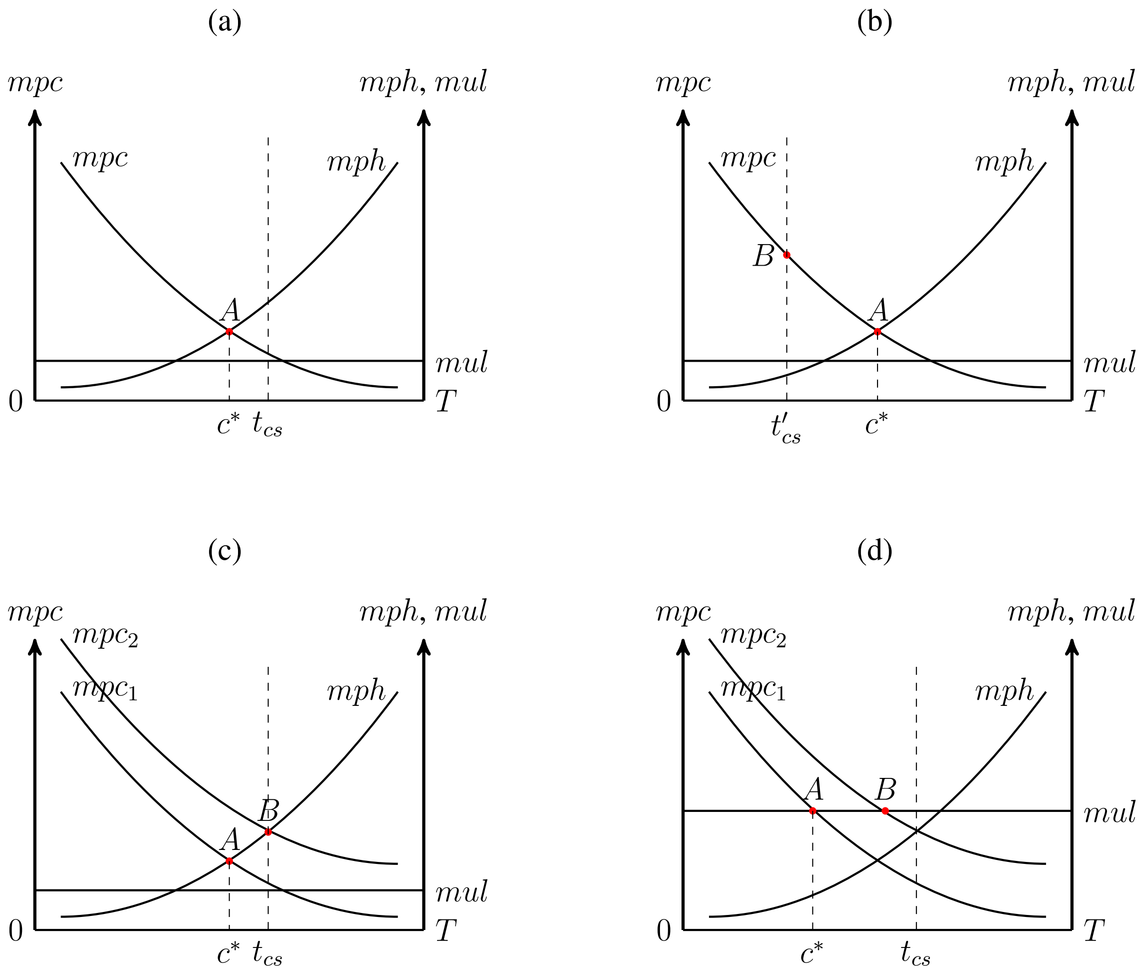

Table 1.1: Upper limits of school absences

	Old Regulation	**Flu Regulation**
Excused Absences	64	83
Unexcused Absences	50	65
Total Hours	114	148

Figure 1.2: NEW TRANSCRIPT DATA

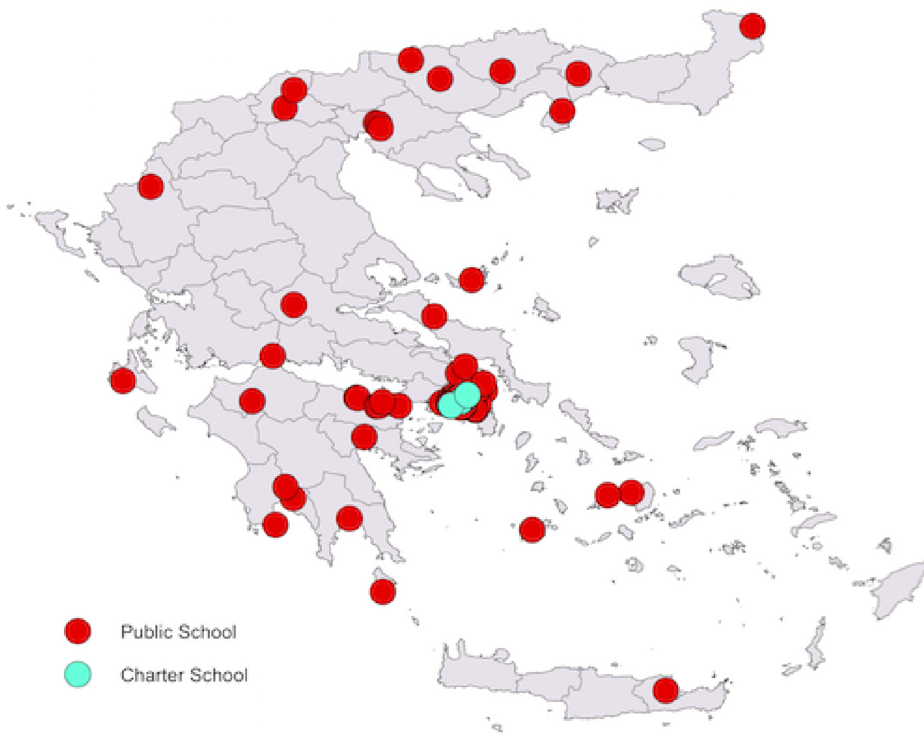

Table 1.2: AVERAGE NUMBER OF ABSENCES

	Total Absences		Excused Absences		Unexcused Absences	
	Mean	s.d.	Mean	s.d.	Mean	s.d.
Average	59.13	63.47	25.02	24.03	34.09	57.35
Math Score						
Highest Quintile	60.06	32.92	31.68	26.40	28.36	13.99
Fourth Quintile	58.46	31.60	28.42	24.40	30.04	14.33
Third Quintile	60.75	30.91	28.45	24.01	32.25	14.09
Second Quintile	64.94	30.33	29.85	23.78	35.03	13.83
Lowest Quintile	88.77	83.91	32.31	25.41	56.47	82.93
Gender						
Male	66.16	29.18	23.68	22.76	36.95	35.59
Female	65.71	42.71	31.90	25.51	33.78	32.23
Grade						
10th	53.69	55.55	19.42	19.79	34.29	51.13
11th	61.79	52.91	23.29	21.50	38.40	47.80
12th	83.36	36.34	45.34	24.02	38.03	27.72
Semester						
Fall	26.50	28.43	9.48	13.63	17.02	23.10
Spring	35.49	28.85	18.26	18.35	17.23	21.03
Setting						
Urban	65.01	43.33	31.57	27.29	33.50	32.74
Rural	66.98	50.62	29.78	24.63	37.17	43.58
Neighborhood Income						
Highest Quintile	68.19	58.10	29.41	24.28	38.48	52.90
Fourth Quintile	66.22	44.80	30.56	25.50	35.77	35.51
Third Quintile	67.83	48.16	30.49	24.47	37.34	39.87
Second Quintile	63.84	45.02	28.15	23.67	35.70	36.71
Lowest Quintile	68.38	54.75	30.51	25.53	37.88	48.97
Peer Quality						
Highest Quintile	66.40	31.54	31.67	25.01	34.55	15.37
Fourth Quintile	69.31	32.00	34.36	25.41	34.67	15.49
Third Quintile	69.88	31.65	34.54	25.54	35.13	14.97
Second Quintile	71.72	32.49	35.92	25.06	35.77	17.58
Lowest Quintile	72.53	32.25	36.57	24.72	35.97	17.07
Year						
2006	62.47	51.68	26.24	22.44	36.22	45.86
2007	66.37	52.53	28.96	24.16	37.41	46.09
2008	63.22	47.80	27.53	24.13	35.71	40.49
2009	66.92	50.35	30.15	24.44	36.53	44.27
2010	76.42	49.78	35.56	27.84	40.89	40.20
2011	66.42	46.81	31.39	24.17	35.06	39.42
2012	65.30	59.96	32.99	27.39	33.81	53.13

Sample: 58,923 obs; 19,641 individuals. Neighborhood income is measured as average family income at postcode in Euros in 2009. Peer quality is the mean lagged grade point average of class peers

Figure 1.3: DISTRIBUTION OF ABSENCES UNDER OLD ATTENDANCE REGULATION

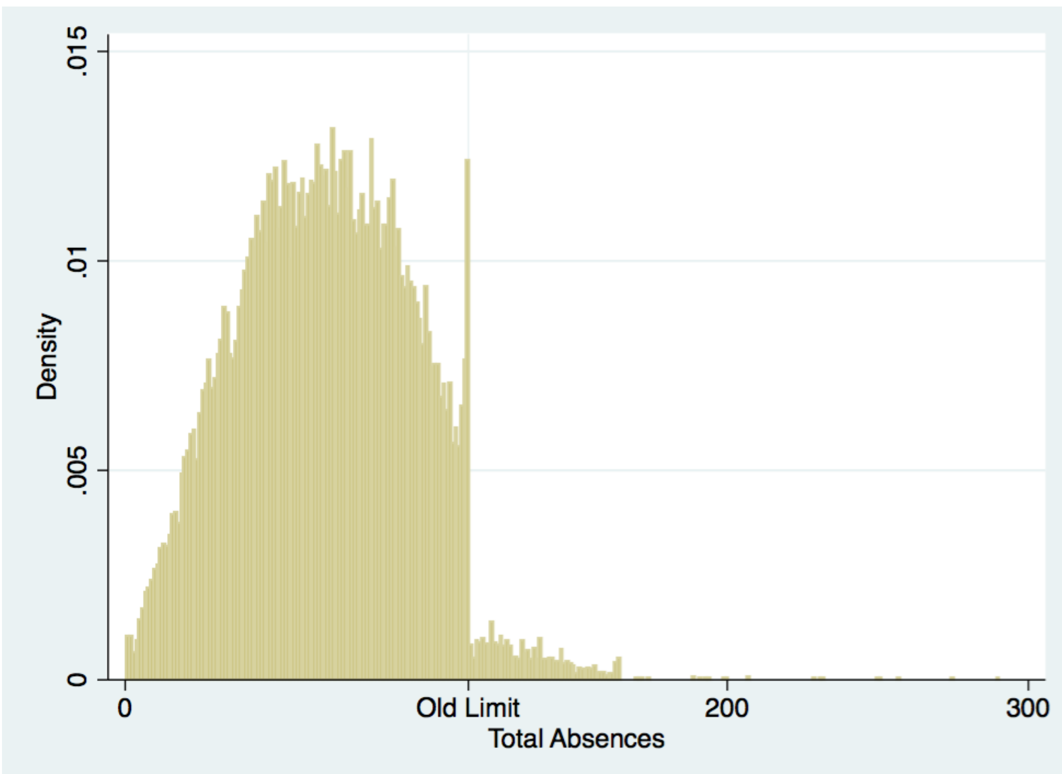

Figure 1.4: DISTRIBUTION OF ABSENCES UNDER NEW ATTENDANCE REGULATION

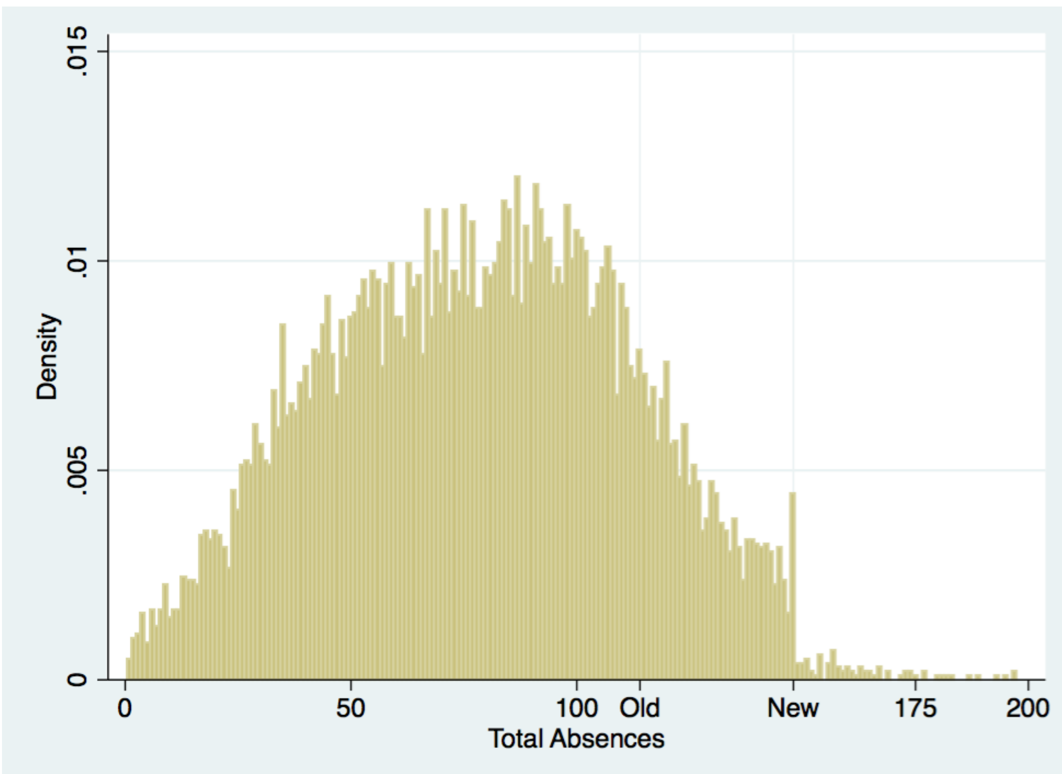

Figure 1.5: DISTRIBUTION OF UNEXCUSED ABSENCES UNDER OLD ATTENDANCE REGULATION

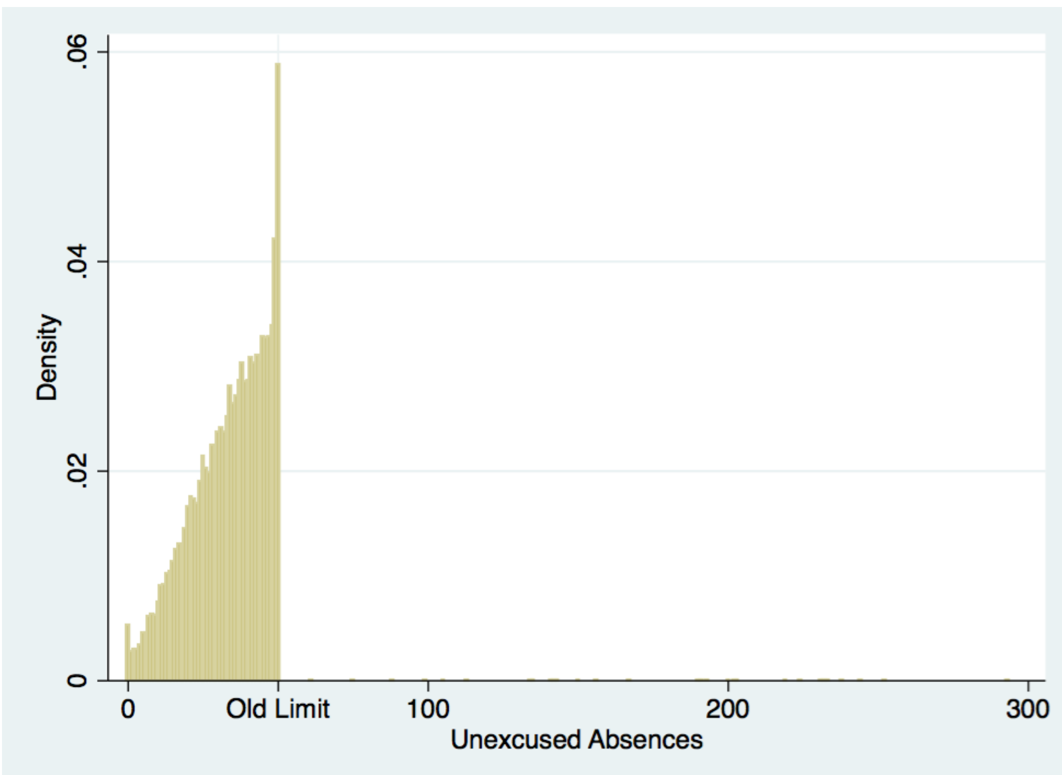

Figure 1.6: DISTRIBUTION OF UNEXCUSED ABSENCES UNDER NEW ATTENDANCE REGULATION

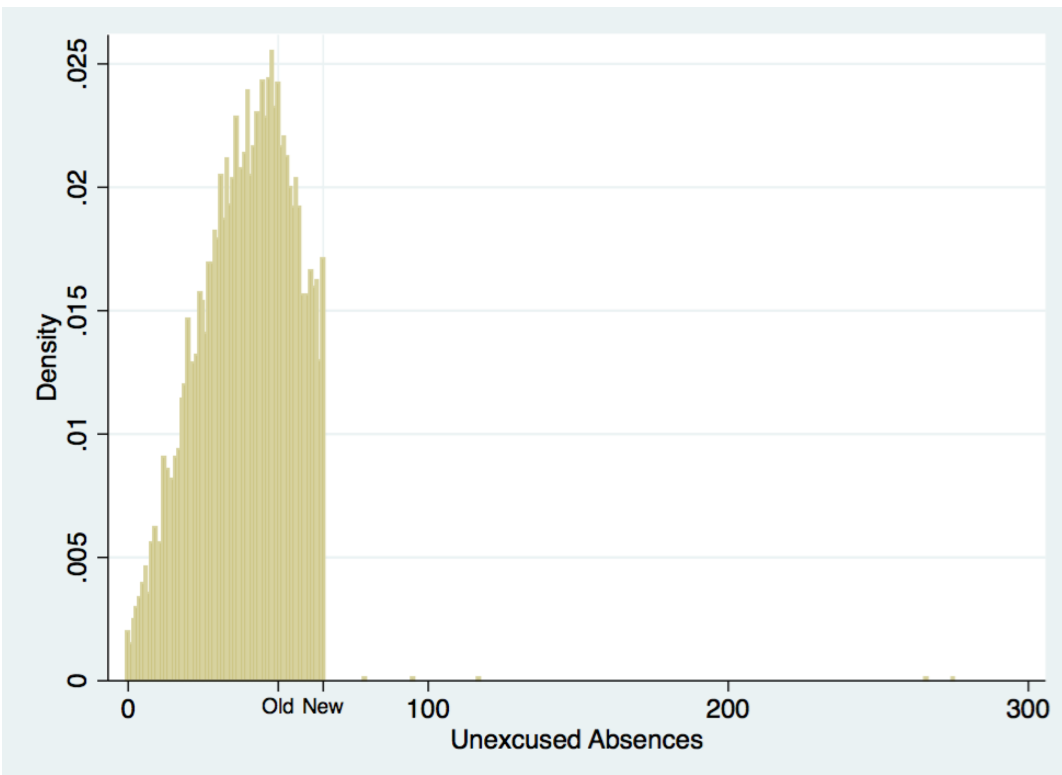

Table 1.3: TREATMENT AND CONTROL GROUP

Variable	Control Mean	Treatment Mean	Difference (b/s.e.)
Student Characteristics			
Age	16.928	17.530	0.602***
			(0.008)
Female	0.558	0.558	0.000
			(0.005)
# of Students	48,528	10,395	
Class Characteristics			
Class Size	22.908	22.296	-0.612***
			(0.038)
Mean Lagged GPA	13.769	13.733	-0.036**
			(0.13)
# of Classes	2,508	534	
School Postcode Characteristics			
Rural	0.060	0.058	-0.002
			(0.003)
log(Population)	11.109	11.103	-0.006
			(0.011)
log(Income)	9.967	9.964	-0.003
			(0.002)
# of Schools	85	83	

Note: Data span graduating classes of 2008-2012 (years 2006-2012). Sample: 58,923 obs (19,641 individuals). Annual Income is in 2009 Euro. We use data from 12 schools in rural areas and 73 in urban areas. Grades use a 20-point scale. Population refers to city population or the population of the smallest unit of area obtained from the 2011 Census. The treatment period is the year 2010 while the control period consists of the pooled years 2006, 2007, 2008, 2009, 2011, 2012. *,**,*** denotes significance at the 10%,5% and 1% level respectively.

Figure 1.7: No Trend Assumption

Figure 1.8: No Trend Assumption

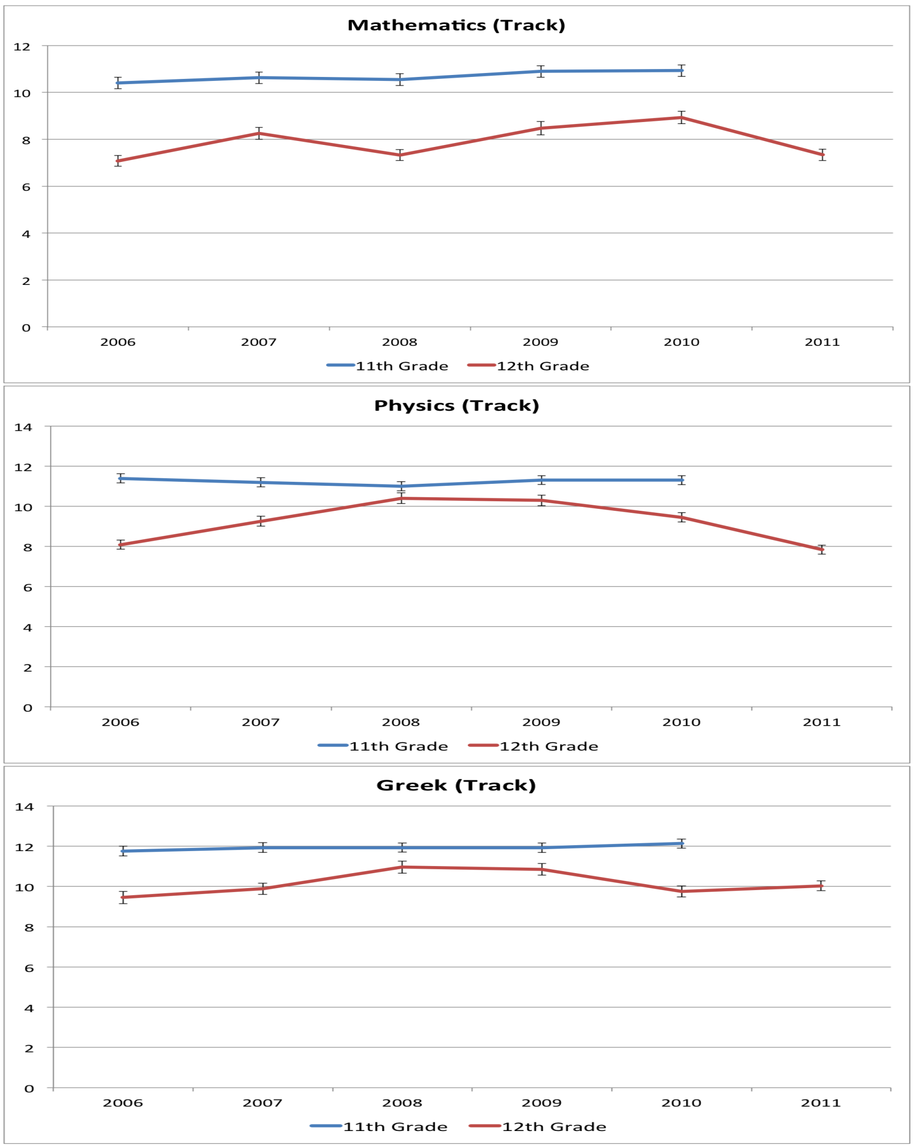

Figure 1.9: H1N1-INFECTED HIGH SCHOOL STUDENTS IN GREECE

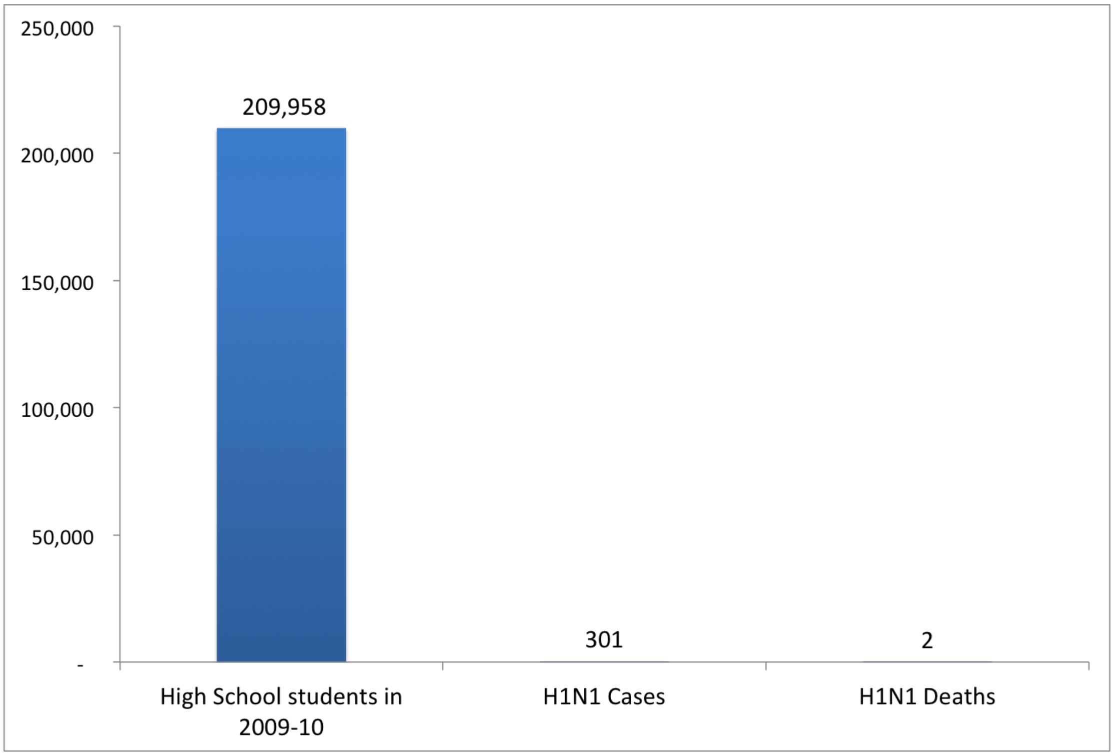

Figure 1.10: Swine Flu Cases in Greece in 2009-2010

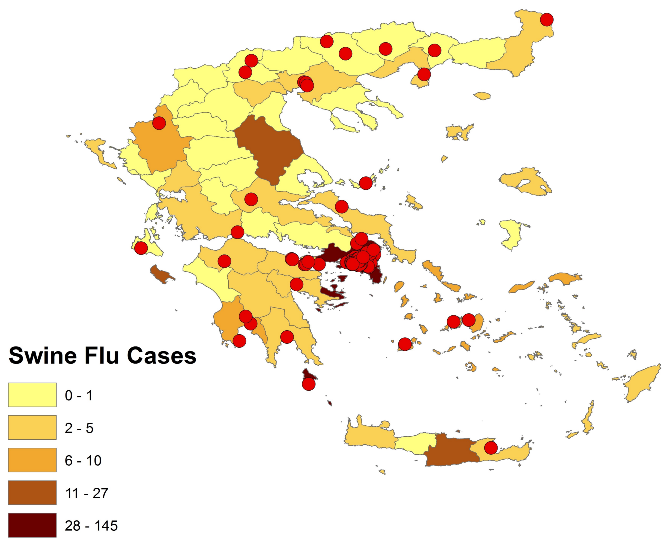

Figure 1.11: Timing of absences in 2009-2010

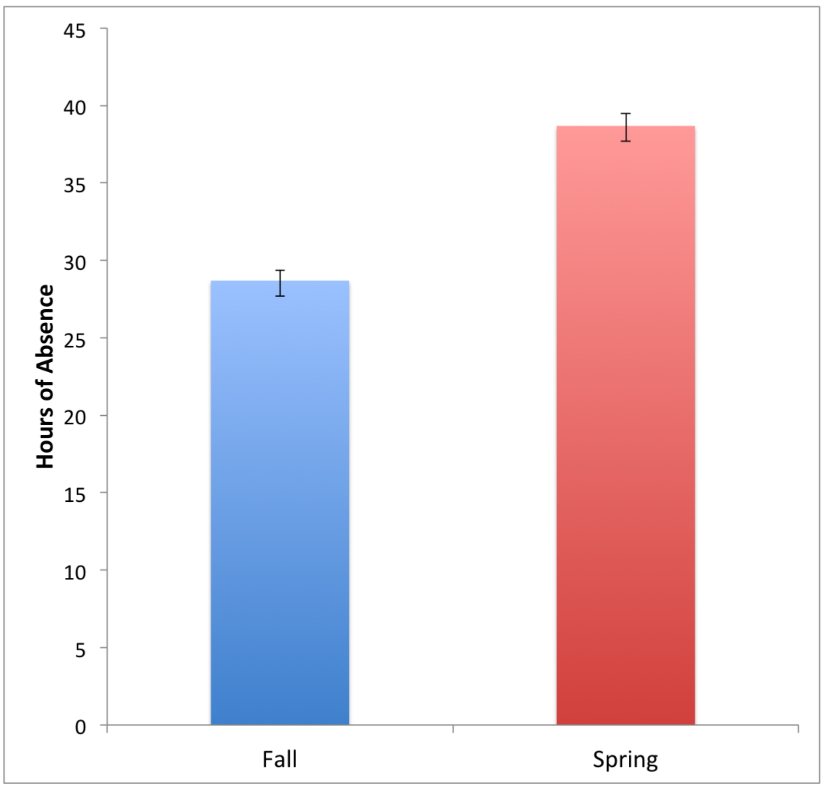

Figure 1.12: Common Trends Assumption

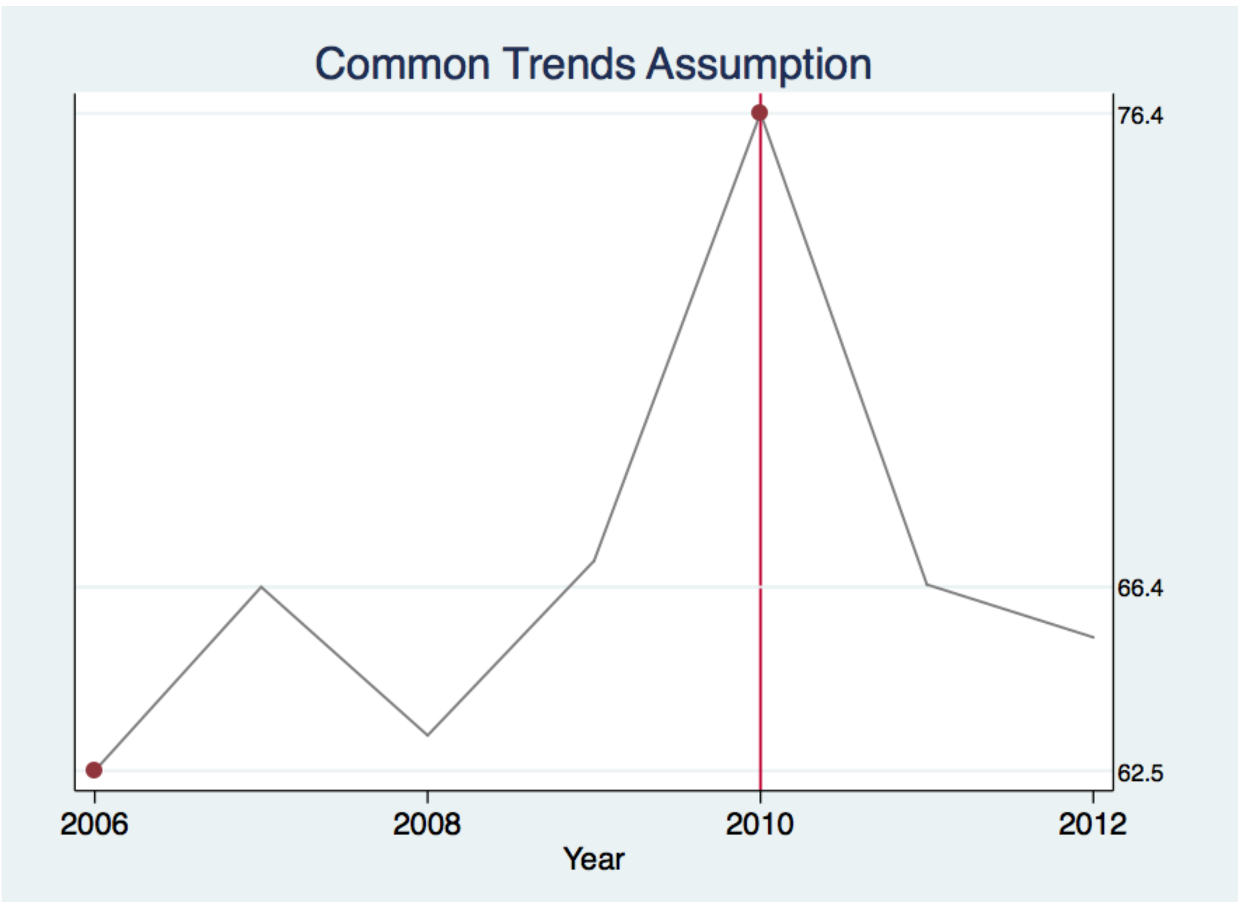

Figure 1.13: Placebo Test

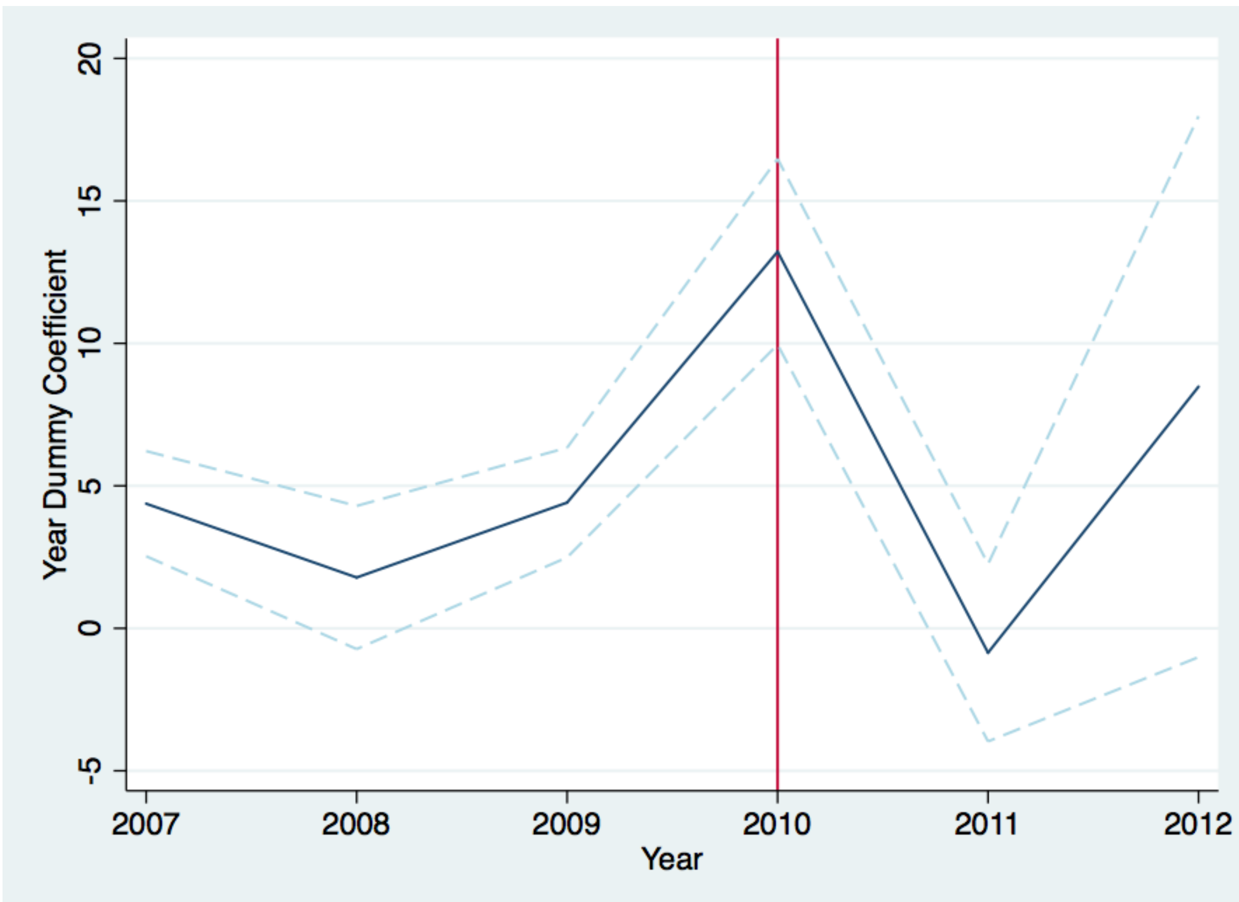

Table 1.4: RETURNS TO ABSENCES USING ATTENDANCE POLICY INSTRUMENT

	GPA			
	(1)	(2)	(3)	(4)
Absences	-0.065	-0.022	-0.020	0.041
	(0.002)***	(0.002)***	(0.002)***	(0.011)***
Instrument	None	None	None	Flu Reform
Grade FE	✓	✓	✓	✓
Class Controls	✓	✓	✓	✓
Lagged Score		✓	✓	✓
School FE			✓	✓

Sample: 58,923 obs (19,641 individuals). Scores are standardized by school and grade. Cluster-robust standard errors at the class level are reported in parentheses. Class controls include average lagged performance, and average lagged absences of class peers. Unit of Absences is tens of hours of classes. All specifications include a constant and student controls. *,**,*** denotes significance at the 10%,5% and 1% level respectively.

Table 1.5: REDUCED FORM, AND FIRST STAGE RESULTS

	Absences	GPA
	First Stage	**Reduced Form**
Flu Shock	1.121	0.046
	(0.067)***	(0.012)***
F-Statistic	277.23	

Note: Sample: 58,923 obs (19,641 individuals). Cluster-robust standard errors at the individual level are reported in parentheses. Specifications include, student controls lagged score, class controls, grade fixed effects, and school fixed effects. Class controls include average lagged performance, and average lagged absences of class peers. Unit of Absences is tens of hours of classes. *,**,*** denotes significance at the 10%,5% and 1% level respectively.

Table 1.6: ROBUSTNESS

	GPA							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Absences	-0.066	-0.069	-0.022	-0.021	-0.020	-0.021	0.026	0.019
	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.011)**	(0.010)*
Instrument	None	None	None	None	None	None	Flu Reform	Flu Reform
Grade FE	✓	✓	✓	✓	✓	✓	✓	✓
Class Controls	✓	✓	✓	✓	✓	✓	✓	✓
Linear Trend	✓	✓	✓	✓	✓	✓	✓	✓
Lagged Score			✓	✓	✓	✓	✓	✓
School FE					✓	✓	✓	✓
School-Specific Linear Trend		✓		✓		✓		✓

Sample: 58,923 obs (19,641 individuals). Scores are standardized by school and grade. Cluster-robust standard errors at the class level are reported in parentheses. Class controls include average lagged performance, and average lagged absences of class peers. Unit of Absences is tens of hours of classes. All specifications include a constant and student controls. *,**,*** denotes significance at the 10%,5% and 1% level respectively.

Table 1.7: REDUCED FORM, AND FIRST STAGE RESULTS

	Absences		GPA	
	First Stage		**Reduced Form**	
Flu Shock	1.222	1.121	0.031	0.023
	(0.077)***	(0.075)***	(0.014)**	(0.012)*
Linear Trend	✓	✓	✓	✓
School-Specific Linear Trend		✓		✓
F-Statistic	254.08	261.25		

Note: Sample: 58,923 obs (19,641 individuals). Cluster-robust standard errors at the individual level are reported in parentheses. Specifications include, student controls lagged score, class controls, grade fixed effects, and school fixed effects. Class controls include average lagged performance, and average lagged absences of class peers. Unit of Absences is tens of hours of classes. *,**,*** denotes significance at the 10%,5% and 1% level respectively.

Table 1.8: HETEROGENEOUS PROPENSITY TO SKIP CLASS: COGNITIVE ABILITY

	Total Absences		Excused Absences		Unexcused Absences	
	(1)	(2)	(3)	(4)	(5)	(6)
Flu	1.007	0.992	0.466	0.464	0.571	0.559
	(0.097)***	(0.094)***	(0.073)***	(0.071)***	(0.046)***	(0.046)***
Flu $\times$ ***Cognitive Ability***	0.192	0.194	0.220	0.222	-0.034	-0.033
	(0.150)	(0.149)	(0.115)*	(0.115)*	(0.067)	(0.066)
Cognitive Ability	-2.002	-2.005	-0.411	-0.413	-1.586	-1.587
	(0.055)***	(0.055)***	(0.042)***	(0.042)***	(0.025)***	(0.025)***
School FE	✓	✓	✓	✓	✓	✓
Grade FE	✓	✓	✓	✓	✓	✓
Class Controls	✓	✓	✓	✓	✓	✓
Linear Trend	✓		✓		✓	
School-Specific Linear Trend		✓		✓		✓
R^2	0.32	0.33	0.29	0.30	0.20	0.20

Note: sample: 58,923 obs (19,641 individuals). Cluster-robust standard errors at the classroom level are reported in parentheses. Class controls include average lagged performance, and average lagged absences of class peers. Unit of Absences is tens of hours of classes. All specifications include a constant and student controls for gender, age, and age squared. *,**,*** denotes significance at the 10%,5% and 1% level respectively.

Table 1.9: HETEROGENEOUS PROPENSITY TO SKIP CLASS: PEER QUALITY

	Total Absences		Excused		Unexcused	
	(1)	(2)	(3)	(4)	(5)	(6)
Flu	5.236	5.429	4.256	4.570	0.705	0.577
	(2.221)**	(2.182)**	(1.625)***	(1.605)***	(1.167)	(1.151)
Flu* $\times$ *Peer Quality	-1.494	-1.573	-1.342	-1.458	-0.037	0.004
	(0.830)*	(0.816)*	(0.608)**	(0.600)**	(0.434)	(0.428)
Peer Quality	0.204	0.456	0.049	0.258	0.063	0.104
	(0.413)	(0.419)	(0.306)	(0.312)	(0.230)	(0.226)
School FE	✓	✓	✓	✓	✓	✓
Grade FE	✓	✓	✓	✓	✓	✓
Class Controls	✓	✓	✓	✓	✓	✓
Lagged Score	✓	✓	✓	✓	✓	✓
Linear Trend	✓		✓		✓	
School-Specific Linear Trend		✓		✓		✓
R^2	0.26	0.27	0.27	0.28	0.14	0.14

Note: sample: 58,923 obs (19,641 individuals). Cluster-robust standard errors at the classroom level are reported in parentheses. Class controls average lagged absences of class peers. Unit of Absences is tens of hours of classes. All specifications include a constant and student controls for gender, age, and age squared. *,**,*** denotes significance at the 10%,5% and 1% level respectively.

Table 1.10: HETEROGENEOUS PROPENSITY TO SKIP CLASS: POSTCODE INCOME

	Total Absences		Excused		Unexcused	
	(1)	(2)	(3)	(4)	(5)	(6)
Flu	10.838	11.013	7.867	8.552	1.708	1.067
	(3.189)***	(3.381)***	(2.682)***	(2.815)***	(1.407)	(1.508)
Flu $\times$ ***Log(Income)***	-0.965	-0.984	-0.724	-0.792	-0.111	-0.048
	(0.320)***	(0.340)***	(0.269)***	(0.283)***	(0.142)	(0.152)
School FE	✓	✓	✓	✓	✓	✓
Grade FE	✓	✓	✓	✓	✓	✓
Class Controls	✓	✓	✓	✓	✓	✓
Lagged Score	✓	✓	✓	✓	✓	✓
Linear Trend	✓		✓		✓	
School-Specific Linear Trend		✓		✓		✓
R^2	0.26	0.27	0.27	0.28	0.14	0.14

Note: sample: 58,923 obs (19,641 individuals). Cluster-robust standard errors at the classroom level are reported in parentheses. Class controls average lagged absences of class peers. Unit of Absences is tens of hours of classes. All specifications include a constant and student controls for gender, age, and age squared. *,**,*** denotes significance at the 10%,5% and 1% level respectively.

Table 1.11: HETEROGENEOUS RETURNS TO ABSENCES: COGNITIVE ABILITY

Dependent Variable: GPA		
	(1)	(2)
Absences	-0.002	-0.002
	(0.016)	(0.015)
Absences* × *Cognitive Ability	0.004	0.004
	(0.002)***	(0.002)***
Cognitive Ability	2.154	2.156
	(0.106)***	(0.105)***
School FE	✓	✓
Grade FE	✓	✓
Class Controls	✓	✓
Linear Trend	✓	
School-Specific Linear Trend		✓
$Cragg - Donald\ F\ statistic$	556.717	545.294

Note: sample: 58,923 obs (19,641 individuals). Cluster-robust standard errors at the classroom level are reported in parentheses. Class controls include average lagged performance, and average lagged absences of class peers. Unit of Absences is tens of hours of classes. All specifications include a constant and student controls for gender, age, and age squared. *,**,*** denotes significance at the 10%,5% and 1% level respectively.

Table 1.12: HETEROGENEOUS RETURNS TO ABSENCES: PEER QUALITY

Dependent Variable: GPA		
	(1)	(2)
Absences	-0.010	-0.276
	(0.424)	(0.459)
Absences* × *Peer Quality	0.013	0.110
	(0.159)	(0.172)
Peer Quality	-0.474	-1.275
	(1.149)	(1.234)
School FE	✓	✓
Grade FE	✓	✓
Class Controls	✓	✓
Lagged Score	✓	✓
Linear Trend	✓	
School-Specific Linear Trend		✓
$Cragg - Donald\ F\ statistic$	204.957	170.676

Note: sample: 58,923 obs (19,641 individuals). Cluster-robust standard errors at the classroom level are reported in parentheses. Class controls include average lagged performance, and average lagged absences of class peers. Unit of Absences is tens of hours of classes. All specifications include a constant and student controls for gender, age, and age squared. *,**,*** denotes significance at the 10%,5% and 1% level respectively.

Table 1.13: HETEROGENEOUS RETURNS TO ABSENCES: POSTCODE INCOME

Dependent Variable: GPA		
	(1)	(2)
Absences	0.120	-0.400
	(0.479)	(1.272)
Absences $\times$ ***Log(Income)***	-0.010	0.042
	(0.048)	(0.128)
School FE	✓	✓
Grade FE	✓	✓
Class Controls	✓	✓
Lagged Score	✓	✓
Linear Trend	✓	
School-Specific Linear Trend		✓
$Cragg - Donald\ F\ statistic$	293.669	43.371

Note: sample: 58,923 obs (19,641 individuals). Cluster-robust standard errors at the classroom level are reported in parentheses. Class controls include average lagged performance, and average lagged absences of class peers. Unit of Absences is tens of hours of classes. All specifications include a constant and student controls for gender, age, and age squared. *,**,*** denotes significance at the 10%,5% and 1% level respectively.

Table 1.14: HETEROGENEOUS RETURNS TO ABSENCES: SUBJECTS

	Greek		**Mathematics**	
	(1)	(2)	(3)	(4)
Absences	-0.033	-0.038	0.026	0.029
	(0.016)**	(0 .016)**	(0.014)*	(0.014)**
First Stage F-Statistic	236.19	256.92	236.58	256.26
Grade FE	✓	✓	✓	✓
Class Controls	✓	✓	✓	✓
Lagged Score	✓	✓	✓	✓
School FE	✓	✓	✓	✓
Linear Trend	✓		✓	
School-Specific Linear Trend		✓		✓

Sample: 58,923 obs (19,641 individuals). Scores are standardized by school, and grade. Cluster-robust standard errors at the class level are reported in parentheses. Class controls include average lagged performance, and average lagged absences of class peers. Unit of Absences is tens of hours of classes. All specifications include a constant and student controls. *,**,*** denotes significance at the 10%,5% and 1% level respectively.

Table 1.15: HETEROGENEOUS RETURNS TO ABSENCES: TRACK

	Track Average Score					
	Classics		IT		Science	
	(1)	(2)	(3)	(4)	(5)	(6)
Absences	-0.012	-0.006	0.039	0.042	0.164	0.164
	(0.012)	(0.013)	(0.015)**	(0.016)**	(0.035)***	(0.035)***
First Stage F-Statistic	168.66	138.89	132.44	114.15	35.48	33.70
Class Controls	✓	✓	✓	✓	✓	✓
Lagged Score	✓	✓	✓	✓	✓	✓
School FE	✓	✓	✓	✓	✓	✓
Linear Trend	✓		✓		✓	
School-Specific Linear Trend		✓		✓		✓
$\#\ of\ Students$	7,750		8,809		2,384	

Data: Panel for 18,943 individuals. Scores are standardized by school, grade, and track. Cluster-robust standard errors at the class level are reported in parentheses. Class controls include average lagged performance, and average lagged absences of class peers. Unit of Absences is tens of hours of classes. All specifications include a constant and student controls. *,**,*** denotes significance at the 10%,5% and 1% level respectively.

CHAPTER 2

THE EFFECT OF FEEDBACK INFORMATION ON SHORT AND LONG TERM OUTCOMES

2.1 Introduction

Knowing how one's characteristics compare to those of others is important in many settings of decision making. Humans are social beings, and it is natural to make comparisons in terms of characteristics and abilities in given tasks (Festinger 1954). The theory of social comparison is about "our quest to know ourselves, about the search for self-relevant information and how people gain self knowledge and discover reality about themselves" (Mettee and Smith 1977). One measure that provides social comparison information is ordinal rank in addition to relative or absolute information. In the workplace, recent evidence suggests that an individual is influenced not only by his relative or absolute income but also by the ranked-ordered position of his wage within a reference group. So one's ordinal rank is used when people make comparisons with others (Brown, Gardner, Oswald, and Qian 2008, Card, Mas, Moretti, and Saez 2012). Social comparison is also an indispensable part of bonding among adolescents. In education, it is rare for teachers or principals to provide relative performance information to students. Thus, there is little understanding of the effects of providing information on students' ranks, because there is limited variation in the nature of such information that students receive-thus precluding causal analyses. However, knowing one's ordinal rank may affect education investment decisions and, hence, future academic and labor market outcomes.

Improving pupil attainments has been and continues to be an important issue for policy makers and academics alike. In an effort to improve students' grades, education policies have focused on improving a wide array of school inputs, among them, reducing class size (Angrist and Lavy 1999a, Krueger 1999), improving the quality of teachers (Chetty, Friedman, and Rockoff 2014a, Rothstein 2010, Aaronson, Barrow, and Sander 2007), extending the term length (Card and Krueger 1992) and improving the quality of a student's peer group (Lavy, Silva, and Weindhardt 2012, Zimmerman 2003a, Hoxby 2000b). Disclosing social comparison information (such as rankings), and manipulating the availability of this information would be significantly less costly than the above-mentioned interventions.

This paper presents empirical evidence about whether providing high school students with information about their rank on externally marked, high-stake exams affects future performance in similar exams. Our analysis relies on the fact that different cohorts are subject to different policies regarding the provision of social comparison information.

We exploit a large-scale, natural experiment that took place in 2005, when Greece adopted policies that altered the education testing regime, and eliminated information on rank that has been available to students nationwide. Until 2005, all students had to take national exams in two consecutive grades: one year before high-school graduation (eleventh grade), and the year of high-school graduation (twelfth grade). Results for all students were published nationally. From the results, students could calculate their rank within the school and across the nation, thus enabling comparison within the school and nationwide.

After 2005, the penultimate-year (eleventh grade) national exams were abolished and replaced with school-level exams. As a result, penultimate-year students take exams on the same subjects as before, but only receive report cards with their own exam results; they no longer receive information about their penultimate-year *relative* performance in relation to others in their school or across the nation. These cohorts -like the previous ones- continue to take twelfth-grade, national exams that will determine their post-secondary placement. However, they no longer receive information regarding previous relative performance in similar exams (national exams).

We define a feedback regime as receiving information regarding one's performance in comparison to peers in school and nationwide. In the feedback regime that existed until 2005, eleventh-grade students could compare themselves to others allowing for social comparison. Thus, we find the effect of the feedback on students' short-term (subsequent exam performance) and long-term outcomes (repetition of national exams one year after graduation, university placement and expected annual earnings) by comparing students of the same prior performance (tenth-grade performance) across regimes. In other words, we compare students who do receive information on rank in the eleventh grade and those who do not receive this information. We then use two sets of subjects. The main analysis is conducted using those subjects on which students receive the relative performance information in the eleventh grade. The second group of subjects, the counterfactual group, is a group of subjects on which students do not receive any relative performance information in the eleventh grade in either regime. Using new data on school performance, school quality and national exams for university admission, we test the hypothesis that students' final-year exam (twelfth-grade) performance is independent of the feedback regime.

When students knew their performance in the eleventh-year exams, they could "translate" their hours of effort into their exam result. For a given level of effort exerted by their peers, they discover how much effort they need to put into the final-year exam to rank accordingly. Knowing their relative performance could affect investment decisions such as the amount of effort students decide to exert in their final year of school. Students' performance in the final-year (twelfth-grade) national exams is the most important determinant for university admission in this setting.

Our first finding is that high-achieving students perform better in the final-year national (externally graded) exams when they receive feedback. Providing relative performance information the year before, improves the next period's exam performance of the better students by 0.2 standard deviations and their relative national rank by 4-6 percentiles. This is of comparable magnitude to being taught by a teacher 1.5-2 standard deviations above the average (Chetty et al. 2014a, Hanushek, Kain, and Rivkin 2005), or to reducing the class size by 15 percent. (Angrist and Lavy 1999a, Krueger 1999). Additionally, we find evidence that the performance of students in the lower percentiles deteriorates when feedback is provided. In particular, their consecutive-year performance declines by 0.3 standard deviations, and their national rank decreases by 6-8 percentiles.

Our second finding reports the responses of males and females to feedback at different parts of the ability distribution. High-achieving students of both genders respond positively to positive feedback, and low-achieving students of both genders respond negatively to negative feedback. However, females seem to be considerably more sensitive to feedback at all parts of the ability distribution compared to males. Our results are consistent with the existing literature regarding the gender differential response to performance due to initial different levels of self-confidence (McCarty 1986), or competitiveness of the exam (Gneezy, Niederle, and Rustichini 2003).

Our third finding is that the provision of feedback changes the matching of students to university departments. First, we rank all university departments (programs) based on selectiveness and we construct a program list from the most-selective programs (e.g. engineering and medicine) and to the least-selective (e.g. geo-technology and environmental studies). We find that feedback provision corresponds to high-achieving students moving up the program selectiveness ladder by 30 positions, which is 0.15 of a standard deviation. On the other hand, low-achieving students move down the program selectiveness ladder by 35 positions, which is 0.18 of a standard deviation. Using the national Hellenic Labor Force Survey information, we find the annual earnings of older people in each occupation, and we map them to university departments. When the social comparison information is disclosed, we find that high-achieving students experience an increase in expected earnings by 0.13 standard deviations. Further, feedback provision for low-achieving students imposes a decrease in expected earnings of 0.23 standard deviations.

Additionally, we find evidence to suggest that feedback encourages students from low-income neighborhoods to enroll in university and, as such, alters the socio-economic composition of students who are admitted to the top programs. More students from low-income neighborhoods gain admission to the most selective programs with the highest expected earnings after graduation (such as engineering and law), when feedback information is provided. This implies that feedback encourages social elevation for students from low income families.

This paper makes two main contributions. First, this is the first large-scale study that documents the long-term effects of providing relative performance information in an educational setting. In particular, we document the direction and size of the effect of feedback on long-term outcomes such as repetition rates for the national exams, students' post-secondary placement, and expected earnings. We contribute to the literature by providing evidence that knowing one's ranking within the senior high school or nationwide has long-lasting effects, and changes an individual's career path.

Secondly, we explore how information transparency-particularly about a student's rank-affects educational outcomes. We accomplish this by making use of a unique information treatment. Disclosing information about ranks is not a standard practice for teachers or principals. Thus, the information treatment that we study in this paper is rare. We exploit a special setting in which high-school students receive explicit information about their relative position in at least two reference groups: school and country. Although students may generally observe their own perspicacity, they do not generally observe everyone's performance in the school and the country to deduce their ranking. Thus, we are able to separately identify the effect of knowing someone's ranking in each of these two groups.

This study has important policy implications. First, we provide evidence that a low-cost instrument -such as providing information on one's rank- has the potential to affect students' educational achievements. Our estimates of impact are at the lower end of those from the current literature on improving school inputs. Nonetheless, all the interventions studied so far (improving teacher quality, reducing class size, enhancing peer-group quality etc.) are significantly more costly than manipulating the availability of social comparison information. Thus, information on rank can be considered a new factor in the education production function[1].

Second, our findings imply that when the relative performance information is disclosed, it can be an important, additional factor in terms of the school choice. Our findings imply that being in a school with higher-achieving peers might not always be optimal for students. That is, students benefit from going to schools in which they are among the high-performing students (i.e. schools with more students with relatively lower performance levels). Making a school choice on the basis of rank only is unlikely to be correct, given that there are many other factors in the education production function.

In the recent years, the economic literature has shown increasing interest in the effect of feedback on feedback on exam performance.[2] Bandiera, Larcinese, and Rasul 2015 examine the effect of feedback information on students' future absolute performance using data for university students registered to departments with different feedback policies. In that study, feedback is defined

[1]Other determinants of the education production function that have been studied include: studies on class size (Angrist and Lavy 1999a, Krueger 1999, Hoxby 2000c), teachers' training and certification (Angrist and Lavy 2001, Kane, Rockoff, and Staiger 2008), quality of teacher (Rockoff 2004, Rivkin, Hanushek, and Kain 2005a), tracking (Duflo, Dupas, and Kremer 2011), peer effects (Hoxby 2000b, Lavy, Paserman, and Schlosser 2012), non-cognitive skills (Heckman, Stixrud, and Urzua 2006), classroom instructional time (Lavy 2015).

[2]The relative feedback information has been studied in the tournament literature. Some studies find that relative performance information has a positive effect for all participants in both tournaments and piece-rate payment schemes (Hannan, Krishnan, and Newman 2008). On the other hand, some other studies find mixed results. Barankay 2012 uses data on furniture sellers' effort, and finds that feedback has negative effects on the low-performing employees.)

as the knowledge of someone's absolute performance in the midterm exam in period one, and before students exert effort on their essay in period two. The authors find that the effect of feedback is positive for all students, and more pronounced for more-able students. Their study refers to feedback involving one's own absolute performance. Our study refers to the provision of feedback regarding relative performance.

The paper most closely related to ours is a study by Azmat and Iriberri 2010. The authors examine the effect of relative performance feedback on students' future absolute performance. They exploit a natural experiment that took place in a high school, in which for one year only, students received information about the average class score in addition to information about their own performance. Their findings suggest that feedback improves the performance of all students in the subsequent test. They do not find differential effects by gender along the ability distribution. A key difference between their paper and ours in the sample size. They use a small sample of one high school; by contrast, we use what is in many dimensions a nationally representative sample of 134 senior high schools. Another important difference is that Azmat and Iriberri 2010 investigate the effect of providing information about someone's relative position within the class only. We contribute to the literature by exploiting an explicit information treatment in which the social comparison information refers to reference groups broader than the class, i.e. the school and the nation.

More recently, Murphy and Weinhardt 2014 examine the effect of one's primary school ordinal rank on future exam performance. In their setting, students figure out their rank within their class from repeated interaction with their classmates. They find that being highly ranked in primary school has large and robust effects on secondary school achievement, with boys being more affected than girls. Our setting differs in that students receive explicit information regarding their rank within the school and nationwide, which facilitates the policy recommendations.

An interesting question is whether the effects are driven by students, parents or teachers or some combination of them. It is almost impossible to disentangle whether the effects are coming from the students or the parents. However, we can rule out the two mechanisms; neither sorting into schools by parents nor sorting into classes by teachers explains the effects. We discuss several possible mechanisms that cannot be fully excluded, and, thus could explain our findings. They are: 1) priors, 2) information about school quality, 3) parental investment, 4) practice and 5) learning about one's ability.

The paper is organized as follows. Section 2 provides a brief description of the institutional setting and the data. Section 3 sets out our empirical strategy. Section 4 presents the main results on short and long-term outcomes and discusses heterogeneous feedback effects by prior performance and gender. Section 5 discusses the potential mechanisms. Section 6 discusses the threats to identification and reports further robustness checks. In Section 7 we conclude and discuss policy implications.

2.2 Institutional Setting and Data

2.2.1 Institutional Setting

All universities in Greece are public and the Ministry of Education manages the admission procedure. Access to tertiary education is based on the "admission grade". The admission grade in both regimes is a weighted average of the grades a student gets in the national exams (70% weight) and the school grades (30% weight). National exams for specific subjects take place on specific dates every year. The questions are the same for all students and the exams are externally marked. The school grade for every subject is the average of the term grades. Only final-year students can participate in the university admission procedure. All students are examined on five general or core subjects, plus several "speciality" elective subjects chosen at the beginning of the twelfth year.

First, students take the final-year exams and then their admission grades are announced. Then students apply by submitting to the Ministry of Education a list, in order of preference, of university departments to which they would like to be admitted in that year. Admission is made to a specific university department. The student's ordering of university departments is crucial: once a student gains admission to a university department in a higher place in his preference list, he cannot be admitted to any departments below that position. This means that students have to be very careful in constructing their preference list. The only way a student can avoid the university admission procedure is by not submitting a list of preferences. Then, each department admits the best students who have included this department in their preference list. All students are compared to each other according to their admission grades and every successful candidate is admitted to the first department in his list where there is an available place, and every student with a higher admission grade has already been allocated. The rest of the students are denied admission for that year.

At the end of this process, every department announces the grade of the last student it admitted in that year. This grade is considered to be the "cut-off grade" in that year. More selective/prestigious departments have higher cut-off grades and students are aware of the cut-off grades of the previous years when they construct their preference lists. The ranking of university departments according to their cut-off grades appears to stay largely unchanged, year after year, and this represents the students' evaluations for these departments.

The admission grade of a student in the non-feedback regime depends entirely on students' performance in the twelfth grade.[3] In the feedback regime, students' performance in the eleventh grade could take some weight (30%) in the calculation of the admission grade, but only if their

[3] It is a combination of the national exams (70%) and the school grades (30%).

eleventh grade performance exceeds that of their final year exams.[4] Again, the overall performance of a student in each grade is a combination of the national exams (70%) and the school grades (30%). The eleventh-grade material in not included/tested in the twelfth-grade exams. The results of the penultimate-year exams are not used in any other way in the university admission procedure. So, students have incentives to perform well in the eleventh-grade national exams, but that performance is not sufficient to secure a specific university placement. Given that the number of seats is pre-determined, a specific score will not guarantee admission to a specific university department, because demand for that university department also comes into play.

2.2.2 How does feedback work?

Knowing one's own relative performance might affect the amount of effort a student exerts with regard to a certain objective. In the context of our study, the student's objective is to maximize his or her score and/or rank at the end of high school.

Consider a student in the treated group. In the world of this experiment, students compete with each other over access to a limited number of university places. At the end of the penultimate year, students take standardized exams in some subjects with external examiners and at least two anonymous external graders per subject.

Then two mechanisms are in action. First, everyone's results within the school become public knowledge: the names and detailed grades are displayed at the entrance of every school. This provides students with information about how well they can do given a specific level of effort, when national exams come around again. This means that students could calculate their distance from the school's average score, and their relative position within their school. Second, the names, details about national exam scores, and the cohort's average national exam score are published in the newspaper. This means that each student could calculate her distance from the national cohort's average score and derive her relative national rank.

We believe that students in the feedback regime calculate their eleventh grade rank within the school and nationally given the importance of their performance in the senior year exams. Knowing a student's national rank provides them with information about the competition in that year. Each year the newspaper reports the following: cohort's average national exam score, the cohort's minimum and maximum score, the score that corresponds to each decile and comparisons with last year's statistics. For each student the following is reported: student's first name, surname and father's name, score given by the first and the second examiners (Figure 2.3 and 2.4). This is published separately for each subject. The score given by each examiner ranges from 0 to 100. If the difference between the score given by the first and the second examiners is not greater than 13/100, then the final score is the average score between the scores given by the first and the second

[4] In this case, the overall performance of a student in the twelfth grade takes a weight of 70% and the overall performance of a student in the eleventh grade takes a weight of 30% in the calculation of the admission grade.

examiners. Otherwise, the final score is the average between the highest two scores given by any examiners. The raw final score used a 1-to-20 scale that we transformed into z-scores to facilitate the interpretation of the results.

Students use this information to calculate their national rank. Given that the names are alphabetically sorted, calculating a student's rank using even a single newspaper's scores is already a good indicator of a student's national rank. Calculating the school rank is much easier given that the average school cohort consists of 79 students.

Consider a student in the control group. During the penultimate year of senior high school, he chooses an effort level to prepare for his exams, which are now given only at the school level. Within the school, teachers coordinate to cover the same material, and usually give the same exam questions. Before the summer break in the penultimate year, our student takes exams on the same five subjects, and receives a written report from school with his own grades. When he reaches twelfth grade, he has access to the same material, study guides and past exam papers as any student in the treated group. However, he is unaware of how his schoolmates and his cohort did relative to him in the penultimate year final exams. Table 3 reports the summary statistics of the variables of interest across the two regimes. Some of the differences seem to be significantly different from zero but they are either very small or economically non-meaningful. The exact timing is presented in Figure 2.

2.2.3 Data Collection

To study the effect of disclosing rankings, we need a prior measure of performance that is not affected by the feedback provision, i.e. students' tenth-grade performance. Data on students' performance in the tenth grade are not centrally collected[5], and can only be found in the school archives. We visited 134 senior high schools across the country and constructed a database of student performance in every subject throughout senior high school. Our novel dataset combines information from various sources:

1. We obtained administrative data from the Hellenic Ministry of Education regarding the performance of all students in the twelfth-grade national exams from 2003 to 2009. This dataset contains student level information about gender, national and school exam results in each subject nationally examined in twelfth grade, the senior high school attended, year of birth, graduation year from senior high school, and speciality subjects chosen at the beginning of twelfth grade. It also contains university admission-related information, such as the university department where each student gained admission, number of applications made to university departments, and the reported ordinal preference position of the admitted university department in the student's preference list. The dataset refers to the period 2003-2009,

[5] The Ministry of Education collects data on students' scores that are used in the calculation of the admission grade.

and gives us information about 435,589 students.

2. Because the Ministry does not collect information on students' tenth-grade performance, we collected this information directly from the schools.[6] More specifically, we have physically visited and collected data from more than 147 public, experimental[7] and private schools from cities and the countryside. The final sample includes 134 schools which corresponds to 10 % of the school population. We exclude evening schools[8] from our analysis because they differ[9] in many aspects from the other types of schools.[10]This dataset includes the following information: year of birth, indicators for gender, indicators for class, graduation year, school and/or national exam results in the tenth, eleventh and twelfth grade in all subjects, speciality chosen at the beginning of the eleventh and twelfth grade and a unique, individual student identification that stays the same throughout senior high school. We have had short interviews with the principal of every school in our sample to find out about any effects potentially affecting our outcomes of interest. Inter alia, principals were asked about the size and history of the school, facilities, attrition and teacher quality.

   We match the twelfth-grade, school-level data with the administrative data using the following combination of information: year of birth, gender, high school attended, graduation year, speciality chosen at the beginning of twelfth grade, and school as well as national exam scores in each subject examined at the national level. The matching between the dataset provided by the Ministry of Education and the school datasets was very satisfactory[11] providing us with a complete senior high-school performance history for 45,746 students, which is our sample size.

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[6]The tenth-grade performance data are recorded in each school's archives either in their computers or in their history books. In most schools the data for all the years were extracted from their computers. There were cases- especially for the data referring to the first years of our sample period- where we photocopied pages from the history books in schools' storage area.

[7]Experimental schools are public schools where admission in these schools is based on a randomized lottery.

[8]Which are public schools that offer evening lessons in order to target employed students.

[9]University cut-offs differ for students graduating from evening schools compared to any other type of school. Including these schools in the analysis provides similar results, only varying at the second decimal point. Contact authors for further results

[10]We also exclude schools that had at least one year school cohort size smaller than ten students because these small schools may be atypical in some dimensions. Results including those schools are very similar. Contact authors for further results.

[11] 92 % of students were matched, with only 8 % missing due to the lack of values either in the school level data or the administrative Ministry level data.

3. We obtained average household income information for 2009 for every postcode in the country from the Ministry of Economy and Finance. We employ this as a proxy for neighborhood income.

4. We obtained postcode data on urban density information from the Ministry of Internal Affairs. Urban areas are those with more than 20,000 inhabitants.

5. We obtained the Labor Force Survey data for the year 2003 from the National Statistical Authority. We use quarterly data to create a variable that maps college occupations into annual earnings.[12] We do that if respondent's reported education is in the same field as her actual occupation in 2003. Respondents report their occupations with high precision.[13] The earnings data are grouped into ten bins indicating the ten national deciles with the highest frequency. We use the lowest bound of each bin[14] to construct a variable that measures minimum expected annual earnings for each occupation.

Every school follows the same curriculum, and students are assigned to public schools based on a school district system. This school district system assigns students to schools based on geographical distance. Students are alphabetically assigned to classes in tenth grade and do not change class throughout senior high school. Moreover, teachers are allocated to public schools based on geographical criteria and no quality criteria are taken into consideration in the process. Figure 1 presents the geographic position of each school included in the sample. The density of the school population in Athens is 32%- thus, many of the schools in our sample are located in Athens.

Table 1 presents descriptive statistics about the available variables in the sample in the twelfth grade. The variable ”internal migration” takes the value one if the district of university department to which the student is admitted is different from the district of residence; the latter being proxied by the school district. Moreover, the variable “early enrollment” takes the value of one if the student enrols in the first grade before the age of six.[15]Interestingly, on average, 82 % of students gain admission to at least one university department. Given that there are no fixed cut-offs, if there is not much demand for a particular university department, the cut-off grade in that year is very low.

Table 2 reports the mean characteristics of the schools in our sample and the whole school population, and, thus , allows us to investigate whether our sample is representative. There are some

[12] We also map college fields to occupations.

[13]209 classified occupations are reported and respondent have to indicate which one is closest to their actual occupations.

[14]Multiplied by 12 months.

[15] According to the law, this happens if the student is born in the first quarter of the calendar year.

variables for which there is a statistically significant difference between the 134 sample schools and the rest of the school population, and these differences are mainly related to the sampling methods that we used.[16] So, though the sample may not be fully representative of national responses, but it nonetheless looks very similar.

2.2.4 Test Scores

Our prior measure of performance is based on the overall students' performance in the tenth grade (GPA). The tenth grade GPA takes into account students' tenth grade performance in thirteen subjects. The performance in each subject is a weighted average of the final school-exam result and the performance of the student during the school year. Teachers receive guidance on how to mark the final tenth-grade school exam and test scores are not curved. We use the within school rank of each student based on the tenth grade GPA as a prior measure of performance.

Our main outcome variable is a student's twelfth grade rank in two reference groups (the school and nationally). These outcome variables -the within school and the national rank- take into account students' twelfth grade performance in five core-education subjects. Students take exams at the end of the twelfth grade in these core-education subjects. A student's performance in these five subjects is the most important determinant for the calculation of the high-school graduation grade under both regimes. Before 2005, these five subjects were all examined at a national level. From 2005 onwards, two subjects are examined at a national level whereas the other three subjects are examined at a school level. This change in the number of subjects examined at a national and school level happened in the same year as the abolition of feedback. We do various robustness checks to examine if this change affects our results. In particular, we use various outcome variables (the rank in each subject separately; the average rank in those subjects examined at the national level; or the average rank in the five core-education subjects) and the estimated effects follow the same patterns. We call the core-education subjects "incentivized", because performance in these subjects is taken into account in the calculation of the admission grade.

All schools in the sample offer three academic tracks in the twelfth grade. Each student has to choose the academic track that is relevant to the post-secondary degree they desire to pursue. Each track offers different subjects. Depending on the track students choose, they take national exams in four track-specific subjects in both regimes [17] We do not include the test scores in these four subjects in the main analysis because the choice of track is based on endogenous criteria, i.e.their perceived differential ability or preferences for a particular degree after high school graduation.

[16]i.e. the share of schools in Athens in our sample is higher than the share of schools in Athens in the population. Furthermore, the share of private schools in the sample is 4 % smaller than the share of private schools in the population and the share of experimental schools in the sample is 4 % higher than the share of experimental schools in the population.

[17] These four subjects differ from the one track to the other. The Tracks are: Classics, Exact Science and Information Technology.

Robustness checks show that the results remain almost unchanged when the track-specific subjects are taken into account.

In addition to the core-education subjects and the track-specific subjects, students take compulsory within-school exams in three subjects (Sociology, Religion and Modern Greek Literature) in both grades; eleventh and twelfth. Students take school exams at the end of the eleventh grade and each student receives a report card. This report card shows each student's own performance in these exams without providing information about the class or school average score. In the twelfth grade, students are examined again on these subjects without having previously received any relative performance information in these three subjects. We call these subjects "non-incentivized" because students' performance in these subjects is not taken into account in the calculation of the university admission grade in any of the regimes. Students take these exams in both regimes. We use these subjects as the main counterfactual group.

In our analysis, we use the rankings instead of absolute scores for a couple of reasons. First, using the tenth-grade ranking allows us to make comparisons across cohorts and across schools. Notice that we do not observe the different feedback policies in the same year. Thus, we use the within-school rank of a student to compare students who are exposed to different peer groups and teachers. Second, a given twelfth-grade national exam score does not represent the same ability level in different years. However, it is important to make sure that students of the same ability obtain the same rank in different years. The comparison of students' absolute scores across cohorts would be problematic, if the difficulty of the exam changes from one year to another. Additionally, calculating the ranking of a student in each subject takes into account the potential difference in the difficulty of the exam from one subject to another. Thus the ranking allows us to compare a students' performance across different subjects. Also note that any school grade inflation that might occur in the tenth grade does not affect our prior performance measure (tenth grade GPA). Grade inflation would make the teacher more lenient in the overall grading procedure, which implies that the ranking of the students remains unaffected. The national exams in twelfth grade are externally graded. As a result, the teacher in a student's school has no way to affect their national exam final scores. Furthermore, the national exam procedure does not receive any grade curving.

2.3 Empirical Strategy

This section identifies the effect of relative performance information on students' senior year exam performance. First, we define our measures of rank. Second, we identify if there is an effect. Because we use as an outcome variable the rank in the twelfth grade, the effect is -if anything- of a distributional nature. Finally, we discuss the empirical method to identify the effect of feedback on students' relative final-year performance.

2.3.1 Calculation of the rank

We use the following normalization in order to calculate our measure of prior performance that allows comparisons of students in tenth grade across schools and cohorts:

$$Rank_{10isc} = \frac{n_{isc}-1}{N_{sc}-1}$$

where n_{isc} is the ordinal ranking of student *i* within school *s* in cohort *c* in tenth grade[18] and is increasing in GPA and N_{sc} is the school cohort size of school *s* in cohort *c*. The higher the $Rank_{10isc}$, the higher the ranking of student i in tenth grade in his school s and cohort c. Moreover $Rank_{10isc}$ is bounded between between 0 and 1, with the lowest ranked pupil in each school having $R_{10isc} = 0$. For example, in a school consisting of 100 students ($N_{sc} = 100$), the student with the fifth highest GPA ($n_{isc} = 95$) will have $Rank_{10isc} = 0.95$ while the student with the first lowest GPA will have ($n_{isc} = 5$) so his rank will become $Rank_{10isc} = 0.05$.

The ranks of the student within his school in the twelfth grade and nationwide are calculated using the following normalisations:

$$Rank - school_{12isc} = \frac{k_{isc}-1}{K_{cs}-1}$$

$$Rank - nationwide_{12ic} = \frac{r_{ic}-1}{R_{c}-1}$$

Where k_{isc} is the ordinal ranking of student *i* in school *s* in cohort *c* in twelfth grade and is increasing in the national exam grade. K_{cs} is the cohort size *c* in school *s*. The $Rank - school_{12isc}$ is projected into the [0,1] interval and the lowest ranked pupil in each school cohort has $Rank - school_{12isc}$=0. Notice that there are five subjects, so we first find the ordinal rank of the student based on the average in the five scores, and then we normalise it using the above formula . $Rank - nationwide_{12ic}$ is calculated in a similar way but is independent of the school the student attends. So both $Rank - school_{12isc}$ and $Rank - nationwide_{12ic}$ are calculated based on the twelfth grade national exams in the incentivized subjects, but they measure relative performance in the school and the country respectively. For example, in a cohort with 50,061 students (R_c=50,061), the student with the tenth highest twelfth grade national exam score (r_{ic}=50,051) will have a national rank of $Rank - nationwide_{12ic}$=0.999. If the same student has 78 schoolmates (K_{cs}=79) and he has the second highest score within his school in that cohort (k_{isc}=77), then the school rank of this student becomes $Rank - school_{12isc}$=0.974.

2.3.2 Identifying the effect

Figure 3 shows the average rank nationwide of each performance group in the twelfth grade exams, conditional on students' prior performance. Cohorts up to 2005 have received the relative

[18] Based on the average of the thirteen subjects, ie.the tenth grade GPA.

performance information. We observe that the lines are parallel in the treatment period (cohorts 2003, 2004 and 2005). This means that the time trends for each quintile of prior performance follow a similar pattern from year to year. Identification is achieved through a difference approach for each prior performance group. The 2006 cohort is the first cohort affected by the abolition of the relative performance information. We observe that from 2005 to 2006 the slopes of the time trends change, meaning that the treatment affected students in all performance groups considerably except the middle quintile, which remained unchanged. In particular, the top quintile achieved a higher average rank nationwide in the twelfth grade when feedback was provided compared to the period after 2006. The opposite applies to the bottom quintile where students end up lower in the distribution of twelfth grade rank when they are aware of their previous relative performance compared to the period after 2006.

Another important observation is that the slopes remain relatively stable after 2006, which is the first affected cohort. So, the change in the slope of the time trends between 2005 and 2006 can be attributed to the abolition of the relative performance information. We produce this figure using students' rank nationwide (Figure 3) and rank within the school (Figure 4). Their measures on rank are derived using the average rank in the general or core subjects.

2.3.3 Method

We adapt two strategies to quantify the effect of feedback provision on future performance.

First, we use the following specification to estimate the effect of feedback information on students' later rank, conditional on their prior performance.

$$Rank-nationwide_{12ic} = \alpha + \beta_{quintile} Feedback_c * Quintiles_{10isc} + \lambda_{quintile} Quintiles_{10isc}$$
$$+\psi Feedback_c + X'\gamma + \psi_c + \phi_s + \epsilon_{ic} \text{ (1a)}$$

$$Rank-school_{12isc} = \mu + \delta_{quintile} Feedback_c * Quintiles_{10isc} + \kappa_{quintile} Quintiles_{10isc}$$
$$+\xi Feedback_c + X'\zeta + \theta_c + \omega_{isc} \text{ (1b)}$$

where $Quintiles_{10isc}$ is a dummy variable that takes the value of one if the student is in the corresponding quintile based on his tenth grade performance in his school. Moreover, $Feedback_c$ is a dummy variable equal to one if the student takes the eleventh grade national exam ie. if the graduation year is smaller than 2006 (feedback regime). The parameter of interest β (δ) measures the effect of feedback on student's rank nationwide (within his school) in the subsequent year, conditional on tenth grade performance. In some specifications, we control for unobserved time and school invariant factors that may affect final year's rank using time and school fixed effects. We also control for students' characteristics (X) like the age and the gender of the student. Specification (1b) exploits within school variation, thus we use (1a) without the school fixed effects when we are interested in exploiting across schools time invariant variation.

In addition to the first strategy, we now use the following difference specification to find the effect of feedback on each decile of students' twelfth grade performance. We run the following specifications for each decile of tenth grade performance $\theta \in [0, 1]$:

$$Rank - nationwide_{12ic\theta} = \delta_\theta + \alpha_\theta X_i + \beta_\theta D_{\mathrm{c}} + \psi_c + \epsilon_{ic\theta} \text{ (2a)}$$

$$Rank - school_{12isc\theta} = \omega_\theta + \alpha_\theta X_i + \gamma_\theta D_{\mathrm{c}} + \theta_c + u_{isc\theta} \text{ (2b)}$$

where δ_θ captures a performance group-specific fixed effect. D_{c} is a feedback dummy that takes the value one in the feedback regime and it takes the value zero in the non-feedback regime. The parameter of interest β is estimated separately for each one of the ten deciles, including clusters at the school level. A similar regression across all decile groups gives the pooled OLS estimator of β_θ which is exactly zero, because as we explained before, the provision of feedback has a zero average effect. A negative coefficient of β_θ orγ_θ implies that feedback induces a deterioration in the rank nationwide or within his school for students at this decile.

2.4 Main Results

2.4.1 Effect on performance

Main OLS results are reported in Table 4. The first column in Table 4 corresponds to the basic specification (1a) without school and year fixed effects. The dummy for the third tenth grade quintile is omitted as a point of comparison. This shows that when feedback is provided, a student in the top quintile in his school has a 0.042 percentile rank gain in his twelfth grade national exam performance compared to a student who is in the median quintile in his school, ceteris paribus. Similarly, a student who receives feedback and is in the bottom quintile in his school has a 0.088 percentile rank loss in his twelfth grade national performance compared to a student in the median quintile in his school. In columns 2 and 3, we see that the results of column 1 are robust when controlling for unobserved heterogeneity across schools and years respectively. Adding school and year fixed effects slightly change the coefficients estimates, which remain statistically significant at a 1 $\%$ significance level. In all specifications, we control for a set of pupil characteristics and we cluster the standard errors at the school level.

In specification (1b) we exploit the within school variation and results are in Table 5. The effect of feedback on students' within school performance in the incentivized subjects is reported in columns (1) and (2) and in the non-incentivized subjects in columns (3) and (4). In the first column, we show that students in the top quintiles, 5 and 4, based on the tenth grade performance, benefit from feedback. This gain is associated with 0.045 and 0.040 school percentile ranks respectively, compared to the third quintile. Similarly, quintiles 2 and 1 (bottom ones) experience a loss of 0.038 and 0.079 school percentile ranks when feedback is provided. In column 2 we control for unobserved heterogeneity across years and as we expect, results are similar to Table 4 column 3 when we controlled for unobserved heterogeneity across years and schools in the national analysis.

We replicate the analysis using the school rank in the non-incentivized subjects as the outcome variable. In columns 3 and 4 (Table 5), we find that the coefficients are not statistically significant and there is no evidence that the provision of feedback affects students' performance in these subjects. What is important here is that students do not receive any social comparison information regarding the non-incentivized subjects neither in the feedback regime nor in the non-feedback regime. These results are also meaningful because they provide evidence that there are no spill-over effects from the feedback towards the non-incentivized subjects. In other words, students do not react to feedback by studying more or less for the school exams rather than the national exams. These findings support that the effects on students' final-year performance are generated by the relative performance information that is provided in the eleventh grade.

We then run specification (2a) and in Figure 5 we plot the β_θ coefficients of the rank nation-wide and the associated 95 $\%$ confidence interval. We observe that receiving information about someone's relative performance has a negative effect for students below the 45th percentile and a positive effect for students above it. At the highest two deciles, the curve is slightly decreasing implying that there is a ceiling effect. In other words, there is some upper bound on how much improvement feedback induces for the highest performing students. Thus, receiving relative performance information the year before the university admission, high stake exams improves the final-year rank nationwide of the high achieving students by up to 5 percentiles. By contrast, when the relative performance information is provided then the final-year rank nationwide of the low achieving students drops by up to 8 percentiles. In Figure 6, we report γ_θ coefficients and the associated 95 $\%$ confidence interval, which shows the effect of feedback on the final-year rank within the school (and not national rank as before). The estimated treatment effects on the final-year rank within the school (in Figure 6) are very similar to the ones found before, when the national rank was considered (in Figure 5). For schools above or below the average quality school a student's rank within the school differs from his rank nationally. However, on average (across all schools) the school rank for each decile might not dramatically differ from the national rank given that the school sample is a representative one in terms of many observed characteristics.

Figure 7 plots the treatment effect coefficients for the non-incentivized subjects that we use as the main non-treated subjects and we explained previously in this section. In line with Table 5, we find no evidence that providing feedback affects students' performance in these subjects.

We then standardize the twelfth-grade scores in each year and school to give a zero mean and a standard deviation of one. Then we run a specification similar to (2b), but the outcome variable is the twelfth-grade standardized score of student i in school s in cohort c in each decile θ. We run this regression for each decile of tenth-grade performance, and we plot the coefficient of the feedback dummy D_c. The treatment effects line for each decile of prior performance is presented in Figure 8. There, the gain for students above the 40th percentile is up to 0.15 standard deviations while the performance of students who are below the 40th percentile drops by up to 0.3 standard deviations.

2.4.2 Gender

Next, we turn to the gender analysis. As literature on evaluating social programs has shown, individuals respond differently to the same policy (Heckman 2001). To test whether boys and girls react differently to the provision of feedback, we estimate the following regression:

$$Rank - nationwide_{ic} = \delta + \beta Feedback_c * Female_i + \kappa Feedback_c$$
$$+\lambda Female_i + \alpha X_i + \mu_t + \epsilon_{ic} \text{ (3a)}$$

$$Rank - school_{isc} = \delta + \beta Feedback_c * Female_i + \kappa Feedback_c$$
$$+\lambda Female_i + \alpha X_i + \mu_t + \epsilon_{isc} \text{ (3b)}$$

where X_i includes the tenth grade GPA performance, a dummy for early enrollment in school and dummies for the speciality chosen in the twelfth grade. OLS results are shown in Table 7. Although girls outperform boys, girls end up in a lower rank on average when feedback is provided. This is the case for both; the rank nationwide and the rank within their school.[19] Running specification (2b)[20] for boys and girls separately produces Figure 10, which presents the treatment lines for boys (on the left) and girls (on the right).

For both genders, the effect of feedback is positive for high-achieving students and negative for low-achieving students. We make two important points here: first, the average effect of feedback is positive for boys' final-year rank and negative for girls' final-year rank, as shown by the horizontal line, which is generated by a regression across all deciles (Figure 9). Second, the effects of feedback are more pronounced for girls. As indicated by the steeper treatment line in Figure 9, girls exhibit greater sensitivity to rankings.

Our evidence is consistent with the literature supporting a differential gender effect of feedback, with females responding more to additional information. In an experimental context, McCarty 1986 shows that women and men may react differently in the absence of feedback information because of different levels of self-confidence. Also using an experimental context, Franz, Frick, and Hanslits 2009 argue that women never have the same level of self-confidence as men because women expect less of themselves than men do. Our gender differential negative feedback effects are consistent with the existing literature on gender specific perceptions regarding competition. (Gneezy et al. 2003, Gneezy and Rustichini 2004). Among women and men of the same prior performance, women are less effective than men when they take the competitive national exams.

[19] In Table 7, if we include school fixed effects in columns (3) and (4), we account for heterogeneity across schools and the coefficient estimates become the same as in columns(1) and (2).

[20] (2a) gives results almost identical to (2b) for both genders.

2.4.3 Long term outcomes

In this section we examine the effect of feedback provision on students' long term outcomes. Students who have not been admitted to their chosen university department may re-apply a year (or more) after graduation using their school grades and re-taking national exams in all subjects. Those students usually do not attend any school/college, or pursue any job, or do military service after graduation and before the next admission period.

We use binary response models to examine whether the provision of feedback affects the decision to retake the exam. In Table 8, we observe that a significant percentage out of the cohort population repeats the exams one year after graduation from senior high school[21].

We define as "misplacement" the difference between the tenth grade rank each student achieves within the school and the rank nationwide in the twelfth grade. Thus, the misplacement variable is bounded between minus one and one. Students with larger differences between the tenth- and twelfth- grade ranks would have a large change in their relative performance. The misplacement variable takes the value zero for students whose twelfth-grade rank happens to correspond exactly to the tenth-grade rank. But it can also take positive (negative) values if the student achieves a better (worse) performance in the tenth grade relative to the twelfth.

To examine if feedback provision affects someone's decision to retake the national exams through the misplacement effect we run the following specification:

$$Retake_{i,t+1,s,d} = a + X'_{itsd}\gamma + \delta Misplacement_{itsd}Feedback_t + \beta Feedback_t$$

$$+\omega Misplacement_{itsd} + \zeta Z_{td} + \xi_s + \omega_t + \epsilon_{itsd}$$

The decision to retake the national exam one year after graduation depends also on the opportunity cost for the student. Thus, we control for the unemployment rate in each year t and district d of student's residence.

Using Linear Probability (LPM), Probit and Logit models we find that when feedback is provided, students with higher misplacement are more likely to repeat the national exams one year after graduation. In Table 10, we interact dummies that capture the magnitude of misplacement with the feedback dummy and we observe that students in the top misplacement quintile (5) are more likely to retake the national exams when feedback is provided. The Top Misplacement Quintile (5) is the most positive one and contains students who get a better rank in the tenth grade compared to the twelfth. In the feedback years, these are the low achieving students. In other

[21] The number of students retaking the exam is calculated using the Ministry of Education dataset. The data about the labor force capacity are collected from the National Statistical Authority.

words, low-achieving students are more likely to resit the national exams when feedback is provided. By contrast, high-achieving students are less likely to retake the exams when feedback is provided.

Having a particular placement in university admission affects an individual's employment and earnings prospects. We examine whether feedback influences the matching of students to university departments. We first rank all programs [22] according to their average cut-offs over the seven years period. Each program's cut-off reflects the demand for this particular university department, with highly demanded programs exhibiting high cut-offs. Students apply 671 to programs based on preferences, social status and expected earnings. There are programs in total. We estimate the effect of feedback on the difference in the selectiveness position and rank of the program admitted conditional on tenth grade performance. Figure 10 presents the treatment effect line for the selectiveness position (on the left) and rank of the admitting program (on the right). The provision of feedback has a positive effect on the selectiveness position and rank of the admitting program in the upper half of the prior performance distribution, and a negative effect on the lower half. In particular, high-achieving students move up the university selectiveness ladder by 30 positions, which is 0.15 of a standard deviation. When feedback is provided, low achieving students move down the program selectiveness ladder by 35 positions which is 0.18 of a standard deviation. Different placements in university admission induce different gains related to the returns to college.

Enrolling into a specific university department may affect students' career paths and their lifetime earnings. Using Labor Force Survey data, we match salaries for each occupation to each university department. In particular, we use the 2003 Labour Force Survey to map each college major into the most related occupation and then into the expected annual earnings after graduation (in Euros).[23] We then use these figures as the expected earnings of current students after graduation from the particular program. In Figure 11, we present the effect of feedback on the expected annual earnings, conditional on the tenth grade performance. For students above the 50th percentile, annual expected earnings increase by 250 Euros per year, which is equivalent to 0.17 of a standard deviation. For students below the 50th percentile, the decline in their expected annual earnings corresponds to 0.20 of a standard deviation.

2.4.4 Social Mobility

In this section, we examine if the provision of feedback changes the relationship between parental income (proxied by neighborhood income) and post-secondary opportunities (indicated by the program the student enrols in). A priori, we might expect that students coming from more advantaged neighborhoods would have better chances of embarking on a better and more-selective

[22] By program we mean each combination of university department.

[23] Mean:12,758 with 1,473 standard deviation.

program with higher expected returns than students coming from less-advantaged neighborhoods. Could the provision of feedback affect this flow of students from high-income families to high-expected-income programs? Providing relative performance information might have a different effect on students whose parents have varying levels of income; the difference in the role feedback plays may be related to other family resources (financial support or social networks) or students from different income backgrounds might value the ranking information differently.

To investigate whether feedback has a differential effect on students from different income backgrounds, we create quintiles based on the neighborhood income and the selectiveness of the program admitted to. In Table 13 we report for each quintile of neighborhood income, the percentage of students who enroll into each quintile of programs by selectiveness, in the feedback and the non-feedback regime. We then calculate the difference between the feedback and the non-feedback percentage. In the last row of Table 13, we vertically add the percentages of students who enroll in any program for each quintile of neighborhood income, to find the total difference of enrolled students between the feedback and the non-feedback period. In the last column of Table 13, we horizontally add the percentages of students who enroll in each quintile of programs. We do that to examine if feedback provision affects the total percentage of students who enroll in higher education. We find that 2.2 % more students (83.7% Vs 81.5%) enroll in a program in the feedback regime.

In Table 13, we find descriptive evidence that more students coming from the lowest-income neighborhoods (Quintile1) enroll in any program when feedback is provided (2.2% more students). A possible explanation is that low achieving students discover that if they do not exert more effort they will not be admitted to any program in tertiary education. Or they might discover that they are not worse than the low achieving students from high income neighborhoods and that they still have a chance to enroll in university. So, they might decide to exert more effort. This may show that feedback benefits students from low-income neighborhoods by reducing social inequalities and possibly future income inequalities. On the other hand, high achieving students from low-income neighborhoods discover in the eleventh grade that they are highly ranked on a national scale and they might react by exerting more effort.

We also find descriptive evidence in Table 13, that feedback provision alters the parental income (proxied by the neighborhood income) composition of students who are admitted into the top-ranked programs (Quintile 5). More students from low income neighborhoods are admitted to the most-selective programs that provide students with the highest expected earnings after graduation (such as engineering and law), when feedback information is provided (2.9% Vs 2.6 %). This implies that providing relative performance information encourages social elevation and improves economic opportunity for these students.

It is crucial from a policy perspective to understand if providing feedback is beneficial for the society as a whole. On one hand, high performing students are usually the ones responsible for innovation and technological breakthroughs. The technological diffusion is beneficial for the society as a whole, because technological innovation is one of the driving forces behind a country's economic prosperity and productivity advance (Nickell and Van Reenen (2001)). On the other hand, our study shows that providing relative performance information improves the performance of high-achieving students whereas low-achieving students perform even worse. This widening of the performance gap caused by feedback may be translated into a wage gap later. We find evidence to this direction using students expected wages. This might be detrimental especially for low achieving students. An economist may be fond of the efficiency achieved though information provision as high achieving students end up higher in the society and the spillover effects of the technological advances to the whole society. Nevertheless, at the end of the day its up to the society to decide whether efficiency can be traded for equality.

Additionally, our descriptive statistics evidence show that providing the relative performance information may encourage students from low income families to enroll in university and especially to more selective programs. From this perspective, providing the relative performance information encourages social elevation for students coming from low income neighborhoods. Thus, feedback decreases the performance or income inequality between students coming from low and high income neighborhoods.

2.5 Mechanisms

In this section, we discuss the most likely mechanisms that could explain our findings. Although it is impossible to distinguish between students' and parents' reaction to the social comparison information, we are able to rule out the possibility that teachers are driving the results. This is because the national exams are externally marked and teachers have no way to affect these grades. Neither students nor parents can select which school to attend, because this allocation is centrally managed and is based on geographical criteria.[24]Additionally, teachers cannot allocate students to classes in a way that facilitates sorting because it is prohibited by the law. Students are allocated to classes based on alphabetical order and they cannot switch classes within the school. Teachers have neither direct nor indirect financial incentives to react to the social comparison information. Teachers' compensation is not linked to teachers' quality nor is it a function of school-based performance: it is based on years of teaching experience and level of education attained. Also, schools' financing is not affected by their students' success in the national exams.

2.5.1 Mechanism 1: Priors- Positive Vs Negative Surprise

In this section, we examine whether students respond to the specific type of feedback that they get. Students might not only compare themselves with their class, school or cohort but they may also compare their own relative performance in different periods in time. We exploit within-school

[24]Parents have to submit an application to the local authority with proof of address

variation in the 134 senior high schools and we restrict this part of the analysis to the feedback years.

A recent paper by Azmat, Bagues, Cabrales, and Iriberri (2015) highlights the importance of students' priors, when evaluating the effects of feedback. The authors provide relative performance information to university students, and that decreases their short-term performance. After conducting a survey, the authors find that students tend to be under-confident and the provision of feedback only increases their self-reported satisfaction.

Although we do not observe students' exact priors, we assume that their priors will be a function of their tenth-grade performance. In the feedback regime, students update their priors using the eleventh-grade performance. If a student receives information that he is in a higher decile in the eleventh grade than in the tenth grade, then the student receives a positive shock, that can be translated into a "positive surprise". On the other hand, if the student discovers that he is in a lower decile in the eleventh grade than in the tenth grade, then this student receives a "negative surprise". Intuitively, students who receive a positive surprise in the eleventh grade might increase their expectations of themselves and exert more effort in the twelfth grade, whereas they might reduce their effort if they receive a negative surprise. In Figure 12, we graph the effect on the twelfth-grade rank for each combination of deciles in tenth and eleventh grade.

The horizontal axis represents the eleventh-grade rank of students, and the vertical axis represents the tenth-grade rank. Different colours express different magnitudes of the treatment effects on the twelfth-grade rank. The diagonal starting from zero towards the right upper edge of the box, represents the case of ”no value feedback”: those students whose eleventh-grade percentile rank equals their tenth-grade percentile rank. The treatment effect is positive for most students experiencing a positive surprise. These are students who are on the right of the diagonal of ”no value feedback”. On the left of the diagonal, feedback effects are mainly negative, meaning that students' twelfth grade rank declines when they receive a negative surprise.

A concern here is that students might not be aware of their tenth grade percentile rank, especially if they attend a school with more than one class. However, the analysis here uses deciles of performance and not percentiles, allowing students to have priors that do not accurately express their exact tenth grade rank.

2.5.2 Mechanism 2: School quality revelation

An alternative mechanism could be that students use the information obtained by the publication of their scores to infer the quality of their senior high school.[25] Students who take the eleventh

[25] We measure school quality based on the schools' average national exam performance in the twelfth grade from 2003 to 2009. Then we construct a rank measure for school quality that varies from zero to one. The average quality of the schools in our sample is 0.52 (sd 0.21, min, 0.018 and max. 0.985) which means that our school sample is of a representative quality.

grade national exams discover their school rank and their national rank, and the comparison of the two ranks reveals information about the quality of the school. If a student discovers that his national rank is greater than the school rank then his school is of good quality. Conversely the school is of lower quality if the national rank is lower than the school rank. The revelation of the school quality in the eleventh grade might affect students' choice of effort in the twelfth grade. Thus, we exploit the across schools variation in quality to identify the effect of feedback on students' rank nationwide.

In Figure 13, we produce the treatment lines separately for students who discover that the school they attend is worse (on the left) and better (on the right) than the average quality school. In Figure 14, we repeat the same exercise and we produce these figures using the standardised national exam score.[26] The average effect for students who realise that they attend a worse-than-average quality school is negative, whereas it is positive for those who realise that they attend a better-than-average quality school.

Starting with the bottom of the prior performance distribution, we observe that low achieving students in good schools do better that those in lower quality schools. Surprisingly, there is a huge increase in the national rank for the top students in the worse schools and this increase even offsets the increase in the national rank of the top students in good schools. There are two possible explanations: first, high performing students in the low quality schools take the eleventh grade national exams and when they receive feedback, they realise that they are actually exceptional on a national scale. Thus they might decide to exert more effort in the next time period, so feedback acts as a motivation boost for these students.

Second, the realisation of their national rank acts as a rude awakening for these students who might initially have a wrong perception about the national competition and about their school's quality. These students might be the top students in their class or school but they now learn that they are behind. In the next time period, they exert more effort to catch up with the national standards.

If students realise the quality of the high school through the eleventh grade national exams, then the response to the feedback would be more consistent across the school group. For example, if students from all parts of the performance distribution in school X discover that their school is of low quality and they are concerned with university admission, they might all exert more effort to catch up with national standards.

2.5.3 Mechanism 3: Parental investment

Another possible mechanism is that parents decide to invest more or less in students based on the eleventh grade results. Parents may start devoting more time helping the child with the

[26] Standardised within each year with zero mean and a standard deviation of one.

homework or they may invest in external support (such as supplementary material/books, private tutors etc). It is true that there might be variation in family income within the school and the neighborhood income represents the average income in each region. We observe considerable differences in neighborhood income.[27]

In Figure 15, we draw the treatment lines for the bottom and the top quintile of neighbordhood income. We run specification (2b) separately for the top and the bottom quintiles of neighborhood income. This may not fully reflect the family income but we examine the effects of feedback across regions by average reported income. A wealthy family may have the financial resources to invest in the child and thus the student may improve his performance in the subsequent year exam. On the other hand, families from low income neighborhoods may not be able to pay enough to further support the student. In Figure 16, we observe that disclosing rankings increases the average subsequent national rank for students coming from the highest-income neighborhoods. The average effect on the subsequent national rank for students from the lowest-income neighborhoods is negative. In high-income neighborhoods the positive effects of feedback hold for students above the 40th percentile while only students above the 60th percentile from low income neighborhoods benefit from feedback. If parental investment was the only driver of the findings, then we would expect students from highest-income neighborhoods to improve at all parts of the prior-performance distribution. That implies that there might be, to some extent, differential parental investment in students by family income (proxied by neighborhood income) but that cannot fully generate our results.

2.5.4 Mechanism 4: Practice

It could be argued that students can accurately place themselves within their class, even if they are not explicitly informed about their rank. This is likely to occur due to repeated interactions among classmates throughout high school. However, here students receive new information that is broader. Consider the within school rank: students receive information about how well they did within their school. In Figure 16, we report the treatment lines for students in schools of different capacity in the eleventh grade. We make four broad categorisations. First, we consider schools with only one class where it is likely that students already know their relative standing and the social comparison information has no extra value (Panel A). Nevertheless, in a school with only two classes students might know their relative performance in their class but not in the whole school. Thus, we see that there is a small positive feedback effect on students who are above the 40th percentile and a small negative effect on those below it (Panel B). Additionally, the treatment lines become steeper when we consider schools with three classes (Panel C). In this case, the information is much broader than that which students can collect from interaction with

[27] mean: 23,517 standard deviation: 8,609 min.:13,005 max.: 66,521

their classmates. This is even more pronounced when we look at students in schools with more than three classes (Panel D). Summary statistics about the capacity of schools in our sample are presented in Table 6. Figure 16 shows that the effect of feedback depends on whether the additional information is actually informative about someone's relative performance.

That could allay the concern that the eleventh grade national exam might provide students with experience or training instead of information about their relative performance. School exams in the eleventh grade have the same format as national exams in the eleventh grade and the past papers are available in both regimes. Students practice on past questions and are aware of the structure and the types of questions in both cases. If students were experienced from sitting the eleventh grade national exams, then the experience or training effect would not vary by the size of the school. In other words, if that was the mechanism then students in small schools would have no reason to react differently than students in regular schools.

2.5.5 Mechanism 5: Learning about own ability

Another possibility is that students have imperfect information about their own ability and they compare their own absolute score with the average school/cohort score to infer their own ability. We adapt a theoretical model proposed by Ertac 2005.[28] In the non-feedback regime, students in the eleventh grade sit school exams and they receive information about their own absolute performance only. In the feedback regime, they receive information about their own performance, but also about the school and cohort average performances.

Students take exams in two time periods; the eleventh and the twelfth grade.

Period 1: This is the learning stage. The eleventh grade own performance provides students with some information about their ability (and the easiness of the exam). This performance acts as a private signal s_i for the student. In the feedback regime, students also observe the average score in the school or nationwide $\bar{s} = \{\bar{s}_{school}, \bar{s}_{cohort}\}$ which is the average signal in the school and the country respectively. In the feedback regime student i may compare his own signal with the average signal (in the school and/or in the cohort) and that may affect student's perceived belief about his own ability. That could, in turn, determine the amount of effort he decides to exert in the second period. The amount of effort students decide to exert in the twelfth grade affects their final year's scores.

Period 2: Following the realisation of the signals, in the second period students choose the effort to exert (e_i). Students' objective is to maximize the second period performance (q_i) after choosing the effort to exert. Assuming that the performance production is a linear function in

[28] Ertac 2005 presents a principal-multiple agents model where agents have imperfect information about their abilities under multiple types of contracts. The model is also used by Azmat and Iriberri 2010. The natural experiment they study gives students information about the average class grade, while here the social comparison information refers to the average school and cohort grade.

effort and that effort and ability are complements[29] in the performance production function[30] it follows that: $q_i = e_i\alpha_i$. There is also a cost associated with the effort exerted that is $c(e_i)$ and is increasing in effort and convex. [31] In the absence of the social comparison information students receive only the private signal and they maximise:

$$u^{NF} = E[p_i(\alpha_i, e_i) - c(e_i)|s_i] = E[\alpha_i|s_i]e_i - c(e_i)$$

and the F.O.C simplifies to $E[\alpha_i|s_i] - c'(e_i^{NF*}) = 0$ (1)

In the feedback regime where social comparison information is provided the student observes the average signal (which could be either the school average signal or the cohort average signal) and maximises:

$$u^F = E[p_i(\alpha_i, e_i) - c(e_i)|s_i, \bar{s}] = E[\alpha_i|s_i, \bar{s}]e_i - c(e_i)$$

and the F.O.C simplifies to $E[\alpha_i|s_i, \bar{s}] - c'(e_i^{F*}) = 0$ (2)

The proof can be found in the web appendix and it comes from the comparison of equations (1) and (2). We also assume that the private signal is given by $s_i = \alpha_i + c for i = 1, 2, ...$ and it depends on student's i ability level (α_i) and a shock that is common to all students[32] ie. the easiness of the exam (c). Let us summarize now the main hypothesis about the effect of the eleventh grade social comparison information on the twelfth grade performance.

Null Hypothesis: Students do not react to the social comparison information

That would suggest that students are not uncertain about their ability or that students have already figured out their relative performance information and the explicit addition of it is redundant or that the private signal that students get in the feedback regime equals the average signal.

Alternative Hypothesis: Positive effect for high ability students and negative effect for low ability students

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[29] Notice here that there is no pass-fail scheme and students do not try to achieve a performance threshold. University cut-offs are determined endogenously based on demand and pre-specified supply of seats. In other words, the model makes these predictions based on the fact that ability and effort are complements in the production function. In a different setting where university cut-offs are pre-determined, effort and ability could be substitutes in the production function. In that case, a student who is above average in the eleventh grade may choose to exert less effort in the twelfth grade in order to achieve a specific performance threshold.

[30] $\frac{dqi}{d\alpha_i de_i} > 0$

[31] $(c'(e_i) > 0, c''(e_i) > 0, c'(0) = c''(0) = 0)$

[32] In the school or the cohort depending on the feedback or non-feedback regime.

That would suggest that students will react differently to feedback. Based on the model, high ability students will perform better when the social comparison information is provided. On the other hand, low ability students will perform worse when the social comparison information is provided. Our findings support the alternative hypothesis, implying that students whose eleventh grade absolute score is above the school/cohort average score, might be encouraged by their relative performance and that makes them exert a higher amount of effort in the twelfth grade. On the other hand, students who realise that they score below the school/cohort average might be discouraged by that and exert a lower amount of effort in the twelfth grade. This channel highlights the importance of non-cognitive skills on educational outcomes and especially self-perception about own ability and confidence. The importance of non-cognitive skills is well established in the literature (Brunello and Schlotter 2011, Heckman et al. 2006, Kautz, Heckman, Diris, Weel, and Borghans 2014)

2.6 Threats to identification

Attrition

In our attempt to evaluate the impact of feedback on different performance groups, the problem of attrition cannot be ignored. If attrition is random and affects different performance groups in a similar way in both regimes, then the estimates remain unbiased. Differential attrition here could arise because students from the lowest percentiles are more likely to drop out from school in comparison to students from the highest percentiles, when they realise their relative performance. What could bias our estimates, is if differential attrition follows the abolition of feedback.[33] In Figure 17, we observe that attrition rates differ for each quintile of prior performance but the attrition rates following the abolition of feedback do not change dramatically compared to previous years.

Notice here that students drop from our sample either because they drop out from school or they move to a different school. The unique student code that identifies students across grades within a school changes if the student switches to another school. We cannot follow students who move to a different school.

Exploiting within school variation, we use the following specification to check for differential attrition that changes with feedback:

$$Drop-out_{12-10isc} = \alpha + \beta_{quintile} Feedback_c * Quintiles_{10isc} + \lambda_{quintile} Quintiles_{10isc}$$
$$+ \psi Feedback_c + X'\gamma + \theta_c + \varphi_s + \epsilon_{isc}$$

Table 11 reports OLS results. The attrition rate is larger for the lowest quintile than any other, compared to the third quintile, when feedback is provided. But most importantly, none of the

[33] The first affected cohort for which feedback is abolished is the cohort that was in the twelfth grade in 2006. Thus, this cohort was in the tenth grade in 2004. This is the first cohort that did not sit national exams in the eleventh grade.

coefficients of interest are statistically significant. This implies that there is differential attrition, but it does not vary with feedback policies.

Robustness checks

In this section, we construct robustness exercises to complement our main analysis.

One concern might be that the change in the variation of performance over time is caused by time trends and not the provision of ranks. Exploiting the within school variation [34], we run specification (2b) but without pooling feedback and non-feedback years together. Instead, we just compare every pair of consecutive years in the sample. We present the placebo treatment lines in Figure 18. Panel A compares the cohort 2003 to the 2004, as if feedback was abolished in 2004. We find similar cohort behavior from 2003 to 2005 as the treatment lines are flat for these pairs of years. The only pair of years that we expect to find a differential response of cohorts is 2005-2006 (the year of the reform). Panel C corresponds to the actual reform and we observe that the treatment effects are negative for all percentiles below the 50th percentile and positive above it. For every other pair of years, we expect to find similar cohort behaviour. We find no evidence that other time specific factors could generate our results or drive ours results. Regarding any policy anticipation effects, the reform was announced in December of 2003-2004. We find very small treatment effects in Panel D, which is the first non-treated cohort. Students in the first non-feedback cohort might observe how last year's peers of similar tenth grade performance did and use this information to slightly adjust their behavior. Again after 2007, the curve is almost flat throughout the ability distribution implying a similar cohort behavior.

We conduct some other placebo exercises to verify that the effect does not depend on the numbers of subjects examined. In Figure 19, we draw the treatment lines for each subject separately. Before 2005, students take national exams on five core or general subjects (Modern Greek, Mathematics, History, Biology and Physics). From 2005 onwards, two core subjects are examined at a national level whereas the other three core subjects are examined at a school level. Students take national exams on Modern Greek which is the only compulsory core subject in both regimes. In the non-feedback regime, students choose the second core subject on which they take national exams among the options of: Mathematics, History, Biology and Physics and they sit school exams on the other three remaining subjects[35]. Panel D presents the feedback line in Modern Greek, which is compulsory examined at a national level in both regimes. We observe that the treatment lines follow a similar pattern for all subjects indicating that the number of subjects examined does not drive our results.

In Table 1, we calculate the twelfth grade rank based on different subjects. In column (1) we

[34] very similar results if we exploit the across schools variation.

[35] Around 80% of students choose to take national exams on Mathematics together with the compulsory Modern Greek subject. These students sit school exams on: History, Biology and Physics.

find the effect of feedback on the final year rank that takes into account the Electives or Track subjects on top of the core subjects[36] and the results are very similar to those reported so far. In column (2) we take into account the effect of feedback on students' performance in Modern Greek which is a common subject in both regimes and takes a special weight in the calculation of the university admission grade. Notice, that in the non-feedback regime two subjects are examined nationally and three within the school. In column (3) we calculate the twelfth year's rank based on the scores in the national exams: five subjects in the feedback regime and the two subjects in the non-feedback regime. Results remain very similar. Feedback effects remain positive for the top quintiles of prior performance, whereas it is negative for the bottom quintiles of prior performance.

2.7 Conclusion

In this paper, we examined the effects of providing information on a student's rank on the student's short- and long-term outcomes. Knowing one's rank may affect investment decisions and, thus, later productivity. Following an unexpected policy change that took place in Greece, we carried out a large-scale, primary data-collection process. Using unique, detailed data on students' performance throughout senior high school and school quality data, we examine the effects of receiving information about the relative performance of students within their own school and across the nation.

We find that disclosing information on rankings has a positive effect on high-achieving students' short- and long- term outcomes. In particular, we find the following results for high-achieving students: Feedback information improves their subsequent performance by 0.15 of a standard deviation; they enroll into more selective university departments by 0.15 of a standard deviation, and their expected annual earnings increase by 0.17 of a standard deviation. The effects on low achieving students are negative: Their subsequent performance drops by 0.3 standard deviations; they are admitted to university departments which are less selective by 0.18 of a standard deviation, and their expected annual earnings decrease by 0.20 of a standard deviation. We also find that the results are more pronounced for females, indicating greater sensitivity to feedback. Our results show that, absent feedback on rankings, high-achieving students are more likely to retake the exams. The resulting delay of the most-able students into university and/or labor market is an important loss of human capital for society.

We also find suggestive evidence that feedback encourages students from low-income neighborhoods to enroll in university and to study in more selective programs. This may, in the long run, reduce income inequality.

We outline several potential mechanisms that may explain why students react to the provision of feedback: 1) priors of students, 2) school quality revelation, 3) parental investment, 4) practice,

[36]Students sit national exams in four Elective subjects. So the overall rank in calculated based on nine subjects.

and 5) learning about own ability. The last mechanism highlights the importance of non-cognitive skills such as self-perception about one's own ability and confidence.

Our findings have important policy implications. Providing rankings is a low-cost instrument that has the potential to affect not only students' high school performance but also labour market outcomes. The relative nature of the above mentioned results restricts the broad implementation of providing feedback, but makes it very important in a competitive process. If the social information is provided and parents can choose the best school for their child, then the relative position of the student among his school peers cannot be ignored. A crucial question concerns the social information transparency and future research is needed to understand which mechanism drives the effects. Our analysis highlights the importance of knowing one's rank on high-stake exams in influencing scholastic and labour market outcomes, and we believe that the rank could be a new factor in the education production function.

Figure 2.1: Map of schools in the sample

Figure 2.2: Timing

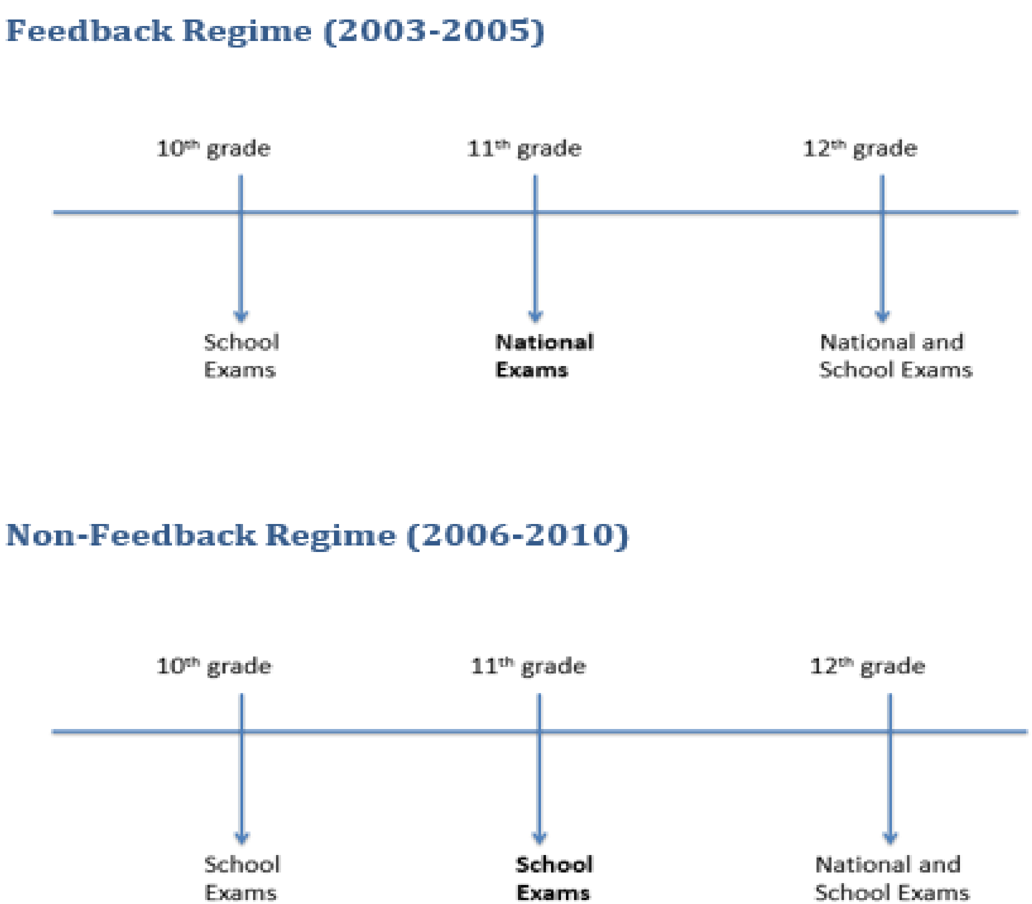

Figure 2.3: Announcement of results

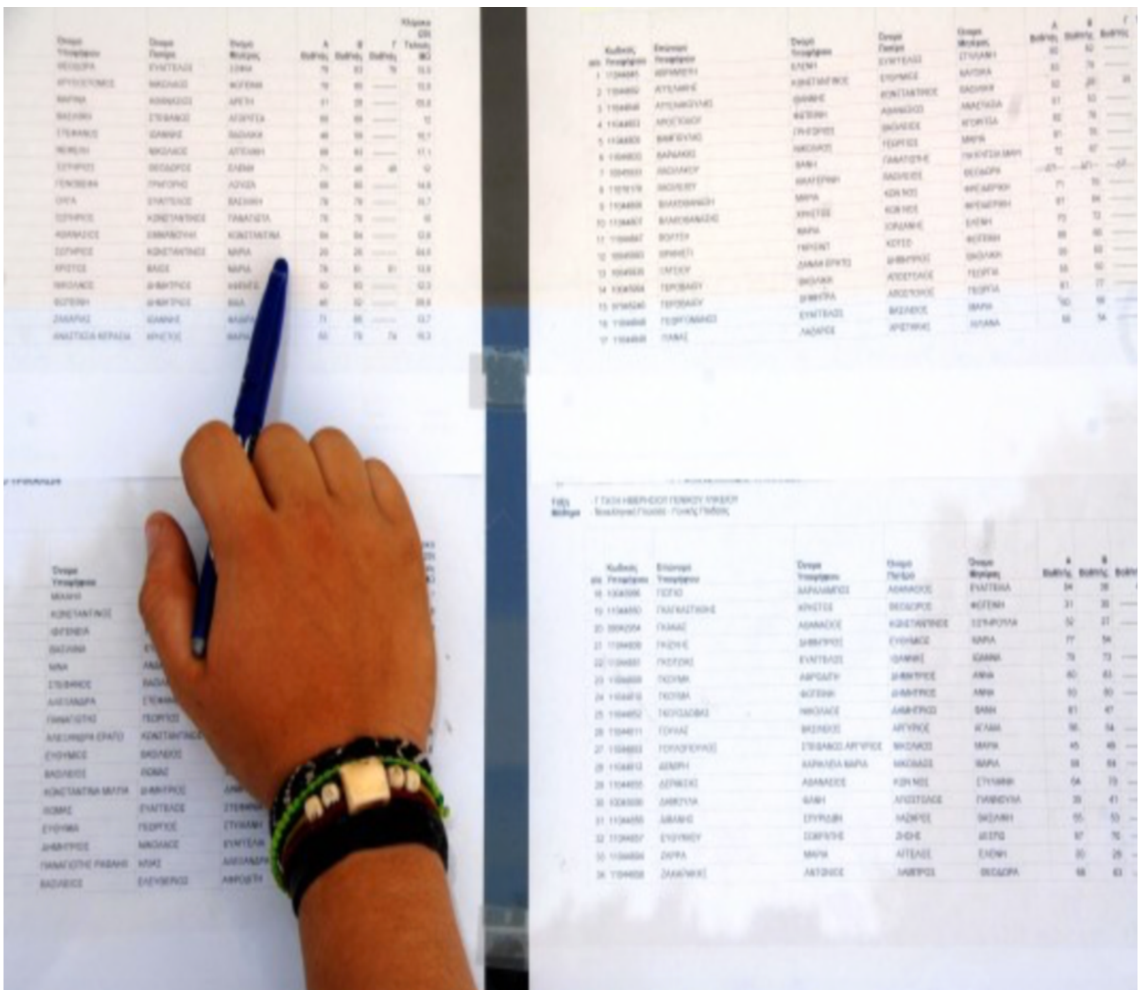

Figure 2.4: Announcement of school results-Zoom in

Α/Α	Κωδικός υποψηφίου	Επώνυμο	Όνομα	Όνομα πατρός	Α Βαθμός	Β Βαθμός	Γ Βαθμός	Τελικός Βαθμός
2	15030628	ΑΛΑΒΕΡΑ	ΜΑΡΙΑ ΕΛΕΝΗ	ΙΩΑΝΝΗΣ	84	79		16,3
4	15030629	ΒΑΡΚΑ	ΙΩΑΝΝΑ	ΘΕΟΔΟΣΙΟΣ	93	84		17,7
8	15030630	ΔΕΜΑΓΚΟΣ	ΚΩΝΣΤΑΝΤΙΝΟΣ	ΙΩΑΝΝΗΣ	27	38		6,5
14	15030631	ΕΞΑΡΧΟΠΟΥΛΟΥ	ΠΑΝΩΡΑΙΑ	ΔΗΜΗΤΡΙΟΣ	80	89		16,9
17	15030632	ΖΕΓΙΑΝΝΗ	ΜΑΡΙΑ ΠΑΝΑΓΙΩΤΑ	ΜΙΧΑΗΛ ΣΤΑΜΑΤΙΟΣ	51	45		9,6
19	15030633	ΚΑΠΠΑ	ΙΩΑΝΝΑ	ΠΕΤΡΟΣ	73	74		14,7
21	15030634	ΚΑΡΑΚΑΤΣΙΩΝΗ	ΒΑΣΙΛΙΚΗ	ΚΩΝΣΤΑΝΤΙΝΟΣ	33	22		5,5
22	15030635	ΚΑΡΠΑΘΑΚΗΣ	ΚΩΝΣΤΑΝΤΙΝΟΣ	ΙΩΑΝΝΗΣ	77	57	70	14,7
23	15030636	ΚΑΡΠΑΘΙΟΥ	ΝΟΜΙΚΗ ΗΛΙΑΝΑ	ΜΗΝΑΣ	40	34		7,4
24	15030637	ΚΑΤΑΚΑΛΕΑΣ	ΑΝΤΩΝΙΟΣ	ΠΑΝΤΕΛΗΣ	82	90		17,2
25	15030638	ΚΑΤΣΑΜΑΚΗ	ΧΑΡΑ ΣΕΒΑΣΤΙΑΝΑ	ΕΥΠΡΕΠΙΟΣ	52	48		10
29	15030639	ΚΟΥΜΠΟΓΙΑΝΝΗΣ	ΑΝΑΣΤΑΣΙΟΣ	ΑΘΑΝΑΣΙΟΣ	90	93		18,3
31	15030640	ΚΡΙΜΙΖΗ	ΧΡΥΣΟΒΑΛΑΝΤΟΥ ΔΟΜΝΙΚΗ	ΜΙΧΑΗΛ	28	24		5,2
32	15030641	ΚΥΔΩΝΑΚΗ	ΣΟΦΙΑ	ΑΝΤΩΝΙΟΣ	80	91		17,1
34	15030646	ΜΑΥΡΙΔΟΥ	ΑΙΚΑΤΕΡΙΝΗ	ΑΛΕΞΑΝΔΡΟΣ	14	15		2,9
43	15030648	ΝΤΙΝΩΡΗ	ΕΛΕΥΘΕΡΙΑ ΣΑΒΒΟΥΛΑ	ΠΑΝΑΓΙΩΤΗΣ	91	95		18,6
45	15030649	ΟΙΚΟΝΟΜΟΥ	ΑΝΝΑ ΦΙΛΙΑ	ΝΙΚΟΛΑΟΣ	96	94		19
51	15030650	ΠΑΡΒΕΡΗ	ΣΕΒΑΣΤΗ ΕΥΑΓΓΕΛΙΑ	ΑΝΑΣΤΑΣΙΟΣ	87	88		17,5
57	15030652	ΣΜΑΡΑΓΔΑΚΗ	ΕΥΑΓΓΕΛΙΑ	ΓΕΩΡΓΙΟΣ	100	99		19,9
58	15030656	ΣΩΤΗΡΙΔΗΣ	ΔΗΜΗΤΡΙΟΣ	ΓΕΡΑΝΤΙΟΣ	77	77		15,4
60	15030659	ΤΡΙΑΝΤΑΦΥΛΛΟΥ	ΔΗΜΗΤΡΑ	ΘΕΜΕΛΗΣ	67	53	68	13,5

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Figure 2.5: Time trends for twelfth grade rank nationwide

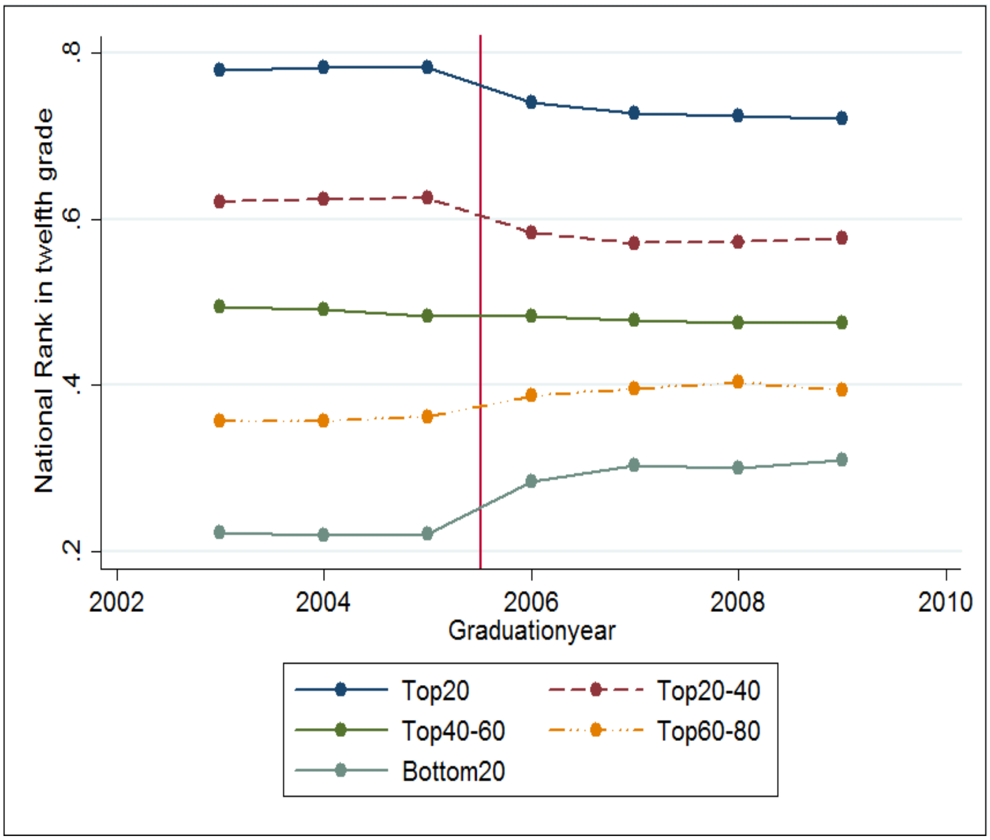

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Figure 2.6: Time trends for twelfth grade rank within the school

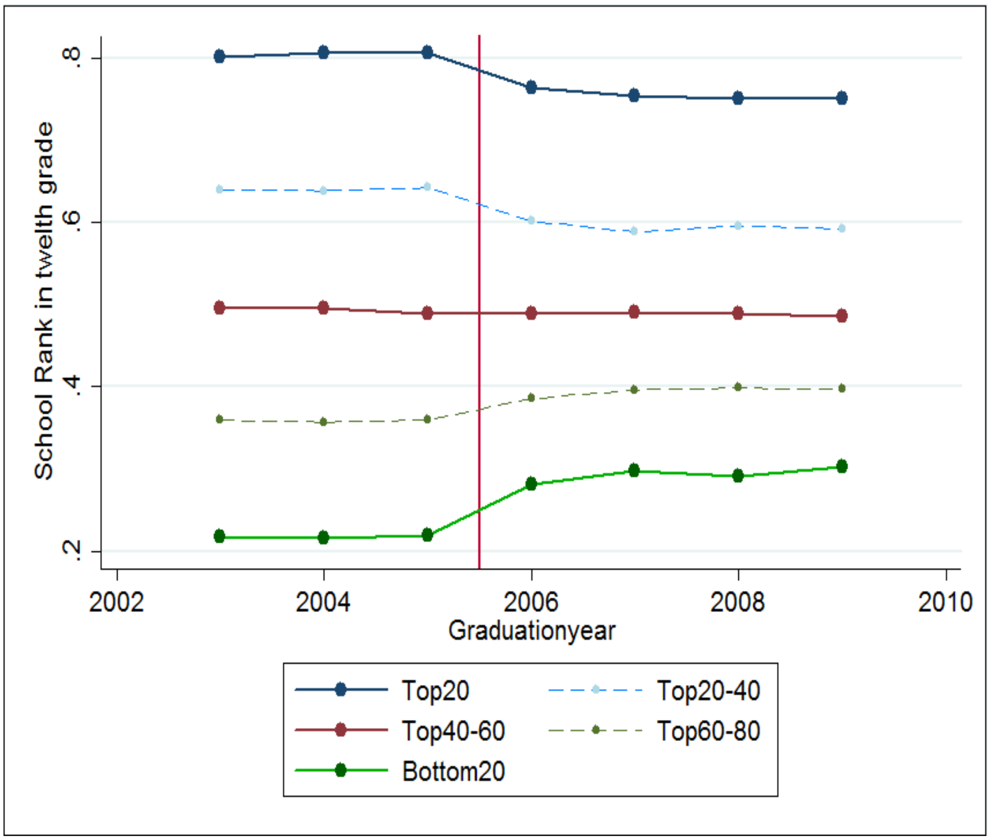

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Figure 2.7: Treatment effects on the rank nationwide conditional on prior performance

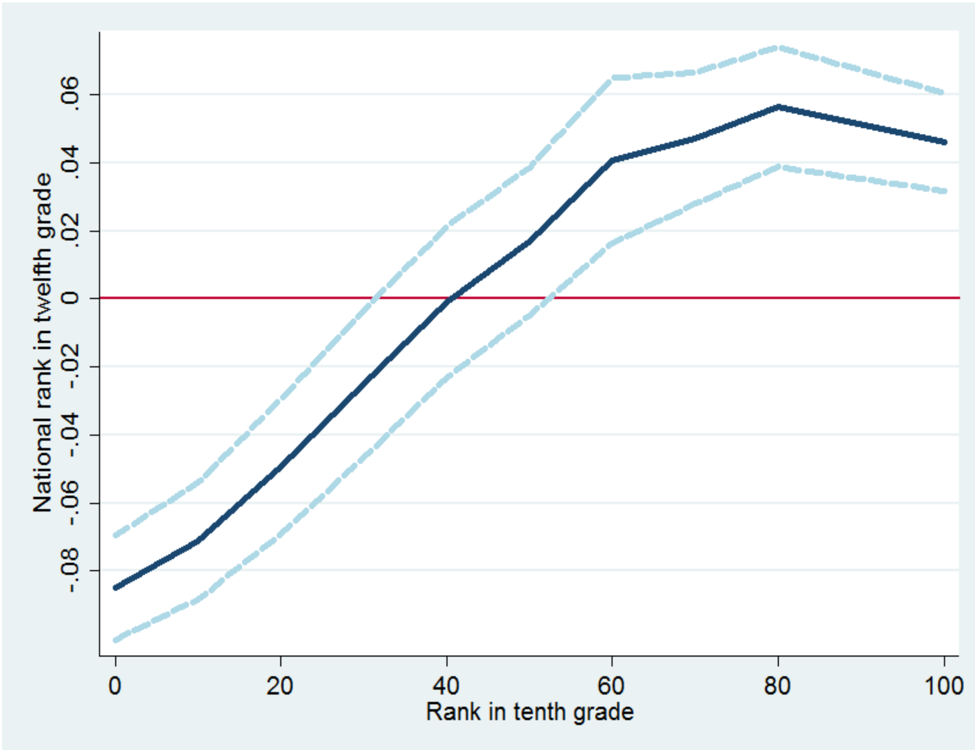

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Figure 2.8: Treatment effects on the rank within the school in incentivized subjects conditional on prior performance

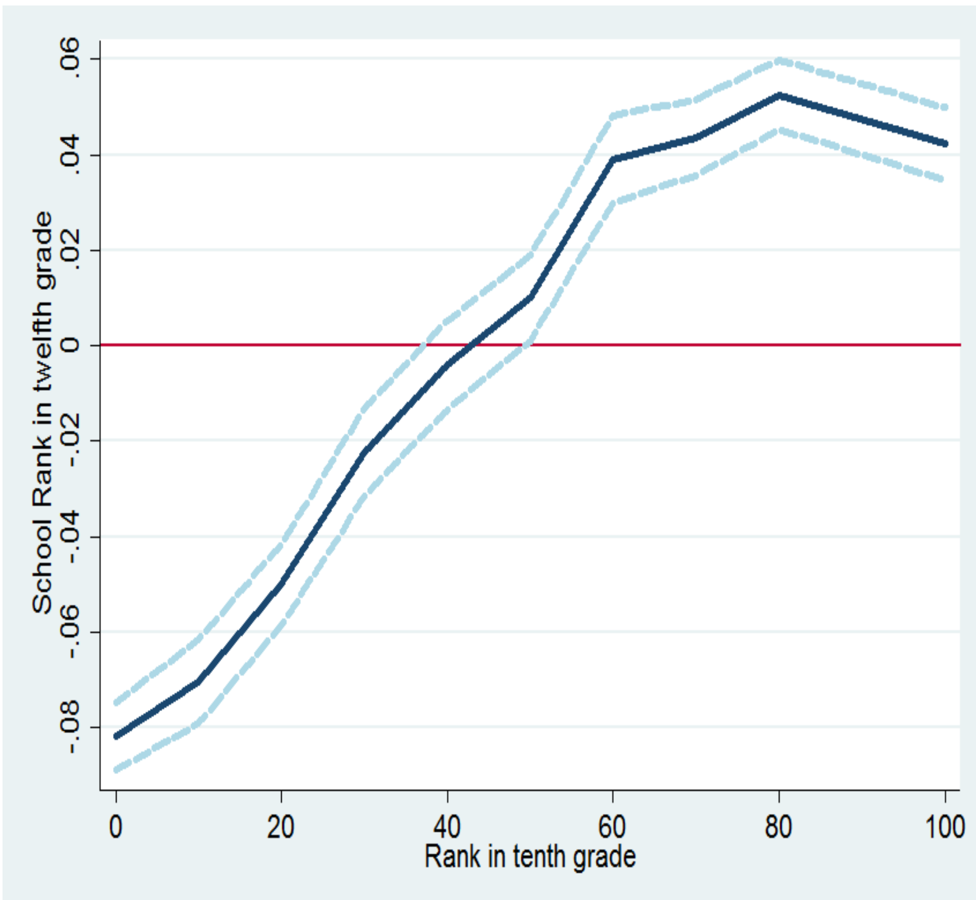

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Figure 2.9: Treatment effects on the rank within the school in non-incentivized subjects conditional on prior performance

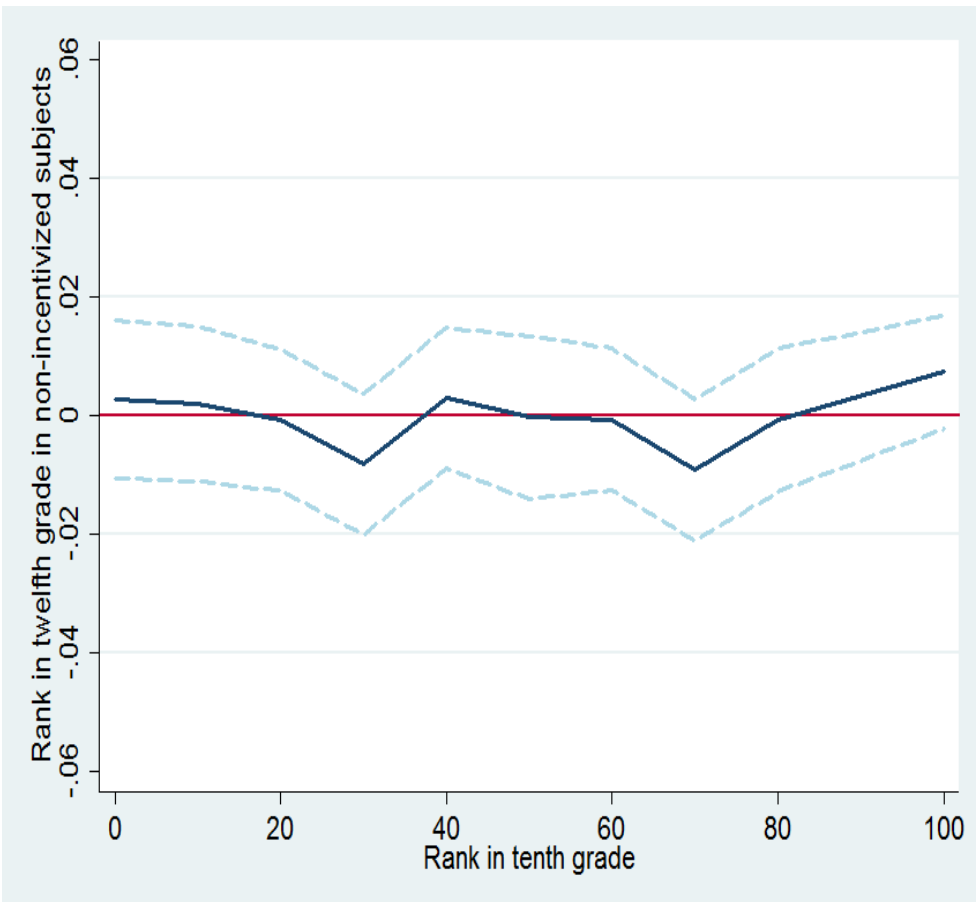

Figure 2.10: Treatment effects on the standardised score conditional on prior performance

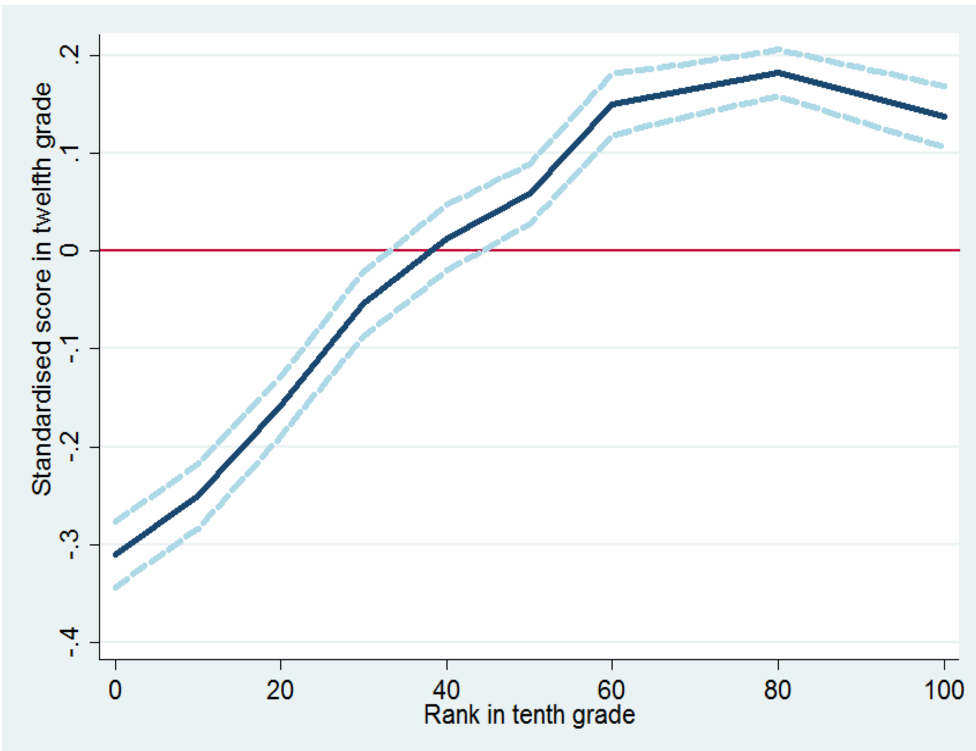

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Figure 2.11: Treatment effects on the rank within the school by gender conditional on prior performance

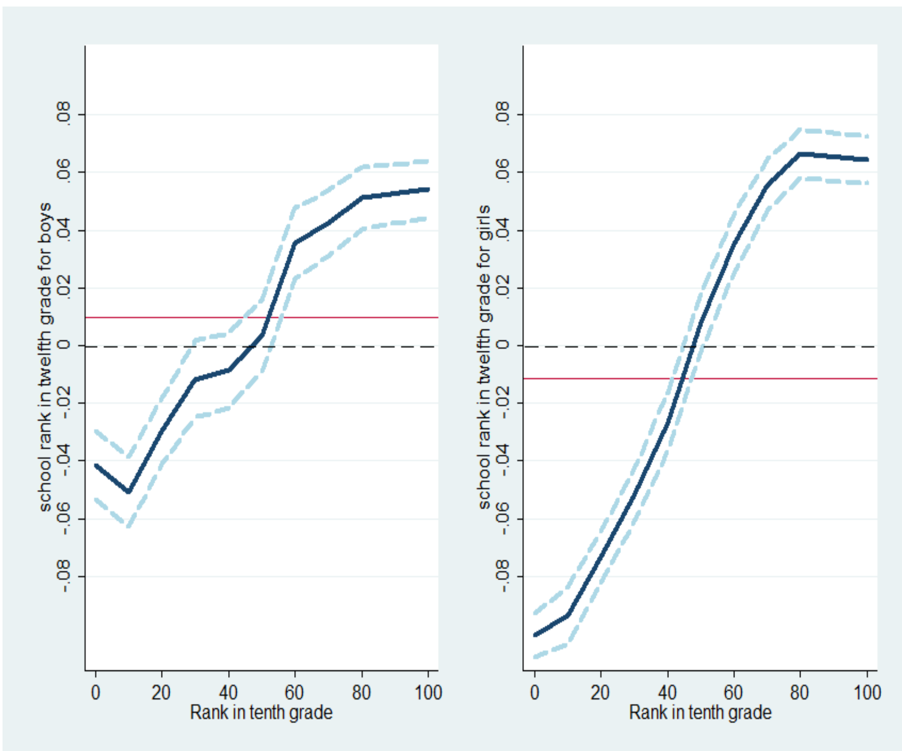

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Figure 2.12: Treatment effects on the selectiveness/prestigiousness position and rank of the program admitted conditional on prior performance

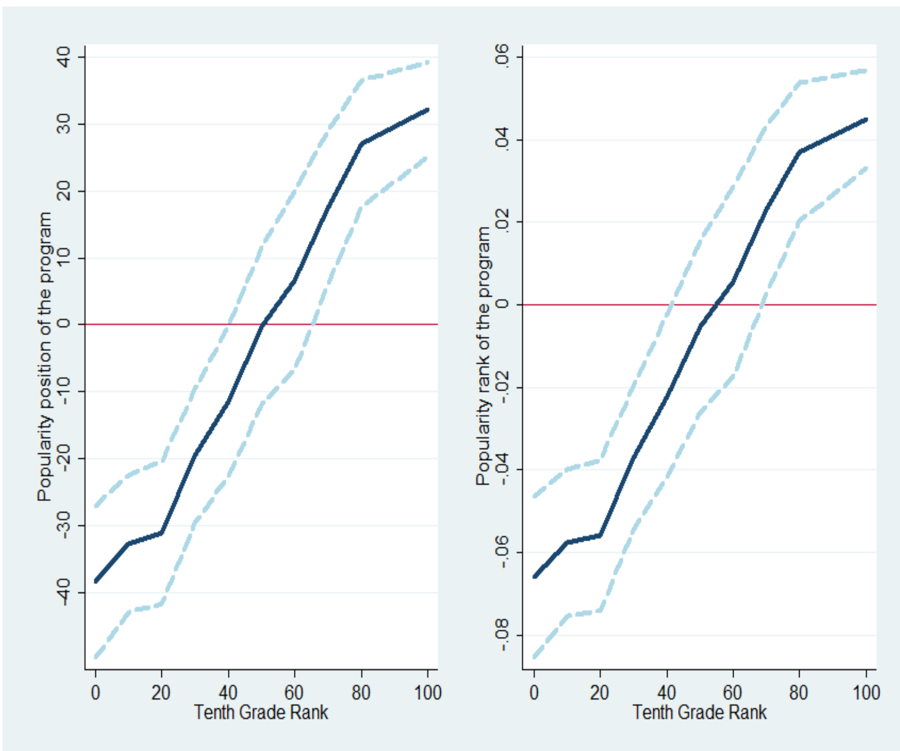

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Figure 2.13: Treatment effects on the annual expected earnings

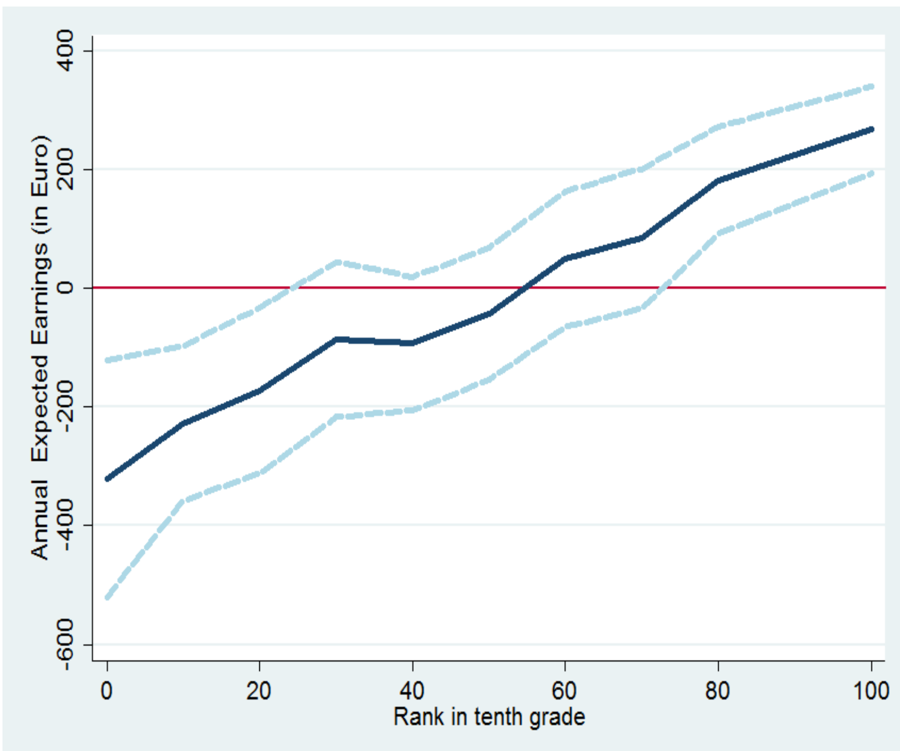

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Figure 2.14: Positive and Negative Surprise

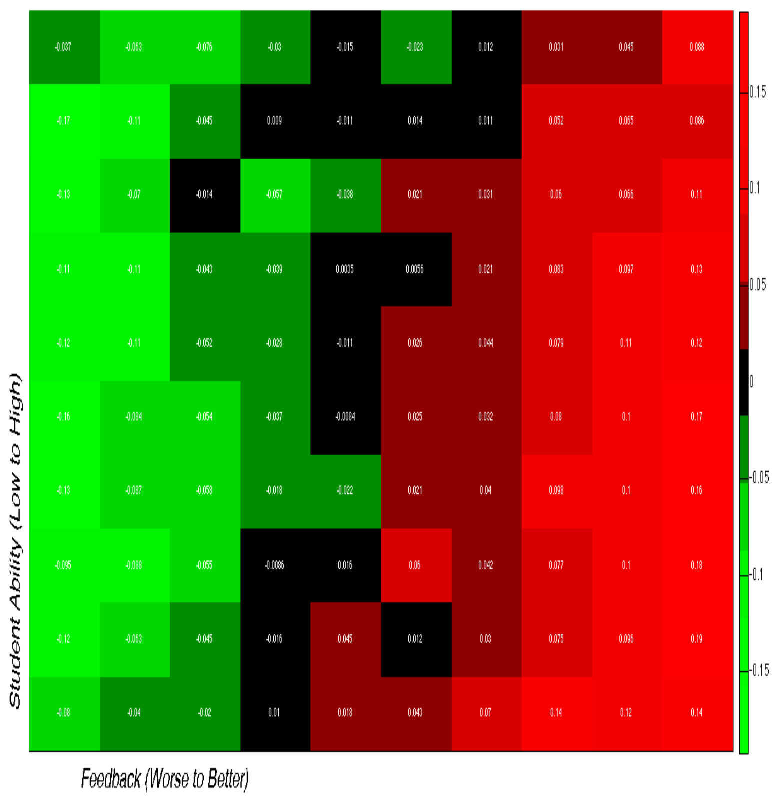

Figure 2.15: Treatment effects on the rank nationwide by school quality conditional on prior performance

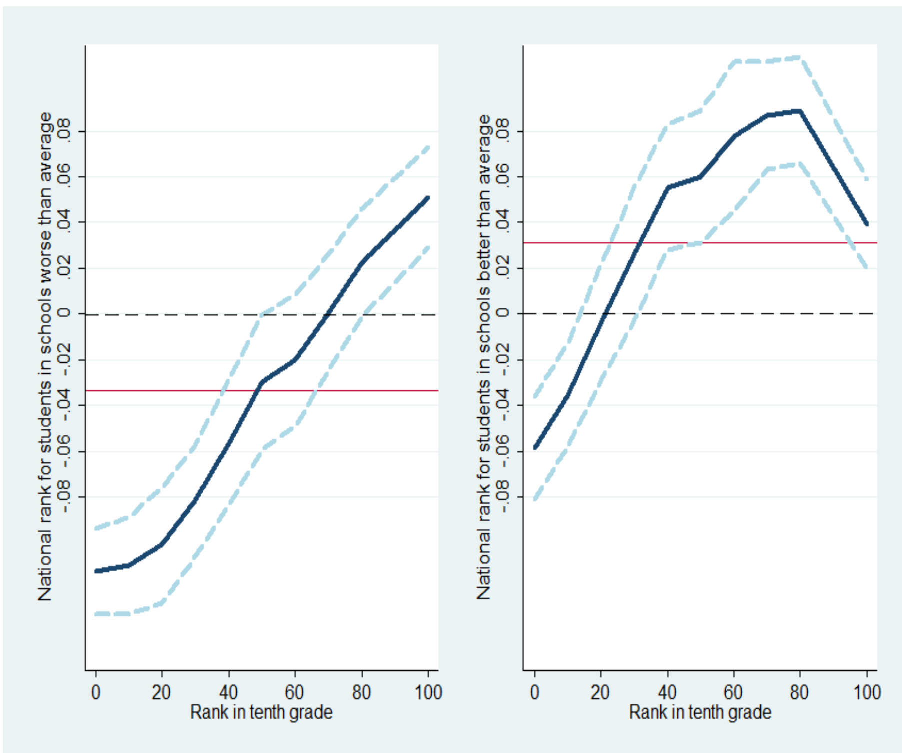

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Figure 2.16: Treatment effects on the standardised score by school quality conditional on prior performance

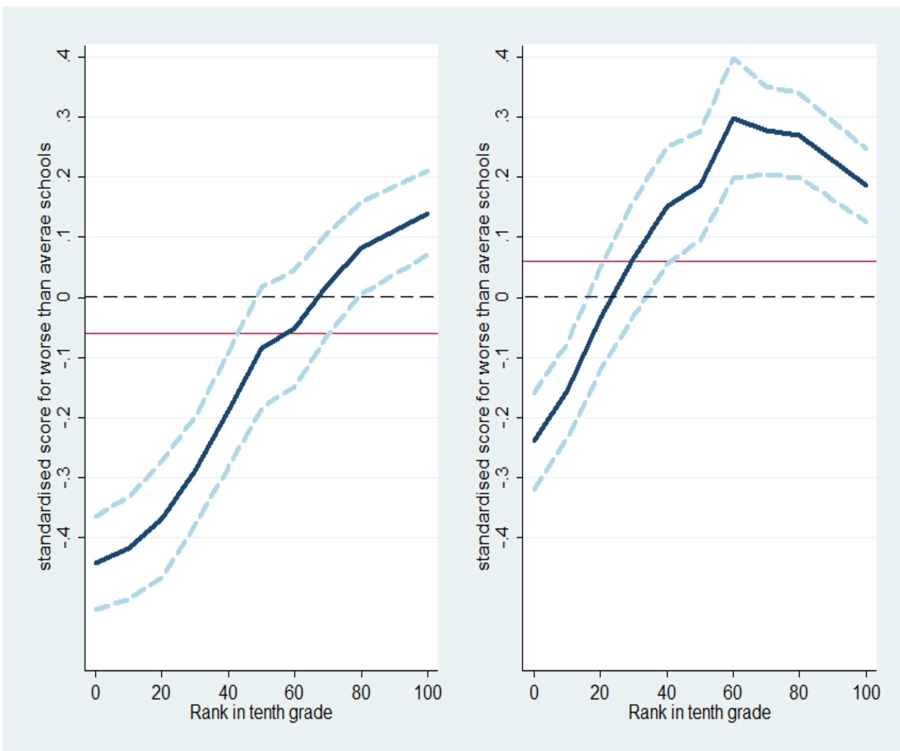

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Figure 2.17: Treatment effects on twelfth grade national rank for the bottom and top quintiles of neighborhood income

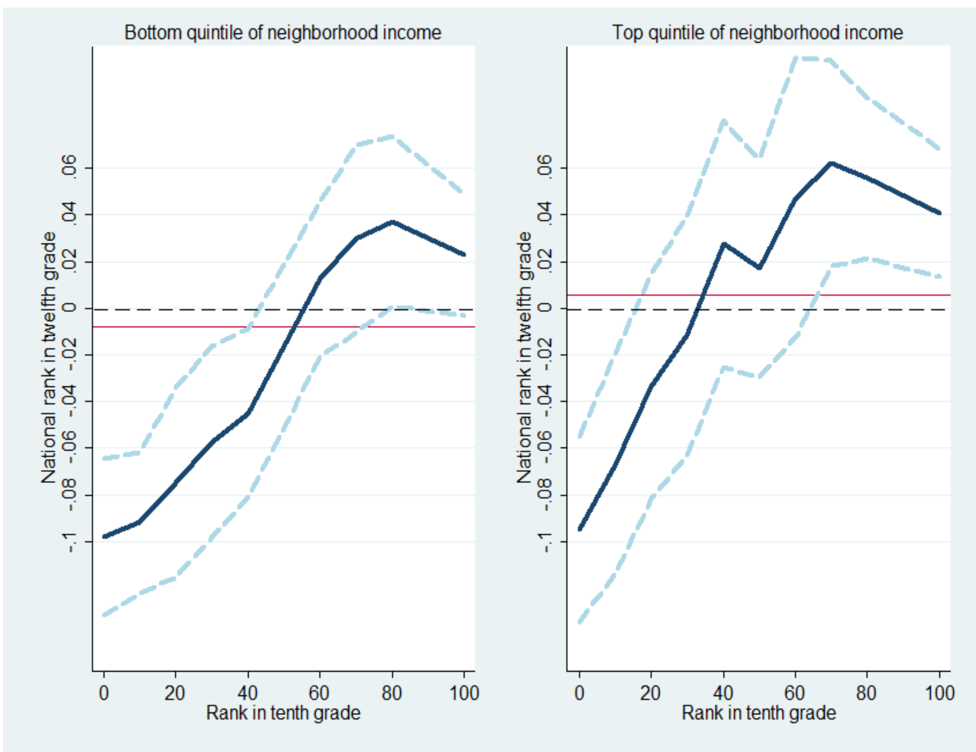

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Figure 2.18: Treatment effects on the rank within the school conditional on prior performance for schools of different capacity

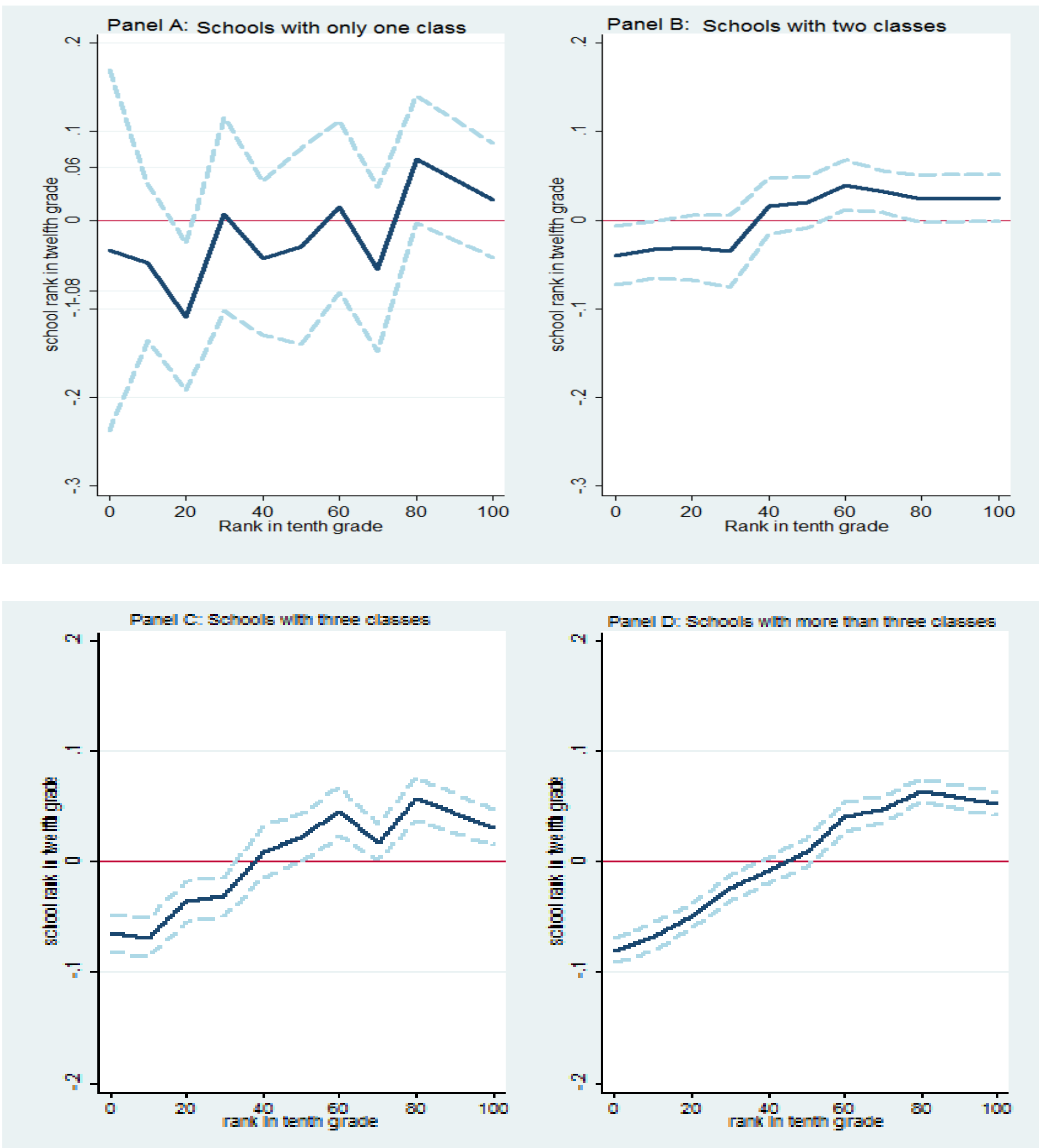

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Figure 2.19: Drop out rates for each quintile of students' prior performance

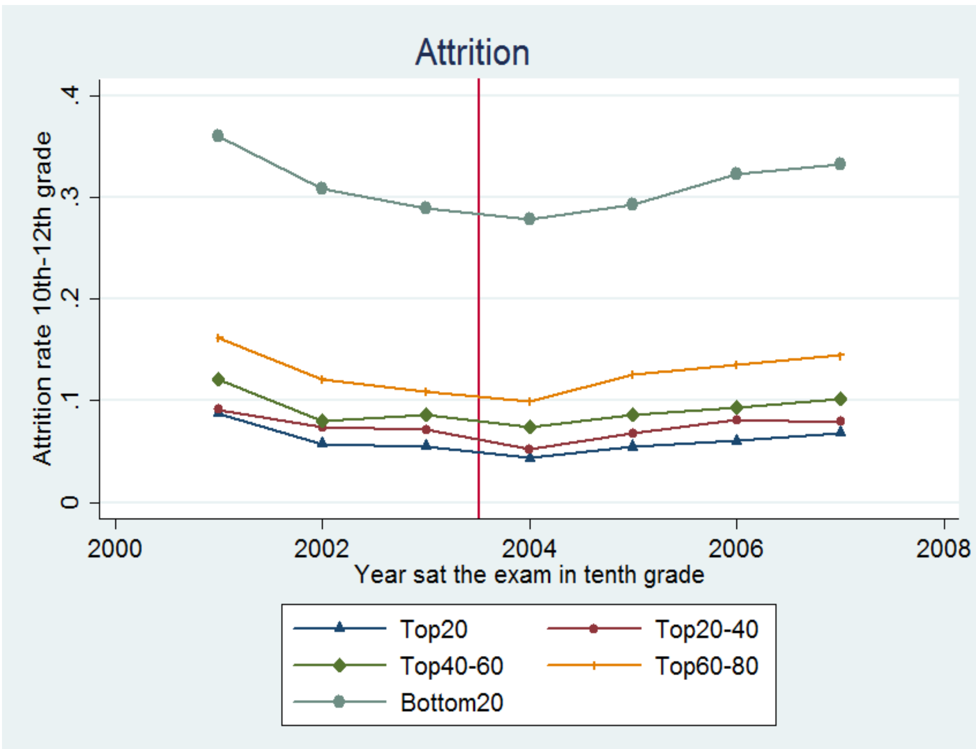

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Figure 2.20: Placebo Tests

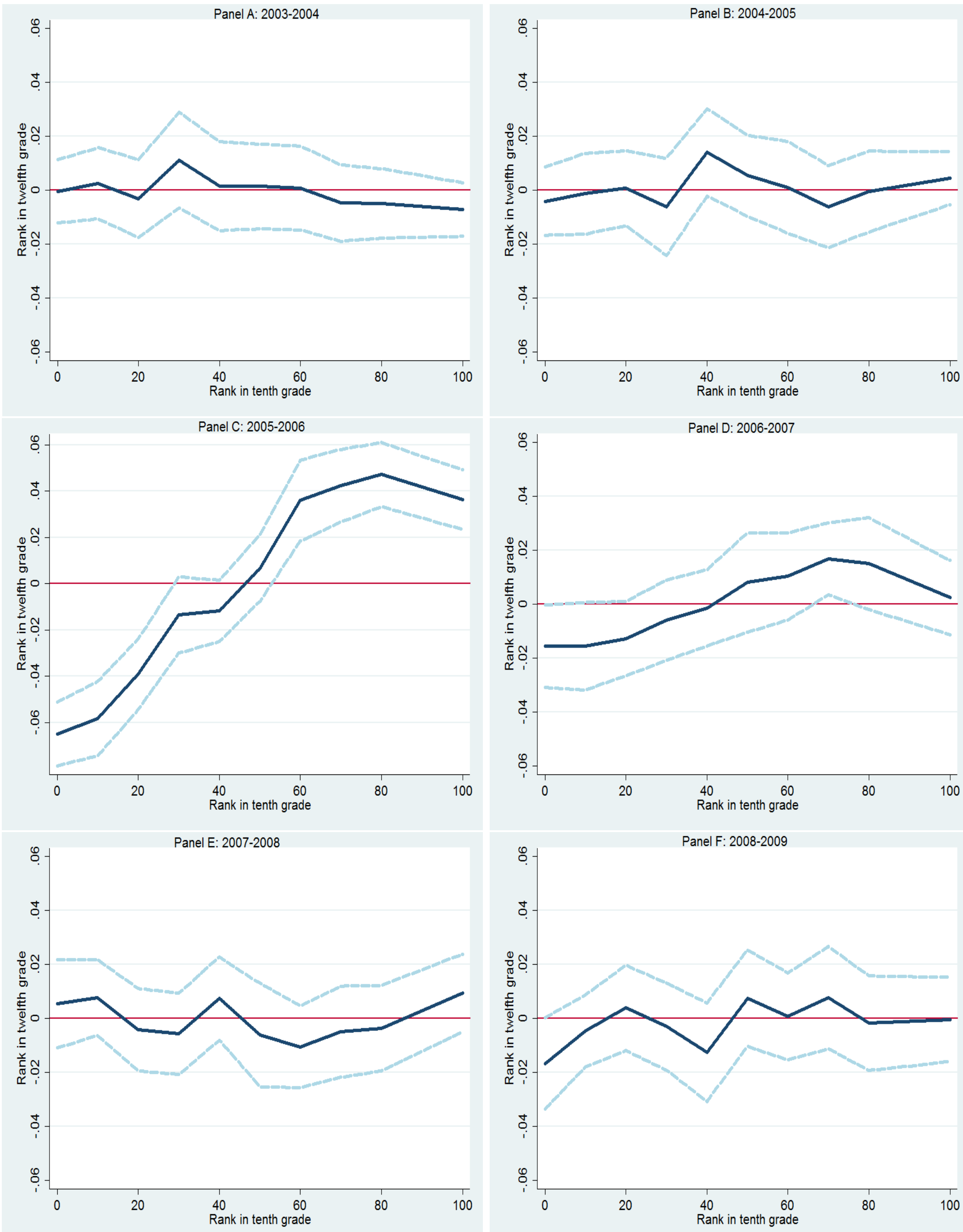

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Figure 2.21: Feedback effects on twelfth grade rank for each subject separately

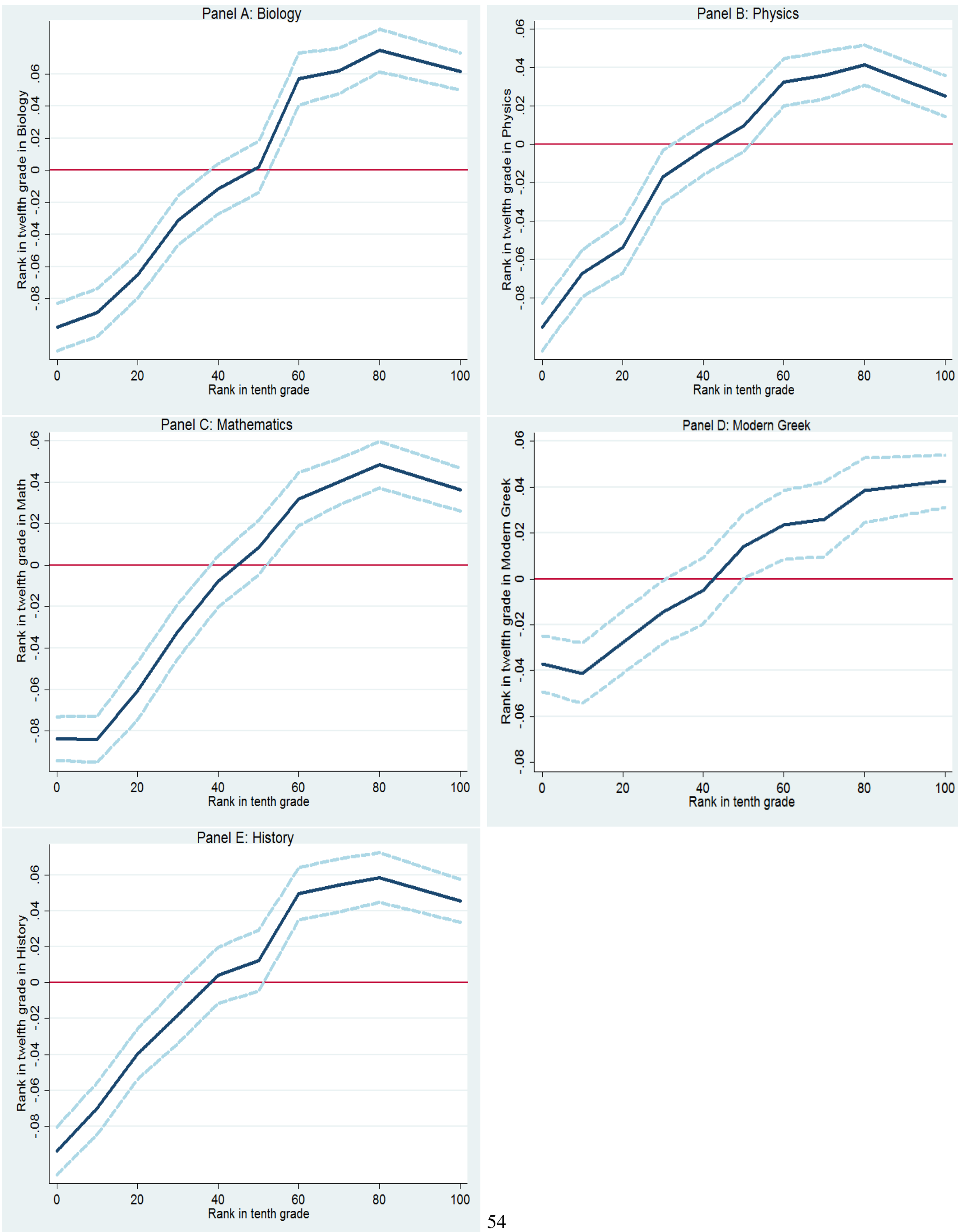

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Table 2.1: Descriptive Statistics in twelfth grade

Variable	Mean	Std. Dev.	Min.	Max.
Student Characteristics				
Age	17.875	0.466	17	27
Early enrollment	0.167	0.373	0	1
Female	0.566	0.496	0	1
School cohort size	78.518	31.17	10	170
School GPA	85.930	10.186	49.44	100
National exam grade	62.843	19.362	7.550	98.857
Cohort size	63,186	8,710	50,061	71,796
Retake the national exam	0.115	0.319	0	1
Specialty Characteristics				
Specialty:Classics	0.359	0.48	0	1
Specialty:Exact Sciences	0.164	0.371	0	1
Specialty:Information Technology	0.477	0.499	0	1
School Characteristics				
Private School	0.039	0.193	0	1
Experimental School	0.061	0.24	0	1
Public School	0.9	0.3	0	1
Urban	0.973	0.161	0	1
logIncome(in 2009 Euro)	9.999	0.270	9.473	11.105
University Admission				
Admitted	0.823	0.381	0	1
College district different from school district	0.677	0.468	0	1
Number of university departments	8.293	10.543	1	242
Rank of admitted college in preference list	24.699	21.618	1	254
Places in tertiary education	60,960	6,268	52,450	68,136

Note: 45,746 obs. 7 cohorts. The variable ”places in tertiary education” is calculated as the average across admitted students.

Table 2.2: Sample and Population

Variable	Sample (134schools) Mean	Population (1189schools) Mean	Difference (b/s.e.)
Student Characteristics			
Age	17.875	17.892	-0.017***
			(0.003)
Early enrollment	0.167	0.167	-0.0004
			(0.002)
Female	0.566	0.565	0.002
			(0.003)
School cohort size	78.518	75.358	3.160***
			(0.197)
Specialty: Classics	0.359	0.366	-0.007
			(0.004)
Specialty: Exact Sciences	0.164	0.159	0.005
			(0.002)*
Specialty: Information Technology	0.477	0.475	0.002
			(0.003)
School Characteristics			
Private school	0.039	0.080	-0.041***
			(0.001)
Public schools	0.900	0.901	-0.001
			(0.002)
Experimental school	0.061	0.019	0.042***
			(0.001)
Urban	0.973	0.892	0.082***
			(0.002)
logIncome (in 2009Euro, annual)	9.999	9.938	0.060***
			(0.001)

Note: 45,746 obs. in sample and 431,469 obs. in population. There are in total 1,323 senior high schools in operation. Evening schools are excluded from the sample and the population.

Table 2.3: Treatment and Control Group

Variable	Feedback Mean	No Feedback Mean	Difference (b/s.e.)
Student Characteristics			
Age	17.835	17.909	0.074***
			(0.004)
Early enrollment	0.209	0.129	-0.080***
			(0.004)
Female	0.553	0.579	0.026***
			(0.005)
School cohort size	88.083	70.030	18.053***
			(0.288)
Specialty: Classics	0.344	0.377	0.033***
			(0.004)
Specialty: Exact Sciences	0.176	0.154	-0.022***
			(0.004)
Specialty: Information Technology	0.480	0.469	-0.011**
			(0.005)
School Characteristics			
Private school	0.037	0.037	0.0003
			(0.002)
Public schools	0.905	0.897	-0.008
			(0.005)
Experimental school	0.058	0.066	0.007
			(0.005)
Urban	0.972	0.974	0.002
			(0.002)
logIncome (in 2009Euro,annual)	9.988	10.005	0.017***
			(0.003)

Note: 21,965 obs. in treatment group and 23,781 obs. in control group. The feedback period is the pooled period from 2003 to 2005 while the non-feedback period consists of the pooled period from 2006 to 2009.

Table 2.4: Estimation results: Rank nationwide

Dependent Variable: Rank nationwide in incentivized subjects			
	Specifications		
Variable	(1)	(2)	(3)
Feedback*quintile5	0.042***	0.043***	0.045***
	(0.004)	(0.006)	(0.004)
Feedback*quintile4	0.036***	0.037***	0.040***
	(0.004)	(0.005)	(0.004)
Feedback*quintile2	-0.045***	-0.045***	-0.038***
	(0.004)	(0.004)	(0.005)
Feedback*quintile1	-0.088***	-0.088***	-0.079***
	(0.004)	(0.004)	(0.004)
Feedback	0.009***	0.009	-0.001
	(0.003)	(0.009)	(0.003)
quintile5	0.234***	0.235***	0.251***
	(0.003)	(0.004)	(0.004)
quintile4	0.094***	0.094***	0.102***
	(0.003)	(0.003)	(0.003)
quintile2	-0.081***	-0.083***	-0.093***
	(0.003)	(0.003)	(0.003)
quintile1	-0.176***	-0.177***	-0.192***
	(0.003)	(0.003)	(0.003)
Female	-0.008***	-0.008***	-0.011***
	(0.001)	(0.002)	(0.002)
Age	-0.010***	-0.010***	-0.010***
	(0.003)	(0.003)	(0.003)
Early enrollment	-0.006**	-0.007**	-0.006*
	(0.002)	(0.003)	(0.003)
Specialty: Science	0.042***	0.041***	0.041***
	(0.002)	(0.002)	(0.002)
Specialty: Classics	-0.022***	-0.021***	-0.021***
	(0.002)	(0.002)	(0.002)
Log Income	0.055***		
	(0.003)		
Experimental school	0.029***		
	(0.004)		
Private school	0.145***		
	(0.004)		
Urban	0.021***		
	(0.004)		
Year FE.	no	no	yes
School FE.	no	yes	yes
Observations	45,746	45,746	45,746
R squared	0.635	0.666	0.675
No of schools	134	134	134

Note: A constant is also included. Clusters at school level. *,**,*** denotes significance at the 10%,5% and 1% level respectively.

Table 2.5: Rank within the school in incentivized and non-incentivized subject

Dependent Variable: School Rank in incentivized and non-incentivized subjects				
	Incentivized subjects		**Non-Incentiv. subjects**	
Variable	(1)	(2)	(3)	(4)
Feedback*quintile5	0.045***	0.045***	0.005	0.005
	(0.004)	(0.004)	(0.006)	(0.006)
Feedback*quintile4	0.040***	0.040***	-0.005	-0.005
	(0.004)	(0.004)	(0.006)	(0.006)
Feedback*quintile2	-0.038***	-0.038***	-0.004	-0.003
	(0.004)	(0.005)	(0.006)	(0.006)
Feedback*quintile1	-0.079***	-0.079***	0.005	0.005
	(0.004)	(0.004)	(0.005)	(0.006)
Feedback	0.001	-0.001	0.003	0.001
	(0.003)	(0.003)	(0.004)	(0.004)
quintile5	0.251***	0.251***	0.256***	0.256***
	(0.004)	(0.004)	(0.005)	(0.004)
quintile4	0.102***	0.102***	0.103***	0.105***
	(0.003)	(0.003)	(0.003)	(0.004)
quintile2	-0.093***	-0.093***	-0.094***	-0.095***
	(0.003)	(0.003)	(0.004)	(0.005)
quintile1	-0.193***	-0.192***	-0.200***	-0.200***
	(0.003)	(0.003)	(0.004)	(0.006)
Female	-0.009***	-0.011***	0.054***	0.054***
	(0.001)	(0.002)	(0.003)	(0.002)
Age	-0.010***	-0.010***	0.002	0.002
	(0.003)	(0.003)	(0.002)	(0.002)
Early enrollment	-0.006*	-0.006*	0.006	0.007
	(0.003)	(0.003)	(0.005)	(0.005)
Specialty: Science	0.047***	0.048***	0.033***	0.034***
	(0.001)	(0.002)	(0.002)	(0.003)
Specialty: Classics	-0.019***	-0.021***	0.097***	0.097***
	(0.001)	(0.002)	(0.002)	(0.003)
Log Income	0.051***	0.049***	0.007	0.007
	(0.0004)	(0.001)	(0.004)	(0.004)
Experimental school	-0.041***	-0.038***	-0.003	-0.004
	(0.002)	(0.004)	(0.004)	(0.003)
Private school	-0.003	-0.004	0.030	0.032
	(0.003)	(0.004)	(0.016)	(0.018)
Urban	-0.017***	-0.016***	-0.003	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)
Year FE.	no	yes	no	yes
Observations	45,746	45,746	45,746	45,746
R squared	0.674	0.675	0.542	0.543
No of schools	134	134	134	134

Note: Standard errors are clustered at the school level. A constant is also included. *,**,*** denotes significance at the 10%,5% and 1% level respectively.

Table 2.6: Estimation results : Differential Response by Gender

Dependent Variable: Rank in twelfth grade				
	Rank within the school		**Rank nationwide**	
Variable	(1)	(2)	(3)	(4)
Female*Feedback	-0.028***	-0.028***	-0.027***	-0.027***
	(0.005)	(0.005)	(0.005)	(0.005)
Female	0.054***	0.054***	0.052***	0.052***
	(0.003)	(0.003)	(0.003)	(0.003)
Feedback	0.009***	0.002	0.009	0.008
	(0.003)	(0.004)	(0.008)	(0.009)
Speciality in Science	0.198***	0.199***	0.198***	0.196***
	(0.004)	(0.004)	(0.004)	(0.004)
Speciality in Classics	-0.040***	-0.039***	-0.039***	-0.040***
	(0.003)	(0.003)	(0.005)	(0.003)
Age	-0.051***	-0.051***	-0.060***	-0.060***
	(0.005)	(0.005)	(0.005)	(0.005)
Early enrollment	-0.047***	-0.048***	-0.058***	-0.058***
	(0.005)	(0.005)	(0.005)	(0.005)
Income	-0.0001	-0.0001	0.0002***	0.0002***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Private	-0.015*	-0.015*	0.134***	0.134***
	(0.008)	(0.008)	(0.016)	(0.017)
Experimental	-0.015**	-0.015**	0.017	0.017
	(0.006)	(0.006)	(0.018)	(0.018)
Urban	-0.029***	-0.029***	0.007	0.007
	(0.007)	(0.007)	(0.015)	(0.015)
R^2	0.14	0.14	0.16	0.16
N	45,746	45,746	45,746	45,746
Year FE		✓		✓
No of schools	134	134	134	134

Note: Standard errors are clustered at the school level. A constant is also included. *,**,*** denotes significance at the 10%,5% and 1% level respectively. The rank in the twelfth grade here takes into account only the incentivized subjects. It is calculated within the school for columns (1) and (2) and across schools in columns (3) and (4)

Table 2.7: Capacity of schools

Variable	Mean	Std. Dev.	Min.	Max.
Schools with one class				
Public	0.899	0.302	0	1
Private	0.101	0.301	0	1
Experimental	0	0	0	0
Urban	0.378	0.485	0	1
Class size	18.130	5.717	10	29
No of schools	14			
No of students	522			
Schools with two classes				
Public	0.932	0.252	0	1
Private	0	0	0	0
Experimental	0.068	0.252	0	1
Urban	0.832	0.375	0	1
Class size	16.000	4.739	10	27
No of schools	38			
No of students	3,709			
Schools with three classes				
Public	0.941	0.235	0	1
Private	0.053	0.223	0	1
Experimental	0.006	0.077	0	1
Urban	0.986	0.115	0	1
Class size	18.211	4.998	10	32
No of schools	63			
No of students	9,959			
Schools with three classes				
Public	0.881	0.324	0	1
Private	0.035	0.184	0	1
Experimental	0.084	0.277	0	1
Urban	1	0	0	1
Class size	20.072	6.973	10	33
No of schools	74			
No of students	26,354			

Note: 111 senior high schools provided us with the eleventh and twelve grade classroom information. The number of classes in a school may not be stable across years. Some schools may expand and some others may shrink in some years.

Table 2.8: Loss of labor force participants

Year	Students Retaking	Potential Impact on Labour Market
2003	7925	0.167%
2004	7223	0.150%
2005	6387	0.131%
2006	10421	0.213%
2007	6642	0.135%
2008	5730	0.116%
2009	4576	0.092%
2010	7680	0.153%

Table 2.9: Decision to Retake and Feedback

Dependent Variable: Repeat the national exams				
	LPM		**Probit**	**Logit**
Variable	(1)	(2)	(3)	(4)
Feedback* Misplacement	0.058	0.059	0.345	0.602
	(0.016)***	(0.016)***	(0.092)***	(0.181)***
Feedback	0.012	0.019	0.070	0.131
	(0.006)*	(0.007)**	(0.036)*	(0.074)*
Misplacement	-0.014	-0.015	-0.071	-0.099
	(0.014)	(0.015)	(0.077)	(0.142)
Age	-0.014	-0.019	-0.076	-0.157
	(0.003)***	(0.006)***	(0.039)*	(0.062)**
Early Enrolled	-0.005	-0.006	-0.011	-0.033
	(0.008)	(0.008)	(0.022)	(0.082)
Female	-0.007	-0.007	-0.044	-0.073
	(0.003)*	(0.004)*	(0.020)*	(0.038)*
Specialization in Classics	-0.020	-0.018	-0.113	-0.200
	(0.004)***	(0.007)*	(0.024)***	(0.046)***
Specialization in Science	0.013	0.016	0.090	0.169
	(0.005)**	(0.004)***	(0.026)***	(0.049)***
District Unemployment	0.005	0.002	0.025	0.046
	(0.002)**	(0.002)	(0.012)*	(0.019)**
If admitted in first place	-0.212	-0.218	-1.041	-1.964
	(0.008)***	(0.008)***	(0.035)***	(0.070)***
Internal Migration	0.064	0.072	0.445	0.889
	(0.005)***	(0.005)***	(0.037)***	(0.077)***
logIncome	-0.009			
	(0.011)			
Urban	0.024			
	(0.013)*			
Private	-0.056			
	(0.007)**			
Public	-0.039			
	(0.009)***			
R^2 or pseudo-R^2	0.05	0.06	0.07	0.07
Log likelihood			-13,432	-13,439
School FE		✓	✓	✓
Year FE	✓	✓	✓	✓
N	45,746	45,746	45,746	45,746

Note: A constant is also included. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 2.10: Decision to Retake, Feedback and Misplacement

Dependent Variable: Repeat the national exams				
	LPM		**Probit**	**Logit**
Variable	(1)	(2)	(3)	(4)
Feedback	-0.031	-0.002	-0.007	-0.017
	(0.007)***	(0.008)	(0.047)	(0.090)
Feedback* Misplacement Quintile 5	0.045	0.040	0.219	0.412
	(0.010)***	(0.009)***	(0.050)***	(0.095)***
Feedback* Misplacement Quintile 4	0.023	0.023	0.120	0.231
	(0.010)**	(0.009)**	(0.049)**	(0.095)**
Feedback* Misplacement Quintile 2	0.004	0.007	0.049	0.103
	(0.010)	(0.011)	(0.054)	(0.101)
Feedback* Misplacement Quintile 1	-0.034	-0.031	-0.151	-0.274
	(0.010)***	(0.010)***	(0.052)***	(0.098)***
Misplacement Quintile 5	-0.017	-0.018	-0.103	-0.184
	(0.007)**	(0.007)**	(0.038)***	(0.073)**
Misplacement Quintile 4	-0.025	-0.025	-0.139	-0.262
	(0.007)***	(0.007)***	(0.038)***	(0.072)***
Misplacement Quintile 2	0.017	0.016	0.076	0.143
	(0.007)**	(0.008)**	(0.039)*	(0.073)**
Misplacement Quintile 1	0.030	0.031	0.148	0.273
	(0.007)***	(0.009)***	(0.043)***	(0.080)***
Female	-0.010	-0.010	-0.056	-0.105
	(0.004)***	(0.004)***	(0.020)***	(0.037)***
Age	0.002	-0.001	-0.001	-0.002
	(0.007)	(0.007)	(0.035)	(0.067)
Early Enrolled	0.011	0.009	0.047	0.087
	(0.008)	(0.008)	(0.041)	(0.078)
Unemployment	0.005	0.002	0.010	0.020
	(0.001)***	(0.002)	(0.011)	(0.021)
Internal migration	-0.024	-0.022	-0.109	-0.211
	(0.007)***	(0.008)***	(0.038)***	(0.075)***
Specialization in Science	-0.007	-0.004	-0.018	-0.036
	(0.005)	(0.005)	(0.025)	(0.048)
Specialization in Classics	-0.018	-0.017	-0.093	-0.175
	(0.004)***	(0.004)***	(0.024)***	(0.045)***
Private	-0.087			
	(0.011)***			
Public	-0.040			
	(0.009)***			
LogIncome	-0.033			
	(0.008)***			
Urban	0.006			
	(0.010)			
R^2 or pseudo-R squared	0.03	0.04	0.06	0.06
Log likelihood			-14,062	-14,063
School FE		✓	✓	✓
Year FE	✓	✓	✓	✓
N	45,746	45,746	45,746	45,746

Note: A constant is also included. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 2.11: Estimation results : Drop out

Dependent Variable: Dummy for drop out		
	Specifications	
Variable	(1)	(2)
Feedback*quintile5	0.009	0.010
	(0.007)	(0.007)
Feedback*quintile4	0.007	0.007
	(0.007)	(0.007)
Feedback*quintile2	0.009	0.010
	(0.008)	(0.008)
Feedback*quintile1	0.013	0.014
	(0.015)	(0.016)
Feedback	0.017	0.041
	(0.019)	(0.033)
quintile5	0.000	-0.001
	(0.004)	(0.004)
quintile4	-0.006	-0.006
	(0.005)	(0.005)
quintile2	0.025***	0.025***
	(0.006)	(0.006)
quintile1	0.153***	0.153***
	(0.003)	(0.014)
Female	-0.011***	-0.011***
	(0.003)	(0.004)
Absences10	0.001***	0.002***
	(0.0001)	(0.0001)
Year FE.	no	yes
Observations	56,041	56,041
R squared	0.130	0.203
No of schools	134	134

Note: A constant is also included. Clusters at school level. *,**,*** denotes significance at the 10%,5% and 1% level respectively. Quintiles are constructed based on the school performance in tenth grade used.

Table 2.12: Drop out rate and Transfers

	Drop out 10 11	**Transfers 10 11**	**Drop out 11 12**	**Transfers 11 12**
	%	%	%	%
2000-2001	8.89			
2001-2002	12.31	8.89		
2002-2003	11.07	8.4	6.07	7.02
2003-2004	8.87	7.93	5.14	9.47
2004-2005	10.71	7.46	6.67	6.45
2005-2006	9.41	6.35	5.67	6.58
2006-2007	12.59	9.46	6.15	8.60
2007-2008	11.71	5.85	8.63	6.80
2008-2009	13.71	7.52	6.13	6.58
2009-2010	10.56	6.45	6.01	8.38
2010-2011	9.92	5.76	6.19	8.61

Note: The first column shown the percentage of students who drop out from school between the tenth and eleventh grade. The second column shown the percentage of students who transfer to a school in the eleventh grade. The third column shown the percentage of students who drop out from school between the eleventh and twelfth grade. The fourth column shown the percentage of students who transfer to a school in the twelfth grade. Data from 134 schools are used.

Table 2.13: Estimation results: Different outcome variables

Dependent Variable: Rank in twelfth grade			
	Specifications		
Variable	(1)	(2)	(3)
Feedback*quintile5	0.026***	0.030***	0.050***
	(0.004)	(0.007)	(0.005)
Feedback*quintile4	0.022***	0.015***	0.032***
	(0.004)	(0.007)	(0.005)
Feedback*quintile2	-0.029***	-0.032***	-0.042***
	(0.004)	(0.007)	(0.005)
Feedback*quintile1	-0.052***	-0.045***	-0.066***
	(0.004)	(0.006)	(0.005)
Feedback	0.002	0.008	-0.0004
	(0.003)	(0.005)	(0.003)
quintile5	0.257***	0.247***	0.245***
	(0.003)	(0.005)	(0.003)
quintile4	0.109***	0.110***	0.107***
	(0.003)	(0.005)	(0.003)
quintile2	-0.097***	-0.100***	-0.091***
	(0.003)	(0.005)	(0.003)
quintile1	-0.207***	-0.231***	-0.210***
	(0.003)	(0.005)	(0.003)
Female	-0.019***	0.030***	-0.014 ***
	(0.001)	(0.002)	(0.001)
Early Enrollment	0.010***	0.009***	0.011 ***
	(0.002)	(0.002)	(0.002)
Specialty: Science	0.006***	0.019*** *	0.023***
	(0.002)	(0.004)	(0.002)
Specialty: Classics	0.010***	0.098***	-0.059
	(0.002)	(0.003)	(0.002)
Observations	45,746	45,746	45,746
R squared	0.661	0.674	0.625
No of schools	134	134	134

Note: A constant is also included. The outcome in the first column is the rank calculated based on the five core subjects and the four Track subjects. The outcome in the second column is the rank in Modern Greek. The outcome variable in the third column is calculated based on five subjects in the feedback regime and two subjects in the non-feedback regime. Standard errors clustered at the school level. Year fixed effects included. Clusters at school level. *,**,*** denotes significance at the 10%,5% and 1% level respectively.

Table 2.14: Descriptive Evidence of Social Mobility

Quintiles of program' popularity	Quintiles of Neighborhood Income											
	Quintile1		**Quintile2**		**Quintile3**		**Quintile4**		**Quintile5**		**Total**	
	Feed.	No Feed.	Feed.	No Feed.	Feed.	No Feed.	Feed.	No Feed.	Feed.	No Feed.	Feed.	No Feed.
Quintile1	4.6	3.4	4.2	3.2	4.5	2.9	2.7	2.1	3.1	2.7	19.1	14.3
	+1.2		+1		+1.6		+0.6		+0.4		+4.8	
Quintile2	3.9	3.5	3.5	3.1	4.1	3.5	2.5	2.7	3.1	3.2	17.1	16
	+0.4		+0.5		+0.6		-0.2		-0.3		+1.1	
Quintile3	3.6	3.4	3.2	3.2	3.8	3.5	2.7	3	3.2	3.4	16.5	16.5
	+0.2		0		+0.3		-0.3		-0.3		0	
Quintile4	3.1	3.0	2.8	3.2	3.5	4.3	2.5	3.2	3.4	3.8	15.3	17.5
	+0.1		-0.4		-0.8		-0.7		-0.4		-2.2	
Quintile5	2.9	2.6	2.6	3.0	3.9	4.1	2.8	3.2	3.6	4.2	15.8	17.1
	+0.3		-0.42		-0.2		-0.4		-0.6		-1.3	
Total	18.1	15.9	16.3	15.7	19.8	18.3	13.2	14.2	16.3	17.4	**83.7**	**81.5**
	2.2		+0.6		+1.5		-1		-1.1		**+2.2**	

Note: "Quintile 1" represents the bottom quintile of program's popularity/ prestigiousness and neighborhood income. "Quintile 5" denotes the top quintile of the program's popularity/ prestigiousness and neighborhood income. For each quintile of neighborhood income two percentages are reported: the first one represents the percentage of students who enroll in university in the feedback period and the second one the corresponding percentage in the non-feedback period. The differences between the percentage in the feedback period and the non-feedback period for each quintile of program's popularity prestigiousness are also reported.

CHAPTER 3

SOCIAL INTERACTIONS IN COLLEGE ENROLLMENT AND ACADEMIC MOBILITY

3.1 Introduction

In the recent years the literature on the role of social interactions in economic behavior has expanded rapidly. This doesn't come as surprise when one thinks the importance of those effects in every day decision-making. The basis of decision-making though in almost every context is information. Humans are social beings and we naturally collect information through social interactions in order to inform our goals and choices. This is even more pronounced among adolescents. In developmental science, it has been widely argued that adolescents and young adults regularly mimic the choices and behavior of role models in their environment (Bell (1970)).

Brock and Durlauf (2001) define social interactions as the idea that an individual's marginal utility with respect to other individuals' choices in his reference group is positive. The desire to conform induces prevalent patterns of behavior even among agents with heterogeneous tastes over externalities from other individuals' choices (Bernheim (1994)). Social interactions within a reference group have been shown to affect students' achievement. However, there is little evidence on the effect of social interactions on the decisions of college enrollment and academic mobility. Moreover, social interactions can explain variation in choices across groups with similar characteristics. For example, Schelling (1973) provide early evidence of social interactions in binary choice in a profusion of contexts such as driving style and athletic play. Intuitively, conformity causes social interactions to be interconnected with neighborhood effects. Physical proximity amplifies the interplay of utility spillovers from other agents' choices and the combined effect becomes area specific. In an educational context, Garner and Raudenbush (1991) provide evidence of a positive relation between neighborhood quality and educational attainment.

There is evidence that peers' decision affect scholastic performance in elementary, middle and high school but also during college. Hoxby (2000a) examines the effect of social interaction in grade school and finds that students who were randomly assigned to classes with students who have high reading scores relative to the school and grade, received higher reading scores. Hanushek, Kain, Markman, and Rivkin (2003) find that peer achievement has a positive effect on achievement growth. In particular, 0.1 standard deviation increase in peer average achievement leads to a 0.02 increase in student's performance. Zimmerman (2003b) examines the effect of social interaction using freshmens' SAT score. He finds strong positive social interaction effects among roommates at almost all parts of the ability distribution. Cipollone and Alfonso (2007) find strong social interactions inter alia the decision to stay longer in school. When men were exempted from the compulsory military services -due to an earthquake- and stayed longer in school, the graduation rates of young women in the affected areas rose by about 2 percentage points. Fletcher (2006) using survey data, finds strong evidence of social interactions college preferences and college enrollment. Giorgi, Pellizzari, and Redaelli (2007) find that ones' behavior influences the educational decision while in college, indicating the importance of social interaction even at a later stage of someone's academic life. Sacerdote (2011) examines social interaction effects at the room and accommodation level where students are randomly assigned. He does not find any significant influence of peers.

In this paper we examine the effect of social interactions on the decisions of adolescents and young adults regarding college enrollment and academic mobility. We use a new dataset from Greece that contains information on exam scores, college enrollment and educational mobility for every student in six cohorts. We exploit the particular institutional setting in Greece, in which schools are build very close to each other. This setting allows for rich variation of school characteristics within a relatively contained geographical area. We exploit this exogenous variation in group characteristics over time and space to address the endogenous nature of the social interaction groups. The social interaction effects are defined as contextual interactions that induce different mappings from individual characteristics to outcomes (Bryk and Raudenbush (2001)). Reference groups are viewed as ecologies in which the social backgrounds affect individual choices of otherwise similar agents (Raudenbush and Sampson (1999)).

Similar age peers in one's vicinity consist a natural reference group that provide valuable and otherwise costly information, necessary in academic decision making. We widen the reference group and examine social interactions with respect to a series of reference groups: same-cohort school peers, different-cohort school peers, same-cohort peers in the neighborhood and different-cohort peers in the neighborhood.

There are particular advantages in having the universe of high school graduates for a country. First, we can observe the behaviour of all students regarding their education decisions and not only of specific groups of students. Second, we are able to observe different reference groups. A student may be affected by the decisions of same age or older peers in his school and neighborhood. We contribute to the literature by comparing the size of the social interaction effects across distance in space and age.

Empirical analysis of social interactions on students' decisions has been open to question because of the difficulties in disentangling these effects from other confounding influences.[1]. We use

[1] The existing literature that deals with identification of the social comparison effects use either laboratory experiments (Armin and Andrea (2006)), natural experiments (Zimmerman (2003b)), quasi-experimental designs (Hoxby (2000a)), or fixed effects (Hanushek et al. (2003)

an instrumental variable approach and we exploit spatial and time variation to combat potential endogeneity problems and the well known ”reflection-problem” (Manski (1993), Manski (2000)). There are two sources of potential endogeneity: Self selection into social groups and common shocks that affect every member of a social group. Reflection may arise from reverse causality between the outcomes of members in the same groups and their decisions are simultaneous. In other words, it is difficult to disentangle if one's actions are the cause or the effect of his peers' influence. These challenges are standard in the social interactions literature. The institutional setting behind our study refrains students from endogenously select their peers in school, facilitating the validity of the identification strategy. Moreover, the geographical density of schools allows us to define social groups wider than a student's schoolmates. Motivating from the idea of role modelship, we battle the simultaneity challenge by investigating social interactions between peers in consecutive cohorts.

By using multiple cohorts and conditioning on school and neighbourhood fixed effects as well as school-and neighborhood- specific time trends we are able to control for unobserved time-varying factors that might confound peer effects in schools and neighbourhoods. We use an instrumental variable approach to combat endogeneity and reflection. We show that within schools and neighbourhoods, there is considerable cohort-to-cohort variation in the proportion of female students that can be attributed to random factors.

We find positive spillover effects between one's decision to enrol in college and that of their peers. More specifically, the results found here indicate that students who attend a high school with a hundred percent more schoolmates who enrol in college are 12.6 percent more likely to attend college. We also find positive spillovers regarding the decision of educational mobility. Students are 10.7 percent more likely to move to a different city to study if their older peers in school do so, a hundred percent more often. We find that these externalities decrease with the size of reference group.

The policy implications of social interactions can be indirect. The skills and resources that characterise a reference group are usually fixed. As a consequence, an improvement in someone's group characteristics means an equivalent deterioration in someone else's group attributes. Some may argue that the redistribution in favor of disadvantaged students can act as a boost in their scholastic outcomes, when the redistribution comes from more advantaged areas where students might depend less on their peers' quality. For example, Arcidiacono and Nicholson (2005) suggest that the existence of social interaction effects supports claims against school vouchers. This is because, the best students leaving public schools can be detrimental to the students left behind.

The paper is organized as follows. Section 2 describes the unique dataset used and the institutional setting related to college admission. The empirical strategy used to identify social interactions is analysed in Section 3. We present and discuss the results in college enrollment and educational mobility in Section 4. Finally, Section 5 concludes.

3.2 Data and Institutional Setting

3.2.1 How are students admitted to college

The transition from high school to post-secondary education in Greece is based on an unusually systematic and transparent allocation of student to university departments[2] In particular, every high school student who completes the twelfth grade receives an admission score, which is the only criterion for university admission and weights: (i) her performance in national twelfth grade exams [3] (ii) her grade twelve within school performance which is a combined score for homework and midterm exams in each subject.

After receiving their admission scores, students are required to submit a list of ranked choices of specific departments in universities that are relevant to their twelve grade track. For example, students outside the Classics track cannot list Law schools. Each university department generally offers one major of bachelor degree and no minor specializations can be declared. Every university department admits a pre-specified number of students. A computerized system at the Ministry of Education ranks students by their admission score and assigns the highest ranked student to her preferred choice. It then moves to the next student and assigns her to the first department in her list in which there is an available place, and so and so forth. In this context, students have incentives to truthfully reveal their preferences.

University departments must enrol the students assigned to them by the Ministry of Education. The Ministry of Education announces the score of the last admitted student in each university department. The last admitted students in more prestigious departments have generally higher scores in comparison to those in less prestigious ones. Once a student admitted they cannot transfer to a different major. College education is completely publicly funded and every student is exempted for college fees. Private donations to colleges are against the law.

3.2.2 Data

For the empirical analysis we construct a unique dataset of all students graduating from high school in Greece from 2003 to 2009. We obtain the information from various sources:

1. Administrative data from the Hellenic Ministry of Education containing course taking information and exam grades in the final year, gender, year of birth, graduation year and college

[2] Every tertiary education institute in Greece is public as free education is a constitutional right. Degrees awarded by private colleges are not recognized by the state.

[3] The twelfth grade exams are written exams administered nationally only once every year and last from late May to early June. The exams are proctored and marked externally. Exam markers do not observe the name, school, or even the city of the student whose paper they grade. Students usually take six component exams, with a combination of common subjects(Language, Mathematics, Physics, Biology or History) and four compulsory track-specific subjects and one elective exam. There are three tracks: Classics, Natural Sciences and Technical Studies. The overall score is the unweighted average of these scores. Students who fail are allowed to retake the exam the next year. In addition, students are not allowed to take the national exams early.

admission information. In addition, the total number of places in tertiary education in each year is provided.

2. School specific information such as name of school, type of school (private, public[4], experimental[5]), geographical coordinates and name of prefecture it belongs to[6]. There are 1319 high schools in Greece[7].

3. The Ministry of Finance provided us with average net income information at the postcode of the school in 2009 Euro.

4. The Ministry of Internal Affairs provided us with urban density information. Urban areas are those with more than 20,000 inhabitants.

5. Geographical coordinates for every tertiary education institute in Greece. There are fifty five college campuses. Not all campuses offer the same majors.

The median distance of a school from each nearest neighbouring school is 0.32 miles.[8] We use cluster analysis to define and construct neighborhoods within a mile from each school. We construct 406 clusters that cover the whole country. Every cluster is a neighborhood that contains all twelve-grade students who attend any other high school within a mile (1.06 miles) radius from one's high school[9]. Figures 1 maps all high schools and tertiary education institutes in our dataset.

Our analysis uses information regarding characteristics and choices of older school peers. Because of this, we use data on student cohorts from 2004 to 2009[10] Furthermore, our discussion of academic mobility refers to the decisions of students to move to a different prefecture in order to study, given they were admitted to some college. Thus, for this part, we focus only on admitted

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[4]Students are assigned to public schools according to a school district system

[5]Admission to experimental schools is based on a lottery

[6]There are fifty two prefectures in Greece. prefectures are classified by the Hellenic National Statistical Authority

[7]Of which, 112 are private, and 1207 public. Of those 1207 public schools, 23 are experimental. There are no private experimental schools in Greece. 74 evening high schools for employed people of usually older age are excluded from our analysis

[8]Mean of distance from nearest neighbour: 1.85 miles. Standard deviation: 18.37 miles. 25th percentile:0.07 miles. 75th percentile: 0.77 miles.

[9]We exploit the fact that many schools were built very close to each other in most urban settings in Greece. This is more prevalent in Attica, the region surrounding the city of Athens, the capital of Greece. To give an example, in the cartier of Grava in Athens, there are six high schools next to each other along with several elementary and middle schools that form a humongous school building complex. According to the 2001 census, Attica holds around 36 percent of the total population.

[10]The first cohort in our sample, 2003 (size: 59,102 obs.), is used as a reference group for the 2004 cohort.

students[11]. Lastly, we drop 35,808 obs. for which the group of schoolmates overlapped perfectly with the social group of their neighborhood. This exclusion allows us to compare spillover effects from social groups of various sizes. We consolidate our sample by dropping observations with missing values.

Table 4.11 describes our pooled data across cohorts. Fifty seven percent are females. Ninety percent of the students reside in urban areas. More than 90 % of schools are public. Although, mean postcode income among private schools is significantly higher compared to public schools, mean national exam score doesn't seem to differ much. Experimental schools are in more affluent areas in comparison to other public schools as revealed by their higher mean postcode income. The mean national exam score of students attending experimental schools is much higher than the score achieved by students in private or public schools. Each neighbourhood contains on average 4 schools and 929 student observations.

3.3 Empirical Strategy

We start off by defining one's reference group as his same-cohort school peers. We investigate the hypothesis that social or collective behaviour patterns drive individual preferences because agents derive utility from conformity or provide access to information.

In particular, we investigate whether a student's decision to enrol in college depends on the decision of his peers in school by using the following regression:

$$IfEnrolled_{i,s,t} = \alpha + \gamma\overline{IfEnrolled_{i-1,s,t}} + \beta X_{i,s,t} + \kappa T_t + \mu S_s + \pi_s year + \epsilon_{ist}(1)$$

where $IfEnrolled_{i,s,t}$ takes the value one if student i in school s and year t enrols into college and $\overline{IfEnrolled_{i-1,s,t}}$ is the fraction of all other students except of student i in school s and year t, who enrol into college. So, we regress a student's i decision to enrol in college on the mean enrollment of his peers in school s in year t and other controls. Our covariates include a dummy for being female, a student's admission score, dummies for chosen track in the senior year of high school, dummies for the school each student attended, school specific time trends and year dummies. To control for time-varying unobserved factors that may be correlated with mean college enrolment we include a full set of school-specific linear time trends.

The main coefficient of interest is γ, which captures how the mean enrollment of one's school peers affects his decision to enrol in college. Initially, we employ ordinary least squares to estimate peer effects in education decisions. There are at least two sources of potential bias here: (1) endogeneity and (2) the reflection problem (Manski (1993), Manski (2000)).

[11] In the academic mobility analysis we exclude 60,356 students who did not enrol in college

Firstly, in many settings individuals self-select themselves into a specific group of peers that generates endogeneity issues if the variables that are responsible for this choice are not fully observable. Students who choose to attend the same high schools might share the same observed and unobserved characteristics. In this case, if we find a relationship between the observed characteristics and the outcome variable it might not be causal. This could be coming from the fact that unobserved characteristics might also affect the outcome variables. This potential unobserved heterogeneity that drives selection into social groups may bias our estimates. Nevertheless, self selection of students into schools is restricted in our setting because students are assigned to public schools[12] based on geographical criteria and they cannot choose their school peers endogenously, by construction. Therefore, social group membership is as good as random, since it does not depend on observables.

Endogeneity may also result from unobserved common group effects, such as teacher and school quality, that affect every student in a social group and render the identification of social interactions challenging. We contribute to the literature by mitigating the endogeneity challenge that stems from common group shocks. We take advantage of a special institutional setting with rich spatial and over time variation in school characteristics. We use cluster analysis to construct geographical units wider than the school district; namely neighborhoods. Those geographical units are big enough to allow for school diversity but also compact enough to capture common behavioral patterns in the area. In particular, we exploit our special institutional setting to identify same-cohort peers who do not attend the same school. We identify same-cohort peers who live in 1 mile radius and attend different schools. We call this group of same-cohort peers who live very close to each other ”neighbours”. In addition to their same-cohort schoolmates, students are likely to interact with their same-cohort neighbours and they might also be affected by their decisions. A students’ neighbours attend different schools and face different school environments. In each neighbourhood, there are students who attend on average four different schools (Table 1). The basic idea here is to compare students’ decisions from consecutive cohorts who have similar characteristics and face the same neighbourhood environment but attend different school, except for the fact that one cohort has more female students than the other. Thus, it becomes feasible to isolate the impact of a peer group from the impact of each student’s school itself.

Second, reflection may arise because we cannot distinguish whether someone’s action is the cause or the effect of his peers’ outcomes. In other words, one’s decision is simultaneous with that of his peers. We battle the simultaneity challenge by using as an IV the *time lagged* gender composition in the school and neighborhood level. So we compare the decisions of students from consecutive cohorts who had more one-cohort-older peers in their school or neighbourhood who enrolled in college. To build some intuition here, peers one-cohort-older might provide information

[12] 92 % of students in our sample attend public or public experimental schools

to younger peers about the costs and the benefits of attending college or migrating to another city or they might function as "role models".

Estimating equation (1) using OLS will lead to biased results. In order to address these concerns we propose the proportion of girls in someone's reference group in the previous period as a source of variation for mean enrollment in college. The intuition is that an individual's academic decision may be related with their gender, but not the gender composition of their environment. This satisfies the exclusion restriction for the validity of our instrument.

We control for unobserved characteristics of schools, students and neighbors that are correlated with the percentage of females that could also be correlated with students' performance by exploiting variation in the gender composition across consecutive years within the same school and neighbourhood. By using multiple cohorts and controlling for school fixed effects, we take into account unobserved factors that might invalidate the school and neighbourhood peer effects analysis.

The first stage regressions are:

$$\overline{IfEnrolled_{-i,g,t}} = \phi_1 + \kappa_1 \overline{IfFemale}_{-i,g,\mathbf{t}} + +\beta_1 X_{i,g,t} + T_t + S_g + \pi_{1g} year + e_{1,-i,g,t} (2)$$

$$\overline{IfEnrolled_{-i,g,t-1}} = \phi_2 + \kappa_2 \overline{IfFemale}_{-i,g,\mathbf{t\text{-}1}} + +\beta_2 X_{i,g,t} + T_t + S_g + \pi_{2g} year + e_{2,-i,g,t-1} (3)$$

$$g \in \{\{school\}, \{neighborhood\}\}$$

where $\overline{IfFemale_{-i,g,t}}$ and $\overline{IfFemale_{-i,g,t-1}}$ is the proportion of females in geographical unit g (school and neighborhood) and year t and year t-1 respectively. The basic idea here is to compare the collective decisions of students (to enrol in college and migrate to another city to pursue tertiary education) from consecutive years who have similar characteristics but the percentage of female peers varies from one year to another. Using the proportion of girls in someone's last year's reference group as an IV relies on the assumption that this proportion has no other effect on someone's decision to enrol in college than through its effect on last year's mean college enrollment and thus this year's someone decision to enrol in college.

The second stage regressions are as follows:

$$IfEnrolled_{igt} = \delta 1 + \kappa_1 \overline{IfEnrolled_{-i,g,\mathbf{t}}} + \psi_1 X_{i,g,t} + T_t + S_g + \lambda_{1g} year + \epsilon_{1ist} (4)$$

$$IfEnrolled_{igt} = \delta 2 + \kappa_2 \overline{IfEnrolled_{-i,g,\mathbf{t\text{-}1}}} + \psi_2 X_{i,g,t} + T_t + S_g + \lambda_{2g} year + \epsilon_{2ist} (5)$$

$$g \in \{\{school\}, \{neighborhood\}\}$$

Our key identifying assumption requires that changes in the proportion of female peers within a school and within a neighborhood are not correlated with changes in unobserved factors that could affect students' decisions. In particular, it is required that changes in the proportion of females within schools and neighbourhoods are not associated with changes in student characteristics ie. age, ethnicity income, parental education. We also provide evidence that these changes in the proportion of girls within a school and within a neighbourhood are not correlated with changes in school enrolment.

Notice that we exploit within school and within neighbourhood variation from one cohort to another. Our analysis does not look at differences in the percentage of females across schools or neighbourhoods. Additionally, we look at the effect of one's peers on their decision to enrol in college and migrate to another city. To do this, we control for one's performance in the senior year national exams.

The fact that students are assigned to schools based on distance alleviates the concern that students respond to these random shocks in gender composition by switching to another school. Students need to provide adequate evidence of residence in a given region in order to have access to the closest in terms of distance school. But even if students could switch schools, then it would be very difficult to choose the destination school based on the percentage of girls in this school for the following reason: the average percentage of female peers by school or neighbourhoods is not publicly known. But even if it was known it would be difficult to know the percentage of females for a cohort that enters the school in a specific year. Additionally, leaving a school should not be correlated to the percentage of female students in this school. It is important to note that any factor affecting the proportion of girls in all geographic units in the same way, such as a female fertility decline 17 years before, will be captured by year fixed effects and would thus not invalidate the identification strategy. We include school or neighbourhood fixed effects to control for school or neighbourhood-invariant unobserved factors respectively. One could be worried that time-varying factors ie. better teachers in some years or a new college in the neighbourhood could affect mean enrolment. To address this concern, we include school- or neighbourhood-specific time trends to control for time-varying factors that could be correlated with changes in the fraction of enrolled students in one's reference group.

Next, we turn to academic mobility. We believe that there might exist social interaction effects in the decision to migrate. We model a person's decision to move to a different city in order to pursue tertiary education, given that they were admitted to some college. This decision is a function of the average decision in one's environment as specified in our regression model:

$$IfMigrate_{igt} = \alpha_1 + \gamma_1 \overline{IfMigrate_{-i,g,t}} + \beta_1 X_{i,g,t} + \kappa_1 T_t + \mu_1 S_s + \phi_{1g} year + \epsilon_{1ist} (6)$$

$$IfMigrate_{igt} = \alpha_2 + \gamma_2 \overline{IfMigrate_{-i,g,t-1}} + \beta_2 X_{i,g,t} + \kappa_2 T_t + \mu_2 S_s + \phi_{2g} year + \epsilon_{2ist} (7)$$

where $IfMigrate_{igt}$ is the decision of student i in geographical unit g and year t and t-1 respectively to migrate in a different city in order to study, conditional on being accepted to college. $\overline{IfMigrate_{-i,g,t}}$ and $\overline{IfMigrate_{-i,g,t-1}}$ are the fractions of students except of student i who migrated to a different city in order to study in geographical unit g and year t and t-1 respectively. We include year fixed effects in order to control for time-invariant unobserved characteristics that could affect the migration decision. When we exploit the within school cohort-to-cohort change in the percentage of female students, we include school fixed effects. When we do the analysis at the neighborhood level, we include neighborhood fixed effects and we exploit the differences in school characteristics in a given year within each neighborhood.

We use an instrumental variable approach in order to estimate the effect of social interaction on the decision of students to move to another city to attend college. Again gender composition seems a likely candidate for an instrumental variable. The proportion of females in a geographical unit g may create an environment more conducive to collective migration as exhibited by average patterns of behavior but it has no direct effect on an individual's decision to migrate.

The first stage regressions is as follows:

$$\overline{IfMigrate_{-i,g,t}} = \phi + \kappa \overline{IfFemale_{-i,g,t}} + \beta X_{i,g,t} + \kappa T_t + \mu S_g + e_{i,g,t} (8)$$

$$\overline{IfMigrate_{-i,g,t}} = \phi + \kappa \overline{IfFemale_{-i,g,t-1}} + \beta X_{i,g,t} + \kappa T_t + \mu S_g + e_{i,g,t} (9)$$

$$g \in \{\{school\}, \{neighborhood\}\}$$

Our main specifications are estimated at the neighborhood level. When estimated at the geographical units of neighborhood, these specifications address both the endogeneity and simultaneity issues.

Potential threats to our analysis may include the following: Actual networks may be very different from ecologies in one's vicinity. In addition, social media may allow for peer effects that are independent of proximity and render our analysis of spatial social interactions irrelevant. This is less of a fear though as internet penetration is relatively low in Greece [13]. Parents, relatives and much older individuals in a student's environment may influence his/her academic decisions more than his/her same-cohort or one year older peers within his school and/or within his neighborhood .

3.4 Validity of Identification Strategy

Our identification strategy requires that fluctuations in the proportion of female students within a school and within a neighborhood should not be correlated with other cohort-to-cohort changes that could affect students' education decisions. In particular, we check if changes in the proportion of female students within a school and within a neighborhood are correlated with changes in students' observable characteristics. For the universe of students (N=355,808 students) the only characteristics we know are: the age of students and if a student enrolled early in school. This is the case if a student is born in the first quarter of his birth year.

However for a smaller sample of 45 schools (observations=18,670)we also know the ethnicity of students. In Table 3.4 , we present some evidence that the schools in the smaller sample have no different characteristics compared to the whole population. We cannot implement the whole analysis based on this smaller sample because we need the universe of students and schools in order to construct the neighbourhoods and exploit within neighbourhood variation. We use this smaller sample of schools to check if changes in the proportion of girls are correlated with changes in students' ethnicity.

Tables 3.2 and 3.3 provide evidence on the balancing tests for the whole sample and the subsample of the 45 schools. Table 3.2 reports the estimated coefficients from the OLS regression and a within school regression (school fixed effects) of students' characteristics on the proportion of females in each school. We also report the estimated coefficients from a within school regression with school specific time trends (columns (3) and (6). Table 3.3 reports the estimated coefficients from the within neighbourhood regression (neighborhood fixed effects) with and without adding neighbourhood linear time trends. Again the OLS estimates are reported as a point of comparison.

As we notice from these two tables, the proportion of females is not related to most of the students' characteristics, both in the OLS and the within school/ neighborhood regressions. There are some exceptions in the OLS and within school regression. In particular, the proportion of females within a school seems to be negatively correlated with the proportion of students with Polish

[13]This is more understandable when one takes into account that Greece has 227 inhabited islands, most of which are quite far from the mainland and have outdated telecommunications infrastructure (Ellinikos Organismos Tourismou (EOT), "Greek islands", April 2012).

and Bulgarian origin, however these correlations are reduced and become statistically insignificant when we add school linear time trends. Within neighbourhoods we find no association between the proportion of females within a neighborhood and students' observed characteristics. All the regressions include year fixed effects. These results suggest that cohort-to-cohort changes in the proportion of female students within a school and within a neighborhood seem to be uncorrelated with changes in students' observed characteristics.

We also examine whether changes in the proportion of female students within a school and within a neighborhood are related to changes in the logarithm of school enrollment. As reported in the first row of Table 3.2 there seem to be a negative association between changes in the proportion of females within a school and changes in the logarithm of school enrolment. Both, the OLS and within school regressions produce negative and statistically significant at 10% coefficients. However, this correlation largely reduces and becomes insignificant when school specific time trends are added.

One could still have concerns that students might react to the unpredicted changes in gender compositions. Although students are assigned to schools based on geographical characteristics and it is not easy to switch school, one could still be worried that students might drop out from or switch to another school after being exposed to this information. For example, students who are in schools where the proportion of girls is high/low could drop out. Or transfers of students could be observed that might be correlated to the observed proportion of females in a given school. We address this concern by looking at the correlation between the proportion of female students in a school and the probability that a student drops out from or switch to another school in that year. We use the smaller sample of schools because only for these schools we have data for multiple years and we can identify students who drop out and transfers.

Our dependent variable is a dummy variable that takes the value of one if the student drops out from school or if the student is transferred to this school at the beginning of the school year. Table 5 reports the outcome means and the regression estimates separately for boys and girls. The first row in each panel indicates that students' mobility from and to a school in low. Approximately 8% of boys and girls drop out from school in the twelfth grade and around 6-8% of boys and girls respectively transfer to another school at the beginning of the twelfth grade. The second row in each panel reports the regression estimates when school linear trends as well as school and time fixed effects are added. All estimates are small and statistically insignificant. Overall, changes in the proportion of females within a school seem to be uncorrelated with students' mobility across schools and drop out rates.

3.5 Results and Discussion

Table 3.6 shows the linear probability model estimates for the decision to enrol in college. Columns (1) and (2) report the effects of the proportion of enrolled students in year t on a student's decision to enrol in college in year t. Columns (3) and (4) report the effects of the proportion of enrolled students in year t-1 on a student's decision to enrol in college in year t. Each cell in the first and second row in Table 3.6 shows the estimated coefficient from a separate regression. The estimates presented are based on four different specifications. All specifications include track and year fixed effects. Columns (1) and (3) include school fixed effects and school specific linear trends. Columns (2) and (4) include neighbourhood and neighbourhood specific linear trends. In all specifications we control for a student's gender and admission score. We also include a dummy for students who were born in the first quarter of each year, following Angrist and Krueger (1992), who found significant differences in school outcomes for those students.

The coefficients of interest are positive in year t and statistically significant, revealing strong positive externalities at all levels. An increase of a hundred percent in the proportion of same-age school peers who enrol in college increases one's probability of enrol in college by 8.6 percent, ceteris paribus. This effect decreases at the neighborhood level. In particular, an increase of a hundred percent in the proportion of same-age neighbours who enrol in college increases one's probability of enrol in college by 4.3 percent, ceteris paribus. Coefficients of interest are negative for year t-1 and not very precise.

However, OLS estimates are likely to be bias due to endogeneity issues and the reflection problem. To address these but also further potential unobserved heterogeneity issues, we employ the novel identification strategy of relying on variation in gender composition to explain differences in mean college enrollment in school and neighborhood level. We use an instrumental variable approach to explore social interactions in space and time. Our instrument, gender composition, is likely to affect mean college enrollment since female-heavy school environments are found to be less disruptive and less violent (Lavy and Schlosser (2011)).

Tables 3.7 and 3.8 report first and second stage estimates, respectively. Both tables distinguish between social interactions among same-age peers and one-cohort-older peers. Each cell in the first and second row in Table 3.7 shows the estimated coefficient from a separate regression. In our setup, the proportion of girls is a strong predictor of mean enrollment as all first stage estimates are positive and statistically significant at 1%. As we observe in Table 3.7, our instrument is a better predictor of mean enrollment at the school rather than the neighborhood level. In particular, an increase of a hundred percent in the proportion of same-age girls within a school increases mean enrollment by 13.3 % whereas an increase of a hundred percent in the proportion of same-age girls within a neighborhood increases mean enrollment by 8.5%. When we consider last year's proportion of girls then the coefficient of interest declines. In particular, a 100% increase in the percentage of girls in the previous cohort within a school increases this year's mean enrollment by 12.3 %. Furthermore, mean college enrollement within a neighborhood increases by 10.2% if the percentage of girls in the previous year increases by 100%.

Moreover, the model is just identified as we have one instrumental variable and one endogenous variable. Stock and Yogo (2002) characterize instruments to be weak not only if they lead to biased IV results but also if hypothesis tests of IV parameters suffer from considerable size distortions. They propose values of the Cragg and Donald (1993) minimum eigenvalue statistic for which a Wald test at the 5 percent level will have an actual rejection rate of less than 10 percent. In our case the critical value is 16.38 which is always below the first stage Cragg-Donald statistic we find for the school and neighborhood regressions regarding college enrollment (32.01 and 34.09 respectively) and academic mobility (582.42, 2,849 for the school and neighborhood respectively). So we do not face a weak instrument problem.

Our second stage estimates suggest positive social interactions in education decisions through space and time, with the size of the effect depending on the size of the reference group. Each cell in the first and second row in Table 3.8 shows the estimated coefficient from a separate regression. In Table 3.8, we observe that a hundred percent increase in the proportion of students who enrol in college within one's school in a given year, increases a student's probability to enrol in college by 12.6 % in the same year. Similarly, a student is 7% more likely to enrol in college in a given year if the proportion of students who enrol in college in his neighborhood increases by a hundred percent in that year. We find positive and significant spillover effects among peers in consecutive cohorts. Intuitively, social interactions among students of consecutive cohorts are important, as older peers may function as role models or may provide access to information. We find that a hundred percent increase in the proportion of students attending college within one's school or within one's neighborhood a year before, increases his probability of enrolling in college by 29.1% or 9.6 % percent respectively. Year and track fixed effects are included in all specification. When we exploit within school variation, we control for school fixed effects and school specific time trends. When we use within neighborhood variation, we control for neighborhood and neighborhood specific time trends.

Moreover, we explore social interactions in the decision to study in a different city. Educational mobility is found in the literature to be greatly affected by social norms, labor market structure and income (Tremblay (2005)). We focus on those students who enrol in college between 2004 and 2009 (sample size: 355,808). Our models include controls for school or neighbourhood, year and area unobserved time-invariant characteristics. We begin our analysis by estimating specifications (6) and (7) using standard OLS. Our estimates reveal positive social interactions among same-cohort peers and smaller positive externalities coming from students in the previous cohort. Table 3.10 reports the effects of the proportions of migrated students on the decision to migrate of same-cohort or one-cohort-older students.

The linear probability model coefficients are biased due to reflection and endogeneity and they show a negative relationship between mean migration and a student's decision to migrate to another city. Thus we use the proportion of female peers in one's reference group as an instrumental variable. Table 3.11 reports first stage estimates. Each column is coming from a separate regression. All coefficients of interest are positive and statistically significant. Again the percentage of female students is a better predictor for the mean migration within a school rather than within a neighbourhood. Ou first stage estimates suggest that changes in the percentage of female peers have significant effects on mean migration in school and neighbourhood among same-cohort students but also in consecutive cohorts. The estimates in columns (1) and (2) are higher than the estimates in columns (3 and (4) respectively implying that the effects are stronger in year t rather than t-1.

Our second stage estimates are reported in Table 3.9. Each column in based on a separate regression. The coefficients of interest are all positive. Our findings suggest significant positive externalities among same-cohort students but significant and smaller positive externalities among students in consecutive cohorts.

3.6 Conclusion

In this paper we have estimated the effects of social interactions on a student's education decisions of college enrollment and academic mobility. Despite the vast literature on the topic, two crucial identification challenges remain: common correlated group effects and simultaneity.

Our contribution to the literature is twofold. First, we propose a new approach in alleviating challenges in identifying spillover effects by using time lagged group characteristics. Second, we provide evidence on social interactions using a special institutional setting that allows for spatial variation of group characteristics. So far, the existing literature on social interactions has focused almost exclusively on scholastic performance. The only exemptions to our knowledge are Sacerdote (2011) who identify the effect of social interactions on drinking, drug use, and criminal behavior and Giorgi et al. (2007) who finds significant effects on the choice of college major.

When social interactions are not taken into account, educational treatments may result in misallocation of resources and may fall short of policy goals. Our results aim to inform public policies that target ability mismatch.

We employ instrumental variable techniques to estimate utility linkages at different space and time levels. We battle the reflection problem and the endogeneity issues by using time lagged school and neighborhood student gender compostion as an instrument. Using repeated cross-sectional data, we exploit within-school and within-neighborhood cohort-to-cohort variation to examine the effect of random changes in gender composition on mean college enrollment. Then we look at the effect on a student's decision to enrol in college.

We find that the choices of a student's peers affect their decision to enrol in college and migrate to another city to pursue tertiary education. We use a novel dataset from Greece that contains the universe of high school graduates from 2004 to 2009. We focus our analysis on four reference groups: same-cohort peers in school, one-cohort-older peers in school, same-cohort peers in neighbourhood and one-cohort-older peers in neighbourhood.

Our evidence supports the hypothesis that individuals derive utility from conformity or have access to information, with the size of the externality decreasing in space distance. Our results show that one is more likely to enrol in college and move to another city to pursue post secondary education when many of his peers make the same choices. A hundred percent increase in the percentage of one-cohort-older peers within a school and within a neighborhood who enrolled in college increases a student's probability of college enrollment by 29.1 and 9.6 percent, respectively. In addition, a hundred percent increase in the percentage of same-cohort students who enrol in college within a school and within a neighborhood increases one's own probability to enrol in college by 12.6 and 7 percent respectively.

While our paper has examined several important determinants of college enrollment and migration decision, several avenues of future research remain. Understanding the mechanism that underlies social interactions is the next big question in the literature. Future research could push forward the front of understanding the mechanism that underlies social interactions.

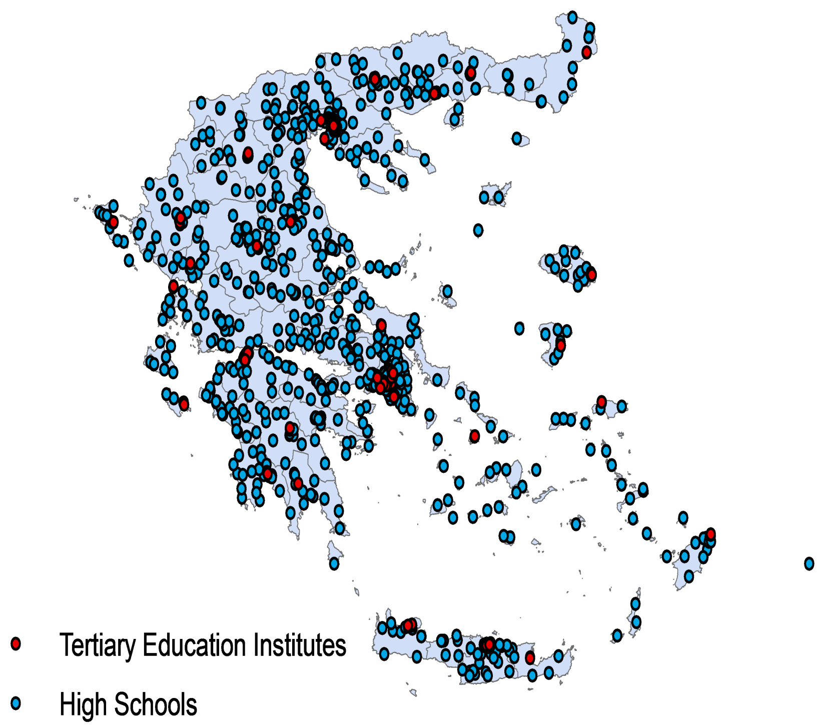

Figure 3.1: Map of schools

Table 3.1: Descriptive Statistics

	Mean	**Std. Dev.**	**Min.**	**Max.**	**N**
Panel A: Individual Level					
First quarter of birth	0.16	0.368	0	1	355,808
Female	0.567	0.495	0	1	355,808
National Exams Score	13.16	4.062	0.52	19.95	355,808
If enrolled	0.812	0.391	0	1	355,808
Mobile students	0.748	0.434	0	1	260,472
Specialty in Classics	0.365	0.481	0	1	355,808
Specialty in Natural Science	0.154	0.361	0	1	355,808
Specialty in Technical Studies	0.484	0.5	0	1	355,808
Postcode Income (Euro, 2009)	29,464	8,441	9,573	122,879	355,808
Aggregate Enrollment	60,206	6,372	52,450	68,136	355,808
Panel B: School Level					
Private	0.081	0.266	0	1	1,319
Income if private (Euro, 2009)	30,575	18,378	16,085	122,879	1,319
National score if private	13.69	2.70	4.7	17.34	1,319
Experimental	0.022	0.149	0	1	1,319
Income if experimental (Euro, 2009)	29,754	14,775	17,583	74,798	1,319
National score if experimental	14.40	1.00	12.23	16.17	1,319
Public	0.89	0.31	0	1	1,319
Income if public (Euro, 2009)	19,327	5,565	9,573	74,798	1,319
National score if public	12.26	1.56	2.97	16.36	1,319
Urban	0.898	0.301	0	1	1,319
Distance to nearest college campus(in miles)	10.871	24.083	0.105	1095.452	1,319
No of students in each school	46	34	0.16	179	1,319
Panel C: Neighborhood Level					
No of schools in each neighborhood	4.449	5.014	2	35	250
No of students in each neighborhood	929.291	1,246.298	8	10,559	250

Note: Data span six cohorts 2004-2009 of 60.119 students on average. Number of schools: 1319. Among those 413 high schools are in Athens or the surrounding suburbs. The national exam score ranges from 0 to 20. Mobile students are those who move to a different city in order to study.

Table 3.2: BALANCING TESTS FOR PROP. OF FEMALES WITHIN SCHOOL

	WHOLE SAMPLE			**SMALLER SAMPLE**		
	OLS	School FE	School FE +school linear time trends	OLS	School FE	School FE +school linear time trends
	(1)	(2)	(3)	(4)	(5)	(6)
logEnrollment	-0.075	-0.074	0.004	-0.125	-0.125	0.006
	(0.036)*	(0.036)*	(0.024)	(0.053)*	(0.054)*	(0.048)
EarlyEnrollment	-0.002	0.005	0.0009	-0.003	-0.006	0.0010
	(0.019)	(0.019)	(0.001)	(0.016)	(0.016)	(0.001)
Age	-0.002	0.002	-0.002	-0.003	-0.001	-0.004
	(0.002)	(0.002)	(0.003)	(0.010)	(0.010)	(0.006)
Ethnicity						
Greece				0.001	0.001	0.003
				(0.001)	(0.001)	(0.002)
Albany				0.004	0.004	0.016
				(0.013)	(0.013)	(0.011)
Bulgaria				-0.060	-0.060	-0.033
				(0.027)*	(0.027)*	(0.022)
Italy				-0.010	-0.010	-0.001
				(0.011)	(0.011)	(0.006)
Russia				-0.012	-0.012	0.011
				(0.027)	(0.027)	(0.017)
Poland				-0.054	-0.054	-0.022
				(0.024)*	(0.024)*	(0.019)
Ukraine				-0.003	-0.003	0.004
				(0.016)	(0.016)	(0.013)
N	355,808	355,808	355,808	18,670	18,670	18,670

Note: Standard errors are clustered at the school level. A constant is also included. *,**,*** denotes significance at the 10%,5% and 1% level respectively. The table reports OLS and school fixed effects estimates from separate regressions. Columns (3) and (6) report school fixed effects estimates having added school linear time trends. Year dummies are included in all regressions.

Table 3.3: BALANCING TESTS FOR PROP. OF FEMALES IN NEIGHBOURHOOD

	WHOLE SAMPLE			SMALLER SAMPLE		
	OLS	neighb. FE	neighb. FE+ neighb. linear time trends	OLS	neighb.FE	neighb. FE+ neighb. linear time trends
	(1)	(2)	(3)	(4)	(5)	(6)
logEnrollment	-0.081	-0.080	0.002	-0.089	-0.089	0.003
	(0.048)	(0.050)	(0.035)	(0.050)	(0.055)	(0.031)
EarlyEnrollment	0.002	0.005	0.001	0.004	0.004	0.001
	(0.003)	(0.003)	(0.035)	(0.007)	(0.007)	(0.023)
Age	0.001	0.003	-0.002	0.003	0.003	-0.001
	(0.001)	(0.001)	(0.005)	(0.004)	(0.003)	(0.002)
Ethnicity						
Greece				0.001	0.001	0.003
				(0.001)	(0.001)	(0.002)
Albany				-0.007	-0.007	-0.004
				(0.006)	(0.006)	(0.003)
Bulgaria				0.002	0.002	0.008
				(0.009)	(0.009)	(0.006)
Italy				-0.004	-0.004	-0.002
				(0.004)	(0.004)	(0.002)
Russia				0.005	0.005	0.009
				(0.009)	(0.009)	(0.006)
Poland				-0.002	-0.002	0.005
				(0.010)	(0.010)	(0.004)
Ukraine				-0.010	-0.010	-0.009
				(0.006)	(0.006)	(0.005)
N	355,808	355,808	355,808	18,670	18,670	18,670

Note: Standard errors are clustered at the neighborhood level. A constant is also included. *,**,*** denotes significance at the 10%,5% and 1% level respectively. The table reports OLS and neighborhood fixed effects estimates from separate regressions. Columns (3) and (6) report neighborhood fixed effects estimates having added neighborhood linear time trends. Year dummies are included in all regressions.

Table 3.4: Descriptive statistics for smaller sample and population

	Smaller Sample Mean	**Population Mean**	**Difference**	**Std. Dev.**
log Postcode income	9.962	9.968	0.006	(0.014)
Private school	0.080	0.081	-0.001	(0.001)
Public school	0.897	0.899	-0.002	(0.003)
Experimental school	0.020	0.022	0.002	(0.003)
Urban	0.899	0.898	0.001	(0.001)

Note: 18,670 obs. in smaller sample and 355,808 obs. in population. 45 schools in sample, 1319 schools in population. Evening schools are excluded from the sample and the population

Table 3.5: Estimation results : Drop out and Transfers

Dependent Variable: Dummy for drop out and Transfers		
	(1)	(2)
Variable	(Males)	(Females)
Drop out		
Outcome mean	0.080	0.078
Regression estimates	0.060	0.020
	(0.046)	(0.038)
Transfers		
Outcome mean	0.068	0.075
Regression estimates	0.020	-0.052
	(0.075)	(0.071)

Note: The table reports means of the dependent variable (first row) and estimates (second row) for the effects of the proportion of females on the probability that a student leaves school the following year. We use the smaller sample here of 45 schools. Clusters at school level. All regressions include controls for student characteristics. Standard errors are clustered at the school level. All regressions include school fixed effects, year fixed effects and school linear time trends.

Table 3.6: Linear Probability Model Estimates

	Dependent Variable: College Enrollment			
	(1)	(2)	(3)	(4)
Sample:	School	Neighborhood	School	Neighborhood
% Enrolled$_t$	0.086	0.043		
	(0.011)***	(0.012)***		
% Enrolled$_{t-1}$			-0.039	-0.017
			(0.012)***	(0.014)
Born in 1st quarter	0.006	0.006	0.007	0.007
	(0.001)***	(0.001)***	(0.001)***	(0.001)***
Female	-0.003	-0.003	-0.003	-0.003
	(0.001)**	(0.001)***	(0.001)**	(0.001)***
Admission Score	0.069	0.069	0.069	0.069
	(0.000)***	(0.000)***	(0.000)***	(0.000)***
Speciality FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
School FE	✓	✓	✓	✓
School specific time trends	✓		✓	
Neighbourhood specific time trends		✓		✓
N	355,808	355,808	355,808	355,808
R^2	0.47	0.47	0.47	0.47

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the school level. An intercept is also included.

Table 3.7: First stage estimates

Dependent variable:	**Mean College Enrollment**$_t$			
	school	neighborhood	school	neighbourhood
Proportion of girls$_t$	0.133	0.085		
	(0.019)***	(0.030)***		
Proportion of girls$_{t-1}$			0.123	0.102
			(0.018)***	(0.003)***
Female	-0.0004	-0.0008	-0.0001	-0.0004
	(0.0003)	(0.0002)***	(0.0003)	(0.0003)
Admission Score	0.001	0.0002	0.00002	0.00003
	(0.0008)***	(0.00004)***	(0.0003)***	(0.00003)
Born in first quarter	0.0001	-0.0006	0.0002	0.0008
	(0.00004)	(0.0002)***	(0.0002)	(0.0007)
Speciality FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
School FE	✓	✓	✓	✓
School specific time trends	✓		✓	
Neighbourhood specific time trends		✓		✓
N	355,808	355,808	355,808	355,808
R^2	0.54	0.70	0.43	0.62
F-statistic 1st stage	14.22	17.57	12.11	18.13

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the school level. An intercept is also included.

Table 3.8: IV Second Stage Estimates

	Dependent Variable: College Enrollment			
	(1)	(2)	(3)	(4)
Sample:	School	Neighborhood	School	Neighborhood
% Enrolled_t	0.126	0.070		
	(0.036)***	(0.033)**		
% Enrolled_{t-1}			0.291	0.096
			(0.037)***	(0.035)***
Admission Score	0.069	0.069	0.069	0.069
	(0.001)***	(0.000)***	(0.000)***	(0.000)***
Female	-0.003	-0.003	-0.003	-0.003
	(0.001)***	(0.001)***	(0.001)***	(0.001)***
Born in first quarter	0.006	0.006	0.006	0.007
	(0.001)***	(0.001)***	(0.001)***	(0.001)***
Speciality FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
School FE	✓	✓	✓	✓
School specific time trends	✓		✓	
Neighbourhood specific time trends		✓		✓
N	355,808	355,808	355,808	355,808

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the school level. An intercept is also included.

Table 3.9: LPM Migration Decision

	Dependent Variable: Migration Decision			
	(1)	(2)	(3)	(4)
Sample:	School	Neighborhood	School	Neighborhood
% Migrated$_t$	-0.099	0.095		
	(0.035)***	(0.037)***		
% Migrated$_{t-1}$			-0.087	-0.067
			(0.026)***	(0.022)***
Born in 1st quarter	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Female	-0.013	-0.013	-0.013	-0.013
	(0.002)***	(0.002)***	(0.002)***	(0.002)***
Admission Score	-0.023	-0.023	-0.023	-0.023
	(0.002)***	(0.002)***	(0.002)***	(0.002)***
Speciality FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
School FE	✓	✓	✓	✓
School specific time trends	✓		✓	
Neighbourhood specific time trends		✓		✓
R^2	0.30	0.30	0.30	0.30
N	355,808	355,808	355,808	355,808

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the school level. A intercept is also included.

Table 3.10: First stage estimates for migration decision

	Dependent Variable: Mean Migration Decision			
	(1)	(2)	(3)	(4)
Sample:	School	Neighborhood	School	Neighborhood
Proportion of girls_t	0.125	0.098		
	(0.023)***	(0.020)***		
Proportion of girls_{t-1}			0.114	0.089
			(0.027)***	(0.022)***
Admission Score	0.0002	0.001	0.0006	0.001
	(0.0001)*	(0.0001)***	(0.0003)***	(0.0001)***
Female	0.002	0.003	0.001	0.002
	(0.0008)**	(0.007)*** (0.0008)	(0.0007)***	
Born in 1t quarter	-0.0006	-0.0007	-0.006	-0.0008
	(0.001)	(0.001)	(0.001)	(0.001)
Speciality FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
School FE	✓	✓	✓	✓
School specific time trends	✓		✓	
Neighbourhood specific time trends		✓		✓
N	260,472	260,472	260,472	260,472
R^2	0.37	0.39	0.37	0.39
$F - statistic 1st stage$	16.20	14.8	15.9	14.7

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the school level. An intercept is also included.

Table 3.11: IV Estimates for Migration Decision

Dependent Variable: Migration Decision				
	(1)	(2)	(3)	(4)
Sample:	School	Neighborhood	School	Neighborhood
% Migrated$_t$	0.107	0.091		
	(0.032)***	(0.027)***		
% Migrated$_{t-1}$			0.101	0.065
			(0.036)**	(0.034)*
Admission Score	-0.022	-0.023	-0.023	-0.023
	(0.000)***	(0.000)***	(0.000)***	(0.000)***
Female	-0.014	-0.014	-0.014	-0.014
	(0.002)***	(0.002)***	(0.002)***	(0.002)***
Born in 1st quarter	-0.001	-0.001	-0.001	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)
Speciality FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
School FE	✓	✓	✓	✓
School specific time trends	✓		✓	
Neighbourhood specific time trends		✓		✓
N	260,472	260,472	260,472	260,472

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the school level. An intercept is also included.

CHAPTER 4

WHICH DEGREES DO STUDENTS PREFER DURING RECESSIONS?

4.1 Introduction

Students may alter their decisions regarding post-secondary education during economic turmoil. The consequences of graduating in a recession are associated with long-term, negative effects on earnings (Kahn 2010; Wee 2013; Oreopoulos, von Wachter, and Heisz 2012). Previous studies have shown that economic fluctuations affect human capital investment including college enrollment (Hershbein 2012), college completion (Kahn 2010) and graduate school attendance (Bedard and Herman 2008; Johnson 2013). The business cycle rearranges the production factors within an economy, causing some sectors to prosper and others to shrink. The short- run oscillations in the growth of various sectors change the available job opportunities, and therefore, the popularity of different college majors. Economic turmoil might affect the labor- market prospects of different professions in different ways, and thus, influence college applicants' expected returns from the related college majors. These differences could be large. For example, Joseph, Blom, and Meghir (2012) show that the income gap of students specializing in different majors could be as large as the income gap between high school and college graduates.

The choice of college major is a good predictor of future earnings. During a recession, students might re-consider their expectations about future career paths and the earnings potential associated with a specific college major. Thus, switching majors could imply significant changes in a student's lifetime income. A large literature focuses on understanding which factors may affect students' choice of college major (Montmarquettea, Cannings, and Mahseredjianc 2002; Arcidiacono, Hotz, and Songman 2010; Beffy, Fougre, and Maurel 2011; Dickson 2010; Wiswall and Zafar 2011; Porter and Umbach 2006). This literature has examined how students form expectations about earnings and career prospects associated with a specific college majors, and how these expectations affect students' educational choices. This literature has largely focused on a static framework, or has been based on the analysis on a single cohort. However, the effect of the business cycle on students' preferences for the field of study or the major they select has received little attention. In this paper, we use new data on admission applications received by the universe of undergraduate degree programs in Greece that span seven rounds of admission cohorts to examine the following two research questions: Do changes in unemployment affect college applicants' preferences for selected university fields? Do these differences in students' preferences affect college admission thresholds?

The contributions of this paper are threefold. First, we examine if the business cycle affects students' self-reported preferences for certain university degrees and majors. We proxy business-cycle fluctuations with a job-insecurity index associated with university degrees, and with the unemployment rate. Second, we undertake what we believe to be the first analysis of data on the universe of college applications and all public tertiary education institutions for an entire country, rather than for applications to departments of a specific university. Our data encompass degree applications submitted by every student who decides to pursue tertiary education nationwide over a period of seven years. Because the Greek system asks students to submit college applications in order of preferences, that specify the desired field of study at a specific university, we know how students rank their degree applications. In particular, we know which application is a student's top-, second-, third-, and later-choice indicating most, second-, third-, and later-most preferred degree choices. Third, we believe our work is the first to examine the effect of students' degree preferences on degree's entry requirement (i.e. admission threshold). Our analysis controls for field, campus city, time and university unobserved heterogeneity.

The crisis in Greece represents one of most severe economic events in the developed world since the Great Depression. Although Greece's GDP had started to decline in 2008, austerity measures taken in late 2009 resulted in a very abrupt and deep deceleration of the economy. Two characteristics of the Greek crisis made the downturn distinct in modern times: First, Greece experienced the most severe drop in GDP of any developed country not involved in a war. Second, the Greek recession was so widespread that if affected virtually every industry and every profession.

In this paper, we explore the short-run impact of a recent financial crisis on the demand for post-secondary education in Greece. As the economic conditions deteriorate, people might adjust their education decisions. Preliminary figures from the OECD suggest that the crisis led more young adults to seek for post-secondary education. According to the OECD (2016), the share of the Greek population ages 25 to 34 with a post-secondary degree grew from 32.5 percent in 2011 to 40.1 percent in 2015 - a level that nonetheless remained below the OECD average of 42.1 percent. In this paper, we investigate how the crisis altered demand for available college majors, and changed admission thresholds. We argue that the business-cycle can redistribute degrees in terms of popularity and difficulty in gaining admission for the degrees (admission thresholds) that lead to various career prospects.

To examine these effects, we use a novel data set from Greece that includes information on college applications and admission thresholds for different degrees. In this way, we uncover information about students' most preferred subjects/degrees for the period 2005-2011, a time that preceded and includes the opening chapter of the economic crisis. We deliberately focus on the early effects of the recession on college application. As the recession progressed, changes in institutional settings as well as changes in the quality of college education due to financial constraints, may have exacerbated the recessions effects. Thus, by focusing on the early years of the recession we avoid the potential of additional uncertainty due to changes in such possible confounding factors - key issues that might make disentangling the short-run variation in demand for college education challenging. Our study is the first one to identify the relationship between youth unemployment and the demand for specific college degrees nationwide, while netting out supply-side dynamics. By analyzing college applications we are able to examine which fields and degrees are the most popular at different stages of the business cycle.

Two features of the analysis bear mention: First, this study focuses on the effect of the recession on students' preferences over university fields rather than their actual college major enrolment decisions. In a setting where the supply of university places is exogenously determined and fixed, we can only examine changes in the popularity of each department rather than changes in the number of students who actually enrol in each field. Although the actual number of students who matriculate in each university department each year is relatively stable, the number of applications each department attracts across years varies significantly. Second, we are able to look at the effect of unemployment on students' top choice (most preferred) degree applications, because college applicants complete an ordered list of preferred university departments (for a field of study at a specific academic destination). All students are required to report their degree applications with a ranking of each preference. In our dataset, we observe the order of all applications each degree attracts. As a results, we are able to provide detailed, stylized facts about the demand for college education, and specific fields of study that students report as their most-preferred degrees.

The rest of the paper is structured as follows. Section 2 describes the institutional background. Section 3 describes the data. Section 4 discusses the drivers of the decision to apply to college. Section 5 provides analytical evidence. Section 6 discusses our results. Section 7 concludes.

4.2 The Greek Post-secondary Education System

4.2.1 How do students participate in the college admission process?

College admission in Greece is based on a centralized system, and students are admitted directly to departments within universities. Many other countries, such as Chile, China, Korea, Taiwan, and Turkey, use the same or similar centralized application systems for post-secondary education. Students apply to a major and university simultaneously (e.g. Chemical Engineering at the University of Athens)[1] as part of a centralized, score-based application process. Each university department in Greece offers a single undergraduate degree program, and transfers to a different degree are not allowed at any stage. We refer to an institution-major combination as a

[1] Similar systems include the state university system in California (see http://admission.universityofcalifornia.edu/how-to-apply/index.html, https://secure.csumentor.edu/support/pdfs/express_app.pdf, Chilean universities (Hastings, Neilson, and Zimmerman 2014) German universities (Braun, Dwenger, and Kubler 2010), and Chinese universities (Chen and Kesten 2013)

degree. Most degrees at these institutions require four or five years to complete on time. College degrees are linked to specific occupations. Access to some occupations is restricted to graduates with specific college degrees. For example, in order to become a licensed tourist guide in Greece one must obtain a college degree in History or Archaeology. Thus, preferences over college majors are strongly related to preferences over occupations.

In Greece, high school graduates and twelfth-grade students who aspire to pursue tertiary education take national exams in May, and their university admission score [2] is the sole criterion for college admission. The same admission process applies to returning high school graduates.[3]

Students usually take national exams in five common subjects (Language, Mathematics, Physics, Biology, History) and four compulsory, track-specific subjects. There are three tracks: Classics, Natural (or Exact Sciences) and Technical Studies (or Information Technology). Students can apply to university departments that are relevant to their track. For example, students outside the Classics track cannot apply to Law schools. Goulas and Megalokonomou (2015) describe the process in detail. Once the results of the national exams are announced, students are required to submit a list, ranking in order the university departments to which they would like to be admitted. The only way a student can avoid this university admission procedure is by not submitting a list of preferences. This might be the case for students who apply to undergraduate programs abroad.[4]

4.2.2 How do they apply to specific university departments?

Submitting a ranked list of preferred university department is equivalent to submitting an application to each university department in the list. A centralized, computerized system at the Ministry of Education ranks students by their admission scores, and assigns the highest ranked student across the country to her top choice. The algorithm then moves to the second-highest ranked student across the country, and assigns her to the first department in her list in which there is an available place, and so on. Essentially, college admission functions like a queue where the choicest university program offers admission to the highest-performing student that has placed this degree in her preference list.

At the end of this process, every department announces the grade of the student with the lowest score it admitted in that year. This grade is considered to be the "admission-threshold score" or "cut-off score" in that year. Each degree has its own admission-threshold score. Students are

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[2]The university admission score combines the national and school exam scores a student receives in twelfth grade. The national exam scores receive much heavier weight in the calculation of the university admission score than the school exam results.

[3]Returning high school graduates could keep their school exam score and retake the national exams any year after school graduation.

[4]These students take national exams but they do not submit a preference list. In this way, these students do not participate in the college application process.

accepted to specific degrees if and only if their admission score is above the cut-off. Thus, it is more difficult to gain admission to departments with higher admission thresholds. Each year, each university department admits a fixed number of applicants every year, as determined by the Ministry of Education. There is only one admission cycle, conducted every year in July. College education in Greece is free of charge for undergraduate students, and there are no pre-admission scholarships that could encourage a student to apply to a certain department instead of another.

Submitting a list is a prerequisite for participating in the university admission process. There is no room for gains from strategic misreporting of preferences. The ordering of university departments in the preference list is very important for a student because once a student gains admission to a specific university department, he cannot enroll in a university department in a lower position. Students report their preferences prior to the announcement of the degrees' admission thresholds and the admission outcomes. When a student completes her preference list, she is aware of previous years' threshold scores and the ranking of degrees based on previous years' threshold values. A student is aware of her own score and the distribution of national exam scores but she is not aware of the threshold score of each department in the same year in which she applies. Nevertheless, a student does have incentives to aspire to university departments that report higher threshold admission scores in previous years than her own admission score. This is the case because admission thresholds vary from one year to another, and listing additional university departments does not involve any financial cost for the student. In this way, students in any given year have incentives to report potentially all university departments they desire to consider for admission and are relevant to their tracks.

In general, students have preferences for specific degrees. For example, a student who aspires to study Economics could potentially list all university departments that offer a degree in Economics in her preference list. In a framework of cost-less applications, each individual who desires to study Economics has incentives to include every Economics department in their preference list. Thus, every department could potentially receive an application from every applicant who desires to study the same major. Potentially, the only thing that differs from one preference list to the next applicant's list is the ordering of degrees. [5]

What determines a degree's admission threshold? The most important determinant is the demand for the specific degree as derived from students' top choice applications. Receiving many top choice applications makes the degree more popular and induces a higher competition for the available seats. In this case, the admission score of the last admitted student (which is equivalent to the cut-off score) is usually higher when there is more competition. The Ministry of Education

[5]There might be students that have stronger preferences for a city than a degree. For example, a student might list degrees that are offered only in Athens and are relevant to his track.

can also affect the admission threshold by changing the number of available university seats. Reducing the supply of degree seats is an indirect way to accept only the highest-achieving students who have listed this specific degree. Thus, the admission score of the last admitted student will be higher, which increases the admission threshold.

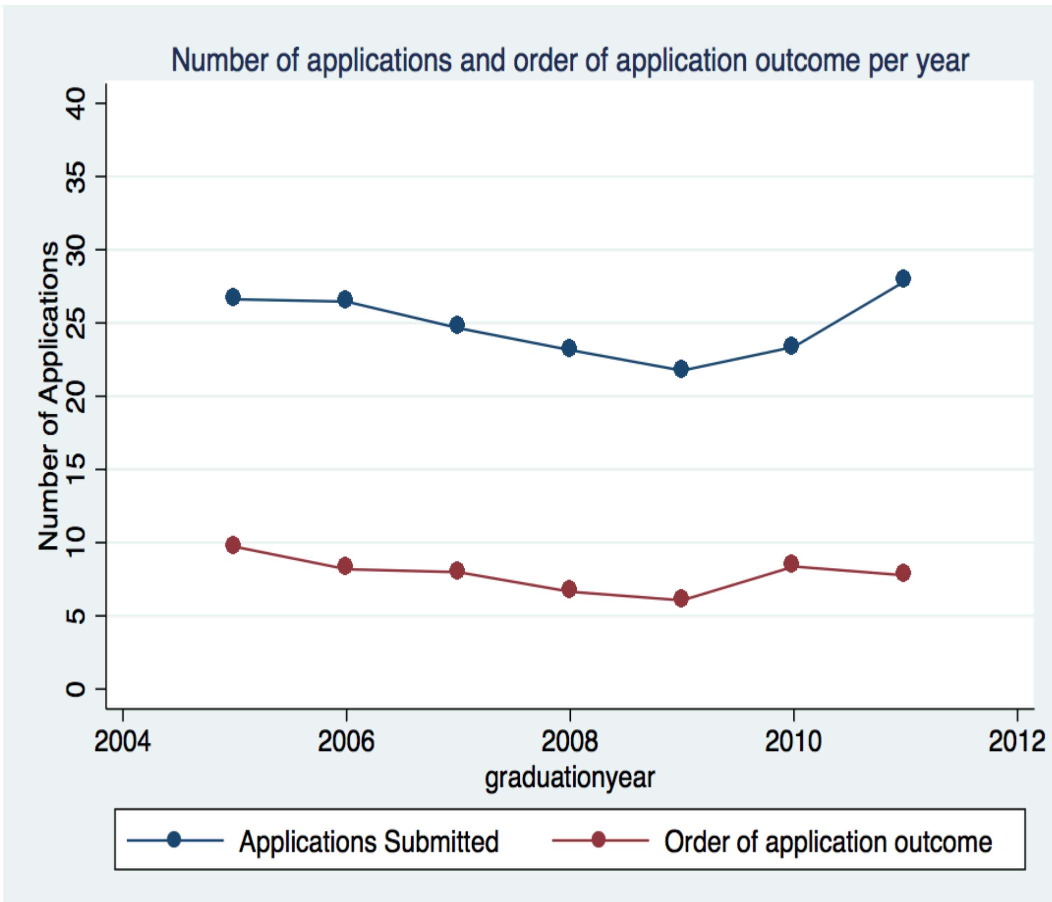
Reporting a degree in any position except the top ones in one's preference list does not necessarily affect a degree's admission threshold. Students might report degrees in lower positions in their preference list because they want to make sure that they will gain admission to some degree course, even if they are not actually committed to enroll. These students might never actually affect the admission threshold score because they might gain admission to a degree higher on their list, and so, at that point, they are no longer under consideration for any other degree course, or part of the process that leads to a degree's threshold determination. The algorithm that the Ministry of Education runs provides a unique application outcome[6] for each student based on his own ordered preferences, his admission score, and everyone else's ordered preferences and admission scores. Once students' ordered preferences are submitted, the algorithm produces only one possible admission outcome for each student. We call this "application outcome" and it is a unique combination of university department for each student. Students who change their minds after submitting their preference lists, and thus want to choose a degree course other than the algorithm match have to reapply for admission the next year. This is the case even if the other degree course is listed in a lower position that the one allocated to them by the Ministry of Education.

Table 1 shows some descriptive statistics for students who participated in the university admission process between 2005 and 2011. More than 80 percent of college applicants were admitted to some degree program. On average, students apply to 24 university departments/degree programs, and they gain admission to the choice that ranks eighth on their list. As indicated in Figure 1, the number of degrees students put on the preference list, and the students' rank for the degree program to which she ultimately gains admission change slightly across time.[7] Almost 70 percent of admitted students enroll in a university department that is in another city, and 56 percent of applicants are female students. The average cohort size is 62,257 students. In the period we study, on average, 60,257 students gain admission to any university department. It is also interesting to mention that, on average, 89 percent of applicants are new high school graduates, while the other 11 percent have graduated from school in the past and are reapplying for college admission. A student might reapply to college for two reasons. First, she might not have been accepted to any university department in the past. Second, she might have previously been accepted to a university department, but decided to apply for admission to a different degree program.

[6]The outcome refers to the degree course in which he is allowed to enroll in that given year.

[7]We thank an anonymous referee for bringing this point into our attention

Figure 4.1: Number of applications and order of the unique application outcome per year



This figure shows the number of degrees students report in their preference lists on average. These reported degrees are equivalent to degree applications. Students compile a list with any degree offered in the country they would like to be admitted to. This figure also shows the order of the unique degree (application outcome) students are accepted.

Table 4.1: Descriptive Statistics on college applicants

	Mean	**Std. Dev.**	**Min.**	**Max.**
If admitted	0.815	0.388	0	1
Number of applications	24.661	21.435	1	290
Rank of admitted college in preference list	8.041	9.981	1	238
Mobile students	0.699	0.458	0	1
Female	0.565	0.496	0	1
Age	17.98	1.139	15	66
Repeat	0.112	0.316	0	1
Cohort size	62,257	8,896	50,061	70,868
Aggregate Enrollment	60,257	6,799	52,450	69,631

Note: Data span seven cohorts from 2005 to 2011. Number of schools: 1403. Among those 442 high schools are in Athens or the surrounding suburbs. Mobile students are those who move to a different city in order to study.

4.3 Data

We examine the effect of a recession on college-major preferences by using college application data prior to and shortly after the beginning of the recent financial recession. We use a new and unique data set that contains administrative information from the Ministry of Education on the number of college applications for the universe of undergraduate degree programs offered in Greece from 2005 to 2011. We use panel data for the universe of undergraduate degree programs over a period of seven years. This data set contains college applications by both recent and returning high-school graduates who wish to enroll in tertiary education. In addition, we observe how many university departments were operating in each year, the fields in which the universities offered degrees, and the city of the campus location.

Because students report preferences prior to their admission outcomes and their enrolment decisions, our data on reported preferences are unconditional on college admission. Actual enrollment may change with changes in the number of slots available in each degree program over time. The Ministry of Education has the entire control over the supply of university seats. We also pull annual data on youth unemployment from the World Bank statistical reports.

We obtained individual level data from the Ministry of Education for each student who applies to college from 2005 to 2011. This dataset includes: gender and age of each applicant, the type of school (public, private, urban) each student attended, if a student is a new high school graduate or a returning student[8], if the student is admitted to some university department, each student's application outcome, number of degrees listed in each student's preference list and order of application outcome, students' annual national exam score, and the supply of seats per year. Figure 2 shows

[8]We refer to applicants who have previously graduated from high school as "returning students"

the number of college applicants who apply for college admission from 2005 to 2011. After 2009, the number of college applicants rises sharply and above the respective increase in new high school graduates. In Figure 3, we disentangle the pool of college applicants into two groups: new high school applicants and returning students, and we look at how these two numbers change over time. There are more returning students after 2009.

Table 2 provides information about the number of university departments operating in each field and each year. Here, we categorize university study fields into 22 broad, major groups.[9] It is interesting to observe that supply of university departments is relatively stable across years for each field. Over the seven years included in our data, 481 university departments operate for the complete time frame, and 24 university departments operate for fewer years.[10] No university department closes during the sample period.

Data on the degrees' admission thresholds are publicly available, and we obtained them from the Ministry of Education. We were unable to fully match the two datasets because some degree programs changed their names, some used different university identifiers in certain years, and other values are missing in the public documents. However, we obtained information on the degree cut-off score of 2,746 combinations of degrees and years.

4.4 The Argument

In this section we are considering the factors that, in our view, substantially drive education-related decision making in the period marking the beginning of the recent Greek crisis. This period offers a particularly interesting window into the decision-making process of applicants because the degree of economic turmoil is so pronounced that it almost certainly influences the potential pursuit of post-secondary education, and because these effects are likely to be heterogeneous for different sectors and professions.

4.4.1 Contextual influences

Our first point concerns contextual influences on the decision to apply to college. We group students into three categories based on the way the students make education-related decisions. In Greece, as elsewhere, one group of students come from families that strongly intend to send them to tertiary education and sometimes push them to pursue a particular academic or professional path - due to income, attitudes, professional and social status, and other factors. For these children, preferences regarding college education, in general, and about specific potential college majors have been formed or induced in advance of the time they actually apply to college. For those

[9]The are 21 categories and a category named "Other". In "Other" we put some degrees that are not associated with any of the remaining 21 categories, for example special religion studies.

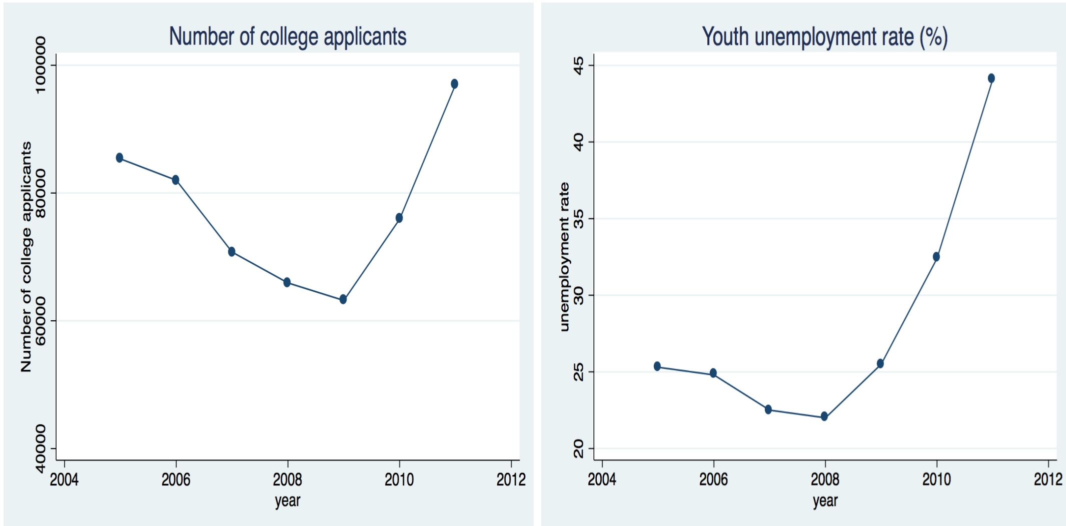
[10]Specifically, one university department operates for six years; two university departments operate for two years; two university departments operate for four years; three university departments operate for five years; and sixteen university department operate for three years.

Table 4.2: # UNIVERSITY DEPARTMENT OFFERING DEGREES IN VARIOUS FIELDS

	2005	2006	2007	2008	2009	2010	2011
Engineering and computer science	105	105	105	105	110	110	110
Agriculture and forestry	30	30	30	30	30	30	30
Economics	18	18	18	18	18	18	18
Mathematics and Statistics	11	11	11	11	11	11	11
Business and Management	67	67	67	67	70	70	70
Biology	6	6	6	6	6	6	6
Other	48	48	50	50	53	53	53
Physics and Earth Science	12	12	12	12	12	12	12
Liberal Arts and Humanities	22	23	23	23	23	23	23
Psychology	3	3	3	3	3	3	3
Social, Political and European Studies	12	12	12	12	13	13	13
Nursing and other Health	31	31	32	32	36	36	36
Journalism	5	5	5	5	5	5	5
Education, Language, History and P.E.	67	67	67	68	68	68	68
Home economics	1	1	1	1	1	1	1
Medicine	7	7	7	7	7	7	7
Pharmacy	3	3	3	3	3	3	3
Law	3	3	3	3	3	3	3
Veterinary Science	2	2	2	2	2	2	2
Dentistry	2	2	2	2	2	2	2
Police and Military	25	25	25	25	24	26	26
Naval Academies	2	2	2	2	2	2	2
Total # of available degrees	482	483	486	487	502	504	504
# of new high school graduates	70,560	68,067	53,552	52,430	50,061	70,868	69,545
# of college applicants	85,343	82,003	70,759	65,932	63,187	75,904	96,953

Note: The table shows the number of existing university departments in each field and year. The # of college applicants consists of the # of new high school graduates plus the # of students of returning applicants.

Figure 4.2: Number of total college applicants and youth unemployment over time

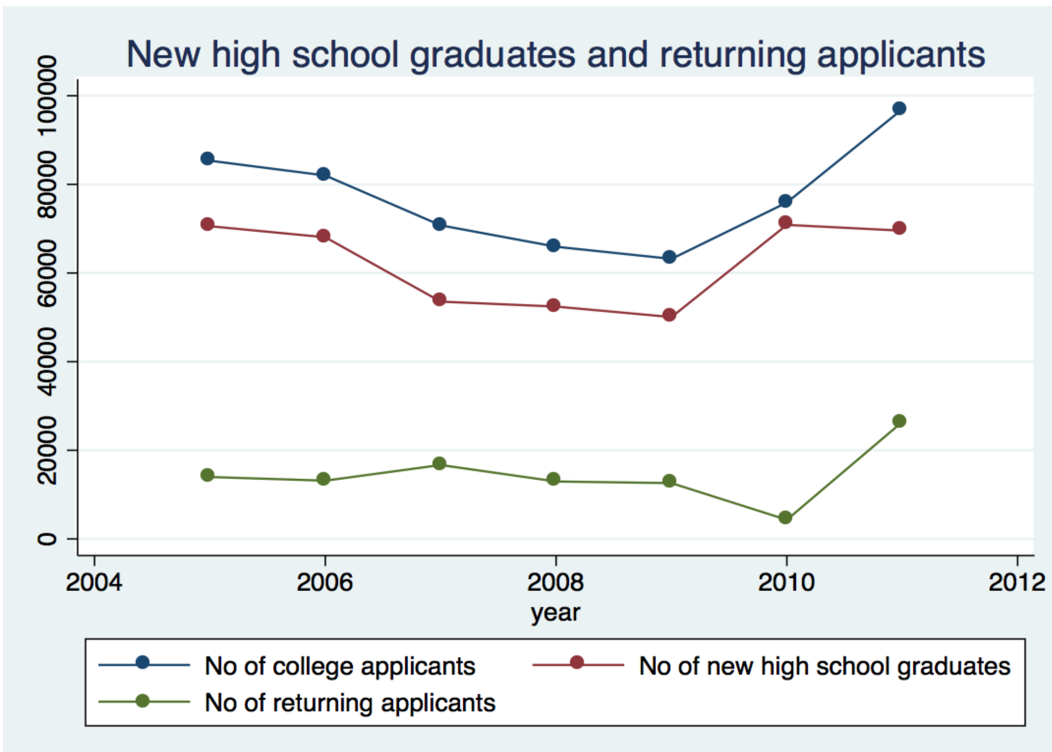


The left figure shows the number of college applicants over time and the right figure shows the evolution of the youth unemployment rate over time. Sample period: 2005-2011. Source for unemployment data: World Bank.

individuals, job-market conditions in the particular years involved likely have little or nothing to do with their predetermined college attendance and choice strategies (which may in itself take into account employment wages, status, and the like). We call these applicants " strategic applicants".

Next, there is a set of students who, either because of attitudes or socio-economic status, are less committed to a college application strategy, and most likely they respond more strongly to current information regarding the costs and benefits of college education. Following Nakata and Mosk (1987) we call the students in this set "marginal applicants". Between these two groups is a third category, students who are less committed to attaining a post-secondary education than the " strategic" group, but who are not part of the "marginal" group. These "core applicants" reach a decision over a significant number of years, and, as a result, they are less influenced by the exact economic conditions for the years in which they apply to college. Such individuals probably constituted a significant fraction of school-goers in Greece around the beginning of the recent financial crisis because household income per capita had increased substantially in the two decades prior to the beginning of the debt crisis in late 2009, when household income per capita had reached what a peak. Presumably the improvement in real family disposable income played an important role in allowing the children of these households to pursue college educations. We believe that changes in the unemployment rate might mainly affect "marginal applicants" and much less so the third category of applicants.

Figure 4.3: Number of total college applicants separated into new high school graduates and returning applicants



This figure shows: a) The total number of college applicants, b) The number of new-high school graduates (who who graduate from high school the year they apply to college) and c) The number of returning students (those who had graduated in a previous year and they reapply for college admission).

4.4.2 Returns to education

Our second argument concerns returns to education. The job market[11] in Greece operates essentially like a queue. That is, persons seeking employment for the first time compete for jobs in a system in which the best-educated person is first in line for the choicest job position. The crisis led to layoffs and job rationing and overall conditions that increased competition for employment. To improve their employment prospects, students invest in more years of education; the same may be true for those who were not students when the crisis began - those who had found a job previously - possibly shortly - before the crisis, but were forced by payroll cutbacks into unemployment and ultimately led back towards additional education. Thus, in times of gloomy job market prospects, we hypothesize, an overall increase in the demand for college education is to be expected, ceteris paribus. Moreover, the drop of salaries across industries and job functions brought about by the crisis altered college applicants' anticipated post-graduation returns to education overall, and to specific college degrees. Graduates of all degrees saw their benefits reduced compared to the pre-crisis era; as a result, candidates began to reconsider each college major's expected costs and benefits, causing a reformulation of preferences or education in general as well as among specific college degrees.

4.4.3 Quality of tertiary education

Our third argument is related to the quality of tertiary education provided during the crisis. During the first years of the recession, changes in the ratio of students to faculty, research work, and facilities are unlikely to affect candidates' decision regarding college application. It was still very early and the general view was that the crisis will not last long. We are not worried about price effects related to the direct college costs (e.g., tuition and fees) because students in Greece do not pay tuition fees and even the books are provided to them for free.

In countries where tertiary education is not free, the recession could affect students' willingness and ability to obtain a student loan, and thus, could also affect students' decisions over a specific college or major based on costs. In such countries, concerns that surfacing over whether mounting education debt and students' inability to repay their loans will be the next big economic bubble to burst Cronin and Horton (2009). Douglas (2016) estimates that the present discounted value of attending college for the median student varies between \$85,000 and \$300,000 depending on the student's major. This is less of a concern in Greece because every tertiary education institution is public, and free post-secondary education is a constitutional right.

A concern would be that students are less able to study in another city, because, after 2009, their parents are more likely to face difficulties in covering the cost of living. Again, we believe that in the early years of the crisis households had not experiences a considerable drop in their purchasing

[11] We refer to all jobs ie. public and private sectors

power. However, as the crisis progressed, after 2012, this financial inability to cover living costs is likely to restrict students options. We believe that potential education quality effects may exist after 2012 because many universities had to cut back on funds for research and facilities. Also, quantity rationing of slots, both overall and in specific degrees, took place, and were of paramount importance for applicants to university departments. Nevertheless, in our study we are able to net out any quantity effects by looking at self-reported preferences among specific degree program choices made by candidates prior to the outcome of their college application.

To summarize, current economic circumstances as well as expectations over returns to education and quality of education constitute crucial information for decisions concerning college applications during the recent recession.

4.5 Analytical Evidence

In this section we explore some of our hypotheses statistically, using both simple tables depicting time series of data, as well as regression analysis. We follow the universe of university departments in Greece which is also identical to the set of available college majors; this is because university department offers exactly one college major, although the same major may be offered by more than one department in different universities.

4.5.1 Time series statistics

Table 3 combines the supply of specific fields with the demand for specific fields. For each field and year we report a measure of weighted popularity for each field (d) that is constructed in the following way:

$$WeightedPopularityIndex_{f,t} = \frac{\#\text{ of applications received as number one choice}_{f,t}}{\#\text{ of existing degrees}_{f,t}}$$

To calculate the weighted popularity index [12], we divide the number of application each department receives as number one choice over the number of existing departments in each field and we look at the evolution of this index over time. Table 3 shows the weighted popularity index over time for various fields. For instance, in 2005, on average, 181 college applicants list economics as their top choice. In the same year, each department in Law receives on average 871 applications reporting a Law department as their top choice. The weighted popularity index clearly reflects relative preferences of college applicants across fields. For example, a Dentistry department receives more applications listing it as top choice compared to a Veterinary department, given their supplies of degrees. Given that the supply of degrees in each field is relatively stable across years (Table 2), if we observe considerable changes in the weighted popularity index within fields over time, they will be caused by changes in the number of applications submitted to each degree this year

[12] An alternative to the weighted popularity index would be the total number of applications a degree receives in a given year. However, it would not take into account possible changes in the supply of existing degrees.

(demand side). For example, in Social, Political and European Studies the relative changes in the weighted popularity index over time are not as large as the relative changes for Naval Academies over time. We also find that each degree across fields attracted on average 174 top-choice applications in 2005, indicated by the mean number of applicants variable.[13] The mean number of applications drops from 2005 to 2009 and then it increases.

4.5.2 Regression analysis

In this section we investigate the effect of the recent recession on the demand for fields of study at the university level, and for changes in degrees' admission thresholds. Using OLS, we examine how changes in the unemployment rate affect the demand for degrees or fields that have different employment prospects.

Job insecurity

We compile information on job prospects and job insecurity that is, the fear of involuntary job loss from a series of long-term surveys of college graduates in Greece published in Katsikas (2006)[14]. This information is used to construct an index of employment prospects of different college degrees, based on the structure of the Greek economy and year specific factors. For each university department, the index takes a value between 1, 2, 3, indicating good, mediocre and poor employment prospects. Katsikas (2006) stresses that the index is the result of the amalgamation of information from the career offices of all universities, the Hellenic Bureau of Statistics, the OAED[15] employment observatory, and various labor unions. The index is intended to represent differences in structural and frictional unemployment among those with available college degrees and, most importantly, time-specific labor market conditions. As a result, it captures the economic and employment prospects associated with a degree in that year. Degrees with a low job insecurity index imply more available and stable employment conditions than degrees that are characterised by a high job insecurity index. The latter imply poor employment prospects, a higher difficulty to find a job and a higher risk of job loss.

Although this job insecurity index is provided for year 2006[16], it is still interesting to exploit across-field variation in this index and examine if job insecurity has an effect on the demand for college education. Intuitively, the demand for university majors that are tied to jobs with low job insecurity might increase. Good employment prospects might make a profession more appealing.

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[13]The mean number of applications is the ratio of total number of applications submitted each year over the number of existing university departments in a given year

[14]This book acts as an informational guide for college applicants.

[15]OAED is the Greek Manpower Employment Organization.

[16]We managed to find the book published in 2006. This book is published every year providing information about the current degree-specific job insecurity index. However, it is not easy to find the book for previous years, but only the current one.

Similarly, the popularity of college majors that are related to professions that face poor employment prospects might drop. This might affect professions subject to cuts in salaries or higher unemployment rate than other professions. These conditions create insecurity about a particular profession, sector or field of study. By restricting our sample to the year 2006, we exploit across-university and across-field variation in the job insecurity index to examine if job insecurity associated with a specific degree or field of study affects demand for college education.

In particular, we investigate the effect on job insecurity on college demand with the following regression model:

$$Y_{d,f,c} = b_0 + b_1 JobInsecurity_p + b_2 UniversityFE_u + b_3 CityFE_c + \epsilon_{d,f,c} \quad \textbf{(1)}$$

where [p]=degree or field of study

where $Y_{i,f,c,t}$ indicates the number of applications a particular degree d in field f and city c attracted that reported it as top, second or third choice.

The coefficient of interest is b_1, indicating how job insecurity affects demand for college degrees. The job insecurity index could refer to the expected employment prospects a specific degree or a specific field yields. In all specifications, we use campus-city fixed effects to control for unobservable time-invariant characteristics in campus-city demographics and characteristics that could drive students' preferences. Students might prefer a specific college because dorms in this city are modern and better-equipped or because the campus is in a lively city. We control for university-specific factors that affect students' preferences and are constant over time by including a full set of university fixed effects. For example, university fixed effects capture any "brand" or reputation effects, as well as other time-invariant unobserved characteristics (different faculty/ student ratio by university, level of resources), that could affect students' preferences. We control for these unobserved characteristics and we try to isolate the effect that changes in the unemployment rate could have on students' preferences to study one particular major over another. Standard errors are clustered at the degree level.

Although the job insecurity index is only reported for different degrees in 2006, it gives us an indication about the overall job-market prospects related to each field. Another, rather broader measure to examine economic conditions and employment quality is the unemployment rate. We examine the time variation of the uncertainty regarding the phase of the economy by looking at the effect of the annual unemployment rate on the demand for degrees in various fields and years.

Analysis of College Majors

In this section, we investigate the effect of the annual youth unemployment on the number of ordered applications submitted in each field with the following regression model:

$$Y_{d,f,c,t} = b_0 + b_1 Unemployment_t + b_2 FieldFE_f \times Unemployment_t + b_3 FieldFE_f +$$
$$b_4 CampusCityFE_c + +b_5 UniversityFE_f + \epsilon_{d,f,c,t} \textbf{(2)}$$

where $Y_{i,f,c,t}$ indicates the number of applications a degree d, in field f, in city c, and year t attracted that reported it as top, second, third, and later choice.

The main coefficient of interest, estimated by standard OLS, is b_2 and measures the effect of youth unemployment on the popularity of each field relative to a benchmark field. Field fixed effects control for mean differences in the popularity of departments that offer degrees in different fields. A field is more popular than another when degrees in that field receive more applications that list them in higher positions in the preference list. We include campus-city and university fixed effects to control for unobserved time - invariant campus city- and university-related factors. Unemployment refers to annual unemployment in the country for people between the ages of 18 and 25 (youth unemployment), and is taken from World Bank statistical reports. The standard errors are clustered at the degree level.

One might worry about potential confounding factors that may have occurred during the recession, and that could affect the demand for higher education and for specific fields. As discussed in a previous section[17], there are no college costs (e.g. tuition and fees) that may alter students' preferences when unemployment rate is high. So students' ability to take out a loan, in this case, does not seem to be very relevant. However, one might worry that changes in the supply of degrees could happen during a recession, and might affect students' choices. We are able to net out supply effects by looking at students' preferences and not the actual outcomes of college applications. To control for possible changes in location that might occur, if, say, a specific university switches the campus-city where a degree course will be offered, we add in some specifications for both university and city campus fixed effects.

From a university perspective, we provide suggestive evidence that the number of existing university department providing degrees in each field does not change significantly (Table 2). Additionally, we believe that no considerable institutional changes within or across universities that may have occurred by 2011 that could affect the demand for higher education and/ or for specific fields. After all, any systematic differences across institutions that are constant over time are captured by the university fixed effects and will not bias our estimate.

One might be concerned that the increase in the unemployment rate might coincide with professor salary cuts and significant drops in research funds that could threaten universities' quality. Any concerns about falling in academic standards and differences in university quality due to the recession are alleviated by the fact that our analysis stops in 2011, when harsh austerity measures had not yet been implemented. For robustness, we include a full set of university-specific, linear time trends to control for any unobserved factors that could change over time within universities. Another worry could be that some campus-city might experience a stronger deterioration in the

[17]Section 4.3

services that they provide (entertainment, library closures, dorms, etc), and thus they might become less or more appealing to students after 2009. To address the concern that there could be campus-city trends in unobserved factors correlated with the unemployment rate, we add to the above regression model a full set of campus-specific, linear time trends.

4.5.3 University admission cut-offs

Degree cut-offs express students' valuations for the corresponding degrees. Table 8 provides a list with the ranking of fields based on their average cut-off values in the period 2005-2011. The factors determining the admission cut-offs are discussed in details in a previous section (Section 2.2). A higher demand for specific fields, as a result of the business cycle, might increase the admission cut-offs of related university departments. This would make admission to specific degree programs more difficult.

To investigate the effect of students' preferences over specific degrees on degrees' cut-off marks, we propose the following regression:

$$DegreeCutoff_{d,f,c,t} = b_0 + b_1 NumberofFirstChoiceApplications_{d,c,t} + b_9 Controls_{d,f,c,t} + b_3 FieldFE_f + b_7 YearFE_t + b_4 CityFE_c + b_5 UniversityFE_f + \epsilon_{d,f,c,t} \quad \textbf{(3)}$$

We regress the cut-off score of a degree d in field f in city c and year t on the number of applications submitted as students' first (but also second, third and later) choice as well as other controls. The main controls are some annual variables, such as the proportion of females, the proportion of students from public or private schools, the aggregate supply of university seats, a measure for the easiness of the exam, a dummy if the tertiary academic institution is an academic university or a university of applied sciences (a technological educational institutes)[18] measure for the easiness of the exam.[19] To control for field, time, campus city and university unobserved heterogeneity we include field, time, campus city, and university fixed effects.

4.6 Main Results

Figure 4 displays the *proportion* of first-choice applications submitted for degree programs in each field averaged over all years in the sample. It shows that the largest percentage of college applicants aspire to study the field consisting of Education, Greek and Foreign Language departments

[18]Technological educational institutes (or universities of applied sciences) offer undergraduate programs. They offer four-years degrees, and are recognised by the state. Twelfth-grade students who take national exams can report in their preference list degrees from both: academic universities and technological educational institutes. Since 2008 these institutions have offered postgraduate degree programs that lead to a master's degree.

[19]We calculate the average national exam performance of students who take the national exams each year. Assuming that cohorts are of similar academic quality across time, the only change from one year to another is the overall difficulty of the exam. If the overall performance in one year is greater than that of another year, then we assume that the exams were on average easier that year.

and the smallest percentage Home Economics. Figure 5 shows the *weighted popularity index* of degree programs submitted as first choice in each field averaged over all years in the sample. The fields that receives the most first-choice applications given their supply over all years are Law and Psychology. The least number of first-choice applications are submitted to Agriculture and Forestry departments.

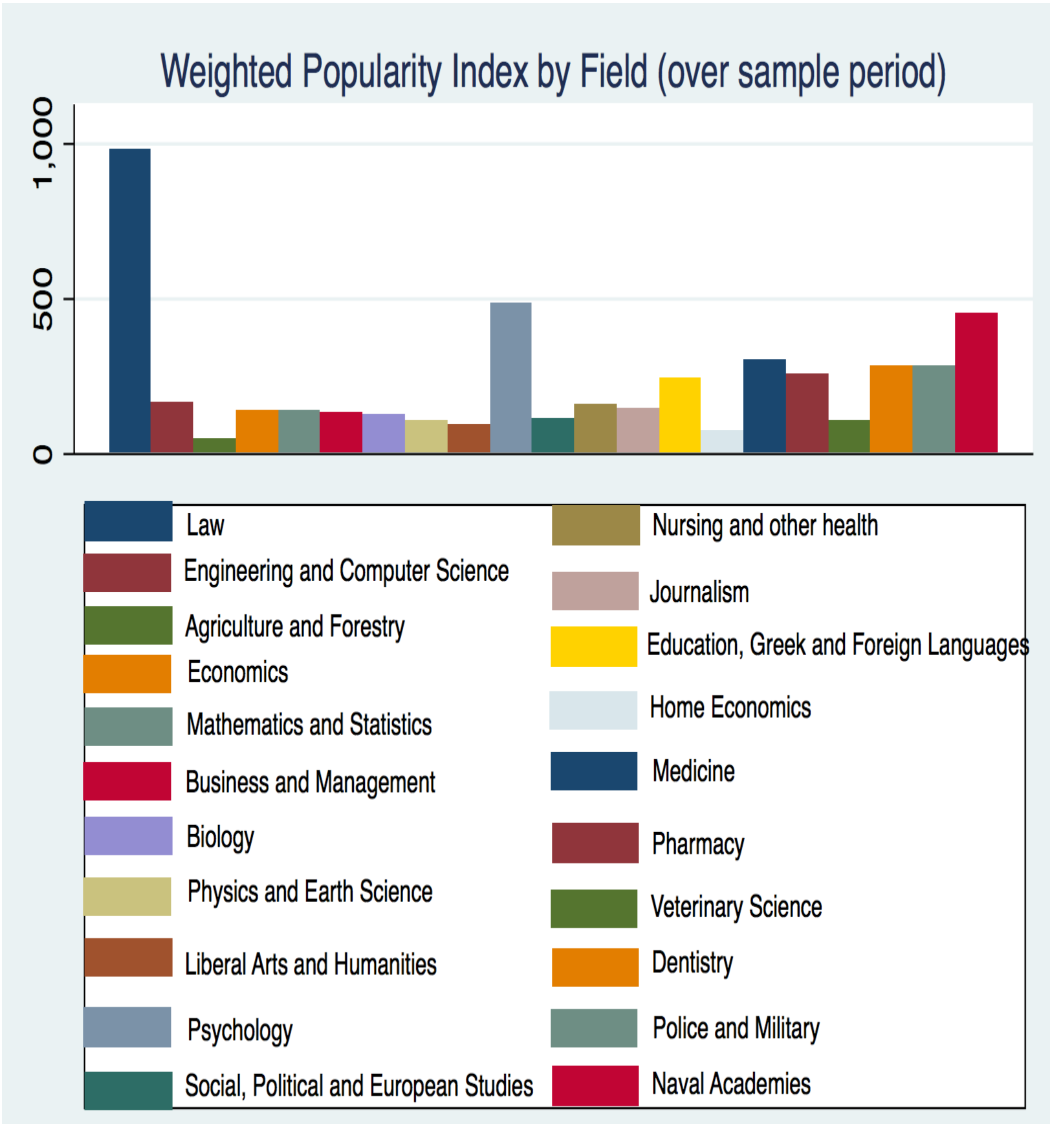
This analysis considers 22 major categories. Table 4 details good employment majors versus poor employment majors as indicated by the value of the degree insecurity index in 2006. The higher the job insecurity is, the worse the employment prospects are. The job insecurity index takes values from 1 to 1.5 for degrees that are characterized by " good employment prospects," 1.5 to 2 for " mediocre employment prospects," 2 to 2.5 for " poor employment prospects," and 2.5 to 3 for " very poor employment prospects." For example, for the enrolling cohort of 2006, studying Engineering and Computer Science offers better employment prospects than studying Agriculture and Forestry; a student embarking on a degree course in Social Political and European Studies faces worse employment prospects than a student studying Mathematics and Statistics.

4.6.1 Degree Preferences and Employment Prospects

Table 5 reports OLS results using equation (1) for the 2006 cohort. In Panel A, we regress the number of degree applications submitted as top, second and third option on a *degree* job insecurity index. The estimates are negative across specifications and statistically significant. When the job insecurity index of a specific degree increases by 1, then the related degree receives 62, 50 and 40 fewer applications listing it as the first, second and third option, respectively (columns 1, 4 and 7). For example, a degree that has good employment prospects (i.e. a degree in the department of Police and Military with job insecurity index=1.08) receives on average 62, 50 or 40 more first, second and third option applications than a degree that has poor employment prospects (i.e. a degree in the department of Journalism with job insecurity index=2.2). In columns 2, 3, 5, 6, 8 and 9 we add university, field and campus-city fixed effects to control for unobserved heterogeneity at the university, field and campus-city level. Our estimates remain negative and statistically significant. Changes in the degree job insecurity index affect more students' first choice preferences, as in it indicated by the higher in magnitude coefficients compared to their second and third choices.

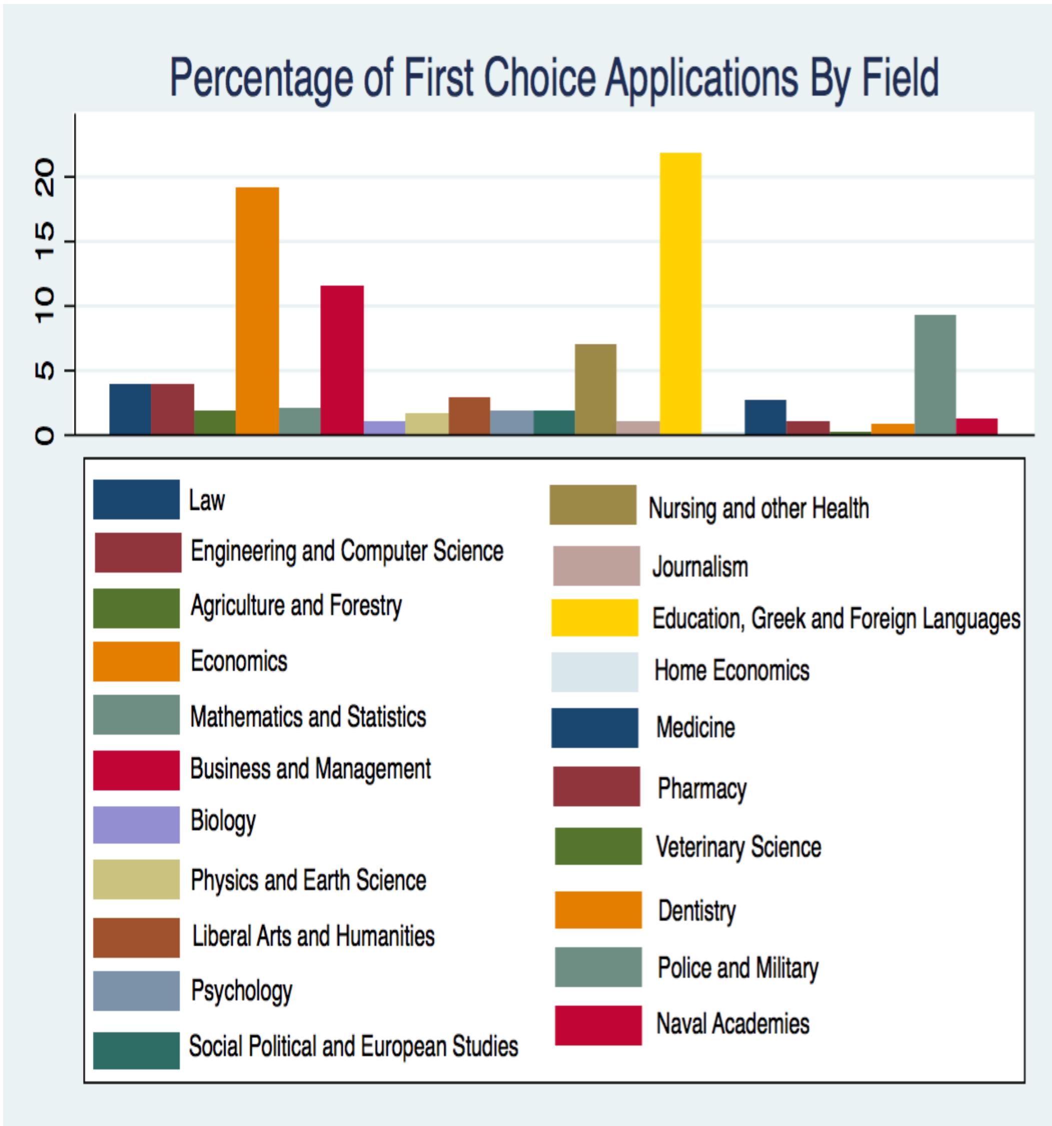
In Panel B, we regress the number of degree applications submitted as top, second and third option on a *field* job insecurity index. We find that when the job insecurity index associated with a field increases by 1 (for example if biology's employment prospects change from good to mediocre), then the related degree receives 53, 44 and 33 fewer applications that list it as first, second and third options respectively (columns 1,4 and 7). The inclusion of campus-city and university fixed effects in columns 2, 5, 8, and 3, 6, 9, respectively, hardly affects the results. Results from both panels support our hypothesis, that students react to changes in the economy and employment prospects related to specific degrees and fields. Students seem to prefer degrees and

Figure 4.4: Number of applications submitted as top choices per field



This figure shows the numbers of first choice applications submitted to degrees in each field. These numbers are averaged over all years in the sample.

Figure 4.5: Percentage of applications submitted as top choices per field



This figure shows the percentage of students who submitted a first choice application to degrees in each field. These percentages are calculated using all years in the sample.

Table 4.3: EVOLUTION OF WEIGHTED POPULARITY INDEX OVER TIME AND FIELDS

	2005	2006	2007	2008	2009	2010	2011
Engineering and computer science	194	164	127	115	103	122	149
Agriculture and forestry	73	45	32	31	30	49	58
Economics	181	167	163	152	147	163	185
Mathematics and Statistics	113	127	128	111	142	164	188
Business and Management	180	154	133	113	93	105	135
Biology	109	143	104	103	96	138	190
Other	92	68	46	45	37	70	68
Physics and Earth Science	102	100	95	97	103	120	136
Liberal Arts and Humanities	94	97	82	82	73	117	130
Psychology	419	510	443	378	312	525	791
Social, Political and European Studies	94	101	111	95	110	130	131
Nursing and other Health	147	184	134	129	108	203	208
Journalism	167	131	145	133	108	163	152
Education, Language, History and P.E.	230	262	240	238	228	225	304
Home economics	67	74	113	102	73	44	25
Medicine	298	287	249	261	274	222	527
Pharmacy	182	225	227	258	235	295	360
Law	871	1016	995	943	815	762	1470
Veterinary Science	95	98	82	80	70	126	177
Dentistry	289	278	249	267	269	265	346
Police and Military	290	298	277	227	261	280	343
Naval Academies	691	293	212	192	170	405	1226
Youth Unemployment (%)	25.3	24.8	22.5	22.0	25.5	32.4	44.1
Mean # applicants	174	169	145	135	125	149	188

Note: The table shows ratio between total number of college applications listing a university department in a particular field as their number one choice in some year over the number of existing university departments in that field in that year. Source of youth unemployment data: World Bank. Mean # applicants is the ratio of the total number of applicants over the number of existing university departments in a given year.

Table 4.4: College Majors and Respective Job Insecurity Index

Insecurity Index : >=1 and <1.5	>=1.5 and <=2	>2 and <=2.5	>2.5 and 3
Employment Prospects are:			
Good	**Mediocre**	**Poor**	**Very Poor**
Economics	Mathematics and Statistics	Education, Greek,	Agriculture and Forestry
Engineering and Computer science	Business and Management	Foreign languages and P.E.	Liberal Art and Humanities
Biology	Physics and Earth Science	Social Political and European Studies	Home Economics
Nursing and other Health	Psychology	Other	
Medicine	Law	Journalism	
Pharmacy			
Naval Academies			
Police and Military			
Veterinary Science			

Note: We derive a field-specific job insecurity index using the job insecurity index for each university department (degree). This measure is constructed using data from series of long-term questionnaire surveys of college graduates in Greece published in Katsikas (2006). This index refers to students who apply to university departments in year 2006.

fields that include a low job insecurity index and imply better employment prospects.

4.6.2 Unemployment and Fields of Study

We then look at the effect of time-varying youth unemployment on the demand for specific fields of study while we look for the whole sample. Tables 6 and 7 report OLS estimates using equation (2). We find that a unit increase in youth unemployment increases the number of applications each degree receives by approximately 1 on average (Table 6). We examine the effect of the unemployment rate on the demand for degree applications submitted as first choice (Table 6, columns 1 and 2), second choice (Table 6, columns 3 and 4), third choice (Table 7, columns 1 and 2) and later choice (Table 7, columns 3 and 4). The omitted field here is Economics. So, the effect of unemployment on the popularity of each field is interpreted compared to Economics. We use economics as our benchmark major, because the changes in the Weighted Popularity Index of Economics degrees over the years are relatively small, as shown in Table 4.

To start with, a unit increase in youth unemployment causes an one unit decrease in the number of first-, second-, and third-choice applications each university department offering a Business and Management degree receives on average. On the other hand, a unit increase in unemployment induces the number of first-, second-, and third-choice applications to each university department offering a Psychology degree to rise by approximately 17, 11 and 11 respectively. The potential increase in the prevalence of depression and mental health during the financial crisis (Caroli and Godard 2016; Cooper 2011; McInerney, Mellor, and Nicholas 2013; Uutela 2010) may explain the rise in the popularity of Psychology degrees. Similarly, a unit increase in youth unemployment increases the number of top-, second-, and third-choice applications each university department offering a Law degree receives by approximately 20, 13 and 10, respectively.

During the recession, there is an increase in students' reported top, second and third preference for destinations such as Military and Naval Academies and fields such as Mathematics and Statistics, Humanities and Liberal Art, Nursing, Veterinary Science, Pharmacy, Medicine, Psychology, Journalism, Biology, and Law. Conversely, Home Economics, Business and Management, Engineering and Computer Science fall in popularity during the crisis. Our findings are in parallel with job categorizations presented in Shatkin (2008)[20] who report that job opportunities in the Military and Health Care sectors are relatively less affected during economic turmoil. Furthermore, as he reports, the wage gap across sectors diminishes during a recession, and thus Humanities and Liberal Art jobs become more popular, as opposed to Engineering and Computer Science jobs.

[20] Shatkin (2008) book "150 Best Recession-Proof Jobs" examines the most secure jobs for the U.S. market. Using databases of the U.S. Department of Labor and the U.S. Census Bureau, and occupational outlook ratings from the Bureau of Labor Statistics, which projects job growth and future job openings for more than 750 occupations, the author identified various jobs' sensitivity to changes in the economy and the projected outlook for jobs for the next 10 years. The author also lists the most recession-proof metropolitan areas and states, the most recession-proof skills, and the jobs that are very sensitive to recession.

Table 4.5: EFFECT OF JOB INSECURITY ON COLLEGE APPLICATIONS IN YEAR 2006

Panel A: Effect of university-specific insecurity on demand for university degrees									
	Dependent Variable: Number of Degree Applications submitted as								
	Top Choice			Second Choice			Third Choice		
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Degree Job Insecurity	-62.100	-55.587	-55.883	-50.562	-43.327	-44.262	-40.423	-32.594	-43.123
	(12.402)***	(13.452)***	(22.712)**	(8.261)***	(9.058)***	(13.325)***	(7.425)***	(8.464)***	(9.975)***
Campus city FE		✓	✓		✓	✓		✓	✓
Field FE			✓			✓			✓
University FE			✓			✓			✓
R^2	0.04	0.19	0.44	0.06	0.20	0.45	0.05	0.20	0.32
Observations	483	483	483	483	483	483	483	483	483

Panel B: Effect of field-specific insecurity on demand for university degrees									
	Dependent Variable: Number of Degree Applications submitted as								
	Top Choice			Second Choice			Third Choice		
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Field Job Insecurity	-53.449	-60.142	-57.496	-44.128	-39.023	-49.063	-33.087	-23.507	-41.610
	(20.510)***	(24.176)**	(17.601)***	(13.073)***	(15.838)**	(15.291)***	(10.727)***	(12.453)*	(14.594)***
Campus city FE		✓	✓		✓	✓		✓	✓
University FE			✓			✓			✓
R^2	0.01	0.17	0.33	0.02	0.18	0.29	0.01	0.18	0.28
Observations	483	483	483	483	483	483	483	483	483

Note: A constant is also included. Standard errors are clustered at the degree level. *,**,*** denotes significance at the 10%,5% and 1% level respectively.

The construction industry suffers heavily during the recent recession, in Greece, as housebuilding, public infrastructure and major development projects stalled.

It's interesting to explicitly look at the effect of the unemployment rate on the popularity of degrees that guarantee an early source of income: degrees from Police, Military as well as Naval Academies.[21] Our findings show that a unit increase in youth unemployment causes a 2-, 3- and 4- units increase, respectively, in the number of top-, second-, and third-, choice applications each military academy receives on average. In addition, a unit increase in unemployment lead the number of top and second choice applications each naval academy receives to rise by approximately 42 and 32 respectively. The military in Greece permits students to enlist and pursue tertiary education at the same time.[22] Individuals who join the armed forces sign an enlistment contract, binding them to service after graduation; in exchange, they immediately begin receiving a monthly stipend. In addition, immediately after completing their degrees at naval academies, graduates are guaranteed work serving on ships, and offered certain specialized training free of cost. Moreover, they have the opportunity to pursue high-paying careers as captains or engineers in commercial shipping. Greece's commercial shipping industry remained among the strongest in the world even during the recent recession, and therefore, employees of ship companies suffered few layoffs, and experienced low or no reductions in wages.

In Figure 6, we draw the percentage of college applications that listed military and police academies as their number-one choice over time (in the left panel). We see that it follows a pattern similar to that of youth unemployment (right panel) with time lag. This is natural as students report preferences based on expectations.

Our results in Tables 6 and 7 are fully aligned with the findings of Arcidiacono (2004) who suggests that college students tend to switch away from degrees that are relatively more challenging (i.e. engineering and computer science) when these degrees don't promise higher economic returns in comparison to other available degrees. Arcidiacono (2004) specifically mentions that fewer students choose to major in business or engineering, when no return premium is anticipated after graduation. We find that a unit increase in youth unemployment decreases the number of first-, second-, and third-choice applications each university engineering program receives by 0.4-0.5 on average.

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[21] Naval academies are Military Academies. Their main responsibility is to educate and train competent Naval Officers for the Hellenic Navy. The academies also educate Deck and Engineering Naval cadets. They also educate Supply Officer cadets as well as Coast Guard Officer cadets.

[22] Interested students include combined choices in their preference list. For example one may list "Economics major while in the armed forces". Both men and women can enlist in the armed forces.

Table 4.6: THE EFFECT OF UNEMPLOYMENT ON FIRST AND SECOND CHOICES

	Top Choice		Second Choice	
	(1)	(2)	(3)	(4)
Unemployment	1.053 (0.000)***	0.656 (0.293)**	1.311 (0.000)***	1.146 (0.220)***
Unemployment × Computer Science and Engineering	-0.448 (0.041)***	-0.008 (0.257)	-0.623 (0.037)***	-0.494 (0.231)**
Unemployment × Agriculture and Forestry	-0.135 (0.000)***	0.502 (0534)	-0.498 (0.000)***	-0.014 (0.361)
Unemployment × Mathematics and Statistics	2.274 (0.000)***	2.671 (0.393)***	1.776 (0.000)***	1.940 (0.219)***
Unemployment × Business and Management	-1.069 (0.055)***	-0.477 (0.395)	-1.393 (0.027)***	-1.180 (0.268)***
Unemployment × Biology	2.762 (0.000)***	3.159 (0.293)***	2.216 (0.000)***	2.379 (0.220)***
Unemployment × Other	-0.341 (0.020)***	0.922 (0.313)**	-0.323 (0.024)***	0.184 (0.237)
Unemployment × Physics and Earth Science	0.830 (0.000)***	1.057 (0.308)***	0.600 (0.000)***	0.788 (0.221)***
Unemployment × Liberal Arts and Humanities	1.263 (0.005)***	1.565 (0.323)***	0.859 (0.006)***	0.989 (0.243)***
Unemployment × Psychology	16.608 (0.000)***	17.005 (0.293)***	11.428 (0.000)***	11.591 (0.220)***
Unemployment × Social, Political and European Studies	1.332 (0.289)***	1.810 (0.450)***	1.127 (0.038)***	1.060 (0.249)***
Unemployment × Nursing and other health	3.171 (0.084)***	3.397 (0.314)***	2.788 (0.565)***	2.777 (0.246)***
Unemployment × Journalism	-0.107 (0.000)***	0.290 (0.293)	0.827 (0.000)***	0.990 (0.220)***
Unemployment × Education, Language, History and P.E.	1.494 (0.009)***	1.692 (0.316)***	0.648 (0.007)***	0.745 (0.233)***
Unemployment × Home Economics	-4.527 (0.000)***	-4.130 (0.293)***	-3.126 (0.000)***	-2.963 (0.220)***
Unemployment × Medicine	9.632 (0.000)***	10.028 (0.293)***	6.827 (0.000)***	6.990 (0.220)***
Unemployment × Pharmacy	5.330 (0.000)***	5.727 (0.293)***	2.945 (0.000)***	3.109 (0.220)***
Unemployment × Law	19.737 (0.000)***	20.134 (0.293)***	13.088 (0.000)***	13.251 (0.220)***
Unemployment × Veterinary Science	3.439 (0.000)***	3.836 (0.293)***	3.243 (0.000)***	3.406 (0.220)***
Unemployment × Dentistry	2.337 (0.000)***	2.734 (0.293)***	2.663 (0.000)***	2.827 (0.220)***
Unemployment × Police & Military	2.355 (0.000)***	2.925 (0.293)***	3.623 (0.026)***	3.891 (0.220)***
Unemployment × Naval Academies	41.531 (0.000)***	41.928 (0.293)***	32.698 (0.000)***	32.861 (0.220)***
Fields and Campus F.E.	✓	✓	✓	✓
University F.E.		✓		✓
R^2	0.32	0.42	0.37	0.45

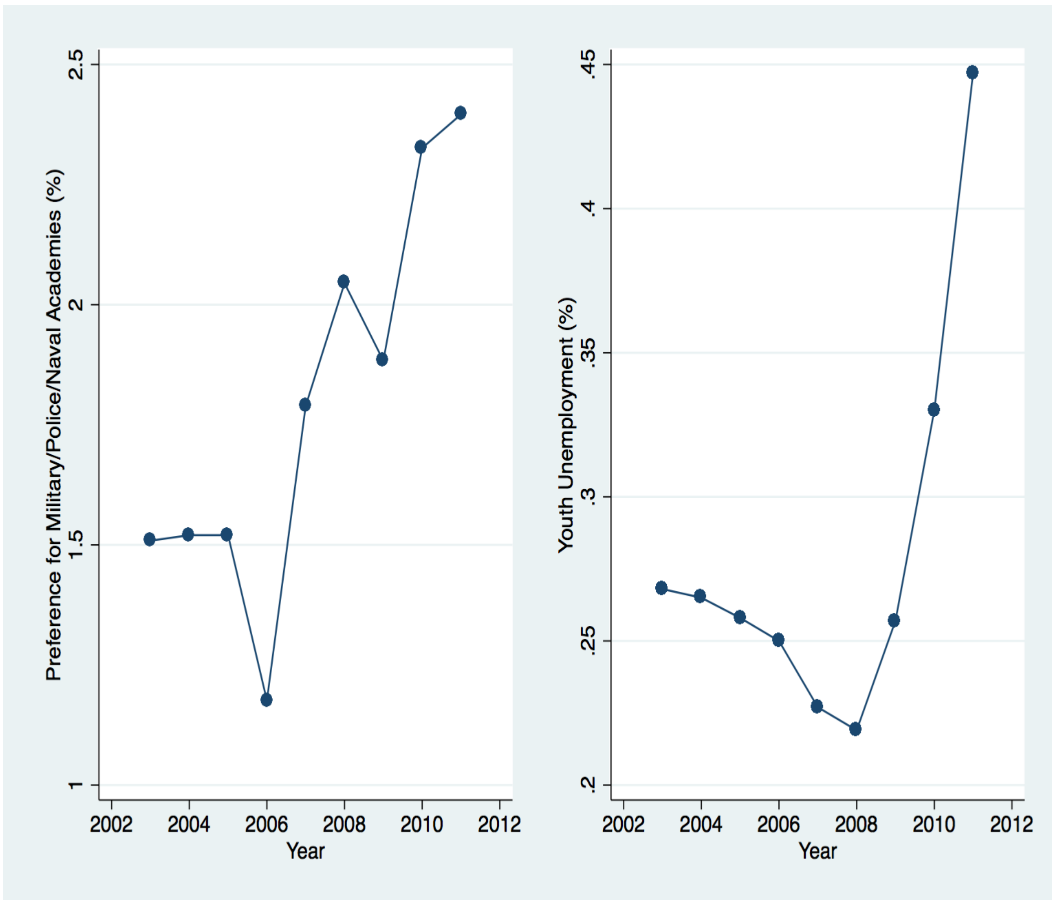
Note: An intercept is included. Number of observations: 3,448 degrees. 43 universities are used. Economics is used as the benchmark field. Standard error are clustered at the field level.

Table 4.7: THE EFFECT OF UNEMPLOYMENT ON THIRD AND LATER CHOICES

	Third Choice		Outside Top3 Choice	
	(1)	(2)	(3)	(4)
Unemployment	1.396 (0.000)***	1.279 (0.291)**	16.967 (0.000)***	12.753 (4.021)**
Unemployment × Computer Science and Engineering	-0.555 (0.000)***	-0.413 (0.296)	19.317 (0.471)***	21.381 (4.021)**
Unemployment × Agriculture and Forestry	-0.450 (0.000)***	-0.029 (0.416)	-18.800 (0.000)***	-28.315 (9.950)**
Unemployment × Mathematics and Statistics	1.249 (0.000)***	1.366 (0.291)***	13.522 (0.000)***	17.736 (4.021)***
Unemployment × Business and Management	-1.359 (0.101)***	-1.197 (0.278)***	-20.030 (0.948)***	-16.442 (5.127)***
Unemployment × Biology	2.077 (0.000)***	2.194 (0.291)***	67.383 (0.000)***	71.596 (4.021)***
Unemployment × Other	-0.406 (0.018)***	-0.306 (0.349)	23.332 (0.639)***	23.746 (5.225)***
Unemployment × Physics and Earth Science	0.912 (0.000)***	1.017 (0.319)**	10.221 (0.000)***	8.841 (4.616)*
Unemployment × Liberal Arts and Humanities	0.649 (0.004)***	0.662 (0.338)*	41.517 (0.766)***	49.244 (4.426)***
Unemployment × Psychology	11.333 (0.000)***	11.450 (0.291)***	36.944 (0.000)***	41.158 (4.021)***
Unemployment × Social, Political and European Studies	0.980 (0.517)***	1.053 (0.496)**	64.637 (0.981)***	71.129 (3.484)***
Unemployment × Nursing and other health	3.528 (0.053)***	3.545 (0.298)***	151.288 (1.182)***	158.163 (3.883)***
Unemployment × Journalism	0.783 (0.517)***	0.900 (0.291)**	102.023 (0.000)***	106.237 (4.021)***
Unemployment × Education, Language, History and P.E.	0.167 (0.008)***	0.275 (0.308)	21.793 (0.142)***	23.937 (4.976)***
Unemployment × Home Economics	-3.167 (0.000)***	-3.050 (0.291)***	-34.510 (0.000)***	-30.296 (4.021)***
Unemployment × Medicine	6.488 (0.000)***	6.604 (0.291)***	35.548 (0.000)***	39.762 (4.021)***
Unemployment × Pharmacy	2.624 (0.000)***	2.741 (0.291)***	53.548 (0.000)***	58.049 (4.021)***
Unemployment × Law	9.949 (0.000)***	10.066 (0.291)***	16.729 (0.000)***	20.942 (4.021)***
Unemployment × Veterinary Science	0.478 (0.000)***	0.594 (0.291)*	30.461 (0.000)***	34.675 (4.021)***
Unemployment × Dentistry	0.291 (0.000)***	0.408 (0.291)	43.496 (0.000)***	47.710 (4.021)***
Unemployment × Police & Military	3.684 (0.024)***	3.903 (0.291)***	8.794 (0.214)***	13.408 (4.021)***
Unemployment × Naval Academies	5.336 (0.000)***	5.452 (0.291)***	57.544 (0.000)***	61.759 (4.021)***
Fields and Campus F.E.	✓	✓	✓	✓
University F.E.		✓		✓
R^2	0.32	0.39	0.37	0.43

Note: An intercept is included. Number of observations: 3,448 degrees. 43 universities are used. Economics is used as the benchmark field. Standard error are clustered at the field level.

Figure 4.6: Preference for Military/ Police/ Naval Academies and Youth Unemployment



The left figure depicts the percentage of college applicants per year that listed military or police or naval related majors as their most preferred choice. The right figure shows annual youth unemployment rate (%). Source for unemployment data: World Bank.

We also report the effect of unemployment on the number of later-choice applications[23] submitted to university departments. As we explained in a previous section (Section 2.2), college applications in Greece bear no cost. In a framework of cost-less applications, each individual has incentive to include every department in their preference list. Potentially, the only difference from one preference list to the next applicant's list is the ordering of the university departments. However, the direction of the effect of the unemployment rate on later choice applications indicated in columns (3) and (4) (Table 7) is not much different than before. For example, a unit increase in unemployment reduces later choice applications (outside top 3 applications) received by Agriculture and Forestry, and Business and Management departments by 18 and 20, respectively, or 28 and 16 respectively when university fixed effects are included. On the other hand, Police, Military and Naval Academies receive more later-choice applications when unemployment rises. As before, Law, Medicine, and Psychology departments become more popular when the overall uncertainty in the economy increases. Results remain almost unchanged when university fixed effects are included.

To make sure that our results for the effect of unemployment on the demand for different fields are not driven by university- or campus-city-specific time trends, that are correlated with the unemployment rate, we include a university- or campus-city-specific linear time trend. These robustness results are presented in Tables 10 and 11. Some coefficients slightly change while some others become statistically insignificant. A couple coefficients flip sign, but they become statistically insignificant. Overall, our results remain unchanged regarding which fields experience a drop or a rise in popularity when unemployment rises.

4.6.3 University Admission Thresholds

Then we look at the effect of students' reported college preferences on degrees' cut-off scores. If the supply of seats is constant over time, but competition for those seats grows, then the degree threshold score should increase. This happens because, for a given supply, the admission score of the last student admitted should be higher when there is more competition[24] over the seats. First, we rank the university fields based on the related degrees' threshold values over the sample period. Table 8 shows that dentistry is the field with the highest cut-off value for the period 2005-2011. This means that among all fields, the most difficult one for admission (the one with the highest cut-off threshold score), over the period of 7 years, is Dentistry. Second and third most difficult fields for university admission are Medicine and Pharmacy, respectively. Over the period of 7 years that is our sample period, naval academies rank low in terms of admission thresholds. But what

[23] Students' submitted applications outside their top-three choices. For example, students' top-four choice, top-five choice, ..., top N-choice.

[24] The only exception to this could be if the average academic quality of students applying to this specific degree drops on average. However, we have no reasons to believe that the average cohort academic quality varies by time.

Table 4.8: ROBUSTNESS CHECKS: CAMPUS AND UNIVERSITY LINEAR TIME TRENDS

	Top Choice		Second Choice	
	(1)	(2)	(3)	(4)
Unemployment	3.167	3.162	2.968	2.670
	(0.530)***	(0.380)***	(0.469)***	(0.329)***
Unemployment × Computer Science and Engineering	-0.235	-0.028	-0.078	0.266
	(0.127)*	(0.405)	(0.124)	(0.352)
Unemployment × Agriculture and Forestry	0.091	-0.692	0.058	0.044
	(0.138)	(0.404)	(0.146)	(0.333)
Unemployment × Mathematics and Statistics	2.423	2.194	1.825	1.686
	(0.230)***	(0.316)***	(0.109)***	(0.247)***
Unemployment × Business and Management	-0.479	-0.092	-0.600	0.174
	(0.093)***	(0.404)	(0.094)***	(0.377)
Unemployment × Biology	2.912	1.789	2.811	2.151
	(0.211)***	(0.321)***	(0.140)***	(0.288)***
Unemployment × Physics and Earth Science	0.791	-0.172	0.646	0.379
	(0.174)***	(0.356)	(0.141)***	(0.370)
Unemployment × Liberal Arts and Humanities	1.173	0.527	0.932	0.715
	(0.128)***	(0.559)	(0.113)***	(0.422)*
Unemployment × Psychology	16.255	15.653	11.093	10.884
	(0.317)***	(0.427)***	(0.285)***	(0.316)***
Unemployment × Social, Political and European Studies	1.516	0.063	1.067	0.455
	(0.341)***	(0.501)	(0.135)***	(0.284)
Unemployment × Nursing and other health	3.485	3.844	3.158	3.885
	(0.175)***	(0.677)***	(0.125)***	(0.532)***
Unemployment × Journalism	0.832	-0.133	1.586	1.290
	(0.149)***	(0.363)	(0.292)***	(0.188)***
Unemployment × Education, Language, History and P.E.	1.331	0.796	0.459	0.528
	(0.062)***	(0.400)*	(0.040)***	(0.326)***
Unemployment × Home Economics	-3.056	-5.456	-2.356	-2.667
	(0.379)***	(0.500)***	(0.197)***	(0.272)***
Unemployment × Medicine	9.545	8.604	6.985	6.422
	(0.221)***	(0.332)***	(0.112)***	(0.336)***
Unemployment × Pharmacy	5.278	4.280	3.330	3.090
	(0.184)***	(0.446)***	(0.231)***	(0.389)***
Unemployment × Law	19.369	18.829	12.519	12.962
	(0.287)***	(0.405)***	(0.190)***	(0.427)***
Unemployment × Veterinary Science	3.264	2.058	3.470	3.114
	(0.279)***	(0.458)***	(0.401)***	(0.277)***
Unemployment × Dentistry	2.426	1.131	2.948	2.468
	(0.280)***	(0.556)*	(0.224)***	(0.458)***
Unemployment × Police & Military	2.524	4.344	3.995	3.624
	(0.250)***	(1.317)***	(0.218)***	(1.059)***
Unemployment × Naval Academies	43.002	46.221	33.470	36.633
	(0.379)***	(5.740)***	(0.197)***	(4.402)***
Fields and Campus F.E.	✓		✓	
Campus City Specific Linear Time Trend	✓		✓	
Fields and University F.E.		✓		✓
University Specific Linear Time Trend		✓		✓

Note: An intercept is included. Number of observations: 3,448 degrees. 43 universities are used. Economics is used as the benchmark field. Standard errors are clustered at the field level. Estimates for the category “Other” are not reported due to space constraints.

Table 4.9: ROBUSTNESS CHECKS: CAMPUS AND UNIVERSITY LINEAR TIME TRENDS

	Third Choice		Later Choice	
	(1)	(2)	(3)	(4)
Unemployment	3.193	2.863	77.767	70.927
	(0.513)***	(0.400)***	(14.843)***	(11.193)***
Unemployment × Computer Science and Engineering	-0.069	0.375	47.502	60.028
	(0.185)	(0.263)	(4.079)***	(10.068)**
Unemployment × Agriculture and Forestry	-0.068	0.215	-33.412	20.013
	(0.256)	(0.271)	(12.214)**	(16.510)
Unemployment × Mathematics and Statistics	1.185	1.188	21.168	13.399
	(0.204)***	(0.255)***	(5.558)***	(2.718)***
Unemployment × Business and Management	-0.771	0.150	-18.508	44.460
	(0.250)**	(0.348)	(6.319)**	(14.483)***
Unemployment × Biology	2.426	1.934	89.124	60.478
	(0.220)***	(0.303)***	(9.448)***	(3.597)***
Unemployment × Physics and Earth Science	0.914	0.672	25.942	11.569
	(0.212)***	(0.296)**	(5.768)***	(3.865)***
Unemployment × Liberal Arts and Humanities	0.693	0.444	53.401	47.560
	(0.121)***	(0.286)	(5.420)***	(3.497)***
Unemployment × Psychology	10.934	10.856	36.793	28.802
	(0.262)***	(0.366)***	(3.937)***	(4.257)***
Unemployment × Social, Political and European Studies	0.826	0.736	66.034	62.470
	(0.534)	(0.274)**	(4.134)***	(3.953)***
Unemployment × Nursing and other health	3.765	4.738	169.704	215.079
	(0.219)***	(0.422)***	(6.471)***	(22.207)***
Unemployment × Journalism	1.282	1.251	130.280	125.705
	(0.588)**	(0.228)***	(27.812)***	(8.478)***
Unemployment × Education, Language, History and P.E.	0.017	0.217	25.934	21.384
	(0.071)	(0.281)	(3.443)***	(3.579)***
Unemployment × Home Economics	-2.692	-2.484	-22.857	-5.632
	(0.248)***	(0.443)***	(3.210)***	(10.867)
Unemployment × Medicine	6.523	6.094	49.985	26.719
	(0.244)***	(0.341)***	(8.929)***	(4.085)***
Unemployment × Pharmacy	2.926	2.615	61.664	51.164
	(0.211)***	(0.370)***	(4.245)***	(3.982)***
Unemployment × Law	9.649	9.773	8.038	7.798
	(0.424)***	(0.542)***	(3.165)***	(3.965)***
Unemployment × Veterinary Science	0.911	0.305	75.797	23.881
	(0.426)**	(0.273)	(29.541)***	(5.302)***
Unemployment × Dentistry	0.476	-0.002	49.951	38.173
	(0.162)***	(0.527)	(3.845)***	(4.844)***
Unemployment × Police & Military	4.081	2.325	12.506	-36.243
	(0.272)***	(0.408)***	(3.994)***	(11.149)***
Unemployment × Naval Academies	5.811	5.242	69.198	19.772
	(0.248)***	(0.684)***	(3.210)***	(18.507)
Fields and Campus F.E.	✓		✓	
Campus City Linear Time Trend	✓		✓	
Fields and University F.E.		✓		✓
University Linear Time Trend		✓		✓

Note: An intercept is included. Number of observations: 3,448 degrees. 43 universities are used. Economics is used as the benchmark field. Standard errors are clustered at the field level. Estimates for the category "Other" are not reported due to space constraints.

is interesting is that, as the unemployment rate increases, Naval academies become more popular and possibly more difficult to enter.

Table 4.10: Ranking of fields based on threshold scores

Field	Threshold	Rank
Dentistry	17,816	1
Medicine	17,563	2
Pharmacy	16,706	3
Military and police	16,601	4
Veterinary Science	16,157	5
Law	16,058	6
Psychology	15,493	7
Home Economics	14,659	8
Biology	14,437	9
Mathematics and Statistics	13,119	10
Education,Language, History and P.E	12,937	11
Engineering and Computer Science	12,510	12
Physics and Earth Science	12,442	13
Social,Political and European Studies	12,162	14
Journalism	11,899	15
Economics	11,813	16
Nursing and Other Health	11,442	17
Liberal Art and Humanities	11,358	18
Business and Management	10,571	19
Other	10,372	10
Agriculture and Forestry	9,165	21
Naval Academies	7,851	22

Note: The "threshold score" or the "cut-off score" for admission for most university departments varies from 0 to 20,000. The higher the threshold value is, the more difficult it is for a student to gain admission. Some university departments require students to take exams in "special subjects" (for example some Architecture departments require students to take an exam in architectural design) and the maximum threshold value for these degrees could exceed 20,000.

Then in Table 9 we present OLS estimates for equation (3). Results suggest a positive relationship between the number of first-, second-, and third-choice applications and the degree-admission threshold. Columns 1-3, shows that for each additional first choice application a degree receives, the threshold score increases by 2,331 when only field fixed effects are included. This estimate drops to 1.381 when year fixed effects are included and becomes 1.519 when campus city and university fixed effects are included. The average degree cutoff in the sample is 12,084.91 (with a s.d of 4,506.325). This means that for each additional unit of unemployment, the threshold for Psychology departments will increase by approximately (17.005*1.519) 25.8, ceteris paribus. If the unemployment rate increases by one, then Medicine, Naval, and Mathematics and Statistics departments will experience a rise in their thresholds by around 15.3, 63 and 3.8 respectively, ceteris paribus. These numbers translate to 1 percent , 2.5 percent and 0.8 percent of the respective cut-off s.d. for degrees in medicine, Naval and, Mathematics and Statistics. For an additional second and third choice application a degree receives, the related degree admission threshold increases by 2.275 and 2.574 respectively, ceteris paribus.

In columns 10, 11 and 12 we examine if there is any effect on the admission threshold coming from later-choice applications. As we expected, there is a negative and statistically insignificant relationship between the number of later-choices applications and degree admission cut-offs. This might be the case because students list many degrees in low positions in the preference list as a risk aversion practice. Students might report degrees that cover a large range of cut-off values in order to make sure that they will be admitted to some university department even if this year's admission threshold drops significantly. Keep in mind that when students submit their degree applications, the actual degree admission thresholds are not determined or announced. Potentially, students have incentives to report all university departments in the field they aspire to study or potentially degrees from other fields too. Thus, intuitively the number of later-choice applications should not matter for degrees' threshold determinations.

4.7 Conclusions

This paper provides the first examination of switching college majors of study as a result of the financial crisis that began in Greece in 2009. We identify the relationship between youth unemployment and the demand for specific college degrees nationwide, while netting out supply-side dynamics. We focus primarily on the abrupt expansion of the Greek college application rate, and its fluctuation around the financial crisis. We document this expansion and develop a theory of the demand for post-secondary education that stresses the importance of short-run economic conditions in the decision-making of "marginal applicants." Finally, we advance a body of empirical evidence that supports a number of the inferences of the theory regarding the role of anticipated job prospects in educational decisions.

We use unique administrative data from Greece for all existing degree programs to study whether and how students' preferences and degree admission thresholds depend on degree-, and field-related employment prospects. Using panel data for the universe of degrees over a seven-year period, we find the following: First, we show that college applicants prefer degrees and majors with lower job insecurity. Second, we find that changes in the unemployment rate have different effects on demand for different college majors. Indicatively, we find a decrease in the popularity of academically rigorous degrees in Engineering and Computer Science. We also document a decrease in the popularity of Business and Management, Journalism and Home Economics during the recession. During the crisis more people turn to Naval Academies, Police and Military Academies,

Table 4.11: THE EFFECT OF THE DEMAND FOR DEGREES ON CUT-OFF SCORES

	Dependent Variable: Degree Cut-off score											
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Number of:												
First choice Applications	2.331	1.381	1.519									
	(0.394)***	(0.366)***	(0.343)***									
Second choice Applications				3.518	2.035	2.275						
				(0.699)***	(0.695)***	(0.645)***						
Third choice Applications							3.607	2.527	2.574			
							(0.564)***	(0.527)***	(0.548)***			
Later choice Applications										-0.016	-0.031	-0.028
										(0.048)	(0.047)	(0.044)
Aggregate supply of seats	-0.030			-0.011			-0.011			-0.007		
	(0.006)***			(0.006)*			(0.006)*			(0.006)		
Easiness of the exam	3.152			3.070			3.067			3.175		
	(0.333)***			(0.331)***			(0.326)***			(0.335)***		
Field FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE		✓	✓		✓	✓		✓	✓		✓	✓
Campus city FE		✓	✓		✓	✓		✓	✓		✓	✓
University FE			✓			✓			✓			✓
Observations	2,746	2,746	2,746	2,746	2,746	2,746	2,746	2,746	2,746	2,746	2,746	2,746

Note: A constant is also included. Standard errors are clustered at the degree level. *,**,*** denotes significance at the 10%,5% and 1% level respectively. In columns (1), (4), (7) and (10), we also control for the annual percentage of girls, annual percentage of students attending a public school/private and experimental school, a dummy if the tertiary institution is an academic university or a technical academy and the annual percentage of students attending an urban school. The average degree cut-off in the sample is 12,084.91 (with a s.d of 4,506.325).

which allow students to enlist and pursue tertiary education at the same time. Student in these degree programs are also guaranteed an early source of income that may begin with enrollment in the academy itself. For example, those who join the army sign an enlistment contract, binding them to serve after graduation, and then immediately begin receiving a monthly stipend. When the unemployment rate rises, we find an increase for the medical-related majors-such as Medicine, Pharmacy, Nursing and Dentistry that lead to high-paying medical employment. We also find an increase in the popularity of Psychology degrees. We speculate that the rise in the incidence of mental health issues during the recession may explain the increase in the popularity of Psychology degrees.

Third, we find that top choice-college applications influence degrees' admission thresholds, making enrollement in degrees with a low employment-insecurity index at the time of the recession more competitive. Our findings contribute to the understanding of workforce dynamics and occupational choice during economic downturns and can inform policies that fight unemployment. Understanding the flows of post-secondary education preferences during the recession might also help to a more optimal allocation of resources.

APPENDIX A

DESCRIPTIVE STATISTICS OF GRADE RETENTION

Table A.1: Average Grade Retention

	Mean	s.d.
Retention Rate	0.038	0.191
Grade		
10th	0.058	0.235
11th	0.043	0.202
12th	0.015	0.122
Gender		
Males	0.051	0.221
Females	0.027	0.162
Age		
16 or younger	0.041	0.199
16-17	0.034	0.180
17-18	0.023	0.151
18 or older	0.190	0.393
Reason		
Due to performance	0.025	0.155
Due to absences	0.013	0.114
Class Mean Math Score		
Highest Quintile	0.021	0.143
Fourth Quintile	0.027	0.163
Third Quintile	0.036	0.186
Second Quintile	0.061	0.239
Lowest Quintile	0.068	0.251
Class size		
Highest Quintile	0.039	0.194
Fourth Quintile	0.039	0.194
Third Quintile	0.038	0.190
Second Quintile	0.040	0.196
Lowest Quintile	0.043	0.203
Year		
2006	0.038	0.191
2007	0.042	0.200
2008	0.039	0.195
2009	0.039	0.194
2010	0.041	0.197
2011	0.038	0.193
2012	0.022	0.148

Sample: 106,838 obs; 51,666 individuals.

Table A.2: Grade Retention due to absences

	Mean	s.d.
Grade		
10th	0.019	0.135
11th	0.016	0.126
12th	0.006	0.075
Midterm Math Score		
Highest Quintile	0.001	0.029
Fourth Quintile	0.001	0.024
Third Quintile	0.000	0.021
Second Quintile	0.000	0.020
Lowest Quintile	0.031	0.173
Class Mean Math Score		
Highest Quintile	0.009	0.095
Fourth Quintile	0.010	0.102
Third Quintile	0.013	0.112
Second Quintile	0.018	0.133
Lowest Quintile	0.024	0.152
Gender		
Males	0.016	0.126
Females	0.011	0.104
Age		
16 or younger	0.006	0.080
16-17	0.010	0.098
17-18	0.011	0.102
18 or older	0.149	0.356
Class size		
Highest Quintile	0.011	0.102
Fourth Quintile	0.013	0.111
Third Quintile	0.013	0.112
Second Quintile	0.015	0.122
Lowest Quintile	0.017	0.130
Year		
2006	0.013	0.115
2007	0.016	0.124
2008	0.015	0.120
2009	0.015	0.121
2010	0.011	0.107
2011	0.011	0.103
2012	0.006	0.080

Sample: 106,838 obs; 51,666 individuals.

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