HOW DOES PEER PRESSURE AFFECT EDUCATIONAL INVESTMENTS?[*]

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Abstract: When academic effort or investment is observable to peers, students may act to avoid social penalties or gain social favor (i.e., "peer pressure") by conforming to prevailing norms. To test this hypothesis, we conducted an experiment in Los Angeles high schools in which 11th grade students were offered complimentary access to a commercial, online SAT preparatory course from a well-known test company. Sign up sheets differed randomly across students (within classrooms) only in the extent to which they emphasized that the decision to enroll would be kept private from their classmates. We find that whether choices are believed to be observable to others has dramatic effects on sign up rates. Further, the effects depend greatly on the setting or prevailing peer group norm. In non-honors classes, the sign up rate was 11 percentage points lower when decisions to enroll were to be public rather than private. But sign up in honors classes was unaffected. Since the differential response in the two types of classes could be driven by differences across students in honors and non-honors classes, to further test for the effects of peer pressure we examine students taking the same number of honors classes (e.g., the set of students taking exactly two honors classes). For these students, it is essentially random whether our team arrived and offered them the course during a period in which they were in class with their honors peers or their non-honors peers. When offered the course in their non-honors class, these students were 25 percentage points less likely to sign up if the decision was public. But if they were offered the course in one of their honors classes, they were 25 percentage points *more* likely to sign up when the decision was public. These results show that students are highly responsive to who their peers are and what the prevailing norm is when they make decisions. These results also allow us to isolate peer social concerns from other peer effect mechanisms, since we changed nothing about a student's actual set of peers (or their teachers, classroom or school), only those peers actually present when decisions were made and thus to whom the choice would potentially be revealed.

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I. INTRODUCTION

It has long been argued that students are likely to be motivated as much by the desire to gain social approval (e.g., being popular or fitting in) or avoid social sanctions (e.g., being teased, made fun of or bullied, or losing social status) as they are by the future benefits of education (e.g., Coleman 1961).[1] An important question then arises as to whether, and how, student effort or investments are affected by such peer pressure.[2] In particular, are students willing to deviate from what they privately believe to be the optimal scholastic effort or investment decision just because of such social concerns? In this paper, we test this hypothesis using a randomized field experiment conducted in Los Angeles high schools.

Testing whether, and how, students' actions are motivated by peer pressure[3] or social concerns presents a number of significant challenges. First, doing so requires identifying and manipulating exposure to an action or decision that peers may sanction (since just observing a particular action, even one considered to be "bad" or "disruptive," could be consistent with many explanations other than peer pressure).[4] Additionally, there needs to be exogenous variation in the extent to which that action is observable by peers[5] in order to distinguish whether, say, conformity, is motivated internally (a desire to do what others do and fit in because of self-image) or externally (a desire to do what others do in order to avoid social sanctions). It is also important to have variation in locally prevailing norms with respect to the behavior in question,

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[1] A prominent example of such peer pressure or social effects is the "Acting White" hypothesis, whereby minorities may face social sanctions from peers if they engage in certain behaviors, such as schooling investments (e.g., see Fordham and Ogbu 1986, Austen-Smith and Fryer 2005 and Fryer 2011). Though more general peer pressure or social sanctions are ubiquitous, so disincentives to invest may be found at least to some degree among other ethnic groups as well.

[2] We define peer pressure as students taking actions that deviate from what they privately consider to be the optimal action (i.e., what they would do if others would not observe their actions) in order to achieve social gains or avoid social costs from peers. Peer pressure therefore need not just refer to active efforts or encouragements by peers to persuade others to undertake an action, but could also include passive effects such as not undertaking an action for fear of peer social sanctions or to gain peer social approval.

[3] Some studies in psychology measure peer pressure through direct survey questions, such as by asking whether a student has faced pressure from others to undertake certain actions (Brown 1982, Brown et al. 1986 and Santor et al. 2000). However, there is some concern with using such subjective self-reports, and further, it is then difficult to link these responses directly and causally to specific changes in behaviors.

[4] Fryer and Torelli (2010) find evidence consistent with the Acting White hypothesis by showing for example that the popularity of black and Hispanic students first increases in grades but then decreases (for white students, popularity increases monotonically in grades).

[5] Similarly, Mas and Moretti (2009) define social pressure in the workplace as the extent to which utility is affected by behavior when that behavior is observable by others.

in order to ensure that students are moving towards some norm, as opposed to observability itself affecting decisions for some other reason.

Second, testing for the effects of peer sanctions requires exogenous variation in peers. This challenge is common to most empirical studies of more general "peer effects" (see Epple and Romano 2011 for a summary). In our case, if we simply observe that an individual changes their behavior when it is observable, and that this effect varies across different peer groups with different norms, there could simply be selection or a difference in attributes across students in the different groups.[6] It is important to in effect hold fixed the characteristics of the individual, and simply vary the audience of peers present at the time they make their decision.

Third, even when peers can be exogenously varied, isolating the effects of peer sanctions requires the ability to rule out the many other forms of peer effects or ways in which peers may influence behavior, such as social learning or externalities from joint consumption.

We present results from a field experiment designed to measure the effects of peer pressure in a way that overcomes these challenges. In four low performing, lower income Los Angeles high schools, we offered 11th grade students complimentary access to an online SAT[7] preparatory course from a well-known private test preparation company. Across students within classrooms, we randomly varied whether the sign up sheet emphasized that the decision to enroll would be kept private from the other students in their class. In particular, students were either told their decision to enroll would be kept completely private from everyone *including* the other students in the room, or *except* those students. Notably, the sole difference between sign up forms in our "private" and "public" treatments was the single word ("including" vs. "except").

We chose both honors and AP classes and regular classes (hereafter "honors" and "non-honors") for the experiment. The online prep class is an educational investment, and making it observable to peers could carry different social costs or benefits in settings where the norms on the acceptability of effort differ, such as in honors and non-honors classes.[8]

We find that observability has a large impact on the decision to sign up for the course, and that the effects differ dramatically based on the setting. In non-honors classes, sign up is 11

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[6] In the peer effects literature, several studies have used settings with randomized peer assignments (Sacerdote 2001, Zimmerman 2003 and Carrell, Fullerton and West 2009). More recently, a few studies have explicitly randomized peers (Duflo, Dupas and Kremer 2011 and Carrel, Sacerdote and West 2013).
[7] The course also provides preparation for the ACT.
[8] And, at least consistent with this hypothesis that the prevailing norms may differ, sign up rates are much higher in honors than non-honors classes when the decision is private.

percentage points lower when students believe others in the class will know whether they signed up, compared to if they believed it would be kept private. In honors classes, there is no difference in sign up rates under the two conditions.

Consistent with these results being driven by a concern over peer social effects, in non-honors classes, students who say that it is important to be popular are less likely to sign up when the decision is public rather than private, whereas students who say it is not important are not affected at all. In honors classes, students who say it is important to be popular are slightly more likely to sign up when the decision is public (though the effect is not statistically significant, due in part perhaps to the fact that sign up rates are already high) whereas those who say it is not important to be popular are again unaffected. Thus in both cases, students concerned with popularity move in the direction of the prevailing norm when the decision is public, whereas those unconcerned with popularity do not change their behavior at all based on whether it is public or private.

However, the results mentioned so far are also consistent with explanations other than peer pressure or social concerns. One possible alternative explanation of the honors vs. non-honors difference in how students respond to whether sign up is public vs. private is selection into honors and non-honors classes. This would not change the important policy interpretation that observability has a big, negative impact in non-honors classes. But the differences between honors and non-honors classes may not actually be driven by a desire to avoid peer sanctions or signaling; instead, students with certain attributes (correlated with being in honors or non-honors classes) may just respond differently to whether their effort is observable.

In order to isolate the role of peer pressure, we restrict our analysis to students taking exactly two honors classes.[9] Honors classes are spread throughout the day, but our team showed up for just two periods. For students taking exactly two honors classes, it is essentially random whether we happened to arrive and conduct our experiment during the period when their honors class was meeting or when their non-honors class was meeting. Thus, we can in effect hold fixed the attributes of students (they are all taking two honors classes) and randomly vary the set of

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[9] For the discussion that follows, focusing on students taking any fixed number of honors classes (other than zero or all honors) would suffice. However, very few students in an honors class take less than two honors classes and very few students in non-honors classes take more than two honors classes. In order to maximize power while keeping students comparable, we focus on students taking exactly two.

peers present when their decision is made.[10] Further, because we are not actually changing a student's peers at all[11] (they are still taking the same set of honors and non-honors classes; nor do we change their teachers, schools, neighborhoods or anything else about their environment), we can rule out most other channels through which peers may influence each other.[12] We will capture the pure effect of varying to which of a student's peers the sign up decision could be revealed, and thus whether and how those peers reward or punish observable effort.

We find that making the decision to enroll public rather than private decreases sign up rates by 25 percentage points when these students are in one of their non-honors classes. In stark contrast, making the decision public *increases* sign up rates by 25 percentage points when they are in one of their honors classes.[13] Viewed another way, when the decision is public, the sign up rate for these students is 43 percentage points greater when they are in one of their honors classes rather than one of their non-honors classes. Thus, we again find that students are highly responsive to their setting and the local prevailing norm, and that peer social pressures are responsible for these effects. But it is also important to emphasize from these results that peer sanctions do not have only negative effects (as with students increasing sign up rates under the public regime when in honors classes). Of course, we cannot generalize these results to all students, nor can we conclude that we might see gains for students taking no honors classes if we were to put them with "better" peers (though the conclusions on improving sign up by making it private in non-honors classes still holds).[14] However, it is still valuable to document a set of students for whom the localized influence of peers can have such a dramatic effect. Further, these students represent about one-eighth of our sample. Finally, these may be the most relevant

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[10] Identifying this as the effect of peer pressure or social concerns requires that information is to an extent localized, i.e., that the choices a student taking some honors classes makes in their honors class does not get fully revealed to their non-honors peers, or vice-versa. We discuss this in more detail below.

[11] This contrasts with studies that rely on explicit peer randomization.

[12] We also note that we can rule out direct social learning through peers (such as about the value or desirability of the course), since the sign up decision is made before students know what their peers did. A recent literature has focused on explicitly disentangling and separating channels of peer influence (e.g., Bursztyn et al. 2014 and Cai et al. 2012).

[13] Consistent with it being effectively random whether we caught these students during their honors or non-honors classes, we find no difference in attributes for these students across the honors and non-honors classes. There is a slightly higher sign up rate for these students when the decision is private when they are in their non-honors class compared to their honors class, but the difference is much smaller, and not statistically significant.

[14] Further, the extent to which we could get gains for non-honors students by putting them around honors students would be limited in the sense that if enough students are shifted to the "positive" setting, the prevailing norms in that setting may change.

"marginal students" if the policy objective is to improve student effort and investments; students taking only honors classes are already making high levels of efforts (and are apparently unconstrained by peer observability and the need to conform), whereas students not taking any honors classes may require deeper interventions, or altogether different policies, in order to increase their effort.

Beyond understanding student motivation and behavior, we believe the results carry important policy lessons. Peer pressure appears to be a powerful force affecting educational choices or whether students undertake important investments that could improve academic performance.[15] In our case, in non-honors classes, even very low income students are willing to forgo free access to an SAT prep course that could improve their educational and possibly later life outcomes in order to avoid having their peers know about it. Changing peers is likely to be quite difficult, particularly on a large scale. But changing the extent to which behaviors are observable is likely to be less so. This is particularly important in light of the fact that many efforts or investments students can make are observable to peers, such as raising a hand in class, seeking extra help or extra credit, or participating in classroom exercises or discussions (or, for behaviors that are not observable, they could be made so when it could lead to greater effort).[16]

These results are also relevant to our related work on whether schools should award good performance (Bursztyn and Jensen 2014). In that study, we take advantage of a natural experiment in high schools that introduced a point system and "leaderboard" into computer-based courses required of low performing students. The leaderboard revealed the top three performers to the rest of the class, potentially exposing those students to sanctions or stigma. Consistent with the present results (for non-honors classes, since the other study uses only low-performing students), while using a much larger sample and wider range of schools, we find that the leaderboard led to a large decline in performance for students that were performing near the top of the class prior to the leaderboard (i.e., those most "at risk" of being revealed to be in the top three).

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[15]Though we do not take a stand on whether responding to peer pressure or conforming to peer norms is welfare-enhancing or efficient, even when it leads to lower levels of educational effort (e.g., individuals may gain more in the long run from stronger social or peer ties than from higher educational effort).

[16] Though of course, it must be recognized that it may be paternalistic to nudge students towards greater levels of effort or investments than they find privately optimal.

The remainder of this paper proceeds as follows. In section II, we discuss the experimental design. Section III provides the empirical analysis and section IV concludes.

II. EXPERIMENTAL DESIGN

A. Experiment

We conducted our experiment in the four largest public high schools in a disadvantaged area of south Los Angeles. We visited each school once, between December 2013 and April 2014. The sample was confined to students in eleventh grade, since this is when many students begin preparing for the SAT. We focused on the largest high schools for logistical and budgetary reasons. To prevent communication among students that could contaminate the experiment (i.e., students either learning about the SAT offer before their class or learning that some students were assured privacy from their classmates while others were not), we wanted to conduct our experiment simultaneously in one period across different classrooms, or in two class periods immediately following each other, with no overlap of enrolled students. Achieving a sufficiently large sample with a limited budget therefore required visiting large schools with many classes running simultaneously each period. The four schools we study all have around 3,000 students. In addition to being larger on average, because we focused on a lower income area, these schools have a higher share of students eligible for free and reduced price meals (84% vs. 68%) and of students of Hispanic ethnicity (96% vs. 69%) compared to the average school in the Los Angeles Unified School District (LAUSD). The median income in the ZIP codes around these four schools is also lower than that around schools in the whole district ($39,533 vs. $48,898).[17] We would therefore not want to generalize our results to what would happen in other schools. However, we do note that these schools account for approximately 7 percent of all high school enrollment in the LAUSD. Further, from a policy perspective, low performing schools such as these are the ones where it is perhaps most important to understand the barriers to educational investments, performance and attainment.

Within each school, our visits were coordinated with principals and counselors to choose on what day we could visit and during which period(s) (though there is some variation, the school day typically consists of 7 periods). These considerations were typically about scheduling logistics for both the schools and our research team. During the selected periods, we visited

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honors and non-honors classrooms, across a range of subjects.[18] Overall, we visited 26 classrooms across the four schools, with a total 825 students (all of whom participated in the study).[19] Neither students nor teachers were informed about the subject of our visit or that there would be an intervention related to the SAT or SAT prep courses (principals were informed in advance, but agreed not to communicate the purpose of our visit ahead of time).

Students in the selected classrooms were offered the opportunity to sign up for free access to a commercial, online SAT preparation course. The course was created by a well-known test prep company that students are familiar with. The course includes practice exams, a library of pre-recorded videos and instructional content, live online class sessions, analysis of individual performance plus areas requiring additional focus and test taking strategy.

Prior to our study, no students in these schools were privately purchasing or using the course. Typically, the company does not offer this software to individuals, instead selling subscriptions to schools, who then make it available to individual students (the cost to the school is about $200 per student). None of the schools in which we conducted our study had purchased this software prior to our intervention. In a separate follow up survey at one of our schools (conducted immediately after the intervention), we asked students to estimate the cost of the software; on average, they estimated the value at $260. Thus, especially for these low income students, this is a valuable offer (both actual and perceived) that they would be forgoing if they chose not to sign up.[20]

After a brief introduction by members of the research team when they arrived at the selected classrooms, students were given a sheet of paper offering them the chance to sign up for the course (copies of the sign up sheets are provided in Appendix A). In particular, after asking students for their name, sex and favorite subject in school,[21] the form contained the following statement:

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[18] In practice, to facilitate the lack of overlap of students across classrooms, and to increase the comparability across schools, principals and counselors were asked to avoid math classes.

[19] We did not contact absent students.

[20] Though not all students plan to take the SAT, and it would be of little value to students not planning on taking it (unless they gave away or sold their online access to someone else).

[21] In a cross-cutting randomization on identity priming, half of the forms also asked students for their ethnicity. Since this intervention was orthogonal to our main intervention, we ignore this part of the design until Section IV.B.

"[Company Name] is offering a free online test preparation course for the SAT that is intended to improve your chances of being accepted and receiving financial aid at a college you like."

The forms then had one of the two following options (presented in bold on the forms):

"Your decision to sign up for the course will be kept completely private from everyone, except the other students in the room."

which we refer to as the "public" sign up, or:

"Your decision to sign up for the course will be kept completely private from everyone, including the other students in the room."

which we refer to as the "private" sign up.

Thus, the sole difference between the forms given to treatment and control students was a single word, "except" or "including" (in practice, we did not reveal sign up outcomes in any cases). We also note that the only difference in expected privacy is for classmates, as opposed to teachers, school administrators or parents.

Students were not given any additional information, and were told that all questions should be held until after all forms had been collected. When all students had finished completing the first form, the research team would collect the forms and then hand out a second form that contained additional questions, such as the importance that students attach to being popular in school (a copy of the second form can be found in Appendix A).[22] When students had completed the second form, the research team would collect it, and hand out written assent and consent forms to get authorization to access students' GPA information. The entire intervention itself took less than 10 minutes.

The forms with the differing privacy assurances had been pre-sorted in an alternating pattern by our research team, and were handed out to students consecutively in their seats. By randomizing at the level of the student within the classroom, we ensure that students in the public and private sign up groups were otherwise treated exactly the same in every other way. So for example there are no differences in how the experimenters or teachers treated students with

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[22] In the fourth school, we included a short set of additional questions at the end of the second form.

different privacy statements, no differences in encouragement to enroll or overall classroom environments or characteristics. We also did not allow students to communicate with each other until all forms were returned, so that there would be no contamination across groups and so that students would not realize that they were being given different terms of privacy (even if students looked at each other's desks, because the forms only differed by one word, they looked essentially identical at a glance).

Because the difference between the two forms was just a single word, the treatment was very small and very subtle. This of course makes it less likely that students would respond to the difference. We chose not to implement treatments that would make sign-up even more explicitly public, such as by asking students to raise their hands in class, come to the front of the room or put their name on a sign up sheet in the room. First, doing so would have required a much greater number of classrooms and schools, and thus significantly higher cost, in order for our tests to have reasonable statistical power, since treatments of this nature could only be implemented at the classroom level, not at the level of individual students within the classroom. Related, introducing variation at the classroom level could introduce more possible random variation in student, classroom or teacher attributes (or implementation of the treatment) across treatment groups that could separately influence sign up. A second reason is that the method of signing up (i.e., having the public treatment involve raising a hand or staying after class to sign up and the private treatment involve signing up on a individual sheet of paper) could itself affect sign up rates, independent of the pure effect of having the decision be public or private. By having all students sign up through the same exact process but varying only a single word for the two groups, we get a much cleaner difference between the two groups, making it clearer that it was the public vs. private nature of sign up that explains any difference in sign up.

As noted above, our priors were that the social acceptability of visibly undertaking effort or an investment could vary across settings, particularly with respect to academic performance or baseline levels of effort or investment.[23] Therefore, we explicitly chose both honors and non-honors classes for the experiment, yielding 560 students in non-honors classes and 265 in honors classes.

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[23] And indeed, in our initial pilot testing, we found that making the decision to enroll public rather than private led to lower sign up rates in a low performing school (in summer session for low performing students), and higher rates in a high performing school (not in summer session).

Table 1 presents tests of covariate balance. As expected given that randomization was among students within classrooms, the two groups are very well balanced on all measured dimensions, including sex, age, ethnicity, number of honors classes and grade point average (the first three are measured directly in our survey, the latter two are drawn from matching our data to administrative records provided by the schools).[24]

B. Isolating the Peer Pressure Mechanism

Any differences in the response to whether the sign up decision is public across students in honors and non-honors classes could arise for a number of reasons other than simply a concern about peers or social sanctions. For example, honors and non-honors students are likely to differ along many other social, economic and demographic attributes, or may have different aspirations or expectations, which could separately affect how they respond to the differences in treatment.

In order to isolate more cleanly the pure effect of changing the composition of peers to whom the sign up decision is potentially revealed from those where they may be stigma to signing up to those where they may not be, without changing the actual full set of peers a student has, and to in addition in effect eliminate heterogeneity and create a comparable set of students in honors and non-honors classes, we can exploit the fact that many students do not take either only honors classes or only non-honors classes. In the schools in our sample, students are allowed to choose whether they want to take each particular honors or non-honors class. By policy, they cannot be denied entry into any honors class that they want to take (nor can they be forced to take honors classes they do not want). So, many students choose to take just a few honors classes, for example choosing a subject that they are particularly interested in or a course taught by a teacher they like or heard good things about or that a friend or sibling has taken.[25]

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[24] We were able to get information on the number of honors classes taken and GPA for 94 percent of our sample. The remainder were students that had moved to different classrooms or schools by the time we entered our data with names and asked to match to school records. Matching these students one by one would require a long time commitment that school counselors were unwilling to provide. Missing information does not significantly correlate with treatment. Also, accessing administrative data on individual students' GPA requires both child assent and parental consent. We did not receive consent from 16 percent of students. Therefore, we can only provide GPA data at the group level (as in this table), and cannot analyze data linked to individual GPAs (as would be required for regressions) for a significant share of our sample. However, separately, we did ask students to self-report grades on the second survey handed out after the sign up form was collected.

[25] Taking just a few, rather than all, honors classes also allows them to manage their workload or keep up their overall grade point average.

Accordingly, we can examine students taking exactly the same fixed number of honors classes (obtained by matching our data with administrative records) who are therefore likely to be very similar, and exploit variation in the timing of those courses relative to the timing of when our research team arrived to conduct the experiment. Thus, among all students taking two honors classes and four non-honors classes, it is essentially random whether the period when we arrived and conducted our study corresponded to one of their honors classes or one of their non-honors classes. The effects of making sign up public or private in honors and non-honors classes for this group of students therefore more cleanly isolates how sign up varies when essentially at random we offer it to them when they are with other honors students or other non-honors students.

We focus in particular on students taking exactly two honors classes, to keep the restricted sample as comparable as possible across the honors and non-honors groups. If we just conditioned on students taking some but not all honors classes (i.e., 1-5 honors classes), the sample of students we find in honors classes would include more students taking a greater number of honors classes than the students we find in non-honors classes (i.e., a student taking 4 of 5 possible honors classes will be more likely to be in an honors class when we arrive than a student taking just one honors class), and these groups of students would therefore likely differ along many dimensions. By focusing on students taking exactly the same number of honors classes, such as two, we ensure that the two groups will be identical in expectation (though we will still expect different numbers of students in the two groups, since we are more likely to find them in non-honors classes (of which they have 4) than honors classes. We focus on two honors classes in particular because there are very few students taking exactly one honors class in the visited honors classes, or exactly three honors classes in the visited non-honors classes (and taking four honors classes is almost identical in many cases to taking all honors classes, since honors may not be offered in more than four subjects in a given grade in a particular school). Henceforth we refer to students taking exactly two honors classes as "some-honors" students.

Of course, it is possible that students will believe that if sign up information is public, it may get back to peers not physically in the classroom with them at that moment. Thus, a some-honors student sitting in an honors class when offered the course under the public regime may worry that their peers in their non-honors classes will know that they signed up (especially since there are likely to be other students in the class who are also taking a mix of honors and non-honors classes). However, this should dampen the effects we expect, working against our

hypothesis. Further, it may be that information does not flow as well across classes (either students simply don't talk much about these kinds of efforts, and thus it is only when it is directly observed first hand that it is relevant, or there may be a norm among such students that "what happens in honors class, stays in honors class," perhaps because of strategic considerations).[26] Unfortunately, it is not possible to measure or assess whether information flows across classes for these students (or whether they believe it does).

Overall, there are 107 students taking exactly two honors classes. Appendix Table A.1 shows that restricting to this sample of students, those we observed in honors classes are, as expected, very similar to those we observed in non-honors classes (and covariates are balanced across public and private treatments as well.

III. EMPIRICAL ANALYSIS

A. Regression Specification

We begin by regressing an indicator for whether individual i in classroom c chose to sign up for the prep course (*SignUp*) on an indicator for whether they were offered the public or private treatment (*Public*), an indicator for whether the class they are in at the time of the offer was an honors or non-honors class (*Honors*) and the interaction between *Public* and *Honors*:[27]

$$SignUp_{i,c} = \beta_0 + \beta_1 Honors_c + \beta_2 Public_i + \beta_3 Honors_c * Public_i + \varepsilon_i$$

where β_2 and β_3 are the coefficients of interest, namely the estimated effects of making the sign up decision public in non-honors classes and the differential impact of public sign up in honors relative to non-honors classes, respectively. In additional specifications, we also add other covariates (age, sex, and Hispanic dummy) as well as surveyor and classroom fixed effects; the latter further isolate the within-classroom variation in the public vs. private condition across students. These results will capture the overall effects of making sign up public rather than private in the two types of classes, which can carry implications for school policies and practices.

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[26] For example, some-honors students may want to work hard and succeed in their honors classes, and may therefore worry that if they tell their non-honors peers what another some-honors student did in an honors class, that second student could in turn tell the same peers what the first student did in the honors class.

[27] We present separate regressions for honors and non-honors classes in the Appendix.

In order to then more cleanly test the isolated peer pressure mechanism, we estimate the same regressions while limiting the sample to students taking exactly two honors classes, as discussed above.

B. Main Results

We begin by providing the raw sign up rates across public and private conditions, both in honors and non-honors classes. Figure 1 displays the findings. In non-honors classes, the private sign up rate is 72%, while the public rate is 61%. The difference between the two is significant at the 1 percent level ($p=0.0067$). In honors classes, private and public sign up rates are very high overall, and very similar: 92% of students sign up under the private treatment, while 93% sign up under the public one (the *p*-value of the difference is 0.6310). These high sign up rates suggest that students valued the course being offered, consistent with their beliefs about the cost of the course mentioned above.[28] Further, the fact that sign up is not affected by privacy in the honors class setting shows that there is no general effect of privacy itself (such as students always having a strong preference for greater privacy); though it is still possible that the value placed on privacy differs between the kinds of students who are in honors and non-honors classes (or, that demand for (or the value of) the course is so much higher in honors classes (since more students are planning to go to college, for example) that students are willing to accept the loss of privacy in exchange for the course). We will be able to separate out this possibility below.

In Table 2, we present the main results from the pooled sample of honors and non-honors students in regression format. In column 1, we present the results without controls (thus replicating the average sign-up rates from Figure 1); in column 2 we add individual covariates; finally, in column 3, we also add classroom and surveyor fixed effects.[29] The results are very similar across specifications, suggesting that randomization was successful. We again conclude that making the sign up public rather than private reduces sign up rates in non-honors classes, but has no effect in honors classes.

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[28] Since private sign up rates are already close to 100% in honors classes, it will be difficult to find a large and positive effect of public sign up, due to data censoring.

[29] The honors class dummy is dropped from the specification with classroom fixed effects. In Appendix Table A.2 we reproduce the three specifications from Table 2 separately for honors and non-honors classes.

This first set of results indicates that there is not a universally negative effect of making the sign up decision public. Nevertheless, these results are not yet sufficient to establish the existence of different social norms in honors vs. non-honors classes and that students are responding to those differences: it could instead just be that honors and non-honors students have different preferences. For instance, honors students might derive utility from signaling to their peers that they care about school, while non-honor students might derive disutility from the same action. This would not be driven by differences in social norms, but intrinsic differences in preferences across the two groups.[30]

In order to deal with this selection issue, we need to hold student characteristics fixed, while varying their peer group, or more specifically, the set of peers who observe the enrollment decision. As described above, to do so we focus on students enrolled in exactly two honors classes. Having established above (see Appendix Table A.1) that there are no differences between students enrolled in exactly two honors classes that were offered the SAT course in an honors or a non-honors class, we can provide evidence suggesting that by contrast their classmates or peers in those classes are different. This will help us formulate our priors about the directions of peer pressure effects. For that purpose, we examine the classmates for whom we were able to get information on number of total honors classes taken, and who are *not* taking exactly two honors classes. In the visited non-honors classes, the average private sign up rate among classmates is 71%, while in the visited honors classes it is 97% (the *p*-value of the difference is 0.0000). The difference in public sign up rates is also large and significant (65% in non-honors vs. 92% in honors, with *p*=0.0000). Finally, we also observe dramatic differences in peers' GPA (2.12 in non-honors vs. 3.29 in honors, with *p*=0.0000). Students taking exactly two honors classes seem to be somewhere in between their peers in honors vs. non-honors classes: their average GPA is 2.67, and their average private sign up rate is 76%.

These findings establish that the peer groups are indeed very different in honors vs. non-honors classes, and in a way that helps us formulate our hypotheses on the direction of social pressure effects for students taking exactly two honors classes. If peer pressure pushes students towards conforming to the locally prevailing norm within the classroom, we expect public sign up to be higher than private sign up in honors classes, and lower than private sign up in non-

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[30] Note that students in honors and non-honors do not significantly differ in how important they think it is to be popular in their school. We discuss concern about popularity in detail in the next subsection.

honors classes. Figure 2 presents results comparing raw sign up rates for students taking exactly two honors classes. In non-honors classes, the private sign up rate is 79%, and the public rate is 54%. This is a very large (25 percentage points) and statistically significant decrease when moving from private to public (p=0.0578).[31] In honors classes, the effects of making sign up public are reversed: private sign up is 72%, while public sign up is 97%. The difference is again dramatic in size (25 percentage points) and statistically significant (p=0.0177).

Figure 2 shows that there is no large difference in private behavior between honors and non-honors classes for the restricted sample: 72% vs. 79% (p=0.5433). This is consistent with students taking exactly two honors classes being similar regardless of whether we visited them in an honors or a non-honors class.[32] When sign up is public, on the other hand, the difference is dramatic: 54% sign up in non-honors classes, compared to 97% in honors classes (p=0.0006). When choices are public, sign up rates are a striking 43 percentage points greater when students make them in one of their honors classes rather than one of their non-honors classes.

A natural question about these findings is whether the statistical inferences are sound, given the relatively small number of observations in each experimental treatment group. As an alternative to standard t-tests to determine statistical significance, we ran permutation tests with 10,000 repetitions for the comparison of the raw sign up rates in private and in public, for the restricted sample of students taking two honors classes, separately in non-honors and honors classes.[33] While the permutation test is not an exact test, it can complement our inferences using

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[31] This effect is larger than the 11 percentage point decline in non-honors classes observed above. Though this is just a point estimate and the samples here are smaller, bigger effect could hold for this sample of students for several reasons. Students taking two honors classes are likely to differ in many ways from students taking no honors classes at all. They may be more responsive to peer pressure for example. It may also be that these students feel an even greater need to signal lower effort to their non-honors peers, as a countersignal to the fact that they are taking some honors classes. Students taking no honors classes may not feel they need to do as much to show their friends that they are part of the group or fit in. Alternatively, since students with two honors classes are choosing which classes to take as honors and non-honors, they may also be doing so strategically in a way that will have them be in more classes with their friends. Students may therefore be responding to a greater extent because a larger share of the students whose opinion they care most about are in the classroom with them (for both honors and non-honors classes).

[32] This also suggests that students believed that their choice was indeed private.

[33] To run the permutation test, we randomly assign "placebo treatment" (public decision) status to students in each group of interest, 10,000 times, and calculate a distribution of "placebo treatment effects" based on the random assignment. We then compare the size of the treatment effects we find (using the actual treatment assignment) to the distribution of "placebo treatment effects" when treatment is randomly assigned.

t-tests. In non-honors classes, the *p*-value of the two-sided permutation test for comparison of the sign-up rate across treatment conditions is 0.0633. In honor classes, the *p*-value is 0.0116. These *p*-values are very similar to those from the *t*-tests, and our inferences are unchanged.

In Table 3, we present our findings in regression format, reproducing Table 2 for the restricted sample. The point estimates are similar across specifications, although we lose statistical significance for the public effect in non-honors classes when we add both individual covariates and surveyor and classroom dummies (the *p*-value is 0.122), since our sample sizes are somewhat smaller in this restricted sample.[34]

In summary, our findings provide evidence of very strong peer pressure effects, pushing students toward less effort in honors classes and more effort in honors classes.

C. Heterogeneity and Other Results

<u>Importance of being popular</u>. Our main underlying hypothesis for why peer observability may affect choices is that students worry about what their peers will think of them. On a second form handed out to students after they had turned in the form to sign up for the SAT course, we asked students how important they thought it was to be popular in their school, on an increasing scale of 1 to 5.[35] If the effects that we observe are driven by fear of social sanctions, or seeking social approval, we would expect students who are more concerned with popularity to be more responsive to whether the sign up is public or private. To assess this hypothesis, we split our sample as close as possible to half, according to the importance attributed by students to being popular (answers 1 and 2 vs. 3, 4 or 5). Figures 3 and 4 present our results in terms of raw sign up rates, respectively for non-honors and honors classes. Figure 3 shows that for students in non-honors classes who say that it is important to be popular in their school, the sign up rate is 20 percentage points lower in the public condition than in the private condition (*p*=0.002). For those who care less about popularity in school, the effect of a public decision is small (4 percentage points) and no longer statistically significant (*p*=0.427). In Figure 4, we observe the opposite pattern for honors classes, although on a smaller scale (since the private take up rates were

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[34] In Appendix Table A.3 we reproduce the three specifications from Table 3 separately for honors and non-honors classes.

[35] The exact wording of the question was: "On a scale 1-5, how important do think it is to be popular in your school? (1: not important…5: very important).

already very close to 100%): a larger, positive effect of public sign up for those who care more about popularity. Table 4 presents the results of heterogeneity of treatment effects based on perceived importance of being popular in regression format, which confirm these results. Thus, overall, we find that students who believe it is important to be popular move in the direction of locally prevailing norms (in both honors and non-honors classes) when sign up is public rather than private, while those who do not think it is important are unaffected by whether sign up is public or private.

Ethnicity. As noted above, our intervention takes place in an ethnically homogenous setting, where 96 percent of students were Latino/Hispanic. Therefore, we have too few students to examine whether the effects vary by ethnicity. However, given the markedly poorer educational performance and attainment of minorities, and in the context of the empirical literature on "Acting White," we wanted to at least explore the possibility that race or ethnicity could be a relevant factor in behavior (though we did not have strong priors, and believe that social sanctions or pressure to conform could be present for student of all ethnicities, even if it there might be difference in which behaviors are sanctioned or to what extent they are sanctioned). Drawing on literature in social psychology, recent studies in economics such as Benjamin, Choi and Strickland (2010) have found that simple priming strategies that make ethnicity more salient, such as asking a subject about their race or language spoken at home, can affect survey responses or behaviors. Therefore, as a cross-cutting experiment, half of the sign up forms (orthogonal to the public vs. private statement) asked students for their ethnicity before asking them whether they wanted to sign up for the course. Appendix Table A.4 shows that asking students about their ethnicity did not systematically or significantly affect sign up in private or in public, in either honors or non-honors classes. We do not however want to conclude that the effects we observe are unrelated to ethnicity. It is possible for example that the priming was insufficient in our case. Alternatively, it is possible that racial priming is only relevant in mixed racial or ethnic settings, whereas in our case there was almost no such heterogeneity within classrooms or schools at all. Further, the lack of a priming effect does not imply that we would not find different effects if we conducted the experiment for students of different races or ethnicities.

Gender. Finally, we consider whether the effects vary by sex. We might think for example that boys and girls are differentially affected by concerns over peer sanctions or social stigma, as has been suggested by some literature in social psychology (e.g., Eagly and Carli 1981). Appendix Table A.5 shows that male students are less likely to privately sign up than female students are (significantly so in non-honors classes), and that the interaction of the public condition with the male indicator is always negative (although never statistically significant). These results suggest that boys might be somewhat more concerned about publicly displaying effort in school, but we look at these findings with caution, given the small sizes of the effects.

D. Other Concerns and Interpretations

Low stakes. One issue to consider is whether students simply viewed the sign up decision for this course as a low stakes decision. For example, they may not have believed that the course was very valuable, or perhaps they were already taking another course and viewed this one as largely superfluous. When the stakes are low (a redundant course with little value), students might not be willing to bear the social costs, even if the latter are also low. Although this would not challenge the interpretation of our results, it could raise external validity concerns, because perhaps any effort or investment that students believed truly could yield educational benefits or for which there were real stakes would not be affected by peer social concerns. However, we believe this is unlikely to be the case. First, we note that take up is extremely high when privacy from classmates was ensured (72 % in non-honors classes and 92% in honors classes). Second, as noted, students estimated the cost at $260, which is high particularly since most students are from lower income households (for example, as noted, 84 percent of students are eligible for free and reduced price meals). We also note that students would not otherwise be able to buy this exact course, since it is offered only to schools, not individual students, so this would be their only opportunity to access this course (though of course there are substitute courses available).

A related possibility is that students may have known that the course was valuable, but felt that the stakes of not signing up were low because they would probably be offered another chance to sign up in the future. However, we believe this is unlikely to be the case, or at least, unlikely to account for our results. First, we note that even if students believed that they would have another chance to sign up, they would have to further believe that this later opportunity

would afford them more privacy than signing up in class did. It is unclear why if students believed that signing up later would also be public, they would not sign up now, with the expectation of having an opportunity to sign up later (though it is possible that they wanted more time to think about whether it was worth it, or to discretely find out if friends or peers signed up). We also note that in the time since we concluded the study, no students communicated to us or their teachers that they were interested in taking the course (but had not signed up in class). Finally, we asked students from the last school we visited (on a second form handed out after sign up was complete) whether they believed that they would have another chance to sign up for the course when they were making their decision: 85% said no.[36]

Sign up as a signal of ability? With some investments that students may make in school, there is also the possibility that undertaking such efforts reveals low ability, such as the need for extra help or assistance. Of course, this is just one possible form of social or peer pressure, or a microfoundation for such behavior, and thus does not challenge our results. However, we believe that such effects are unlikely to underlie our results. SAT preparation, whether in the form of books or classes, is in general very common, and not often associated with representing low ability. In our survey, students reported that they believed that on average about 43% (64% in honors classes) of their classmates were taking some other course to prepare for the SAT. Further, honors students in our sample had very high take up rates (over 90%), suggesting that this is not a course only for worse students.

Alternatively, students may not want to undertake efforts if final outcomes are also observable, such as due to a "fear of failure": in other words, students who believe they have a high likelihood of failure on some observable outcome (such as getting into a good college, or any college at all), may choose not to undertake effort (or even actively signal that they are not putting in effort) so that if they fail, others will believe it was because they did not try, rather than that they tried and still failed. Again, we believe the asymmetric response to the public treatment makes this alternative less likely, since we would then need the effects to go different

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[36] Though we believe that this may even overstate the extent to which students believed they would have another chance to sign up, since the very act of asking the question may suggest or elicit that belief. Further, even with this 15%, it must again be kept in mind that this would only have an effect if students believed that the future chances to sign up would be less public/private than the current opportunity.

ways in different classes (i.e., some-honors students have a fear of failure in their non-honors classes, but the reverse of the fear of failure when in their honors classes).[37]

Consumption Externalities. An additional issue to consider is whether the changes observed here are due not to a desire to avoid social sanctions, but instead consumption externalities. For example, having more of your peers likely to take the course (such as in the honors class) may make the course more valuable because students can study together with the software or learn from each other how to best use it. Alternatively, there may just be a consumption value to working together with a friend on the course, even if it does not directly make the course more useful. And the reverse would hold in non-honors classes, where the course would be less valuable because fewer peers are likely to take it. Though we cannot completely rule out this possibility, we believe it is unlikely to drive our results for several reasons. First, as noted above, for the some-honors students, there aren't large differences in private sign up rates based on whether they are in their honors or non-honors class; the difference is 7 percentage points when we do not control for other covariates and 6 percentage points when we do, and in neither case is the difference statistically significant (columns 1 and 2 of Table 3). Further, the point estimate is actually higher when in the non-honors class (where it should be expected that fewer peers will take it up) compared to the honors class (Figure 2). Since students can of course themselves reveal to their classmates or friends if they are taking the course even if sign up is private, we would expect greater sign up rates in honors than non-honors classes if the above were true.

Also, we note that the consumption externality on its own could explain a difference in sign up rates in honors and non-honors classes, but it is less clear that it should affect differential sign up within each class based on whether sign up is public or private. However, we cannot rule out that beliefs about potential consumption externalities could differ within each class based on whether a particular student was in the public or private sign up regime. This could arise if students themselves share our hypothesis; in other words, students given the public sign up sheet when in their honors class believed more of their classmates would sign up (because it was going

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[37] However, it is possible that fear of failure would be stronger in one setting than another. For example, students may fear failure more around their non-honors peers, who might then mock them for even trying since they should have known that they would never get into college. On the other hand, fewer of their non-honors peers will be going to good colleges, or to college at all, so failing is not as stark a contrast as it might be compared to their honors peers.

to be public) than students given the private sign up sheet in their honors class. Similarly, when in their non-honors class, students given the public sign up sheet believed that fewer of their classmates would sign up than students given the private sign up sheet.[38]

Finally, in an additional set of results (available upon request), we find that the effects of public sign up do not vary with a student's self-reported popularity. Since students who are more popular presumably have more friends that they can study with, they should have (or believe they have) more to gain if there are consumption externalities, and thus we might expect them to respond more.

Relationship to the Acting White literature. Finally, we interpret our results in light of the empirical support of the Acting White hypothesis found in Fryer and Torelli (2010).[39] The students in our sample are almost all Hispanic, and we find big declines in sign up for the course under the public regime when students are in non-honors classes, which is consistent with Acting White. But we also find that students in honors classes either don't respond at all to whether sign up is public or private (and in fact, sign up rates are almost universal under both regimes in honors classes), or, among some-honors students, making sign up public actually increases sign up. However, it is certainly possible within the Acting White framework to have sufficient heterogeneity in the returns to education, discount rates, the value derived from group membership or other factors to allow for a separation of students into two pools, with one of them no longer featuring peer stigma from undertaking educational investments.

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[38]Though this will also depend on beliefs about whether the course was offered to all classes. So for example, a some-honors student in an honors class who gets the public sign up believes more of their peers will sign up; but they may also think that if the course is offered to non-honors classes, the public sign up will reduce the number of those peers that will sign up (though some-honors students may be more likely to study with friends in their honors classes than friends in their non-honors classes). So beliefs about the net difference in the number of friends that will also take the course may be ambiguous. We also note that it is somewhat unusual to conclude that our hypothesis may be incorrect (students are not responding to public vs. private in order to avoid stigma) by proposing that our hypothesis is correct (or at least, that students believe it is). Though it is possible that both peer pressure and expected consumption externality motives are operative. However, the expected consumption externality effects as described would not necessarily undermine our hypothesis, but would more likely amplify the effects and increase the estimated magnitudes.

[39] Though we note that other studies, such as Cook and Ludwig (1997), do not find evidence consistent with the hypothesis.

IV. CONCLUSION

We find that students respond dramatically to whether their decision to enroll for free in an online SAT prep course is visible to their peers, and in a way that depends greatly on who their peers are at the time they are offered the course. We also find evidence suggesting that the results are specifically driven by concerns over popularity and the possibility of facing social sanctions or gaining social approval depending on effort or investments, or at least, a desire to conform to prevailing social norms among peers in the classroom. The results have important implications for school policies, and for understanding the nature and impact of social and peer interactions in the classroom more generally.

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Figure 1: Sign-up Rates –
Private vs. Public Decisions, Non-honors vs. Honors Classes
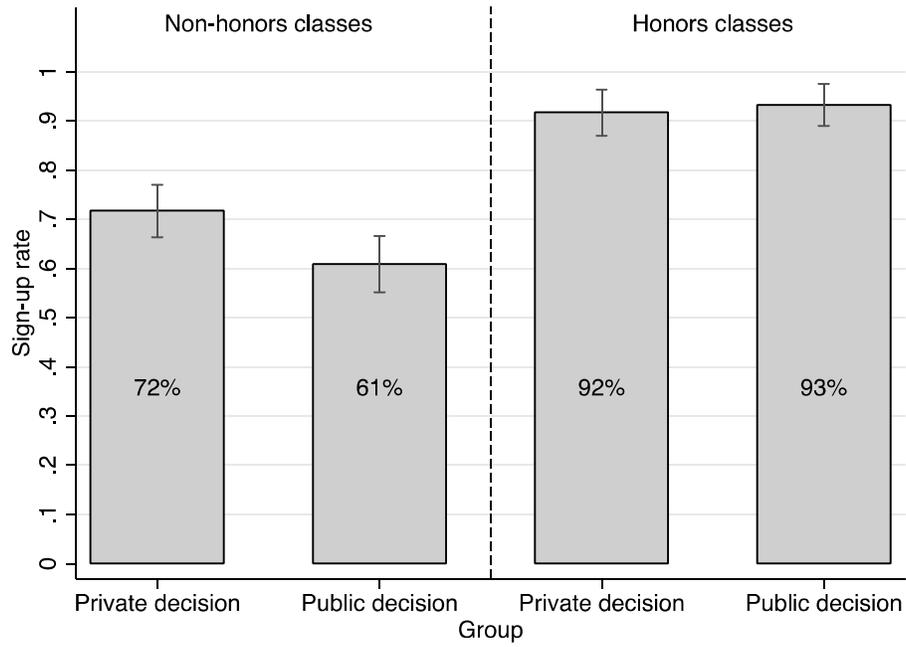
Notes: This figure presents the mean (and 95% confidence interval) of the sign-up rate for students in the private and the public conditions, separately for honors and non-honors classes.

Figure 2: Sign-up Rates – Private vs. Public Decisions, Non-honors vs. Honors Classes for Students Taking Two Honors Classes
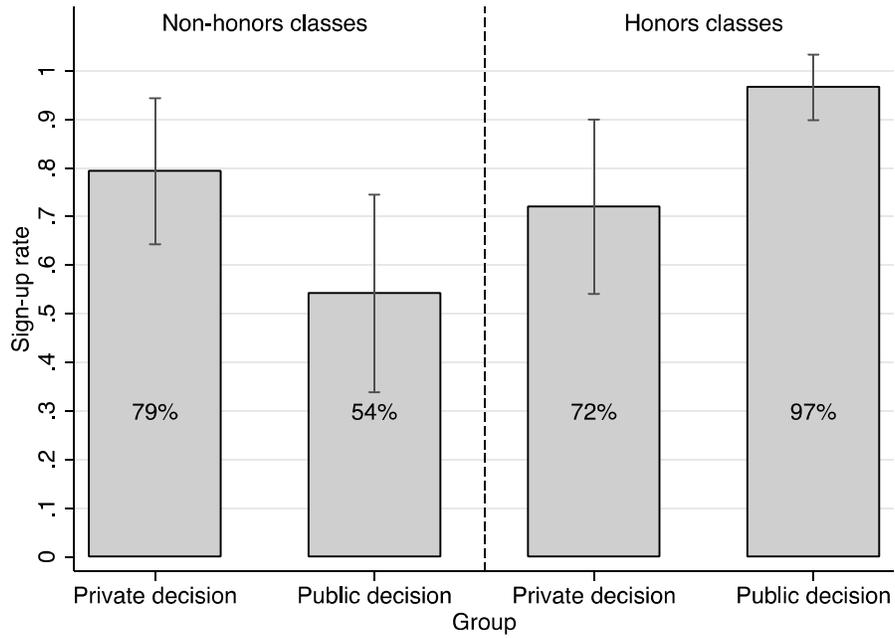
Notes: This figure presents the mean (and 95% confidence interval) of the sign-up rate for students in the private and the public conditions, separately for honors and non-honors classes, restricting the sample to students who were taking exactly two honors classes.

**Figure 3: Sign-up Rates – Private vs. Public Decisions,
Important vs. Not Important to be Popular
(Non-honors Classes)**
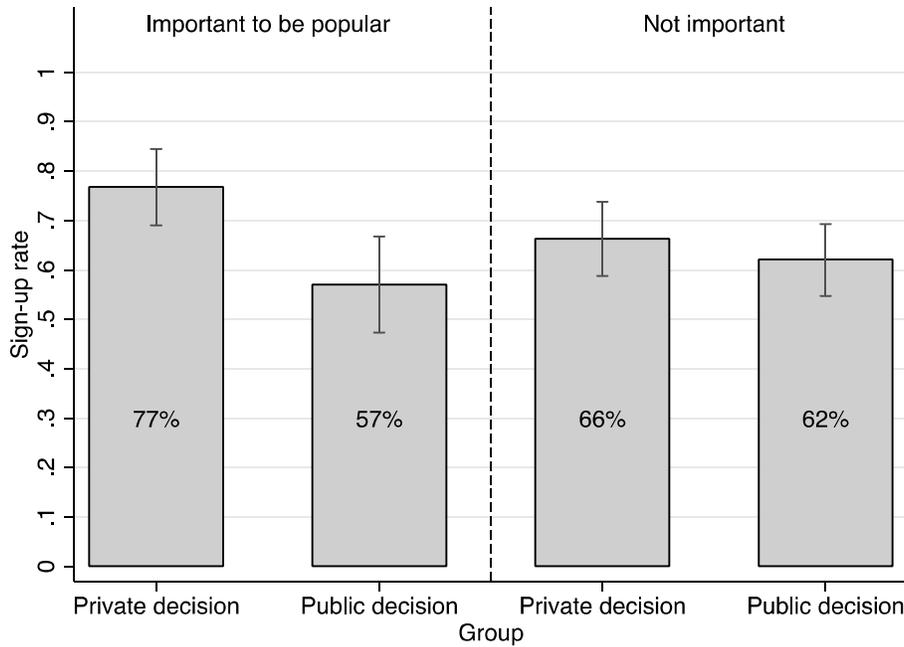
Notes: This figure presents the mean (and 95% confidence interval) of the sign-up rate for students in the private and the public conditions in non-honors classes, separately for students who consider important to be popular in their schools and for those who do not consider it important. The dummy on whether the student considers important to be popular in his/her school is constructed by collapsing the answers to the question "How important is it to be popular in your school?" from a 1-5 scale to a dummy variable (answers 3-5 were coded as considering it important).

**Figure 4: Sign-up Rates – Private vs. Public Decisions,
Important vs. Not Important to be Popular
(Honors Classes)**
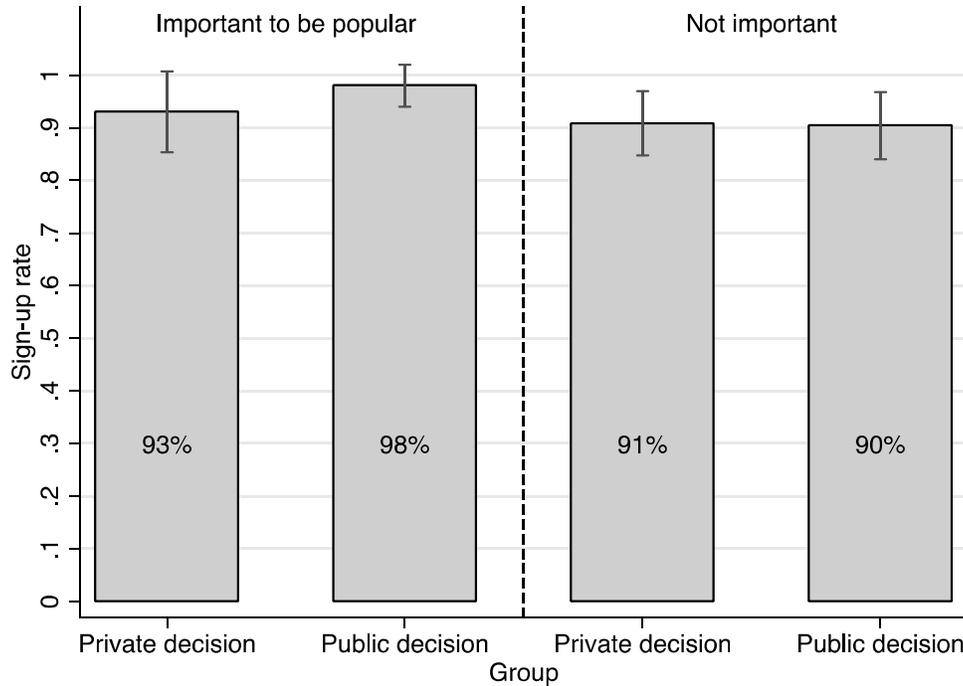
Notes: This figure presents the mean (and 95% confidence interval) of the sign-up rate for students in the private and the public conditions in honors classes, separately for students who consider important to be popular in their schools and for those who do not consider it important. The dummy on whether the student considers important to be popular in his/her school is constructed by collapsing the answers to the question "How important is it to be popular in your school?" from a 1-5 scale to a dummy variable (answers 3-5 were coded as considering it important).

Table 1: Balance of covariates

	Private condition (1)	Public condition (2)	p-value (3)
Male dummy	0.506	0.518	0.704
	[0.501]	[0.500]	
Age	16.739	16.745	0.851
	[0.535]	[0.489]	
Hispanic dummy	0.96	0.959	0.899
	[0.196]	[0.2]	
# of honors/AP classes taken	1.351	1.367	0.88
	[1.486]	[1.477]	
GPA	2.522	2.484	0.546
	[0.894]	[0.856]	

Notes: columns 1 and 2 report the mean level of each variable, with standard errors in brackets, for the private and the public conditions. For each variable, column 3 reports the p-value of a joint test that the mean levels are the same in both conditions.

Table 2: Treatment Effects - Honors and Non-Honors Classes

Dependent variable:	Dummy: The student signed up for the SAT prep course		
	(1)	(2)	(3)
Public treatment	-0.1083***	-0.1194***	-0.1229***
	[0.040]	[0.040]	[0.040]
Honors dummy	0.1998***	0.1718***	
	[0.036]	[0.037]	
Public*Honors	0.1240**	0.1334***	0.1363***
	[0.051]	[0.052]	[0.051]
Mean of private sign-up in non-honors classes		0.717	
Includes individual covariates	No	Yes	Yes
Includes classroom and surveyor FE	No	No	Yes
Observations	825	789	789
R-squared	0.090	0.117	0.180

Notes: column 1 presents OLS regressions of a dummy variable on whether the student signed up for the SAT prep course in public, an honors class dummy, and the interaction of public decision with the honors class dummy. Column 2 replicates column 1 adding individual covariates (male dummy, age, and Hispanic dummy). Column 3 replicates column 2 adding surveyor and classroom fixed effects. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Treatment Effects - Honors and Non-Honors Classes for Students Taking Two Honors Classes

Dependent variable:	Dummy: The student signed up for the SAT prep course		
	(1)	(2)	(3)
Public treatment	-0.2514*	-0.2383*	-0.2078
	[0.129]	[0.133]	[0.133]
Honors dummy	-0.0731	-0.0575	
	[0.119]	[0.118]	
Public*Honors	0.4970***	0.4607***	0.4340**
	[0.162]	[0.168]	[0.168]
Mean of private sign-up in non-honor classes		0.793	
Includes individual covariates	No	Yes	Yes
Includes classroom and surveyor FE	No	No	Yes
Observations	107	102	102
R-squared	0.127	0.219	0.426

Notes: this table restricts the sample to students taking exactly two honors classes. Column 1 presents OLS regressions of a dummy variable on whether the student signed up for the SAT prep course in public, an honors class dummy, and the interaction of public decision with the honors class dummy. Column 2 replicates column 1 adding individual covariates (male dummy, age, and Hispanic dummy). Column 3 replicates column 2 adding surveyor and classroom fixed effects. Robust standard errors in brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4: Treatment Effects - Heterogeneity - "How important is it to be popular in your school?"

Dependent variable:	Dummy: The student signed up for the SAT prep course					
	(1)	(2)	(3)	(4)	(5)	(6)
Public treatment	-0.0425	-0.0518	-0.0483	-0.0044	-0.0220	-0.0215
	[0.053]	[0.054]	[0.054]	[0.045]	[0.043]	[0.043]
Important to be popular dummy	0.1049*	0.1347**	0.1480***	0.0222	0.0113	0.0084
	[0.055]	[0.055]	[0.055]	[0.050]	[0.053]	[0.051]
Public*Important to be popular	-0.1548*	-0.1487*	-0.1672**	0.0538	0.0828	0.0820
	[0.083]	[0.083]	[0.083]	[0.063]	[0.063]	[0.066]
Mean of private sign-up for students who		0.662			0.908	
do not find it important to be popular						
Includes individual covariates	No	Yes	Yes	No	Yes	Yes
Includes classroom and surveyor FE	No	No	Yes	No	No	Yes
Observations	541	521	521	262	256	256
R-squared	0.020	0.053	0.118	0.011	0.051	0.152
SAMPLE		Non-honors classes			Honors classes	

Notes: the first three columns of this table restrict the sample to non-honors classes, while the last three focus on the sample of visited honors classes. The dummy on whether the student considers important to be popular in his/her school is constructed by collapsing the answers to the question "How important is it to be popular in your school?" from a 1-5 scale to a dummy variable (answers 3-5 were coded as considering it important). Columns 1 and 4 present OLS regressions of a dummy variable on whether the student signed up for the SAT prep course in public, a dummy on whether the student consider important to be popular in his/her school, and the interaction of public decision with the dummy on whether it is important to be popular. Columns 2 and 5 replicate columns 1 and 4 adding individual covariates (male dummy, age, and Hispanic dummy). Column 3 and 6 replicate columns 2 and 5 adding surveyor and classroom fixed effects. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

APPENDIX A: Experimental Instruments

Sign Up Sheets

A. "Public" Sign Up Sheet

Student Questionnaire

First name:_____

Last name:_____

Gender (please circle one):   Female   /   Male

What is your favorite subject in school? (Please circle one)
   a. Math   b. English Language Arts   c. History/Social Studies   d. PE/Elective

---

[Company name] is offering a free online test preparation course for the SAT that is intended to improve your chances of being accepted and receiving financial aid at a college you like.

Your decision to sign up for the course will be kept completely private from everyone, except the other students in the room.

Would you like to sign up for the free [Company name] course? (Please pick one option)

Yes / No

If yes, please provide the following contact information:

Email address: _____ _____

Phone number: (_____)_____

TURN OVER FORM AND WAIT PATIENTLY

Form A337

B. "Private" Sign Up Sheet

Student Questionnaire

First name:_____

Last name:_____

Gender (please circle one):   Female   /   Male

What is your favorite subject in school? (Please circle one)
   a. Math   b. English Language Arts   c. History/Social Studies   d. PE/Elective

---

[Company name] is offering a free online test preparation course for the SAT that is intended to improve your chances of being accepted and receiving financial aid at a college you like.

Your decision to sign up for the course will be kept completely private from everyone, including the other students in the room.

Would you like to sign up for the free [Company name] course? (Please pick one option)

Yes / No

If yes, please provide the following contact information:

Email address: _____ _____

Phone number: (_____)_____

TURN OVER FORM AND WAIT PATIENTLY

Form A338

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Second Form

Student Questionnaire (2)

First name:_____

Last name:_____

Gender (please circle one):   Female / Male

Ethnicity (please circle one):
    a. White   b. Black   c. Hispanic   d. Asian   e. Other

Do you plan to attend college after high school? (Please choose one option)
    a. Yes, four-year college
    b. Yes, two-year college/community college
    c. No
    d. Don't know

In general, how are your grades? (Please choose one option)
    a. Mostly A's
    b. Mostly A's and B's
    c. Mostly B's and C's
    d. Mostly C's and D's
    e. Mostly D's and F's

On a scale 1-5, how important do think it is to be popular in your school?
(1: not important … 5: very important)
1   2   3   4   5

On a scale 1-5, how popular would you say you are in your school?
(1: not popular … 5: very popular)
1   2   3   4   5

Hypothetically, which would you prefer? (Please circle one)
    a. 50 dollars now
    b. 75 dollars in six months

On a scale 1-5, how often do you think about your life when you are 40 years old?
(1: never … 5: very often)
1   2   3   4   5

Do you ever skip/ditch school with your friends?
    a. Sometimes
    b. Never

Do most of your closest friends plan to graduate and go to a good college?
    a. Yes
    b. No

Which of the following defines you the best?
    a. I do what my friends do
    b. I do things my own way

Second form (continued) (these questions were only asked in the fourth school)

How much do you think is the regular price of the SAT prep course that was just offered to you free of charge? _____ dollars.

When you made your choice on whether to sign up for the SAT prep course, did you expect you might have another chance to sign up in the future? (Please pick one option)
    a.   Yes
    b.   No

What % of your classmates do you think have already taken or plan to take an SAT prep course other than the one we offered today? _____%

Have you been listed as a Gifted/Talented student in your school? (Please pick one option)
    a.   Yes
    b.   No
    c.   Don't know

<div align="center">TURN OVER FORM AND WAIT PATIENTLY</div>

APPENDIX TABLES

Appendix Table A.1: Balance of Covariates for Students Taking Two Honors Classes

	Private condition	Public condition	p-value (1)=(2)	Non-honors classes	Honors classes	p-value (4)=(5)
	(1)	(2)	(3)	(4)	(5)	(6)
Male dummy	0.333	0.434	0.289	0.415	0.352	0.506
	[0.476]	[0.50]		[0.498]	[0.482]	
Age	16.648	16.703	0.519	16.731	16.617	0.177
	[0.423]	[0.44]		[0.45]	[0.406]	
Hispanic dummy	0.944	0.981	0.327	0.942	0.981	0.3
	[0.231]	[0.139]		[0.234]	[0.136]	
GPA	2.756	2.582	0.212	2.765	2.576	0.1725
	[0.687]	[0.744]		[0.55]	[0.846]	

Notes: this table restricts the sample to students taking exactly two honors classes. Columns 1 and 2 report the mean level of each variable, with standard errors in brackets, for the private and the public conditions. For each variable, column 3 reports the p-value of a joint test that the mean levels are the same in both conditions. Columns 4 and 5 report the mean level of each variable, with standard errors in brackets, for non-honors and honors classes. For each variable, column 6 reports the p-value of a joint test that the mean levels are the same in both types of classes.

Appendix Table A.2: Treatment Effects - Honors and Non-Honors Classes Separately

Dependent variable:	Dummy: The student signed up for the SAT prep course					
	(1)	(2)	(3)	(4)	(5)	(6)
Public treatment	-0.1083***	-0.1195***	-0.1231***	0.0157	0.0095	0.0092
	[0.040]	[0.040]	[0.040]	[0.033]	[0.032]	[0.031]
Mean of private take-up		0.717			0.917	
Includes individual covariates	No	Yes	Yes	No	Yes	Yes
Includes classroom and surveyor FE	No	No	Yes	No	No	Yes
Observations	560	531	531	265	258	258
R-squared	0.013	0.042	0.104	0.001	0.035	0.139
Sample:	Non-honors classes			Honors classes		

Notes: the first three columns of this table restrict the sample to non-honors classes, while the last three focus on the sample of visited honors classes. Column 1 and 4 present OLS regressions of a dummy variable on whether the student signed up for the SAT prep course in public. Column 2 and 5 replicate column 1and 4 adding individual covariates (male dummy, age, and Hispanic dummy). Column 3 and 6 replicate columns 2 and 5 adding surveyor and classroom fixed effects. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A.3: Treatment Effects - Honors and Non-Honors Classes Separately for Students Taking Two Honors Classes

Dependent variable:	Dummy: The student signed up for the SAT prep course					
	(1)	(2)	(3)	(4)	(5)	(6)
Public treatment	-0.2514*	-0.2081	-0.2135	0.2455**	0.1902*	0.1958*
	[0.129]	[0.136]	[0.138]	[0.098]	[0.103]	[0.102]
Mean of private take-up		0.793			0.72	
Includes individual covariates	No	Yes	Yes	No	Yes	Yes
Includes classroom and surveyor FE	No	No	Yes	No	No	Yes
Observations	53	51	51	54	51	51
R-squared	0.072	0.276	0.468	0.119	0.237	0.416
Sample:		Non-honors classes			Honors classes	

Notes: this table restricts the sample to students taking exactly two honors classes. The first three columns of this table restrict the sample to non-honors classes, while the last three focus on the sample of visited honors classes. Column 1 and 4 present OLS regressions of a dummy variable on whether the student signed up for the SAT prep course in public. Column 2 and 5 replicate column 1 and 4 adding individual covariates (male dummy, age, and Hispanic dummy). Column 3 and 6 replicate columns 2 and 5 adding surveyor and classroom fixed effects. Robust standard errors in brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Appendix Table A.4: Treatment Effects - Heterogeneity - Ethnicity Priming

Dependent variable:	Dummy: The student signed up for the SAT prep course					
	(1)	(2)	(3)	(4)	(5)	(6)
Public treatment	-0.1287**	-0.1359**	-0.1357**	0.0426	0.0479	0.0421
	[0.057]	[0.057]	[0.058]	[0.049]	[0.049]	[0.045]
Ethnicity priming	-0.0011	0.0267	0.0247	0.0455	0.0627	0.0627
	[0.054]	[0.055]	[0.053]	[0.048]	[0.047]	[0.043]
Public*Ethnicity priming	0.0410	0.0338	0.0261	-0.0534	-0.0756	-0.0646
	[0.080]	[0.081]	[0.080]	[0.065]	[0.066]	[0.061]
Mean of private sign-up for students without ethnicity priming		0.717			0.894	
Includes individual covariates	No	Yes	Yes	No	Yes	Yes
Includes classroom and surveyor FE	No	No	Yes	No	No	Yes
Observations	560	531	531	265	258	258
R-squared	0.014	0.044	0.106	0.005	0.042	0.145
SAMPLE	Non-honors classes			Honors classes		

Notes: the first three columns of this table restrict the sample to non-honors classes, while the last three focus on the sample of visited honors classes. Half of the questionnaires (randomly chosen) included a question asking the student's ethnicity before the decision to sign up for the SAT prep course. We code the students that had this version of the questionnaire with a dummy on "ethnicity priming." Columns 1 and 4 present OLS regressions of a dummy variable on whether the student signed up for the SAT prep course in public, the ethnicity priming dummy, and the interaction of public decision with ethnicity priming. Columns 2 and 5 replicate columns 1 and 4 adding individual covariates (male dummy, age, and Hispanic dummy). Column 3 and 6 replicate columns 2 and 5 adding surveyor and classroom fixed effects. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A.5: Treatment Effects - Heterogeneity - Gender

Dependent variable:	Dummy: The student signed up for the SAT prep course					
	(1)	(2)	(3)	(4)	(5)	(6)
Public treatment	-0.0836	-0.1053*	-0.1088*	0.0232	0.0249	0.0360
	[0.056]	[0.057]	[0.059]	[0.031]	[0.032]	[0.034]
Male student dummy	-0.0887*	-0.1119**	-0.0951*	-0.0814	-0.0555	-0.0333
	[0.054]	[0.054]	[0.054]	[0.053]	[0.051]	[0.049]
Public*Male	-0.0454	-0.0256	-0.0257	-0.0078	-0.0366	-0.0640
	[0.079]	[0.081]	[0.081]	[0.070]	[0.069]	[0.069]
Mean of private sign-up for female students		0.766			0.95	
Includes individual covariates	No	Yes	Yes	No	Yes	Yes
Includes classroom and surveyor FE	No	No	Yes	No	No	Yes
Observations	560	531	531	265	258	258
R-squared	0.027	0.042	0.104	0.026	0.036	0.142
SAMPLE		Non-honors classes			Honors classes	

Notes: the first three columns of this table restrict the sample to non-honors classes, while the last three focus on the sample of visited honors classes. Columns 1 and 4 present OLS regressions of a dummy variable on whether the student signed up for the SAT prep course in public, a male student dummy, and the interaction of public decision with male student. Columns 2 and 5 replicate columns 1 and 4 adding individual covariates (male dummy, age, and Hispanic dummy). Column 3 and 6 replicate columns 2 and 5 adding surveyor and classroom fixed effects. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1