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ABSTRACT

When effort is observable to peers, students may act to avoid social penalties by conforming to prevailing norms. To test for such behavior, we conducted an experiment in which 11th grade students were offered complimentary access to an online SAT preparatory course. Signup 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 classmates. In non-honors classes, the signup rate was 11 percentage points lower when decisions to enroll were public rather than private. Sign up in honors classes was unaffected. To further isolate the role of peer pressure we examine students taking the same number of honors classes. The timing of our visits to each school will find some of these students in one of their honors classes and others in one of their non-honors classes; which they happen to be sitting in when we arrive to conduct our experiment should be (and, empirically, is) uncorrelated with student characteristics. When offered the course in a non-honors class, these students were 25 percentage points less likely to sign up if the decision was public rather than private. 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. Thus, students are highly responsive to who their peers are and what the prevailing norm is when they make decisions.

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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.

Despite the common perception that peer pressure is widespread, there is very little direct empirical evidence of its effects.[3] Testing whether, and how, students' actions are motivated by peer pressure 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 or reward. Additionally, there needs to be exogenous variation in the extent to which that action is observable by peers, since peer pressure should only apply when an agent changes their behavior specifically because they believe it will (or might) be observed by their peers;[4] just seeing an agent undertake an action that peers may favor or sanction does not necessarily imply that the action was motivated by peer pressure. It is also helpful to have some variation in locally prevailing norms with respect to the behavior in question, in order to ensure that observability causes students to move towards the prevailing norm, as opposed to observability affecting behavior in some fixed direction for another reason.

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[1] A prominent example of such peer social effects is the "Acting White" hypothesis (Fordham and Ogbu 1986, Austen-Smith and Fryer 2005, Fryer 2011 and Fryer and Torelli 2010). But peer sanctions may also be found in many other settings and contexts.

[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 social 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 difficult to link these responses directly and causally to specific behaviors.

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

Second, testing for the effects of peer pressure requires exogenous variation in peers. This challenge is common to studies of more general forms of peer effects beyond just peer pressure (see Manski 1993, and Epple and Romano 2011 for a summary of the literature).[5] 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 between students in the different groups. It is important to in effect hold the characteristics of the individual fixed and simply vary the audience of peers present at the time they make their decision.

Third, even when peers can be exogenously varied, the ability to test specifically for the effects of peer pressure or peer social concerns, as we wish to do here, requires ruling out the many other forms of peer effects or ways in which peers may influence behavior, such as social learning or consumption externalities.

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, low-income Los Angeles high schools, we offered 11th grade students complimentary access to an online SAT preparatory course from a well-known 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 the classroom. In particular, students were either told that 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/Advanced Placement 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.[6] Such differences in norms could arise for example in the context of the models of social interactions found in Austen-Smith and Fryer (2005) and Fryer (2007). If students face a tension

[5] Several studies of peer effects more generally have used preexisting randomized peer assignments (e.g., Sacerdote 2001, Zimmerman 2003 and Carrell, Fullerton and West 2009), or explicitly randomized peers themselves (e.g., Duflo, Dupas and Kremer 2011 and Carrel, Sacerdote and West 2013).

[6] And, at least consistent with the hypothesis that the prevailing norms may differ, when the decision is private, sign up rates are much higher in honors than non-honors classes.

between investments in activities rewarded by the labor market and signaling loyalty or value to a peer group, one possible equilibrium involves sorting wherein higher ability individuals invest in the labor market oriented activities rather than those likely to increase acceptance by the group, and lower ability individuals choose the reverse. As a result of this sorting, there may then be social penalties to observable investments for students in non-honors classes, but not in honors classes.

We find that observability has a large impact on the decision to sign up for the course, and that the effects do differ dramatically based on the setting. In non-honors classes, sign up is 11 percentage points lower when students believe others in the class will know whether they signed up, compared to when 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 peer social concerns, 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 to the fact that sign up rates are already high) whereas those who say it is not important are again unaffected. Thus in both cases, students concerned with popularity move in the direction of the locally prevailing norm when the decision is public, whereas those unconcerned with popularity do not change their behavior at all based on whether they believe their peers will learn of their decision.

The differential responses to observability by class type could be consistent with explanations other than peer pressure or social concerns. For example, students in honors and non-honors classes are likely to differ from each other in many ways, and those differences may affect how much they care about privacy or how they respond when their decisions are observable. This would not change the important policy implication that observability has a large impact on decisions in non-honors classes, but the underlying mechanism could differ.

In order to test the role of peer pressure more cleanly, we can address this selection problem and make the set of students we examine in honors and non-honors classes more comparable by restricting our analysis to students taking the same number of honors classes. For every subject, students are free to choose whether to take an honors or non-honors version

(provided both are available). To fix ideas, consider the set of students who take exactly two honors classes (hereafter, "two-honors" students). Honors classes are spread throughout the day, but our team showed up for just two periods. The timing of our arrival will find some two-honors students in an honors class and others in a non-honors class. Just as important, the timing of our visit, and therefore which type of class we find them in, will be uncorrelated with student characteristics. Thus, though this approach does not explicitly randomize peers, the set of two-honors students who happen to be sitting in one of their honors classes when we arrive and conduct our experiment should be similar in expectation to those who happen to be sitting in one of their non-honors classes – the only thing that will differ is whether they are at that moment sitting with their honors or non-honors peers.[7] This strategy in effect takes otherwise similar students and just varies the set of peers present when their decision is made.[8] Further, because we are not actually changing a student's peers at all[9] (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.[10] We will capture the effect of varying just 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 a dramatic 25 percentage points when these two-honors students are in one of their non-honors classes (where the sign up rate among their "no-honors" peers is low). In stark contrast, making the decision public *increases* sign up rates by 25 percentage points when they are in one of their honors classes (where the sign up rate among their "all-honors" peers is higher). Viewed another way, when the decision is public (as many educational investments are), the sign up rate for these students is 43 percentage points greater when they are in one of their honors classes

[7] In Section II.B, we discuss this argument in more detail, addressing concerns about scheduling in particular (demonstrating that this approach does not for example also effectively split two-honors students based on which honors subjects they are taking (i.e., math vs. English), since different sections of the same honors and non-honors subjects are offered throughout the day within a given school, plus schedules vary across schools). We also show that the two sets of students do indeed look similar in terms of observable attributes, honors subjects and sign up rates when their decision is private.

[8] 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.

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

[10] We can also rule out social learning from peers (e.g., 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 disentangling and separating channels of peer influence (e.g., Bursztyn et al. 2014 and Cai et al. 2012).

rather than one of their non-honors classes. The results are similar, though slightly smaller in magnitude, if we consider "some-honors" students taking between one and three honors classes. Thus, we find that students are highly responsive to their setting and the locally prevailing norm. But it is important to emphasize that peer sanctions can have positive or negative effects; increasing sign up rates when peer sign up is high, and decreasing it when peer sign up is low. Of course, we cannot generalize the results for these some-honors students to all students (though the conclusions on improving sign up by making it private in non-honors classes still holds). 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, the set of two-honors students represent about one-eighth of our sample, while the set of students taking one to three honors classes represents about 42 percent of the sample. Finally, these some-honors students may be the most relevant "marginal students" if the policy objective is to improve student effort and investments; students taking all honors classes are already making high levels of efforts (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 and whether students undertake important investments that could improve academic performance or outcomes.[11] 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, solely in order to avoid having their peers know about it. Changing either norms or peers is likely to be quite difficult, particularly on a large scale;[12] changing the extent to which behaviors are observable by peers 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

[11]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).

[12] The difficulties in changing peers is even greater in light of the findings of Carrel, Sacerdote and West (2013), who show that even when you construct peer groups, students may endogenously sort into more homogenous subgroups. Further, the extent to which changing peer groups might help is limited by the fact that if enough students are shifted, the dominant norm may change from a positive to a negative one.

classroom exercises or discussions (or, for behaviors that are not observable, they could be made so when it could lead to greater effort).[13]

The finding that our sample of predominantly Hispanic students in non-honors classes are less likely to take the course when it is observable is also consistent with the Acting White hypothesis, whereby minorities face social sanctions from peers for engaging in certain behaviors such as schooling investments (Fordham and Ogbu 1986, Austen-Smith and Fryer 2005, Fryer 2011). It is also supportive of the empirical evidence of this hypothesis in Fryer and Torelli (2010).[14] However, given the composition of the schools we study (96% Hispanic), we cannot provide a more complete test.

More generally, our setup and results are relevant to the models of social interactions in Austen-Smith and Fryer (2005) and Fryer (2007). First, as noted, the differential response in honors and non-honors classes for the full set of students is consistent with one possible sorting outcome of these models. Second, when looking among some-honors students, who have both honors and non-honors peers, the differential response based on which set of peers their behavior will be revealed to demonstrates the tension for these students between their desire to make their preferred educational investments and the costs of peer sanctions. Finally, the use of an SAT prep course is particularly relevant given that it signals very specifically, perhaps more than many other education investments, an increased likelihood that the individual will leave the local community or group (since you only take the SAT if you plan to go to college) and is thus precisely the type of behavior we would expect to be sanctioned under these models.

Beyond this, the results showing how differences in peers and locally prevailing norms regarding accepted vs. sanctioned behavior can affect investments is also relevant to the literature examining the role of schools and neighborhoods in the educational outcomes of poor and minority students (Dobbie and Fryer 2011, Fryer and Katz 2013, Jacob 2004, Kling, Liebman and Katz 2007 and Oreopoulos 2003), as well as the broader literature on peer effects in education (Epple and Romano 2011).

Finally, the present 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

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

[14] Though other studies (e.g., Cook and Ludwig 1997) do not find evidence consistent with the hypothesis.

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 our 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).

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 11th 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 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).[15] We would therefore not want to generalize our results to other schools. However, we do note that

[15] Source: California Department of Education (http://www.cde.ca.gov/ds/dd/), for academic year 2012-3.

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. Finally, we note that despite these differences, the fraction of seniors in these four schools who take the SAT is the same as for LAUSD as a whole (51%).[16]

Within each school, our visits were coordinated with principals and counselors to choose on what day we could visit and during which period(s). These considerations were typically about scheduling logistics for both the schools and our research team. During the selected periods, we visited honors and non-honors classrooms, across a range of subjects. Overall, we visited 26 classrooms across the four schools, with a total 825 students (all of whom participated in the study).[17] 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 in these schools 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 using the course. The company does not currently 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 that they would be forgoing if they chose not to sign up (confirmed by the fact that sign up rates are very high when the decision is private). If we find that observability alone is sufficient to deter sign up, it is an indication that these peer social concerns can be quite powerful.[18]

[16] Data on actual college attendance are not available.
[17] We did not contact absent students.
[18] 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).

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,[19] the form contained the following statement:

> "[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 following two options:

> "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 completed the first form, the research team collected the forms and handed 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).[20] When students had completed the

[19] 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 until Section III.E.

[20] In the fourth school, we included additional questions at the end of the second form (see Appendix A).

second form, the research team collected it and handed 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.[21] 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 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; see Appendix A).

Because the difference between the two forms was just a single word, the treatment was very small and subtle. This makes it less likely that students would respond to the difference, and we will therefore likely underestimate the effects of peer pressure. 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 an

[21] In some classrooms, students are seated alphabetically, while in others they choose where to sit. Thus, we cannot rule out that students sitting near each other have more connection to each other than students chosen at random (they may be more likely to be friends, or perhaps even related (such when seating is alphabetical and students have the same last name)). However, this should not affect our estimates of the public vs. private treatments (particularly since students were not allowed to communicate during the experiment, nor could they even tell by looking that the forms differed).

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. Finally, having a more public treatment such as through raising hands or coming to the front of the room to sign up could have allowed for the other kinds of peer effects that we want to exclude, such as social learning or coordination.

As noted above, our priors (aided and confirmed by initial pilot testing) were that the social acceptability of undertaking effort or an investment could vary across settings, particularly with respect to academic performance or baseline levels of effort or investment. 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.

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).[22]

B. Testing the Peer Pressure Mechanism

As noted in the introduction, any differences in the response to whether the sign up decision is public or private across students in honors and non-honors classes could arise for reasons other than simply differences in norms. For example, honors and non-honors students are likely to differ along many social, economic and demographic attributes, or may have different aspirations or expectations, which could separately affect how they respond to differences in whether information is private.

[22] 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 and then asked for school records; school counselors were unable to assist us in matching these students. 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 asked students to self-report grades on the second survey handed out after the sign up form was collected.

In order to reduce this heterogeneity and create a comparable set of students in honors and non-honors classes, which will allow us to estimate more cleanly the effect of changing just the composition of peers to whom the sign up decision is potentially revealed, 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 an honors or non-honors version of each subject that is offered. Per school policy, they cannot be denied entry into any honors class that they want to take (even if they have poor grades), nor can they be forced to take an honors class they do not want to take. So, many students choose to take just a few honors classes, for example choosing a subject that they are particularly interested in or a class with a teacher they like or heard good things about.[23]

Accordingly, we can examine students taking exactly the same 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, for example, among all students taking exactly two honors classes, whether the period when we arrived and conducted our study corresponded to one of their honors classes or one of their non-honors classes should be exogenous with respect to their attributes, and thus the two sets of students should be very similar in expectation. 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 sitting in the room with other honors students or other non-honors students.

We first show results focusing on students taking exactly two honors classes, to keep the restricted sample as comparable as possible across the honors and non-honors groups (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 (since they have more of them) than honors classes).[24] We focus on two honors classes in particular because there are few students taking exactly one honors class in

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

[24] If we just conditioned on students taking some but not all 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 be more likely to differ along other dimensions.

the visited honors classes, or exactly three honors classes in the visited non-honors classes (and taking four honors classes is almost identical to taking all honors classes, since honors may not be offered in more than four subjects in a given grade in a particular school). We then show results where we pool all students taking between one and three honors classes (hereafter, "some-honors" students) and control for the number of honors classes a student is taking.[25]

Of course, it is possible that students will believe that if the sign up decision 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 would work against our hypothesis of finding differences based on whether these students are offered the course when with their honors or non-honors peers, and again suggests we may under estimate the effects of peer pressure. 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 practice 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).

One potential concern to address is class scheduling. For example, suppose in the extreme case that we visited only one school and that in addition, honors classes for various subjects are offered uniquely across periods, i.e., period 1 offers honors only in English, and honors English is only offered period 1, and similarly for each honors subject and period. In this setting, if we arrived 1st period, the set of two-honors students found in an honors class will all be taking honors English and the two-honors students in a non-honors class will be taking honors only in other subjects. If two-honors students taking honors in different subjects differ from each

[25] Though students may in principle take even more honors classes in some of our sample schools, we exclude those with 5 honors classes since there are only 9 of them (and they come from one school only; the others do not offer that many honors classes). We exclude those taking 4 honors classes because in our sample, there are no such students in any of our non-honors classes.

[26] For example, some-honors students may want to work hard and succeed in their honors classes, and may then 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.

other, particularly in ways that affect how they respond to whether their decisions are public or private (independently of peer pressure), then we will not rule out selection. Though we have no strong priors that such students would respond differently, we believe that in practice this is not a relevant concern for our analysis. First, because these are large schools, there are multiple honors and non-honors sections for each subject, offered during different periods throughout the day. So visiting during one particular period will not necessarily skew the two-honors students we find in an honors class towards a particular honors subject relative to two honors students we find in a non-honors class. Further, we visited each school during two separate periods (back-to-back). Finally, we visited different schools, each of which has different class schedules (and we also visited different schools during different periods).[27]

Overall, there are 107 students taking exactly two honors classes (and for our subsequent analysis, 343 students taking one to three honors classes). Appendix Table A.1 shows that restricting to this sample of students, those we surveyed in honors classes are, as expected, similar to those we surveyed in non-honors classes in terms of attributes (and covariates are balanced across public and private treatments as well).[28] They are also well-balanced across honors subjects; of the two total honors classes they are taking, the groups differ only by 0.08, 0.02 and 0.12 in terms of the number of math/sciences, social sciences and humanities honors classes. Though none of the differences are statistically significant, to absorb any residual variation across the two groups, in separate results we also include controls for student attributes and honors subjects taken (which does not change the estimates appreciably).

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 (*Sign up*) on an indicator for whether they were offered the public or

[27] In addition, we note that among students taking two honors classes, the majority take one honors in humanities (e.g., English) and one in social science (e.g., social studies); see Appendix Table A.1. Thus, for example, very few take math or science honors. Thus, there is not a great deal of variation in honors subject taken among these two-honors students.

[28] One concern is that honors classes may be smaller than non-honors classes, and peer pressure effects, or the responsiveness to public information, may differ in smaller and larger classes. However, in our sample, the difference is very small, and not statistically significant; the average class size is 33.7 in non-honors classes and 32.6 in honors classes.

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*:[29]

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

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, and dummies for sex and Hispanic) 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.

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, and then students taking one to three honors classes, adding dummies for the number of honors classes (and in additional specifications, controls for student attributes and which honors subjects they are taking).

B. Main Results

We begin by providing the raw sign up rates across public and private conditions, in both 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 (the *p*-value of the difference is 0.007). 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 (p=0.631). These high sign up rates suggest that students indeed valued the course being offered, consistent with their beliefs about the cost of the course mentioned above.[30] Further, the fact that sign up is not affected by privacy in the honors class shows that there is no general effect of privacy itself (such as students always having a strong preference for greater privacy); though it is possible that the value placed on

[29] We present separate regressions for honors and non-honors classes in the Appendix.
[30] 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.

privacy differs between the kinds of students who are in honors and non-honors classes or that the demand for (or value of) the course is so much higher in honors classes (since more students want to go to college) that these 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 results in regression format. In column 1, we present the results without controls (replicating the sign-up rates from Figure 1); in column 2 we add individual covariates and in column 3 we further add classroom and surveyor fixed effects.[31] 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, by a statistically significant 11−12 percentage points. But there is again no effect in honors classes. We believe these results are valuable in themselves, aside from any ability to test for peer pressure as the driving mechanism, carrying important implications for school policy and practices by showing a large, negative effect of observability on investment choices in 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, they are not yet sufficient to establish the existence of different social norms in honors vs. non-honors classes and in addition that students are responding to those differences: it could instead just be that honors and non-honors students have different preferences, or differ along other dimensions. In order to deal with this selection issue, we turn our analysis to the subset of students enrolled in exactly two honors classes. Having established and shown above that there are no significant differences between two-honors students 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 very different. For that purpose, we examine the classmates for whom we were able to get information on the number of total honors classes taken, and who are not taking exactly two honors classes. In the non-honors classes, the average private sign up rate among classmates is 71%, while in the honors classes it is 97% (the *p*-value of the difference is 0.000). There are also dramatic differences in peers' GPA (2.12 in non-honors vs. 3.29 in honors, with p=0.000). Students taking

[31] 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.

exactly two honors classes fall between their peers in honors and non-honors classes: their private sign up rate is 76% and their average GPA is 2.67.

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 lower than private sign up in non-honors classes, and higher in non-honors classes. The effects, seen in Figure 2, are large and striking. 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=0.058).[32] 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=0.018). We can reject the hypothesis that the effects in the two types of classes are equal (p=0.003).

The figure also shows that there aren't large differences in private behavior between honors and non-honors classes for the restricted sample: 72% vs. 79% (p=0.543; the differences are even smaller for the larger sample of students taking one to three honors classes; see below). 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. 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). In other words, 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 (though the samples are larger below when we consider students taking one to three honors classes). As

[32] 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 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 low 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.

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 the public and private conditions, 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 *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 the 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 in column 3 (p=0.122) and in Column 4 when we then further add controls for honors subject taken (p=0.155), since our sample sizes are somewhat smaller in this restricted sample.[34] Appendix Table A.4 shows that the same conclusions hold if we consider the set of students taking one to three honors classes and add controls for the number of honors classes the student is taking. For ease of presentation, the table shows results from separate regressions for honors and non-honors classes, so the results can be read from the public dummy, rather than several different interaction terms. Overall, the results are broadly similar. In non-honors classes, the effect of the public treatment is to reduce sign up rates by a statistically significant 15−17 percentage points. In honors classes, the public treatment increases sign up rates by 7−9 percentage points, with statistical significance at the 10 percent level in three of the four specifications. In both cases, the results are smaller in absolute value than in the case of two-honors students, particularly for those in honors classes. However, we note that when we include some-honors students taking three honors classes (there are more of them than students taking just one honors class), the sign up rates under the private treatment are already very high, since these students are very similar to all-honors students (who typically

[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.

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

are taking four honors classes), so there is less scope for them to increase sign up under the public treatment. Finally, we also note from the table that the mean sign up rates when the decision is private are very similar between those surveyed in an honors class and those surveyed in a non-honors class (85% vs. 87%), again indicating that the strategy of focusing on students taking the same number of honors classes yields comparable groups of students in the two types of classes.

In summary, our findings provide evidence of very strong peer pressure effects, pushing students toward lower sign up in non-honors classes and greater sign up in honors classes.

C. Heterogeneity and Other Results

Importance of being popular. 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] Though these are of course just subjective, self-reports, they can at least provide further suggestive corroborating evidence of our hypothesis and proposed mechanism. 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 (not important) vs. 3, 4 or 5 (important)). Figures 3 and 4 present the results for the raw sign up rates. Figure 3 shows that for students in non-honors classes who say that it is important to be popular, 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, 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 already very close to 100%): a positive effect of public sign up for those who care more about popularity, and no difference for those who care less. Table 4 presents the results 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

[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)."

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, again, we believe that social sanctions or pressure to conform could be present for student of all ethnicities, even if there might be differences in which behaviors are sanctioned or the degree to which they are sanctioned). Drawing on studies in social psychology, recent studies in economics such as Benjamin, Choi and Strickland (2010) have found that simple priming strategies that make identity more salient, such as asking a subject about their sex, 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.5 shows that asking students about their ethnicity did not 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 overall effects we observe are unrelated to ethnicity. It is possible that the priming was insufficient in our case. Alternatively, it is possible that ethnic or racial priming is only relevant in mixed racial or ethnic settings, whereas in our case there was almost no heterogeneity within classrooms or schools at all. Finally, 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. Some studies in social psychology have argued that boys and girls are differentially affected by concerns over peer sanctions or social stigma (e.g., Eagly and Carli 1981). We therefore test for such differences in response to the public treatment. Appendix Table A.6 shows that male students are less likely to sign up when the decision is private 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 significantly so). 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 size of the effects and the lack of statistical significance.

D. Account Login Data

Our main objective is to test for the effects of peer pressure, for which the sign up decision is the relevant outcome. However, we also obtained data on whether students actually logged into the online system later to activate their accounts (data on intensity of usage are unfortunately not available). It is worth emphasizing that in analyzing this outcome, we lose experimental control since students in the public and private treatments are likely to have communicated and/or coordinated with each other after our team left the classroom. In doing so, they may have updated their beliefs or level of certainty about whether others would learn about their decision. Such communication also provides scope for other forms of peer effects beyond peer pressure, such as social learning or consumption externalities. So the point estimates from this analysis are not as useful for testing our hypothesis. Further, our analysis was designed to detect effects on sign up rates, and we may therefore be underpowered to detect subsequent account login rates (which are lower). However, activating the account is a useful policy outcome, indicating how much you can actually change adoption of an important investment decision just by variation in whether it is public or private. Examining this outcome can also help establish that signing up for the course was not just "cheap talk," i.e., whether students at sign up actually intended to follow through and make use of the system.

Overall, 81 percent of students who signed up for the course logged in to activate their account, which is a fairly high "follow-through" rate and again confirms that students indeed valued the course. Overall, the unconditional mean take-up (login, conditional on being offered the course) is 61 percent. This is broadly similar to the 51 percent of students in our sample schools who take the SAT.[36] Students in honors classes had a slightly higher follow-through rate (78% vs. 84%), though the difference is not statistically significant.

The results are shown in Appendix Figure A.1 (the conclusions from regressions are similar). For the full sample of students in Panel A, we find that in non-honors classes, making

[36] The rate here is slightly higher, but there may be students in 11th grade who still think they would like to go to college, but who ultimately do not (because of performance, finances or other factors).

the course public reduces the rate of logging in to use the system by 8.2 percentage points (from a base of 57 percent when sign up is private; *p*-value=0.051). In honors classes, as with the sign up decision, there is no difference in login rates between public and private treatments (77% for private, 78% for public). For two-honors students (Panel B), the results are very similar to the results for sign up, though less precisely estimated: the public treatment decreases unconditional login rates by 20 percentage points when they are in a non-honors class (*p*-value=0.158) and increases login rates by 23 percentage points when in an honors class (*p*-value=0.071).

Finally, we also note that the follow-through rates did not differ across any of the (honors/non-honors)×(public/private) groups, for either the full sample or the two-honors students. Among two-honors students, the follow-through rate was 82.6% under the private treatment when they were in a non-honors class and 83.3% when in an honors class, again indicating that these are broadly comparable groups. Follow-through rates for this group are slightly higher under the public treatment in both non-honors (84.6%) and honors (85.7%) classes, though the differences are not statistically significant. Note in particular that the increase in sign up rates under the public treatment for two-honors students sitting in honors classes (i.e., the positive peer pressure effects) are therefore unlikely to be just cheap talk (i.e., a student who wants to conform to the prevailing norm could just check the box on the form with little effort), since if anything, those students were more likely to follow-through and actually login and activate their account (though again, we do not have data on intensity of use).

E. 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 would yield future benefits would not be affected by peer social concerns. However, we believe that 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). Further, as noted above, follow-through rates for actual login and activation were very high. Second, as noted, students estimated the cost at $260, which is particularly high given that most students are from very low income households. We also note that students would not otherwise be able to buy this 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 other 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 believed they would be offered another chance to sign up in the future. However, we believe that 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 had signed up). We also note that in the time since we concluded the study, no additional students who had not signed up in class communicated to either our team (students took away consent and asset forms with our email and other contact information) or their teachers that they were interested in taking the course. 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.[37]

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 peer social concerns or pressure, or a micro foundation 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

[37] And 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 in our case if students believed that the future chances to sign up would be less public/private than the current opportunity.

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 the worst 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 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).[38]

Consumption Externalities. A final issue to consider is whether the changes observed here are due not to a desire to avoid social sanctions, but instead consumption externalities. Having more of your peers take the course (as might be expected in honors classes) may make the course more valuable because students can study together 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. 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. First, consider the set of some-honors students. If they believed that students in all classes would also be offered the prep course, then the full set of a given student's friends who will be offered and take up the course, and thus the extent of expected consumption externalities, should not differ based on whether they are sitting with their honors or non-honors peers when they are offered the course.[39] If these students

[38] Though fear of failure effects could differ across settings. For example, students may fear failure more around non-honors peers, who might mock them for even trying (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.

[39] Though it is possible that beliefs differ by class type; some-honors students in honors classes may believe the course is only being offered to honors classes, while those in non-honors classes may believe it is being offered to all students, or only those in non-honors classes.

instead believed that the course was only being offered to those in the class with them at that time, then under the private condition we should expect higher sign up rates for those sitting in an honors class than for those sitting in a non-honors class (since they should expect more of their honors class peers to take it up). However, as noted above, the private sign up rates for some-honors do not differ significantly between honors and non-honors classes. In fact, the sign up rate among two-honors students is actually slightly higher (though not statistically significantly so) in non-honors classes than in honors classes (Figure 2). Thus, though there may be consumption externalities in the use of this course, students do not appear to act as though there are when they make their private decisions.

In addition, we note that though consumption externalities on their own could explain a difference in sign up rates in honors and non-honors classes, 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 in an honors class believe more of their classmates will sign up than students given the private sign up sheet. Similarly, students given the public sign up sheet in a non-honors class believe that fewer of their classmates will sign up than students given the private sign up sheet.[40]

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.

IV. CONCLUSION

[40]Though this will again depend on beliefs about whether the course was offered to all classes. For example, a some-honors student in an honors class who gets the public sign up may believe that more of their peers will sign up; but they may also think that same public sign up condition will reduce the number of peers that will sign up in their non-honors classes (though some-honors students may be more likely to study with friends in their honors classes). So beliefs about the net difference in the number of friends that will take the course may be ambiguous. We also note though that if students behave as though our hypothesis is correct, and make their decision based on expected consumption externalities given that hypothesis, the consumption externality effects as described would not necessarily undermine our hypothesis, but instead amplify the effects and increase the estimated magnitudes.

We find that students respond dramatically to whether their decision to sign up for a complementary version of a valuable, 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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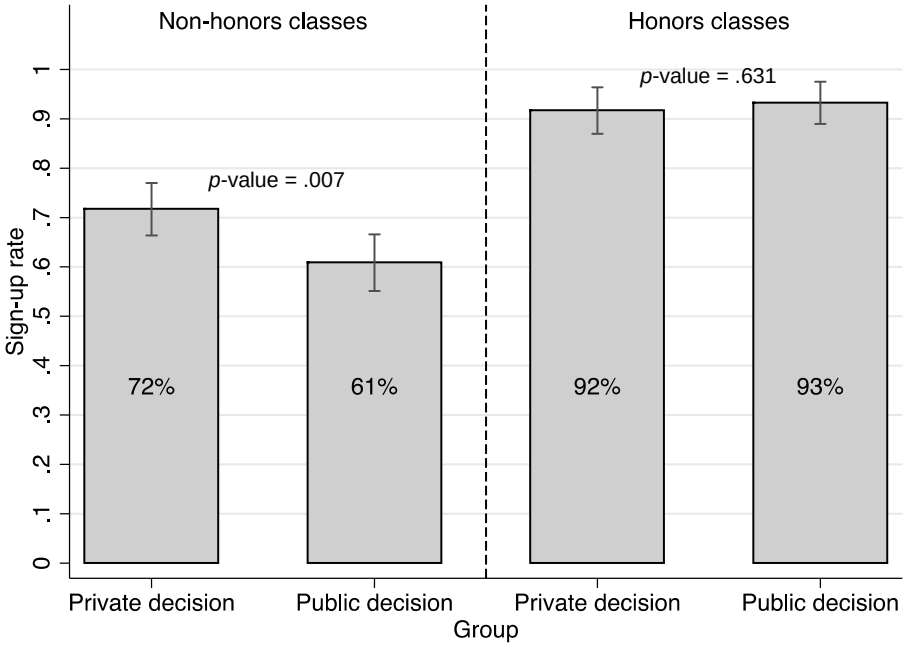
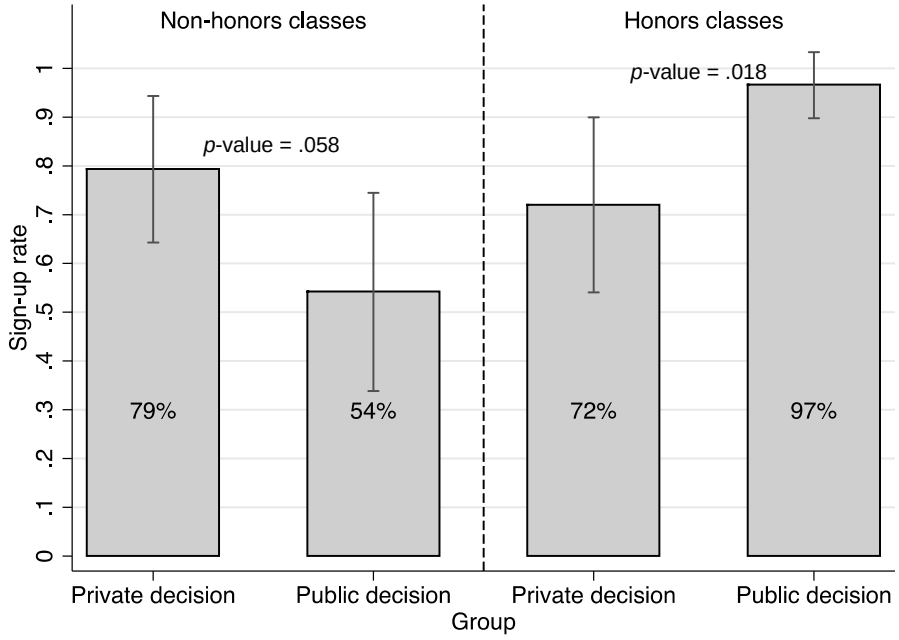
Figure 1: Sign-up Rates –
Private vs. Public Decisions, Non-Honors vs. Honors Classes



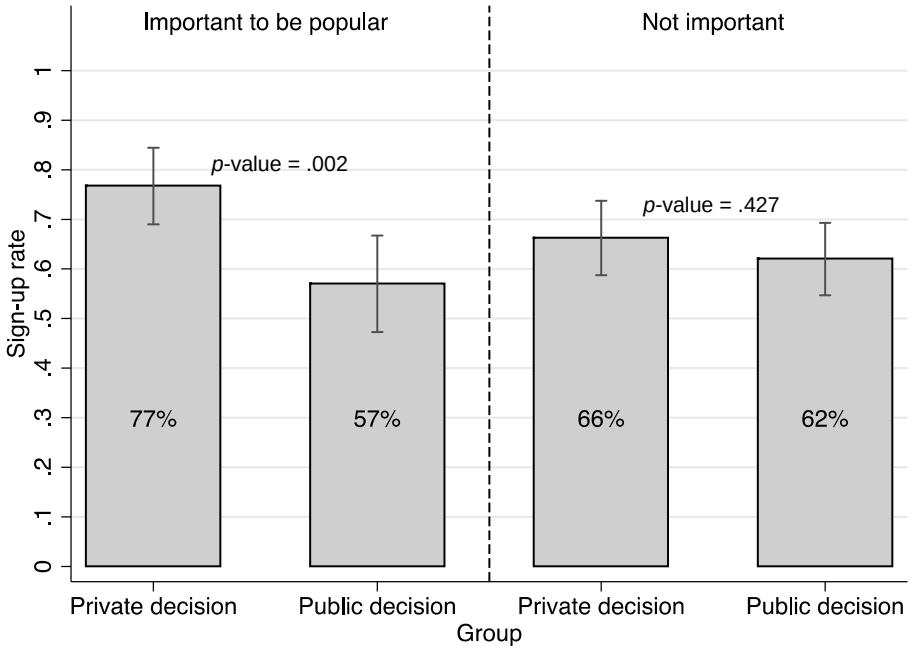
Notes: This figure presents the means and 95% confidence intervals of the sign-up rates for students in the private and public conditions, separately for honors and non-honors classes.

Figure 2: Sign-up Rates – Private vs. Public Decisions, Non-Honors vs. Honors Classes for Students Taking Two Honors Classes



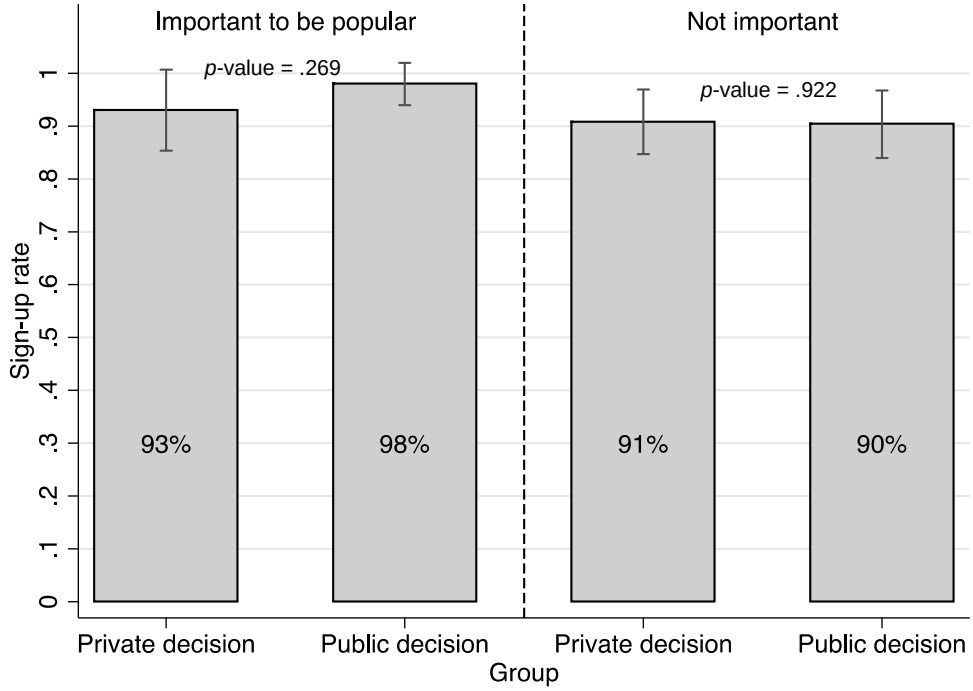
Notes: This figure presents the means and 95% confidence intervals of the sign-up rates for students in the private and public conditions, separately for honors and non-honors classes, restricting the sample to students taking exactly two honors classes.

Figure 3: Sign-up Rates – Private vs. Public Decisions, Important vs. Not Important to be Popular (Non-Honors Classes)



Notes: This figure presents the means and 95% confidence intervals of the sign-up rates for students in the private and public conditions in non-honors classes, separately for students who consider important to be popular in their school and those who do not. The dummy for whether the student considers it important to be popular 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, 1-2 as not important).

Figure 4: Sign-up Rates – Private vs. Public Decisions, Important vs. Not Important to be Popular (Honors Classes)



Notes: This figure presents the means and 95% confidence intervals of the sign-up rates for students in the private and public conditions in honors classes, separately for students who consider important to be popular in their school and those who do not. The dummy for whether the student considers it important to be popular 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, 1-2 as not important).

Table 1: Balance of covariates

	Private condition	Public condition	*p*-value
	(1)	(2)	(3)
Male dummy	0.506	0.518	0.704
	[0.501]	[0.500]	
Age	16.74	16.75	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	2.52	2.48	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 public conditions. Column 3 reports the *p*-value for the test that the means are equal in the two conditions.

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

Dependent variable:	Dummy: 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 for whether the student signed up for the SAT prep course on a public sign up dummy, an honors class dummy and the interaction of the two. Column 2 replicates column 1 adding individual covariates (age and dummies for male and Hispanic). 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: Student signed up for the SAT prep course			
	(1)	(2)	(3)	(4)
Public treatment	-0.2514*	-0.2383*	-0.2078	-0.1956
	[0.129]	[0.133]	[0.133]	[0.136]
Honors dummy	-0.0731	-0.0575		
	[0.119]	[0.118]		
Public*Honors	0.4970***	0.4607***	0.4340**	0.4131**
	[0.162]	[0.168]	[0.168]	[0.179]
Mean of private take-up in non-honor classes	0.793			
Public effect in honors classes	0.245**	0.222**	0.226**	0.217*
	[0.098]	[0.1]	[0.102]	[0.11]
Includes individual covariates	No	Yes	Yes	Yes
Includes classroom and surveyor FE	No	No	Yes	Yes
Includes honors subjects variables	No	No	No	Yes
Observations	107	102	102	102
R-squared	0.127	0.219	0.426	0.437

Notes: This table restricts the sample to students taking exactly two honors classes. Column 1 presents OLS regressions of a dummy variable for whether the student signed up for the SAT prep course on a public sign up dummy, an honors class dummy and the interaction of the two. Column 2 replicates column 1 adding individual covariates (age and dummies for male and Hispanic). Column 3 replicates column 2 adding surveyor and classroom fixed effects. Column 4 replicates column 3 adding controls for the number of honors classes taken by subject categories (math/sciences and social sciences - the omitted category is humanities). 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: 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 do not find it important to be popular		0.662			0.908	
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 honors classes. The dummy for whether the student considers it important to be popular 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, 1-2 as not important). Columns 1 and 4 present OLS regressions of a dummy variable for whether the student signed up for the SAT prep course on a public sign up dummy, a dummy on whether the student consider it important to be popular in his/her school and the interaction of the two. Columns 2 and 5 replicate columns 1 and 4 adding individual covariates (age and dummies for male and Hispanic). Columns 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

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? _____%

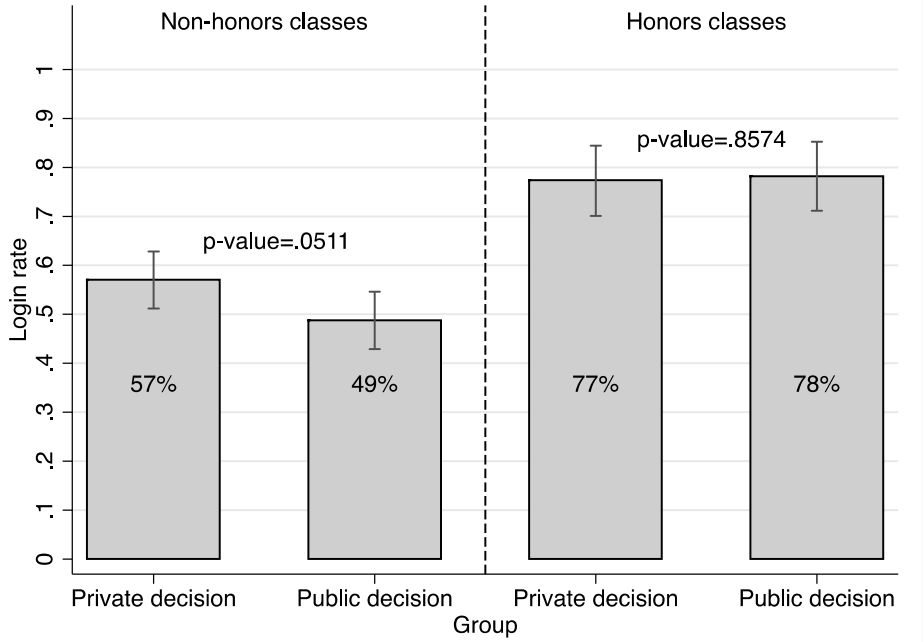
Have you been listed as a Gifted/Talented student in your school? (Please pick one option)

a. Yes
b. No
c. Don't know

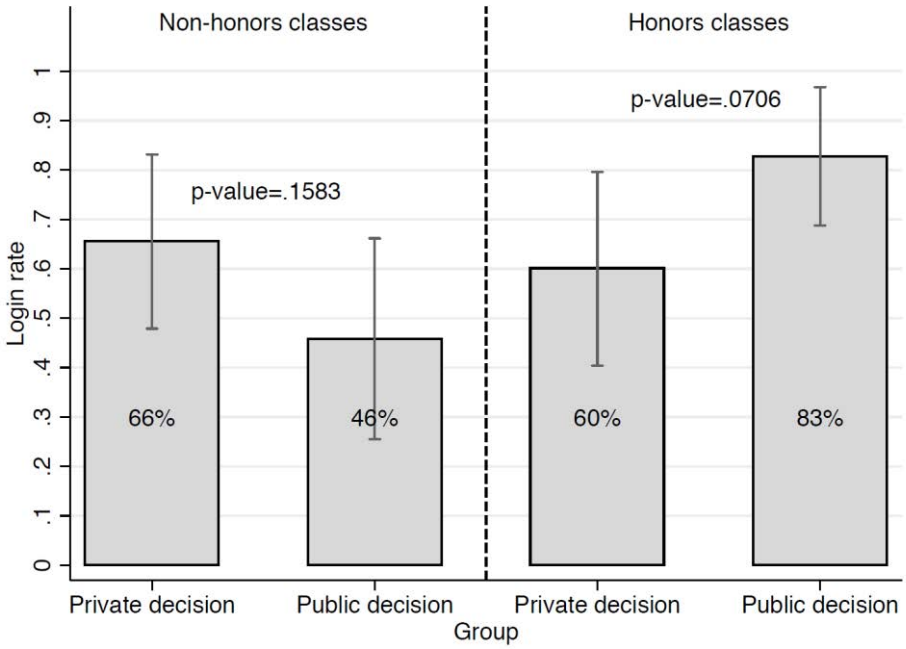
TURN OVER FORM AND WAIT PATIENTLY

Appendix Figure A.1: Login Rates – Private vs. Public Decisions, Non-honors vs. Honors Classes

A. Full Sample



B. Two-Honors Students



Notes: These figure present the means and 95% confidence intervals of the login rates for students in the private and public conditions, separately for honors and non-honors classes for the full sample (A) and the sample of two-honors students (B).

APPENDIX TABLES

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

	Private condition [1]	Public condition [2]	*p*-value [1]=[2] [3]	Non-honors classes [4]	Honors classes [5]	*p*-value [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	0.300
	[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]	
# of math/sciences honors taken	0.278	0.283	0.955	0.321	0.241	0.384
	[0.452]	[0.5]		[0.510]	[0.432]	
# of social sciences honors taken	0.926	0.906	0.828	0.906	0.926	0.827
	[0.47]	[0.491]		[0.30]	[0.61]	
# of humanities honors taken	0.815	0.774	0.665	0.736	0.852	0.224
	[0.517]	[0.466]		[0.56]	[0.408]	

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 public conditions; column 3 reports the *p*-value of a test that the means 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; column 6 reports the *p*-value of a test that the means are the same in both types of classes.

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

Dependent variable:	Dummy: 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 honors classes. Columns 1 and 4 present OLS regressions of a dummy variable on whether the student signed up for the SAT prep course on a public sign up dummy. Columns 2 and 5 replicate columns 1 and 4 adding individual covariates (age and dummies for male and Hispanic). Columns 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: 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 sign up	0.79			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 restrict the sample to non-honors classes, while the last three focus on honors classes. Columns 1 and 4 present OLS regressions of a dummy variable for whether the student signed up for the SAT prep course on a public sign up dummy. Columns 2 and 5 replicate column 1 and 4 adding individual covariates (male dummy, age, and Hispanic dummy). Columns 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 - Honors and Non-Honors Classes Separately for Students Taking 1-3 Honors Classes

Dependent variable:	Dummy: Student signed up for the SAT prep course							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Public treatment	-0.1673***	-0.1486**	-0.1465**	-0.1467**	0.0850*	0.0729	0.0834*	0.0887*
	[0.061]	[0.061]	[0.063]	[0.064]	[0.047]	[0.046]	[0.045]	[0.048]
Mean of private take-up	0.849				0.87			
Includes individual covariates	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Includes classroom and surveyor FE	No	No	Yes	Yes	No	No	Yes	Yes
Includes honors subjects variables	No	No	No	Yes	No	No	No	Yes
Observations	184	176	176	176	159	155	155	155
R-squared	0.074	0.149	0.269	0.269	0.046	0.086	0.201	0.205
Sample:	Non-honors classes				Honors classes			

Notes: This table restricts the sample to students taking one, two or three honors classes. Columns 1 to 4 restrict the sample to non-honors classes, and columns 5 to 8 restrict to honors classes. Columns 1 and 5 present OLS regressions of a dummy variable for whether the student signed up for the SAT prep course on a public sign up dummy, controlling for dummies on the number of honors classes taken by the student. Columns 2 and 6 replicate columns 1 and 4 adding individual covariates (age and dummies for male and Hispanic). Columns 3 and 7 replicate columns 2 and 5 adding surveyor and classroom fixed effects. Robust standard errors in brackets. Columns 4 and 8 replicate column 3 and 7 adding controls for the number of honors classes taken by subject categories (math/sciences and social sciences - the omitted category is humanities). *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A.5: Treatment Effects - Heterogeneity by Ethnicity Priming

Dependent variable:	Dummy: 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 honors classes. Half of the questionnaires (sorted randomly) included a question asking the student's ethnicity before asking whether they wanted to sign up for the SAT prep course. Columns 1 and 4 present OLS regressions of a dummy variable for whether the student signed up for the SAT prep course on a public sign up dummy, the ethnicity priming dummy and the interaction of the two. Columns 2 and 5 replicate columns 1 and 4 adding individual covariates (age and dummies for male and Hispanic). Columns 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.6: Treatment Effects - Heterogeneity by Gender

Dependent variable:	Dummy: 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 honors classes. Columns 1 and 4 present OLS regressions of a dummy variable for whether the student signed up for the SAT prep course on a public sign up dummy, a male dummy and the interaction of the two. Columns 2 and 5 replicate columns 1 and 4 adding individual covariates (age and male and Hispanic dummy). Columns 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.