Cool to be Smart or Smart to be Cool? Understanding Peer Pressure in Education*

Leonardo Bursztyn† **Georgy Egorov**‡ **Robert Jensen**§

July 2016

Abstract

Students may care deeply about their image or what others think of them, and these concerns may affect schooling behavior. We identify two potentially important peer cultures: one that stigmatizes effort (thus, where it is "smart to be cool") and one that rewards ability (where it is "cool to be smart"). We build a model that shows that either may lead to lower educational effort when effort and performance are potentially observable by peers. We design a field experiment to both test whether students are influenced by these concerns in general, and to separately test which of these concerns they are responding more to. We implement the experiment in two different settings: a high school in a low income, high minority share area and two high schools in higher income, lower minority share areas. In both settings, fewer students sign up for a lottery to get complimentary access to an online SAT prep package when they are unsure that their decision would stay completely private from their classmates. We further show that this behavior is consistent with a greater concern for hiding effort in the lower income, higher minority share school, and a greater concern with hiding low ability in the higher income, lower minority share schools.

Keywords: peer pressure, education, experiment, signaling, stigma.
JEL Classification: I21, I24, D83, C93.

*We would like to thank Alex Frankel, Roland Fryer, Emir Kamenica, John List, Gautam Rao, Dmitry Taubinsky, Noam Yuchtman, and seminar participants at MIT, Northwestern, UC Berkeley, the University of Chicago, the University of Zurich, and the World Bank for helpful comments and suggestions. Ahmed Ali-Bob, Natalia Baclini Cameron Burch, Diego De La Peza, Stefano Fiorin, Misha Galashin, Vasily Korovkin, Shelby McNabb, Matthew Miller, and Benjamin Smith provided excellent research assistance. Our study was approved by the UCLA Institutional Review Board and the Los Angeles Unified School District Committee on External Research Review.

†University of Chicago and NBER, bursztyn@uchicago.edu
‡Kellogg School of Management, Northwestern University and NBER, g-egorov@kellogg.northwestern.edu
§Wharton School, University of Pennsylvania and NBER, robertje@wharton.upenn.edu

1 Introduction

Most people care, to at least some degree, about their social image or what others think about them. For example, we might care whether others think we are intelligent, fun, trustworthy or important. The idea that a desire to shape one's social image or signal one's type may affect how we behave in interactions with others is at the core of the concepts of signaling in economics (Spence 1973) and impression management or self-presentation in sociology and psychology (Goffman 1959).

Understanding whether, and why, behavior is influenced by concerns about social image is particularly important when considering adolescents. First, adolescence is the period in which there is the greatest focus on establishing an image or identity, and adolescents may therefore be more highly concerned with or susceptible to others' opinions or influences. Second, many decisions made during adolescence may have large and long-lasting consequences, such as those relating to schooling investments, smoking, drinking, drug use or risky sexual activity.

An important setting where peer social concerns have been examined is education. For example, Bursztyn and Jensen (2015) find that educational effort or investments can be greatly influenced by whether behavior is observable to peers. They find that students cut back on the number of high school exit exam practice questions they answer after the introduction of a leaderboard showing which students had the most correct answers. Separately, students are less likely to sign up for a complementary SAT prep course when their decision will be revealed to peers in regular (non-honors) classes, and more likely to sign up when it will be revealed to honors class peers.

Despite these apparently large and powerful peer influences, little is known about *why* students change their behavior when it is observable, which is likely to be critical for designing corrective policy strategies. In this paper, we model two potential underlying mechanisms for negative peer pressure effects and provide a field test that allows us to disentangle them.

Although there are many factors that can affect educational performance, from the perspective of a student concerned about how they appear to their peers, the two most pertinent are likely to be effort and ability. Based on our field work and previous literature, we identify two specific associated peer social cultures: one where effort is stigmatized and one where ability is rewarded.[1]

With respect to the former, students in some settings may want to hide that they are undertaking effort towards academic success. There are several potential underlying reasons why effort may be stigmatized. For example, if we think of students as having a social type (e.g., "cool" or not) and the opportunity or psychic cost of effort are greater for high social types, students may worry that

[1]It is possible that the reverse hold in some environments, i.e., effort is rewarded or ability is stigmatized. We do not address the former, since our underlying motivation derives from the potential negative consequences of peer pressure. We also do not model the possibility of ability stigmatization, though we discuss whether our results can be explained by such effects. We view it as unlikely that such a culture could arise, since high ability students could always easily portray themselves as low ability. We should also add that although common perception may be that academically successful students are in some settings stigmatized, it is unclear whether such effects are in fact driven by stigmatization of ability itself, rather than stigmatization of effort (successful students work hard or are believed to) or the signaling of low social skills, both of which are consistent with our first proposed mechanism.

exerting effort signals to peers that they are a low social type (not fun or cool) or not good at social activities. Such students may lose out on social opportunities or face other social penalties. This idea is at the heart of the "Acting White" framework in Austen-Smith and Fryer (2015). More broadly, the "Acting White" hypothesis suggests that minority students may face punishment from peers for exerting effort because it signals they are weakly attached to the group (Fordham and Ogbu 1986, Austen-Smith and Fryer 2005, Fryer 2007, Fryer and Torelli 2010). In the face of potential social costs, students may decide they would rather reduce effort when it is observable to peers, concluding instead that it smarter to try to appear cool.

Regarding ability, in many settings intelligence is seen as a desirable attribute, while being viewed as unintelligent is undesirable. For example, ability may signal one's "economic type," such as their future earnings potential or value to a network of peers, particularly in settings where educational and economic opportunities are greater. In such settings, it may be "cool to be smart," so low ability students may engage in behavior designed to hide their true ability. For example, behaviors such as raising one's hand to answer a question in class or entering an academic competition can subsequently reveal the student's ability to their peers. Students may then prefer not to undertake such efforts, and instead portray themselves as high social types, more interested in non-academic pursuits, rather than economically low ability types.

We present a model that incorporates both of these concerns and generates predictions about how they will influence educational investment behavior, as well as how the two mechanisms can be teased apart (or at least, how we can infer which of the two is dominant if both are present). In doing so, we build on a much simplified version of Austen-Smith and Fryer (2005), where students have a two-dimensional type (social and economic) and want to signal their type to their peers. We show that the motive to signal either of the two components (social or economic) is sufficient to result in negative peer pressure, and thus both stories are potentially consistent with the empirically observed phenomenon. (In Austen-Smith and Fryer, 2005, students signal their social type to their peers, but their economic type to firms that would eventually hire them rather than peers, thus, only one mechanism of peer pressure is present in their model.) We further show that augmenting the model with a particularly designed lottery leads to different predictions if concerns for signaling social skills or concerns for signaling economic skills prevail.

We test the model using a field experiment in Los Angeles public high schools. In particular, we offer students access to a specially designed SAT prep package that includes free access to an online app, a diagnostic test, plus free one-on-one tutoring. The core of our test builds on Bursztyn and Jensen (2015) in varying at the student level whether students believe the decision to sign up (and here, the diagnostic test score) will potentially be revealed to classmates. If students behave differently when they believe their decision will be revealed to peers, it indicates that peer pressure is present.

To distinguish between the two proposed mechanisms, we add a lottery and vary the likelihood

that students who sign up will win the free SAT package. Assume that a student who signs up for the lottery will, with probability p, win the package and get the benefit associated with it. When the decision is public, others will also learn that the student signed up. And if they win, their diagnostic test will also be public, which will reveal their ability to others. If effort is stigmatized, signup rates should increase in p when the decision is public (the model predicts that p will have no effect on signup when decisions are private, since there are no costs associated with signing up or winning when everything is private). In effect, if students face (and fear) a strong social cost to just signing up, then they will be more likely to sign up and incur this cost when they have a greater chance of winning the lottery and receiving the benefit of the package. By contrast, if fear of revealing ability is present, then signup rates should decrease in p when the signup decision is public. The intuition is that students with a low ability can sign up for the course, which allows them to pool with the high ability types, with very little risk of being revealed to be a low ability type. Thus when the decision to sign up is public, the differential response to p, whether signup increases or decreases in p, allows us to distinguish whether either of the two motives is present (or, which of the two dominates, since both may apply).

We implement this experiment in three Los Angeles high schools. Based on field work, we chose one school where we predict effort stigmatization is likely to be more important (a lower income, higher minority share school), and two where signaling high ability is more likely to be important (higher income, lower minority share schools), pre-registering these choices. Subsequent surveys in these schools reveal that indeed, students report greater concerns with whether people think they are smart in the latter schools, compared to the former.[2]

Overall, we find that signup rates are lower in all schools when the decision (and potentially the diagnostic test score) is public, consistent with Bursztyn and Jensen (2015). This holds across all three schools (holding p constant). In the lower income school, when decisions are public, signup rates are indeed higher when p is greater, consistent with a greater concern over revealing effort (signup rates are unaffected by p when the decision is private). By contrast, signup rates are lower when p is greater in higher income schools when the decision is public, consistent with a greater concern over revealing ability (again, private signup rates are unaffected by p).[3] Further,

[2]It is beyond the scope of the present paper to model or test the origin or evolution of culture and why it may differ across settings. However, we can offer some suggestive intuition. In the Acting White literature, when students have more limited mobility and fewer labor market opportunities with higher human capital requirements, it might be more important to signal social type, since one is likely to keep the same group of friends after high school and derive value from maintaining membership in a network with them. Moreover, group loyalty might be particularly important among groups formed by ethnic minorities (see, Berman 2000, Gans 1962, Lee and Warren 1991, and Ausubel 1977). By contrast, in settings where students are more likely to go to college or have higher mobility, concerns about maintaining membership in a network of high school friends may be less significant. Alternatively, signaling a higher economic type might in fact be more valuable when many of your peers may hope for or go on to higher paying jobs with high human capital requirements.

[3]The effects in the higher income schools are greatest among students with lower grades, who most want to hide their ability, consistent with our model (though grades are not a perfect measure of ability and are instead an outcome).

even cutting across schools, if we examine classrooms where students report a greater concern over whether others think they are smart, we see similar patterns. Similarly, we see the biggest effects among students who say it is important to be popular (these are the students who will have the highest concern about how others perceive them) and, in the higher income schools, when students report that it is important to be smart in their school in order to be popular. Thus, we find strong support for the model, and evidence of both types of concerns (as well as adverse consequences of making investment or performance public) exactly where they would be predicted.

Although our primary goal is to uncover and disentangle mechanisms behind peer pressure, we also find that students in the public treatment in both types of schools, having been less likely to sign up for the SAT prep course, are significantly less likely to have taken the SAT as of our last follow up survey.[4] These results suggest that peer pressure concerns may be strong, since students were willing to give up a lot in order to not reveal effort or ability (the SAT package was valued at $100 dollars and the median expected score gain among all students offered the package was about 100-120 points). They also suggest the serious consequences of peer pressure effects.

Our paper aims to offer contributions to several areas of research. Since the seminal work by Spence (1973), there has been a large theoretical literature on social signaling. Recently, a few papers have provided evidence of the importance of social signaling in a variety of settings, from voting, to prosocial behavior, to conspicuous consumption (e.g., DellaVigna et al. 2014, Ariely et al. 2009, DellaVigna et al. 2012, Charles et al. 2009). We contribute to this literature by experimentally disentangling different *reasons* for social signaling.

More generally, this paper also relates to a number of recent studies that use field experiments to separate the role of different potential mechanisms behind economic phenomenon (e.g., Karlan and Zinman 2009, DellaVigna et al. 2012, and more closely related, Bursztyn et al, 2014). Unlike previous studies, however, our experiment explicitly departs from different settings where the dominant mechanism is *expected* to be different: it is precisely our goal to show that similar results can be explained by very different channels in different settings. As such, our approach highlights the importance of thinking of the heterogeneity of environments when designing mechanism experiments linked to theory – this could have important implications when considering generalizing a set of findings. For example, consider our basic finding of nearly identical effects of public signup (pooling the signup rates across levels of p) in the two types of schools. If one implemented our design in just one of the two, one could incorrectly infer that the same mechanism might be explaining the basic findings in both settings.

Our results on the potentially deleterious effects of peer pressure also contribute to the literature on the causes of educational underachievement. The two very different mechanisms suggest different

[4]However, these are only self-reports. Further, our last follow up was near the end of the academic year, and many students will take the SAT in their senior year. Students in the private treatment might therefore have just taken the SAT sooner than those in the public treatment, or perhaps will take it more times. Both may still be potentially valuable to the student.

policy implications. Further, our results indicate that peer pressure can involve more than just a stigma against effort (particularly in lower income or minority communities). Effects such as ability rewarding may be found more widely, in other settings. The results also however suggest a sobering implication in this regard. Many efforts to promote gains in disadvantaged schools attempt to de-stigmatize effort or promote and value success. However, in doing so, they may lead to a stigmatization of low ability and a reduction in effort by such students.

We view the fact that the two channels of signaling generate predictions of changes in signup rates from changes in p going in *different directions* as a methodological advantage of our experimental design. This allows us to test for the presence of either mechanism in a given setting, with the same treatments, without the need to guess which mechanism dominates in a specific environment.[5] For studies interested in understanding different cultural settings, whether school-based or otherwise, this choice-based approach offers a strategy for identifying or revealing underlying cultural factors without the need for subjective appraisals or direct elicitation from respondents.

The remainder of this paper proceeds as follows. In the next section, we present the theoretical framework that incorporates the two types of peer concerns and generates predictions on how they will influence educational investments and how the two mechanisms can be distinguished from each other. Section 3 discusses the experimental design and the connection to the theory. Section 4 presents the results and considers alternative explanations. Finally, Section 5 discusses the policy implications of these results and concludes.

2 Theoretical Framework

The model below is a simplified and modified version of Austen-Smith and Fryer (2005), which was adapted for the purposes of describing the two mechanisms (as opposed to a single 'acting white' story in that paper) and for designing a test to tease them apart. Notable differences include payoffs from education. In Austen-Smith and Fryer (2005), ability is not observed, and firms pay students wages based on both education and inferred ability, which is higher for higher levels of education, because educational effort is decreasing in ability (as in the canonical model by Spence, 1974). Thus, higher effort is a signal of higher ability, and if educational effort is not stigmatized (students are not treated differentially depending on peers' inference of their social type), all students would study more. In contrast, in our paper, economic ability is being judged by the peers just like social

[5] Our approach with designing a single treatment for testing for two mechanisms is also more economical. Suppose, indeed, that we had two mechanisms M_1 and M_2 and two statistics σ_1 and σ_2, such that $\sigma_i > 0$ if and only if mechanism M_i is at work, for $i \in \{1, 2\}$. To check if one of the mechanisms is present, one would have to compute both σ_1 and σ_2, which would be expensive if obtaining the two statistics requires different treatments. In addition, this would also be wasteful, because the two tests are one-directional and would ignore information if $\sigma_i < 0$ for either i. In these terms, our tests satisfy $\sigma_2 = -\sigma_1$, which allows us to perform a two-directional test and make use of all information retrieved.

type, and educational effort is assumed to help reveal true ability[6] (to the classmates). We show that this can alone make students reduce educational effort in order to avoid revealing that they are low economic types.

In what follows, we first present a simple model of signaling social skills, then augment it to get a model of signaling economic skills. We then introduce a general model that includes a parameter p that would be used to disentangle the two cases.

2.1 Simple model of "signaling social skills"

There is a continuum of students. They students have an opportunity to participate in a certain educational activity that has benefit $b > 0$, but requires time. The opportunity cost of time is assumed to equal c_i, which is student's private information. We follow Austen-Smith and Fryer (2005) in assuming that this opportunity cost of time depends on the 'social type'. Specifically, there are two social types, low and high, $c_i = l$ for low social types and $c_i = h$ for high social types, where $l < h$; in this way, we save on notation by having c_i denote the social type, $c_i \in \{l, h\}$. We denote the share of low social types by q: $\Pr(c_i = l) = q$. In what follows, we assume that $l < b < h$, so low social types have a positive net benefit $b - l > 0$ from the educational activity, and high social types have a negative net benefit $b - h < 0$ from this activity. To save on notation, we normalize $l = 0$, so the net benefit of low social types equals b.

Students care about their peers' perception of their social type, and get utility $\lambda_s \mathbf{Pr}_{-i}(c_i = h \mid Info)$, where the latter factor reflects the probability that the peers put on student i being high social type conditional on $Info$, which denotes the history of student's actions available to the peers. If we let $s_i \in \{0, 1\}$ be the student's decision to sign up for the educational activity ($s_i = 1$ if the student signs up and $s_i = 0$ otherwise), then a student i solves

$$\max_{s_i \in \{0,1\}} (b - c_i) s_i + \lambda_s \mathbf{Pr}_{-i}(c_i = h \mid Info). \tag{1}$$

In what follows, we distinguish between two settings: private and public. In the private setting, a student's decision is not observed by the peers, so $Info = \{\varnothing\}$ (empty history) regarless of the student's choice. In the public setting, the decision is observed by the peers, and thus $Info = s_i$.

The resulting model is easy to analyze. In the private setting, the second term in (1) is a constant not affected by s_i, and the student maximizes $(b - c_i) s_i$. The student therefore chooses $s_i = 1$ if and only if $b - c_i > 0$, i.e., only if $c_i = l$. Consequently, the share of students who sign up is q, and these are all low social types, whereas high social types do not sign up.

In the public setting, high social types (students with $c_i = h$) do not sign up either (the proof of the proposition below fills in the details). Suppose that share r of students with $c_i = l$ sign up.

[6] Thus, our model of education is not a pure 'signaling' model. For this reason, we do not have to deal with multiple equilibria and refinements, which is common in signaling models.

If so, the payoff of an individual student from signing up is $b-c_i$ (in this case, their peers positively know the student is a low social type); the payoff from not signing up equals, by Bayes' formula, $\lambda_s \frac{1-q}{q(1-r)+1-q} = \lambda_s \frac{1-q}{1-qr}$. Solving for r, we obtain the following proposition.

Proposition 1. ***(Signaling social skills)*** *In the private setting, only students with positive net benefit (low opportunity cost $c_i = l$) sign up, so the share of students who sign up equals q. In the public setting, the share of students who sign up equals q if $\lambda_s \leq b$; equals $1 - \frac{\lambda_s}{b}(1-q) \in (0,q)$ if $\lambda_s \in \left(\frac{b}{1-q}, b\right)$, and equals zero if $\lambda_s \geq \frac{b}{1-q}$.*

In other words, signup in the public setting is weakly lower than that in the private setting, and strictly lower if λ_s is high enough ($\lambda_s > b$).

2.2 Simple model of "signaling economic skills"

Consider the same model, but assume that each student also has ability a_i ('economic type'). Suppose that this ability is distributed on $[0,1]$ for students with either value of c_i.[7] Suppose that students do not get stigmatized or rewarded for being high or low social type, so $\lambda_s = 0$; however, they get rewarded for their perceived ability, with coefficient λ_e. In addition, assume that in the public setting, signing up reveals not only the fact of signing up s_i, but also the student's ability a_i. The student's problem is therefore

$$\max_{s_i \in \{0,1\}} (b - c_i) s_i + \lambda_e \mathbf{E}_i \left(\mathbf{E}_{-i} \left(a \mid Info \right) \mid a_i \right); \tag{2}$$

here, $Info = \{\varnothing\}$ in the private setting and $Info = (s_i, a_i)$ in the public setting. In what follows, we assume that $h \gg 0$, specifically, that $h > b + \lambda_e$; this ensures that students with high opportunity costs do not sign up just to reveal their high ability, which would lead to positive peer effects, whereas our focus in the paper is on negative peer effects.

In this version of the model, the private setting is unchanged: a student signs up if and only if $c_i = l$. In the public setting, among students with $c_i = l$, smarter students sign up, as they are more interested in revealing their economic type. More precisely, students with a_i close to 1 always sign up. The condition that everyone signs up is that type $a_i = 0$ prefers to sign up; he does so if $\lambda_e \leq 2b$ (indeed, if this were the equilibrium, then by signing up, this student reveals his low economic type but gets the benefit b; if he does not sign up, he pools with high social types, who

[7] We follow Austen-Smith and Fryer (2005), who also adopt this assumption for simplicity. In general, there is no reason to believe that the distributions are the same or, more generally, that ability (economic skills) and social skills are uncorrelated. Furthermore, the correlation may have either sign. E.g., it is possible that students with high opportunity cost (i.e., high social type) also have low ability because they have never invested in this ability, which would imply negative correlation between ability and social type. Alternatively, it is possible that high ability students are already very well prepared for the SAT, and their opportunity cost of studying further to obtain the same benefit is high; this would imply positive correlation between ability and social type. We prefer to be agnostic about the true correlation and adopt the independence assumption for convenience. We note, however, that the results would remain unchanged for low or moderate levels of correlation, because the baseline results are not knife-edge.

on average have ability $\frac{1}{2}$). For $\lambda_e > 2b$, the equilibrium takes the form of a cutoff: students with $a_i \geq t$ sign up and students with $a_i < t$ do not. The cutoff t may be found from the following indifference condition:

$$b + \lambda_e t = h\left(\frac{1-q}{1-q+qt}\frac{1}{2} + \frac{qt}{1-q+qt}\frac{t}{2}\right).$$

Solving for t, we obtain the following proposition.

Proposition 2. ***(Signaling economic skills)*** *Suppose h is sufficiently high, specifically $h > b + \lambda_e$. In the private setting, the share of students who sign up equals q. In the public setting, the share of students who sign up equals q if $\lambda_e \leq 2b$; and it equals*

$$1 + \frac{bq}{\lambda_e} - \sqrt{1 - q + \frac{b^2 q^2}{\lambda_e^2}} < q$$

for $\lambda_e > 2b$.

In other words, the shares of students who sign up in private and public settings are identical for low λ_e, and is lower in the public setting for λ_e above a certain threshold.

2.3 Introducing a lottery to separate the two mechanisms

This subsection considers a joint model of signaling social and economic skills (in other words, we consider the case where both λ_s and λ_e may be positive). Furthermore, we now assume that a student who chose $s_i = 1$ (signed up) gets to participate in the educational activity with probability $p \in (0,1)$ (formally, there is a random variable $w_i \in \{0,1\}$ that is drawn independently of (a_i, c_i) and such that $\Pr(w_i = 1) = p$). Technically, this means that with probability p, the student gets the benefit b and pays the opportunity cost c_i (and reveals his ability a_i in the public setting); with complementary probability $1-p$, he neither gets the benefit nor pays the cost, and in the public setting only s_i is revealed, but not a_i.

The student of type (a_i, c_i) therefore solves

$$\max_{s_i \in \{0,1\}} p(b - c_i) s_i + \lambda_s \mathbf{Pr}_{-i}(c_i = h \mid Info) + \lambda_e \mathbf{E}_i(\mathbf{E}_{-i}(a \mid Info) \mid a_i). \tag{3}$$

Here, $Info = \{\varnothing\}$ in the private setting. In the public setting, $Info$ is a vector $(s_i = 0, \varnothing, \varnothing)$ if the student did not sign up, a vector $(s_i = 1, w_i = 0, \varnothing)$ if the student signed up but lost the lottery, or a vector $(s_i = 1, w_i = 1, a_i)$ if the student signed up and won the lottery, in which case his ability a_i is also revealed.

The result in the private setting is identical to the previous cases: the share of students who sign up is q. In the public setting, high social types ($c_i = h$) do not sign up, and the strategies of low social types satisfy a single-crossing condition: if a student i with ability a_i (and $c_i = l$) signs

up, then so does a student j with ability $a_j > a_i$. Thus, there is a threshold t such that students with $a_i > t$ sign up and those with $a_i < t$ do not. For a student with type $(a_i, c_i = l)$, the expected utility if he signs up equals

$$U_{s_i=1}(a_i, c_i) = pb + \lambda_e \left(pa_i + (1-p) \frac{1+t}{2} \right),$$

and the expected utility if he does not equals

$$U_{s_i=0}(a_i, c_i) = \lambda_s \frac{1-q}{1-q+qt} + \lambda_e \left(\frac{t}{2} \frac{qt}{1-q+qt} + \frac{1}{2} \frac{1-q}{1-q+qt} \right);$$

notice that the latter does not depend on the student's type. An interior threshold $t \in (0,1)$ corresponds to an equilibrium if and only if $U_{s_i=1}(a_i, c_i) = U_{s_i=1}(a_i, c_i)$ for $a_i = t$.

We thus have the following proposition.

Proposition 3. ***(Characterization of equilibrium)*** *Suppose $h > b + \lambda_e$. In the private setting, the share of students who sign up equals q. In the public setting, the share of students who sign up equals q if and only if $\lambda_s + \lambda_e \frac{p}{2} \leq pb$. If $\lambda_s \geq \frac{pb}{1-q} + \frac{\lambda_e}{2(1-q)}$, then nobody signs up, and for $\lambda_s \in \left(pb - \lambda_e \frac{p}{2}, \frac{pb}{1-q} + \frac{\lambda_e}{2(1-q)} \right)$, the share of students who sign up is given by*

$$\frac{1+p}{2p} + \frac{qb}{\lambda_e} - \sqrt{\left(\frac{1+p}{2p} + \frac{qb}{\lambda_e} \right)^2 - q \left(\frac{1}{p} + \frac{2b}{\lambda_e} + \frac{2\lambda_s (1-q)}{\lambda_e} \right)} \in (0, q).$$

Thus, the share of students in the public setting is the same as in the private setting if both λ_s and λ_e are small, and is smaller than in the private setting if either λ_s or λ_e are large. We now turn to comparative statics.

Proposition 4. ***(Comparative statics)*** *The share of students who sign up in the public setting is (weakly) decreasing in λ_s. It is also (weakly) decreasing in λ_e if λ_s is low enough,*[8] *and is increasing in λ_e otherwise. Furthermore, as p increases, more students sign up if $\lambda_s > \frac{\lambda_e - 2b}{2(1-q)}$ and fewer students sign up otherwise.*

These comparative statics results are summarized in Figure 1. Most importantly, they imply the following. If λ_e is high relative to λ_s (for example, if $\lambda_s = 0$), then an increase in p would decrease the share of students who sign up in the public setting. If λ_e is small and λ_e is not, then an increase in p would increase this share. In other words, the effect of p depends on the relative importance of signaling of one's social type and economic type to the peers.

[8] More precisely, if $\lambda_s < \frac{b}{2(1-q)} \left(\sqrt{(1-p)^2 + 4p(1-q)} - (1-p) \right)$.

3 Experimental Design and Connection with Theory

3.1 Experimental Design

We conducted our experiment in three public high schools in two areas of Los Angeles, between December 2015 and February 2016. We focused on 11th grade classrooms, since this is when students typically begin preparing for the SAT. The first school is large, with over 3,000 students, and located in a lower income area of Los Angeles. In this school, 97% of students are Hispanic/Latino, 74% are eligible for free or reduced-price meals and the median income in the school's ZIP code is about $44,000. Approximately 54% of seniors take the SAT, and the average score is around 1,200. Our sample contains 257 students from this school. The second and third schools both have over 2,000 students and are located in higher income areas of Los Angeles. Averaging across the two schools, 33% of students are Hispanic/Latino, 41% are white, 41% are eligible for free or reduced-price meals and the median income is about $66,000. Approximately 60% of seniors in these schools take the SAT, and the average score is around 1,500. We have 254 students from these two schools in our sample. Combined across the three schools we have 511 students, in 17 classrooms.

Based on our priors and field work, we chose, and pre-registered, these particular schools for testing our model because we expected effort stigmatization to dominate in the first school and ability rewarding to dominate in the other two. Though ultimately our experiment is specifically designed to test whether this is the case, we can provide some preliminary evidence that supports our priors. After our experiment was complete, we asked students to fill out a survey (see below) that included the following: "To be popular in my school it is important that people think I am smart." (1: strongly disagree ... 5: strongly agrees). In the lower income, higher minority share school, the mean response was a 2.39. By contrast, the mean was 2.90 in the higher income, lower minority share schools. The difference is significant at the 1 percent value.

As in Jensen and Bursztyn (2015), the core of our experiment involved offering students the opportunity to sign up for complimentary access to an SAT preparation package. Students were handed a form at their desks that included the following:

> "[Company Name] is offering a chance to win an SAT prep package intended to improve your chances of being accepted and receiving financial aid at a college you like. The package includes:

- Premium access to the popular [App Name] test prep app for one year;
- Diagnostic test and personalized assessment of your performance and areas of strength and weakness;
- One hour session with a professional SAT prep tutor, tailored to your diagnostic test.

This package is valued at over $100, but will be provided completely free."

Within this offer, we used a 2x2 design, cross-randomizing: (i) the probability of winning the package conditional on signing up during the experiment, and (ii) whether students believed the other students in the room would observe their signup decision or their diagnostic test score. Accordingly, the signup form continued as follows:

> "If you choose to sign up, your name will be entered into a lottery where you have a 25% [75%] chance of winning the package.
>
> Both your decision to sign up and your diagnostic test score will be kept completely private from everyone, including [except] the other students in the room.
>
> Would you like to sign up for a chance to win the SAT prep package?"

We refer to the forms containing the 25% lottery as the *Low probability* condition, and those with the 75% lottery as the *High probability* condition. Forms with the word "including" are the *Private* condition, and those with the word "except" are the *Public* condition. The forms, shown in the Supplemental Appendix, were otherwise completely identical for the various treatment groups.

Forms with the differing treatments were pre-sorted in an alternating pattern and handed out to students consecutively in their seats.[9] By varying treatment status among students within classrooms, our design ensures that students in the various groups otherwise experience the very same classroom, teacher and overall experimental environment.

Students were instructed to hold their questions and refrain from communicating with anyone until after all of the forms had been collected by our team. Thus, students could not coordinate on their decisions or observe what other students were choosing. Further, because students could not communicate with each other, and because the forms looked nearly identical at a glance, they would not have been aware that others were being given different privacy assurances or a different likelihood of winning the lottery.

After the first form was collected, we distributed a second form containing additional questions, discussed in more detail below, followed by assent and consent forms.[10]

[9] The nature of our experiment, which required handing out forms with varying treatment scripts in the classroom, precluded us from assigning treatment to each student based on a pure random draw. However, what is most important for our analysis is that the assignment procedure used should result in treatment groups that are similar in expectation, which we verify below. The fact that students may be sitting near friends (when students are free to choose where to sit), or those with the same last name and thus who may be related (when seats are assigned alphabetically) should not in itself affect our test, since students filled out the forms without communicating with each other.

[10] As originally distributed, the first form in the first school did not include a small number of questions that were added before the visits to the second and third schools. The research team therefore revisited the first school again in February 2016 and collected answers to these additional questions. We were able to reach over 86% of the students from the main intervention in that school. Treatments are still balanced for the sample that was reached in the second visit (see Appendix Table A.1).

Though we have four different conditions, the forms (shown in the Supplemental Appendix) were extremely similar, varying only in a single word, "except" or "including," and/or a single digit, 2 or 7. As with varying treatment among students within classrooms, a big advantage to this approach is that the different treatment arms are therefore treated identically in every other way, with nothing else differing that might drive different responses, other than the single word relating to privacy or the single digit relating to the likelihood of receiving the package. One disadvantage is that if students don't read carefully or pay close attention, they could potentially miss some of these critical details. However, to the extent that they do, this would weaken our test, suggesting the effects are even stronger than what we measure. Another strength of our design is that the two mechanisms generate predictions of changes in take-up as a response to varying p that go in *different directions*.

Table 1 presents tests of covariate balance. As expected, given that randomization was among students within classrooms, the four groups are very well balanced on the measured dimensions: sex, age, and ethnicity.

Students were told the value of the package was over $100 and appear to have highly valued it. Beyond the very high signup rates, as shown below, students reported on the second survey form believing the package could have a big impact on their test scores. Though the signup forms did not mention it, in school 1, the median expected point gain reported by students was 100 (with an average of 426). In schools 2 and 3, the median was 123 (with an average of 338).

3.2 Linking the Experiment to the Theoretical Framework

It is worth highlighting some distinctions between the experimental design applied here and the one used in Bursztyn and Jensen (2015). First, we include a lottery with varying probabilities of winning the package, rather than giving it to all students who sign up. Second, the SAT prep package in the current design includes a diagnostic test, which in the public condition will be revealed for students who win the package. Finally, in the public condition, there is a difference in the likelihood that it is revealed that you signed up for the course (this happens with certainty) and whether others learn your diagnostic score (which only occurs if you win the lottery). These variations are critical for testing and differentiating *why* students change their educational choices when others observe those choices, rather than just *whether* they change their choices, as in the previous paper.

The key model predictions that we can test with our experiment are:

(i) Under both mechanisms, the signup rate with the public condition is lower than with the private condition;

(ii) Under both mechanisms, p should not affect signup rates in the private condition.[11]

[11] Outside of the behavior or motives that we are trying to model and test, one could construct theories for why

(iii) In a setting where effort is stigmatized, the signup rate in public with $p = 0.75$ is *higher* than with $p = 0.25$. The intuition is that conditional on publicly signing up (and thus paying the stigma cost), the marginal student would prefer to get the package.

(iv) In a setting where ability is rewarded, the signup rate in public with $p = 0.75$ is *lower* than with $p = 0.25$. The intuition is that conditional on publicly signing up (and thus signaling that one is high ability), the marginal-type student would prefer *not* to get the package.

Thus, it is precisely the differential response to p in the public condition (and, as we will show, a lack of any effect in the private condition) that allows us to isolate and test two very different underlying mechanisms with our single experiment.

4 Results

4.1 Main Results

Figure 2 provides visual evidence of the main results on peer pressure. In both types of schools, making the signup decision public rather than private results in a striking decline in signup rates. Further, despite the large socioeconomic differences between the two types of schools, the results (both in baseline levels and treatment effects) are nearly identical, with private signup rates around 80, and a decline to 53 percent when the decision is believed to be public. The results are large and statistically significant, and consistent with the results in Bursztyn and Jensen (2015).[12]However, as noted, despite their similarity, these effects could be driven by very different underlying mechanisms in the two types of schools.

Figure 3 focuses on the lower income school. In the panel on the left, signup rates are unaffected by the likelihood of winning the lottery when the signup decision is private. As noted, the model makes a prediction that p will not affect signup when the decision is private. And although one might expect that students should be more likely to sign up when there is a greater chance of winning, since the costs of signing up are zero (just checking a box on the form), students who perceive any positive value to the prep package should sign up regardless of the likelihood of winning. When decisions are public, however, signup rates are dramatically lower when the likelihood of winning

even private signup rates could be affected by p. For example, students may dislike losing so much that they are less likely to sign up for a lottery when they have a small chance of winning, even when the cost of signing up is otherwise zero and the outcome is purely random. Finding no effect of p, as we do, rules out such possibilities (or, alternatively, indicates that different effects cancel each other out perfectly)

[12]One might note that the effects are somewhat larger than those found in Bursztyn and Jensen (2015), particularly in the lower income school, which is directly comparable to the sample of schools examined in that paper. It is important to note though that the effects we report here pool the impact of public signup for the two levels of p (0.75 and 0.25), whereas in Bursztyn and Jensen (2015) the effects are for $p = 1$. As we predict theoretically and find experimentally, a lower level of p increases the negative effect of public signup, so it is not surprising that we find larger effects in our current setting.

the lottery is 25% rather than 75%. The 18 percentage point difference is statistically significant at the 5 percent level. This result is consistent with the presence of a fear of revealing a high social type as outlined above.

Figure 4 examines the higher income schools. As before, there is no effect of p on signup rates when the signup decision is private. However, when the decision is public, the likelihood of winning the lottery has a dramatic effect on signup rates. As predicted when fear of signaling social type is the operative (or dominant) motive, students are more likely to sign up when the chances of winning are 25% rather than 75%, again consistent with students attempting to pool with the high economic types when there is less of a risk that their own economic skill will be revealed. The 26 percentage point decline is very large, and statistically significant at the 1 percent level.

We can confirm this visual evidence estimating regressions. To replicate Figure 2, we begin by regressing an indicator for whether individual i in school s chose to sign up for the prep package ($Signup$) on an indicator for whether he was offered the public or private treatment ($Public$), separately for the lower and higher income schools:

$$Signup_{i,j} = \beta_0 + \beta_1 Public_{i,j} + \epsilon_{i,j}, j \in \{lower\ income, higher\ income\}, \quad (4)$$

where β_1 is the coefficients of interest, namely the estimated effect of making the signup decision public. In additional specifications, we 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 signup public rather than private in the two types of schools and are displayed in Table 2.

Next, to replicate Figures 3 and, 4, we add to the previous equation a dummy for whether the individual faced a 0.25 (i.e., low) probability of winning the lottery to get the SAT prep package ($Low\ probability$) and the interaction of the public treatment with the dummy on facing a low probability ($Public \times Low\ probability$), also separately for the two types of schools:

$$Signup_{i,j} = \beta_0 + \beta_1 Public_{i,j} + \beta_2 Low\ probability_{i,j} + \beta_3 Public \times Low\ probability_{i,j} + \epsilon_{i,j}, \\ j \in \{lower\ income, higher\ income\}, \quad (5)$$

where β_2 measures the effect of facing a low probability of winning the package in the private treatment, and β_3 measures the differential effect of facing a low probability in the public condition. In additional specifications, we again add other covariates, as well as surveyor and classroom fixed effects. These results are presented in Table 3.

In addition to p-values from robust standard errors, all tables we also present p-values from wild bootstrap clustered standard errors and from permutation tests.

The results are very much consistent with what was revealed in the figures. Table 2 shows that making the decision public reduces signup in types of schools, with point estimates of about 0.25 – 0.27. The results are all significant at the one percent level, and are robust to including individual covariates and classroom and surveyor fixed effects. Table 3 shows that when the decision to sign up is public, the lottery with the higher likelihood of winning the SAT package increases signup in the lower income school, but decreases it in the higher income schools. And again, the results are all significant and robust to the inclusion of individual covariates or the classroom and surveyor fixed effects.

4.2 Heterogeneity

The motivating hypothesis is that students who care about what others think of them will behave in ways intended to signal either their economic or social skills. Implicit in this approach is that all students care about what others think of them. However, some may care more than others. Though we do not have a perfect measure of true underlying concerns, in our survey we asked students how important it was for them to be popular in their school. They were given the choice of answering on a 1 to 5 scale (from "Not important" to "Very important"). Figures 5 and 6 split the sample into as close to 50:50 as possible, between those who most think it is important (responses 3 to 5) and those who think it is less important (responses 1 and 2). The figures show that as predicted, those who think it is more important to be popular cut back their signup rates dramatically (34 percentage points in the lower income school and 43 percentage points in the higher income schools, both significant at the 1 percent level) when the decision is public compared to those who think it is less important. The latter group still reduces signup when the decision is public, however the differences are much smaller (half or less the size of the effect for those who think it is important) and even for this group, some still rank the importance of being popular a 2 of 5. Table 4 shows these results in regression format, and the conclusions are unchanged.

As noted above, our survey also asked students about whether being considered smart is important for being popular in their school. Though we chose one school where we expected social type to dominate student concerns and two where we expected economic type to dominate, there may be variation within schools as well. Schools may not be monolithic bodies and may instead consist of various subgroups or cliques that are more relevant. Thus, for example, even in the higher income schools, it is possible that for some subset of students, being smart is important for being popular, but for other students it is not. Figures 7 and 8 therefore split students in social skills and economic skills schools based on whether they say being seen as smart is important to being popular (responses of 3, 4 or 5 vs. 1 or 2).

Figure 7 shows that in the lower income school, the difference between signup rates in the public and private treatments seems to be nearly independent of whether the student thinks it is important to be smart in order to be popular. By contrast, Figure 8 shows that in the higher income

schools, the difference between signup rates in public and private is extremely large (41 percentage points, significant at the 1 percent level) for students who say that being smart is important for being popular. For students who say being smart is not important for being popular however, the difference between the public and private treatments still goes in the same direction, but is much smaller and not significant at conventional levels. The results are again affirmed when we estimate them in regression form (Table 5).

Finally, Figures 9 and 10 split students according to whether their classroom average response (rather than their individual response) to whether being viewed as smart is important for being popular is above or below the median for the 17 classrooms pooled across the three schools. This again allows us to both explore heterogeneity in the response as predicted by the model. It can also help validate whether the difference across the two different kinds of schools is in fact likely driven by the different peer concerns rather than other differences across schools. With that said, however, we should note that almost all of the classrooms above the median in their response to this question are from the two higher income schools and almost all those below are from the lower income school (in itself, this observations validates our choice of schools as reflecting the two different types of peer concerns, economic and social; it also suggests that when discussing different policy implications for the two types of schools, it may be easier to predict whether the students in any particular school are likely to be driven more by concern about revealing or signaling economic or social skills).

Using this approach to split our sample in those who care about revealing economic type vs. not, both Figures 9 and 10 and the regression results in Table 6 confirm the main results from Section 4.1. Students in classrooms with a lower concern about economic type are more likely to sign up in the public treatment when the likelihood of winning the SAT package is high and vice-versa for students in classrooms with a greater concern for economic type.

4.3 Additional Evidence of Mechanisms

Effort stigma. As described in the theoretical framework, and following the model in Austen-Smith and Fryer (2005), we hypothesize that a driver for a social cost associated with displaying public effort in schools is the fact that this display signals that the student has a low opportunity cost of studying, which in turn signals a low "social type." Additionally, this might also indicate that the student is less likely to be around his/her community after graduation. Though we cannot directly measure which particular story drives the stigma associated with effort, we collected additional evidence during a follow-up visit to all three schools between May and June 2016.[13] As displayed in the follow-up survey form in the Supplemental Appendix, we asked the following question: "Suppose a classmate becomes less popular because he/she is studying too hard. Why do you think this would happen?" Students were asked to pick one option among the following:

[13]We were able to reach 77% of students from the original sample.

a) Because other students don't like hard workers; b) Because other students now think he/she is not a fun person to spend time with; c) Because other students now think he/she is less likely to be around in the future; Other reason (open ended); Don't know. In the lower income school, 37% of students picked option b). Option a) was picked by 7% of students, and option c) was only mentioned by 2% of students. Albeit just suggestive, these numbers indicate that there is indeed an update in terms of peers' perception of a student's social type stemming from a decision to study harder. The low number of students choosing option a) also suggests that there is no direct stigma coming from effort *per se*. It is worth noting that the most common motives under "Other reason" in the lower income school were related to the student now being too busy to spend time with their friends (9% of students). In the higher income schools, where our evidence indicates that effort stigma is not the main driver of negative peer pressure, the evidence follow-up survey suggests that students seem to understand the mechanisms that would underly that type of channel if it were present in their school: the numbers are very similar to the ones from the lower income school (8% picking option a), 36% choosing b), and 4% mentioning c)).

Ability reward. A direct prediction of our framework is that, under the public condition in the higher income school where reward for high ability is the driver for negative peer pressure, students who sign up under $p = 0.75$ are expected to be of higher ability than those who sign up under $p = 0.25$. Under $p = 0.75$, high ability students are less worried about the risk of revealing their (economic) type. To test this prediction, we ask students in the second form in the first visit to describe their typical grades. We asked: "In general, how are your grades? 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." The closest to a median split is to separate students into those choosing a) and b) and those choosing the other options.

The data support the prediction from the model. In the higher income schools, under $p = 0.25$ in public, students among students who did not sign up for the prep package, 49% reported to generally have high grades (mostly A's or mostly A's or B's). Among those who signed up, 50% reported to generally have high grades (the p-value of the difference is 0.931). Under $p = 0.75$, among those who did not sign up for the prep package, 45% reported to generally have high grades; among those who signed up, the share went up to 75% (the p-value of the difference is 0.016). The 31 percentage point difference in the effect of signing up on the probability of reporting high grades when going from $p = 0.25$ to $p = 0.75$ in public in the higher income schools is also significant (p=0.084). In the private condition, there are no significant differences in the probability of reporting high grades between those who signed up and those who did not, and there is no significant effect of having a higher p or a differential effect of a higher p among those who signed up.

As an additional approach to provide suggestive evidence of the proposed mechanism, we asked in the follow up survey the following question: "Now suppose a classmate becomes more popular because he/she is studying too hard. Why do you think this would happen?" Students were asked

to pick one option among the following: a) Because other students admire hard workers; b) Because other students now think he/she is a smart person and they admire smart people; c) Because other students now think they can get help in their studying from him/her; d) Other reason: (open ended); e) Don't know. In the higher income schools, 58% picked either option a) or b) (29% for each option), and 21% picked option c). In the lower income school, 17% picked option a), 20% chose b), and 30% chose option c. These numbers are merely suggestive but they indicate that in the higher income school, there seems to be a culture that supports hard work and being smart.

4.4 Further Evaluating the Stakes: Impact on Probability of Taking the SAT

Our main objective is to test for mechanisms underlying negative peer pressure: the signup decision is the relevant outcome. However, as an additional way to evaluate the stakes of that decision, we revisited the three schools between late May and early June 2016, right before the end of the academic year. Students reported whether they had already taken the SAT (or the ACT, the vast majority choosing the SAT), their score (in case they had taken the exam(s) and already had access to the score), whether they were planning to take one of the exams, and if so, when. Our goal is to assess whether the SAT prep package that was offered to these students had an impact on these longer-term outcomes. It is important to note though that in analyzing these outcomes, we lose experimental control since students in the different treatments are likely to have discussed or coordinated with each other after our team left the classroom, which may have changed their beliefs about whether others would learn about their decision. Other types of peer effects, such as social learning, could have been triggered through such communication. As a result, the estimates we compute here are not useful for testing our main hypotheses.

In Appendix Table A.2, we present the effects on longer-term outcomes. In panel A, we restrict our sample to students in the private condition, across all schools. As discussed earlier, we observe similar signup within the private condition, across the two levels of p. In fact, we also observe a similar selection of students that sign up in the private condition across the two levels of p. Individual characteristics are balanced for students who sign up in private for $p = 0.75$ and $p = 0.25$ (results available upon request). We can therefore examine the reduced-form impact of p in private on longer-term outcomes. In the first three columns of Panel A, we analyze the effect of a higher p on the probability that student reported to have already taken the SAT (or ACT) by the time of our follow-up visit. We find evidence of a marginally significant, positive effect of over 10 percentage points on the probability that students reported to haven already taken the test. This amounts to a 40-50% in that probability. A sizable share of students take the SAT on the first June test date, which was a few days after our visit. We therefore create another dummy variable on whether the student has already taken the SAT, or will take it on that date, which is the last one during 11th grade. We also find significant increases on that probability for students assigned to the higher

probability of getting the prep package.[14]

In Appendix Table A.2, Panel B, we examine the same outcomes focusing instead of the effect of the private condition: by how much are these outcomes changed when the effects of peer pressure are turned off during the signup stage? This is relevant for evaluating the reduced-form effects of a policy that made signup private. For the outcomes of columns 1-3, we observe an increase of about 8 percentage points (or a 30% increase) in the probability of reporting to have already taken a college admissions test by the time of the visit. For the second outcome (columns 4-6), we also observe a large and significant increase.

These results suggest that our intervention might have had longer-term effects. However, in addition to the caveats mentioned above in terms of loss of experimental control, we interpret our findings with extra caution. Students can still take the SAT later, so the measured effect might just have been to make them take the test earlier. The outcome is self-reported and maybe there is more desirability bias/demand effects for students who chose to take the prep package or for those who won the lottery to gain access to it. Finally, it could be that the effects are not coming just from the package, but instead the package got them to use other types of prep services (though this could still be considered a mechanism through which the intervention affected additional outcomes).

4.5 Discussion

The evidence presented above suggests that in all three schools in our experiment, students face peer pressure not to engage in educational effort, at least in the form of preparing for SAT. At the same time, the effect of high-vs.-low probability of winning the prep is different between the lower income and the higher income schools.

At the very least, this result implies that different mechanisms must be responsible for the negative peer effects in different schools. In section 4.4, we showed that the available evidence is consistent with the two mechanisms ("stigmatizing effort" and "signaling ability") that we discussed in the Introduction and formally described in Section 2. In what follows, we discuss other possible mechanisms and alternative explanations for our results, and argue that they are either inconsistent with our evidence or rely on additional assumptions that are unlikely to hold.

1. The effect of lotteries

Since there is virtually no difference in sign-up rates for low and high probabilities in the private setting, we can infer that probabilities per se do not play a role. This does not, however, rule out the possibility that participation in different lotteries may be approved or stigmatized by peers.

[14] We were not able to use test scores as an outcome. First several students who had already taken a college admissions test did not report their scores, either because they had not received yet or because they chose not to report them. As a result, we end up with too few observations. Moreover, regressions using test scores would either be conditional on the student having already taken the test (thus implying differential selection across treatments) or would bundle the intensive and extensive margins, making it difficult to isolate the intensive margin effect.

It may, in principle, be possible that participation in lotteries is stigmatized. But then in lower-income schools there should be a higher stigma associated with low-probability lotteries, and in higher-income schools the opposite should be the case. Alternatively, in lower-income schools there might be a stigma associated with losing a lottery, no matter the odds, and in higher-income schools there may be a stigma of failing to win a high-probability lottery only.

We think that neither of these explanations (lotteries have no direct effect but a stigmatized in a particular pattern) is plausible. None of these stories would explain why the effect of public-vs.-private setting is different for students who believe it is important to appear smart, which is present at least in the higher-income schools. We should also mention that Bursztyn and Jensen (2015) found similar negative peer pressure effects without lotteries, which suggests that stigmatizing lottery participation cannot be responsible for the observed results.

2. Preferences for privacy

It may be possible that students have concerns about privacy, in the sense that they prefer to have as little disclosed about them as possible. Specifically, they would prefer to hide their willingness to sign up, and if they have to reveal it, they would prefer to hide their diagnostic score. Such explanation is partly consistent with the results in higher-income schools, where students are more likely to sign up in public when the probability is winning is 25% rather than 75%. However, if a student who would not sign up under 75% decides to sign up under 25%, then the benefit of winning net of associated privacy costs should be positive, in which case he might as well have signed up under 75% (in this case, the cost and benefit would just triple). Also, this still does not explain why students who believe that appearing smart is important are more affected. Lastly, this explanation cannot explain negative peer pressure effects in lower income schools.

3. Effort may be stigmatized, but for a different reason.

In lower income schools, we showed that the direction of the effect of the lottery probability in the public setting is consistent with effort being stigmatized. In the model, we assumed that effort is stigmatized because a student who exerts effort signals that their opportunity cost of studying is low, and in particular they do not value the company of their peers. However, the results would be equally consistent with anything that is correlated with propensity to exert high effort and that could be stigmatized.

Specifically, it is possible that effort is stigmatized per se (we show that the predictions of our model are robust to this possibility in the Supplemental Appendix). It is possible that high effort signals desire to leave the community; that it signals putting parents and/or teachers in high regard; that it signals having a full family where parents have time to parent the children; that it signals intention to choose a specific profession, perhaps a lawyer or a police officer; that it signals

belief that effort rather than luck determines success; or virtually anything else. These individual stories are difficult to disentangle; for this reason, we tend to think that we have identified a class of mechanisms that can explain negative peer pressure effects in each of the schools.

A propensity to exert high effort may also be correlated with high ability or low ability. Given the special role that signaling ability have to do in the paper, we consider these possibilities separately.

4. Effort is associated with having low ability, unless proven otherwise

There may, in principle, be reasons why low-ability students study more. It may be that high-ability students ace all the tests and only low-ability students have to study. It may also be that studying makes it harder for others to understand the student's true ability, effectively pooling high and low ability students together; in this case, naturally, low ability students would have a higher propensity to study, thus giving rise to such correlation.

If this is the case, then students would be more reluctant to study under 25% lottery than under 75% lottery, because they value opportunity to disclose their ability. Thus, such mechanism is consistent with lower income school, but not with higher income school. However, in the lower-income school, students who do not care about signaling high ability are affected by the lottery just as much as students who do. In addition, in a follow-up questionnaire, we explicitly asked if a student could become less popular because high effort implies low ability, and this does not appear likely.

5. Effort is associated with having high ability

If effort is largely a signal of high ability, which is in turn considered a virtue, then in public setting there should be a higher signup, which is not what we see in either school. Of course, the diagnostic test is also informative about a student's ability, and thus it is possible that students try to pool with smarter peers by exerting high effort, unless the diagnostic test is likely to prove otherwise. But this is exactly the mechanism we describe for the higher-income school: signing up signals high ability, especially in the 25% setting where the student is unlikely to have the diagnostic test revealed. Thus, assuming that effort is associated with high ability and this can lead to negative peer pressure is not an alternative explanation, but another way to state one of our mechanisms.

6. High ability is stigmatized

For various reasons, students may dislike smart peers (e.g., they may believe they would leave the community, regardless of whether they study or not). We do not think such culture is very likely to emerge, because low ability is easy to fake by a high-ability person.

However, suppose, for completeness, that high ability is stigmatized, and students seek opportunities where they could fail spectacularly. This alone would explain positive effects of the public setting on sign-up, as students would sign up for a chance to fail the test. (Deliberately failing the test likely renders the prep package less useful, because tutoring is supposed to be tailored to the performance on this test; however, if the alternative is not signing up at all, making the test useless should not be a concern.)

It may be possible to get negative peer pressure effect if we assume that there is a subset of students who are both high ability and who always exert effort ("committed types"); in this case, the name of the game for a student who thinks about signing up in the public setting is getting a chance to get the test and failing it, as merely signing up would pool the student with these committed types, which is highly undesirable. This would predict a higher propensity to sign up under 75% than under 25%, and is thus inconsistent with the evidence from the higher income school, but may be consistent with the results from the lower income school.

To rule this possibility out, we show that there is no difference in the distribution of grades between takers and non-takers in the public setting (neither 25% nor 75%) in the lower income school), which implies that signing up per se, without additional information, should not be a signal of high ability.

Finally we do not think that the following additional potential concern plays an important role in our setting:

7. Ability to announce the facts of signup, winning the lottery and the diagnostic test in the private setting.

We cannot rule out that in private setting, students could plan to make their decisions public and believe that they could do so credibly. This, however, should not invalidate the comparison between private and public settings in either of the schools. In the private setting, all students who have a net positive benefit from the prep package (i.e., those who are supposed to sign up in private setting) would still sign up; some of them might disclose their grades and some would not, but in either case signing up and not disclosing anything is better than not signing up and not disclosing. Thus, on the margin, the decision to sign up would not be affected – even though the peers' beliefs about the students who eventually disclose and who do not may be different.

5 Conclusion

We find strong evidence consistent with peer pressure. High school students are willing to forgo educational investment or effort opportunities due to concerns about how they will be perceived by their classmates. The results have important implications for school policy with respect to whether effort or investment should be kept private, due to such peer pressure concerns. We also show that

such behavior can arise from two very separate motives. Further, which particular motive is behind peer concerns is predictable, and simple questions can help reveal it.

But the heterogeneity in motives for peer concerns is particularly important to recognize for other reasons as well. In schools where the biggest concern is about revealing a low economic type, privacy of grades is likely to be important. Otherwise, low ability students may reduce effort in order to signal that they are cool. However, in schools where the main worry is to signal a high social type, keeping grades private could in fact be detrimental to performance. In general, not all effort or investments students can make in their education can be kept private. Students must raise their hand in class to ask questions if they want to understand material better, and attending extra sessions or academic clubs will be revealed. In these cases, if students are going to face stigma costs for engaging in effort or investments, it would in fact be preferable for these students to have their grades revealed, so they can at least get the benefit of revealing a higher economic type. This also suggests that the increased emphasis on the privacy of grades, common in the U.S. but less common elsewhere, which may have been a policy designed to enhance performance at good schools, may in fact have a detrimental effect on performance in worse schools.

Though when thinking about what is kept private for students, we may in fact arrive at the opposite conclusion when considering what is revealed about inputs rather than outcomes. For example, though in schools where students care about signaling their ability we may want to keep grades private (so low ability students do not cut back and attempt to signal higher social skills), by contrast we might want to make inputs (effort or investments) as visible as possible. Doing so would enable low ability students to pool with the high ability students. And in schools where effort is stigmatized, though we may want to make grades more visible so that students who try hard at least get the benefits of revealing a high economic type, we may want to make effort or investments as private as possible, so students can try hard without facing social stigma.

Finally, understanding whether students in a particular school are driven more by peer concerns regarding economic vs. social skills (which as noted above may not be difficult to determine ex-ante, or which may be revealed with some simple survey questions) may also help in the design of information or marketing campaigns intended to improve school performance by tailoring the campaign to the specific peer concern that may be holding students back.

References

[1] Ariely, Dan, Anat Bracha, and Stephan Meier (2009). "Doing Good or Doing Well? Image Motivation and Monetary Incentives in Behaving Prosocially," *American Economic Review*, 99(1), p. 544-555.

[2] Austen-Smith, David and Roland Fryer (2005). "An Economic Analysis of Acting White,'" *Quarterly Journal of Economics*, 120(2), p. 551-583.

[3] Ausubel, David (1977). *Maori Youth: A Psychoethnological Study of Cultural Deprivation.* Massachusetts: The Christopher Publishing House.

[4] Berman, Eli (2000). "Sect, Subsidy, and Sacrifice: An Economist's View of Ultra-Orthodox Jews," *Quarterly Journal of Economics*, 115(3), p. 905-953.

[5] Bursztyn, Leonardo, Florian Ederer, Bruno Ferman, and Noam Yuchtman (2014). "Understanding Mechanisms Underlying Peer Effects," *Econometrica*, 82(4), 1273-1301.

[6] Bursztyn, Leonardo and Robert Jensen (2015). "How Does Peer Pressure Affect Educational Investments?," *Quarterly Journal of Economics*, 130(3), p. 1329-1367.

[7] Charles, Kerwin K., Erik Hurst, and Nikolai Roussanov (2009). "Conspicuous Consumption and Race," *Quarterly Journal of Economics*, 124(2), p. 425-467.

[8] DellaVigna, Stefano, John A. List and Ulrike Malmendier (2012). "Testing for Altruism and Social Pressure in Charitable Giving," *Quarterly Journal of Economics*, 127(1), p. 1-56.

[9] DellaVigna, Stefano, John A. List, Ulrike Malmendier, and Gautam Rao (2014). "Voting to Tell Others," NBER Working Paper No. 19832.

[10] Fordham, Signithia and John U. Ogbu (1986). "Black Students' School Success: Coping with the 'Burden of Acting White'," *The Urban Review*, 18, p. 176-206.

[11] Fryer, Roland (2007). "A Model of Social Interactions and Endogenous Poverty Traps," *Rationality and Society*, 19(3), p. 335-366.

[12] Fryer, Rolandand Paul Torelli (2010). "An Empirical Analysis of 'Acting White'," *Journal of Public Economics*, 94(5-6), p. 380-96.

[13] Gans, Herbert (1962). *The Urban Villagers: Group and Class in the Life of Italian-Americans.* New York: The Free Press.

[14] Goffman, Erving (1959). *The Presentation of Self in Everyday Life.* New York: Anchor Books.

[15] Karlan, Dean and Jonathan Zinman (2009). "Observing Unobservables: Identifying Information Asymmetries With a Consumer Credit Field Experiment," *Econometrica*, 77(6), 1993-2008.

[16] Lee, Kenneth W. and William G. Warren (1991). "Alternative Education: Lessons from Gypsy Thought and Practice," *British Journal of Educational Studies*, 39 (3), p. 311-324.

[17] Spence, Michael (1973). "Job Market Signaling," *Quarterly Journal of Economics*, 87(3), p. 355-374.

Figures and Tables

Figure 1: **Comparative Statics of the Model**

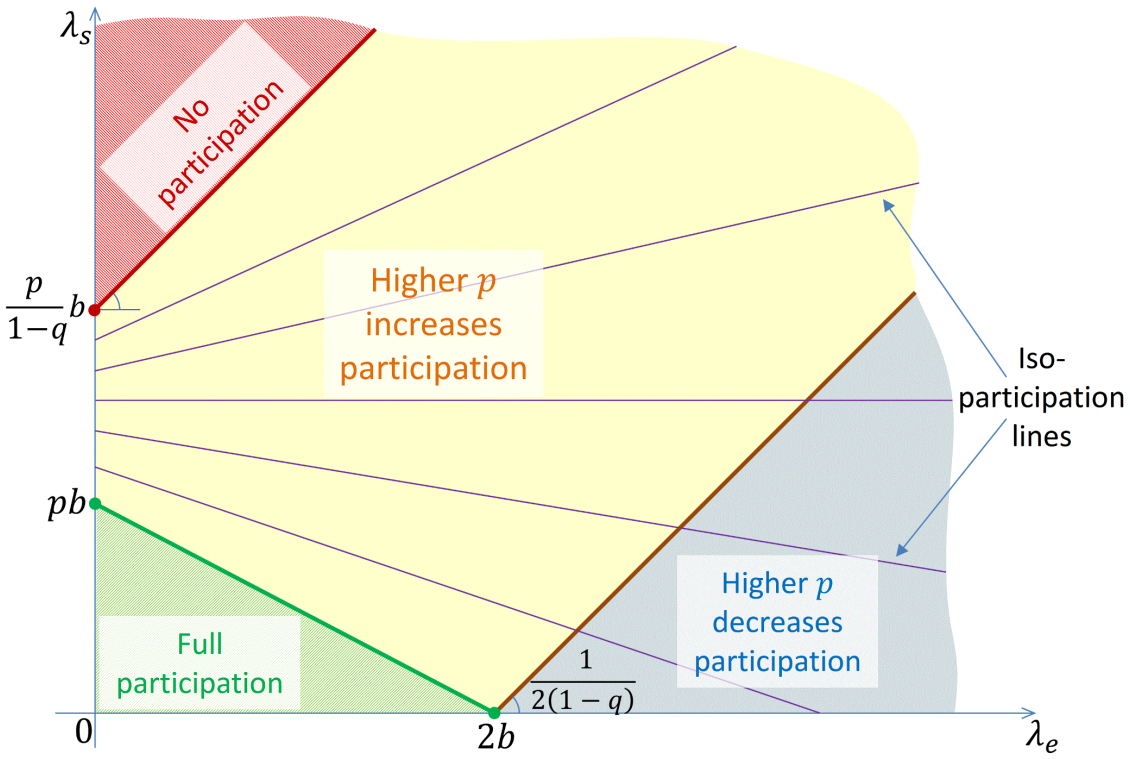
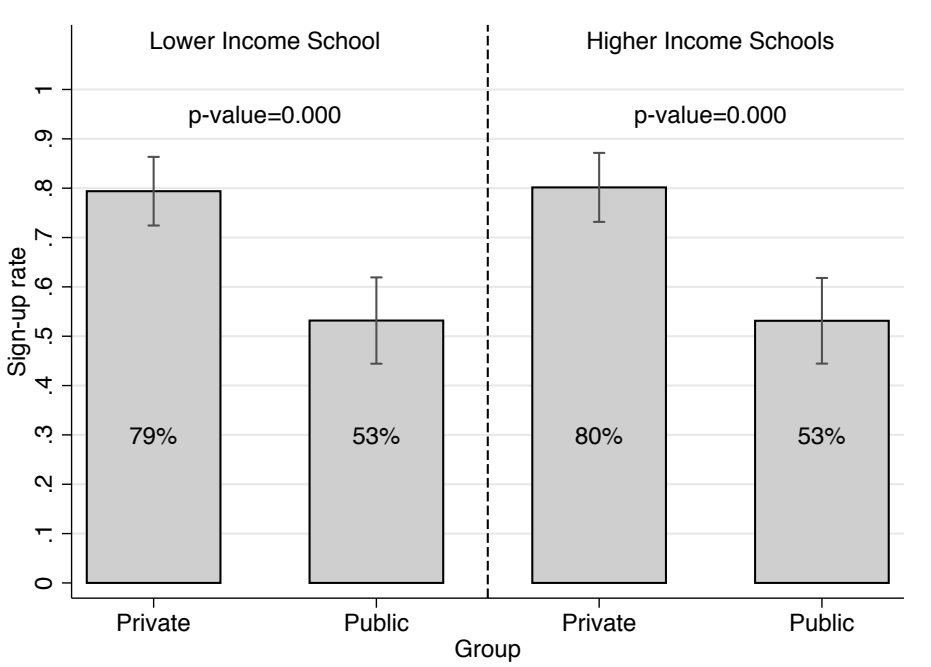
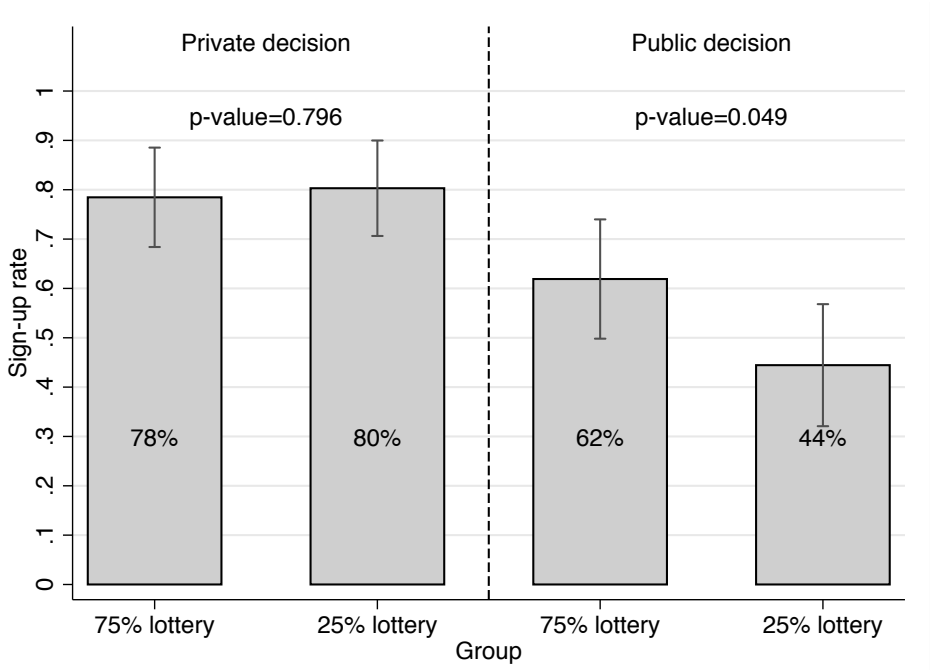


Figure 2: **Effect of Public Treatment on Signup Decision**



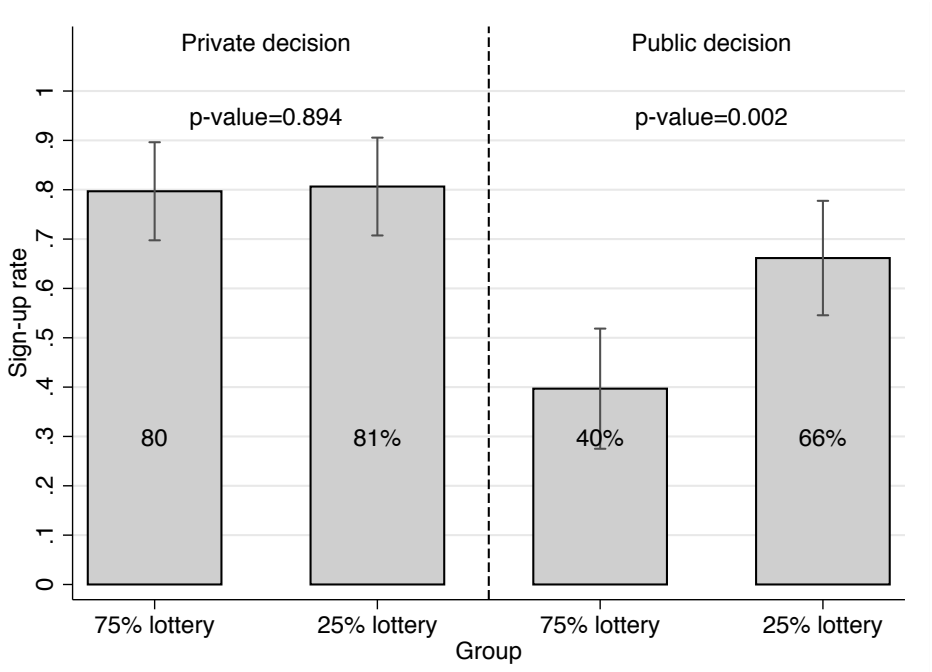
Notes: This figure presents the means and 95% confidence intervals of the signup rates for students in the private and public conditions, across all schools. There are 511 observations in total, 257 in the lower income school and 254 in the higher income schools.

Figure 3: **Effect of Public Treatment and Probability of Winning the SAT Prep Package on Signup Decision – Lower Income School**



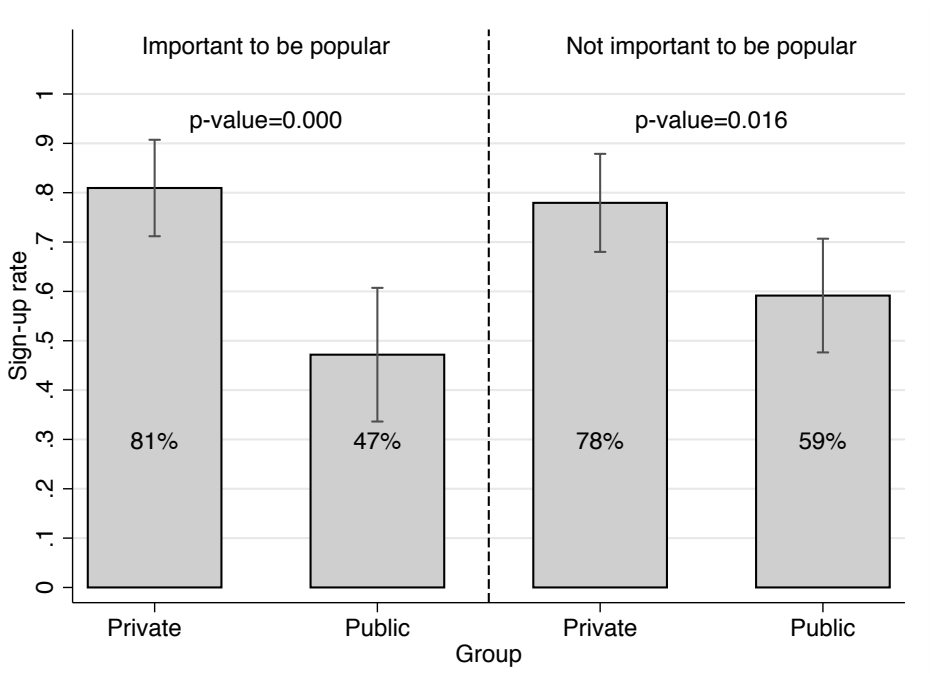
Notes: This figure presents the means and 95% confidence intervals of the signup rates for students across four conditions: private/high probability (N=65), private/low probability (N=66), public/high probability (N=63), and public/low probability (N=63), for the lower income school. There are 257 observations in total.

Figure 4: **Effect of Public Treatment and Probability of Winning the SAT Prep Package on Signup Decision – Higher Income Schools**



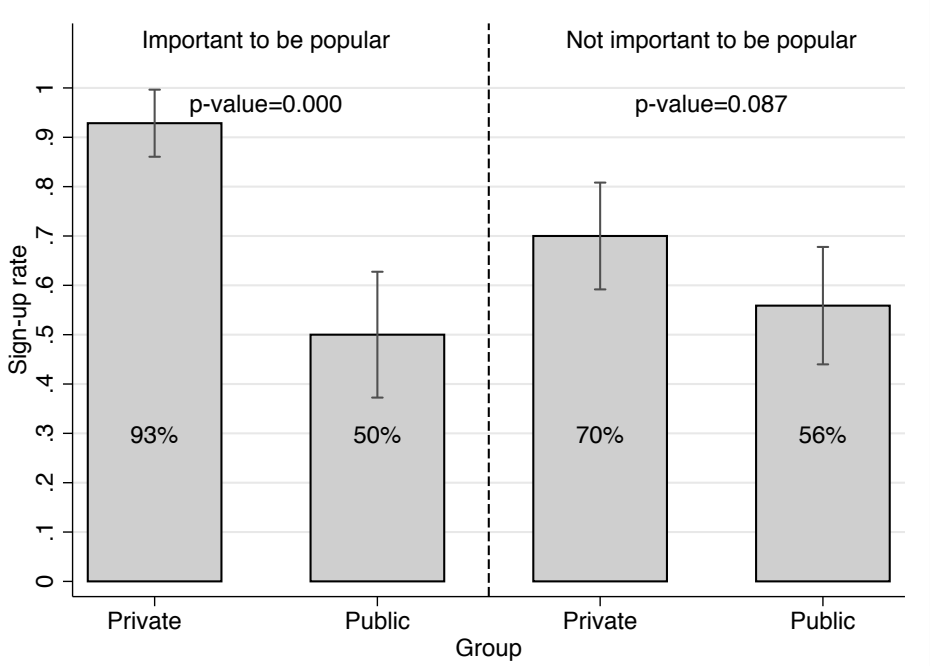
Notes: This figure presents the means and 95% confidence intervals of the signup rates for students across four conditions: private/high probability (N=64), private/low probability (N=62), public/high probability (N=63), and public/low probability (N=65), for the higher income schools. There are 254 observations in total.

Figure 5: **Signup Rates for Private vs. Public Decisions: Importance of Being Popular – Lower Income School**



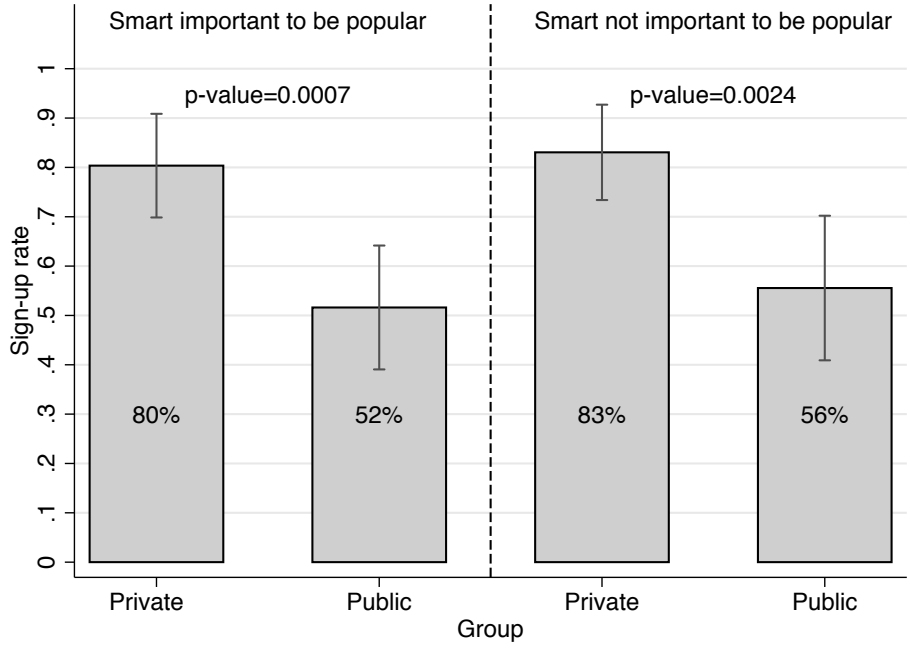
Notes: This figure presents the means and 95% confidence intervals of the signup rates for students in the private and public conditions in the lower income school, separately for students who consider important to be popular in their school and those who do not. The dummy for whether the student considers 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). There are 116 observations in the "important to be popular" panel and classes and 139 in the "not important" panel.

Figure 6: **Signup Rates for Private vs. Public Decisions: Importance of Being Popular – Higher Income Schools**



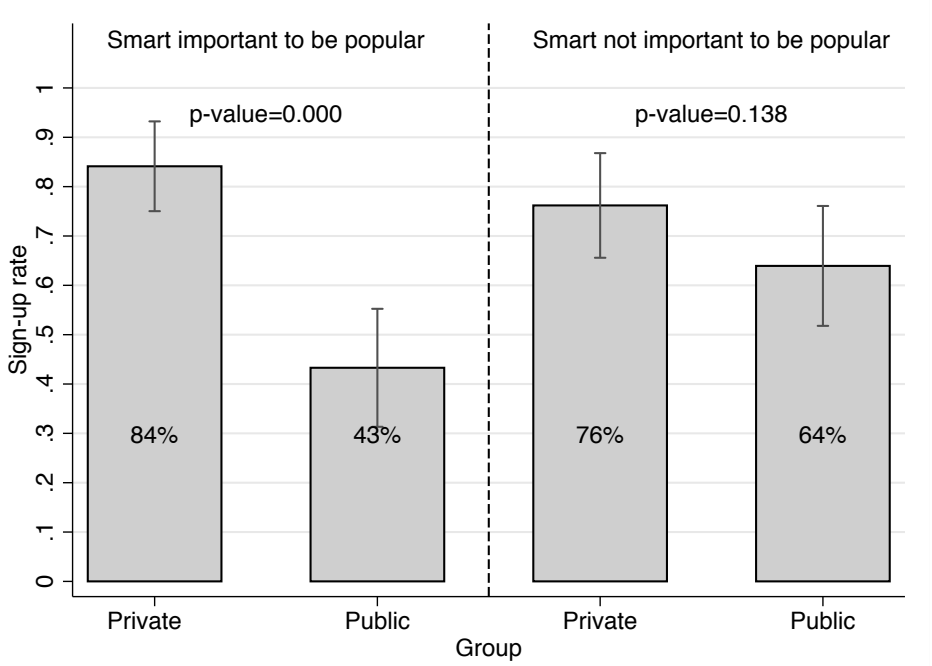
Notes: This figure presents the means and 95% confidence intervals of the signup rates for students in the private and public conditions in the higher income schools, separately for students who consider important to be popular in their school and those who do not. The dummy for whether the student considers 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). There are 116 observations in the "important to be popular" panel and classes and 138 in the "not important" panel.

Figure 7: **Signup Rates for Private vs. Public Decisions: Importance of Being Considered Smart to be Popular – Lower Income School**



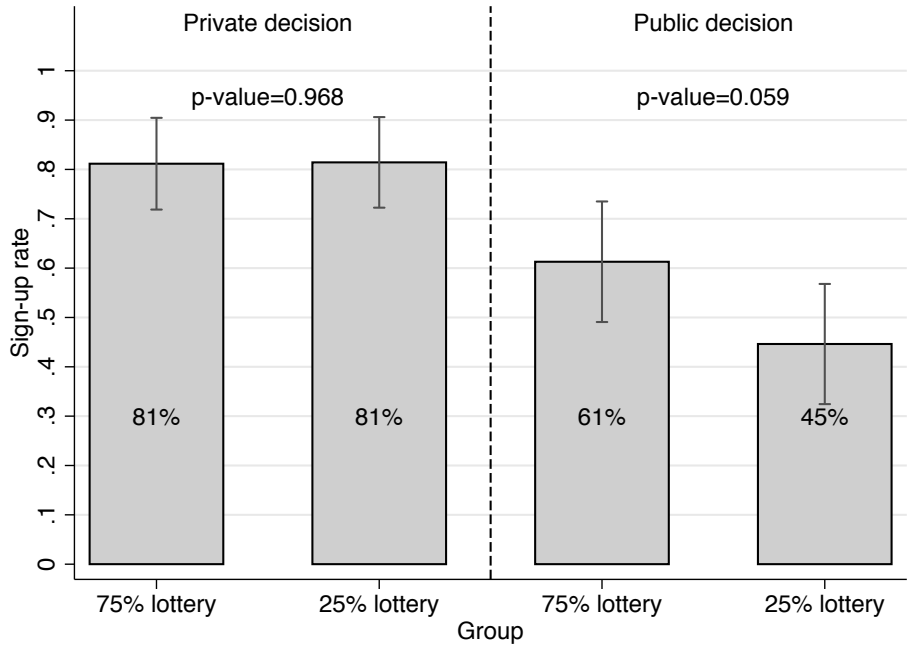
Notes: This figure presents the means and 95% confidence intervals of the signup rates for students in the private and public conditions in the lower income school, separately for students who think it is important to be considered smart in order to be popular in their school and those who do not. The dummy for whether the student thinks it is important to be considered smart to be popular is constructed by collapsing the answers to the statement, "To be popular in my school it is important that people think I am smart" from a 1-5 scale to a dummy variable (answers 3-5 were coded as considering it important, 1-2 as not important). There are 118 observations in the "important to be popular" panel and classes and 104 in the "not important" panel.

Figure 8: **Signup Rates for Private vs. Public Decisions: Importance of Being Considered Smart to be Popular – Higher Income Schools**



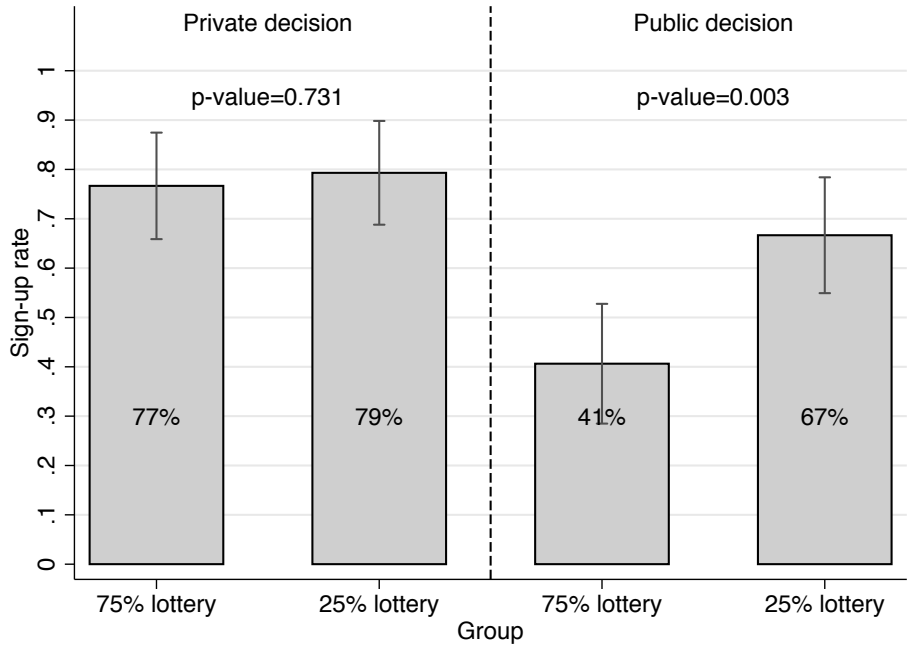
Notes: This figure presents the means and 95% confidence intervals of the signup rates for students in the private and public conditions in the higher income schools, separately for students who think it is important to be considered smart in order to be popular in their school and those who do not. The dummy for whether the student thinks it is important to be considered smart to be popular is constructed by collapsing the answers to the statement, "To be popular in my school it is important that people think I am smart" from a 1-5 scale to a dummy variable (answers 3-5 were coded as considering it important, 1-2 as not important). There are 130 observations in the "important to be popular" panel and classes and 124 in the "not important" panel.

Figure 9: **Effect of Public Treatment and Probability of Winning the SAT Prep Package on Signup Decision – Classrooms Below Median in Opinion on Importance of Being Considered Smart to be Popular**



Notes: We split classrooms across all schools by their average 1-5 answer to the statement "To be popular in my school it is important that people think I am smart." This figure restricts the sample to classrooms below the median, and presents the means and 95% confidence intervals of the signup rates for students across four conditions: private/high probability (N=69), private/low probability (N=70), public/high probability (N=62), and public/low probability (N=65). There are 266 observations in total.

Figure 10: **Effect of Public Treatment and Probability of Winning the SAT Prep Package on Signup Decision – Classrooms Above Median in Opinion on Importance of Being Considered Smart to be Popular**



Notes: We split classrooms across all schools by their average 1-5 answer to the statement "To be popular in my school it is important that people think I am smart." This figure restricts the sample to classrooms above the median, and presents the means and 95% confidence intervals of the signup rates for students across four conditions: private/high probability (N=60), private/low probability (N=58), public/high probability (N=64), and public/low probability (N=63). There are 245 observations in total.

TABLE I: BALANCE OF COVARIATES

	Private High probability [1]	Private Low probability [2]	Public High probability [3]	Public Low probability [4]	p-value [1]=[2]=[3]=[4]
Male dummy	0.543	0.531	0.516	0.5	0.913
	[0.5]	[0.501]	[0.502]	[0.502]	
Age	16.310	16.226	16.31	16.266	0.788
	[0.464]	[0.461]	[0.464]	[0.568]	
Hispanic dummy	0.713	0.75	0.683	0.695	0.645
	[0.454]	[0.435]	[0.467]	[0.462]	
Number of observations	129	128	126	128	

Notes: Columns 1-4 report the mean level of each variable, with standard deviations in brackets, for the four different experimental conditions. Column 5 reports the *p*-value for the test that the means are equal in the four conditions.

TABLE II: EFFECT OF PUBLIC TREATMENT ON SIGNUP DECISION

Dependent variable:	Dummy: The student signed up for the SAT prep package					
	(1)	(2)	(3)	(4)	(5)	(6)
Public treatment	-0.2621***	-0.2595***	-0.2561***	-0.2703***	-0.2517***	-0.2484***
	[0.057]	[0.057]	[0.057]	[0.057]	[0.056]	[0.057]
Inference Robustness						
p-value Robust S.E.	0.000	0.000	0.000	0.000	0.000	0.000
p-value Wild Bootstrap	0.005	0.017	0.005	0.005	0.005	0.005
p-value Permutation test	0.000	0.000	0.000	0.000	0.000	0.000
Mean of private take-up		0.794			0.802	
Includes individual covariates	No	Yes	Yes	No	Yes	Yes
Includes classroom and surveyor FE	No	No	Yes	No	No	Yes
Observations	257	257	257	254	254	254
R-squared	0.077	0.078	0.116	0.082	0.135	0.153
Sample:	Lower income school			Higher income schools		

Notes: Columns 1 to 3 restrict the sample to the lower income school, and columns 4 to 6 restrict to the higher income schools. 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 add individual covariates (age and dummies for male and Hispanic). Columns 3 and 6 further add surveyor and classroom fixed effects. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

TABLE III: EFFECT OF PUBLIC TREATMENT AND LOW PROBABILITY ON SIGNUP DECISION

Dependent variable:	Dummy: The student signed up for the SAT prep package					
	(1)	(2)	(3)	(4)	(5)	(6)
Low probability dummy	0.0184	0.0199	0.0101	0.0096	0.0076	0.0077
	[0.071]	[0.072]	[0.074]	[0.072]	[0.070]	[0.070]
	-	-	-	-	-	-
Public sign-up dummy (A)	0.1656**	0.1633**	0.1645**	0.4000***	0.3752***	0.3794***
	[0.080]	[0.081]	[0.081]	[0.080]	[0.081]	[0.082]
Low probability*Public (B)	-0.1930*	-0.1938*	-0.1839	0.2551**	0.2414**	0.2571**
	[0.113]	[0.114]	[0.114]	[0.112]	[0.109]	[0.110]
Inference Robustness (A)						
p-value Robust S.E.	0.040	0.045	0.043	0.000	0.000	0.000
p-value Wild Bootstrap	0.087	0.101	0.087	0.011	0.015	0.013
p-value Permutation test	0.047	0.052	0.048	0.000	0.000	0.000
Inference Robustness (B)						
p-value Robust S.E.	0.090	0.092	0.108	0.023	0.028	0.020
p-value Wild Bootstrap	0.073	0.065	0.039	0.085	0.127	0.077
p-value Permutation test	0.024	0.033	0.039	0.002	0.004	0.002
Mean of private take-up in high prob. group		0.785			0.797	
Includes individual covariates	No	Yes	Yes	No	Yes	Yes
Includes classroom and surveyor FE	No	No	Yes	No	No	Yes
Observations	257	257	257	254	254	254
R-squared	0.094	0.095	0.133	0.122	0.170	0.192
Sample:	Lower income school			Higher income schools		

Notes: Columns 1 to 3 restrict the sample to the lower income school, and columns 4 to 6 restrict to the higher income schools. Column 1 and 4 present OLS regressions of a dummy variable on whether the student faced a 0.25 (low) probability of getting the SAT prep package conditional on signing up, whether the student signed up for the package in public, and the interaction of low probability with public decision. Column 2 and 5 replicate columns 1 and 4 adding individual covariates (male dummy, age, and Hispanic dummy). Column 3 and 6 replicate columns 2 and 5 adding surveyor and classroom fixed effects. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

TABLE IV: EFFECT OF PUBLIC TREATMENT ON SIGNUP DECISION: BY IMPORTANCE OF POPULARITY

Dependent variable:	Dummy: The student signed up for the SAT prep package					
	(1)	(2)	(3)	(4)	(5)	(6)
Public*Important to be popular (A)	-0.3378***	-0.3374***	-0.3268***	-0.4286***	-0.3820***	-0.3878***
	[0.085]	[0.086]	[0.087]	[0.074]	[0.074]	[0.075]
Public*Not important to be popular (B)	-0.1879**	-0.1857**	-0.1932**	-0.1412*	-0.1479*	-0.1355*
	[0.078]	[0.078]	[0.077]	[0.082]	[0.080]	[0.081]
Important to be popular dummy	0.0301	0.0315	-0.0050	0.2286***	0.2196***	0.2255***
	[0.071]	[0.072]	[0.074]	[0.065]	[0.064]	[0.066]
Inference Robustness (A)						
p-value Robust S.E.	0.000	0.000	0.000	0.000	0.000	0.000
p-value Wild Bootstrap	0.005	0.005	0.005	0.005	0.005	0.005
p-value Permutation test	0.000	0.000	0.000	0.000	0.000	0.000
Inference Robustness (B)						
p-value Robust S.E.	0.016	0.018	0.013	0.087	0.066	0.095
p-value Wild Bootstrap	0.037	0.059	0.035	0.303	0.287	0.327
p-value Permutation test	0.014	0.015	0.014	0.081	0.064	0.104
Mean of private signup for students who do not find it important to be popular		0.779			0.7	
Includes individual covariates	No	Yes	Yes	No	Yes	Yes
Includes classroom and surveyor FE	No	No	Yes	No	No	Yes
Observations	255	255	255	254	254	254
R-squared	0.081	0.081	0.120	0.113	0.161	0.180
Sample:	Lower income school			Higher income schools		

Notes: Columns 1 to 3 restrict the sample to the lower income school, and columns 4 to 6 restrict to the higher income schools. 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 package on a public signup 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 add individual covariates (age and dummies for male and Hispanic). Columns 3 and 6 further add surveyor and classroom fixed effects. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

TABLE V: EFFECT OF PUBLIC TREATMENT ON SIGNUP DECISION: BY IMPORTANCE OF BEING CONSIDERED SMART TO BE POPULAR

Dependent variable:	Dummy: The student signed up for the SAT prep package					
	(1)	(2)	(3)	(4)	(5)	(6)
Public*Important to be considered smart (A)	-0.2874***	-0.2871***	-0.2790***	-0.4084***	-0.3950***	-0.4068***
	[0.084]	[0.083]	[0.083]	[0.077]	[0.076]	[0.077]
Public*Not important to be considered smart (B)	-0.2750***	-0.2671***	-0.2776***	-0.1226	-0.0983	-0.0797
	[0.090]	[0.090]	[0.089]	[0.082]	[0.079]	[0.082]
Important to considered smart to be popular dummy	-0.0269	-0.0267	-0.0080	0.0794	0.0891	0.1030
	[0.073]	[0.074]	[0.076]	[0.071]	[0.070]	[0.069]
Inference Robustness (A)						
p-value Robust S.E.	0.001	0.001	0.001	0.000	0.000	0.000
p-value Wild Bootstrap	0.017	0.023	0.045	0.005	0.005	0.005
p-value Permutation test	0.000	0.000	0.000	0.000	0.000	0.000
Inference Robustness (B)						
p-value Robust S.E.	0.002	0.003	0.002	0.137	0.217	0.332
p-value Wild Bootstrap	0.011	0.033	0.033	0.351	0.489	0.591
p-value Permutation test	0.003	0.004	0.004	0.139	0.213	0.317
Mean of private signup for students who do not find it important to be considered smart to be popular		0.831			0.762	
Includes individual covariates	No	Yes	Yes	No	Yes	Yes
Includes classroom and surveyor FE	No	No	Yes	No	No	Yes
Observations	222	222	222	254	254	254
R-squared	0.094	0.100	0.135	0.110	0.164	0.186
Sample:	Lower income school			Higher income schools		

Notes: Columns 1 to 3 restrict the sample to the lower income school, and columns 4 to 6 restrict to the higher income schools. The dummy for whether the student thinks it is important to be considered smart to be popular is constructed by collapsing the answers to the statement, "To be popular in my school it is important that people think I am smart" 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 package on a public signup dummy, a dummy on whether the student thinks it is important to be considered smart to popular, and the interaction of the two. Columns 2 and 5 add individual covariates (age and dummies for male and Hispanic). Columns 3 and 6 further add surveyor and classroom fixed effects. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

TABLE VI: EFFECT OF PUBLIC TREATMENT AND LOW PROBABILITY ON SIGNUP DECISION: MEDIAN SPLIT OF CLASSROOMS BY AVERAGE OPINION ON IMPORTANCE OF BEING CONSIDERED SMART TO BE POPULAR

Dependent variable:	Dummy: The student signed up for the SAT prep package					
	(1)	(2)	(3)	(4)	(5)	(6)
Low probability dummy	0.0027	0.0015	-0.0088	0.0264	0.0261	0.0251
	[0.067]	[0.068]	[0.069]	[0.077]	[0.075]	[0.076]
Public sign-up dummy (A)	-0.1987**	-0.1988**	-0.2060**	-0.3604***	-0.3466***	-0.3436***
	[0.078]	[0.079]	[0.079]	[0.083]	[0.083]	[0.083]
Low probability*Public (B)	-0.1694	-0.1672	-0.1531	0.2340**	0.2296**	0.2419**
	[0.110]	[0.111]	[0.111]	[0.115]	[0.114]	[0.114]
Inference Robustness (A)						
p-value Robust S.E.	0.012	0.012	0.010	0.000	0.000	0.000
p-value Wild Bootstrap	0.079	0.093	0.079	0.014	0.018	0.018
p-value Permutation test	0.018	0.020	0.014	0.000	0.000	0.000
Inference Robustness (B)						
p-value Robust S.E.	0.126	0.133	0.169	0.044	0.044	0.035
p-value Wild Bootstrap	0.103	0.137	0.127	0.152	0.152	0.108
p-value Permutation test	0.045	0.046	0.068	0.008	0.008	0.005
Mean of private take-up in high probability group		0.812			0.767	
Includes individual covariates	No	Yes	Yes	No	Yes	Yes
Includes classroom and surveyor FE	No	No	Yes	No	No	Yes
Observations	266	266	266	245	245	245
R-squared	0.108	0.111	0.144	0.105	0.143	0.172
Sample:	Below median			Above median		

Notes: In this table, we split the classrooms by their average 1-5 answer to the statement "To be popular in my school it is important that people think I am smart." Columns 1 to 3 restrict the sample to the classrooms below the median, and columns 4 to 6 restrict to those above the median. Column 1 and 4 present OLS regressions of a dummy variable on whether the student faced a 0.25 (low) probability of getting the SAT prep package conditional on signing up, whether the student signed up for the package in public, and the interaction of low probability with public decision. Column 2 and 5 replicate columns 1 and 4 adding individual covariates (male dummy, age, and Hispanic dummy). Column 3 and 6 replicate columns 2 and 5 adding surveyor and classroom fixed effects. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

Appendix Tables

APPENDIX TABLE A.I: BALANCE OF COVARIATES FOR SAMPLE REACHED IN THE SECOND VISIT TO THE LOWER INCOME SCHOOL

	Private High probability [1]	Private Low probability [2]	Public High probability [3]	Public Low probability [4]	p-value [1]=[2]=[3]=[4]
Male dummy	0.571	0.576	0.538	0.455	0.4397
	[0.499]	[0.498]	[0.503]	[0.503]	
Age	16.393	16.305	16.288	16.236	0.5503
	[0.493]	[0.500]	[0.457]	[0.543]	
Hispanic dummy	0.946	0.966	0.962	0.927	0.8083
	[0.227]	[0.183]	[0.194]	[0.262]	
Number of observations	56	59	52	55	

Notes: Columns 1-4 report the mean level of each variable, with standard errors in brackets, for the four different experimental conditions. Column 5 reports the *p*-value for the test that the means are equal in the four conditions.

APPENDIX TABLE A.II: LONGER-TERM OUTCOMES

Panel A - dependent variable: dummy that the student reported to have taken SAT by the time of early June 2016 visit

	(1)	(2)	(3)	(4)	(5)	(6)
High probability treatment	0.1332*	0.1305*	0.0989	0.1580**	0.1591**	0.1307*
	[0.068]	[0.068]	[0.068]	[0.072]	[0.072]	[0.074]
Inference Robustness						
p-value Robust S.E.	0.051	0.056	0.150	0.029	0.029	0.079
p-value Wild Bootstrap	0.008	0.008	0.074	0.006	0.010	0.042
p-value Permutation test	0.037	0.041	0.154	0.021	0.022	0.081
Mean of take-up under low probability		0.26			0.427	
Includes individual covariates	No	Yes	Yes	No	Yes	Yes
Includes classroom and surveyor FE	No	No	Yes	No	No	Yes
Observations	190	190	190	190	190	190
R-squared	0.020	0.029	0.129	0.025	0.034	0.108
Sample:	Private condition					

Panel B - dependent variable: dummy that the student reported that he/she would taken the SAT by the end of 11th grade academic year

	(1)	(2)	(3)	(4)	(5)	(6)
Private treatment	0.0824*	0.0787*	0.0807*	0.1297***	0.1244**	0.1165**
	[0.045]	[0.045]	[0.045]	[0.050]	[0.050]	[0.050]
Inference Robustness						
p-value Robust S.E.	0.071	0.083	0.074	0.021	0.028	0.044
p-value Wild Bootstrap	0.092	0.102	0.098	0.012	0.014	0.018
p-value Permutation test	0.077	0.087	0.087	0.025	0.042	0.042
Mean of public take-up		0.244			0.395	
Includes individual covariates	No	Yes	Yes	No	Yes	Yes
Includes classroom and surveyor FE	No	No	Yes	No	No	Yes
Observations	395	395	395	395	395	395
R-squared	0.008	0.026	0.084	0.013	0.026	0.083
Sample:	Full sample					

Notes: Columns 1 to 3 restrict the sample to the lower income school, and columns 4 to 6 restrict to the higher income schools. 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 add individual covariates (age and dummies for male and Hispanic). Columns 3 and 6 further add surveyor and classroom fixed effects. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.