

Letting Down the Team?

Social Effects of Team Incentives

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Abstract: This paper estimates social effects of incentivizing people in teams. In three field experiments featuring exogenous team formation and opportunities for repeated social interactions, we find large team effects that operate through social channels. In particular, assignment to a team treatment increases productivity by 9 to 17 percent relative to an individual incentive treatment, even though the individual incentive yields a higher private return. Further, we find that in a choice treatment individuals overwhelmingly prefer the individual incentive to the team incentive, despite the latter being more effective. These results are most consistent with the team effects operating through guilt or social pressure as opposed to pure altruism.

JEL Classifications: B49, C93, J01, J33

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1. Introduction

Recent interest in incentives to elicit effort or alter behavior is pervasive and growing, particularly in the domains of education and health.¹ One common feature of these incentive programs is their use of individual-based incentives; that is, an individual's payment is tied to his/her own behavior. But can we elicit more effort by changing the structure of these incentives? In particular, a vibrant but separate literature suggests that peers influence one another through social interactions or social pressure.² Incentive structures, then, that take advantage of the full potential of these social influences may be quite effective.

Some believe that team compensation accomplishes this. One can imagine that peer influences are magnified when teammates' incentives are linked. In real-world environments such as firms, the military, and health and wellness programs, it is not uncommon to see team-based incentives.³ A best-selling management consultant goes so far as to argue that team incentives are more effective than any other policy, and that the effectiveness derives from social factors: "More than any policy or system, there is nothing like the fear of letting down respected teammates that motivates people to improve their performance."⁴

To understand the impact of team incentives, we analyze the results of three separate field experiments in a university setting, two featuring pay-for-studying, which incentivized attendance at a study room in the library and the other featuring pay-for-exercise, which incentivized gym attendance. We study the effect of team-based incentives, focusing primarily how they work relative to individual-

¹ In the context of schooling, Angrist, Lang, and Oreopoulos (2009), Kremer, Miguel, and Thornton (2009), Barrow et al. (2012), Bettinger (2012), and Fryer (2011) study how incentives affect students' performance. Charness and Gneezy (2009), Volpp et al. (2009), Acland and Levy (2011), John et al. (2011), Royer, Stehr, and Sydnor (2012), and Babcock and Hartman (2012) show that financial incentives can potentially promote healthy behaviors such as exercise, weight loss, and smoking cessation.

² References include Sacerdote, 2001, Zimmerman 2003, Bandiera, Barankay, and Rasul, 2005, 2010, 2013, Falk and Ichino 2005, Boisjoly et al, 2006, Foster 2006, Lyle, 2007, Kremer and Levy, 2008, Carrell, Fullerton, and West, 2009, Mas and Moretti, 2009, and Carrell, Hoekstra, and West, 2011.

³ As a small snapshot of examples, see <http://businessfinancemag.com/article/tailored-team-compensation-0501>, <http://compensationmaster.com/articles/tips-for-compensating-teams.html>, http://www.teambuildinginc.com/article_incentives.htm, <http://smallbusiness.chron.com/advantages-offering-teambased-incentive-pay-plan-21644.html>, and <http://www.mbaknol.com/human-resource-management/team-based-compensation-system/>.

⁴ Lencioni (2002), p. 213.

based incentives. These are two settings in which the power of peers has been emphasized and provide controlled environments in which to learn about social effects related to effort elicitation.⁵

In both settings, subjects were randomly assigned to (a) a control group, which received either no incentives or minimal incentives,⁶ (b) an individual-incentive group, which earned incentives based on their own behavior, or (c) a team-incentive group, which was subject to the same-sized monetary incentives as the individual incentive group but whose payment was mainly contingent on the behaviors of a randomly-assigned and known teammate. Our primary interest was to contrast how individuals behaved in the team-incentive versus in the individual-incentive group. Under our incentive schemes, individuals could earn a \$25 bonus for attending the gym at least 5 times or the library at least 4 times, during a specified two-week period. However, under the team incentive, the bonus was dependent on whether both partners went the required number of times.⁷ The goal of our experimental design is to tease out the social effects of incentives. To be clear, we define such effects as *those that are related in a direct way to the utility an individual derives from interacting with others, including but not limited to effects from altruism, guilt, shame, embarrassment, commitment devices, social pressure, or a desire to be liked or respected.*

Despite the pervasiveness of the perception about the effectiveness of team incentives and their prevalence, especially in the workplace, there is a scarcity of empirical research. In a series of papers on workplace incentives among fruit pickers, Bandiera, Barankay, and Rasul (2005, 2010, 2013) emphasize the value of social-type incentives. Bandiera, Barankay, and Rasul (2005) contrast piece rate and relative pay schemes. In the relative pay scheme, a worker's productivity is compared to the average productivity of his/her co-workers. If he/she is more productive, he/she earns more. Bandiera, Barankay, and Rasul (2010) test the value of social connections in an observational design to test whether workers are more

⁵ The new Go4Life campaign funded by the National Institute on Aging (<http://go4life.niapublications.org/>) to encourage physical activity for older Americans allows participants to sign up with a buddy to increase motivation. The website (<http://www.exercisefriends.com/home.aspx>) allows individuals to find others with whom to exercise.

⁶ In the case of studying, we incentivized attendance to a study room in the library. Since we would not be able to monitor the attendance of students who did not receive incentives, the control group in the study experiment earned \$2 per visit up to a certain number of visits.

⁷ Subjects in the individual- and team-incentive groups both earned \$2 per visit up to a certain number of visits.

productive if they have more social ties to their co-workers. Bandiera, Barankay, and Rasul (2013) contrast piece rates, rank incentives, and tournaments among endogenously-chosen teams but do not compare them to individually-based incentive schemes. In the domain of education, Fryer et al (2012) is primarily interested in how loss aversion can be exploited to harness the power of incentives but they have an individual versus team component in their experiment. Teachers received a \$4,000 incentive payment at the beginning of the year but were forced to pay it back if performance measures were not met. For those in the team treatment, the performance measure was based on the team's average performance. In an environment more closely related to our own, Kullgren et al (2013) compares how well individual-based incentives for weight loss work relative to team-based incentives, but teammates are unknown. Generally speaking, these studies conclude that team-based incentives and social connections can improve task performance.

Several features of our incentive scheme make this study unique. First, the potential payout under both the individual and team incentive schemes is the same. In Kullgren et al (2013), those in the team treatment can earn more than those in the individual treatment; team members who are successful earn more than individual-treatment subjects when their teammates are unsuccessful. Second, the success of one's teammate exhibits a positive externality on oneself whereas the opposite is true in Bandiera, Barankay, and Rasul (2005) and Kullgren et al (2013). Third, our tasks involve minimal production complementarities. In most research, the possibility of production complementarities is significant. For example, for workers at a garment factory (Hamilton, Nickerson, and Owan, 2003) or teachers (Fryer et al., 2012), complementarities in the form of specialization, knowledge transfer, or other factors could lead to team incentives being particularly effective. Fourth, unlike in Fryer et al. (2012), our team incentive structure precludes free riding.⁸

⁸ This nonlinear incentive structure is similar in spirit to Holmstrom's (1982) "forcing contracts" except that effort here is observable.

While the last two features of our experiment may make the study less generalizable to real-world settings,⁹ we view them as assets of this study. Since social effects can arise for many reasons (e.g., production complementarities, free riding, knowledge transfer), closing off these channels helps to uncover the mechanisms behind why team incentives may work. We have set up our experiment in an attempt to isolate the social pressures aspect of team incentives. Kandel and Lazear (1992) argue that many practices at firms have more to do with creating social pressure in the form of “empathy, loyalty, and guilt” than with improving the production process in a direct way. Understanding why team incentives may work can help in ascertaining when and in which environments team incentives may be effective.

In an effort to understand our reduced-form treatment effects, we develop and estimate a simple structural model. In this model, if there is no social component to utility and there is some nonzero probability of default by one’s partner, then attendance will be higher for those in the individual treatment than for those in the team treatment. But if attendance is higher (or even the same) in the team treatment, we interpret this as evidence of the existence of social effects. We leverage differences across the different treatments to estimate the size of the social effect.

We try to unpack the social effect further using results from two other treatments. In the first round of the studying experiment, we included an anonymous team treatment arm, which was identical to the team treatment but the teammate was unknown. In such a setting, we would expect the social effect to be less strong except in the presence of strong altruism or guilt (i.e., social-component utility being equally strong in the presence of strangers or known persons). In the second round of the studying experiment we included a choice treatment arm in which subjects could choose between the individual-incentive treatment and the team-incentive treatment. In this treatment, those who chose team incentives were assigned a partner at random from the class. The inclusion of the choice treatment helps us to

⁹ In the military, for example, it is common in boot camp for individuals to be incentivized in much the same manner as in our experiment: When one fails, all members of the team are punished. Kandel and Lazear (1992) argue that “Guilt, in the form of loyalty to... comrades, provides incentives that operate even in the absence of observability. Thus the military spends much time and money creating loyalty and team spirit” (p.807).

understand whether the social effect we estimate is better interpreted as altruism or social pressure. The social-pressure model, as in DellaVigna, List, and Malmendier (2012) for example, posits that people dislike the pressure of making others fail, rather than genuinely caring about others.

This study adds to the growing literature on the effects of social pressure and guilt. Perhaps the earliest work is Asch (1951), who found that subjects in a perceptual task were dramatically swayed by the estimations of their peers. More recently, experimental work by Falk and Ichino (2006) shows that people work differently when a peer is present in the same room and working on the same task; peer effects raise the mean of productivity and reduce its variance. Mas and Moretti (2009) consider peer effects in the workplace, using data on worker productivity from a large supermarket chain. They find strong evidence of positive productivity spillovers from the introduction of highly productive check-out personnel into a shift. Charness, Rigotti, and Rustichini (2007) and Andreoni and Bernheim (2009) observe strong effects from being observed by an audience of one's peers. In the realm of guilt, Charness and Dufwenberg (2006, 2011) find strong evidence of guilt aversion, where one feels guilty to the extent that one believes that one has not met another party's expectations.¹⁰

Our analysis points to several interesting results. First, to our surprise, the team treatment outperformed the individual treatment. Specifically, in the pay-for-study intervention, the average visitation rate for subjects in the team treatment was 20 percent higher than that for subjects in the individual treatment. We estimate similar but slightly more nuanced effects for the gym study. Second, if the partner was unknown as in the anonymous treatment, the treatment worked less well. Subjects in the anonymous team treatment visited the study room 29 percent less often than those in the individual treatment. Thus, any social component of utility – either through altruism or guilt – is less likely to operate when the teammate is unidentifiable. Third, when given the choice between the team and individual treatments, nearly all individuals (97 percent) chose the individual treatment. This suggests that the estimated social effects operate through guilt rather than altruism.

¹⁰ Battigali and Dufwenberg (2007, 2009) provide theoretical models of guilt aversion.

An interesting side point of the study compares subjects in the choice treatment with subjects who were assigned to the individual treatment. We find that those assigned to the choice treatment had a visitation rate that was 27 percent higher than for the individual treatment group. This difference cannot possibly be explained by selection since only 3 percent of the subjects chose the team treatment. Previous studies have found that people who select into an activity perform that activity better, but these differences could be due to both choice and selection. Because we have essentially no selection, the present study is one of the first clean demonstrations that *per se* choosing one's environment can have a beneficial effect on one's performance.¹¹

We view our study as a first step to developing programs to address the lingering issues of obesity and poor academic success. To be clear, the purpose of our study is to develop effective incentive structures that alter studying and exercising behaviors. We believe that the length of these interventions is too short to observe effects on downstream outcomes such as educational outcomes and health. But given a link between studying and exercise behaviors and educational outcomes and obesity, it is possible that more intensive versions of our intervention could lead to improvements in these long-run outcomes.

2. Conceptual Framework

To fix ideas, it is worth developing a simple framework to track benefits and costs for the incentive schemes in our experiment. Consider a program analogous to our own in which individuals receive a bonus pay-off for completing an effort-intensive task. We imagine two incentive schemes. In the individual treatment, person i gains utility $U_i = V_i + B - C_i$ from completing the task, where V is the intrinsic value i has for completing the task, B is the utility derived from the bonus earned for completing

¹¹ Dal Bó, Foster, and Putterman (2010) and Sutter, Haigner, and Kocher (2010) do make very serious attempts at dealing with the selection problem, but the results are nevertheless not as pure as in our study. Corgnet, McCarter, and Hernan (2014) assign people to a setting in which the internet is turned off in the first part of the experiment, while people vote on whether to do so in the second part. All but one group voted to turn off the internet, with production (at least for the subjects who used the Internet in the first half of the experiment) larger with voting. Herbst, Konrad, and Morath (2014) compare behavior of individuals who self-select into teams to behavior in exogenously-formed teams. They find that choice leads to higher efforts than assignment to teams, even though, in their experiment, selection works against this effect.

the task, and C is the effort cost of completing the task.¹² If person i does not complete the task, he earns zero. In the second treatment, there is an additional condition: The individual is assigned a teammate j and receives the bonus *only if his teammate also completes the task*. We define p_j as the probability that person i assigns to his/her partner (person j) completing the task and θ as the magnitude of the social effect. This is the degree to which enabling person j to earn the bonus enters person i 's utility function. We emphasize that this is not, strictly speaking, an altruism parameter, though it could be due in part to altruism and enters the utility function in the way traditionally used to capture altruism. (It could instead capture guilt, embarrassment, fear of social punishment, commitment and other subtle social responses that will be discussed in Section 5.). Lastly, imagine there is also a control group that receives no external compensation for completing the task and whose utility for completion, $U_i = V_i - C_i$, is based entirely on the intrinsic benefit and cost. The conditions under which various subjects complete the task are then:

- (1a) Control Group: Undertake the action if: $V_i - C_i > 0$
- (1b) Individual Treatment: Undertake the action if $V_i - C_i + B > 0$
- (1c) Team Treatment: Undertake the action if: $V_i - C_i + p_j B + p_j \theta B > 0$.

There are, of course, other ways of modelling these behaviors. An alternative view on the team treatment is to consider that an individual receives no social utility gain when his/her partner is successful. Instead, an individual loses utility if his/her partner is successful and she/he is not. This follows in spirit from the DellaVigna, List, and Malmendier (2012) social-pressure model. In this case, the analog to (1c) is equivalently:

- (1c') Team Treatment: Undertake the action if: $V_i - C_i + p_j B > -p_j \tilde{\theta}$

¹² We make the simplifying assumption that all individuals receive the same benefit.

where $p_j\tilde{\theta}$ is expected utility loss if the task is not completed.¹³ One should note that the condition simplifies to (1c) when $\tilde{\theta} = \theta B$. While the structural estimation is therefore unchanged, the welfare implications are different.

We use the choice treatment to shed light on which explanation is more important. Under (1c), only individuals with large p_j and/or large θ would choose the team treatment. Under (1c'), no one would ever choose the team treatment. Of course, these predictions assume that the parameters of the utility function do not change when one is forced to perform an action as opposed to when one chooses to perform an action.

We model the decision to undertake the action in the team treatment for person i to be independent of the cost of effort for person j (his/her partner) and we also consider that p_j is exogenous to person i . Person i takes j 's effort level as given, *ex ante*. The decision to abstract from strategic behavior is motivated by the fact that it would be difficult or nearly impossible to model such interactions without information about beliefs. Also, we do not allow for strategic behavior in the form of side payments between teammates. We monitored teammates closely when the assignment of teammates occurred and subsequently when payments were delivered. We observed no evidence of threats or negotiation of side payments. In fact, at the time of payment, most teammates seemed unaware of the degree of their partner's attendance.

Overall, if there is no social component to utility ($\theta = 0$) and the probability of partner default is greater than 0, then individuals are more likely to undertake the action under the individual treatment than under the team treatment. If incentives work at least as effectively in the team treatment as in the individual treatment, we will interpret this as evidence that social effects exist and are large enough to compensate for lowered expectations of monetary gain. Leveraging the three treatments in the experimental design will also allow us to estimate θ , and quantify the magnitude of the social effect

¹³ We thank the editor, Stefano Della Vigna, for suggesting this.

relative to the direct pecuniary effect. At the end of the paper, we will consider several different behavioral mechanisms and speculate about which are most consistent with our findings.

3. Experimental Design and Sampling

A. Experimental Design, Pay for Studying

In the pay-for-studying part of the experiment, subjects were recruited in several classes in two separate rounds at University of California, Santa Barbara in fall 2011 and fall 2013. We summarize the design here and relegate further details to the appendix. Each round of the experiment consisted of two phases: (1) a recruitment phase involving the completion of a paper survey at the beginning of class, and (2) following class (and outside of the classroom), informing participants of their treatment status.

At the beginning of class, we asked students to fill out a short survey. Most students present in class filled out this survey. Each survey had a unique identifier that determined the treatment arm but which students were unable to decipher. In fact, few (if any) students were cognizant of the presence of the identifier.¹⁴ At the end of class, we organized students into groups based on each individual's unique identifier. Then, we informed each of these groups of their treatment assignment. We had a separate member of the research team describe the treatment to each group. To induce students to show up at the end of class, we told them of their opportunity to earn additional money and of their eligibility to win one, or more, \$50 raffles (large classes were allocated more than one prize). Participation rates were high (68 percent or higher).¹⁵ It is also important to note that essentially everyone who came outside to participate in the experiment stayed through the assignment of treatments and the raffle; four individuals left during treatment assignment in 2011 and six people left during treatment assignment in 2013. In other words, this type of attrition is always less than one percent. For descriptive ease, we refer to the students who

¹⁴ We varied the scheme mapping the identifier to treatment assignment across classes as described in the appendix.

¹⁵ Participation rates are defined as the fraction of students filling out the survey at the beginning of class that showed up after class to participate in the experiment.

decided to participate in the after-class part of the experiment as participants throughout the paper. These participants form our main estimation sample.¹⁶

Participants were incentivized to attend the 24-hour study room in the UCSB library over a two-week period during the fall quarter.¹⁷ We required students to attend at least 40 consecutive minutes between the hours of 11am and 7pm on Monday-Friday. Visits at the study room were supervised by a member of the research team; the research team did not divulge the attendance record of others when subjects inquired. Further details on data collection are discussed in the appendix. Subjects could receive credit for no more than one visit in a day. The treatment groups and control group were as follows:

Control: Subjects were eligible to earn \$2 per visit (up to 4 visits).

Individual Treatment: Subjects were eligible to earn \$2 per visit (up to 4 visits), but also received an additional bonus of \$25 for attendance equal to or exceeding 4 visits (\$33 in total possible earnings).

Team Treatment: Subjects were eligible to earn \$2 per visit (up to 4 visits), but also received an additional bonus of \$25 if and only if both team members accumulated four or more eligible visits. Team members were randomly assigned via unique identifiers on their initial in-class survey. Team members were not required to attend the study room at the same time. As it could be important that team members had a chance to meet and talk, we had teammates stand next to each other during the second phase of the experiment (i.e., the sign-up process outside of class) and exchange names by filling out their partner's name and email on a sheet of paper.

¹⁶ Online Appendix Tables 1 and 2 report the distribution of enrollment sizes, the number of in-class surveys collected, and the number of experiment participants in fall 2011 and 2013. The difference between the survey completion rates and the rates of showing up after class of roughly 30 percent is not large when gauged against other field experiments (e.g., Card, Mas, Moretti, and Saez, 2012). The difference between enrollment size and survey response reflects almost entirely differences in class attendance, late arrival, and subjects enrolled in multiple classes used in this study. While exact class attendance is unknown on the day of recruitment, the vast majority of students present in class completed the survey. Online Appendix Table 3 reports the analogous information for the Rec Center experiment described later.

¹⁷ During the first round the study-room dates were October 10-21, 2011, and during the second round they were October 28 - November 8, 2013.

Anonymous Team Treatment: Subjects were eligible to earn \$2 per visit (up to 4 visits), but also received an additional bonus of \$25 *if and only if both team members* accumulated four or more eligible visits. Differently from the team treatment, the teammate was randomly assigned but unknown (i.e., a member of a different class). This treatment was included in the fall 2011 round only.

Choice Treatment: During the sign-up process, subjects assigned to this treatment were told about both the team and individual treatments. They then had the choice of either one. Subjects were clearly told that they would not learn the identity of their partner until after choosing between the treatment options. This treatment was included in the fall 2013 round only.

Several aspects of the experimental design warrant comment. First, the “control” subjects were paid a minimal incentive of \$2 per visit. This was done because absent our experiment, study room usage is not recorded. In all treatments, students can earn \$2 per visit. Thus, the experiment offers variation in bonus size (\$0 versus \$25) and the method of earning the bonus (either dependent on one’s own behavior or the combined behavior of oneself and a randomly-assigned partner). Second, to encourage studying we emphasized that it was a study room and we monitored subjects. Students appeared to be studying rather than socializing. Third, since subjects in the team treatment are in the same class and we conducted the experiment mid-way through the quarter, the experiment was designed to allow for repeated interaction between teammates. To ensure treatment salience, subjects were reminded via email of their treatment at the end of the recruitment week. In addition, subjects were informed that payments would be made with several weeks left in the quarter. Thus, there was ample time for social interaction after team members received information (i.e., payment) that potentially revealed whether they had “let down the team.”

B. Experimental Design, Pay for Exercise

For the pay-for-exercise part of the experiment, subjects were recruited at several classes at University of California-Santa Barbara during the second summer session in 2010. The sign-up process

was similar to that for the library experiment except that there was no anonymous team treatment or choice treatment. All details are the same as in the library experiment, except as stated below.¹⁸

In this part of the experiment, subjects were incentivized to attend the UCSB Recreation Center (“Rec Center”) from August 7-20, 2010. The Rec Center is the on-campus student gym, which is free for registered students. The Rec Center collects electronic data of ID card swipes. Note, unlike in the studying experiment, we do not require that students spend a specified amount of time at the Rec Center, but it should be noted that the Rec Center is located sufficiently far away from the academic buildings that Rec Center attendance incurs substantial time cost.

As in the studying experiment, all subjects were eligible for the \$50 raffle. Unlike in the pay-for-study experiment, control subjects were not paid for visits. This is because the Rec Center, unlike the library, requires subjects to sign in and records their logins. Subjects were randomly assigned to one of the following groups:

Control: Subjects were not eligible for extra payment.

Individual Treatment: Subjects were eligible to earn \$2 per visit (up to 5 visits), but also received an additional bonus of \$25 for attendance equal to and exceeding 5 visits (\$35 in total possible earnings).

Team Treatment: Subjects were eligible to earn \$2 per visit (up to 5 visits), but also received an additional bonus of \$25 if and only if *both team members* accumulated 5 or more eligible visits.

4. Empirical Results – Pay for Studying

We divide the discussion of the empirical results in two sections. We first describe the results for the pay-for-studying experiment and then describe the results for the pay-for-exercise experiment.

¹⁸ As in the pay-for-studying set-up, attrition during treatment assignment was less than one percent, only three people left during this process.

A. Pre-Treatment Descriptive Statistics

Table 1 shows the sample means of self-reported characteristics (gender, age, and pre-treatment library usage) by treatment status for participants in the pay-for-studying experiment. Columns (2) – (6) report the differences in mean characteristics between the control group and each of the other treatment groups. Age, gender, and pre-treatment library usage do not differ significantly between the control and treatment groups as indicated by the p-values reported in the table.

B. Results

The primary treatment effect finding is easily seen graphically. Figure 1 shows the distribution of study room visits during the treatment period, by treatment group. The data are pooled across the two rounds. The top category, four visits, includes all participants with four or more visits. Notice the stark rightward shift of the distribution for all treatments relative to the control group. For our purposes, the most important contrast is between the individual and team treatments. While both incentive schemes produced an effect, the team treatment resulted in more visits. This is in spite of the fact that within the team treatment the risk of a partner's default (not making four or more visits and hence making their partner ineligible for the bonus) was 54 percent. It is clear, then, even at first glance, that large, team-related social effects are implied, because the visitation rate in the team treatment is 22 percent higher than in the individual treatment.

We formalize the analysis using the following simple regression specification:

$$(2) \quad Y_{ij} = \beta_0 + \beta_1 T_{ij}^{Any} + \beta_2 T_{ij}^{Team} + \beta_3 T_{ij}^{Anon} + \beta_4 T_{ij}^{Choice} + \delta_j + \varepsilon_{ij},$$

where Y_{ij} is an attendance outcome for individual i in round j (either 2011 or 2013), T_{ij}^{Any} is an indicator variable for having been randomized into any of the four treatment groups, T_{ij}^{Team} is an indicator variable for being in the team treatment, T_{ij}^{Anon} is an indicator variable for being in the anonymous treatment, T_{ij}^{Choice} is an indicator variable for being in the choice treatment, δ_j is an indicator for 2013, and ε is the usual error term. The coefficient of primary interest is β_2 , as this captures the difference between team and

individual treatment effects. In the absence of social effects, we would expect β_2 to be negative (i.e., the team treatment to do worse).

We examine three attendance outcomes: (1) the number of study room visits during the treatment period; (2) whether the subject went at least once to the study room during the treatment period; (3) whether the subject used the study room on four or more different days during the treatment period (which is the threshold for receiving the \$25 bonus).¹⁹

The baseline results for equation (2) using each of the three outcome measures defined above are reported in columns (1) – (3) in Table 2.²⁰ The individual treatment estimates, reported in the first row, show that subjects responded strongly to the direct individual pecuniary incentive. Specifically, subjects in the individual treatment made about 0.9 more visits to the study room during the treatment period than did controls, were 18 percentage points more likely to have gone to the study room at least once, and were more than twice as likely (24 percentage points) to have met the 4-visit bonus payment threshold. All these differences are statistically significant at the 5 percent level. It is clear that subjects eligible for a bonus for study room visits visited more often.

The primary focus of this paper is the difference between the individual and team treatments. Participants randomized into the team-incentive scheme made 0.4 more visits to the study room during the treatment period, were 11 percentage points more likely to have gone to the study room at least once, and were not significantly less likely to meet the 4-visit threshold compared to those in the individual treatment, despite the substantial risk of default by their partner.

Perhaps more surprisingly, participants assigned to the choice treatment also exerted more effort than the individual treatment. This is despite the fact 97 percent of participants assigned to the choice treatment selected the individual treatment option over the team treatment option (see Figure 2).²¹ On

¹⁹ For the first round of the studying experiment, we also had administrative data on grades. However, because the intervention was so short and almost half of the participants were transfer students (for whom we have no pre-treatment grades), the treatment effects are noisy, partially due to the fact that we cannot control for previous term grades.

²⁰ Online Appendix Tables 4 and 5 show the estimates separately for the two years of the studying experiment – 2011 and 2013.

²¹ For completeness, Online Appendix Table 6 includes separate indicators for choice treatment subjects who chose the team treatment and choice treatment subjects who chose the individual incentive. The *chose* the team treatment group includes the five subjects who *chose* the team option and three people moved from the control group to partner with them. The odd numbers are

average, the choice treatment group made 0.5 more visits to the study room, were 12 percentage points more likely to go to the study room at least once, and were 10 percentage points more likely to attain the 4 visit bonus payment threshold than the individual treatment despite facing the same incentives. This finding is not driven by selection since nearly everyone chose the individual treatment. It must therefore be the case that simply having made it a *choice* to be in the individual treatment spurred this increase in effort. While there are a number of experimental studies (see footnote 10) that find that people who select an activity outperform those who are assigned to an activity, the two populations in these studies typically differ in important ways due to selection; here they are essentially identical. To the best of our knowledge, we are one of the first to demonstrate this pure choice effect. This effect may arise because people feel more committed to engaging in an activity when they have chosen it; this might be a form of self-signaling (Prelec and Bodner, 2003; Benabou and Tirole, 2006; Grossman, 2011).

In contrast, subjects paired with an anonymous partner put forth more effort than those in the control group (0.3 more visits), but the anonymous treatment was less effective than the individual treatment (0.6 fewer visits), and much less effective than the team treatment in which subjects met their partners (1.0 fewer visits). Looking back at the conceptual model, one would expect the anonymous treatment to be weaker than the team treatment if θ and/or p_j is small. Thus, the results are consistent with the social effect being smaller in the anonymous case, but are also consistent with the same social effect and a larger ex ante estimate of partner default. Either way, knowing the identity of one's partner (and knowing that said partner also knew one's identity, etc.) seems to be important.

Columns (4) – (6) add controls for age, gender, pre-treatment library, and class fixed effects. Not surprisingly, the results are very similar in all cases.

The cost per study room visit is reported at the bottom of Table 2. Not only is the team treatment more effective, it also costs less because it includes subjects who put forth effort to meet the threshold but did not get paid the bonus (due to a teammate defaulting). The average per visit cost in the team treatment

due to a matching error; in one instance two choice subjects opting for the team treatment were accidentally paired. All results are the same if they are excluded from the sample.

was \$4.87, whereas the average cost for the same outcome in the individual treatment was \$6.86. The cost per visit for the choice treatment is similar to the cost for the individual treatment at \$6.83 per visit. At the other end of the spectrum, the cost per visit for the anonymous treatment is only \$2.90 because the partner default rate is very high (67 percent). At least two caveats warrant comment. First, the above costs are for a one-time interaction. If this experiment was repeated multiple times, with the same partners, the cost estimates would certainly differ as individuals learn of their partner's probability of default over time. Second, the cost calculations do not include any non-monetary (perhaps emotional) costs associated with avoiding letting down the team. If we interpret the social parameter as arising mainly through guilt aversion these un-monetized costs may be non-trivial.

In the next subsection we use the model of Section 2 to separate out pecuniary and social effects, and to estimate their relative magnitudes.

C. The Social Effect

How large is the social effect implied by these results? The decision to complete the task and earn a \$25 bonus for person i is given by equations (1a), (1b) and (1c) in the model of Section 2, if she is assigned to the control, individual, or team treatments, respectively. For the moment, we restrict the analysis to these treatments, and do not consider the anonymous or choice treatments. In the discussion below, if (1c') is the appropriate characterization for the team treatment, then our estimates of θ should be interpreted as guilt aversion rather than a pro-social effect. In particular, following the guilt/shame characterization of (1c'), our estimates of $\tilde{\theta}$ would be equivalent to estimates of θB .

We estimate the components of utility described in Section 2, allowing individuals to vary in their tastes and predicted behavior based on observable characteristics. First, we model the utility that individual i derives from his/her partner completing the task. Second, we use the generated probability of individual i 's partner completing the task, based on the first step as an input in the utility model for individual i (along with the treatment status and individual characteristics). More formally, this non-linear model featuring a set of probit regressions is as follows:

$$(3) \quad Y_i^{p*} = \alpha_0 + \alpha_1 X_i^p + \varepsilon_i, \quad Y_i^p = 1[Y_i^{p*} > 0]$$

$$(4) \quad Y_i^* = \delta_0 + \delta_1 X_i + \delta_2 IT_i + \delta_3 \widehat{p}_i^p + v_i, \quad Y_i = 1[Y_i^* > 0],$$

where Y_i^* is the utility for individual i associated with completing the task, Y_i is 1 if i completes the task and zero otherwise, Y_i^p is 1 if i 's partner completes the task and zero otherwise (note that it is zero by definition when no partner is present), X_i and X_i^p are background characteristics of person i and his partner, respectively, IT_i is an indicator variable identifying assignment to the individual-treatment group, \widehat{p}_i^p , predicted from the probit in equation (3), indicates i 's belief about the probability that his/her partner will complete the task (and is zero if i is not in the team treatment), and ε_i and v_i are the usual probit error terms. These background characteristics include age, sex, previous library days, and year of experiment fixed effects. Here, equation (3) is only relevant for the subjects with partners (i.e., the team sample). For everyone else, $Y_i^{p*} = 0$ (thus, $Y_i^p = 0$). We start with this approach and later consider deviations from this approach where we vary the way in which \widehat{p}_i^p is derived.

There are three identifying assumptions implicit in this approach. The first is that the non-monetary utility (net of cost) associated with going to the study room is independent of the availability of the monetary incentive. The second simplifying assumption is that beliefs about partner study room attendance are based on partner's initial observables (which we gather from our recruitment survey done at the beginning of class), and that subjects do not take into consideration their partner's reactions to their own initial observables when predicting their partner's behavior (i.e., the lack of strategic behavior discussed earlier). The third is that predictions of the partners' probability of completing the task are correct on average, since the estimate is based on observed data. Under these assumptions, we can estimate the probability of going to the study room at least four times for all individuals in the three treatments in a single equation that includes a generated regressor (i.e., the probability of the partner completing the task).

This estimation strategy leverages the experimental design in several ways to identify the components of utility (compare equation (4) with 1a-c): (1) Observed characteristics (age, gender, and

pre-treatment library usage) identify intrinsic benefits and costs associated with study room visits without compensation ($\widehat{V_i - C_i} = \hat{\delta}_0 + \hat{\delta}_1 X_i$); (2) The difference between observationally similar subjects in control and individual treatments identifies utility gains associated with own pecuniary benefits ($\hat{B} = \hat{\delta}_2$); (3) a comparison of observationally-similar subjects in individual and team treatments identifies social effects related to partners' payoff ($(1 + \hat{\theta})\hat{B} = \hat{\delta}_3 \rightarrow \hat{\theta} = \frac{\hat{\delta}_3}{\hat{\delta}_2} - 1$). One interpretation of the θ is the utility derived from one's partner obtaining the bonus (interpretation consistent with equation (1c)). Under the alternative interpretation, equation (1c'), where individuals are subject to a utility loss due to shame or guilt if their partner does not achieve the bonus, our estimate of θ is equivalent to an estimate of $\frac{\bar{\theta}}{B}$, i.e., the utility loss as a result of shame or guilt divided by the utility gain from one achieving the bonus.

We bootstrap to account for the presence of the generated regressor. Table 3 displays the results.²² The estimates imply that subjects received a utility gain of 0.68 utils from their own pecuniary benefit of being paid \$25 (i.e., the estimate in the individual treatment indicator row), and received a utility gain of 1.85 utils from pecuniary and social benefits together (i.e., the estimate from the predicted partner bonus status row). Bootstrapped confidence intervals indicate that both estimates are distinguishable from zero at the 5 percent level. Thus, our estimate of the social parameter θ , is 1.72, and is also statistically distinguishable from zero at the 5 percent level. The implied social impact of team compensation is very large; 1.72 times as large as the effect of own pecuniary compensation. Importantly, we emphasize that one cannot infer from the findings that agents care more about others than about themselves. We have captured a broad social effect, rather than simple altruism, and will attempt to interpret it more carefully in the next subsection.

The coefficient of 1.72 in Table 3, derived from a model in which agents use information to predict teammates' choices, is our preferred estimate of θ . However, it could be argued that subjects have

²² Online Appendix Tables 7 and 8 display the analogous estimates for the two years of the experiment – 2011 and 2013, separately.

difficulty estimating their partners' probability of meeting the payment threshold, given observables. For example, one might question whether previous library usage is observable.²³ We therefore explore the robustness of the social-parameter estimate to different beliefs about the probability of partner default.

Panel B of Table 3 displays alternative estimates of θ , given two different focal beliefs about teammate performance. First, we consider what happens under the scenario where subjects are correct on average but lack the ability to make finer distinctions between individuals, based on observables (i.e., they use the unconditional team-treatment mean). The estimated θ is 1.70, very close to our preferred estimate in Panel A. Second, we consider the case of perfect foresight; they perfectly predict what their partner will do. This also yields a similar estimate of the social effect parameter, $\hat{\theta} = 1.57$.

We can also consider the implied social effect under the anonymous treatment. The structural model of equations (3) and (4) is inappropriate for the anonymous treatment because a subject cannot observe the characteristics of his/her teammate and use them to predict default probability. But it is possible in the anonymous treatment for subjects to form estimates of teammate default probability based on group means or possible focal beliefs. Panel C shows estimates of θ in the anonymous treatment assuming subjects use (1) the unconditional anonymous treatment mean and (2) the unconditional team treatment mean. Under both assumptions we fail to reject the null hypothesis of $\theta = 0$.

D. Evidence on Potential Mechanisms

The parameter θ is intended to capture incentive effects due to social factors. It is an umbrella term covering a number of potential mechanisms. We now turn our attention to which of some commonly-positied classes of mechanisms are consistent or inconsistent with the evidence at hand.

²³ One might also be concerned that subjects consider how their partners will react to their own observables when they form their estimates of their partners' probability of completing the task. The simultaneous two-person game that could be used to represent the team treatment is a simple coordination game with two pure-strategy Nash equilibria and a mixed-strategy Nash equilibrium. We do not argue that agents find their way to the mixed-strategy Nash equilibrium in this one-shot game (which depends on the form of their utility functions) but map out scenarios consistent with a range of beliefs.

1) **Altruism, guilt, shame, fear of social punishment.** There are a number of different forms of social motivations that could come into play in our environment. One such motivation is altruism, in which the payoff of another person (or persons) enters into one's own utility function regardless of circumstances, beliefs, actions, etc. But it bears emphasizing that social motivations come in many other flavors, and that θ in our framework could capture any of these.²⁴ For example, guilt aversion involves an individual feeling guilty about disappointing the expectations of people who act favorably on one's behalf; the more one believes that other people expect one to perform favorably, the more guilty one would feel from non-performance. Shame involves negative feelings about one's behavior, regardless of the expectations of or observation by others. It is also possible that subjects feel neither altruism, nor guilt, nor shame, but simply wish to avoid reprisal and social punishment from peers they disappoint.

Two of our treatments can give further evidence on this point. Our finding that participants in the anonymous team treatment exert less effort than those in the individual treatment makes it less likely that strong altruism or guilt with respect to strangers is a primary driver. The fact that almost no one in the choice treatment opted for the team treatment suggests that the social parameter in our model may be best interpreted as social pressure or guilt avoidance rather than altruism. While opting not to have a partner does not rule out the possibility that if forced to have a partner one would then behave altruistically, it does hint that responding to social pressure may be the more likely explanation.

The exit-survey data further support the social-pressure interpretation. A few weeks after each experimental round we invited all participants to complete an online survey. To incentivize participation, we entered all participants with completed surveys in a random draw for \$100 in the 2011 round and an iPad mini in the 2013 round. Response rates did not significantly differ across groups. Participants who attended the study room four or more times were asked why they went four or more times. They could check as many answers as applied. These statistics are only suggestive as the rates of attending the study room differed across the treatments, and thus, the sample is selected on an endogenous outcome. Table 4

²⁴ Recent papers investigating forms of social preferences include Loewenstein, Bazerman, and Thompson (1989), Bolton (1991), Fehr and Schmidt (1999), Bolton and Ockenfels (2000), and Charness and Rabin (2002). See Charness and Kuhn (2011) for a survey of the literature on these (and other) social motivations.

displays the mean response rates for each question by treatment group. By and large, the responses are similar across groups. The key exception is the fraction of participants who say that they went four times so as not to disappoint their partner: 55 percent of team-treatment members indicate this reason as do 28 percent of anonymous-team-treatment members.²⁵

2) **Production Complementarities.** The potential for production complementarities has been the dominant justification for the construction of teams in the workplace. Lazear (2000) asserts that production complementarities are absolutely necessary for team incentives to be effective, and that teams should not be used when these are not present. Production complementarities could explain the large estimate of θ if subjects put forth more effort in the team treatment because studying is more valuable or productive (or even more fun) when done jointly.

Online Appendix Table 9 suggests that almost no teams coordinated study room attendance because they enjoyed studying together. Only four percent of the team treatment group report that at least part of the reason they attended the study room four or more times was that they enjoyed studying with their partner. Online Appendix Table 10 corroborates this by comparing the percentage of times team treatment pairs attend the study room at the same time compared to placebo pairs (randomly-assigned placebo partners from the team treatment in the same class). We estimate 29 extra same time visits (arrival within 10 minutes of each other) for teammates compared to placebo pairs. This accounts for 0.07 visits per participant. Since the difference in visits between team and individual treatment groups was 0.40 visits per subject, coordination of this type could explain approximately 18 percent of the difference.

3) **Self-control and pre-commitment.** In models of self-control and pre-commitment, individuals fail to meet goals because the present self lacks the ability to bind itself to a plan of action that would benefit the

²⁵ One person in the choice treatment who chose the individual treatment incorrectly responded that he/she did not want to disappoint his/her partner even though he/she did not have a partner. Also, one subject in the anonymous treatment incorrectly responded that he/she liked going to the study room with his/her partner when she/he did not know his/her partner.

future self; the present self would instead rather engage in a more-immediately-pleasurable activity.²⁶ Having a partner could remedy this problem, even if the individual does not value the teammate's payoff. Individuals who have been jointly incentivized may use each other to devise commitment mechanisms.

While our ability to say much about these issues is limited, we have some evidence pointing towards this self-control mechanism operating through coordination. Looking at the coordination of visits in Online Appendix Table 10, we see that for 28.6 percent of pairs both team members completed 4+ visits compared to only 21.5 percent of placebo pairs. At the same time 20.6 percent of team pairs both completed 0 visits compared to 16.5 percent of placebo pairs. This suggests that some fraction of team pairs coordinated.

In summary, we find some evidence that teammates coordinated their visits, but not enough to fully explain the difference in visits. There is evidence to suggest that it might be reasonable to interpret the results more as social pressure (or guilt avoidance) as opposed to altruism. But, there is little evidence to support a case for guilt or altruism towards strangers (i.e., pure altruism).

Distinguishing more finely between these subtle channels is a subject for future research. One might imagine that manipulation of subjects' interaction with their teammates may be an interesting line of study. For instance, a face-to-face meeting with a teammate may have a larger effect on inducing effort than an online meeting. Understanding how these effects operate is useful—for example, workers at a workplace may not necessarily have in-person meetings with their co-workers but instead be in different physical environments.

Are the observed social effects an artifact of the pay-for-study environment? In any field experiment, generalizability is a concern. It is worth investigating, then, whether there is evidence of social effects of team incentives in other effort-elicitation contexts, beyond the library. Our pay-for-

²⁶ For some models of self-control and commitment, see Laibson (1997), O'Donoghue and Rabin (1999, 2001), Gul and Pesendorfer (2001), Bénabou and Tirole (2004), Fudenberg and Levine (2006), and Ozdenoren, Salant, and Silverman (2012). For empirical and experimental work on this topic, see DellaVigna and Malmendier (2006), Ashraf, Karlan, and Yin (2006), Burger, Charness, and Lynham (2011), and Houser, Schunk, Winter, and Xiao (2009).

exercise field experiment, the results of which are reported in the next section, parallels the analysis above in a different context.

5. Empirical Results – Pay for Exercise

A. Pre-Treatment Descriptive Statistics

Table 5 shows sample means of baseline characteristics (gender, age, self-reported exercise, previous gym visits) by treatment status for participants in the pay-for-studying experiment. Self-reported exercise is the number of times per week that individuals claim to have exercised during the previous month and previous gym visits is the number of times subjects went to the Rec Center in the week prior to the treatment period, based on data provided by the Rec Center. Columns (2) and (3) report the differences in mean characteristics between the control group and the individual and team treatment groups. Average age, self-reported exercise, and previous Rec Center visits do not differ significantly between the control group and either the individual or team treatment groups. In contrast, the randomization was such that more males ended up in the team treatment group than in either of the other two groups. However, conclusions from regressions reported in the remainder of the paper are not sensitive to the inclusion or omission of gender controls.

B. Results

As in the pay-for-studying case, the primary findings are easily seen graphically. Figure 3 shows the distribution of Rec Center visits during the treatment period, by treatment group. The top category, five visits, includes all participants with five or more visits. Notice the stark rightward shift of the distribution for both treatments relative to the control group. While both incentive schemes produced an effect, the team treatment induced more visits: Average visits for the control, individual, and team treatment groups were 2.1, 3.8, and 4.2, respectively. This is in spite of the fact that within the team treatment the risk that one's partner would not reach the threshold for the bonus was 44 percent.

Again we formalize the analysis using a simple regression specification:

$$(5) \quad Y_i = \beta_0 + \beta_1 T_i^{Any} + \beta_2 T_i^{Team} + \varepsilon_i,$$

where Y_i is an attendance outcome for individual i , T_i^{Any} is an indicator variable for having been randomized into one of the two treatment groups (individual or team), T_i^{Team} is an indicator variable for being in the team treatment, and ε is the usual error term.

Table 6, analogous to Table 2, reports the results for equation (5). Unlike in the study room experiment, the rate at which a bonus was earned in the team treatment was about the same as in the individual treatment in the Rec Center case. But this similar task completion rate across individual and team treatments again hints at the existence of a social effect. Despite the high risk of teammate default, subjects in teams were just as likely to put forth effort to earn bonuses as subjects for whom there was no default risk (i.e., those who had no teammate). While this is consistent with a sizeable social effect, ultimately, we will not be able to rule out the absence of a social effect under all focal beliefs about partner gym attendance.

In the exercise experiment, participants may have had fairly good *ex-ante* measures of Rec Center attendance, as observable physical fitness may have given subjects a good indication of their partner's propensity to exercise. Moreover, subjects could have inquired about their partner's past Rec Center usage, which is a strong predictor of their use of the incentives. In contrast, propensity to study may have been harder for subjects to observe. Thus, it is not surprising the effects of the intervention are different across the two studied settings.

If gym-going propensity is indeed easily observed, then one might imagine there could be heterogeneous treatment effects, by type and partner type. In Table 7 we show heterogeneous effects—dividing individuals into “active” types, who visited the Rec Center in the pre-treatment period, and “inactive” types, who did not. On balance, the coefficients on team treatment in Panels B and C reveal that active types go to the Rec Center more when incentivized as individuals and inactive types show up more when incentivized in the team setting.

We further investigate differences between active types and inactive types by partner type in Panels D and E. We should not make strong conclusions from this Table as the sample sizes are smaller in the subgroup analysis and we are testing multiple hypotheses simultaneously. In Panel D, the point estimates on team treatment for the actives are all negative, indicating that active types go to the Rec Center less when incentivized in teams than when incentivized as individuals, regardless of partner type. For actives, responses to changes in expected own monetary payoff appear to dominate social effects.

Panel E, however, tells a very different story for the inactive types—individuals who are less likely, *ex ante*, to go to the Rec Center. Focusing on the specification with no controls (columns 1-3), inactive types with active types as partners go to the Rec Center 1.49 more times, are 21.6 percentage points more likely to go at least once, and are 25.5 percentage points more likely to meet the 5-visit bonus threshold than inactive types incentivized as individuals. This occurs despite the fact that the expected monetary pay-off is lower than in the individual treatment. A large non-pecuniary effect and/or strong beliefs that the partner will be successful may explain this finding. However, interestingly, inactive types randomly partnered with inactive types do not behave in this way. The team treatment is less strong when an inactive type is matched with another inactive as opposed to being matched to an active type. We take this as evidence that subjects estimate the probability of default by their partners, based on observables. One's own expected payout matters, but so too do social factors.

C. The Social Effect

Exactly as in the analysis of the study-room experiment, we use the model defined by equations (3) and (4) to estimate pecuniary and social components of utility for the exercise experiment. Table 8 displays the results. The estimates imply that subjects received a utility gain of 1.29 utils from their own pecuniary benefit of being paid the \$25 bonus, and received a utility gain of 2.17 utils from pecuniary and social benefits together. Bootstrapped confidence intervals indicate that both estimates are distinguishable from zero at the 5 percent level. The social parameter, θ , is estimated to be 0.68 and is statistically distinguishable from zero at the 5 percent level. Subjects choose their effort as if they valued a marginal

dollar of compensation for their teammate as much as they value 68 cents of compensation for themselves. The social impact of team compensation, then, is large enough to compensate for the 44 percent teammate default rate, and is two thirds as large as the effect of own pecuniary compensation.

Panel B of Table 8 displays the estimate of θ , given two different focal beliefs about teammate performance. First, we consider what happens under the scenario where subjects are correct on average but lack the ability to make finer distinctions between individuals, based on observables. The estimated θ is 0.70, very close to our preferred estimate in Panel A. Second, we consider the case of perfect foresight; subjects can perfectly predict what their partner will do. In this case, the estimated social-effect parameter is much smaller at $\hat{\theta} = 0.17$. The difference between the baseline estimate of θ and the perfect foresight estimate reflects the fact a very high percentage of subjects achieve the bonus threshold regardless of what their partner does (see Online Appendix Table 12). In particular, 44 percent of subjects whose partner does not attend the Rec Center a sufficient number of times to earn the bonus themselves do attend five or more times. Like the pay-for-studying experiment, we see very little coordination in gym attendance on the dimensions of timing or visit distribution (see Online Appendix Tables 11 and 12).

7. Conclusion

There is a vast and growing literature on the role of incentives in altering behavior. However, there is limited empirical work on behavioral interventions that compensate individuals for team behavior. Management consultants allege that team compensation harnesses a powerful social mechanism, in that individuals will be more likely perform actions for their team than they would be strictly for themselves. If this is true, in part or in total, then it should be taken into account in designing interventions that seek to elicit effort.

A first step is to observe the effect in simple settings that allow for rigorous causal inference but also preserve the possibility of repeated social interactions over time. Our primary contribution is that we demonstrate the existence of a social effect of team compensation: We observe people in two real-world settings raising their effort level because a teammate's payoff is at stake. Findings indicate that the magnitude of this effect can be large. In addition, the team-incentive scheme in our experiment was 26

percent to 29 percent more cost effective than individual incentives. We caution against extrapolating these effects to exist in an environment with long-run interactions. Over time, individuals are likely to be cognizant of their partner's ability or inability to complete a task, potentially affecting the effectiveness of the team treatment.

Ultimately, this study examines a crucial issue related to the optimal structure of incentive schemes and how best to use incentives to elicit behaviors at lowest cost. We have just scratched the surface on this crucial issue. Future work will help to understand more fully the mechanisms and the environments under which team incentives are most effective.

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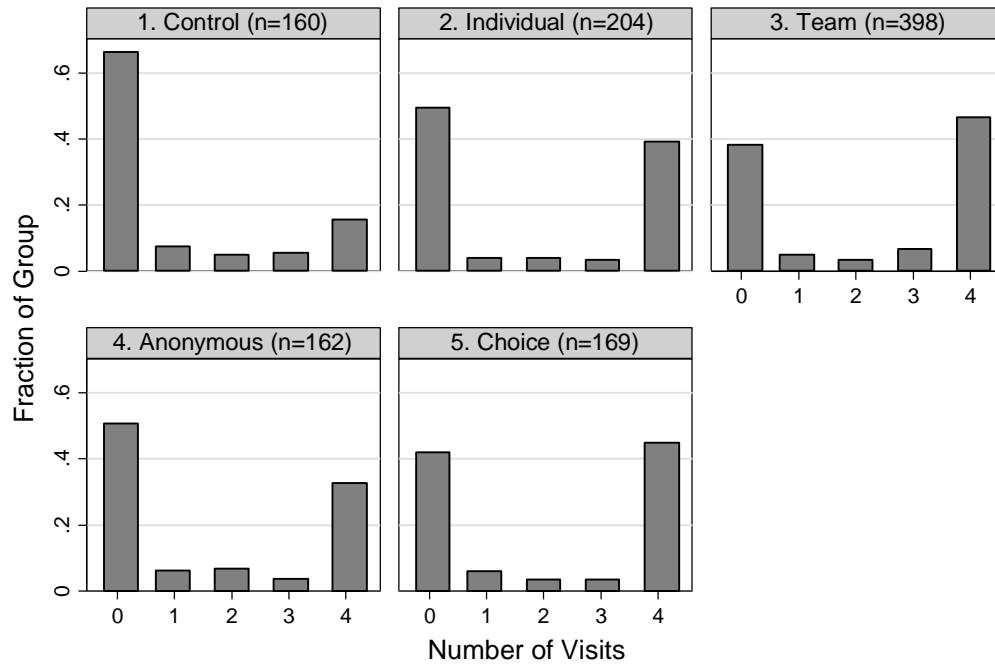
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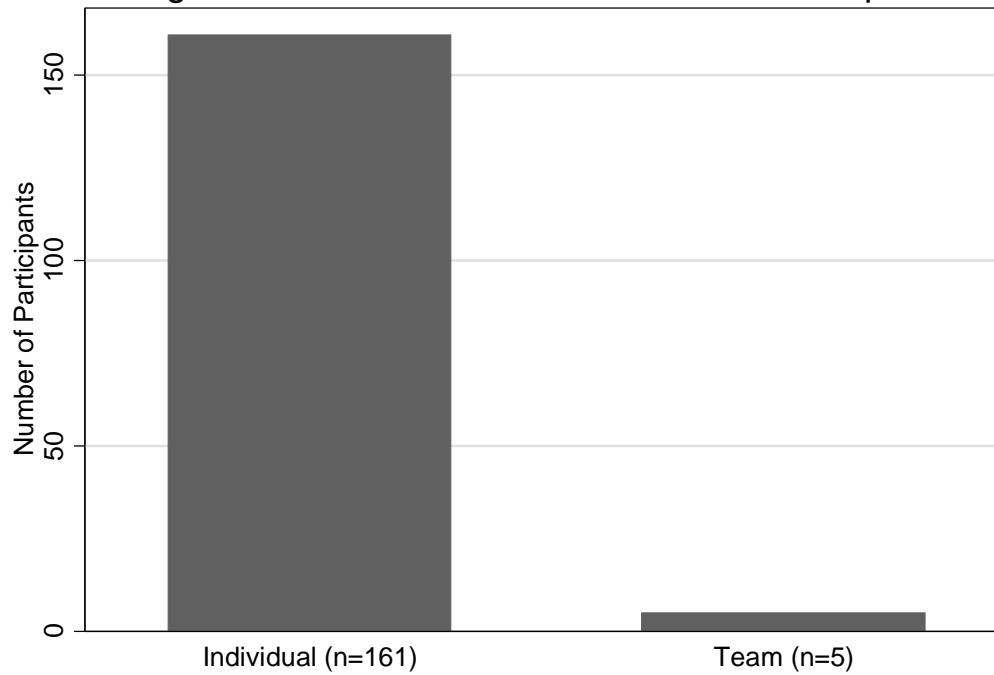
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Figure 1. Distribution of Study Room Visits



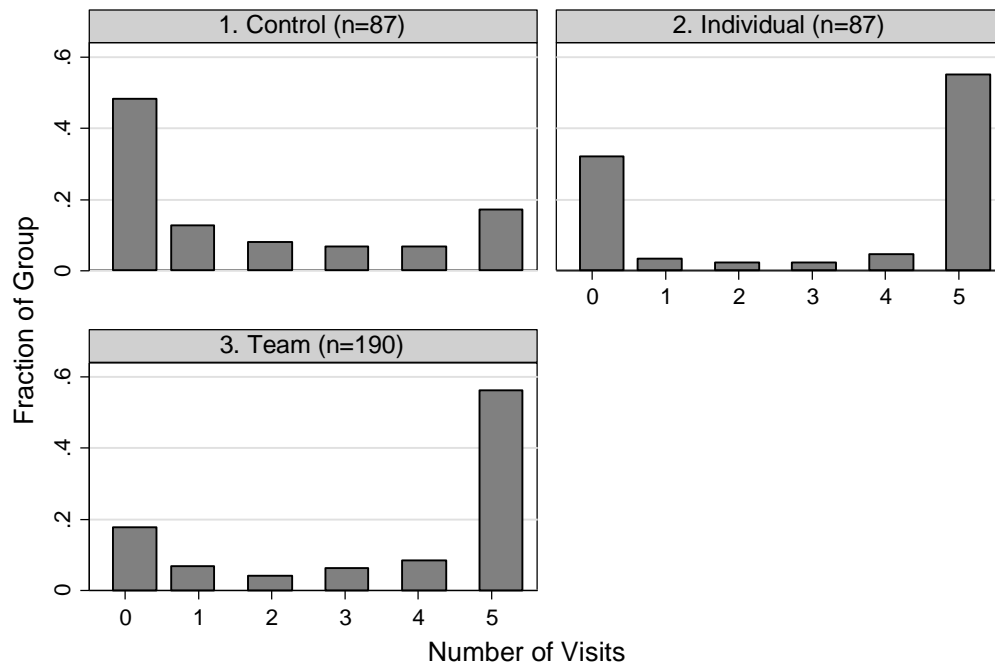
Note: The bars labeled 4 visits include all participants with 4 or more visits.

Figure 2. Distribution of Choice Treatment Options



Note: Includes 166 participants randomized into choice treatment. Excludes the three participants moved from a control group to serve as partners. The odd numbers are due to a matching error; in one instance two choice subjects opting for the team treatment were accidentally paired. All results are the same if they are excluded from the sample.

Figure 3. Distribution of Rec Center Visits



Note: The bars labeled 5 visits include all participants with 5 or more visits.

Table 1. Pre-Treatment Descriptive Statistics for Study Room Rounds

	Control Group Mean (1)	Difference Between Treatment and Control			
		Individual Treatment (2)	Team Treatment (3)	Anonymous Team Treatment (4)	Choice Treatment (5)
Male	0.55 (0.50)	-0.09 [0.09]	-0.03 [0.52]	-0.05 [0.47]	-0.05 [0.44]
Age	20.57 (2.62)	-0.19 [0.34]	-0.25 [0.14]	0.08 [0.81]	-0.35 [0.17]
Days studied in library last week	1.64 (1.88)	0.30 [0.16]	0.18 [0.33]	0.19 [0.46]	0.16 [0.59]
Sample size	160	204	398	162	169

Differences in columns 2-6 are from OLS regressions that include class-year fixed effects. Regression samples in columns 2-6 include the control group and the group listed at the top of each column. Standard deviations for the control group are in parentheses. P-values for differences are in square brackets. Sample size refers to the group listed in the column heading.

Table 2. Study Room Visits by Treatment Status

	Number of Visits (1)	≥1 Visit (2)	≥4 Visits (3)	Number of Visits (4)	≥1 Visit (5)	≥4 Visits (6)
Any treatment	0.900** (0.211)	0.176** (0.051)	0.243** (0.045)	0.842** (0.212)	0.170** (0.051)	0.242** (0.045)
Team treatment	0.397** (0.197)	0.109** (0.044)	0.069 (0.044)	0.428** (0.193)	0.110** (0.044)	0.072 (0.044)
Anonymous team treatment	-0.579** (0.238)	-0.088 (0.055)	-0.133** (0.053)	-0.612** (0.234)	-0.102* (0.055)	-0.147** (0.052)
Choice treatment	0.542** (0.167)	0.127** (0.054)	0.103* (0.053)	0.550** (0.237)	0.121** (0.055)	0.095* (0.055)
Male				-0.116 (0.138)	-0.026 (0.032)	-0.022 (0.031)
Age 20				-0.122 (0.194)	-0.040 (0.047)	-0.037 (0.045)
Age 21				-0.133 (0.209)	-0.018 (0.050)	-0.032 (0.050)
Age 22				-0.326 (0.269)	-0.047 (0.062)	-0.132 (0.058)
Age 23+				-0.045 (0.274)	-0.030 (0.062)	-0.025 (0.064)
Library days				0.162** (0.036)	0.033** (0.008)	0.020** (0.008)
Year is 2013	-0.637** (0.167)	-0.129** (0.037)	-0.114** (0.037)	--	--	--
Constant	1.432** (0.176)	0.406** (0.043)	0.217** (0.036)	--	--	--
Includes year-specific class fixed effects	No	No	No	Yes	Yes	Yes
R-Squared	0.06	0.05	0.06	0.12	0.11	0.11

Sample size is 1093. Standard errors are clustered at the group level (i.e., in case of team treatment, the pair level) and reported in parentheses. ** (*) indicates statistical significance at the 5 (10) percent level. Columns 4-6 also include indicators for missing age and sex.

Cost per Visit

	Control Group	Individual Treatment	Team Treatment	Anonymous Team Treatment	Choice Treatment
Average visits per participant	1.09	1.95	2.37	1.75	2.23
Cost per visit	\$1.77	\$6.86	\$4.87	\$2.90	\$6.65

Table 3. Study Room Structural Estimates

	Coefficient	95% Percentile Lower Bound	95% Percentile Upper Bound
<u>Panel A - Structural parameters</u>			
Male	-0.001	-0.193	0.190
Age 20	-0.178	-0.470	0.115
Age 21	0.019	-0.275	0.309
Age 22	-0.287	-0.682	0.083
Age 23+	0.083	-0.309	0.443
Library days	0.085	0.0351	0.138
Predicted partner bonus status	1.852	1.319	2.345
Individual treatment indicator	0.681	0.405	0.950
Year is 2013	-0.169	-0.408	0.045
Constant	-0.967	-1.290	-0.599
Theta	1.717	1.109	2.722
<u>Panel B - Theta under different probability of completion assumptions</u>			
Theta: unconditional team treatment mean	1.700	1.164	2.609
Theta: perfect foresight (true partner visits)	1.567	0.687	3.950
<u>Panel C - Anonymous treatment - theta under different probability of completion assumptions</u>			
Theta: unconditional anonymous treatment mean	0.957	-0.092	2.130
Theta: unconditional team treatment mean	0.376	-0.450	1.145

1000 bootstrap replications. The sample in Panels A and B excludes anonymous and choice treatment group members and individuals with missing data for themselves or their partners. Panels A and B sample sizes are 745. The sample in Panel C excludes team and choice treatment group members and individuals with missing data for themselves or their partners. Panel C sample size is 517.

Table 4. Reasons for Attending Study Hall 4+ Times

	Individual Treatment (1)	Team Treatment (2)	Anonymous Team Treatment (3)	Choice Treatment (4)
I often study in the library	0.47	0.46	0.36	0.48
I wanted to earn the bonus	0.92	0.92	0.95	0.92
I didn't want to disappoint my partner	0.00	0.55	0.28	0.04
I enjoyed going to the study room with my partner	0.00	0.04	0.03	0.02
I like participating in experiments	0.29	0.31	0.26	0.41
The potential bonus helped overcome my tendency to procrastinate	0.31	0.35	0.33	0.37
Sample size	55	139	39	54
Proportion of eligible subjects participating in exit survey	0.69	0.75	0.74	0.71

Proportion indicating the specified reason reported in all columns. Respondents are asked to indicate all reasons that apply. The sample includes only those attending 4 or more times.

Table 5. Pre-Treatment Descriptive Statistics for Rec Center Round

	Control Group Mean (1)	Difference Between Treatment and Control	
		Individual Treatment (2)	Team Treatment (3)
Male	0.55 (0.50)	0.01 [0.86]	0.14 [0.02]
Age	21.14 (2.43)	0.22 [0.52]	0.04 [0.88]
Self-reported days of exercise/week	3.98 (2.29)	0.13 [0.73]	0.21 [0.51]
Gym visits in the previous week	1.17 (1.65)	-0.02 [0.93]	-0.13 [0.49]
Sample size	87	87	190

Differences are from OLS regressions that include class fixed effects. Regression samples in columns 2-3 include the control group and the group listed at the top of each column. Standard deviations are in parentheses. P-values for differences in square brackets. Sample size refers to the group listed in the column heading.

Table 6. Rec Center Visits for Individual and Team Treatments

	Visits (1)	≥1 Visit (2)	≥4 Visits (3)	Visits (7)	≥1 Visit (8)	≥4 Visits (9)
Any treatment	1.690** (0.471)	0.161** (0.074)	0.379** (0.067)	1.806** (0.365)	0.178** (0.064)	0.395** (0.059)
Team treatment	0.347 (0.412)	0.143** (0.059)	0.011 (0.067)	0.288 (0.340)	0.135** (0.054)	-0.014 (0.061)
Male				0.319 (0.259)	0.056 (0.044)	0.081* (0.048)
Age 20				-0.724 (0.440)	-0.121* (0.062)	-0.111 (0.076)
Age 21				-0.556 (0.490)	-0.077 (0.069)	-0.124 (0.083)
Age 22				-1.208** (0.611)	-0.210** (0.082)	-0.224** (0.097)
Age 23+				-1.704** (0.528)	-0.279** (0.083)	-0.317** (0.092)
Pre-period Rec Center visits				0.938** (0.097)	0.102* (0.012)	0.091** (0.015)
Constant	2.126** (0.320)	0.517** (0.054)	0.172** (0.041)	--	--	--
Includes class fixed effects	No	No	No	Yes	Yes	Yes
R-Squared	0.08	0.08	0.11	0.39	0.30	0.27

Sample size is 364. Standard errors are clustered at the group level (i.e., in case of team treatment, the pair level) and reported in parentheses. ** (*) indicates statistical significance at the 5 (10) percent level. Columns 4-6 also include indicators for missing age.

Cost per Visit

	Control Group	Individual Treatment	Team Treatment
Average visits per participant	2.13	3.82	4.16
Cost per visit	\$3.56	\$5.24	\$3.89

Table 7. Rec Center Visits for Individual and Team Treatments - Restricted Samples

	Visits (1)	≥1 Visit (2)	≥4 Visits (3)	Visits (4)	≥1 Visit (5)	≥4 Visits (6)	Sample Size
<u>Panel A</u>							364
Any treatment	1.690** (0.471)	0.161** (0.074)	0.379** (0.067)	1.806** (0.365)	0.178** (0.064)	0.395** (0.059)	
Team treatment	0.347 (0.412)	0.143** (0.059)	0.011 (0.067)	0.288 (0.340)	0.135** (0.054)	-0.014 (0.061)	
<u>Panel B: Sample restricted to actives</u>							164
Any treatment	1.759** (0.679)	0.023 (0.059)	0.409** (0.106)	1.862** (0.625)	0.031 (0.061)	0.443** (0.099)	
Team treatment	-0.883 (0.539)	-0.022 (0.050)	-0.122 (0.089)	-0.634 (0.502)	-0.020 (0.045)	-0.129 (0.092)	
<u>Panel C: Sample restricted to inactives</u>							200
Any treatment	1.782** (0.444)	0.286** (0.091)	0.372** (0.072)	1.811** (0.428)	0.308** (0.090)	0.380** (0.072)	
Team treatment	1.047** (0.486)	0.240** (0.086)	0.088 (0.088)	0.711 (0.464)	0.188** (0.084)	0.036 (0.086)	
<u>Panel D: Sample restricted to actives</u>							164
Any treatment	1.759** (0.682)	0.023 (0.059)	0.409** (0.106)	1.877** (0.628)	0.030 (0.061)	0.443** (0.099)	
Team treatment: Inactive partner	-0.887 (0.667)	-0.033 (0.063)	-0.160 (0.110)	-0.400 (0.603)	-0.029 (0.061)	-0.133 (0.110)	
Team treatment: Active partner	-0.881 (0.584)	-0.016 (0.057)	-0.099 (0.102)	-0.797 (0.530)	-0.014 (0.050)	-0.127 (0.106)	
<u>Panel E: Sample restricted to inactives</u>							200
Any treatment	1.782** (0.445)	0.286** (0.092)	0.372** (0.072)	1.840** (0.427)	0.309** (0.091)	0.390** (0.072)	
Team treatment: Inactive partner	0.818 (0.542)	0.252** (0.094)	0.002 (0.097)	0.450 (0.480)	0.183** (0.089)	-0.055 (0.089)	
Team treatment: Active partner	1.490** (0.612)	0.216** (0.106)	0.255** (0.108)	1.235* (0.651)	0.199* (0.109)	0.217* (0.115)	
Includes control variables	No	No	No	Yes	Yes	Yes	
Includes class fixed effects	No	No	No	Yes	Yes	Yes	
R-Squared	0.08	0.08	0.11	0.39	0.30	0.27	

Sample size is 364. Standard errors are clustered at the group level (i.e., in case of team treatment, the pair level) and reported in parentheses. ** (*) indicates statistical significance at the 5 (10) percent level. Columns 4-6 also include indicators for missing age.

Table 8. Rec Center Structural Estimates

	Coefficient	95% Percentile Lower Bound	95% Percentile Upper Bound
<u>Panel A - Structural parameters</u>			
Male	0.275	1.138	1.138
Age 20	-0.292	-0.796	0.205
Age 21	-0.357	-0.841	0.113
Age 22	-0.613	-1.257	0.002
Age 23+	-0.913	-1.473	-0.389
Pre-period Rec Center visits	0.293	0.1880	0.417
Predicted partner bonus status	2.168	1.403	2.789
Individual treatment indicator	1.291	0.833	1.722
Constant	-1.152	-1.676	-0.613
Theta	0.679	0.269	1.138
<u>Panel B - Theta under different probability of completion assumptions</u>			
Theta: unconditional team treatment mean	0.699	0.375	1.127
Theta: perfect foresight (true partner visits)	0.169	-0.236	0.890

1000 bootstrap replications. Sample excludes individuals with missing data for themselves or their partners. The sample size is 362.

Online Appendix Table 1. Study Room Samples 2011

Course Number and Name	Official Enrollment	Survey Respondents	Experiment Participants
Econ 101: Intermediate Macroeconomic Theory	205	40	20
Econ 106: Managerial Economics	91	32	27
Econ 114: Economic Development	85	22	13
Econ 118: Financial Accounting Analysis and Planning	58	13	10
Econ 122: Natural Resource Economics	145	34	23
Econ 130: Public Finance	54	37	22
Econ 132A: Auditing	58	42	29
Econ 136A - 1: Intermediate Accounting	59	52	34
Econ 136A - 2: Intermediate Accounting	55	40	34
Econ 136B: Intermediate Accounting	76	60	41
Econ 137A: Managerial Accounting	56	23	16
Econ 138A: Income Taxation	118	38	24
Econ 140A: Introduction to Econometrics	113	54	29
Econ 160: Economics of Crime and Justice	50	19	16
Econ 171: Game Theory	55	32	27
Econ 189 - 1: Law and Ethics	59	28	20
Econ 189 - 2: Law and Ethics	72	32	31
Psych 7: Introduction to Experimental Psychology	201	100	76
Total	1610	698	496
Removing 1 mismatched anonymous treatment group member		697	495
Removing duplicate survey respondents		687	495
Removing individuals who left experiment in the midst of treatment assignment		683	491

Online Appendix Table 2. Study Room Samples 2013

Course Number and Name	Official Enrollment	Survey Respondents	Experiment Participants
As Am 2: American Migration	165	93	62
Comm 88: Communication Research Methods	294	169	106
Econ 3A: Financial Accounting	249	94	70
Econ 3B: Financial Accounting	130	80	63
Econ 9: Introduction to Economics	219	110	74
Econ 114A: Economic Development	73	43	29
Econ 136B - 1: Intermediate Accounting	45	25	22
Econ 136B - 2: Intermediate Accounting	57	34	29
Econ 137B: Managerial Accounting	55	23	15
Econ 140A: Introduction to Econometrics	98	51	36
Econ 150A: Labor Economics	61	46	34
Globl 2: Global Socioeconomic and Political Processes	270	122	72
Total	1716	890	612
Removing duplicate survey respondents and their contaminated partners		880	602

Online Appendix Table 3. Rec Center Samples 2010

Course Number and Name	Official Enrollment	Survey Respondents	Experiment Participants
Econ 2: Principles of Macroeconomics	101	79	62
Econ 3B: Financial Accounting	119	85	66
Econ 100B: Intermediate Microeconomic Theory	83	35	21
Econ 101: Intermediate Macroeconomic Theory	79	76	58
Econ 114: Economic Development	62	47	39
Econ 118: Financial Accounting Analysis and Planning	62	37	28
Econ 136A: Intermediate Accounting	48	33	21
Econ 136B: Intermediate Accounting	55	41	30
Econ 136C: Intermediate Accounting	75	60	50
Total	684	493	375
Removing individuals and their contaminated partners who left experiment in the midst of treatment assignment		490	373
Removing duplicate survey respondents and their contaminated partners		480	364
Removing respondent who gave a fake name		479	364

Online Appendix Table 4. Study Room Visits by Treatment Status 2011

	Visits (1)	≥1 Visit (2)	≥4 Visits (3)	Visits (4)	≥1 Visit (5)	≥4 Visits (6)
Any treatment	0.886** (0.347)	0.173** (0.079)	0.216** (0.071)	0.768** (0.352)	0.149** (0.081)	0.199** (0.073)
Team treatment	0.606* (0.317)	0.125* (0.067)	0.167** (0.068)	0.740** (0.333)	0.148** (0.072)	0.186** (0.071)
Anonymous team treatment	-0.466 (0.295)	-0.079 (0.065)	-0.075 (0.064)	-0.444 (0.299)	-0.084 (0.070)	-0.079 (0.065)
Male				-0.153 (0.204)	-0.069 (0.049)	-0.044 (0.047)
Age 20				-0.382 (0.397)	-0.123 (0.092)	-0.029 (0.096)
Age 21				-0.451 (0.411)	-0.142 (0.097)	-0.017 (0.098)
Age 22				-0.792 (0.503)	-0.154 (0.115)	-0.173 (0.112)
Age 23+				-0.394 (0.494)	-0.111 (0.110)	-0.030 (0.117)
Library Days				0.164** (0.065)	0.032** (0.013)	0.012 (0.014)
Constant	1.333** (0.235)	0.400** (0.057)	0.187** (0.045)	--	--	--
Includes class fixed effects	No	No	No	Yes	Yes	Yes
R-Squared	0.06	0.05	0.08	0.12	0.11	0.13

Sample size is 491. Standard errors are clustered at the group level (i.e., in case of team treatment, the pair level) and reported in parentheses. ** (*) indicates statistical significance at the 5 (10) percent level. Columns 4-6 also include indicators for missing age and sex.

Online Appendix Table 5. Study Room Visits by Treatment Status 2013

	Visits (1)	≥1 Visit (2)	≥4 Visits (3)	Visits (4)	≥1 Visit (5)	≥4 Visits (6)
Any treatment	0.888** (0.262)	0.177** (0.067)	0.256** (0.057)	0.907** (0.266)	0.191** (0.066)	0.272** (0.058)
Team treatment	0.247 (0.250)	0.099* (0.059)	0.000 (0.058)	0.191 (0.236)	0.080 (0.056)	-0.011 (0.056)
Choice treatment	0.466* (0.253)	0.121** (0.059)	0.064 (0.058)	0.419* (0.249)	0.100* (0.059)	0.046 (0.059)
Male				-0.106 (0.188)	0.004 (0.043)	-0.012 (0.042)
Age 20				-0.101 (0.236)	-0.030 (0.058)	-0.052 (0.053)
Age 21				-0.073 (0.267)	0.039 (0.064)	-0.059 (0.066)
Age 22				-0.055 (0.348)	-0.018 (0.079)	-0.070 (0.074)
Age 23+				0.094 (0.354)	-0.024 (0.083)	-0.007 (0.085)
Library Days				0.163** (0.044)	0.033** (0.010)	0.025** (0.010)
Constant	0.882** (0.180)	0.282** (0.049)	0.129** (0.037)	--	--	--
Includes class fixed effects	No	No	No	Yes	Yes	Yes
R-Squared	0.04	0.04	0.04	0.13	0.12	0.11

Sample size is 602. Standard errors are clustered at the group level (i.e., in case of team treatment, the pair level) and reported in parentheses. ** (*) indicates statistical significance at the 5 (10) percent level. Columns 4-6 also include indicators for missing age and sex.

Online Appendix Table 6. Study Room Visits by Treatment Status, including Choice Treatment - Chose Team Treatment

	Visits (1)	≥1 Visit (2)	≥4 Visits (3)	Visits (4)	≥1 Visit (5)	≥4 Visits (6)
Any treatment	0.900** (0.212)	0.176** (0.051)	0.243** (0.045)	0.842** (0.212)	0.171** (0.051)	0.242** (0.045)
Team treatment	0.397** (0.197)	0.109** (0.044)	0.069 (0.044)	0.428** (0.193)	0.110** (0.044)	0.072 (0.044)
Anonymous team treatment	-0.579** (0.238)	-0.088 (0.055)	-0.133** (0.053)	-.613** (0.234)	-0.102* (0.055)	-0.147* (0.052)
Choice treatment - Chose IT	0.504** (0.239)	0.112** (0.055)	0.094* (0.054)	0.503** (0.237)	0.104* (0.055)	0.084 (0.055)
Choice treatment - Chose TT	1.305** (0.429)	0.422** (0.115)	0.278** (0.115)	1.470** (0.555)	0.451** (0.171)	0.304* (0.161)
Male				-0.114 (0.139)	-0.025 (0.032)	-0.022 (0.032)
Age 20				-0.112 (0.194)	-0.036 (0.047)	-0.035 (0.045)
Age 21				-0.132 (0.208)	-0.017 (0.050)	-0.032 (0.050)
Age 22				-0.326 (0.269)	-0.047 (0.062)	-0.132** (0.058)
Age 23+				-0.038 (0.274)	-0.027 (0.062)	-0.024 (0.064)
Library Days				0.161** (0.036)	0.033** (0.008)	0.020** (0.008)
Year is 2013	-0.637** (0.167)	-0.129** (0.037)	-0.114** (0.037)	--	--	--
Constant	1.432** (0.176)	0.406** (0.043)	0.217** (0.036)	--	--	--
Includes year-specific class fixed effects	No	No	No	Yes	Yes	Yes
R-Squared	0.06	0.05	0.06	0.12	0.12	0.11

Sample size is 1093. Standard errors are clustered at the group level (i.e., in case of team treatment, the pair level) and reported in parentheses. ** (*) indicates statistical significance at the 5 (10) percent level. Columns 4-6 also include indicators for missing age and sex.

Online Appendix Table 7. Study Room Structural Estimates 2011

	Coefficient	95% Percentile Lower Bound	95% Percentile Upper Bound
<u>Panel A - Structural parameters</u>			
Male	0.013	-0.316	0.314
Age 20	-0.123	-0.714	0.512
Age 21	0.171	-0.418	0.760
Age 22	-0.200	-0.902	0.560
Age 23+	0.189	-0.481	0.956
Library days			
Predicted partner bonus status	1.642	1.000	2.277
Individual treatment indicator	0.542	0.111	0.929
Constant	-0.976	-1.637	-0.389
Theta	2.030	0.967	5.805
<u>Panel B - Theta under different probability of completion assumptions</u>			
Theta: unconditional team treatment mean	1.910	1.076	4.506
Theta: perfect foresight (true partner visits)	3.282	-19.003	21.994
<u>Panel C - Anonymous treatment - theta under different probability of completion assumptions</u>			
Theta: unconditional anonymous treatment mean	0.900	-0.452	2.390
Theta: unconditional team treatment mean	0.086	-0.668	1.005

1000 bootstrap replications. The sample in Panels A and B excludes anonymous and choice treatment group members and individuals with missing data for themselves or their partners. Panels A and B sample sizes are 745. The sample in Panel C excludes team and choice treatment group members and individuals with missing data for themselves or their partners. Panel C sample size is 517.

Online Appendix Table 8. Study Room Structural Estimates 2013

	Coefficient	95% Percentile Lower Bound	95% Percentile Upper Bound
<u>Panel A - Structural parameters</u>			
Male			
Age 20	-0.143	-0.497	0.190
Age 21	-0.064	-0.435	0.300
Age 22	-0.288	-0.894	0.156
Age 23+	0.059	-0.529	0.633
Library days	0.086	0.0251	0.153
Predicted partner bonus status	1.783	1.023	2.617
Individual treatment indicator	0.690	0.329	1.134
Constant	-1.113	-1.539	-0.784
Theta	1.583	0.779	2.884
<u>Panel B - Theta under different probability of completion assumptions</u>			
Theta: unconditional team treatment mean	0.862	0.378	1.597
Theta: perfect foresight (true partner visits)	1.169	0.441	3.204

1000 bootstrap replications. The sample in Panels A and B excludes anonymous and choice treatment group members and individuals with missing data for themselves or their partners. The sample size is 418.

Online Appendix Table 9. Percent of Pairs Visiting the Study Room Together

Number of Same Time Visits	Same Time = +/- 5 Minutes		Same Time = +/- 10 Minutes		Same Time = +/- 20 Minutes		Same Time = +/- 30 Minutes	
	Actual Partner	Random Partner	Actual Partner	Random Partner	Actual Partner	Random Partner	Actual Partner	Random Partner
0	95.0	97.5	92.5	95.5	90.0	93.4	86.9	91.6
1	3.0	2.3	4.5	4.1	7.0	5.8	9.1	7.2
2	1.0	0.2	2.0	0.4	2.0	0.6	3.0	1.1
3	0.0	0.1	0.0	0.1	0.0	0.1	0.0	0.2
4+	1.0	0.0	1.0	0.0	1.0	0.0	1.0	0.0

Random pairs randomly matched within class within the team treatment (true partners are excluded). Random pairs redrawn 1000 times.

Online Appendix Table 10. Percentage Distribution of Visits by Partners - Study Room Rounds

Partner Visits	Own Visits					Total
	0	1	2	3	4+	
<u>Team treatment: True partners</u>						
0	20.6	2.0	1.5	3.3	10.8	38.2
1	2.0	0.0	0.3	0.5	2.3	5.0
2	1.5	0.3	0.0	0.0	1.8	3.5
3	3.3	0.5	0.0	0.0	3.0	6.8
4+	10.8	2.3	1.8	3.0	28.6	46.5
Total	38.2	5.0	3.5	6.8	46.5	100.0
<u>Team treatment: Random pairings</u>						
0	16.5	1.8	1.5	2.3	16.9	38.9
1	1.8	0.1	0.1	0.4	2.7	5.0
2	1.5	0.1	0.1	0.3	1.4	3.4
3	2.3	0.4	0.3	0.5	3.4	6.8
4+	16.9	2.7	1.4	3.4	21.5	45.9
Total	38.9	5.0	3.4	6.8	45.9	100.0
<u>Anonymous team treatment: True partners</u>						
0	21.0	3.7	3.7	2.5	19.8	50.6
1	3.7	0.0	0.0	0.0	2.5	6.2
2	3.7	0.0	0.0	0.6	2.5	6.8
3	2.5	0.0	0.6	0.0	0.6	3.7
4+	19.8	2.5	2.5	0.6	7.4	32.7
Total	50.6	6.2	6.8	3.7	32.7	100.0

Random pairs randomly matched within class within the team treatment (true partners are excluded). Random pairs redrawn 1000 times.

Online Appendix Table 11. Incidence of Pairs Visiting the Rec Center Together

Number of Same Time Visits	Same Time = +/- 5 Minutes		Same Time = +/- 10 Minutes		Same Time = +/- 20 Minutes		Same Time = +/- 30 Minutes	
	Actual Partner	Random Partner	Actual Partner	Random Partner	Actual Partner	Random Partner	Actual Partner	Random Partner
0	95.8	96.9	93.7	95.0	93.7	91.9	89.5	89.8
1	3.2	2.6	5.3	4.3	5.3	6.9	9.5	8.9
2	1.1	0.3	1.1	0.5	1.1	1.0	1.1	1.2
3+	0.0	0.2	0.0	0.2	0.0	0.2	0.0	0.2

Random pairs randomly matched within class within the team treatment (true partners are excluded). Random pairs redrawn 1000 times.

Online Appendix Table 12. Percentage Distribution of Visits by Partners - Rec Center Round

Partner Visits	Own Visits						Total
	0	1	2	3	4	5+	
<u>True partners</u>							
0	6.3	1.6	0.5	1.1	0.5	7.9	17.9
1	1.6	1.1	0.5	1.6	0.5	1.6	6.9
2	0.5	0.5	0.0	0.5	0.5	2.1	4.2
3	1.1	1.6	0.5	1.1	0.5	1.6	6.3
4	0.5	0.5	0.5	0.5	0.0	6.3	8.4
5+	7.9	1.6	2.1	1.6	6.3	36.8	56.3
Total	17.9	6.9	4.2	6.3	8.4	56.3	100.1
<u>Random pairings</u>							
0	3.4	1.8	0.6	1.1	1.4	9.6	8.2
1	1.8	0.5	0.3	0.4	0.5	3.5	3.3
2	0.6	0.3	0.3	0.3	0.3	2.5	1.7
3	1.1	0.4	0.3	0.3	0.7	3.6	2.7
4	1.4	0.5	0.3	0.7	0.7	5.1	8.4
5+	9.6	3.5	2.5	3.6	5.1	32.0	56.3
Total	17.8	6.9	4.2	6.3	8.4	56.3	43.6

Notes: Random pairs randomly matched within class within the team treatment (true partners are excluded).
Random pairs redrawn 1000 times.

Appendix A: Details of Experimental Design

Pay for Studying Experiment

Subjects were recruited at the beginning of 17 Economics classes and 1 Psychology class at University of California Santa Barbara (UCSB) during the fall quarter of 2011 and at the beginning of 12 undergraduate classes (9 from Economics and 1 each from Asian American Studies, Global Studies, and Communication) at UCSB during fall quarter 2013. All sign-ups for the experiment occurred about two (four) weeks into the quarter in 2011 (2013). This translates to October 3-7 in 2011 and October 17-24 in 2013. The first stage of recruitment involved asking students to fill out a brief survey at the beginning of each lecture (Appendix B contains an in-class survey).¹ Students were told that they would be entered in a draw to win \$50 if they filled out the survey.² In order to claim the \$50 they were also told that they would have to bring the bottom portion of their survey (which they were instructed to tear off and keep) and be present at the drawing that would take place outside the lecture hall after class.

All surveys had a unique identification code. From the perspective of the students this appeared to be an alphanumeric code for the lottery to be held after class. For our purposes, it was a random code that identified treatment group and, in the case of the team treatment, potential partners. Survey identification codes included an A, B, C, D, E, or F as the first character, followed by a number. The letter indicated group assignment. For example, A might indicate control group, B individual treatment, C and D anonymous treatment, and E and F team treatment. We rotated the letter-experimental group match across classes to ensure that students in subsequent classes could not successfully inform their friends about what specific letters meant. The letter codes were not explained to students prior to their arrival after class and were designed to look like a random raffle identifier, or even go unnoticed, until described after class. There is no evidence that students were able to infer their treatment status from these codes or that the codes influenced their decision to participate: There were no significant differences in participation rates between treatment groups.

Subjects were informed of their treatment status both verbally at the end of class and via email. The study room seats about 100 people, so the researchers can visibly see who arrives and leaves the room.

Assignment of Team Treatment

To facilitate rapid pairing, the in-class team treatment surveys had a built-in pairing; for example, if subjects with sign-up forms with letters C and D in their alphanumeric code, students with the same number would automatically be matched with each other. For example, if C8 and D8 both

¹ All surveys, for all treatment groups, came from a randomized pile. This ensured that subjects did not know with whom they were matched until after lecture, and that subjects were not sitting near their potential partner (except by random chance).

² Due to larger class sizes in 2013, the number of \$50 winners depended on class size; it ranged from 1-4 drawings.

show up after class, they would be matched together. Subjects with a “partner” who did not show up after lecture were randomly re-matched with another subject without a “partner.”³

B. Experimental Design, Pay for Exercise

Subjects were recruited during and after lectures in all nine Economics classes at University of California Santa Barbara (UCSB), during the second six-week summer session in 2010. All sign-ups for the experiment occurred during week one (August 3-6). The sign-up process only differed slightly from that for the library experiment.

Assignment of Team Treatment

Subjects with the same alphanumeric code were matched when possible (e.g. C1 and C1*), with random re-matching when the potential partner did not show up outside.

Subjects were informed that payments would be made in week five of the six-week session. Just as in the library study, we wanted to ensure that subjects in the team treatment knew that they would potentially see their partners after the partner knew if the bonus threshold was reached.

C. Measuring Visits

One benefit of the library experimental design is that it allows for study room visits to be supervised by a researcher or research assistant. Logs were kept every day to determine who studied at the study room each day. Identities were checked by photo identification at check-in. When a subject asked if another subject had visited the study room, the person with access to the daily log would deny the subject’s request.

In the case of Rec Center attendance, we used electronic collection. Whenever anyone wishes to enter the Rec Center the attendant at the front desk takes her or his student photo ID card and electronically scans it. The time, date, and student card barcode of every gym entry is stored electronically. The Rec Center generously provided us with data that included all gym visits for every in-class survey respondent from July 21 through August 20, 2010. Because the Rec Center has the universe of student names and identification numbers they also verified for us that every student who filled out an in-class survey was in their database. In other words, there are no cases in which we are confounding non-attendance with an incorrect name and/or student identification number.

³ This was done by matching in ascending sequential order. If C10 showed up but not D10 and the next unmatched group treatment number was C12, we matched C10 and C12. This preserves randomization since surveys were distributed randomly in class. In the few circumstances in which this process left a group treatment participant without a partner, we randomly selected a control group member to pair with her/him.

Appendix B

Survey and Consent Forms

Pay-For-Study - 2011

1. In-Class Consent Form and Survey
2. Participant Consent Forms
 - a. Control Group
 - b. Individual Treatment
 - c. Team Treatment
 - d. Anonymous Treatment

Pay-For-Study - 2013

1. In-Class Consent Form and Survey
2. Participant Consent Forms
 - a. Control Group
 - b. Individual Treatment
 - c. Team Treatment
 - d. Choice Treatment

Pay-For-Exercise – Summer 2010

1. In-Class Consent Form and Survey
2. Participant Consent Forms
 - a. Control Group
 - b. Individual Treatment
 - c. Team Treatment

Hi, you are being asked to participate in a study by Philip Babcock, Kelly Bedard, Gary Charness, John Hartman, and Heather Royer. You must be at least 18 years old to participate. For your participation today, we will enter you in a random drawing, in which one person in this class will receive \$50 cash today (photo ID required).

We are conducting a study to analyze monetary incentives to study. By signing up for this experiment, you are acknowledging that the authors of this study will follow your academic records at UCSB from the beginning of your enrollment through Summer 2012. By participating in the study, you will be given monetary incentives for studying at a given location at UCSB. In some cases, monetary incentives will depend solely on your attendance. In other cases, the monetary incentives will depend partially on your attendance and partially on the attendance of you and one other person. In this case, you may or may not be notified of the identity of your partner.

I am aware that in this study, I allow Philip Babcock, Kelly Bedard, Gary Charness, John Hartman, Heather Royer, and research assistants related to this study, to access my academic records at UCSB through Summer 2012. I am aware that my UCSB student number will be used to link survey and study data. It will not be used for any other purpose nor will any information ever be made public. That being said, absolute confidentiality cannot be guaranteed, since research documents are not protected from subpoena.

We would also like to ask you a few questions:

What is your sex? M F

How old are you? 18 19 20 21 22 23 24 25 other_____

(indicate age)

Over the last 7 days, how many HOURS did you study in TOTAL (outside of class time)? _____

(indicate hours)

Over the last 7 days, how many DAYS did you study for 40 minutes or more ANYWHERE in the library?

0 1 2 3 4 5 6 7

Over the last 7 days, how many DAYS did you study for 40 minutes or more in the 24-HOUR STUDY ROOM in the library? The 24-hour study room is located near the main entrance to the library.

0 1 2 3 4 5 6 7

Please state whether you agree or disagree with the following statement:

"I encounter difficulties motivating myself to study." (Circle one of the answers below)

--	--	--	--	--	--	--

Strongly
Disagree

Disagree

Somewhat
Disagree

Neutral

Somewhat
Agree

Agree

Strongly
Agree

Print name

Signature

October _____, 2011
Date

Perm #

Primary e-mail address

Local phone number

Print last name

first name

You have been selected to earn additional money for attendance at the 24-hour study room in the library. You will receive \$2 per study visit for up to 4 visits in the specified 2-week treatment period. Only one visit per day is eligible for payment. In order for a study visit to count, students must log in with a researcher posted in the 24-hour study room and remain in the study room for at least 40 minutes before logging out with the researcher. We will man the 24-hour study room from 11:00am to 7:00pm Monday through Friday from October 10-21, 2011. Students must log in by 6:20pm for the visit to qualify.

We will pay you for qualifying study room visits in approximately four weeks. You will receive an e-mail in about three weeks with more information.

Your participation is voluntary. There will be no repercussions should you decide not to participate. Please note that you may withdraw your participation at any time, and you will be paid based on your attendance at the study room up to the point that you withdraw from participating. If you have any questions, you may contact Philip Babcock at babcock@econ.ucsb.edu or 805-893-4823, or John Hartman at hartman@econ.ucsb.edu.

If you have any questions concerning any matter relating to your participation, you may also call the University of California Santa Barbara Human Subjects committee at 805-893-3807.

By signing below, I acknowledge the above information.

Signature_____ Print name_____

You have been selected to earn additional money for attendance at the 24-hour study room in the library. You will receive \$2 per study visit for up to 4 visits in the specified 2-week treatment period. Only one visit per day is eligible for payment. In order for a study visit to count, students must log in with a researcher posted in the 24-hour study room and remain in the study room for at least 40 minutes before logging out with the researcher. We will man the 24-hour study room from 11:00am to 7:00pm Monday through Friday from October 10-21, 2011. Students must log in by 6:20pm for the visit to qualify.

If you have qualifying visits on at least 4 different days from October 10-21 you will earn an additional \$25.

We will pay you for qualifying study room visits in approximately four weeks. You will receive an e-mail in about three weeks with more information.

Your participation is voluntary. There will be no repercussions should you decide not to participate. Please note that you may withdraw your participation at any time, and you will be paid based on your attendance at the study room up to the point that you withdraw from participating. If you have any questions, you may contact Philip Babcock at babcock@econ.ucsb.edu or 805-893-4823, or John Hartman at hartman@econ.ucsb.edu.

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By signing below, I acknowledge the above information.

Signature_____ Print name_____

You have been selected to earn additional money for attendance at the 24-hour study room in the library. You will receive \$2 per study visit for up to 4 visits in the specified 2-week treatment period. Only one visit per day is eligible for payment. In order for a study visit to count, students must log in with a researcher posted in the 24-hour study room and remain in the study room for at least 40 minutes before logging out with the researcher. We will man the 24-hour study room from 11:00am to 7:00pm Monday through Friday from October 10-21, 2011. Students must log in by 6:20pm for the visit to qualify.

You have also been matched with another person for this part of the study. If both of you have qualifying visits on at least 4 different days from October 10-21 you will both earn an additional \$25. Note that if either one of you does not meet this requirement, the \$50 that you could have collectively earned is lost.

We will pay you for qualifying study room visits in approximately four weeks. You will receive an e-mail in about three weeks with more information.

Your participation is voluntary. There will be no repercussions should you decide not to participate. Please note that you may withdraw your participation at any time, and you will be paid based on your attendance at the study room up to the point that you withdraw from participating. If you have any questions, you may contact Philip Babcock at babcock@econ.ucsb.edu or 805-893-4823, or John Hartman at hartman@econ.ucsb.edu.

If you have any questions concerning any matter relating to your participation, you may also call the University of California Santa Barbara Human Subjects committee at 805-893-3807.

By signing below, I acknowledge the above information.

Partner's name _____

My Signature _____

Print my name _____

My number (e.g. B6) _____

Partner's number _____

.....

Partner's name _____

You have been selected to earn additional money for attendance at the 24-hour study room in the library. You will receive \$2 per study visit for up to 4 visits in the specified 2-week treatment period. Only one visit per day is eligible for payment. In order for a study visit to count, students must log in with a researcher posted in the 24-hour study room and remain in the study room for at least 40 minutes before logging out with the researcher. We will man the 24-hour study room from 11:00am to 7:00pm Monday through Friday from October 10-21, 2011. Students must log in by 6:20pm for the visit to qualify.

You will be matched with another person from another class for this part of the study. The identity of your partner will not be revealed to you or your partner – it is entirely anonymous. If both of you have qualifying visits on at least 4 different days from October 10-21 you will both earn an additional \$25. Note that if either one of you does not meet this requirement, the \$50 that you could have collectively earned is lost.

We will pay you for qualifying study room visits in approximately four weeks. You will receive an e-mail in about three weeks with more information.

Your participation is voluntary. There will be no repercussions should you decide not to participate. Please note that you may withdraw your participation at any time, and you will be paid based on your attendance at the study room up to the point that you withdraw from participating. If you have any questions, you may contact Philip Babcock at babcock@econ.ucsb.edu or 805-893-4823, or John Hartman at hartman@econ.ucsb.edu.

If you have any questions concerning any matter relating to your participation, you may also call the University of California Santa Barbara Human Subjects committee at 805-893-3807.

By signing below, I acknowledge the above information.

Signature_____ Print name_____

Hi, you are being asked to participate in a study by Kelly Bedard, Gary Charness, John Hartman, and Heather Royer. You must be at least 18 years old to participate. For your participation today, we will enter you in a random drawing, in which one person in this class will receive \$50 cash today (photo ID required).

We are conducting a study to analyze monetary incentives to study. By participating in the study, you will be given monetary incentives for studying at a specific location at UCSB. In some cases, monetary incentives will depend solely on your attendance. In other cases, the monetary incentives will depend partially on your attendance and partially on the attendance of you and one other person.

I am aware that in this study, I allow Kelly Bedard, Gary Charness, John Hartman, Heather Royer, and research assistants related to this study, to collect information about my participation in this study. I am aware that my UCSB student number will be used to link survey and study data. It will not be used for any other purpose nor will any identifiable information ever be made public. That being said, absolute confidentiality cannot be guaranteed, since research documents are not protected from subpoena.

We would also like to ask you a few questions:

What is your sex? M F

How old are you? 18 19 20 21 22 23 24 25 other_____ (indicate age)

Are you Hispanic/Latino? Yes No

What is your race? White Black Asian Other

Over the last 7 days, how many HOURS did you study in TOTAL (outside of class time)? _____ (indicate hours)

Over the last 7 days, how many DAYS did you study for 40 minutes or more in the library?

0 1 2 3 4 5 6 7

Please state whether you agree or disagree with the following statement:

"I encounter difficulties motivating myself to study." (Circle one of the answers below)

Strongly Disagree	Disagree	Somewhat Disagree	Neutral	Somewhat Agree	Agree	Strongly Agree

_____	_____	October _____, 2013	_____
Print name	Signature	Date	Perm #

_____	_____
Primary e-mail address	Local phone number

.....

_____	_____
Print last name	first name

You have been selected to earn additional money for attendance at the “experimental study area” in the library. The experimental study area will be located on the second floor of the library near the elevators. You will receive \$2 per study visit for up to 4 visits in the specified 2-week treatment period. Only one visit per day is eligible for payment. In order for a study visit to count, students must log in with a researcher posted in the experimental study area and remain in the area for at least 40 minutes before logging out with the researcher. We will man the experimental study area from 11:00am to 7:00pm Monday through Friday from October 28 – November 8, 2013. Students must log in by 6:20pm for the visit to qualify.

We will pay you for qualifying study area visits in approximately four weeks. You will receive an e-mail in about three weeks with more information.

Your participation is voluntary. There will be no repercussions should you decide not to participate. Please note that you may withdraw your participation at any time, and you will be paid based on your attendance at the study room up to the point that you withdraw from participating. If you have any questions, you may contact Kelly Bedard at (805) 893-5571 or kelly@econ.ucsb.edu. If you have any questions regarding your rights and participation as a research subject, please contact the Human Subjects Committee at (805) 893-3807 or hsc@research.ucsb.edu. Or write to the University of California, Human Subjects Committee, Office of Research, Santa Barbara, CA 93106-2050.

By signing below, I acknowledge the above information.

Signature_____ Print name_____

You have been selected to earn additional money for attendance at the “experimental study area” in the library. The experimental study area will be located on the second floor of the library near the elevators. You will receive \$2 per study visit for up to 4 visits in the specified 2-week treatment period. Only one visit per day is eligible for payment. In order for a study visit to count, students must log in with a researcher posted in the experimental study area and remain in the study area for at least 40 minutes before logging out with the researcher. We will man the experimental study area from 11:00am to 7:00pm Monday through Friday from October 28 – November 8, 2013. Students must log in by 6:20pm for the visit to qualify.

If you have qualifying visits on at least 4 different days from October 28 – November 8 you will earn an additional \$25.

We will pay you for qualifying study area visits in approximately four weeks. You will receive an e-mail in about three weeks with more information.

Your participation is voluntary. There will be no repercussions should you decide not to participate. Please note that you may withdraw your participation at any time, and you will be paid based on your attendance at the study room up to the point that you withdraw from participating. If you have any questions, you may contact Kelly Bedard at (805) 893-5571 or kelly@econ.ucsb.edu. If you have any questions regarding your rights and participation as a research subject, please contact the Human Subjects Committee at (805) 893-3807 or hsc@research.ucsb.edu. Or write to the University of California, Human Subjects Committee, Office of Research, Santa Barbara, CA 93106-2050.

By signing below, I acknowledge the above information.

Signature_____ Print name_____

You have been selected to earn additional money for attendance at the “experimental study area” in the library. The experimental study area will be located on the second floor of the library near the elevators. You will receive \$2 per study visit for up to 4 visits in the specified 2-week treatment period. Only one visit per day is eligible for payment. In order for a study visit to count, students must log in with a researcher posted in the experimental study area and remain in the study area for at least 40 minutes before logging out with the researcher. We will man the experimental study area from 11:00am to 7:00pm Monday through Friday from October 28 – November 8, 2013. Students must log in by 6:20pm for the visit to qualify.

You have also been matched with another person for this part of the study. If both of you have qualifying visits on at least 4 different days from October 28 – November 8 you will both earn an additional \$25. Note that if either one of you does not meet this requirement, the \$50 that you could have collectively earned is lost.

We will pay you for qualifying study area visits in approximately four weeks. You will receive an e-mail in about three weeks with more information.

Your participation is voluntary. There will be no repercussions should you decide not to participate. Please note that you may withdraw your participation at any time, and you will be paid based on your attendance at the study room up to the point that you withdraw from participating. If you have any questions, you may contact Kelly Bedard at (805) 893-5571 or kelly@econ.ucsb.edu. If you have any questions regarding your rights and participation as a research subject, please contact the Human Subjects Committee at (805) 893-3807 or hsc@research.ucsb.edu. Or write to the University of California, Human Subjects Committee, Office of Research, Santa Barbara, CA 93106-2050.

By signing below, I acknowledge the above information.

Partner’s name _____

My Signature_____

Print my name_____

My number (e.g. B6) _____

Partner’s number _____

.....

Partner’s name _____

You have been selected to earn additional money for attendance at the “experimental study area” in the library. The experimental study area will be located on the second floor of the library near the elevators. You will receive \$2 per study visit for up to 4 visits in the specified 2-week treatment period. Only one visit per day is eligible for payment. In order for a study visit to count, students must log in with a researcher posted in the experimental study area and remain in the study area for at least 40 minutes before logging out with the researcher. We will man the experimental study area from 11:00am to 7:00pm Monday through Friday from October 28 – November 8, 2013. Students must log in by 6:20pm for the visit to qualify.

You have also been selected to be given the opportunity to choose between two options for earning an additional \$25:

- (1) If you (on your own) have qualifying visits on at least 4 different days from October 28 – November 8 you will earn an additional \$25.
- (2) You will be matched with another person from this class, but not from this group. If both of you have qualifying visits on at least 4 different days from October 28 – November 8 you will both earn an additional \$25. Note that if either one of you does not meet this requirement, the \$50 that you could have collectively earned is lost. Also note that you will not learn the identity of your partner until after choosing between options (1) and (2), and you will only be matched if you choose option (2).

We will pay you for qualifying study area visits in approximately four weeks. You will receive an e-mail in about three weeks with more information.

Your participation is voluntary. There will be no repercussions should you decide not to participate. Please note that you may withdraw your participation at any time, and you will be paid based on your attendance at the study room up to the point that you withdraw from participating. If you have any questions, you may contact Kelly Bedard at (805) 893-5571 or kelly@econ.ucsb.edu. If you have any questions regarding your rights and participation as a research subject, please contact the Human Subjects Committee at (805) 893-3807 or hsc@research.ucsb.edu. Or write to the University of California, Human Subjects Committee, Office of Research, Santa Barbara, CA 93106-2050.

Please mark your choice with and X: I choose option (1) _____

I choose option (2) _____

By signing below, I acknowledge the above information.

Signature _____ Print name _____

Hi, you are being asked to participate in a study by Philip Babcock, Kelly Bedard, Gary Charness, John Hartman, and Heather Royer. You must be at least 18 years old to participate. For your participation today, we will enter you in a random drawing, in which one person in this class will receive \$50 cash today (subject to presentation of photo ID).

We are conducting a study to analyze monetary incentives to exercise. By signing up for this experiment, you are acknowledging that the authors of this study will follow your attendance at the UCSB Recreation Center ("Rec Center") for June through September 2010. By participating in the study, you may be randomly selected to earn money for attending the Rec Center. In some cases, the monetary incentives will depend solely on your attendance. In other cases, the monetary incentives will depend partially on your attendance and partially on the attendance of you and one other person (whom you will be notified about if you are selected).

I am aware that in this study, I allow Philip Babcock, Kelly Bedard, Gary Charness, John Hartman, Heather Royer, and research assistants related to this study, to access my attendance records at the UCSB Recreation Center for June to September 2010.

I also acknowledge the following information: Exercise has potential risks and benefits. Before starting any exercise program, you may want to consider contacting a doctor or other professional qualified to help determine what types of exercise are appropriate for you. When exercise is tailored to your physical condition and health, the gains from exercise usually outweigh the costs. Please also note that pregnancy may complicate the type and amount of exercise that you need. If you are pregnant or plan on becoming pregnant in the next six weeks, or if you are 17 years old or younger, you are not allowed to participate in this study.

After making payment to participants, all identifiers will be immediately removed from the data. The anonymized attendance records will be kept in a locked drawer in the office of Gary Charness.

We would also like to ask you a few questions:

What is your sex? M F

How old are you? 18 19 20 21 22 23 24 25 other_____

In the last month, how many times per week did you moderately or vigorously exercise for 30 minutes or more?

0 less than 1 1 2 3 4 5 6 7 more than 7

Print name

Signature

August _____, 2010
Date

Perm #

Primary e-mail address

Local phone number

You have been selected to receive information on the benefits of exercise.

Exercise has potential risks and benefits. Before starting any exercise program, you may want to consider contacting a doctor or other professional qualified to help determine what types of exercise are appropriate for you. When exercise is tailored to your physical condition and health, the gains from exercise usually outweigh the costs. Please also note that pregnancy may complicate the type and amount of exercise that you need.

If you have any questions, you may contact Philip Babcock at babcock@econ.ucsb.edu or 805-893-4823, or John Hartman at hartman@econ.ucsb.edu.

If you have any questions concerning any matter relating to your participation, you may also call the University of California Santa Barbara Human Subjects committee at 805-893-3807.

The University of California does not provide compensation for injury to human subjects of research except that the University will provide for any medical care required to treat any injury resulting from participation as a human subject in a University-approved activity. If you have any questions concerning this or any other matter relating to your participation in this activity, please call 893-3807.

By signing below, I acknowledge the above information. I will also do the following immediately if I become pregnant or suspect that I am pregnant:

- Stop attending the UCSB Recreation Center.
- Notify one of the researchers listed above.

Signature_____

Print name_____

You have been selected to earn additional money from attendance at the UCSB Recreation Center (“Rec Center”). From August 7-20, 2010, you will earn \$2 for exercising at the Rec Center on any of these dates, up to \$10. If you attend the Rec Center at least five different days from August 7-20, 2010, you will earn an additional \$25.

We will pay you for qualifying Rec Center visits in approximately four weeks. You will receive an e-mail in about three weeks with more information.

Recall the following information that you acknowledged earlier today: Exercise has potential risks and benefits. Before starting any exercise program, you may want to consider contacting a doctor or other professional qualified to help determine what types of exercise are appropriate for you. When exercise is tailored to your physical condition and health, the gains from exercise usually outweigh the costs. Please also note that pregnancy may complicate the type and amount of exercise that you need. If you are pregnant or plan on becoming pregnant in the next six weeks you are not allowed to participate in this exercise study.

Your exercise participation is voluntary. There will be no repercussions should you decide not to participate. Please note that you may withdraw your participation at any time, and you will be paid based on your attendance at the Rec Center up to the point that you withdraw from participating. If you have any questions, you may contact Philip Babcock at babcock@econ.ucsb.edu or 805-893-4823, or John Hartman at hartman@econ.ucsb.edu.

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- Stop attending the UCSB Recreation Center.
- Notify one of the researchers listed above.

Partner’s name _____

Signature _____ Print name _____

.....

Partner’s name _____