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Social Effects of Team Incentives

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Abstract: This paper estimates social effects of incentivizing people in teams. In two field experiments featuring exogenous team formation and opportunities for repeated social interactions, we find large team effects that operate through social channels. The team compensation system induced agents to choose effort as if they valued a marginal dollar of compensation for their teammate from two-thirds as much (in one study) to twice as much as they valued a dollar of their own compensation (in the other study). We conclude that social effects of monetary team incentives exist and can induce effort at lower cost than through direct individual payment.

JEL Classifications: B49, C93, J01, J33

Keywords: Field experiment, team incentives, social effects

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1. Introduction

The interest in incentives to elicit effort or alter behavior is pervasive and growing. In the context of schooling, Angrist, Lang, and Oreopoulos (2009), Kremer, Miguel, and Thornton (2009), Barrow et al. (2012), Bettinger (2010), and Fryer (2010) study how incentives affect students' performance. Charness and Gneezy (2009), Volpp et al (2009), Ackland and Levy (2011), John et al (2011), Royer, Stehr, and Sydnor (2011), and Babcock and Hartman (2012), show that financial incentives can promote healthy behaviors such as exercise, weight loss, and smoking cessation. In many cases, the effectiveness of such incentives, however, has been questioned. In a review article, Gneezy, Meier, and Rey-Biel (2011) argue that the experimental results for educational incentives have been "somewhat disappointing." Similarly, for healthy behaviors, incentives are often large (e.g., $750 for smoking cessation in Volpp et al (2009)) with limited improvements in behavior.

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 (Sacerdote, 2001, Zimmerman 2003, Bandiera, Barankay, and Rasul, 2005, 2010, forthcoming, 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, Carrell, Hoekstra, and West, 2011). 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. The popular press is full of accounts of harnessing the power of a team.[1] In real-world environments such as firms, the military, and health and wellness programs, it is not uncommon to see team-based incentives. A best-selling management consultant goes

[1] 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/.

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."[2]

Despite the prevalence of this perception, there is a scarcity of empirical research on group incentive effects of this kind, research connecting the pay-for-behavior literature with the literature on peer effects. We study the effect of team-based incentives in two separate randomized field experiments, one featuring pay-for-studying, which incentivized attendance at a study hall in the library and the other featuring pay-for-exercise, which incentivized gym attendance in a university setting. These are two settings in which the power of peers has been emphasized,[3] each providing a controlled environment in which to learn about social effects related to effort elicitation. Given concerns about poor education outcomes and rising obesity, we consider both these outcomes, in and of themselves, to be of first-order importance. Evidence shows that study times among college students are declining steadily (Babcock and Marks, 2011) and rates of inactivity among adults are nearly fifty percent.[4] We focus on inputs (e.g., studying and exercising) rather than outputs (e.g., grades or weight loss), in line with the Fryer (2010) argument that incentivizing inputs is more effective than incentivizing outputs because individuals may be unclear about the production function.

Our study is a rare combination of the positive elements of laboratory experiments, where the environment can be carefully manipulated, and field experiments, which more closely resemble reality. In both settings, subjects were randomly assigned either to a) a control group, which received either no incentives or minimal incentives,[5] 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

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[2] Lencioni (2002), p. 213

[3] 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.

[4] See http://www.cdc.gov/nchs/data/hus/hus10.pdf#070.

[5] In the case of studying, we incentivized attendance to a study hall in the library. Since we would not be able to monitor the attendance of students who did not receive incentives, our control group for the study experiment received a small monetary incentive for attendance.

incentives as the individual incentive group but whose payment was partially contingent on the behaviors of a randomly-assigned and known teammate. In the pay-for-studying experiment, we also had an additional treatment, the anonymous treatment, which was identical to c), except that the teammate was unknown. Within the incentive groups, all individuals received small per-visit incentives with a much larger bonus payment contingent on attending at least a specified number of times. To create the team incentive, the awarding of the bonus was dependent on both the individual and his/her partner attaining the threshold number of visits in the case of the team treatments.

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, fear of social punishment, or a desire to be liked or respected*. Thus, these effects exclude specialization in production or knowledge transfers, even though these, too, could be viewed as types of peer effects. It has been theorized that social pressures are an important factor in the design of incentives to elicit effort. 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. It is effects of this kind, rarely analyzed explicitly in environments with team-based compensation, that are of interest in this paper.[6]

There are several key features of the experimental design that enable us to uniquely isolate the social effects. First, the tasks involve minimal production complementarities. In most research, the possibility of production complementarities is significant. For example, Hamilton, Nickerson, and Owan (2003), find that worker productivity rose at a garment plant with the introduction of team incentives. But it is not clear whether these gains were due to increased effort that resulted from social pressure or from complementarities in production among workers that involved specialization, knowledge transfer, and

[6] Bandiera, Barankay, and Rasul (forthcoming), a notable exception, study performance under different team compensation schemes (rank and tournament incentives) allowing for endogenous team formation. Though they are able to study endogenous team formation, involving friendships and other social factors, they are not able to compare individual-based incentive schemes to those that are team-based, and so do not estimate social effects.

other factors directly related to the production process. Second, the team incentive structure is such that a subject cannot rely exclusively on his/her partner's effort to earn rewards: If anyone defaults, no one earns the bonus.[7] While this may seem less generalizable to real-world settings that feature payment for group performance, we do observe this kind of incentive structure outside of academic settings. 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.[8] Moreover, since the goal of our experiment is to understand the social effects of incentives, closing off the channel for this type of free-riding is desirable. Third, since subjects in the team treatment belong to the same class in the middle of the term, the setting facilitates social interaction between teammates over an extended period of time—hard to accomplish in the laboratory, where one-time sessions predominate.

These features of the experiment imply that 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 the reverse is true, we interpret this as evidence of the existence of social effects. Using a structural model, we can leverage differences across the different treatments to estimate the size of the social effect.

To our surprise, we find in the pay-for-study intervention that individuals assigned to the team treatment frequented the study room roughly twice as often as individuals assigned to the individual treatment. Individuals assigned to the anonymous team treatment performed only about as well as the individual treatment, suggesting that the knowledge of your partner has important effects. We estimate similar but slightly more nuanced effects for the gym study. Our findings are all the more unanticipated given the claim of earlier literature that production complementarities are absolutely necessary for team incentives to be effective, and that arbitrarily assigned or "artificial" teams do not produce positive results (Lazear 2000). Results from our structural model suggest that the pay-for-study experiment induced

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

[8] 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).

agents to choose their effort as if they valued a marginal dollar of compensation for their teammate more than twice as much as they valued a dollar of their own compensation, while in the pay-for-exercise experiment, agents chose effort as if they valued a marginal dollar for their teammate two thirds as much as a dollar of their own compensation.

We highlight two central implications of these findings. Firstly, social effects of team compensation can be decisive in inducing agents to accomplish effort-intensive tasks. Secondly, team compensation schemes can be designed that elicit effort at much lower cost per unit than direct individual payment, even in the absence of production complementarities. As in any field experiment, it is not clear how well the findings generalize beyond the two environments studied. However, the findings here—robust across two settings and based on tasks that institutions and policymakers have attempted to incentivize in the past—raise the intriguing possibility that there may be large returns to harnessing the power of the group.

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. Given a link between these behaviors and educational outcomes and obesity, it is possible that more intensive versions of the intervention studied here could lead to improvements in these long-run outcomes. Stated somewhat differently, now that we have provided two examples showing that team incentives can be effective, it would be interesting to look at the effects on obesity and academic outcomes of a longer-term intervention.

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 the task, and C is the effort cost of completing the task. 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[9] 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 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: $V_i - C_i + p_j B + p_j \theta B > 0$.

We note first that the incentive structure in the team treatment does not allow for subjects to earn the bonus by depending solely on partner effort. If person i does not put forth effort, neither she nor her partner receives payment. Secondly, in both experiments, the task in question did not allow in any direct way for knowledge transfers. (All subjects were told where the library was or where the gym was—which, of course, nearly everyone already knew.) We will also show that there is little evidence of subjects harnessing any other possible production complementarities related to teamwork (e.g., studying more effectively by studying together, exercising more effectively by going to the gym together): Pairs of

[9] We all assume all individuals receive the same benefit.

teammates in the team treatment made simultaneous library (or gym) visits only slightly more often than randomly-paired subjects.[10]

Thirdly, 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, their 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. But simply gathering information about beliefs could change the nature of the intervention, so we avoided collecting such information. Inquiring about a subject's beliefs about his partner may lead the subject to change beliefs about the partner's future actions. Fourthly, we do not allow for strategic behavior in 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 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 as or more effectively for the team treatment than for 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 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.

[10]This also suggests that teamed subjects do not go to the library or gym together as a commitment mechanism to help them overcome time-inconsistency in their preferences. See the discussion in section 5.

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 at University of California-Santa Barbara. We summarize the experiment design here and relegate further details to the appendix. 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, the informing of participants of their treatment status outside of the classroom.

At the beginning of class, we had students 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 most students were not cognizant of the presence of the identifier.[11] At the end of class, students were notified of their treatments. 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 earn a $50 raffle. Participant rates were high (over 70 percent).[12] Participant rates did not differ across treatment status; p-values of differences always exceed 0.25. For descriptive ease, we refer to the students who decide to participate in the after-class part of the experiment as participants throughout the paper. These participants form our main estimation sample.

Unlike many settings, here we are able to understand the degree of selectiveness of our sample because of the setup of the experiment. In the first stage of the experiment, we gathered the distribution of characteristics of the intended target population (i.e., students in the class[13]), and thus, we can see how the intended target population differs from the students who partake in the experiment.

Participants were incentivized to attend the 24-hour study room in the UCSB library over a two-week period at the beginning of the quarter. We required students to attend at least 40 consecutive

[11] We varied the scheme mapping the identifier to treatment assignment across classes as described in the appendix.
[12] Participation rates are defined as the fraction of students filling out the survey at the beginning of class showing up after class to participate in the experiment. Participation rates for the control, individual, team, and anonymous treatments were 67, 74, 74 and 69 percent, respectively.
[13] Of course, we will miss collecting data for those who do not attend class. But any classroom-based program is likely to miss those persons too.

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 one visit in a day. The treatment groups and control group were as follows:

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

<u>Individual Treatment</u>: 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).

<u>Team Treatment</u>: Subjects were eligible to earn \$2 per visit (up to 4 visits), but also received an additional bonus of \$25 if and only if <u>both team members</u> 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 hall at the same time. As it was 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.

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

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 vs \$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 hall and 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 at the beginning of the quarter, the experiment was designed to allow for repeated interaction. To ensure treatment salience, subjects were reminded 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 a summer session in 2010. The sign-up process was similar to that for the library experiment except that there was no anonymous team treatment. All details are the same as in the library experiment, except as stated below.

In this part of the experiment, subjects were incentivized to attend the UCSB Recreation Center ("Rec Center") at the beginning of the summer session during a two-week period. 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. Additionally, subjects were randomly assigned to one of the following groups:

<u>Control</u>: 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 five or more eligible visits.

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 the information. Just as in the study experiment, there was no significant difference in participation rates between treatment groups. Participation rates for the control, individual, and team treatments were 79, 75, and 76 percent, respectively.

D. Survey Response and Experiment Participants

Table 1 reports the distribution of enrollment sizes, the number of in-class surveys collected, and the number of experiment participants (students who came outside after class and were assigned to a treatment or control group) for the library experiment. In terms of participation in the experiment, conditional on completing a survey, 71 percent of survey participants stayed after class for the lottery and were assigned to a treatment or control group. This attrition rate is not large when gauged against other field experiments (e.g., Card, Mas, Moretti, and Saez, 2010). 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.[14] The last row of Table 1 reports the survey and participant sample sizes used in all analyses.[15]

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[14] Students were told not to sign-up more than once. Since some students enroll in multiple Economics classes simultaneously each quarter, this lowered the participation rate in some classes. For example, the last four Economics classes (122, 118, 106, and 114) we signed up each had lower-than-average participation rates.

[15] The small number of sample exclusions stem from three possible reasons: ten subjects filled out surveys in two different classes, four people who came to the after-class treatment assignment left after being assigned a partner but

Table 2 shows the analogous table for the Rec Center experiment. Again, participation in the experiment given completion of the in-class survey is high at 76 percent. Overall participation rates (the fraction of the enrolled students partaking in our study) is higher here likely for two reasons: the recruitment was done earlier in the term and there are fewer students taking multiple classes in our experimental pool. Only a few observations are excluded from the final analysis sample.[16]

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 finish with the results for the pay-for-exercise experiment.

A. Descriptive Statistics

Panel A of Table 3 shows sample means of descriptive characteristics (gender, age, and pre-treatment library usage) by treatment status for all subjects who completed the in-class survey in the pay-for-studying experiment. For this sample, there were no statistically significant differences across groups in these characteristics as seen in the p-values for mean differences. Panel B reports the same comparisons for participants (i.e., those who showed up after class) and non-participants (i.e., those who did not show up after class). The only statistically significant difference is in age: Older students were somewhat less likely to participate. Inferences will be based on the sample of participants. We draw no conclusions about the 28 percent of in class-responders who were non-participants and who appear to have been somewhat older, on average. Panel C shows sample means of descriptive characteristics by treatment status for the 491 subjects who participated in the experiment. Age, gender, and pre-treatment library usage do not differ significantly between group treatment, individual treatment, anonymous treatment, and control groups, as indicated by the p-values at the bottom of the table.

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before signing the informational treatment sheet given to subjects, and one subjects is excluded from the anonymous treatment because we had an odd number of people assigned to that treatment.

[16] About three percent of subjects were excluded due to leaving at the beginning of phase 2 of the experiment when students showed up after class, signing up in multiple classes, giving a fake name on the initial survey, or having a partner excluded for one of the reasons just mentioned. The partner exclusion includes one subject that was excluded for being matched with a person whose partner had left the experiment the beginning of phase 2.

B. Results

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 different days during the treatment period (which is the threshold for receiving the $25 bonus). These outcomes are designated "Visits," "Try," and "Bonus," respectively. Table 4 displays means of these outcomes for the different treatment groups. Control subjects in the sample visited the study room 1.3 times on average during the treatment period. 40 percent of the control subjects showed up to the study room at least once, and 19 percent reached the 4-visit bonus threshold. The individual treatment estimates in Table 4 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 17 percentage points more likely to have gone to the study room at least once, and were more than twice as likely (21 percentage points) to have met the 4-visit bonus payment threshold. All these differences are statistically significant at the 5 percent level as indicated by the p-values at the bottom of the table. It is clear that subjects eligible for a bonus for study room visits visited more often.

The more striking finding is for the difference between individual and team treatments. Participants randomized into the team-incentive scheme made about 0.6 more visits to the study room during the treatment period, were 13 percentage points more likely to have gone to the study room at least once, and were 17 percentage points more likely to have met the 4-visit threshold than those in the individual treatment, with all differences being statistically significant at the 5 percent level. The team incentive elicited higher effort on all margins.

Interestingly, subjects paired with an anonymous partner put forth more effort than controls (1.8 visits versus 1.3 for the controls), but the anonymous treatment was somewhat less effective than the individual treatment (2.2 visits), and much less effective than the team treatment in which subjects met their partners (2.8 visits). Thus, knowing the identity of one's partner (and knowing that said partner also knew one's identity, etc.) would appear to be a crucial factor influencing the magnitude of the social effect.

In is also interesting to see the distribution of effects. Figure 1 shows the distribution of Study Room visits during the treatment period, by treatment group. We emphasize the stark rightward shift of the distribution for team treatment relative to individual treatment. In short, both incentive schemes produced an effect, but the team treatment was more effective. This was in spite of the fact that the risk of a partner's default in the team treatment was 43 percent. It is clear, then, even at first glance, that large, team-related social effects are implied, because the rate of bonus-earning in the team treatment is more than 40 percent higher than in the individual treatment, despite the fact that both teammates had to satisfy the requirement.

In order to check the robustness of the findings to the inclusion of covariates, we formalize the group mean comparisons using the following simple regression specification:

(2) $$Y_i = \beta_0 + \beta_1 T_i^{Any} + \beta_2 T_i^{Team} + \beta_3 T_i^{Anon} + \varepsilon_i,$$

where Y_i is an attendance outcome for individual i, T^{Any} is an indicator variable for having been randomized into either the individual or the team treatment, T^{Team} is an indicator variable for being in the team treatment, T^{Anon} is an indicator variable for being in the anonymous treatment, and ε is the usual error term. The coefficient of primary interest is β_2, as this captures the difference between team treatment 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).

Table 5, column 1 displays results of OLS regressions for the continuous outcome variable "Visits" on treatment status; columns 2 and 3 report results of analogous linear probability regressions for the dichotomous outcome variables "Try" and "Bonus."[17] These columns duplicate results from Table 4 in a regression setting, using equation 2). To test the robustness of our results, we add varying controls to

[17] Standard errors are clustered at the group level. Thus, group sizes are two for those assigned to the team and anonymous treatments and one for those assigned to the control and individual treatment. All conclusions for this table and all subsequent tables are similar if probit models are used instead of linear probability models.

these basic regressions. The inclusion of such controls should not affect our treatment effect estimates much because of the randomness of our treatments, but regardless, some may worry that our recruitment process induced some selection. Columns 4-6 of Table 5 show results of regression models that include age, gender, and pre-treatment library usage as covariates. Columns 7-9 add class fixed effects. Under all specifications, the same pattern emerges and the differences in bonus-earning between treatments are similar.

In addition to the relative magnitudes of the effects of the different treatments, we are also interested in relative costs of the different treatments. Not only is the team treatment more effective, it 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 was \$5.00, whereas the average cost for the same outcome in the individual treatment was \$6.30. Thus the per-visit cost of the individual treatment is 26 percent higher than for the team treatment.

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? We revisit the model of Section 2 to answer this question. The decision to complete the task and earn a \$25 bonus for person i is given by equations (1a), (1b) and (1c), 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 treatment.

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

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

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. 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 in which 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 hall is independent of the availability of the monetary incentive. The second simplifying assumption is that beliefs about partner study hall 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 hall 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 hall 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' pay-off ($\left(1 + \hat{\theta}\right)\hat{B} = \hat{\delta}_3 \rightarrow \hat{\theta} = \frac{\hat{\delta}_3}{\hat{\delta}_2} - 1$).

We bootstrap to account for the presence of the generated regressor. Table 6 displays the results. The estimates imply that subjects received a utility gain of 0.54 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 1.64 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 2.03, and is also statistically distinguishable from zero at the 5 percent level. Recall that this social parameter identifies the weight one puts on one's partner's utility in determining one's own utility. Thus, the implied social impact of team compensation is very large – more than twice 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 2.030 in Table 6, 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 difficulty estimating their partners' probability of meeting the payment threshold, given observables. One might question whether previous library usage is observable.[18] To explore robustness, then, it is worth estimating the social parameter given different beliefs about the probability of partner default.

Panel B of Table 6 displays the estimate of θ, given several different focal beliefs about teammate performance. If subjects believe their teammates will meet the bonus payment threshold with certainty, then they are more willing to meet the threshold, themselves, for their own pecuniary benefit. We do not think it is realistic that subjects would be so optimistic, but have reported this result because it gives us a lower bound on θ. Belief with certainty that teammates will earn the bonus yields a lower bound on possible values of θ because it implies that agents think they will receive the maximum possible monetary gain (and leaves less utility gain to be explained by social factors). As shown in Row 1 of Panel B, even if agents believe their partners will never default, the implied social parameter is large: Agents act as though they value a marginal dollar of compensation for their teammate 64 percent as much as they value a marginal dollar of compensation for themselves.

In Row 2, 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 1.88, close to our preferred estimate in Panel A. On the other hand, if subjects are very pessimistic about teammate performance, the implied social effect is even larger. Row 2 of Table 6, Panel B, shows results if subjects believe their partners will not respond at all to the $25 bonus incentive, and will instead behave exactly as the controls do. In this somewhat unrealistic case, pessimism about own pecuniary compensation would imply that the only way to explain higher observed effort choices in the team treatment would be with an extremely large social parameter: $\hat{\theta}$ =7.79.

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

[18] 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.

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 other possible focal beliefs. Column 2 shows estimates of θ in the anonymous treatment for the three beliefs described above. If subjects believe their anonymous partners will never default, the estimated θ is negative but not significantly different from zero. If subjects are correct, on average, about teammate performance, then the implied social factor is 0.86, but imprecisely estimated, and if subjects believe their anonymous teammates will not respond to the prospect of earning a bonus, the implied $\hat{\theta}$ is 2.28. Not surprising, the social effect, then, is stronger when subjects have met their partners than when they are paired with an anonymous stranger. This could be because knowing and being known by one's teammate creates the opportunity for social punishment that is not present in the anonymous case. It could also be that subjects value the pay-offs of people they have met more than they value the pay-offs of anonymous strangers.

D. 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 will focus on three broad classes of mechanisms that have been posited in previous research.

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 flavors besides this, and that θ in our framework could capture any of these.[19] For example, guilt aversion involves an

[19] 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). Guilt aversion is considered in Dufwenberg and Gneezy (2000), Charness and Dufwenberg (2006), and Battigalli and Dufwenberg (2007, 2009). See Charness and Kuhn (2011) for a survey of the literature on these social motivations.

individual feeling guilty about disappointing the expectations of people who act favorably on one's behalf; the more one believes that the other people expect one to perform an act, the more guilty one would feel from non-performance. Shame involves negative feelings about one's observed behavior, regardless of the expectations of 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.

2) **Production Complementarities.** Production complementarities have 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.

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 future self; the present self would instead rather engage in a more-immediately-pleasurable activity.[20] Having a partner could remedy this problem, even if the individual does not value the teammate's payoff. If one commits oneself to study *with a partner*, it is more difficult to back out. In short, individuals who have been jointly incentivized may use each other to devise commitment mechanisms.

We now briefly offer some evidence to distinguish among these channels. The production complementarities mechanism requires some coordination. If production complementarities make studying more productive and valuable, then in order to take advantage of the complementarities individuals must visit the study room at the same time. Similarly, those facing a commitment problem

[20] 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).

may use coordination to overcome their self-control problem. Since we have data on visit times, we can test whether we observe coordination of this type.

In Table 7, we test whether subjects in the team treatment go to the study hall at the same time as their teammates much more often than would random pairs of "placebo" teammates. In the team treatment, there were 26 instances in which a subject showed up at the study hall at about the same-time (plus or minus 10 minutes) as a teammate. For comparative purposes, we randomly assigned placebo 'teammates' to all subjects in the team treatment within classes. For the placebo pairings, there were 10 instances of simultaneous visits. There were thus 16 additional simultaneous visits associated with true teammate pairings for the 168 subjects in the team treatment. This accounts for only 0.095 visits per subject, a very small impact. The difference in visits between team treatment and individual treatment was more than 6 times larger, at 0.606 visits per subject. These few visits associated with coordination could explain only about 18 percent of the estimated social parameter, $\hat{\theta}$, in Table 6. We conclude that mechanisms requiring coordination explain at most a small part of the effectiveness of the team treatment.

We do not find strong evidence, then, that effectiveness of team incentives in our setting arises from production complementarities or a need for commitment mechanisms related to joint study hall attendance. This leaves guilt, shame, altruism, embarrassment, fear of reprisal, commitment devices unrelated to joint attendance, and other social factors as possible mechanisms.

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 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-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. Descriptive Statistics

Panel A of Table 8 shows sample means of descriptive characteristics by treatment status for all subjects who filled out the in-class survey for the exercise experiment. There were no statistically significant differences between average characteristics of subjects randomized into the individual treatment, the group treatment, or the control group (Panel A). We report two measures of exercise for the pre-treatment period. "Self-Reported Exercise" is the number of times per week that individuals claim to have exercised during the previous month. 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. For the remainder of the paper, we focus on the second pre-treatment measure, as it is not self-reported and relates more directly to the outcome we incentivize in the experiment: usage of the Rec Center.

Not all subjects chose to participate in the second stage of the experiment (i.e., come outside of class) at which point they formally became a part of the experiment and learned their treatment assignment. As displayed in Panel B, 364 of the 479 students who filled out surveys in class, or about 76 percent, went on to participate in the experiment, net of exclusions. There were no statistically significant differences in age or gender between participants and non-participants. However, participants were more apt to have used the Rec Center before, on average, than non-participants. We infer that this is the case either because subjects who are energetic enough to come outside for a lottery are also more apt to have the self-discipline to go to the Rec Center, or because students inferred from the survey questions that the experiment might be about exercise. Inferences now will be based on the sample of participants. We draw no conclusions about the 24 percent of in class-responders who were non-participants and who appear to have been less likely to go to the gym, on average. However, our sample population includes many individuals who are similar to these non-participants at least in terms of observable dimensions—potentially allowing us to infer the effect of the incentive schemes on these non-participants.

Panel C shows sample means of descriptive characteristics by treatment status for the 364 subjects who participated in the experiment. Average age, self-reported exercise, and previous Rec Center visits do not differ significantly between group treatment, individual treatment, and control groups. The randomization was such that more males ended up in the group treatment 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 age and gender controls.

B. Results

In Table 9, analogous to Table 4, we report mean effort outcomes by treatment status. Control subjects in the sample visited the Rec Center 2.1 times on average during the treatment period. 52 percent of the control subjects showed up to the Rec Center at least once, and 17 percent reached the 5-visit bonus threshold. We see that subjects responded to the incentives provided by the treatments. Subjects in the individual and team treatments made about 1.7 more and 2.0 more visits to the Rec Center, respectively, during the treatment period than did controls. They were also 16 and 30 percentage points more likely, respectively, to have gone to the Rec Center at least once and about 38 and 39 percentage points more likely to have met the 5-visit bonus payment threshold. Figure 2 shows the distribution of Rec Center visits during the treatment period, by treatment. We emphasize the noticeable rightward shift of the distribution for team and individual treatments relative to the distribution of the control group and that of the non-participants. In short, both incentive schemes produced an effect: Incentivized subjects went to the Rec Center more than non-incentivized subjects.

Table 10, analogous to Table 5, shows that the effects persist in a regression setting with additional covariates, based on the regression model (equation (2)). Evidence that the team and individual compensation schemes evoked significantly different responses is visible in the set of no controls regressions, column 2: Subjects randomized into the team-incentive scheme were 14.3 percentage points more likely to have visited the Rec Center during the treatment period than subjects in the individual treatment, and the difference is statistically significant at the 5 percent level. Unlike in the study room experiment, the rate of bonus-earning in the team treatment for the exercise was about the same as in the individual treatment. However, even though team and individual treatments evoked very similar task completion rates in this setting, the team treatment cost substantially less per visit, just as in the pay-for-study setting. The average per visit cost for the team treatment was $3.89, whereas the average per visit cost for the individual treatment was $5.23, or 31 percent higher. The team incentive again elicited similar effort at lower cost per person.

Further, the results in Tables 9 and 10 contain evidence on the existence of a social effect of the team incentive. The risk of a partner's default in the team treatment was 44 percent. Despite this high risk of 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). We will show that this requires a large social effect, though not as large an effect as was found in the study hall experiment.

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. 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 are heterogeneous treatment effects, by type and partner type. In Table 11, 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 investigate differences between active types and inactive types by partner type in Panels D and E. In Panel D, the point estimates on team treatment 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. This panel reports results for the inactive types—individuals who are less likely, ex ante, to go to the gym, and for whom these external incentives are more likely to be a decisive factor. 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 must exist for these choices to make sense. 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 clear evidence that subjects estimate the probability of default by their partners, based on observables. Own expected pay-out matters, but so too do social factors.

C. The Social Effect

Exactly as in the analysis of the study hall experiment, we use the model defined by equations (3) and (4) to estimate pecuniary and social components of utility for the exercise experiment. Table 12 displays the results. The estimates imply that subjects received a utility gain of 1.29 utils from their own pecuniary benefit of being paid $35, 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 0.44 probability of teammate default, and is two thirds as large as the effect of own pecuniary compensation.

Panel B of Table 12 shows the estimated social effect for various focal beliefs about partner default. If subjects are wrong in their belief about teammate defaults, and instead believe their teammates will meet the bonus threshold with certainty, there is no social effect. In such a case, subjects would put forth effort in the team treatment simply because they believe they'd earn the bonus. However, if the subjects are correct on average about partner performance (the remaining two estimates of θ), the social component of utility is large.

Exactly as in study room setting, production complementarities and commitment mechanisms do not appear to explain the social factor. The Rec Center data contain information on the precise time of the Rec Center visit. As reported in Table 13, in the team treatment, there were 14 instances in which a subject showed up at the Rec Center at about the same-time (plus or minus 10 minutes) as a teammate. For the placebo pairings, there were 6 instances of simultaneous visits. There were thus 8 additional simultaneous visits associated with true teammate pairings for the 190 subjects in the team treatment. This accounts for only 0.04 visits per subject, and accounts for at most 7 percent of additional bonus-earning behavior. Again, we conclude that that mechanisms requiring coordination explain at most a very small part of the effectiveness of the team treatment in the exercise setting.

In short, social effects of team incentives do not appear to be an idiosyncratic feature of being paid to study. We find large social effects in both the pay-for-study and pay-for-exercise settings; however, the effect is larger in the pay-for-study intervention.

7. Conclusion

Incentives that offer direct pay for behavior have been studied extensively and have generally been found to produce modest or insignificant results. However, there is not much credible empirical work on behavior interventions that compensate individuals for team behavior. Management consultants allege that team compensation harnesses a powerful social mechanism, 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 considerably larger than that of own pecuniary compensation. In addition, the team incentive scheme in our experiment was 26 percent to 31 percent more cost effective than the individual incentive.

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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Table 1. Study Room Samples

Course Number	Official Enrollment	Survey Respondents	Experiment Participants
Econ 101	205	40	20
Econ 106	91	32	27
Econ 114	85	22	13
Econ 118	58	13	10
Econ 122	145	34	23
Econ 130	54	37	22
Econ 132A	58	42	29
Econ 136A - 1	59	52	34
Econ 136A - 2	55	40	34
Econ 136B	76	60	41
Econ 137A	56	23	16
Econ 138A	118	38	24
Econ 140A	113	54	29
Econ 160	50	19	16
Econ 171	55	32	27
Econ 189 - 1	59	28	20
Econ 189 - 2	72	32	31
Psych 7	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

Table 2. Rec Center Samples

Economics Course Number	Official Enrollment	Survey Respondents	Experiment Participants
2	101	79	62
3B	119	85	66
100B	83	35	21
101	79	76	58
114	62	47	39
118	62	37	28
136A	48	33	21
136B	55	41	30
136C	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

Table 3. Study Room Survey Response and Experiment Participation

	Male	Age	Library Days	Sample
Panel A: Classroom Survey Response				
Sample Means				
Control (C)	0.65	21.04	1.70	111
	(0.48)	(1.48)	(1.98)	
Individual Treatment (IT)	0.57	21.22	1.81	109
	(0.50)	(1.48)	(1.77)	
Team Treatment (TT)	0.60	21.13	1.68	232
	(0.49)	(1.45)	(1.88)	
Anonymous Team Treatment (AT)	0.56	21.18	1.67	231
	(0.50)	(1.70)	(1.81)	
Mean Differences (P-Values)				
IT - C	0.20	0.35	0.69	
AT - C	0.11	0.42	0.89	
AT - IT	0.91	0.82	0.52	
TT - C	0.32	0.57	0.91	
TT - IT	0.61	0.60	0.54	
TT - AT	0.44	0.75	0.98	
Panel B: Experiment Participation				
Sample Means				
Non-Participants (NP)	0.57	21.42	1.77	192
	(0.50)	(1.80)	(2.01)	
Participants (P)	0.60	21.04	1.67	491
	(0.49)	(1.43)	(1.79)	
Mean Differences (P-Values)				
P - NP	0.46	0.01	0.56	
Panel C: Treatment Assignment Conditional on Participation				
Sample Means				
Control	0.60	20.93	1.51	75
	(0.49)	(1.40)	(1.78)	
Individual Treatment	0.59	21.13	1.82	82
	(0.49)	(1.39)	(1.81)	
Team Treatment	0.64	21.02	1.67	172
	(0.48)	(1.27)	(1.83)	
Anonymous Team Treatment (AT)	0.57	21.07	1.68	162
	(0.50)	(1.61)	(1.76)	
Mean Differences (P-Values)				
IT - C	0.85	0.37	0.28	
AT - C	0.61	0.51	0.50	
AT - IT	0.76	0.74	0.56	
TT - C	0.55	0.63	0.50	
TT - IT	0.41	0.54	0.99	
TT - AT	0.17	0.78	0.81	

Standard deviations are in parentheses. P-values are for two-sided t-tests assuming unequal variances for continuous variables and two-sample proportion tests for discrete variables. Some sample sizes vary slightly due to non-reporting for specific variables.

Table 4. Study Room Visits by Treatment Group

	Visits	Try	Bonus	Sample
Control (C)	1.33	0.40	0.19	75
	(2.04)	(0.49)	(0.39)	
Individual Treatment (IT)	2.22	0.57	0.40	82
	(2.32)	(0.49)	(0.49)	
Team Treatment (TT)	2.83	0.70	0.57	172
	(2.18)	(0.46)	(0.50)	
Anonymous Team Treatment (AT)	1.75	0.49	0.33	162
	(2.06)	(0.50)	(0.47)	
Mean/Proportion Differences (P-Values)				
IT - C	0.01	0.03	0.00	
AT - C	0.14	0.18	0.03	
AT - IT	0.13	0.24	0.25	
TT - C	0.00	0.00	0.00	
TT - IT	0.05	0.05	0.01	
TT - AT	0.00	0.00	0.00	

Standard deviations are in parentheses. P-values are for two-sided t-tests assuming unequal variances for continuous variables and two-sample proportion tests for discrete variables.

Table 5. Study Room Visits for Individual, Team Treatments, and Anonymous Team Treatments

	No Controls			With Controls			With Controls and Class Fixed Effects		
	Visits (1)	Try (2)	Bonus (3)	Visits (4)	Try (5)	Bonus (6)	Visits (7)	Try (8)	Bonus (9)
Any Treatment	0.886**	0.173*	0.216**	0.850**	0.164**	0.217**	0.768**	0.149*	0.199**
	(0.347)	(0.079)	(0.071)	(0.345)	(0.079)	(0.072)	(0.352)	(0.081)	(0.073)
Team Treatment	0.606*	0.125*	0.167**	0.622*	0.130*	0.164**	0.740**	0.148**	0.186**
	(0.317)	(0.067)	(0.068)	(0.321)	(0.067)	(0.069)	(0.333)	(0.072)	(0.071)
Anonymous Team Treatment	-0.466	-0.079	-0.075	-0.486*	-0.084	-0.085	-0.444	-0.084	-0.079
	(0.295)	(0.065)	(0.064)	(0.294)	(0.066)	(0.065)	(0.299)	(0.070)	(0.065)
Male				-0.143	-0.054	-0.049	-0.153	-0.069	-0.044
				(0.199)	(0.047)	(0.046)	(0.204)	(0.049)	(0.047)
Age 20				-0.155	-0.040	-0.010	-0.382	-0.123	-0.029
				(0.352)	(0.081)	(0.082)	(0.397)	(0.092)	(0.096)
Age 21				-0.152	-0.056	0.013	-0.451	-0.142	-0.017
				(0.330)	(0.080)	(0.077)	(0.411)	(0.097)	(0.098)
Age 22				-0.377	-0.055	-0.114	-0.792	-0.154	-0.173
				(0.411)	(0.096)	(0.087)	(0.503)	(0.115)	(0.112)
Age 23+				-0.004	-0.006	0.023	-0.394	-0.111	-0.030
				(0.428)	(0.097)	(0.097)	(0.494)	(0.110)	(0.117)
Library Days				0.168**	0.033**	0.013	0.164**	0.032**	0.012
				(0.064)	(0.013)	(0.013)	(0.065)	(0.013)	(0.014)
Constant	1.333**	0.400**	0.187**	1.312**	0.423**	0.206**	--	--	--
	(0.235)	(0.057)	(0.045)	(0.370)	(0.091)	(0.081)			

Sample size is 491. Standard errors are clustered at the group level and reported in parentheses. ** (*) indicates statistically significant at the 5 (10) percent level. Columns (4)-(9) also include indicators for missing age and sex.

Table 6. Structural Estimates

	Coefficient	95% Percentile Lower Bound	95% Percentile Upper Bound
Panel A - Structural parameters			
Male	0.013	-0.290	0.320
Age 20	-0.123	-0.728	0.555
Age 21	0.171	-0.397	0.797
Age 22	-0.200	-0.890	0.568
Age 23+	0.189	-0.506	0.938
Library Days	0.086	0.0002	0.181
Predicted partner bonus status	1.642	0.970	2.266
Individual treatment indicator	0.542	0.079	0.955
Constant	-0.976	-1.678	-0.366
Theta	2.030	0.939	6.167
Panel B - Theta under different probability of completion assumptions			
Theta: certainty	0.643	0.156	2.027
Theta: unconditional team treatment mean	1.883	1.029	4.312
Theta: unconditional control group mean	7.786	5.182	15.186
Panel C - Anonymous Treatment - Theta under different probability of completion assumptions			
Theta: certainty	-0.386	-0.797	0.059
Theta: unconditional anonymous treatment m	0.864	-0.394	2.412
Theta: unconditional control group mean	2.284	0.088	4.661

1000 bootstrap replications. Sample excludes anonymous treatment group members and individuals with missing data for themselves or their partners. The sample size is 327.

Table 7. Incidence of Pairs Visiting the Study Room Together

Number of Same Time Visits	Same Time = +/- 10 Minutes		Same Time = +/- 20 Minutes	
	Team Treatment	Random Partner	Team Treatment	Random Partner
0	150	158	146	156
	(89.3)	(94.1)	(86.9)	(92.9)
1	14	10	18	10
	(8.3)	(6.0)	(10.7)	(6.0)
2	2	0	2	2
	(1.2)	(0.0)	(1.2)	(6.0)
3	0	0	0	0
	(0.0)	(0.0)	(0.0)	(0.0)
4	2	0	2	0
	(1.2)	(0.0)	(1.2)	(0.0)
Total	168	168	168	168

Percentage of visits in pairs in parentheses. Random partners are random 'pairs' within class within the team treatment.

Table 8. Rec Center Survey Response and Experiment Participation

	Male	Age	Self-Reported Exercise	Previous Gym Visits	Sample Size
Panel A: Classroom Survey Response					
Sample Means					
Control (C)	0.59	21.19	3.80	1.01	112
	(0.49)	(2.24)	(2.21)	(1.55)	
Individual Treatment (IT)	0.57	21.39	4.30	1.05	116
	(0.50)	(2.72)	(2.63)	(1.59)	
Team Treatment (TT)	0.65	21.09	4.19	1.04	251
	(0.48)	(2.16)	(2.48)	(1.40)	
Mean Differences (P-Values)					
IT - C	0.69	0.55	0.12	0.84	
TT - C	0.27	0.69	0.15	0.86	
TT - IT	0.11	0.29	0.69	0.95	
Panel B: Experiment Participation					
Sample Means					
Non-Participants (NP)	0.64	21.26	4.13	0.80	115
	(0.48)	(2.43)	(2.51)	(1.33)	
Participants (P)	0.61	21.16	4.13	1.11	364
	(0.49)	(2.30)	(2.45)	(1.52)	
Mean Differences (P-Values)					
P - NP	0.56	0.71	1.00	0.04	
Panel C: Treatment Assignment Conditional on Participation					
Sample Means					
Control	0.55	21.14	3.98	1.17	87
	(0.50)	(2.43)	(2.29)	(1.65)	
Individual Treatment	0.54	21.24	4.16	1.17	87
	(0.50)	(2.19)	(2.49)	(1.69)	
Team Treatment	0.67	21.14	4.18	1.05	190
	(0.47)	(2.30)	(2.51)	(1.38)	
Mean Differences (P-Values)					
IT - C	0.88	0.77	0.62	1.00	
TT - C	0.05	0.99	0.51	0.56	
TT - IT	0.03	0.71	0.94	0.56	

Standard deviations are in parentheses. P-values are for two-sided t-tests assuming unequal variances for continuous variables and two-sample proportion tests for discrete variables. Some sample sizes vary slightly due to non-reporting for specific variables.

Table 9. Rec Center Visits by Treatment Group

	Visits	Try	Bonus	Sample
Control (C)	2.13	0.52	0.17	87
	(2.99)	(0.50)	(0.38)	
Individual Treatment (IT)	3.82	0.68	0.55	87
	(3.23)	(0.47)	(0.50)	
Team Treatment (TT)	4.16	0.82	0.56	190
	(2.78)	(0.38)	(0.50)	
Mean/Proportion Differences (P-Values)				
IT - C	0.00	0.03	0.00	
TT - C	0.00	0.00	0.00	
TT - IT	0.39	0.01	0.86	

Standard deviations are in parentheses. P-values are for two-sided t-tests assuming unequal variances for continuous variables and two-sample proportion tests for discrete variables.

Table 10. Rec Center Visits for Individual and Team Treatments

	No Controls			With Controls			With Controls and Class Fixed Effects		
	Visits (1)	Try (2)	Bonus (3)	Visits (4)	Try (5)	Bonus (6)	Visits (7)	Try (8)	Bonus (9)
Any Treatment	1.690**	0.161**	0.379**	1.736**	0.169**	0.388**	1.806**	0.178**	0.395**
	(0.471)	(0.074)	(0.067)	(0.362)	(0.063)	(0.059)	(0.365)	(0.064)	(0.059)
Team Treatment	0.347	0.143**	0.011	0.330	0.137**	-0.005	0.288	0.135**	-0.014
	(0.412)	(0.059)	(0.067)	(0.341)	(0.054)	(0.061)	(0.340)	(0.054)	(0.061)
Male				0.454*	0.079*	0.099**	0.319	0.056	0.081*
				(0.270)	(0.044)	(0.047)	(0.259)	(0.044)	(0.048)
Age 20				-0.593	-0.108*	-0.076	-0.724	-0.121*	-0.111
				(0.439)	(0.058)	(0.075)	(0.440)	(0.062)	(0.076)
Age 21				-0.560	-0.078	-0.093	-0.556	-0.077	-0.124
				(0.434)	(0.055)	(0.072)	(0.490)	(0.069)	(0.083)
Age 22				-1.274**	-0.235**	-0.193**	-1.208**	-0.210**	-0.224**
				(0.572)	(0.077)	(0.091)	(0.611)	(0.082)	(0.097)
Age 23+				-1.580**	-0.271**	-0.271**	-1.704**	-0.279**	-0.317**
				(0.503)	(0.074)	(0.082)	(0.528)	(0.083)	(0.092)
Pre-period Rec Center visits				0.945**	0.098**	0.092**	0.938**	0.102*	0.091**
				(0.097)	(0.012)	(0.015)	(0.097)	(0.012)	(0.015)
Constant	2.126**	0.517**	0.172**	1.499**	0.477**	0.127*	--	--	--
	(0.320)	(0.054)	(0.041)	(0.471)	(0.067)	(0.070)			

Sample size is 364. Standard errors are clustered at the group level and reported in parentheses. ** (*) indicates statistically significant at the 5 (10) percent level. Columns 4- 6 also include indicators for missing age.

Table 11. Rec Center Visits for Individual and Team Treatments - Restricted Samples

	No Controls			With Controls			With Controls and Class FEs			
	Visits (1)	Try (2)	Bonus (3)	Visits (4)	Try (5)	Bonus (6)	Visits (7)	Try (8)	Bonus (9)	Sample Size
<u>Panel A</u>										364
Any Treatment	1.690**	0.161**	0.379**	1.736**	0.169**	0.388**	1.806**	0.178**	0.395**	
	(0.471)	(0.074)	(0.067)	(0.362)	(0.063)	(0.059)	(0.365)	(0.064)	(0.059)	
Team Treatment	0.347	0.143**	0.011	0.330	0.137**	-0.005	0.288	0.135**	-0.014	
	(0.412)	(0.059)	(0.067)	(0.341)	(0.054)	(0.061)	(0.340)	(0.054)	(0.061)	
<u>Panel B: Sample Restricted to Actives</u>										164
Any Treatment	1.759**	0.023	0.409**	1.682**	0.027	0.418**	1.862**	0.031	0.443**	
	(0.679)	(0.059)	(0.106)	(0.610)	(0.058)	(0.100)	(0.625)	(0.061)	(0.099)	
Team Treatment	-0.883	-0.022	-0.122	-0.489	-0.012	-0.100	-0.634	-0.020	-0.129	
	(0.539)	(0.050)	(0.089)	(0.480)	(0.040)	(0.086)	(0.502)	(0.045)	(0.092)	
<u>Panel C: Sample Restricted to Inactives</u>										200
Any Treatment	1.782**	0.286**	0.372**	1.829**	0.304**	0.382**	1.811**	0.308**	0.380**	
	(0.444)	(0.091)	(0.072)	(0.429)	(0.089)	(0.071)	(0.428)	(0.090)	(0.072)	
Team Treatment	1.047**	0.240**	0.088	0.815	0.204**	0.051	0.711	0.188**	0.036	
	(0.486)	(0.086)	(0.088)	(0.469)	(0.086)	(0.086)	(0.464)	(0.084)	(0.086)	
<u>Panel D: Sample Restricted to Actives</u>										164
Any Treatment	1.759**	0.023	0.409**	1.682**	0.027	0.417**	1.877**	0.030	0.443**	
	(0.682)	(0.059)	(0.106)	(0.612)	(0.058)	(0.100)	(0.628)	(0.061)	(0.099)	
Team Treatment: Inactive Partner	-0.887	-0.033	-0.160	-0.498	-0.025	-0.134	-0.400	-0.029	-0.133	
	(0.667)	(0.063)	(0.110)	(0.596)	(0.058)	(0.105)	(0.603)	(0.061)	(0.110)	
Team Treatment: Active Partner	-0.881	-0.016	-0.099	-0.484	-0.004	-0.077	-0.797	-0.014	-0.127	
	(0.584)	(0.057)	(0.102)	(0.508)	(0.044)	(0.098)	(0.530)	(0.050)	(0.106)	
<u>Panel E: Sample Restricted to Inactives</u>										200
Any Treatment	1.782**	0.286**	0.372**	1.844**	0.303**	0.387**	1.840**	0.309**	0.390**	
	(0.445)	(0.092)	(0.072)	(0.429)	(0.089)	(0.071)	(0.427)	(0.091)	(0.072)	
Team Treatment: Inactive Partner	0.818	0.252**	0.002	0.581	0.211**	-0.036	0.450	0.183**	-0.055	
	(0.542)	(0.094)	(0.097)	(0.507)	(0.093)	(0.091)	(0.480)	(0.089)	(0.089)	
Team Treatment: Active Partner	1.490**	0.216**	0.255**	1.303**	0.191*	0.232**	1.235*	0.199*	0.217*	
	(0.612)	(0.106)	(0.108)	(0.625)	(0.108)	(0.112)	(0.651)	(0.109)	(0.115)	

Sample size is 364. Standard errors are clustered at the group level and reported in parentheses. ** (*) indicates statistically significant at the 5 (10) percent level. Columns 4-9 also include indicators for missing age.

Table 12. 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: certainty	-0.043	-0.226	0.198
Theta: unconditional team treatment mean	0.699	0.375	1.127
Theta: unconditional control group mean	4.486	3.440	5.868

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

Table 13. Incidence of Pairs Visiting the Rec Center Together

Number of Same Time Visits	Same Time = +/- 10 Minutes		Same Time = +/- 20 Minutes	
	Team Treatment	Random Partner	Team Treatment	Random Partner
0	178	184	178	176
	(93.7)	(96.8)	(93.7)	(92.6)
1	10	6	10	14
	(5.3)	(3.2)	(5.3)	(7.4)
2	2	0	2	0
	(1.1)	(0.0)	(1.1)	(0.0)
Total	190	190	190	190

Percentage of visits in pairs in parentheses. Random partners are random 'pairs' within class within the team treatment.

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. All sign-ups for the experiment occurred about two weeks into the quarter (October 3-7). 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. 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 paring; 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 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.”[2]

[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] 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.

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.

Appendix B

Survey and Consent Forms

<u>Pay-For-Study</u>

1. In-Class Consent Form and Survey

2. Participant Consent Forms
    a. Control Group
    b. Individual Treatment
    c. Team Treatment
    d. Anonymous Treatment

<u>Pay-For-Exercise</u>

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

....

F1

____________________________ 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_____________________________________

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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 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

______________________	______________________________	August _____, 2010
Print name	Signature	Date
______________________	____________________________	_______________________
Perm #	Primary e-mail address	Local phone number

……

A1

________________	________________	
Print last name	first name	*

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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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.

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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.

Partner's name ____

Signature______________________________ Print name_________________________

……………………………………………………………………………………………………..

Partner's name ____