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ABSTRACT

This paper attempts to isolate, document, and measure a social effect of incentivizing people in teams. In a field experiment featuring exogenous team-formation and opportunities for repeated social interactions over time, we find social effects that are nearly as large as direct pecuniary effects: the team compensation system implemented in our paper induced agents to choose their effort as if they valued a marginal dollar of compensation for their teammate three-fourth as much as they value a dollar of their own compensation. We conclude that social effects in teams exist and can be decisive in motivating effort-intensive tasks.

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1. Introduction

Firms frequently group people into teams and pay on the basis of group performance. One possible reason for the pervasiveness of team incentives is that social effects of teams are perceived by many to be powerful tools for motivating people. Indeed, a best-selling management consultant argues that social effects of team incentives are *more* effective at eliciting effort and improving performance than any other single factor induced by policy: "More than any policy or system, there is nothing like the fear of letting down respected teammates that motivates people to improve their performance."[1] Others have argued similarly: "Letting down the team may be worse than letting down the boss."[2] And yet, clean evidence on this issue is scarce: We are aware of no previous field-level empirical research that confirms or refutes this common perception. Do social effects of team incentives exist, and are they important, first-order effects, as some managers have claimed? Answering this question sheds light on basic economic principles and enhances understanding of what has become a frequent practice in workplace environments. In this paper, we conduct a field intervention to provide an unusually clean test of social effects of team compensation.

Most theoretical and empirical work on team incentives focuses on free-riding associated with the sharing of a pay-off (Holmstrom 1982). However, it has long been theorized that social pressures may also play an important role. Kandel and Lazear (1992) observe 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. Individuals may be more willing to make sacrifices in support of people with whom they have bonded, than in support of the distant, unseen principals for whom they are agents.

[1] Lencioni (2002), p. 213
[2] Brown (2006), p. 551.

These "social effects" can be defined narrowly or broadly. Here, we define them in the spirit of the workplace managers and management consultants quoted above. By social effects, we mean effects that are related in a direct way to the utility a worker 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. By social effects, we do not mean specialization in production or knowledge transfers, even though these, too, could be viewed as types of peer effects. We define terms in this restrictive way in order to distinguish a directly social impact of team membership on effort from the production-process-oriented effects that have often been emphasized in the literature. Indeed, one strong claim in the literature is 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). Our research allows a rare test of this claim. The findings show that in the specific context of our experiment, powerful social effects of team incentives are present even when production complementarities are not. "Artificial" teams, in our setting, do harness team incentives in productive ways.

Recent empirical research documents some effort-related social effects in the workplace, but these studies *do not involve team compensation*. Mas and Moretti (2009) find that supermarket checkers work faster when observed by other checkers who work fast. Falk and Ichino (2006) find similar results for workers whose task is to stuff envelopes. Bandiera, Barankay, and Rasul (2010) observe that a worker's productivity rises when she works alongside other workers in her social network. Bandiera, Barankay, and Rasul (2005) report that workers reduce effort when compensated through relative pay rather than piece rates, and convincingly interpret their findings as evidence of altruism and collusion. Though they provide compelling

evidence of social effects, none of these important and innovative studies sheds light on team incentives.[3]

There does exist some compelling empirical research on team incentives.[4] These studies, however, *do not attempt to isolate social effects*.[5] Hamilton, Nickerson, and Owan (2003), for example, 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 other factors directly related to the production process. What is missing from the literature, then, is an analysis that cleanly isolates social effects of monetary team incentives. This would seem an important gap, because in real-world settings, precisely these social effects are often emphasized as the dominant rationale for the adoption of teams.

One reason for the gap in the literature may be the difficulty of isolating the social effects of teams in a way that captures salient aspects of real-world interactions. First, one needs an environment in which potential social interaction between teammates is possible over an extended period of time—hard to accomplish in the laboratory, where one-time sessions are the mode. Second, the environment must satisfy a number of conditions rarely feasible in a workplace experiment: objectively-measurable effort outcomes not easily observed by other team members, exogenous team formation, exogenous variation in exposure to incentives, and a

[3] By "team incentives" we mean positive monetary incentives for team production. Bandiera, Barankay, and Rasul (2005) study a relative pay structure that has no potential to generate positive social effects. Mas and Moretti (2009) study a setting without explicit monetary team incentives.

[4] We have focused on field studies. From the laboratory, there is also some experimental evidence involving teams. This has tended to verify the existence of free-rider effects. Examples include Nalbantian and Schotter (1995), Meidinger, Rullière, and Villeval (2003), and van Dijk, Sonnemans, and van Winden (2001). Bornstein, Gneezy, and Nagel (2002) investigate coordination problems within teams and find that competition between teams helps to combat the free-rider problem and improves performance; however, they do not investigate social pressure directly.

[5] Leibowitz and Tollison (1980), Gaynor and Gertler (1995), Encinosa, Gaynor, and Rebitzer (2007).

credible means of isolating social effects from production complementarities and free rider effects. Our pay-for-exercise experiment, which uses college students who share a common class, meets all of these requirements.

In an exercise that decomposes utility into its social and pecuniary components, we find that the team compensation system implemented in our paper induced agents to choose their effort as if they valued a marginal dollar of compensation for their teammate three-fourths as much as they value a dollar of their own compensation. Thus, a major contribution of our study is that it offers what may be the first clean evidence confirming a major claim made in the workplace—that social effects of monetary team incentives can be large and powerful, a quiver in the bow for personnel managers and others. Further dimensions of the incentive structure still need to be explored, but our message is an encouraging one for those who wish to design incentive programs to enhance performance.

The remainder of this paper is structured as follows. We describe the conceptual framework in section 2. We cover details of the experimental design in section 3. We present and discuss our experimental results in section 4, and offer some concluding remarks in section 5.

2. Conceptual Framework

Before we proceed to the main analysis, it is worth performing a simple accounting exercise 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 their partner (person *j*) completing the task. The magnitude of the social effect is given by θ. 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 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 free-riding. If person *i* does not put forth effort, neither she nor her partner receives payment. The nonlinear incentive structure is similar in spirit to Homstrom's (1982) "forcing contacts" except that effort here is observable. It also may be worth noting that this kind of incentive structure can be found in real-world 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.[6]

Secondly, the task in question, going to the gym, does not allow in any direct way for knowledge transfers. (All subjects were told where the gym was—which, of course, the vast majority already knew.) We will also show that there is no credible evidence of subjects harnessing any other possible production complementarities related to teamwork in exercise (e.g., by hitching rides with each other, or by spotting for each other in weight-lifting): Teammates in the team treatment did not go to the gym at the same time any more often than randomly paired subjects in other treatments.[7] Thus, we will be able to conclude that any observed effect of the team incentive that cannot be traced to own pecuniary benefits is a social effect, rather than an effect of free-riding or production spillovers.[8]

Thirdly, if there is no social component to utility ($\theta = 0$) and there is some nonzero probability of default by person *j*, then the expected gain to performance of the task is higher in the individual treatment than in the team treatment. Thus, a strictly pecuniary model, in which there are no social effects, suggests that subjects will put forth more effort toward completion of the task when compensated as individuals than when incentivized using the team compensation scheme. If subjects do not do this—if, instead, they maintain or increase effort when incentivized in teams—we will interpret this as evidence that social effects exist and are large enough to compensate for lowered expectations of monetary gain. Large risks of teammate default, in this

[6] 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).

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

[8] It could be argued, of course, that if social interactions make people more productive, these are "production spillovers" by definition. We emphasize that the term, as used here, is more restrictive. By production spillovers here, we mean technical, non-social spillovers due to specialization, coordination, or knowledge transfer. Again, the goal is to distinguish the social impact of team membership on production from the more technical effects that are more commonly emphasized in the literature.

case, would imply large social effects. Leveraging the three treatments in the experimental design will also allow us to estimate θ, and thus quantify the magnitude of the social effect relative to the direct pecuniary effect. Lastly, we will attempt to shed light on which of several behavioral mechanisms are most consistent with our findings.

3. Experimental Design and Sampling

A. Experimental Design

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 first stage of recruitment involved asking students to fill out a brief survey at the beginning of each lecture (Appendix 1 contains an in-class survey).[9] 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. Students were also informed that a random selection of students who came outside after class would have the opportunity to make more money by participating in an experiment.

All surveys had a unique identification code. From the perspective of the students this appeared to be a numeric 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 were alphanumeric and included an A, B or C as the first

[9] 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).

character, a number as the second character, and possibly a third character. The letter indicated group assignment. For example, A might indicate control group, B individual treatment, and C team treatment. In this case, all C codes would also have a matching set of C* codes allowing us to form teams (C1 and C1* for example; see below for more detail about the after-class matching process). 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.[10]

The students who participated in the post-class portion of the sign-up process were placed in one of three groups as described above. For descriptive ease, we refer to these students as participants throughout the paper.

Control: Participants assigned to the control group were asked to sign a standard waiver explaining that exercise has risks and told that they would have to wait a few minutes for the $50 drawing.[11]

Individual Treatment: Participants who were assigned to the individual treatment were given the same waiver as the control group, as well as instructions about how they could earn money by

[10] Participation rates for the control, individual, and team treatments were 79, 75, and 76 percent, respectively. None of these percentages is statistically different from any other.

[11] All groups were given instructions both orally and in writing. The written instructions and exercise waivers given to each group are included in Appendix 1.

visiting the UCSB Recreation Center ("Rec Center"). The Rec Center is the on-campus student gym, which is free for registered students. More specifically, people in the individual-treatment group were told (both verbally and in writing) that they would be paid $2 for each eligible gym visit,[12] up to five visits, from August 7-20, and that they would also be paid a $25 bonus if and only if they accumulated five or more eligible visits during this time period.[13]

Team Treatment: To facilitate rapid pairing, the in-class team treatment surveys had a built-in pairing; for example, there might be a C1 and a C1*. Subjects with a "partner" who did not show up after lecture were randomly re-matched with another subject without a "partner."[14] As with the individual treatment, all group-treatment participants were given a standard exercise waiver as well as instructions describing how they could earn money by visiting the gym. In particular, they were told that they would earn $2 for each eligible Rec Center visit, up to five visits, from August 7-20 and that they would also be paid a $25 bonus if and only if both team members accumulated five or more eligible visits during the same time period. As it was important that team members had a chance to meet and talk, we had teammates stand next to each other on numbered cards during the sign-up process and exchange names by filling out their partner's name and email at the bottom of their waiver. This design gave the teammates the opportunity to see each other, talk for several minutes, and have a written document containing the partner's name.

[12] Participants could only accumulate one gym visit per day for payment purposes.
[13] Charness and Gneezy (2009) and Babcock and Hartman (2010) incentivize individuals similarly in their pay-for exercise experiments.
[14] This was done by matching in ascending sequential order. If C10 showed up but not C10* 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 him/her.

Both treatment groups were also informed that payments would be made in week five of the six-week session. The sharing of this information was important because it meant that subjects knew before making their exercise choices that there would be at least one full week of class remaining after payments were made. As a result, group treatment participants who accumulated five eligible visits, but only earned $10, would know their partner had failed to accumulate five eligible visits and would have the potential to interact with their partner during the last week of the summer session. To ensure that the experiment had salience, we contacted all participants in the control, individual, and team treatments via email at the end of the recruitment week to remind them about their treatment.

B. Measuring Gym Visits

One benefit of our experimental design is that it allows for the electronic collection of effort response (Rec Center attendance) data. 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.[15] 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.

[15] All UCSB students can use the recreation facilities for free by simply presenting their student card. Faculty and guests can obtain gym passes for a fee.

C. 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). The difference between enrollment size and survey response reflects almost entirely differences in class attendance and late arrival. While we do not have attendance for the day we signed people up for the experiment, the experimenters in the room reported that the vast majority of students present when the surveys were distributed completed the survey.[16] In terms of participation in the experiment, conditional on completing a survey, approximately 75 percent of survey respondents stayed after class for the lottery and were assigned to a treatment or control group.

The last row of Table 1 reports the survey and participant sample sizes used in all analyses reported in the remainder of the paper. There are a small number of sample exclusions; these exclusions stem from four possible reasons. First, and somewhat surprisingly, only one person who came out after class, across all classes, left after being assigned a partner but before signing the exercise waiver and experiment instructions. As we then re-paired the abandoned partner, we have excluded all three individuals from the sample. Second, we have excluded four individuals who signed up in two different classes, as well as all partners. Third, we removed the one person who reported a fake name and student number, or at least individuals that could not be located in the Rec Center computer system. Fourth, we excluded the small number of people with incomplete in-class surveys.

[16] Students were told not to sign-up more than once. Since some students enroll in multiple Economics classes simultaneously during the summer, this lowered the participation rate in some classes. For example, Economics 100B was the final class to be signed up, and had by far the lowest participation rate.

4. Empirical Results

A. Descriptive Statistics

Panel A of Table 2 shows sample means of descriptive characteristics by treatment status for all subjects who filled out the in-class survey. There were no statistically significant differences between average characteristics of subjects randomized into the individual treatment, the group treatment, or the control group. 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 documentation provided by the Rec Center. For the remainder of the paper, we will 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.

Table 2 also includes columns for "Low" and "High" types. The conceptual framework described in Equations 1a, 1b, and 1c suggests that differing individual-specific costs and benefits of gym-going (V_i and C_i) influence a person's responsiveness to the external incentives. For example, a person with very low intrinsic benefits may never be induced to earn the bonus. Thus, individual-specific traits can give us a sense of how close subjects are to the margin at which the differing external incentive schemes become decisive in motivating them. Further, effort choices may differ based on characteristics of the *teammate* to whom a subject has been assigned. For example, one would expect effort choices to rise with the expected performance of a subject's teammate.

To allow for heterogeneous treatment effects for subjects and teammates with differing internal net benefits of gym-going (V_i - C_i), we define types based on their pre-treatment Rec

Center attendance. Low types did not use the Rec Center at all in the week prior to the treatment period; High types used the Rec Center at least once in the week prior to the treatment period.[17] There were no statistically significant differences in the fraction of Low and High types assigned to control, individual and team treatments.

Not all subjects chose to participate in the lottery at which they formally became a part of the experiment and learned their treatment assignment. As displayed in Panel B, 356 of the 464 students who filled out surveys in class, or about 77 percent, went on to participate in the experiment (i.e., show up outside of class), 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. For the remainder of the paper, inferences will be based on the sample of participants. We draw no conclusions about the 23 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. This attrition rate is not large when gauged against other field experiments (e.g., Card, Mas, Moretti, and Saez, 2010).

[17] We emphasize the Low and High types need not correspond directly to physically active and physically inactive people. These categories will also capture individual-specific factors such as distance to the Rec Center relative to other exercise venues and differences in the types of exercise preferred. Because Rec Center attendance is the activity incentivized in our experiment, this form of heterogeneity is arguably the most relevant. We note, however, that results in the remainder of the paper are very similar when Low and High types are defined using the self-reported exercise measure for the pre-treatment period, rather than documented Rec Center visits.

Panel C shows sample means of descriptive characteristics by treatment status for the 356 subjects who participated in the experiment. Average age, self-reported exercise, previous Rec Center visits, and fractions Low and High 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 (the difference in means is significant at the 10 percent level). 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

We estimate effects of group and individual treatment relative to controls in regressions based on the following model:

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

where Y_i is an effort 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, 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. We will examine three effort-related outcomes: 1) The number of visits to the Rec Center during the treatment period; 2) whether the subject went at least once to the Rec Center during the treatment period; 3) whether the subject used the Rec Center on five different days during the treatment period (which is the threshold for receiving the bonus). These outcomes are designated "Visits," "Try," and "Bonus," respectively. Table 3, column 1 contains 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."[18] Standard errors are clustered at the group level. This means that group sizes are two for those assigned to the team treatment and one for those assigned to the control and individual treatment.

The estimate of the constant in Panel A shows mean effort choices of control subjects. Control subjects in the sample visited the Rec Center 2.2 times on average during the treatment period.[19] 50.6 percent of the control subjects showed up to the Rec Center at least once, and 18.1 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 17.6 and 31.8 percentage points more likely to have gone to the Rec Center at least once and about 38 percentage points more likely to have met the 5-visit bonus payment threshold. Figure 1 shows the distribution of Rec Center visits during the treatment period, by treatment. We emphasize the stark 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. Perhaps more surprising, *the rate of bonus-earning in the team treatment was about the same as in the individual treatment*. The risk of a partner's default in the team treatment was 43 percent. Despite the 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). It is clear, then, even at first glance, that large, team-related

[18] All conclusions for this table and all subsequent tables are similar if probit models are used instead of linear probability models. All results are robust to the inclusion of age and gender controls as shown in Appendix Table 1.
[19] Further, there is no evidence of a Hawthorne effect for Controls: On average, control subjects visited the Rec Center at about the same rate before the treatment period (1.19 visits per week) as they did during the treatment period (1.08 visits per week), and the difference is not statistically significant.

social effects are implied, because these must compensate for a large risk of default. This previews a key result which we will explore in detail in Section 4C.

We now focus on the differing responses the differing incentive schemes elicited. The first evidence that the two compensation schemes evoked different responses is visible in Panel A, column 2: In apparent contradiction to predictions of the standard pecuniary model, in which the team incentive necessarily reduces an individual's expected utility, subjects randomized into the team-incentive scheme were 14.2 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. The remaining panels disaggregate the sample to explore differences in compensation schemes in more detail.

As discussed in Section 4A, we should expect heterogeneous treatment effects by type and partner type. Panels B and C restrict the sample to High types and Low types, respectively. On balance, the coefficients on Team treatment in Panels B and C reveal that High types go to the Rec Center more when incentivized as individuals and Low types show up more when incentivized in the team setting. (This is also visible in Figure 2.) Thus, whereas responses of High types appear consistent with predictions of the standard model, the responses of Low types are clearly inconsistent with these predictions and imply the existence of important behavioral effects.[20] The violation of the standard model's prediction in Panel A, then, was driven by the responses of Low types.

We investigate differences between High types and Low types by partner type in panels D and E. In Panel D, the point estimates on Team Treatment are all negative, indicating that High types go to the Rec Center less when incentivized in teams than when incentivized as

[20] Of course, a response to incentives could be considered behavioral *per se*, but by behavioral effects we mean a non-neoclassical response.

individuals, regardless of partner type. As most High types in the team treatment reached the five-visit threshold (see Figure 3), it is difficult to discern differences in behavior based on teammate type. Here, it would appear that responses to changes in expected own monetary payoff dominate, and the prediction of the standard model holds: The lower expected payout associated with team bonus compensation yields marginally lower effort. Thus, we cannot infer the existence or non-existence of social effects for High types from this first pass at the data.

Panel E, however, tells a very different story. This panel reports results for the Low 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. We observe large and statistically significant differences between Low types in the individual treatment and Low types randomly assigned High types as teammates in the group treatment. Low types with High types as partners go to the Rec Center 1.6 more times, are 22.7 percentage points more likely to go at least once, and are 26.7 percentage points more likely to meet the 5-visit bonus threshold than Low types incentivized as individuals. *This occurs despite the fact that the expected monetary pay-off is lower than in the individual Treatment.* We argue that a large non-pecuniary effect must exist for these choices to make sense at all. Low types randomly partnered with Low types behave differently from Low types matched with High types: When matched with a less active partner—as measured by pre-treatment gym visits—the subject is significantly less likely to go to the gym. We take this as clear evidence that subjects estimate the probability of default by their partners, based on observables, as suggested by the accounting exercise in Section 2. Own expected pay-out matters, but so too do social factors. In the next section we use the model of Section 2 to separate out pecuniary and social effects, and to estimate their relative magnitudes.

C. The Social Effect

It has been argued in real world settings that social effects of team incentives are large. Do our findings support this assessment, or do they suggest a modest, ignorable effect? We revisit the model of Section 2 to answer this question. The decision to complete the task and earn the $35 for person i is given by equations 1a, 1b and 1c, if she is assigned to the control, individual, or team treatments, respectively. Notice that the decision to undertake the action in the team treatment does not depend on C_j. Person *i* takes *j*'s effort level as given, ex ante. We do not model actions person i could take that directly influence *j*'s effort level or its cost. The monetary benefit to person *j* is thus the source of person *i*'s utility gain from social factors.

In the data we see about the same rate of bonus-earning in the team treatment as in the individual treatment. This observation motivates a first pass, back-of-the-envelope approximation of θ. One way for individual and team bonus-earning rates to be equal would be for the average utility derived from completing the task in the two compensation schemes to be equal:

$$(3) \qquad B = V_i - C_i + p_j B\ \ 1 + \theta\ \ ;\ \text{therefore}\ \ \theta = \frac{(1-p_j)}{p_j}\ ,$$

which gives us a simple relationship between θ and p_j. The lower p_j is, the higher θ must be, because the only way to make up for the risk of default in the team treatment is for the subject to place more weight on her teammate's pay-off. Given rational expectations, by which we mean that subjects are correct on average about teammate bonus-earning behavior, the appropriate estimate of p_j is the rate of bonus-earning in the team treatment. In the data, this rate is approximately 0.57, and thus our first pass estimate of theta is 0.75. This first pass captures a fundamental intuition. In the data, the risk of default, at 43 percent, is quite high. The only way

for there to be equal bonus-earning rates in the individual and team treatments, then, is for the social effect in the team treatment to be so large that it makes up for the default risk.

But one can go further. A more careful approach is to estimate the components of utility described above, allowing individuals to vary in their tastes and predicted behavior, based on observable characteristics. Consider the following non-linear model with a generated regressor:

(4) $$Y_i^{p*} = \alpha_0 + \alpha_1 G_i^p + \varepsilon_i \,,\; Y_i^p = 1[Y_i^{p*} > 0]$$

(5) $$Y_i^{\,*} = \delta_0 + \delta_1 G_i + \delta_2 IT_i + \delta_3 Y_i^p + v_i,\; Y_i \;\; = 1\; Y_i^{\,*} > 0 \;,$$

where $Y_i^{\,*}$ is the utility for individual *i* associated with completing the exercise 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), G_i and G_i^p are person *i* and his partners' gym attendance during the week prior to the experimental period, IT_i indicates assignment to the individual treatment group, GT_i indicates assignment to the group treatment, and ε_i and v_i are the usual probit error terms. Here, equation 4 is only for the subjects with partners (i.e., the group sample) and $Y_i^{p*} = 0$ for everyone else. 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 gym is independent of the availability of the monetary incentive. The second simplifying assumption is that beliefs about partner gym attendance are based on partner's initial observables, and that subjects do not take into consideration their partner's reactions to their own initial observables when predicting their partner's behavior. The third is that predictions of 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 gym at least five times for all individuals in the three treatments in a single equation that includes a generated regressor.

This estimation strategy leverages the experimental design in several ways to identify the components of utility (compare equation 5 with 1a-c): 1) Observed pre-treatment gym visits identify intrinsic benefits and costs associated with gym-going without compensation ($V_i - C_i = \delta_0 + \delta_1 G_i$); 2) The difference between observationally similar subjects in control and individual treatments identifies utility gains associated with own pecuniary benefits ($B = \delta_2$); 3) the difference between observationally similar subjects in individual and team treatments identifies social effects related to partners' pay-off ($1 + \theta\ B = \delta_3\ \rightarrow\ \theta = \frac{\delta_3}{\delta_2} - 1$).

We estimate the probits and bootstrap confidence intervals to account for the presence of the generated regressor. Table 4 displays results. The estimates imply that subjects received a utility gain of 1.21 utils from their own pecuniary benefit of being paid $35, and received a utility gain of 2.12 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, estimated to be 0.76 and statistically distinguishable from zero at the 5 percent level, is very similar in magnitude to our first-pass estimate of 0.75, above. The social impact of team compensation would appear to be very large, then, nearly as large as the effect of own pecuniary compensation: agents choose their effort as if they valued a marginal dollar of compensation for their teammate as much as they value 76 cents of compensation for themselves. We emphasize again that θ need not be an altruism factor. Proponents of team incentives, including the military, do not generally insist that people are blatantly unselfish. They tend to argue that teams create an opportunity for subtler social channels to operate. We have captured a broad social effect with the parameter usually used to characterize altruism

strictly in order to gauge the size of that effect. Our main contribution here is to document this large, economically significant, and heretofore unmeasured component of team incentives.

D. Social Mechanisms

The parameter θ captures incentive effects due to social interactions. 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) **Guilt, shame, altruism, 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 simple 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 altruism, and that θ in our framework could capture any of these. Guilt aversion involves an individual feeling guilty about disappointing the expectations of people who act favorably on one's behalf; the more one believes that the other people expect one to perform, the more guilty one would feel from non-performance.[21] 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.

[21] Recent papers investigating altruism (in which individuals care about the payoffs of others regardless of the perceived intentions of others) include Loewenstein, Bazerman, and Thompson (1989), Bolton (1991), Fehr and Schmidt (1999), and Bolton and Ockenfels (2000). 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.

2) **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 the future self to a plan of action; the present self would instead rather engage in a more-immediately-pleasurable activity.[22] Having a partner could remedy this problem, even if the individual does not value the teammate's payoff. If one commits oneself to exercise *with a partner*, it is more difficult for one's future self to back out. In short, individuals who have been jointly incentivized may use each other to devise commitment mechanisms.

3) **Imitation**. A model commonly used in empirical studies of peer effects posits that individuals seek to imitate the expected behavior of others in their reference group or team.[23] In this framework, individuals need not care about the pay-off to their teammates, they need not fear reprisals, nor are they assumed to require commitment mechanisms. Rather, they seek to minimize the difference between their own effort choices and the effort choices of their teammates because they derive utility from imitation or sameness.

We now briefly offer some evidence to distinguish between these channels. Mechanism 2) is that individuals with teammates may be better able to coordinate to solve commitment problems. We are able to investigate one potential source of coordination. If subjects in the group treatment coordinate with their partners by making appointments to go to the gym together, and thereby pre-commit their future selves to this activity, then the coordination should

[22] 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 (forthcoming). For empirical and experimental work on this topic, see DellaVigna and Malmendier (2006), Ashraf, Karlan, and Yin (2006), Burger, Charness, and Lynham (forthcoming), and Houser, Schunk, Winter, and Xiao (2009).

[23] For convenience of exposition, we have classified "imitation" in a separate category from "social motivations." This model yields different predictions in our setting from the models in 1) above. For models of imitation and identity, see Akerlof (1997) and Akerlof and Kranton (2000).

be visible in the timing of their visits. Specifically, subjects who are teammates should go to the gym *at the same time* more often than would randomly matched pairs of subjects in the individual treatment. We investigate this possibility in Table 5.

The Rec Center data contain information on the precise time of the gym visit. In the team treatment, there were 12 subjects (of 85) who showed up at the gym at least once at about the same-time (plus or minus 10 minutes) as a teammate. For comparative purposes, we randomly assigned placebo 'teammates' to subjects in the individual treatment. In the individual treatment, there were seven subjects who showed up at the Rec Center within 10 minutes of their placebo teammate at least once. But there were more than twice as many subjects in the team treatment as in the individual treatment. Thus, the probability that a teammate pair would have a coincident gym visit was actually slighter higher for a 'placebo' team in the individual treatment than for a true team in the group treatment.[24] We find no evidence, then, of higher rates of gym-visit coordination in the team treatment. This finding also suggests that there were no production spillovers in the team treatment. Subjects did not leverage coordinated visits to enhance their exercise productivity.

Mechanism 3) emphasized imitation. In Table 3, Low types matched with Low types are 24.2 percentage points more likely to try going to the Rec Center at least once than Low types in the individual treatment. This would seem to argue against imitation: In the standard peer-effects story of imitation, low-performers who have low-performing peers do not improve their performance. However this is consistent with social mechanisms that emphasize guilt, shame, altruism, or fear of reprisal. Interestingly, though, Low types matched with Low types appear no more likely to reach the five-visit threshold. This would appear consistent with a story in which teammates learn about each other over time: The subject perceives his teammate's type more

[24] Similar results hold if we investigate subjects who went to the gym at the same time at least twice.

accurately as time passes. A subject who discerns from observable characteristics (or through conversation) that his teammate is a Low type may be less concerned about defaulting because default is less likely to cost the teammate a bonus. If one does not expect that one's teammate will meet the requirements for the bonus, then there is no reason to worry about defaulting and there is no way to raise the partner's pay-off.

We do not find strong evidence, then, that social effects of team incentives arise from a need for commitment mechanisms related to joint gym attendance or the desire to imitate. 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 and other channels is a subject for future research.

5. Conclusion

In summary, we find that social effects of team incentives are nearly as large, on average, as the effects of direct pecuniary compensation. Team compensation induced agents to choose their effort as if they valued a marginal dollar of compensation for their teammate three-fourths as much as they value a dollar of their own compensation. Subjects were also are more likely to attempt an effort-intensive task when motivated as part of a team. The findings are consistent with some form of social motivation, and less consistent with simple peer imitation or coordination to solve commitment problems.

We do not claim to have shown that team incentives are preferable to piece rates. We have abstracted from important factors in order to study a social component. We note, though, that in our context, effort is observable and piece rates could be expected to dominate team incentives. Even in this setting, effort elicited is just as high with team incentives as with piece

rates. In workplace settings where effort is not observable, piece rates are not an option. Teams may offer distinct advantages there.

Moral hazard in the form of free-riding, a clear disadvantage of team compensation, has been studied extensively and found to exist. Why, then, are teams and team incentives used so often? One explanation is that teams take advantage of complementarities in worker productivity by fostering specialization, cooperation, and/or knowledge transfers. It has been argued that production complementarities are essential for team incentives to work. Our finding is that social effects do not seem to require production complementarities, and thus that there may be room for team incentives, even in artificial teams where production complementarities are absent. This may be the case because team compensation harnesses a social mechanism, in addition to free-rider effect and productions complementarities, that alters workers' willingness to put forth effort. Management consultants allege that this kind of social effect is very powerful. If this is true, in part or in total, then it should be taken into account in any understanding of personnel economics.

A first step is to observe the effect in a simple setting that allows for rigorous causal inference but also preserves the possibility of repeated social interactions over time. Previous studies have sometimes found social effects not tied to teams and sometimes found team effects that can't be proven to be social, but none of these appear to have identified social effects of monetary team incentives. Our primary contribution is that we demonstrate the existence of a social effect of team compensation: We observe people in a real-world setting raising their effort level because a teammate's payoff is at stake.

Ultimately, this addresses the question of how best to use incentives to evoke a desired performance at lowest cost. We have just scratched the surface on this crucial issue in personnel

economics. Future work will help to delineate the boundaries of the observed effect and the environments under which team incentives are most effective.

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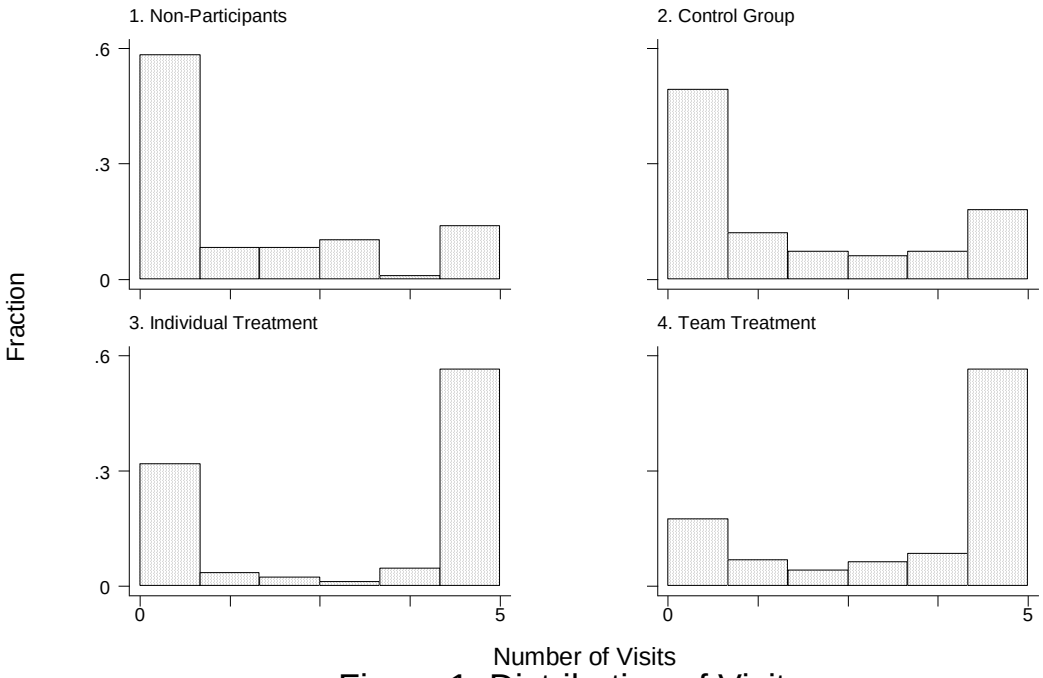

Figure 1. Distribution of Visits

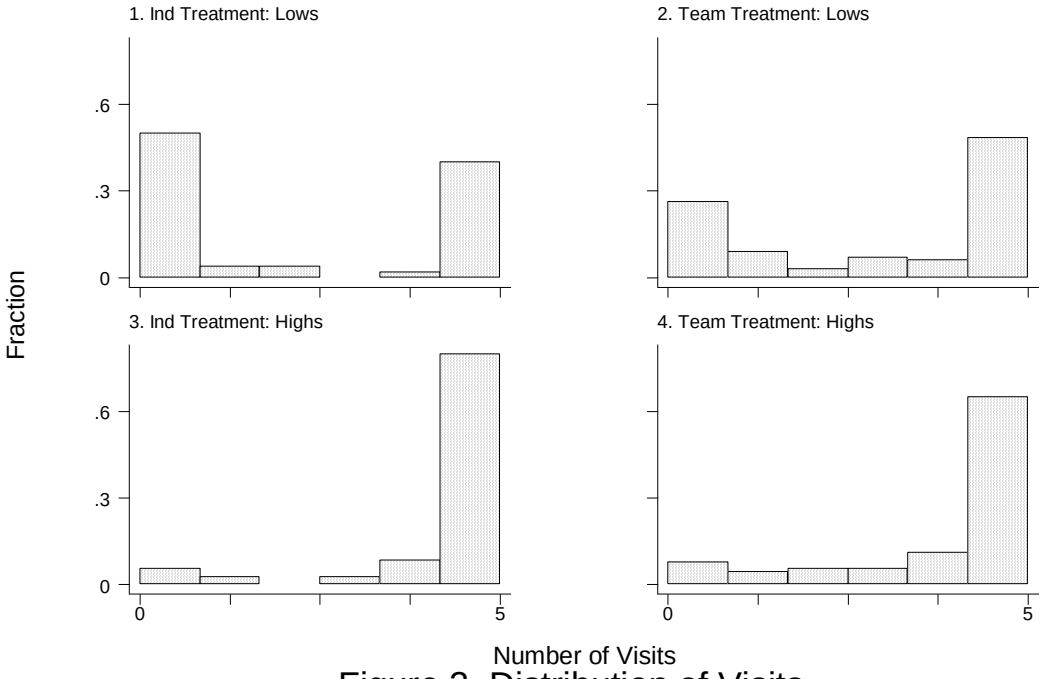

Figure 2. Distribution of Visits

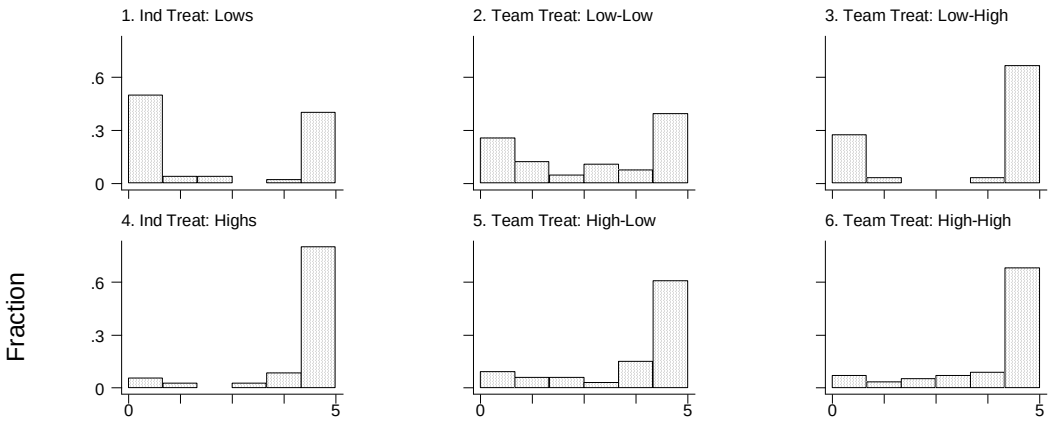

Figure 3. Distribution of Visits

Table 1. 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 early leaver and contaminated partners		490	373
Removing duplicates and their partners		478	362
Removing respondent who gave a fake name		477	362
Removing respondents with missing data		464	356

Table 2. Survey Response and Experiment Participation

	Male	Age	Self-Reported Exercise	Previous Gym Visits	Lows	Highs	Sample Size
Panel A: Classroom Survey Response							
Sample Means							
Control (C)	0.60	21.21	3.85	1.02	0.61	0.39	105
	(0.49)	(2.27)	(2.24)	(1.57)	(0.49)	(0.49)	
Individual Treatment (IT)	0.58	21.39	4.33	1.04	0.61	0.39	113
	(0.50)	(2.74)	(2.64)	(1.58)	(0.49)	(0.49)	
Team Treatment (TT)	0.65	21.05	4.18	1.04	0.54	0.46	246
	(0.48)	(2.10)	(2.49)	(1.40)	(0.50)	(0.50)	
Mean Differences (P-Values)							
IT - C	0.71	0.60	0.15	0.91	0.99	0.99	
TT - C	0.38	0.54	0.23	0.90	0.26	0.26	
TT - IT	0.18	0.24	0.62	0.98	0.24	0.24	
Panel B: Experiment Participation							
Sample Means							
Non-Participants (NP)	0.64	21.24	4.14	0.80	0.66	0.34	108
	(0.48)	(2.46)	(2.52)	(1.32)	(0.48)	(0.48)	
Participants (P)	0.62	21.15	4.14	1.11	0.55	0.45	356
	(0.49)	(2.26)	(2.47)	(1.52)	(0.50)	(0.50)	
Mean Differences (P-Values)							
P - NP	0.66	0.72	0.99	0.04	0.05	0.05	
Panel C: Treatment Assignment Conditional on Participation							
Sample Means							
Control	0.55	21.18	4.00	1.19	0.57	0.43	83
	(0.50)	(2.46)	(2.31)	(1.68)	(0.50)	(0.50)	
Individual Treatment	0.55	21.24	4.18	1.15	0.59	0.41	85
	(0.50)	(2.20)	(2.51)	(1.67)	(0.50)	(0.50)	
Team Treatment	0.67	21.09	4.18	1.05	0.53	0.47	188
	(0.47)	(2.21)	(2.52)	(1.38)	(0.50)	(0.50)	
Mean Differences (P-Values)							
IT - C	0.99	0.88	0.62	0.88	0.77	0.77	
TT - C	0.08	0.77	0.56	0.51	0.55	0.55	
TT - IT	0.07	0.62	1.00	0.63	0.34	0.34	

Standard deviations are in parentheses. P-Values are for two-sided t-tests assuming unequal variances.

Table 3. Rec Center Visits for Individual and Team Treatments

	Visits	Try	Bonus	Sample Size
Panel A				356
Any Treatment	1.714**	0.176**	0.384**	
	(0.485)	(0.075)	(0.069)	
Team Treatment	0.305	0.142**	-0.001	
	(0.418)	(0.059)	(0.067)	
Constant	2.157**	0.506**	0.181**	
	(0.334)	(0.055)	(0.042)	
Panel B: Sample Restricted to Highs				160
Any Treatment	1.750**	0.026	0.411**	
	(0.701)	(0.061)	(0.107)	
Team Treatment	-0.978*	-0.022	-0.148*	
	(0.545)	(0.051)	(0.088)	
Constant	4.250**	0.917**	0.389**	
	(0.530)	(0.047)	(0.082)	
Panel C: Sample Restricted to Lows				196
Any Treatment	1.827**	0.309**	0.379**	
	(0.453)	(0.092)	(0.073)	
Team Treatment	1.034**	0.237**	0.085	
	(0.490)	(0.086)	(0.089)	
Constant	0.553**	0.191**	0.021	
	(0.240)	(0.058)	(0.021)	
Panel D: Sample Restricted to Highs				160
Any Treatment	1.750**	0.026	0.411**	
	(0.703)	(0.061)	(0.107)	
Team Treatment: Low Partner	-1.000	-0.034	-0.194*	
	(0.681)	(0.064)	(0.110)	
Team Treatment: High Partner	-0.964	-0.014	-0.121	
	(0.588)	(0.058)	(0.100)	
Constant	4.250**	0.917**	0.389**	
	(0.532)	(0.047)	(0.082)	
Panel E: Sample Restricted to Lows				196
Any Treatment	1.827**	0.309**	0.379**	
	(0.454)	(0.092)	(0.073)	
Team Treatment: Low Partner	0.772	0.242**	-0.006	
	(0.545)	(0.094)	(0.097)	
Team Treatment: High Partner	1.559**	0.227**	0.267**	
	(0.615)	(0.106)	(0.109)	
Constant	0.553**	0.191**	0.021	
	(0.241)	(0.058)	(0.021)	

Standard errors are clustered at the group level and reported in parentheses. ** (*) indicates statistically significant at the 5 (10) percent level.

Table 4. Structural Parameter Estimates

	Coefficient	95% Percentile Lower Bound	95% Percentile Upper Bound
Own pre-period gym visits	0.314	0.227	0.418
Predicted partner gym visits	2.124	1.464	2.969
Individual treatment indicator	1.206	0.789	1.665
Constant	-1.355	-1.738	-1.023
Theta	0.762	0.377	1.315

1000 bootstrap replications.

Table 5. Incidence of Pairs Visiting the Rec Center Together

Number of Same Time Visits	Individual Treatment	Team Treatment
0	78	176
	(91.8)	(93.6)
1	5	10
	(5.9)	(5.3)
2	2	2
	(2.4)	(1.1)
Total	85	188

Percentage of visits in pairs in parentheses. Individual treatment 'pairs' are randomly matched within class. Same time defined as a plus or minus ten minutes.

Appendix Table 1. Rec Center Visits for Individual and Team Treatments

	Visits	Try	Bonus	Sample Size
Panel A				356
Any Treatment	1.786**	0.186**	0.394**	
	(0.396)	(0.061)	(0.061)	
Team Treatment	0.051	0.106	-0.036	
	(0.358)	(0.053)	(0.061)	
Inactive	-2.334**	-0.338**	-0.226**	
	(0.315)	(0.044)	(0.054)	
Male	0.497	0.062	0.101**	
	(0.292)	(0.044)	(0.049)	
Age 20	-1.0481**	-0.146**	-0.119	
	(0.482)	(0.054)	(0.077)	
Age 21	-0.921*	-0.096*	-0.132	
	(0.475)	(0.054)	(0.075)	
Age 22+	-1.750**	-0.252**	-0.265**	
	(0.518)	(0.063)	(0.077)	
Constant	4.245**	0.801**	0.398**	
	(0.577)	(0.068)	(0.085)	
Panel B: Sample Restricted to Highs				160
Any Treatment	1.744**	0.031	0.422**	
	(0.702)	(0.061)	(0.107)	
Team Treatment	-0.971*	-0.019	-0.156*	
	(0.543)	(0.048)	(0.085)	
Male	0.350	0.029	0.137	
	(0.505)	(0.043)	(0.086)	
Age 20	-0.641	-0.031	-0.054	
	(0.697)	(0.051)	(0.106)	
Age 21	-0.822	-0.001	-0.095	
	(0.670)	(0.044)	(0.103)	
Age 22+	-1.501*	-0.147*	-0.205*	
	(0.822)	(0.084)	(0.122)	
Constant	4.755**	0.930**	0.374**	
	(0.887)	(0.066)	(0.137)	
Panel C: Sample Restricted to Lows				196
Any Treatment	1.855**	0.319**	0.386**	
	(0.440)	(0.090)	(0.071)	
Team Treatment	0.842*	0.208**	0.055	
	(0.473)	(0.086)	(0.087)	
Male	0.454	0.054	0.068	
	(0.351)	(0.065)	(0.062)	
Age 20	-1.264**	-0.234**	-0.160	
	(0.632)	(0.097)	(0.109)	
Age 21	-1.141*	-0.238**	-0.169	
	(0.616)	(0.093)	(0.105)	
Age 22+	-1.866**	-0.343**	-0.296**	
	(0.619)	(0.090)	(0.098)	
Constant	1.636**	0.408**	0.179**	
	(0.643)	(0.094)	(0.091)	

Age 19 or less, female, control group, and in Panel A Active, are the omitted categories. Standard errors are clustered at the group level and reported in parentheses. ** (*) indicates statistically significant at the 5 (10) percent level.

Appendix Table 1 (Continued). Rec Center Visits for Individual and Team Treatments

	Visits	Try	Bonus	Sample Size
Panel D: Sample Restricted to Highs				160
Any Treatment	1.744**	0.031	0.422**	
	(0.704)	(0.061)	(0.107)	
Team Treatment: Low Partner	-0.996	-0.033	-0.197*	
	(0.679)	(0.063)	(0.107)	
Team Treatment: High Partner	-0.957	-0.010	-0.132	
	(0.579)	(0.054)	(0.097)	
Male	0.347	0.028	0.132	
	(0.503)	(0.045)	(0.086)	
Age 20	-0.641	-0.031	-0.054	
	(0.699)	(0.050)	(0.106)	
Age 21	-0.822	-0.001	-0.095	
	(0.673)	(0.044)	(0.103)	
Age 22+	-1.503*	-0.148*	-0.207*	
	(0.821)	(0.084)	(0.122)	
Constant	4.757**	0.931**	0.378**	
	(0.886)	(0.067)	(0.136)	
Panel E: Sample Restricted to Lows				196
Any Treatment	1.868**	0.319**	0.391**	
	(0.441)	(0.091)	(0.071)	
Team Treatment: Low Partner	0.565	0.207**	-0.039	
	(0.512)	(0.093)	(0.092)	
Team Treatment: High Partner	1.425**	0.208*	0.252**	
	(0.629)	(0.108)	(0.112)	
Male	0.357	0.054	0.035	
	(0.358)	(0.066)	(0.061)	
Age 20	-1.310**	-0.234**	-0.175	
	(0.640)	(0.097)	(0.112)	
Age 21	-1.191*	-0.238**	-0.186*	
	(0.621)	(0.093)	(0.106)	
Age 22+	-1.924**	-0.343**	-0.315**	
	(0.617)	(0.090)	(0.098)	
Constant	1.718**	0.408**	0.207**	
	(0.646)	(0.095)	(0.092)	

Age 19 or less, female, control group, and in Panel A Active, are the omitted categories. Standard errors are clustered at the group level and reported in parentheses. ** (*) indicates statistically significant at the 5 (10) percent level.

Appendix 1

Survey and Consent Forms

1. In-Class Consent Form and Survey

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

A1*

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.

<u>We would also like to ask you a few questions</u>:

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.

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.

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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.

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. You have also been matched with another person for this part of the study. If both of you attend the Rec Center at least five different days from August 7-20, 2010, you will each 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 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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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 _____