

**Coordination and Contagion:
Peer Effects and Mechanisms in a Randomized Field Experiment**

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Abstract

This paper investigates peer effects at the level of individual connections and leverages the approach to shed light on peer mechanisms. In a field experiment conducted on college freshmen, we elicited friendship networks and offered monetary incentives for using the recreation center to a treated subset. We find large spillovers from treated subjects to treated best friends but no spillover from treated subjects to control best friends. We also find clear evidence of a mechanism: Subjects coordinate with best friends to overcome pre-commitment problems or reduce effort costs. Results highlight subtle peer effects and mechanisms that often go undetected.

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1. Introduction

In settings where social ties are salient, data on the microstructure of social connections may be essential to move beyond “black box” models of spillovers. The goal of this paper is to investigate peer effects at the level of individual connections—arguably, the level at which these effects are most likely to operate—and to leverage this approach to shed light on peer mechanisms. Previous work on peer effects has often relied on variation in peer reference groups that have been externally imposed or defined, rather than on self-identified friends. Since most real-world connections are endogenously formed, it is worth augmenting this research with studies of spillovers between individuals whose connections were self-chosen. This paper adds to previous research both because it penetrates the black box to identify peer mechanisms, and because it features exogenous variation in peer behavior while allowing peer effects to operate through chosen social connections.

We incentivize a random subset of college students by offering to pay them for repeated visits to the campus recreation center, as in Charness and Gneezy (2009) and Acland and Levy (2010), and use this carefully controlled setting to study spillovers and mechanisms. Much important research on peer effects has relied on random variation in assigned classes, grade-levels, roommates, residence halls, squadrons, or other imposed groupings (Sacerdote, 2001, Zimmerman 2003, Boisjoly et al, 2006, Foster 2006, Lyle, 2007, Kremer and Levy, 2008, Carrell, Fullerton, and West, 2009, Carrell, Hoekstra, and West, 2011), rather than reference groups of self-chosen peers. Some recent studies of spillovers in fitness and obesity have focused on naturally occurring social connections, but these have tended to lack plausibly exogenous sources of variation. Christakis and Fowler (2007, 2008), for example, argue that social spillovers in obesity are large, but their results have been criticized by Cohen-Cole and Fletcher (2008a and 2008b) due to the endogeneity of friendship formations used in the study. A unique feature of our experiment is that prior to treatment, we elicit a detailed friendship network from the subjects. Random assignment of treatment creates random variation in the treatment status of the peers to

whom the subject is exposed, and from this we identify peer effects. The methodology we propose can be applied in other interventions, particularly for settings in which subjects have the potential to be connected to one another by naturally occurring social ties. These could include school-level interventions to raise test scores, promote college enrollment decisions, combat drug and alcohol abuse, or raise civic participation; workplace interventions to promote health or productivity; village-level interventions to improve sanitation, prevent disease, or raise school attendance; and other effort-elicitation contexts in which there is an opportunity for repeated social interaction over time.

Our main findings are that treated subjects with treated best friends put forth significantly more effort toward the incentivized task than do treated subjects with control best friends. The peer effect is about 20% as large the direct individual effect of the incentive. There is no observed spillover for control subjects, and thus a partial population experiment that lacked data on individual connections would not have isolated this peer effect. We do not detect a statistically significant influence of room-level peers, suggesting that studies based on imposed groupings may sometimes miss important spillovers. Further, we find evidence of a mechanism that explains the entire peer effect: Subjects coordinate with their best friends to help them overcome pre-commitment problems or reduce effort costs.

2. Background

Our strategy for investigating the well known problems involved in identifying social effects is based largely on Moffitt's (2001) description of a partial population intervention. Partial population designs have recently been used to estimate spillovers hypothesized to arise from information transfer in schools or departments (Duflo and Saez, 2003, Miguel and Kremer, 2004), from imitation or social insurance between households (Angelucci and DiGiorgio, 2009, and Kuhn *et al.*, forthcoming) and from learning or imitation in schooling decisions (Bobonis and Finan, 2009, Lalive and Cattaneo, 2009). One could conceptualize an ideal partial population

design as one which begins with a sample of social groups or villages between which there are no spillovers. Within the groups or villages designated for treatment, a random subset of agents or households is treated. Differences in group-level means in this setting identify several quantities of interest: a) The difference between mean outcomes for treated and control villages captures average group-level treatment effects; b) The difference between mean outcomes of treated subjects in treated villages and control subjects in untreated villages estimates the average effect of village treatment on treated subjects; c) The difference in the mean outcomes of untreated subjects in treatment and control villages captures the effect of village treatment on untreated subjects.

Though the partial population experiment is a powerful tool for introducing exogenous variation into settings with social spillovers, in practice there are several limitations associated with the design of most recent partial population experiments: 1) Agents are assumed to respond to mean behavior of the reference group, because the internal structure of the group is unknown. But a subject is not likely to be influenced by a “peer” he barely knows, even if both agents are a part of the same reference group (e.g., a class, a grade, or a village). When data on individual ties between individual agents is lacking, significant noise may be present. 2) The driving mechanism is often unclear. A spillover may be observed from treated subjects to controls, but there is often little that can be done to infer the mechanism. 3) Usually, only spillovers from treated subjects to controls are isolated and estimated.

To flesh out 3), we note that in the recent literature there are a number of concrete examples of partial population experiments that do not identify certain kinds of peer effects. In Kuhn *et al.* (forthcoming), the authors estimate consumption spillovers on lottery non-participants resulting from exposure to lottery winners. But they cannot detect whether lottery winners in a winning postal code influence each others’ consumption. Angelucci and DeGiorgi (2009) estimate consumption spillovers from households eligible for transfers to wealthier, ineligible households, but cannot estimate spillovers between eligible households. Lalive and Cattaneo

(2009), using similar data, summarize concerns about precisely this kind of spillover between the treated (p. 460): “If children from poor households only interact with other children from poor households, there could be important social spillover effects that can not be detected with the PROGRESA experiment.” It bears emphasizing that the problem the authors describe is not unique to the PROGRESA experiment, but is a general feature of partial population interventions that do not collect data on connections between individuals.

By incorporating social network data into the framework of a partial population experiment, we attempt to address these challenges. One advantage of our design is that we randomize at the individual level. This may be useful for several reasons. 1) Ultimately, this may be where the most relevant action can be observed: Individuals influence individuals. It is unlikely that individuals compute the average behavior of a large group and then respond to that. It would seem much more likely that they respond strongly to certain individuals in the group and are much less influenced by the rest. All peers are not equal. 2) Data on friendship networks combined with treatment assignment at the individual level allow us to shed light on peer mechanisms. 3) Our experiment will highlight spillovers that are present even though there is no discernible effect of group treatment on controls, i.e., *when the difference in group means described in c) of an idealized partial population experiment is zero.*¹

Two previous studies that we know of use friendship networks to examine spillovers in real world settings.² Miguel and Kremer (2007) elicit friendship networks from some of the subjects in the intestinal worms intervention (those who were exposed to the program later). They examine how these ties predict subjects’ decisions to take up treatment. Our paper resembles this

¹ We note that it is possible, even without network data, to calculate spillovers from treated subjects to treated subjects in a partial population experiment by running experiments in many different groups or villages and randomizing fraction treated (e.g., Philipson, 2000). However, network data allow the estimation of social spillovers using much less data and at less expense. If running experiments in enough villages to get statistical power is logistically challenging and expensive, or if the immediate social circle is likely to be a more salient reference group than the village, then an approach that relies on exogenous variation in treatment across individual social ties offers advantages.

² In addition, there exists important research using real world social networks to explore altruism and reciprocity in experimental games. See Leider *et al.* (2009).

work in the use of networks, but differs in that our design is based on randomization at the individual level and that we elicit ties from all participating subjects. Similarly, Oster and Thornton (forthcoming) elicit friendship ties of at most 3 friends and estimate spillovers in menstrual cup usage. There, the setting allows for the analysis to be simplified in a way that is not always practical in other environments: Spillovers for the controls are not estimated (and may have been implausible, because controls were not provided with the product.) These important and innovative studies provide strong evidence that social spillovers matter. They do not attempt to estimate treatment-status-specific peer effects for all subjects using individual data. Our work leverages a setting in which spillovers may be estimated both for treated subjects and controls. We also analyze individual data for all subjects in order to explore the peer mechanism directly and extensively.

3. Experimental Design

Wave I of the experiment entailed surveying 838 incoming freshman students in Santa Catalina Residence Hall at the University of California, Santa Barbara. Students were recruited at our table outside the dormitory dining hall from Jan. 14 to Jan. 20 in 2011, and were paid \$5 for filling out a survey and giving consent to be contacted later. They were also entered in a lottery for an iPod Nano, retail value \$149.00. The dormitory had 1178 residents, so the 838 students represented about 70% of the student population in the residence hall.

In Wave II, which took place Jan. 27-Feb. 3, we administered a friendship survey to the 614 students who responded to our emails or returned to our table at the residence hall, and so remained in the experiment. About 73% of the original sample (614 of 838 students) went on to participate fully in the experiment.³ This was more than half of the student population in Santa Catalina residence hall.

³ 614 students showed up to fill out the friendship survey. A few were dropped for data and compliance reasons: Two students indicated that they had no friends. In addition, 25 students who chose friends did not

In the friendship survey, which participants filled out online or on a laptop at the residence hall, students were instructed to click on the checkboxes next to the names of their friends. The list of names from which they chose included only students in the experiment, and they were shown at most 50 names per screen. Subjects were paid \$8 for filling out the friendship survey. Their treatment or control assignment was revealed to students after they filled out their friendship surveys. Subjects randomized into the treatment group were informed they would be paid \$60 if they visited the University Recreation Center (“Rec Center”) 8 times or more between Feb. 6 and Mar. 6.

Whenever anyone wishes to enter the Rec Center, the attendant at the front desk takes his or her student photo ID card and electronically scans it. The student photo is clearly visible to the attendant who scans the card and only one card can be presented per student. (Thus, it was extremely difficult, if not impossible, for subjects in our experiment to cheat by using someone else’s card or by scanning multiple cards for a single visit.) The time, date, and student card barcode of every gym entry is stored electronically. This was the source of data on recreation center visits. At the end of the treatment period, we paid \$60 to each subject in the treatment group who had visited the facility eight times or more in the allotted period.

Several unique aspects of the research design in this pay-for-exercise experiment merit comment, as they relate directly to the identification strategy. The recreation center was about a 25-30 minute walk from the residence hall. Thus, visits to the recreation center involved nontrivial commitments of time and effort. Subjects were recruited from the same dormitory so that they would be likely to know some of the other subjects in the experiment. More than 50% of the students living in the dormitory participated in the experiment. Students were surveyed in the middle of the academic year (January), so that having lived together in the same residence hall for several months, they would have had time to form meaningful friendships. Friendship networks

comply fully by denoting which of their friends was their best friend. These not fully complying subjects, along with the 2 isolates, were dropped from the main sample and treated as non-participants, leaving 614.

were elicited before students learned their treatment assignment. Thus, the measured friendship networks were not influenced by treatment or treatment status. Random assignment of treatment served two purposes in this experiment. It induced random variation in exposure to treatment, which allowed identification of individual effects, and it induced random variation in exposure to treated and untreated peers, which allowed identification of spillover effects. We emphasize that the main purpose of the experiment was not to perform a full-scale evaluation of pay-for-exercise as a policy to improve long-run health outcomes on college campuses. The important issues of habit formation and health effects in interventions of this kind have already been explored carefully in previous work.⁴ The analysis here focuses on peer effects and the fundamental behaviors that drive them in an effort-elicitation setting.

Table 1, Panel A compares demographic characteristics of the 614 participants with the 224 non-participants (by which we mean the students who filled out an initial survey but did not go on to Wave II and participate fully.) There were no statistically significant differences between participants and non-participants, except that more males chose not to return for Wave II. Table 1, Panel B shows summary statistics for participants in treatment and control groups of the Santa Catalina experiment, by age, race, gender, financial aid status, pre-treatment recreation center usage, pre-treatment self-reported exercise, number of friends, and treatment status of best friend. The composition of control and treatment groups appears very similar in most categories, and none of the differences between the groups is statistically significant. Subjects in the Santa Catalina network report having about 18 friends, on average. This may seem a high number of friends and there may be some question as to whether all of these friendships are people with whom the subject interacts frequently. We will revisit this idea in Section 4.F.

What may be striking about the Santa Catalina Residence Hall is its social connectedness.

Figure 1, a histogram of degrees of separation between subjects, fleshes this out. There is

⁴ Charness and Gneezy (2009) find evidence of habit formation and health benefits in a short-term exercise intervention very similar to ours. Acland and Levy (2010) also find some evidence of habit formation, though the acquired habits fade quickly over time.

essentially one cluster in the Santa Catalina network.⁵ Every subject who reported having any friends is connected to every other subject by 5 or fewer degrees of separation. Over 90% of subject pairs are connected with 3 or fewer degrees of separation. The average number of degrees of separation between any two subjects is 2.7. In short, students who were not friends with each other were nevertheless very likely to be friends of friends, or friends of friends of friends.

4. Results

A. Individual effects

Before we investigate whether the intervention caused effort spillovers, we first estimate the overall effect of treatment on effort outcomes. Throughout the analysis, we focus on two outcomes: The first is the number of times the subject visited the Rec Center in the treatment period. Figure 2A shows Rec Center usage for treated and control subjects before and during the treatment period. Control subjects and treated subjects made 2.16 and 2.3 recreation center visits, respectively, in the 4-week pre-treatment period. During the 4-week treatment period, control subjects visited the recreation center 2.4 times, while treated subjects increased their usage dramatically to 7.3 visits. This 5-visit difference in differences is highly significant (with $p\text{-value} < 0.001$).

The second effort measure is whether the subject visited the Rec Center at least 8 times in a 4-week period, as would be required for a treated subject to receive payment in the treatment period. Figure 2B shows the fraction of subjects who reached the 8-visit threshold for treated and control subjects before and during the treatment period. 11% of control subjects and 10% of treated subjects went to the Rec Center 8 times or more in the 4-week pre-treatment period. During the 4-week treatment period, the fraction of control subjects visiting the recreation center 8 times or more stayed constant at 10%, whereas fully 63% of treated subjects reached the payment threshold. This difference in differences is again highly significant (with $p\text{-value} < 0.001$).

⁵ This is conditional on the fact that the 2 subjects with no friends at all were dropped.

value <0.001). We conclude that treatment incentivized subjects to visit the Rec Center significantly more than they would have otherwise.⁶

B. Reference Groups

To investigate peer effects one first needs to specify a reference group. One could worry that definitions of “friendship” are subjective and vary between subjects. This is a particularly important concern in the Facebook era, in which students often report having hundreds of Facebook “friends” with whom they rarely, if ever, interact. To mitigate concerns that definitions of “friendship” differed between subjects, we focus on the individual’s best friend.

Best friend could be defined in several ways. One’s best friend may be defined as the person the subject denotes as his best friend on the friendship survey. A best friend could also be defined as an individual who chooses the subject as best friend. Thirdly, a best friend may be defined as an individual who has a reciprocated best-friend relationship with the subject. Table 2, a first pass at the data, shows differences in mean effort outcomes by subject treatment and best friend treatment for all three definitions of best friend.

Panel A shows mean effort outcomes for treated subjects. Treated subjects visited the Rec Center 7.7 times in the treatment period if their self-reported best friend was treated and 6.7 times if their self-reported best friend was not treated. Thus, they made about 1 more visit to the Rec Center if their best friend was offered the financial incentive. The difference in means is significant at the 2% level. Panel A also shows that treated subjects were 6 percentage points more likely to reach the 8-visit threshold if their self-reported best friend was treated. Though this estimate lacks precision, we will demonstrate that for the types of students most likely to be on

⁶ In order to explore spillovers, we have focused on a fundamental and objectively measurable behavioral response: observed effort outcomes. We do not make strong claims about whether increased Rec Center visits led to better long-run health outcomes or led to more exercise, overall. It is possible that students substituted from one form of exercise to another, or that they came to the Rec Center and did little or no exercise. Going to the Rec Center, itself, required effort. We note, however, that a pilot study, discussed in more detail in section 4.F, showed that students’ self-reported level of overall exercise did, in fact, rise significantly when they were compensated for Rec Center visits, as in this study.

the margin, having a treated best friend did significantly increase the probability of reaching the payment threshold.

Similar results hold when best friend is defined as reported by the friend, rather than the subject; however the difference in mean effort outcomes between treated subjects with treated best friends and treated subjects with control best friends is smaller and less precisely estimated. Sample size is smaller because some subjects did not receive any best friend nominations. For reciprocated friendships the difference in Rec Center visits between treated subjects with treated best friends and treated subjects with control best friends is largest, at about 1.2 visits. The sample size is smallest for this definition of best friend, because best friend designations are reciprocated only about a third of the time. Lack of reciprocation is a common feature of social networks and not an idiosyncrasy of the Santa Catalina Residence Hall. In the National Longitudinal Survey of Adolescent Health, for example, best friend nominations are reciprocated about a third of the time, as here (Card and Giuliano, 2011).

All three definitions of best friend yield similar results for treated subjects. As might be expected, self-reported best friends appear to have greater influence on subjects than friend-reported best friends, and reciprocated best friends appear the most influential. For the remainder of the paper, we focus on self-reported best friends, as this allows for the largest sample sizes and appears to be a salient measure of friendship.

Panel B shows differences in effort outcomes by best friend treatment status for control subjects. Interestingly, there are no statistically significant differences in Rec Center visits or in reaching the 8-visit threshold between control subjects with treated best friends and control subjects with control best friends. Having a best friend randomly assigned to treatment appears to alter behavior for treated subjects but not for controls.

There are, of course, other reference groups one could have chosen. Subjects could be influenced by the fraction of their roommates or full set of friends that are treated. They could be influenced by the friends of their friends. We find no clear evidence of peer effects in these

reference groups.⁷ This may be related to the connectedness of the Santa Catalina network. It may be the case that when students name a large number of friends, only some of those friends are people the subject interacts with frequently or is influenced by. Recent data from Facebook, for example, suggest that agents typically identify many people as friends that they do not interact with at all.⁸ In such a setting, the best friend designation is apt to be particularly meaningful, distinguishing a friend who has been integrated into the subject's life from someone on the far periphery. We will investigate this idea more carefully in Section 4.F, and bring evidence to bear on it, by examining data from a pilot study that featured a less-connected network.

A first pass at the data produces several main findings: 1) Subjects go to the Rec Center 5 more times in the treatment period if treated. 2) Treated subjects visit the Rec Center 1 more time if their best friend was randomly assigned to treatment. 3) There is no visible peer effect for control subjects. It is worth noting that the observed peer effect, a spillover from treated subjects to treated subjects, would not have been identified by a standard partial population experimental design, where only spillovers from treated subject to controls are identified.

A number of questions remain. Why do effects differ by treatment status? What types of subjects are most responsive to best friend spillovers? How large is the peer effect as a social multiplier, and is it robust to alternative specifications? What is the peer effect mechanism? How dependent are the results on the experimental design? In the remainder of the paper we will investigate each of these questions in turn.

C. Peer Effects and Recreation Visits

In a standard story of time-inconsistent preferences, agents may wish to go to the gym more, but do not do so because their present selves and future selves are in conflict. There is some

⁷ We report summaries of regressions of Rec. Center visits on the fraction of roommates treated in section 4.D. Supporting regressions for other measures are available from the authors upon request.

⁸ The Economist, Feb 26th 2009.

evidence of this phenomenon in an exercise setting.⁹ If having a financially incentivized best friend creates opportunities for pre-commitment to Rec Center usage (or lowers the barriers to Rec Center usage in some other way), one might expect best friend treatment status to influence a subject's Rec Center usage, as here. The absence of a spillover for subjects in the control group would make sense if financial remuneration is the main reason subjects wish to alter their Rec Center usage in the first place. For control subjects, who may have no intrinsic desire to go to the Rec Center more often, there exists no commitment problem or other obstacle to be overcome, no conflict between present and future self, and thus no marginal gain to having a treated best friend. Treated subjects, in contrast, may wish to go to the Rec Center because of the financial incentive provided, and may thus benefit from having a best friend to help them muster the discipline to do it.

We can explore this possibility further by measuring heterogeneous treatment effects and spillovers by income. If financial remuneration is the primary motivator, then subjects with fewer resources, i.e., those with higher marginal utility of wealth, may be most responsive to the incentive and the spillovers. Table 3 duplicates Table 2, except we divide subjects into four groups, based on their financial aid status: Subjects receiving financial aid whose best friends are also receiving financial aid; those on financial aid with best friends who are not; those who are not receiving financial aid but whose best friends are; and pairings in which neither person receives financial aid. Treated students on financial aid matched with best friends who were also on financial aid visited the Rec Center 2.3 more times and were 17 percentage points more likely to reach the payment threshold when their best friend was treated. These differences in means were statistically significant, with $p\text{-value} < .001$ and $.07$, respectively. No other pairings produced spillovers large enough to be statistically discernable at conventional levels.

Table 1 shows that subjects visited the Rec Center 2.3 times on average during the pre-treatment period. However, a large fraction of subjects (56%) did not visit the Rec Center at all in the month before the treatment period. Conditional on going at least once, subjects averaged 5

⁹ DellaVigna and Malmendier, 2006.

pre-treatment Rec Center visits. These two groups, who we call “Users” and “Non-users,” may be expected to respond in different ways to best friend treatment status. Specifically, subjects without a history or habit of Rec Center usage may be more dependent on external motivators to get them there than subjects who used the Rec Center in the absence of financial incentives. In short, Non-users may be the marginal subjects, those for whom a peer nudge would be decisive.

Table 4 duplicates Table 3, except that we divide subjects into four groups, based on their pre-treatment Rec Center usage: Rec Center Non-users whose best friends are also Non-users; Non-users whose best friends are Users; Users with Non-user best friends; and Users with User best friends. Panel A reveals that treated Non-users with Non-user best friends visited the Rec Center 2.2 more times and were 26 percentage points more likely to reach the payment threshold when the best friend was treated. Both of these differences in means are statistically significant, with $p\text{-value} < .001$. No other pairings produced spillovers large enough to be statistically discernable at conventional levels.¹⁰

In summary, best friend treatment status is most important when both the subject and the best friend have no history of Rec Center usage. This would seem to suggest that commitment mechanisms are most important for those without a gym-going habit, or that these are the students on the margin. Similarly, best friend treatment status strongly influences effort outcomes when both the subject and the best friend are on financial aid. This, and the fact that there are no spillovers for control subjects, would seem to suggest that the students’ main motivation to alter Rec Center usage is financial, and that peer mechanisms that help students visit the Rec Center and earn payments in this setting are most important for financially constrained students.¹¹

¹⁰ One might also be interested in other types of heterogeneous effects. For example, we find larger Rec Center peer effects for male students. We also find that “popular” best friends (those with many nominations) appear to influence popular students and that unpopular best friends influence unpopular students. (This may suggest a kind of homophily—a stronger influence between subjects with certain similarities.) For brevity, we do not include a full-scale analysis of these groupings here. This analysis is available from the authors upon request.

¹¹ Moreover, it is not the case that financial aid status is a proxy for being a non-user. The correlation between the two measures is only .05. These measures capture distinct, if slightly related, characteristics.

Up to this point, we have looked only at differences in means, or differences in differences in means. In the following subsection we investigate whether the findings hold up in a regression setting. We also attempt to interpret the magnitude of the peer effect using metrics traditionally found in peer studies.

D. Instrumental Variables Strategy

Table 5 summarizes results from regressions of effort outcomes on best friend treatment status, with and without control variables. The regressions in columns 1 and 2 do not include controls. Thus, they duplicate the estimates reported earlier: The coefficients on best friend treatment in columns 1 and 2 of the table match the mean differences reported in the first rows of Table 2, Panel A and Panel B, respectively. Control variables were added to the models for Table 5, columns 3 and 4. The randomization appears to have been effective. The main result, that best friend treatment status influences Rec Center visits for treated subjects but not controls, holds in regressions that control for observable individual characteristics.¹² In Table 5, Panel B, the regressor of interest is the fraction of the subject's roommates who were treated. The sample size is somewhat smaller in Panel B, because we do not have data on the roommates of all students in the main sample. However, given this caveat, there is no statistically detectable effect of the treatment status of roommates on a subject's Rec Center visits.¹³

The regression setting allows us to move beyond simple comparisons of means and to interpret findings from the previous subsection in ways that may be particularly useful. Peer studies often attempt to answer the question: How much does peer behavior influence own behavior? In our setting, the task is to estimate the effect of best friend Rec Center visits on own visits.

¹² Similar results hold for Poisson, Negative Binomial, and Ordered Probit variants of these regressions that do not impose linearity.

¹³ Most roommates in the Santa Catalina Residence Hall are randomly assigned, conditional on a few requested characteristics.

As has been strongly emphasized in the literature, peer selection is likely to be a serious confounding factor in this exercise. Students who visit the Rec Center a lot may choose best friends who do the same. Peer influence may be confounded with peer selection. Table 6 summarizes results from naïve OLS regressions of subject visits on best friend visits. For both treated and control subjects, one additional best friend visit is associated with about 0.2 additional own visits, when no individual characteristics are included in the regressions. When covariates for individual traits are included, the association goes away for control subjects, but not for treated subjects. For controls, then, even though effort choices look similar between subjects and their best friends, there is no clear evidence of peer influence: Once one accounts for pre-existing characteristics (such as pre-treatment Rec Center visits), the association vanishes. For treated subjects, the peer effect is robust to the inclusion of observed individual traits. But unobservables remain a source of concern.

An empirical model elucidates the challenges. Let $i=1$ or 2 reference the subject or the subject's best friend, respectively. Let y_i be i 's effort outcome, T_i be treatment status, and ε_i the usual error term. Suppose the true causal structure to be

$$y_1 = \alpha_1 + \beta_1 y_2 + \delta_1 T_1 + \theta_1 x_1 + \varepsilon_1 \quad (1)$$

$$y_2 = \alpha_2 + \beta_2 y_1 + \delta_2 T_2 + \theta_2 x_2 + \varepsilon_2 \quad (2)$$

In the canonical simultaneous equation model, the exclusion condition rule for identification requires that there be at least one exogenous variable excluded from each equation. Usually, it is a challenge to find exogenous variables to satisfy the exclusion restrictions. Here, T_2 is excluded from equation (1) and T_1 is excluded from equation (2), and random assignment of T_1 and T_2 obviates the usual concerns about selection. Thus, the exclusion restrictions here may be more plausible than in peer studies that either lack random assignment or do not feature it at the individual level. The exclusion restriction is interpretable as the assumption that a subject's

treatment assignment influences his own effort outcome but has no direct influence on his friend's outcome. It is possible, of course, that the exclusion restriction is violated.¹⁴ We do not insist that the exclusion restriction is valid, because we view the IV regressions as offering a convenient normalization, scaling, or metric by which to interpret the findings: They tell us the size of the spillover from best friend's treatment if we imagine that it functioned entirely through best friend's visits.

In the peer effect variant of the simultaneous equation model, coefficients in the two structural equations are often assumed to be identical.¹⁵ We do not make that assumption here. In particular, we do not assume $\beta_1 = \beta_2$. Because subjects often denote best friends who do not reciprocate the bond, the channel of influence may not be symmetrical. We allow that the influence of the chosen best friend on the subject's own outcomes may differ from the subject's influence on his (frequently non-reciprocating) best friend's outcomes. Standard Instrumental Variable techniques allow us to estimate this set of structural equations.

One other subtle deviation from the standard peer model merits comment. The evidence in Tables 2-5 (and from a pilot study that will be discussed in Section 4.F) indicates that spillovers differ by treatment status. Thus, we wish to estimate a *treatment-specific* β_1 . When we restrict the sample to subjects of a single treatment type, T_1 drops out of equation (1). However, as long as the exogenous variable T_2 is included in equation (2), β_1 is still identified. We note that the estimation of a treatment-specific effect is usually not possible, because without data on social ties (or variation across groups in fraction treated) there is no observed variation between treated subjects in their exposure to spillovers.

Table 7 shows first and second stages of IV regressions based on the model in (1) and (2), with and without individual characteristics as covariates. Panel A shows that Best Friend's treatment assignment, the instrument, predicts the endogenous regressor with high degrees of

¹⁴ For example, the restriction is violated if individuals effectively encourage or exhort their best friends to go to the Rec Center more often, even when they themselves do not.

¹⁵ See, for example, Moffitt (2001).

statistical significance in all regressions. The T-statistic is close to 10 and the F-statistic is close to 100. Treatment assignment, then, is not a weak instrument for Rec Center visits. Panel B shows the result of the second stage: A treated subject's effort choice rises by about 0.2 visits when his Best Friend's effort choice rises by 1 visit. This finding is robust to the inclusion of control variables.

The IV regressions indicate that if one imagines the spillover as resulting entirely from best friend's visits, then the spillover is about 20% as large as the direct effect of the intervention. In Table 5, the results of the OLS regressions of individual visits directly on best friend's treatment assignment showed that subjects visited the Rec Center about 1 more time if their best friend was treated. This was about 20% of the overall effect of the intervention (a 5-visit increase in Rec Center usage). Interestingly, the size of the spillover is similar, whether one imposes assumptions that force best friend's treatment assignment to work only through best friend's outcomes, or whether one allows it to have a direct effect, as well.

E. Mechanisms

What, then, is the causal mechanism for the peer effect? In the literature it is often difficult to discern the peer mechanism, even when a peer effect has been isolated or identified. In partial population experiments that capture mean responses to mean behavior of a large group of peers, investigating peer mechanisms that operate at the individual level has proved to be challenging. In the broader literature as well, peer influence has tended to be a black box phenomenon. Detailed data on Rec Center visits allow us to look more closely at mechanisms than is sometimes possible. We will focus on 4 broad categories of potential mechanisms.

1. 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 rather engage in an activity that is more immediately

pleasurable (O'Donoghue and Rabin, 1999, 2001). Having a treated best friend could remedy this problem. If one commits oneself to exercise with a friend, it is more difficult for one's future self to back out. In short, individuals who have both been incentivized may use each other to devise commitment mechanisms.

2. Complementarities in production of utility. Rec Center visits with a friend may produce more utility (or reduce the effort costs of Rec Center usage) relative to single-person visits. This mechanism, then, is essentially the standard concept of complementarity in production (e.g., Lazear, 2000) applied to a context in which the produced good is utility (Stigler and Becker, 1977). If it is less onerous (or more fun) to go the gym with a best friend, then having a treated best friend (who visits the Rec Center more, *ceteris paribus*) provides more opportunities for lower cost visits.

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. In this framework, individuals seek to minimize the difference between their own effort choices and the effort choices of peers because they derive utility from imitation or sameness (Akerlof, 1997, Akerlof and Kranton, 2000).

4. Information exchange. Many models and empirical analyses of networks and social interactions emphasize information exchange (e.g., Duflo and Saez, 2003). An information exchange story in our setting would be that best friends, when treated, communicate information to subjects that facilitates Rec Center visits.

We begin with self-control and pre-commitment. One obvious commitment mechanism would be to make plans to use the Rec Center with one's best friend, and so commit one's future self to a visit. If this explains the increase in Rec Center usage by treated subjects with treated best friends, then it should be visible in the timing of Rec Center visits. Though we do not observe whether individuals make plans together, we do see whether a subject arrived at the Rec

Center at the same time as the subject's best friend. If pre-commitment associated with joint visits is a salient peer mechanism, we should observe treated subjects showing up *at the same time* as their best friend more often if the best friend is treated.

In the Rec Center data, every visit has a time stamp. Figure 4 shows a plot of average Rec Center visits per person by day of visit for the 4 groups of subjects in Table 2 (based on treatment assignment and best friend treatment assignment.) The zero point on the horizontal axis denotes the beginning of the treatment period. Main findings from Table 2 are visible in the figure. In the top row of graphs, which depicts outcomes for control subjects, the plots look very similar. For controls, best friend treatment assignment does not alter the number or timing of visits. In the bottom row of graphs, which depicts outcomes for the treated, the plots appear similar during the pre-treatment period but diverge in the treatment period. For treated subjects, there were more visits in the treatment period if the best friend was treated.

In Table 8, we tighten the focus on the timing of visits. We define a simultaneous visit for a subject, as one in which the time-stamp for a subject's visit was within 10 minutes of a subject's best friend's visit. Panel A, Column 1 reveals that treated subjects with treated best friends averaged 1.72 simultaneous visits during the treatment period, whereas the average for treated subjects with control best friends was .52. The difference in means, 1.2 simultaneous visits, is statistically significant ($p\text{-value} < .001$). Treated subjects made 1.2 more simultaneous visits with their best friend during the treatment if their best friend was treated.¹⁶

But it is possible that this difference in means does not indicate deliberate coordination of visits. Treated best friends go to the Rec Center more often, other things equal, and so the probability of a simultaneous visit is higher through pure chance when a subject's best friend is treated. We created "placebo best friends" for the data summarized in Panel B, in order to

¹⁶ The findings in Table 8 are very similar if alternate definitions of a "simultaneous" visit are used. Statistically significant differences in mean simultaneous visits between treated subjects with treated best friends and treated subjects with control best friends are .95, .95, 1.22, and 1.26, respectively, for definitions of simultaneous visits that feature 1 minute, 5 minute, 15 minute and 20 minute windows.

investigate whether unplanned simultaneous visits could account for the mean difference estimated in Panel A. Each subject was randomly assigned a “best friend.” We found that, indeed, the number of simultaneous visits with a randomly assigned placebo best friend was about a tenth of a visit higher if the placebo best friend was treated. But this effect is clearly quite small. Although the number of “accidental” simultaneous visits is slightly higher when a subject’s best friend is treated, this accounts for only a small fraction of the observed difference in simultaneous visits with true best friends. In brief, the evidence indicates that treated subjects visited the Rec Center jointly with a best friend about 1 additional time if the best friend was treated, and that unplanned coincidental visits do not account for this difference.

Table 2 showed that having a best friend assigned to treatment led to one more visit *overall*. The results in Table 8 imply that having a best friend assigned to treatment led to 1 more *joint visit*. Thus, joint or simultaneous visits would appear to account for the entirety of the observed peer effect on visits.

This is consistent with mechanism 1) above, that having a treated friend allows agents to pre-commit. It may also be consistent with explanation 2), that joint visits yield more utility. But the finding would seem to argue against explanation 3): Subjects who derive utility strictly from imitation need not visit the Rec Center at the same time in order to imitate peer behavior.

Lastly, there would seem to be little room for explanation 4), the information story. Very little information was needed to visit the Rec Center: In the experiment, subjects were informed of the location of the Rec Center, and the vast majority of them already knew. Though one could argue that for subjects who had never been to the Rec Center, a simultaneous visit with a best friend could provide useful information about precisely where to go or what to do, there is little evidence in the data that this is the driving mechanism. In such a story the observed difference in Rec Center visits by treatment status of best friend would be driven by treated subjects whose *first visit to the Rec Center was a simultaneous visit with a best friend*. One can drop all such subjects from the dataset and the results in Table 2 are virtually unchanged: Subjects for whom

the first observed Rec Center visit in the treatment period was *not* a joint visit with a best friend nevertheless visited the Rec Center more if their best friend was treated.¹⁷

In summary, having a treated best friend produces more Rec Center visits and would appear to do so primarily by producing more simultaneous visits. This is most consistent with explanations in which simultaneous visits allow agents to coordinate to overcome obstacles arising from time inconsistent preferences or obstacles relating to high effort cost of solitary visits.

F. Sensitivity to Experimental Design

In any field experiment, context and framing matter. Are the results here robust to changes in experimental design? A pilot study conducted prior to the main experiment allows us to investigate this question. In the fall of 2009, we conducted a pilot study at the Santa Catalina Residence Hall. These were different students from the participants in the 2011 dataset (used in the present analysis), because a new wave of students takes up residence in Santa Catalina each year. The dormitory houses incoming freshmen, almost exclusively.

The pilot study differed from the present analysis in two main ways: The sample size was smaller, and the study was conducted much earlier in the academic year. There were 222 students in the pilot, compared to 614 for the 2011 study. This meant that whereas the main study included more than 50% of the 2011 population of the residence hall, only 17% of the 2009 population participated in the pilot. The pivotal group in both samples was the treated group. There were 86 treated subjects in the pilot, compared to 352 for the 2011 study. In the pilot, the friendship network was elicited in October, two weeks into the fall quarter, when newly arrived freshman students were just getting to know each other. In the 2011 study, the friendship network was elicited in January, after students had lived in close proximity for several months. Because friendships were more mature, the elicited 2011 network is much more connected: Subjects in the

¹⁷ Regressions and two-sample T-tests available from the authors upon request.

pilot averaged 7 friends each, whereas subjects in the 2011 sample had 18 friends. The greater number of friendships in the larger sample may also be due in part to the greater number of eligible friends to choose from (more than half of the students in residence hall compared to 17%). The larger sample allows for greater statistical power, but what may be even more important is that it allows for elicitation of a more accurate friendship network, because less of the network goes unobserved.

In both studies, spillovers were observed for treated students but not for controls. However, in the pilot, treated subjects appeared to have been influenced by the number or fraction of treated friends, but not by their best friend.¹⁸ In the 2011 study, there were large effects of best friend treatment assignment, but no observed effect of number of treated friends or fraction treated.

Timing, design, and implementation of the experiment appear to matter. In the pilot study, friendships may have been elicited too early for best friend to be a meaningful designation. After 2 weeks, one has acquired acquaintances, perhaps, but there may have not have been time for a best friend to emerge. And because only a small fraction of the residents participated in the pilot, there was a large chance that one's "true" best friend was not among the participants. In the larger sample, we may be observing the Facebook phenomenon for mature friendship networks: Subjects identify many friends but there is a smaller subset with whom they interact regularly and by whom they are influenced. The best friend designation may be particularly meaningful in this setting.

We conclude that the timing of the elicitation of friendship networks may be crucial in studies of this kind, as well as the fraction of the community surveyed.

¹⁸ A full-scale analysis of the pilot project is available from the authors upon request.

5. Summary

Strong claims have been made about the “contagiousness” of health-related behaviors and outcomes, but truly exogenous variation in behaviors has been hard to come by. A major goal of our field experiment has been to bring exogenous variation in behaviors to an endogenous set of connections and thus to learn about spillovers in effort elicitation settings and the mechanisms that drive them. We elicited friendship networks among college students and offered monetary incentives for using the recreation center to a treated subset. We found that treated students with treated best friends increased their usage of the recreation center more than treated students with control best friends. Control subjects did not alter their recreation center usage at all, and were not influenced by the treatment status of their best friend. The effect was largest for students likely to be on the margin, those without a pre-existing habit of Rec Center usage, and for students on financial aid, for whom the financial incentives may have been most valued. The observed spillovers are explained by coordination among students: Treated subjects made more joint visits with treated best friends.

The setting is stylized and was designed to investigate fundamental behavioral patterns. We have not offered a full-scale analysis on the effectiveness of short-term exercise interventions in changing long-run habits or health outcomes, as these issues have been explored carefully in previous work. However, in a more general way, the findings here may offer insights for how to improve the effectiveness of a variety of targeted interventions that seek to elicit effort from people in social settings. The large peer effect between the treated, particularly among subjects on the margin, may suggest that interventions designed to alter behaviors or elicit effort would be more cost-effective if they targeted and saturated a small number of networked populations than if they targeted small portions of many different networked populations. In the former approach, there would be a higher incidence of strong social ties between treated individuals, and this could generate significant spillovers that would otherwise not occur. Some examples of interventions that feature effort-elicitation in a social context (and about which this insight may be relevant)

include: programs that try to induce people to lose weight, to raise test scores, to increase college enrollment, to donate blood, to get vaccinated, to use contraceptives, to find jobs, and to improve sanitation practices. However, findings also suggest that even when peer effects amplify the effectiveness of an intervention among the treated, they may fail to spread to those who were not treated.

We highlight several additional contributions of this research to the evolving literature on peer effects: Firstly, the findings suggest that proximity, the metric commonly used to define a peer reference group, may be an inadequate proxy for influence. Secondly, the research design made visible a meaningful but often neglected distinction between spillovers from treated subjects to treated subjects and spillovers from treated subjects to controls. Lastly, whereas peer effects are often estimated as a black-box phenomenon, the analysis here allowed a glimpse inside.

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Table 1 - Descriptive Statistics

Panel A. BY PARTICIPATION CHOICE

	Participant		Nonparticipant		P-Value Diff >0
	Mean	St. Dev.	Mean	St. Dev.	
Age	18.21	0.47	18.26	0.52	0.17
Black	0.02	0.15	0.04	0.19	0.30
Hisp	0.26	0.44	0.28	0.46	0.57
Male	0.43	0.50	0.53	0.50	0.01
Low Inc (Fin Aid)	0.54	0.50	0.55	0.50	0.90
Self-reported Exercise (/Week)	3.45	2.02	3.20	2.04	0.11
Pre-Treat Rec Cen (Visits/Month)	2.24	3.97	1.94	3.39	0.31
Obs	614		224		

Panel B. BY TREATMENT STATUS OF PARTICIPANTS

	Treatment		Control		P-Value Diff >0
	Mean	St. Dev.	Mean	St. Dev.	
Age	18.24	0.51	18.18	0.41	0.14
Black	0.02	0.13	0.03	0.17	0.27
Hisp	0.25	0.44	0.26	0.44	0.77
Male	0.46	0.50	0.39	0.49	0.11
Low Inc (Fin Aid)	0.53	0.50	0.57	0.50	0.29
Self-reported Exercise (/Week)	3.46	2.02	3.45	2.01	0.93
Pre-Treat Rec Cen (Visits/Month)	2.30	3.94	2.16	4.01	0.67
#Friends (Self-report)	18.16	12.75	18.51	13.10	0.74
#Friends (Friend-report)	18.25	9.70	17.70	8.53	0.47
Best Friend Treated	0.57	0.50	0.57	0.50	0.97
Obs	352		262		

Table 2. Mean Effort Outcomes by Treatment and Best Friend Treatment

Best Friend Definition	Best Friend Treated		Best Friend Control		Dif	P-value Dif
	Mean	Obs	Mean	Obs		
<u>Panel A. TREATED SUBJECTS</u>						
Outcome: Visits						
BF Self-reported	7.72	202	6.73	150	0.98**	0.02
BF Friend-reported	7.82	102	7.05	74	0.77	0.18
BF Reciprocated	7.86	69	6.70	50	1.16*	0.10
Outcome: > 7 Visits						
BF Self-reported	0.65	202	0.59	150	0.06	0.23
BF Friend-reported	0.68	102	0.64	74	0.04	0.57
BF Reciprocated	0.67	69	0.60	50	0.07	0.46
<u>Panel B. CONTROL SUBJECTS</u>						
Outcome: Visits						
BF Self-reported	2.49	150	2.33	112	0.16	0.75
BF Friend-reported	2.48	69	2.83	54	-0.36	0.65
BF Reciprocated	1.94	48	2.03	39	-0.09	0.90
Outcome: > 7 Visits						
BF Self-reported	0.11	150	0.09	112	0.01	0.64
BF Friend-reported	0.13	69	0.15	54	-0.02	0.78
BF Reciprocated	0.10	48	0.10	39	0.00	0.98

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3. Mean Effort Outcomes by Treatment, Best Friend Treatment, and Financial Aid Status

	Subject Fin Aid Status	Best Friend Fin Aid Status	Best Friend Treated		Best Friend Control		Dif	P-value Dif
			Mean	Obs	Mean	Obs		
<u>Panel A. TREATED SUBJECTS</u>								
Outcome: Visits								
	Fin Aid	Fin Aid	8.00	77	5.70	44	2.30***	0.00
	Fin Aid	No Fin Aid	8.12	43	8.19	21	-0.07	0.94
	No Fin Aid	Fin Aid	7.05	41	7.53	36	-0.48	0.49
	No Fin Aid	No Fin Aid	7.58	41	6.45	49	1.14	0.15
Outcome: > 7 Visits								
	Fin Aid	Fin Aid	0.65	77	0.48	44	0.17*	0.07
	Fin Aid	No Fin Aid	0.63	43	0.71	21	-0.09	0.50
	No Fin Aid	Fin Aid	0.63	41	0.69	36	-0.07	0.53
	No Fin Aid	No Fin Aid	0.7	41	0.55	49	0.15	0.15
<u>Panel B. CONTROL SUBJECTS</u>								
Outcome: Visits								
	Fin Aid	Fin Aid	2.24	51	1.82	38	0.42	0.63
	Fin Aid	No Fin Aid	1.53	36	2.63	24	-1.10	0.18
	No Fin Aid	Fin Aid	4.10	19	2.45	20	1.66	0.20
	No Fin Aid	No Fin Aid	2.86	44	2.67	30	0.20	0.84
Outcome: > 7 Visits								
	Fin Aid	Fin Aid	0.08	51	0.08	38	0.00	0.99
	Fin Aid	No Fin Aid	0.03	36	0.08	24	-0.06	0.34
	No Fin Aid	Fin Aid	0.26	19	0.10	20	0.16	0.19
	No Fin Aid	No Fin Aid	0.14	44	0.11	30	0.03	0.72

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4. Mean Effort Outcomes by Treatment, Best Friend Treatment, and Previous Rec Cen Use

	Subject: Previous Rec Cen Use	Best Friend: Previous Rec Cen Use	Best Friend Treated		Best Friend Control		Dif	P-value Dif
			Mean	Obs	Mean	Obs		
<u>Panel A. TREATED SUBJECTS</u>								
Outcome: Visits								
	Non-user	Non-user	6.73	80	4.52	62	2.21***	0.00
	Non-user	User	5.83	36	6.25	16	-0.42	0.71
	User	Non-user	9.47	36	7.96	24	1.51	0.13
	User	User	9.4	45	9.15	35	0.25	0.68
Outcome: > 7 Visits								
	Non-user	Non-user	0.61	80	0.35	62	0.26***	0.00
	Non-user	User	0.47	36	0.63	16	-0.15	0.32
	User	Non-user	0.78	36	0.71	24	0.07	0.55
	User	User	0.24	45	0.17	35	0.07	0.43
<u>Panel B. CONTROL SUBJECTS</u>								
Outcome: Visits								
	Non-user	Non-user	0.63	54	0.71	45	-0.08	0.80
	Non-user	User	0.88	32	1.06	17	-0.18	0.78
	User	Non-user	5.26	19	3.4	15	1.86	0.29
	User	User	4.69	50	4.57	48	0.12	0.92
Outcome: > 7 Visits								
	Non-user	Non-user	0.00	54	0.02	45	-0.02	0.28
	Non-user	User	0.00	32	0.06	17	-0.06	0.17
	User	Non-user	0.26	19	0.13	15	0.13	0.37
	User	User	0.74	50	0.81	48	-0.07	0.40

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Rec Center Visits and Best Friend Treatment - OLS Regression Results

	1	2	3	4
	Subject	Subject	Subject	Subject
	Treated	Control	Treated	Control
	Visits	Visits	Visits	Visits
<u>Panel A - Best Friends</u>				
Best Friend Treated	.98** (.42)	.16 (.48)	.82** (.37)	-.23 (.29)
Controls	NO	NO	YES	YES
Observations	352	262	352	262
R-squared	0.02	0.00	0.28	0.66
<u>Panel B - Roommates</u>				
Fraction Roommates Treated	.46 (.51)	.47 (.69)	.5 (.45)	-.45 (.39)
Controls	NO	NO	YES	YES
Observations	242	181	242	181
R-squared	0.00	0.00	0.28	0.69

* significant at 10%; ** significant at 5%; *** significant at 1%.

Control variables include the full set of individual characteristics shown in Table 1.A.

Table 6: Own Rec Center Visits and Best Friend Visits - OLS Regression Results

	1	2	3	4
	Subject	Subject	Subject	Subject
	Treated	Control	Treated	Control
	Visits	Visits	Visits	Visits
Best Friend Visits	.22*** (.046)	.18*** (.05)	.18*** (.04)	.048 (.031)
Controls	NO	NO	YES	YES
Observations	352	262	352	262
R-squared	0.06	0.05	0.31	0.66

* significant at 10%; ** significant at 5%; *** significant at 1%.

Control variables include the full set of individual characteristics shown in Table 1.A.

Table 7. Peer Effects and Rec Center Visits - IV Regression Results

	1	2	3	4
	Best Friend Treated Visits	Best Friend Control Visits	Best Friend Treated Visits	Best Friend Control Visits
<u>Panel A - IV First Stage</u>				
Best Friend Treated	4.25*** (.43)	3.61*** (.54)	4.27*** (.43)	3.43*** (.53)
Controls	NO	NO	YES	YES
Observations	352	262	352	262
R-squared	0.22	0.15	0.24	0.21
	1	2	3	4
	Subject Treated Visits	Subject Control Visits	Subject Treated Visits	Subject Control Visits
<u>Panel B - IV Results</u>				
Best Friend Visits	.23** (.097)	.043 (.13)	.19** (.086)	-.067 (.086)
Controls	NO	NO	YES	YES
Observations	352	262	352	262
R-squared	0.06	0.02	0.31	0.65

* significant at 10%; ** significant at 5%; *** significant at 1%.

Control variables include the full set of individual characteristics shown in Table 1.A.

**Table 8. Number of Simultaneous Visits with Best Friend
During the Treatment Period**

	1	2
	Subject	Subject
	Treated	Control
Panel A. Best Friend Self-reported		
Best Friend - Treated	1.72	0.63
Best Friend - Control	0.52	0.55
Dif	1.21***	0.08
P-Value Dif	0.00	0.69
Panel B. Placebo Best Friend		
Best Friend - Treated	0.16	0.04
Best Friend - Control	0.03	0.00
Dif	0.12**	0.04*
P-Value Dif	0.02	0.06

* significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 1

Santa Catalina Friendship Network, 2011
Degrees of Separation
(Relative Frequency)

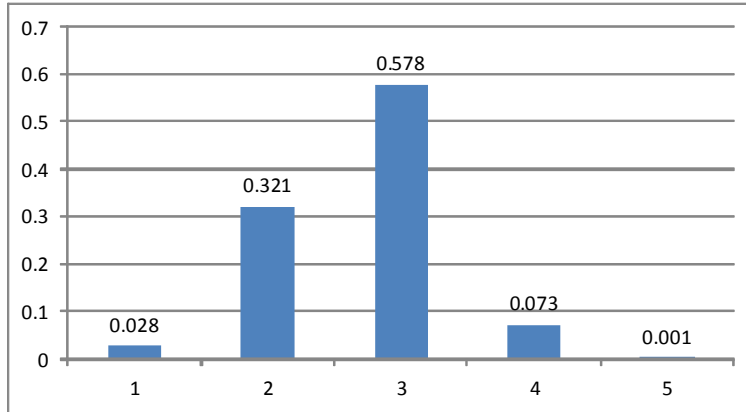
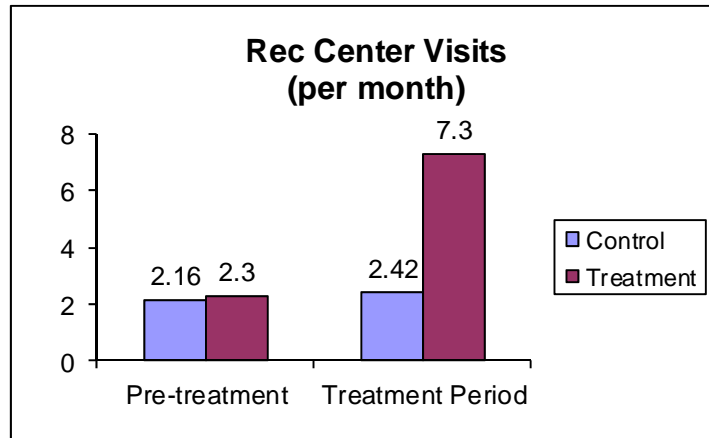


Figure 2

A.



B.

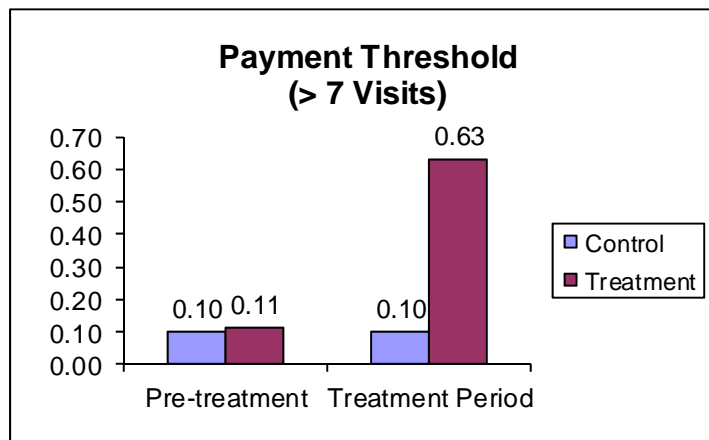
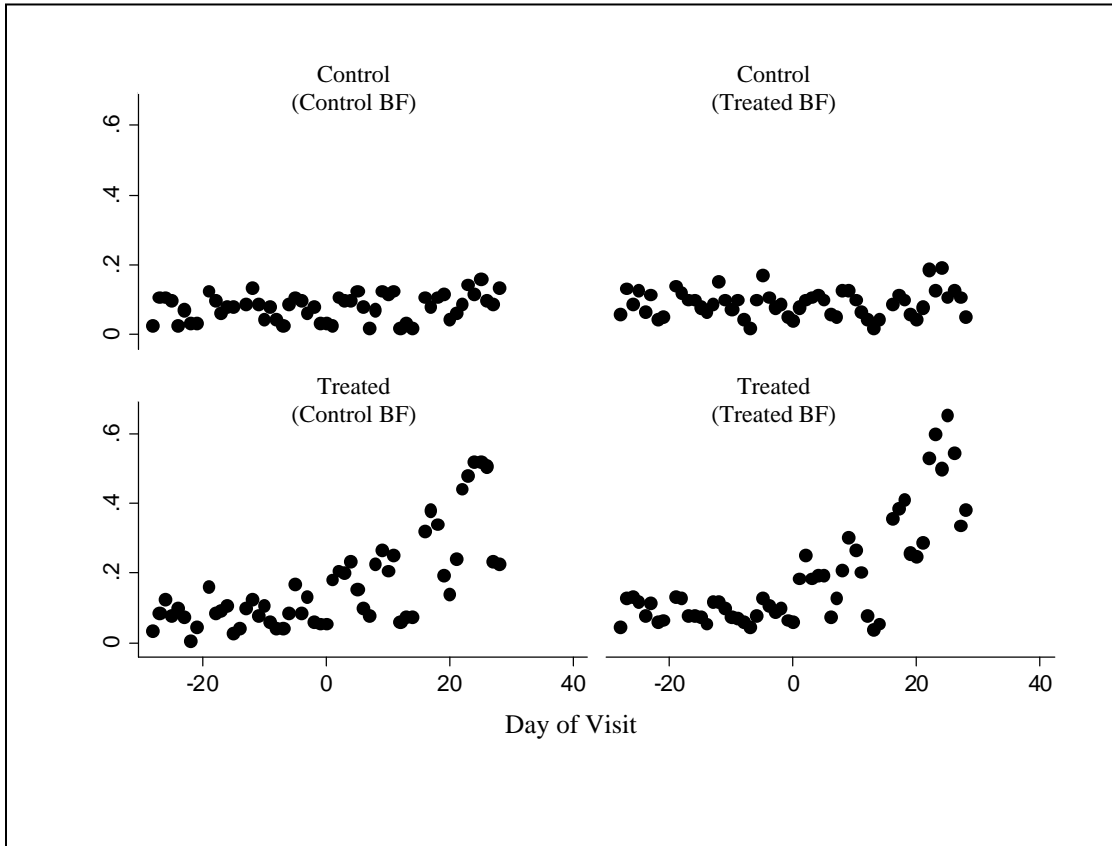


Figure 3

Visits per person
(by Day)



FOR ONLINE PUBLICATION

Appendix 1

Survey and Consent Forms

1. Consent Form
2. Survey
3. Informational Form for Treated Subjects

PURPOSE:

You are being asked to participate in a research study. The purpose of the study is to discern the effects of exercise on the health outcomes of individuals and possible spillover effects on social groups to which an individual belongs.

PROCEDURES:

If you decide to participate, you will be asked to fill out a questionnaire about your personal background. Your consent today will mean that your name will be included on a list to be distributed to all participants of this study later in the quarter, for a follow-up friendship survey. And your consent today will allow us to access your attendance records at the UCSB Recreation Center between September 2010 and June 2011.

Your participation today will probably take about 5 minutes. There will be about 900 subjects in this study.

RISKS:

There are no significant risks for your participation today.

BENEFITS:

There will be no substantial benefit to you from completing these measurements and filling out the questionnaire, other than the money that you receive.

CONFIDENTIALITY:

Any records of your identity will be securely stored. Furthermore, any data stored on computers will only match to your identity by a numeric code. The information that matches your code to your identity will not be stored on a computer. When we no longer need to match your identity to your records, we will destroy any records that could match your data with your identity. Absolute confidentiality cannot be guaranteed, since research documents are not protected from subpoena.

COSTS/PAYMENT:

You will receive \$5 cash for your participation today, which will be paid when your participation has concluded. If you begin participation today and decide to continue in the research, you will receive \$8 to fill out a follow-up survey in a week or two. If you complete both surveys, we will invite some of you to participate in a follow up study on incentives to exercise. If you begin participation today and decide not to continue in the research, you will receive \$3. If you withdraw now, you will not be able to participate in the other portions of this study.

EMERGENCY CARE AND TREATMENT FOR INJURY:

If you are injured as a direct result of research procedures, you will receive reasonably necessary medical treatment at no cost. The University of California does not provide any other form of compensation for injury.

RIGHT TO REFUSE OR WITHDRAW:

You may refuse to participate and still receive any benefits you would receive if you were not in the study. You may change your mind about being in the study and quit after the study has started.

QUESTIONS:

If you have any questions about this research project or if you think you may have been injured as a result of your participation, please contact:

John Hartman (805-893-7309) or Philip Babcock (805-893-4823) or send an e-mail to: hartman@econ.ucsb.edu and/or babcock@econ.ucsb.edu.

If you have any questions regarding your rights and participation as a research subject, please contact the Human Subjects Committee at (805) 893-3807 or hsc@research.ucsb.edu. Or write to the University of California, Human Subjects Committee, Office of Research, Santa Barbara, CA 93106-2050

Signature

Name

Perm #

QUESTIONNAIRE

Please note that in order to participate in this study, you need to be a UCSB student living in Santa Catalina Residence Hall. I would like to ask you a few questions regarding some of your characteristics. Thank you.

NAME _____ Perm Number _____ Dormitory and room _____

Please list two e-mail addresses that we can contact you at:

Local phone number that we can contact you at: _____

1. What is your class standing Freshman Sophomore Other (specify)_____

2. What is your gender? Male _____ Female _____

3. How old are you? _____ years

4. What is your birth date? Month _____ Day _____ Year _____

5. Do you have at least one parent with a bachelor's degree?

Yes _____ No _____

6. Are you of Hispanic and/or Latino ethnicity? Yes No

7. What is your race?

_____ African-American
_____ Asian
_____ Caucasian
_____ Pacific Islander
_____ Other (Please specify _____)

8. How many times per week do you moderately or vigorously exercise for 30 minutes or more?

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

9. Did you qualify for financial aid [Any of the following: Federal Pell grant, Federal Supplemental Educational Opportunity Grants (SEOG), Academic Competitiveness Grant (ACG), Federal work study, Federal subsidized loan, Federal unsubsidized loan, Cal grant, University of California Grant]? Yes No

PURPOSE:

You are being asked to participate in a research study. The purpose of the study is to discern the effects of exercise on the health outcomes of individuals and possible spillover effects on social groups to which an individual belongs.

PROCEDURES:

If you decide to participate in this research follow-up to your recent participation in filling out a questionnaire, you will be given the opportunity to receive \$60 by exercising. In order to receive \$60, you will need to exercise at the UCSB Recreation Center (commonly known as the Rec Center) a minimum of eight different days over a four week period (**FEBRUARY 6 TO MARCH 6, 2011**). In order for any exercise session to count, you simply need to make sure that the computer at the front desk of the Recreation Center acknowledges your presence each day you exercise. You will receive the \$60 only if you make the eight visits to the UCSB Recreation Center as described above, but you do not have to make any visits if you do not want to.

RISKS:

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 two months you may not participate in this exercise study.

BENEFITS:

There will be no substantial benefit to you from completing these measurements and filling out the questionnaire, other than the money that you receive.

CONFIDENTIALITY:

Any records of your identity will be securely stored. Furthermore, any data stored on computers will only match to your identity by a numeric code. The information that matches your code to your identity will not be stored on a computer. When we no longer need to match your identity to your records, we will destroy any records that could match your data with your identity. Absolute confidentiality cannot be guaranteed, since research documents are not protected from subpoena.

COSTS/PAYMENT:

Again, you will receive \$8 for filling out a friendship survey. You will also be given the opportunity to receive an additional \$60 by exercising. In order to receive \$60, you will need to exercise at the UCSB Recreation Center (commonly known as the Rec Center) a minimum of eight different days over a four week period.

EMERGENCY CARE AND TREATMENT FOR INJURY:

If you are injured as a direct result of research procedures, you will receive reasonably necessary medical treatment at no cost. The University of California does not provide any other form of compensation for injury.

RIGHT TO REFUSE OR WITHDRAW:

You may refuse to participate and still receive any benefits you would receive if you were not in the study. You may change your mind about being in the study and quit after the study has started.

QUESTIONS:

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Signature

Name

Perm #