# B For Online Publication

## B.1 Screening mechanism and Randomization

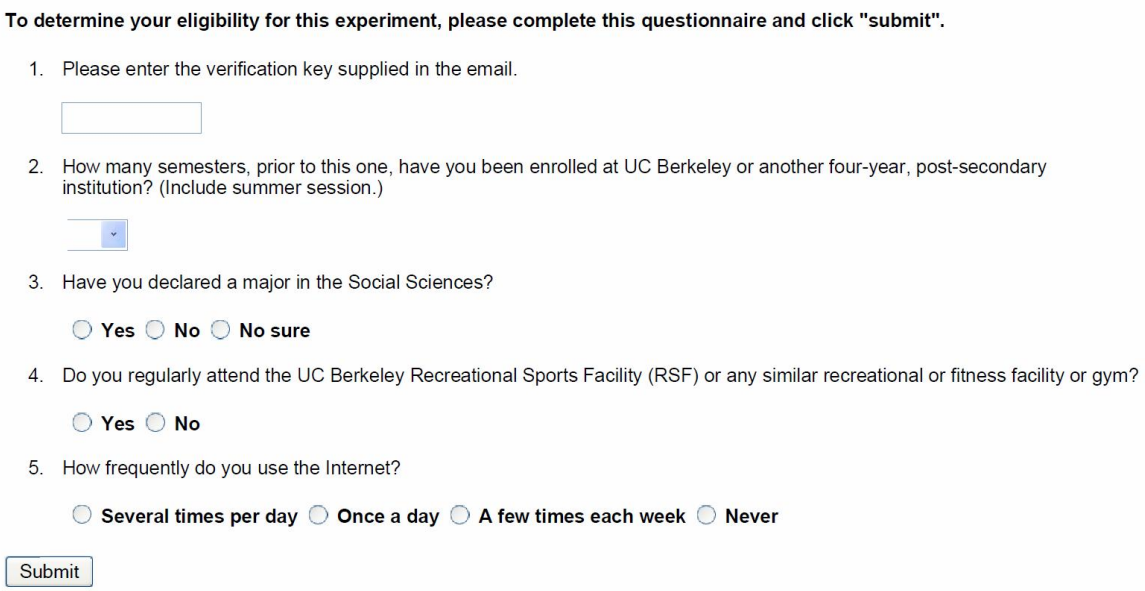
The webpage we used to screen for non-attenders is shown below. We included three "dummy" questions to make it harder for subjects to return to the site and change their answers in order to be able to join the experiment. Despite this precaution, a handful of subjects did return to the screening site and modify their answers until they hit upon the correct answer to join the experiment. (Which was a "no" on question four.) Out of a total of 497 unique IP addresses in our screening log, we found 5 instances of subjects possibly gaming the system to gain access to the study. We have no way to determine if these subjects wound up in our subject pool.

Randomizing subjects into treatment and control presented some challenges. Our design required that treatment and control subjects meet separately. For each of the three sessions we scheduled four timeslots, back-to-back, and staggered them between Control and Treatment. When subjects responded to the online solicitation, and after they had completed the screening questionnaire, they were randomly assigned to either treatment or control and were then asked to choose between the two timeslots allocated to their assigned group. Subjects who could not find a timeslot that fit their schedule voluntarily left the study at this point.[22] As it turned out, subjects assigned to the treatment group were substantially less likely to find a timeslot that worked for them, and as a result the desired number of subjects were successfully enrolled in the control group well before the treatment group was filled. Wanting to preserve the balanced number of Treatment and Control subjects, maintain power to identify heterogeneity within the Treatment group, and stay within the budget for the study, we capped the control group and continued to solicit participants in order to fill the treatment group. Subjects who responded to the solicitation after the Control group was filled were randomly assigned to treatment or control, and those assigned to control were then thanked and told that the study was full. Our treatment group therefore includes subjects who were either solicited later, or responded to the solicitation later than any of the subjects in the control group.[23]

[22] Technically they were considered to have never joined the study, and received no payment.

[23] Additionally, the two groups of subjects were available at different times of day. To the extent that what made it hard for Treatment subjects to find a timeslot that fit the schedule may have been correlated with gym-attendance behavior (if, for example, the Treatment timeslots happen to have coincided with the most preferred times for non-gym exercise), then the group averages for

Figure B.2: Screening Site



To the extent that these temporal differences are correlated with any of the behaviors we are studying, simple comparisons of group averages may be biased. It appears, however, that the two groups are not substantially different along any of the dimensions we observed in our dataset, as a joint F-test does reject that the two groups were randomly selected from the same population based on observables. A comparison of the two groups appears in Table A.1.

We are able to examine the pre-experimental gym-attendance history of our subjects, and conclude that the screening mechanism was largely effective. Out of 120 subjects, only 4 (3.3%) visited the RSF at least once a week on average in the pre-experimental period, and none visited twice a week (i.e. the level of attendance in our intervention). Just over half of subjects never visited the RSF at all during the pre-experimental period.

Table B.2 compares subjects who attended at least one time in the 37-week pre-treatment period with those who did not. There are a handful of significant differences: those with non-zero pre-treatment attendance are younger, more likely to be female, and have lower travel time and cost (e.g. live on campus). These are unsurprising predictors of gym attendance. Race-Asian is marginally significant, suggesting that there could be cultural differences among race categories regarding the desirability of gym attendance (it is not significant in a regression that includes the other characteristics). We use individual fixed effects in all our regressions to adjust for these differences. As mentioned in the text, there is no significant difference in the proportion of pre-attenders between treatment and control. There is also no difference in the proportion between compliers and non-compliers (p=0.77). Pre-treatment attendance is also not correlated with habit-formation (p=0.46).

## B.2 Elicitation mechanisms

Figure B.3 depicts the sample p-coupon and instructions that subjects saw to prepare them for the incentive-compatible elicitation task. Verbal instructions given at this time further clarified exactly what we were asking subjects to do. Note that the sure-thing values in column A are increments of $\$P$. The line number where subjects cross over from choosing column B to choosing column A bounds their valuation for the p-coupon. We used a linear interpolation between these bounds to create our "BDM"

some outcome variables may be biased.

Table B.2: Comparison of Subjects with and without pre-treatment attendance

| | (1) No Previous Attendance | (2) Some Previous Attendance | (3) T-test p-value |
|---|---|---|---|
| *Demographic covariates* | | | |
| Age | 23.463 | 20.456 | 0.01 |
| | (1.132) | (0.297) | |
| Gender (1=female) | 0.611 | 0.754 | 0.011 |
| | (0.067) | (0.058) | |
| Proportion white | 0.444 | 0.281 | 0.074 |
| | (0.068) | (0.060) | |
| Proportion Asian | 0.463 | 0.649 | 0.049 |
| | (0.068) | (0.064) | |
| Proportion other race | 0.093 | 0.070 | 0.669 |
| | (0.040) | (0.034) | |
| *Economic covariates* | | | |
| Discretionary budget | 170.370 | 213.158 | 0.251 |
| | (23.993) | (28.029) | |
| Travel cost to campus | 1.750 | 0.096 | 0.002 |
| | (0.535) | (0.066) | |
| Travel time to campus (min) | 17.500 | 11.974 | 0.009 |
| | (1.976) | (0.786) | |
| *Naivete proxy covariates* | | | |
| Forget[a,b] | 1.574 | 1.614 | 0.767 |
| | (0.094) | (0.096) | |
| Spontaneous[a,b] | 2.481 | 2.491 | 0.951 |
| | (0.120) | (0.104) | |
| Things come up[a,b] | 2.519 | 2.649 | 0.366 |
| | (0.108) | (0.095) | |
| Think ahead[a,b] | 2.833 | 2.912 | 0.583 |
| | (0.105) | (0.098) | |
| Procrastinate[a,b] | 3.074 | 3.000 | 0.622 |
| | (0.109) | (0.103) | |
| *Exercise experience and attitude covariates* | | | |
| Pre-trt Godin Activity Scale | 37.991 | 34.211 | 0.429 |
| | (3.541) | (3.198) | |
| Fitness is important[a,b] | 3.056 | 3.105 | 0.115 |
| | (0.081) | (0.082) | |
| Appearance is important[a,b] | 3.333 | 3.175 | 0.129 |
| | (0.092) | (0.091) | |
| $N$ | 54 | 57 | |
| F-test of joint significance | | | 0.007 |

Notes: [a] 1= Disagree Strongly, 2=Disagree Somewhat; 3=Agree Somewhat; 4=Agree Strongly. [b] Wording of questions in appendix. Standard errors in parentheses. Pre-attendance is defined as having any attendance in the 37 weeks prior to the treatment.

variable. Thus, for example, if a subject chose B at and below line four, and then chose A at and above line five we assigned them a p-coupon valuation of $\$P \times 4.5$ In general subjects appear to have understood this task clearly. There were only three subjects who failed to display a single crossing on every task, and all of them appear to have realized what they were doing before the end of the first elicitation session. The observations for which these three subjects did not display a single crossing have been dropped from our analysis.

By randomly choosing only one target week for only one subject we maintain incentive compatibility while leaving all but one subject per session actually holding a p-coupon, and for only one target week. This is important because what we care about is the change in their valuation of a p-coupon from first to second elicitation sessions. Subjects who are already holding a coupon from the first session would be valuing a second coupon in the second session, making their valuations potentially incomparable, rather like comparing willingness-to-pay for a first candy bar to willingness-to-pay for a second candy bar.

The instructions and example for the unincentivized prediction task and the task for prediction of other people's attendance appear as figure B.4.

Figure B.3: Sample p-coupon and incentive-compatible elicitation task

# [PRACTICE]

This exercise involves nine questions, relating to the Daily RSF-Reward Certificate shown at the top of the page. Each question gives you two options, A or B. For each question check the option you prefer.

You will be asked to complete this exercise four times, once each for four of the five target weeks. The daily value of the certificate will be different for each of these four target weeks. For one of the five weeks you will not be asked to complete this exercise.

At the end of the session I'll choose one of the five target weeks at random. Then I'll choose one of the nine questions at random. Then I'll choose one subject at random. The randomly chosen subject will receive whichever option they checked on the randomly chosen question for the randomly chosen target week. Thus, for each question it is in your interest to check the option you prefer.

**$1** **Daily RSF-Reward Certificate** **$1**

*This certificate entitles the holder to*

**$1**

*for every day that he or she attends the RSF during the week of*

**Monday, Oct 13** *through* **Sunday, Oct 19.**

**$1** **$1**

| | S | M | T | W | T | F | S |
|---|---|---|---|---|---|---|---|
| **SEPT** | | 1 | 2 | 3 | 4 | 5 | 6 |
| | 7 | **8** | 9 | 10 | 11 | 12 | 13 |
| | 14 | **15** | 16 | 17 | 18 | 19 | 20 |
| | 21 | 22 | 23 | 24 | 25 | 26 | 27 |
| **OCT** | 28 | 29 | 30 | 1 | 2 | 3 | 4 |
| | 5 | 6 | 7 | 8 | 9 | 10 | 11 |
| | 12 | **13** | 14 | 15 | 16 | 17 | 18 |
| | 19 | 20 | 21 | 22 | 23 | 24 | 25 |
| **NOV** | 26 | 27 | 28 | 29 | 30 | 31 | 1 |
| | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
| | 9 | 10 | 11 | 12 | 13 | 14 | 15 |
| | 16 | 17 | 18 | 19 | 20 | 21 | 22 |
| | 23 | 24 | 25 | 26 | 27 | 28 | 29 |

**For each question, check which option you prefer, A or B.**

| | **Option A** | | | **Option B** | |
|---|---|---|---|---|---|
| **1.** Would you prefer | ☐ | $1 for certain, paid Monday, Oct 20. | or | ☐ | The Daily RSF-Reward Certificate shown above. |
| **2.** Would you prefer | ☐ | $2 for certain, paid Monday, Oct 20. | or | ☐ | The Daily RSF-Reward Certificate shown above. |
| **3.** Would you prefer | ☐ | $3 for certain, paid Monday, Oct 20. | or | ☐ | The Daily RSF-Reward Certificate shown above. |
| **4.** Would you prefer | ☐ | $4 for certain, paid Monday, Oct 20. | or | ☐ | The Daily RSF-Reward Certificate shown above. |
| **5.** Would you prefer | ☐ | $5 for certain, paid Monday, Oct 20. | or | ☐ | The Daily RSF-Reward Certificate shown above. |
| **6.** Would you prefer | ☐ | $6 for certain, paid Monday, Oct 20. | or | ☐ | The Daily RSF-Reward Certificate shown above. |
| **7.** Would you prefer | ☐ | $7 for certain, paid Monday, Oct 20. | or | ☐ | The Daily RSF-Reward Certificate shown above. |
| **8.** Would you prefer | ☐ | $8 for certain, paid Monday, Oct 20. | or | ☐ | The Daily RSF-Reward Certificate shown above. |
| **9.** Would you prefer | ☐ | $9 for certain, paid Monday, Oct 20. | or | ☐ | The Daily RSF-Reward Certificate shown above. |

Figure B.4: Unincentivized and other elicitation tasks

# [PRACTICE]

For each target week you will also be asked to complete the following two exercises. Both of these exercises relate to the Daily RSF-Reward Certificate shown at the top of the page, which is the same as the one shown at the top of the preceding page. In addition, there will be one target week for which you will be shown no certificate, and you will be asked to complete only these last two exercises.

**$1** **Daily RSF-Reward Certificate** **$1**

*This certificate entitles the holder to*

**$1**

*for every day that he or she attends the RSF during the week of*

**Monday, Oct 13** *through* **Sunday, Oct 19.**

**$1** **$1**

| | S | M | T | W | T | F | S |
|---|---|---|---|---|---|---|---|
| **SEPT** | | 1 | 2 | 3 | 4 | 5 | 6 |
| | 7 | **8** | 9 | 10 | 11 | 12 | 13 |
| | 14 | **15** | 16 | 17 | 18 | 19 | 20 |
| | 21 | 22 | 23 | 24 | 25 | 26 | 27 |
| **OCT** | 28 | 29 | 30 | 1 | 2 | 3 | 4 |
| | 5 | 6 | 7 | 8 | 9 | 10 | 11 |
| | 12 | **13** | 14 | 15 | 16 | 17 | 18 |
| | 19 | 20 | 21 | 22 | 23 | 24 | 25 |
| **NOV** | 26 | 27 | 28 | 29 | 30 | 31 | 1 |
| | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
| | 9 | 10 | 11 | 12 | 13 | 14 | 15 |
| | 16 | 17 | 18 | 19 | 20 | 21 | 22 |
| | 23 | 24 | 25 | 26 | 27 | 28 | 29 |

Imagine that you have just been given the Daily RSF-Reward Certificate shown above, and that this is the only certificate you are going to receive from this experiment.

How many days would you attend the RSF that week if you had been given that certificate? _____

---

Now imagine that everyone in the room *except you* has just been given the Daily RSF-Reward Certificate shown above, and that this is the only certificate they are going to receive from this experiment.

What do you think would be the average number of days the other people in the room *(not including you)* would go to the RSF that week? _____

*(Your answer does not have to be a round number. It can be a fraction or decimal.)*

**Notes:** As part of this experiment some subjects will receive real certificates.

I will give a $10 prize to the subject whose answer to this exercise is closest to the correct, average RSF-attendance for subjects *(other than themselves)* who receive the certificate shown above. The prize money will be paid by check, mailed on Monday,.Oct 20.