Students, Volunteers and Subjects: Experiments on Social Preferences

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Abstract

Economic experiments are usually conducted with university students who voluntarily choose to participate. Outside as well as within the discipline, there is some concern about how this “particular” subject pool may systematically produce biased results. Focusing on social preferences, this study employs a representative sample of a city's population and reports behavioral data for five experimental decisions. The dataset allows for a ceteris paribus comparison between self-selected students (i.e. the standard subject pool) and the representative population. We demonstrate that in spite of volunteers' and students' effects, experimental subjects seem to be an appropriate subject pool for the study of social preferences.

Keywords: experimental economics, external validity, subject pool, self-selection bias, field experiment.

JEL Codes: C90, D03

An introduction on the importance of experimental techniques in economics is no longer necessary. Experimental economics has reached maturity and enjoys large-scale acceptance among economists as a useful tool for studying human behavior. The debate has now evolved towards the degree to which data from experiments can be used to build positive theories and ultimately to inform policy (Levitt and List 2007, Falk and Heckman 2009, Henrich, Heine, and Norenzayan 2010, Camerer 2011). In other words, are results coming from experimental economics externally valid?

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The main concern about external validity is related to certain features of experimental practices on the one hand (high levels of scrutiny, low stakes and the abstract nature of the tasks), and a very particular subject pool on the other. The latter has two dimensions. First, the subject pool in economic experiments is almost exclusively comprised of university students. More than the narrow socio-demographic array of characteristics that this group offers, what really threatens external validity is the existence of different behavioral patterns once such characteristics have been controlled for. We should say that there is student bias if, after controlling for socio-demographics, students behave differently than the general population.¹ Second, participants are volunteers. Naturally, the behavior of non-volunteers is not observed. There is a self-selection bias if volunteers share some attributes that make their behavior systematically diverge from that of non-volunteers.

Concerning student bias, there are two main sources of insights. The first comes from experiments using both students and individuals pooled from a target population (see for example Cooper et al. 1999; Fehr and List 2004; Haigh and List 2005; Cárdenas 2005; Palacios-Huerta and Volij 2009; and the recent review by Fréchette 2011).² The second comes from databases containing behavioral data drawn from more general populations. This allows economists to test whether different sub-samples (e.g. students) exhibit different behavioral patterns (Harrison, Lau, and Williams 2002, Fehr et al. 2003, Gächter, Herrmann, and Thöni 2004, Bellemare, Kröger, and van Soest 2008, Egas and Riedl 2008, Dohmen et al. 2010). In the realm of social preferences, these practices have been extensively used over the last years, giving rise to a large number of field experiments. There is now plenty of evidence demonstrating that students are slightly less “pro-social” than other

¹ That certain strata of the population are under-represented is obviously true. However, once the distribution of these characteristics is known for the general population, researchers can account for such differences by adjusting the right weights to their statistical models. The real question in extrapolating students’ behavior to general populations is whether the coefficient estimates differ across the groups due to non-controllable variables. Bothelo et al. (2005) carefully illustrate the appropriateness and importance of including socio-demographic controls in regression analyses.
² These belong to the family of the so-called artefactual field experiments (Harrison and List 2004). Despite all of their insights, their main purpose is not to serve as general tests for student bias.
groups in a variety of designs and settings.\(^3\) Note, however, that the bulk of this evidence comes from self-selected subjects and prior to controlling for socio-demographics. The reported differences cannot therefore be attributed to what we have called student bias without making some extra assumptions.

Concerning self-selection bias, research has been relatively limited since it involves obtaining behavioral data of individuals *not willing* to participate. For student populations, economists get hold of such datasets by making participation semi-obligatory during a class (classroom experiments). Among non-student populations, such a dataset is even more difficult to obtain. Reviewing the relevant studies in economics and taking into account the differences in the designs and methodologies, one cannot be conclusive.\(^4\)

Taking the evidence on student and self-selection bias in combination, it is tempting to suggest that self-selected students should not be a researcher’s first choice when studying social preferences and consequently prompts running field experiments and using other samples instead. However, such a suggestion would be implicitly assuming either no self-selection bias or no interaction between the student and self-selection. Studying the extrapolation of subjects’ behavior requires the *simultaneous* examination of student bias within both volunteers and non-volunteers and self-selection bias within both students and non-students.

Using the 2x2 factorial design depicted in *Figure 1a*, this paper reports data from a survey-experiment that allows such a ceteris paribus investigation of student and self-selection bias. A representative sample of a city’s adult population participated in three experimental games involving five decisions.

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\(^3\) Students have been shown to behave less generously (Carpenter, Burks, and Verhoogen 2005, Carpenter, Connolly, and Myers 2008, Belot, Duch, and Miller 2010), less cooperatively (Gächter, Herrmann, and Thöni 2004, Egas and Riedl 2008, Burks, Carpenter, and Goette 2009, Belot, Duch, and Miller 2010; Anderson et al. 2010) and less trustfully (Bellemare and Kröger 2007, Belot, Duch, and Miller 2010, Falk, Meier, and Zehnder forthcoming).

\(^4\) Within students: Cleave, Nikiforakis, and Slonim (2011) observe that volunteers reciprocate less in a Trust Game. Falk, Meier, and Zehnder (forthcoming) finds that students who volunteer to participate in experiments and students who do not donate equally to a charity. Eckel and Grossman (2000) report pseudo-volunteers as more generous than volunteers in a DG where the recipient is a charity. Within non-students: Bellemare and Kröger (2007) find no difference when comparing attributes between volunteers and non-volunteers. Anderson et al. (2010) compares truck drivers (a kind of pseudo-volunteer) with volunteers sampled from a non-student population and report non-significant differences when they play a social dilemma game.
In addition, a rich socio-demographic set of information was gathered in order to serve as controls, which we argue are necessary to analyze student and self-selection bias. Lastly, each individual was classified as a volunteer or non-volunteer based on their willingness to participate in future experiments in the laboratory. Our final sample ($N=765$ after excluding incomplete observations) therefore consists of both students and non-students as well as both volunteers and non-volunteers (see Figure 1b).

![Image](image.png)

**Figure 1: Experimental design sample classification**

I. Procedures

The experiment took place from November 23rd to December 15th 2010. A total of 835 individuals aged between 16 and 91 years old participated in the experiment. One out of ten participants was randomly selected to be paid. The average earnings among winners, including those winning nothing (18.75%), were €9.60.

**Sampling:** A stratified random method was used to obtain the sample. In particular, the city of Granada (Spain) is divided into nine geographical districts, which served as sampling strata. Within each stratum we applied a proportional random method to minimize sampling errors.\(^5\) Our sample

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\(^5\) The sample was constructed in four sequential steps: 1. We randomly selected a number of sections proportional to the number of sections within each district; 2. We randomly selected a number of streets proportional to the number of streets within each section; 3. We randomly selected a number of buildings proportional to the number of buildings on each street; 4. Finally, we randomly selected a number of apartments proportional to the number of apartments within each building. Detailed information can be found in supplementary materials available at www.ugr.es/local/pbg/City.htm.
consists of individuals who agreed to complete the survey at the moment the interviewers asked them to participate. Being interviewed in their own apartments decreased opportunity cost (thus increasing the participation rate). In order to control for selection bias within households, only the individual who opened the door was allowed to participate. Lastly, the data collection process was well distributed across both daytime and weekday. Our sampling procedure resulted in a representative sample (see Table S7 in the supplementary materials).

**Interviewers:** The data were collected by 216 university students (grouped in 108 pairs) enrolled in a course on field experiments in the fall of 2010. The students underwent ten hours of training in the methodology of economic field experiments, conducting surveys, and sampling procedures. Their performance was carefully monitored through a web-based system (details in the supplementary materials).

**Protocol:** The interviewers introduced themselves to the prospective participants and explained that they were carrying out a study for the University of Granada. Upon agreement to participate, the participants were informed that the data would be used for scientific purposes only and under conditions of anonymity according to the Spanish law on data protection. One interviewer always read the questions aloud, while the other noted down the answers (with the exception of the experimental decisions). The survey lasted on average 40 minutes and consisted of three parts. In the first part, extensive socioeconomic information of the participants was collected including, among others, risk and time preferences, and social capital. In the second part, participants played three paradigmatic games of research on social preferences, namely the Dictator Game (DG), the Ultimatum Game (UG) and the Trust Game (TG) (see Figure 2). In the last part, they had to state their willingness to participate in future monetary-incentivized experiments (which would take place in the laboratory at the School of Economics).

**Experimental Games:** At the beginning of the second part, and before any details were given about each decision in particular, the participants received some general information about the nature of the experimental games
According to standard procedures in experimental economics. In particular, participants were informed that:

- The five decisions involved real monetary payoffs coming from a national research project endowed with a specific budget for this purpose.
- The monetary outcome would depend only on the participant’s decision or on both his/her own and another randomly matched participant’s decision, whose identity would forever remain anonymous.
- One of every ten participants\(^6\) would be randomly selected to be paid, and the exact payoff would be determined by a randomly selected role. Matching and payment would be implemented within the next few days.
- The procedures ensured absolute double-blinded anonymity by using a decision sheet, which they would place in the envelope provided and then seal. Thus, participants’ decisions would remain forever blind in the eyes of the interviewers, the researchers, and the randomly matched participant.

\[\begin{array}{|c|c|c|c|c|c|c|c|c|c|}
\hline
\text{Dictator Game} & \text{Ultimatum Game (proposer)} & \text{Ultimatum Game (respondent) - strategy method} & \text{Trust Game (1st pl.)} & \text{Trust Game (2nd pl.)} \\
\hline
\text{Forsythe et al. 1994} & \text{Güth, Schmittberger, and Schwarze 1982} & \text{(Mitzkewitz and Nagel 1993)} & \text{Ermisch and Gambetta 2006} & \text{Ermisch and Gambetta 2006} \\
\hline
\text{"Mark with a circle the number of Euros you want to send to the other person"} & \text{"Mark with a circle the number of Euros you want to send to the other person"} & \text{"Mark the A with a circle in case you accept. If you reject the proposed division, mark the R"} & \text{"Mark with a circle the number of Euros you want to loan to the other person"} & \text{"Mark the 22 with a circle if you want to send back 22€ and keep 18€, or the 0 if you want to send nothing and keep all the 40€"} \\
\hline
0 & 2 & 4 & 6 & 8 & 10 & 12 & 14 & 16 & 18 & 20 \\
\hline
0 & 2 & 4 & 6 & 8 & 10 & 12 & 14 & 16 & 18 & 20 \\
\hline
\end{array}\]

**Figure 2: Experimental decisions**

Once the general instructions had been given, the interviewer read the details for each experimental decision separately. After every instruction set,

\(6\) In deciding 1/10 instead of higher probabilities (for instance 1/5), we took into account two issues: the cognitive effects of using other probabilities and the (commuting) costs of paying people given the dispersion of participants throughout the city. Interestingly, 297 subjects (39% of the sample) believed that they would be selected to be paid (last item of the second part).
participants were asked to write down their decisions privately and proceed to the next task. To control for possible order effects on decisions, the order both between and within games was randomized across participants, resulting in 24 different orders (always setting aside the two decisions of the same game).

Classifying students: Individuals between 18 and 26 years old who reported to be studying at the moment were classified as students. The upper age bound (26 years old) was selected taking into account the mean maximum age of the lab experiments taken place in the University of Granada and a large drop in the age histogram of our sample. Alternative upper bounds were also tested (see supplementary materials).

Classifying volunteers: Following Van Lange, Schippers, and Balliet (2011) in their application of the measure developed by McClintock and Allison (1989), we classified participants according to the response to the following question:

“At the School of Economics we invite people to come to make decisions with real money like the ones you made earlier (the decisions in the envelope). If we invite you, would you be willing to participate?”

Furthermore, in order to differentiate self-selection in economic experiments from the general propensity to help research studies and the need for social approval (see Levitt and List 2007), we also asked individuals about their willingness to participate in future surveys. A total of 478 stated that they would be willing to participate in future surveys, while only 350 said they would participate in experiments. Of these, 49 stated that they would not participate in a survey. In addition, two months after the experiment, we hired an assistant to call all the individuals classified as volunteers in order to confirm their interest. In particular, we requested participants’ authorization to include their data in the experimental dataset of the Economics Department (ORSEE). Of those who we were able to contact after two attempts on two

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Note, however that we have intentionally removed any helping framing. Van Lange, Schippers, and Balliet (2011, pg. 281) for example first stated: “the quality of scientific research of psychology at the Free University depends to a large extent on the willingness of students to participate in these studies” and then proceeded in asking them their willingness to participate in future studies.
consecutive days (60%)\(^8\), 97% of students and 83% of non-students confirmed their interest.

III. Results

As Figure 1b illustrates, our final sample (N=765) consists of:

- 22% students (n=170) according to the above classification.
- 46% volunteers (n=350) comprised of individuals who responded positively to the classification question explained above.
- 12% “standard” subject pool (students x volunteers) (n=90).

The first models in Table 1 (left-hand side) report the estimated behavioral effects of being a student, and a volunteer on an aggregate level. The second models explore the interaction effects of the two (student x volunteer). These models allow student bias to be studied separately within volunteers and non-volunteers and in the same manner, self-selection bias within students and non-students. The regressions in columns i, ii, and iii model participants’ offers in the DG, the UG and the difference between the two, thus capturing strategic behavior, respectively. Columns iv, v, and vi repeat the same exercise for the minimum acceptable offer (MAO) as a second mover in the UG, the decision to pass money or not in the binary TG, and the decision to return money or not as a second mover in the same game, respectively. Note that in all regressions we control for basic socio-demographics (age, sex, income and educational level) as well as for risk and time preferences, cognitive abilities and social capital. Table 2 reports the coefficient estimates from the between-group comparisons obtained by the corresponding Wald tests.

.... Table 1: Student and self-selection biases on behavior ...

**Student bias:** Students are more strategic players ($p=0.012$) mostly because they make less generous DG offers ($p=0.060$). However, these differences are never larger than 6% and 5.4% of the pie, respectively, for DG and UG-

\(^8\) Not answering the phone makes sense if we consider the enormous amount of telemarketing calls people receive in Spain and even more so given that the assistant made calls from a university phone number which is comprised of 13 digits like those of telemarketing companies. Note that regular private numbers in Spain have 9 digits.
DG. Through Wald tests, we identify the student bias that is mainly manifested among volunteers (A vs. C, \(p=0.028\); see Table 2).

**Self-selection bias:** Volunteers are more likely to both trust (6.6%) and to reciprocate the trust (7.7%)\(^9\) than non-volunteers in the TG (\(p=0.051\) and \(p=0.011\), respectively). However, the first difference vanishes when making pairwise comparisons within groups. That is, the aggregate effect is not specifically attributable to either students (A vs. B) or non-students (C vs. D) (\(p>0.12\) in both cases). The second difference can be essentially traced back to non-students (\(p=0.023\)) since it is largely insignificant for students (\(p=0.440\)). However, self-selection bias affects students as well: self-selected students make (marginally) significantly higher offers than the rest of students in the UG (\(p=0.084\)).

As a final exercise we compare self-selected students with both the rest of the sample (A vs. B+C+D) and group D, which comprises non-students, non-volunteers. We find the behavior of group A to be different from the rest of the sample only regarding UG offers, and at marginally significant levels (\(p=0.092\)), as they offer €0.66 more. As can be inferred from Table 2, this effect must be emanating from the self-selection bias revealed in this decision among students. The comparison between groups A and D yields only one (marginally) significant result as well. Self-selected students increase their offers between DG and UG by €0.94 more than non-self-selected, non-students (\(p=0.094\)). This effect makes sense as well since students have been reported previously to be more strategic players than non-students (A+B vs. C+D). Finally, since self-selection was revealed to be an issue only among non-students (C vs. D), the absence of significant differences in TG behavior (\(ps>0.49\)) is not surprising.

...Table 2: Between-group comparisons....

Due to the complexity of non-linear interaction effects (Ai and Norton 2003), we replicate the regressions of columns iv, v, and vi using one dummy for each group (A, B, C, and D). The results remain exactly the same.

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\(^9\) These values refer to the marginal effects corresponding to the probit estimates reported in Tables 1 and 2.
Additionally, replication of the regressions using alternative upper bounds for age (i.e. 24 and 28 years old) in the definition of students does not alter the general picture (see Tables S2 and S3 in the supplementary materials).

IV. Discussion

The present paper presents data that allows disentangling the separate effects of student and self-selection bias. Evidence for both is found. However, the results also tell another parallel story: in five experimental decisions and following the exact same procedures for all subjects, self-selected students have been proven to behave in a very similar manner with every other group separately and in combination. Indeed, at the conventional 5% level only one significant effect concerning self-selected students is observed and, in addition, the difference is economically small. That said, we suggest that the findings do not discredit the use of self-selected students in economic experiments measuring social preferences. Models and policy suggestions by choice architects\textsuperscript{10} built on experimental results should thus be considered valid. The results caution, however, on the use of alternative samples such as self-selected non-students that typically participate in artefactual field experiments since the effect of self-selection can be even more pronounced outside the student community (self-selection bias is proved to be an issue mainly among non-students).

Appendix

An extensive appendix (with procedures, sampling information, analysis of the representativeness of the population, robustness, etc.) is provided at the project webpage: \url{http://www.ugr.es/~pbg/City.htm}

\textsuperscript{10} Blood and organ donations, tax, environmental and savings policy, and health care and retirement programs are examples (Bernheim and Rangel 2005; Amir et al. 2005; Riedl 2009 and Thaler and Sunstein 2008).
References


Burks, Stephen V., Jeffrey P. Carpenter, and Lorenz Goette. 2009.


<table>
<thead>
<tr>
<th></th>
<th>DG</th>
<th>UG</th>
<th>UG-DG</th>
<th>MAO</th>
<th>TG trustor</th>
<th>TG trustee</th>
</tr>
</thead>
<tbody>
<tr>
<td>students</td>
<td>-0.060* (0.032)</td>
<td>-0.067 (0.044)</td>
<td>0.007 (0.015)</td>
<td>-0.006 (0.021)</td>
<td>0.054** (0.021)</td>
<td>0.047 (0.030)</td>
</tr>
<tr>
<td>volunteers</td>
<td>0.039 (0.026)</td>
<td>0.036 (0.024)</td>
<td>0.023 (0.015)</td>
<td>0.016 (0.016)</td>
<td>-0.010 (0.019)</td>
<td>0.013 (0.019)</td>
</tr>
<tr>
<td>students x</td>
<td>0.013 (0.052)</td>
<td>0.027 (0.027)</td>
<td>0.013 (0.015)</td>
<td>0.013 (0.016)</td>
<td>0.0769 (0.039)</td>
<td>0.149 (0.201)</td>
</tr>
<tr>
<td>volunteers</td>
<td>0.039 (0.026)</td>
<td>0.036 (0.024)</td>
<td>0.023 (0.015)</td>
<td>0.016 (0.016)</td>
<td>-0.010 (0.019)</td>
<td>0.013 (0.019)</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.0941</td>
<td>0.0943</td>
<td>0.0223</td>
<td>0.0224</td>
<td>0.0600</td>
<td>0.1012</td>
</tr>
<tr>
<td>$LR$</td>
<td>3.80***</td>
<td>3.79***</td>
<td>1.46**</td>
<td>1.46**</td>
<td>5.81***</td>
<td>81.52***</td>
</tr>
</tbody>
</table>

Notes: The dependent variables are (i) the fraction offered in DG; (ii) the fraction offered in UG; and (iii) the fraction offered in UG - the fraction offered in DG; (iv) the minimum acceptable offer as a fraction of the pie in UG; (v) TG decision as a trustor - 1 if (s)he makes the loan, zero otherwise; and (vi) TG decision as a trustee - 1 if (s)he returns part of the loan, zero otherwise. Models i and ii are Tobit regressions, model iii is an OLS regression, model iv is an ordered probit regression, while the last two models are Probit regressions. $N=765$ in all regressions. Controls are: age, gender, education, household income, social capital, risk preferences, time preferences, and cognitive abilities. The variables are explained in depth in the supplementary materials. All models are also controlling for order effects. All the likelihood ratios ($LR$) shown correspond to $Chi^2$ statistics, except for column iii, where they are based on $F$. Robust SE clustered by interviewer (108 groups) and presented in brackets. *, **, *** indicate significance at the 0.10, 0.05 and 0.01 levels, respectively.
Table 2: Between-group comparisons

<table>
<thead>
<tr>
<th></th>
<th>DG</th>
<th>UG</th>
<th>STRAT</th>
<th>MAO</th>
<th>TG trustor</th>
<th>TG trustee</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Student bias</strong></td>
<td></td>
<td></td>
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<tr>
<td>(A+B) vs (C+D)</td>
<td>-0.060*</td>
<td>0.008</td>
<td>0.054**</td>
<td>-0.039</td>
<td>-0.168</td>
<td>-0.083</td>
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<tr>
<td>A vs C</td>
<td>-0.031</td>
<td>0.021</td>
<td>0.061**</td>
<td>-0.002</td>
<td>-0.093</td>
<td>-0.130</td>
</tr>
<tr>
<td>B vs D</td>
<td>-0.068</td>
<td>-0.007</td>
<td>0.047</td>
<td>-0.079</td>
<td>-0.242</td>
<td>-0.034</td>
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<tr>
<td><strong>Self-selection bias</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(A+C) vs (B+D)</td>
<td>0.040</td>
<td>0.023</td>
<td>-0.010</td>
<td>0.020</td>
<td>0.197*</td>
<td>0.240**</td>
</tr>
<tr>
<td>A vs B</td>
<td>0.051</td>
<td>0.044*</td>
<td>0.000</td>
<td>0.078</td>
<td>0.309</td>
<td>0.170</td>
</tr>
<tr>
<td>C vs D</td>
<td>0.037</td>
<td>0.017</td>
<td>-0.013</td>
<td>0.001</td>
<td>0.159</td>
<td>0.266**</td>
</tr>
<tr>
<td><strong>Subject-pool bias</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>A vs (B+C+D)</td>
<td>-0.012</td>
<td>0.033*</td>
<td>0.039</td>
<td>0.021</td>
<td>0.080</td>
<td>0.049</td>
</tr>
<tr>
<td>A vs D</td>
<td>-0.017</td>
<td>0.038</td>
<td>0.047*</td>
<td>-0.002</td>
<td>0.067</td>
<td>0.136</td>
</tr>
</tbody>
</table>

Notes: Letters A, B, C and D refer to the groups depicted in Figure 1a. Group A denotes students volunteers; B students non-volunteers; C non-students, volunteers; D non-students, non-volunteers. (A+B) correspond to all students (volunteers and non-volunteers); (C+D) to all non-students (volunteers and non-volunteers); (A+C) to all volunteers (students and non-students); (B+D) to all non-volunteers (students and non-students). Lastly (B+C+D) correspond to the sum of the subject pool except students volunteers. *, ** indicate significance at the 0.10, and 0.05 levels, respectively.
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