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ECONOMIC EXPERIMENTS OUTSIDE THE LAB

FILIPPOS EXADAKTYLOS

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Abstract

In doing economic experiments one can infer causal relationships. Control and replicability are the two main virtues that allow this. Conducting experiments in the field one almost necessarily sacrifices both control and chances for exact replicability. However, I propose that there are three very good reasons that make it worth. All three are emanating from viewing Economics' ultimate goal as to shape public policy and to improve public health. The first reason is in order to answer research questions, which require features not available in the lab. The second is in order to test the validity of a result in more "real-world" environment that can be found in the field. The last is because experiments being done in the field can be communicated easier to policy makers and choice architects and can be potentially more convincing.

This Thesis is comprised of four essays, all of which are analyzing data gathered from experiments in the field. The first one introduces a new bias that can systematically affect laboratory experiments coming from the fact that these experiments are being done with subjects who (a) are recruited by an open-call, (b) take their decisions in the artificial environment of a laboratory and (c) have a significant opportunity cost in participating in the study. In an attempt to study the potential influence of these factors combined an experiment has been run in the field where participants were the passengers of a train with no prior information about the experiment and no opportunity cost. The second essay is about the appropriateness of self-selected students serving as subjects in the study of social preferences and its separate effect from what we call a student bias. Traditionally the two has been studied in combination. This study aimed in separate the two by recruiting a representative sample of a city's population and classifying them as volunteers and non-volunteers. The third experiment, explores how actual alcohol intoxication and beliefs about alcohol intoxication interact in order to determine risk behavior. Measuring beliefs in a traditional laboratory experiment, we argue, creates artificialities that obscure the real effect. For this reason we opted for a field experiment where alcohol consumption occurred without any interference whatsoever of the researchers. The last filed experiments uses a large database (the same as experiment two) in order to study the determinants of what is called in the literature altruistic giving. We suggest that in order a clear answer to be given, one should employ a sample that allows socio-demographic factors to affect behavior. The point of this study is to demonstrate that this kind of altruistic giving can be explained a number, rather than one ingle factor.

I suggest that conducting experiments in the field is a valuable source of knowledge. However it should be seen as a complement of traditional laboratory experiment, other empirical data and firm econometric methodology.

Resumen

Por medio de los experimentos económicos se pueden inferir relaciones causales. Las características que permiten esto son la existencia de control y la posibilidad de réplica. Cuando se realizan experimentos de campo, casi ineludiblemente no obstante, uno tiene que sacrificar parte de ese control y de la replicabilidadperfecta. Sin embargo, mi propuesta es que existen tres potentes razones que aportan gran valor a este método. Las tres razones emanan de la visión de que la Economía tiene la finalidad última de influir y mejorar tanto la salud como las políticas públicas. La primera se basa en responder a preguntas de investigación que requieran factores ausentes en el laboratorio. La segunda se refiere a examinar la validez de un resultado en un ambiente más real, que es el que aporta el campo. La última radica finalmente en que los resultados de experimentos realizados en el campo son más fáciles de comunicar a los hacedores de política económica y, potencialmente, pueden convencen más fácilmente.

Esta tesis consta de cuatro ensayos, los cuales analizan datos obtenidos mediante experimentos en el campo. El primer capítulo presenta un nuevo sesgo que puede afectar sistemáticamente a los experimentos de laboratorio y que nace del hecho de que éstos son realizados con sujetos que (a) son "reclutados" mediante una llamada abierta y general, (b) toman sus decisiones en el ambiente artificial del laboratorio y (c) hacen frente a un coste de oportunidad por participar en el estudio. En un intento por explorar la influencia potencial de la combinación de estos factores, realizamos un experimento de campo cuyos participantes eran pasajeros de un tren, sin información a priori sobre el experimento y sin coste de oportunidad. El segundo ensayo versa sobre la idoneidad de usar estudiantes auto-seleccionados como sujetos para el estudio de las preferencias sociales aislando el efecto de la auto-selección del efecto de lo que nosotros venimos a denominar "sesgo estudiantil". Tradicionalmente, los dos efectos han sido estudiados en combinación. Este estudio tenía como finalidad el separar éstos mediante el reclutamiento de una muestra representativa de la población de toda una ciudad y la clasificación de los sujetos como voluntarios y no voluntarios. El tercer experimento incide en cómo la intoxicación alcohólica y la percepción sobre la propia intoxicación interactúan para determinar el comportamiento de riesgo. Al medir tales percepciones en un experimento de laboratorio tradicional, creemos, se crean artificialidades que generan opacidad sobre el efecto real. Por esta razón, optamos por un experimento de campo donde el consumo de alcohol ocurre sin ninguna participación de los investigadores. El último experimento de campo emplea una base de datos grande (la misma usada para el experimento segundo) para explorar los determinantes de lo que se conoce en la literatura como donación altruista. Nuestra sugerencia es que para poder dar una respuesta clara se necesita explotar una base de datos que permita a los factores socio-demográficos afectar el comportamiento estimado. El trasfondo de este estudio es demostrar

que este tipo de donaciones altruistas se pueden explicar por un grupo de factores, en vez de por uno solo de ellos.

Desde aquí quiero sugerir que la realización de experimentos de campo se constituye en una fuente fundamental de conocimiento. Sin embargo, éstos deberían ser considerados como un complemento de los experimentos de laboratorio tradicionales, basados en un prototipo diferente de datos empíricos y en una metodología econométrica firmemente fundamentada.

Table of Contents

Chapter 1

Introduction

Ever since Economics evolved from mainly an empirical-based discipline to one that is based on theoretical assumptions, it has become overcrowded with theories and hypotheses. In science, an experiment is a way to test and falsify theories and hypotheses. Therefore, I think that is a positive thing that economists are conducting experiments, even if it took thirty years, a coalition with Behavioral Economists and a Nobel Prize to be established among economists.

In what follows I present a behavioral approach about why some economists even nowadays might be reluctant of conducting experiments and even more in the field. Then I discuss some good reasons in order to do field experiments and then some caveats. In continuation I shortly present the articles included in this Thesis and the basic results.

Rarely do people change their opinion. Most often we form beliefs and then just stick to them by searching for evidence to support it while consciously or subconsciously overlooking evidence against it. Scientists are no exception. Neither are Economists. Probably it even applies more since ego and the ability to find supporting evidence is pronounced among them. In addition, most academics have spend all of their adult life in only one job and many times dedicated to only one issue (growth is achieved through democratization or free market, poor people are happier or less happy than rich ones, Keynesian policy does or does not work). Scientific evidence can make that hypotheses, subjective views and wishful thinking looks as if they were facts (God does not exist, parents should “invest” on their second offspring, impatience people are less happy, colored people have less abilities etc). The Clever Hans example is a famous case where scientists thought that a horse could make maths, but it finally turned out that it was the scientists’ belief that drove the results. Economists of the so-called School of Chicago who have spent their whole academic life supporting the Neo-classical model (rationality and private interest being in the heart of it) would found it difficult to abandon it because of new evidence. And the truth is that enough of such evidence is being provided in order to make one doubt about the descriptive ability of the model. However, behaviorally, I think it is more convenient to adapt the evidence on the existing model. Gary Becker, responding to evidence coming from behavioral and experimental economics, recently stated in an interview: “It doesn’t matter if 90 percent of people can’t do the complex analysis required to calculate probabilities. The 10 percent of people who can, will end up in the jobs where it’s required”. I thus think that it could be that experiments are not rejected as a methodology, but due to the fact that are connected with research whose evidence goes against the Neo-classical paradigm.

Why experimenting in the field?

Nevertheless I think that there are at least three reasons for one to conduct experiments in the field. All three are emanating from viewing Economics' ultimate goal as to shape public policy and to improve public health. (1) The laboratory can be restrictive in that some features of the real life, necessarily in order for the research question to be answered, cannot be reproduced or simulated in the laboratory. If a one for example wants to test the effect of cannot be achieved (or maybe it can, but the point remains; if one thinks that the laboratory is not appropriate or not the most appropriate is the field then he or she go to the field). (2) The second reason field experiments can be preferable can be due to the artificiality that laboratory experiments sometimes create (even if it is not always a point against): laboratory experimentation is important because of the control it offers. However when control is referring to human's behavior it can create artificialities that are misleading. In particular in economic experimentation, control often refers to restricting the possible options among which subjects can choose and to de-contextualization of the task. It is important to see the extent that a robust laboratory result survives in a more complex situation and the generality of the result. For example suppose that laboratory results inform that less educated people give less in allocations games (a Dictator Game for example). Measuring whether this is the case in a philanthropic giving in a real charity is important to see the validity of the DG experiments (based on which we create models). I addition, testing whether this is true for other forms of altruism (say, blood donation) has mediate policy implications. Once such feedback is taken, the researcher should return to the laboratory and probably run more treatments. (3) The last reason might seem only a little scientific, however I suggest that it is equally important. Field experiments are more easily communicable and more persuading to policy makers and choice architects. Even economists' most robust evidence that norm enforcement is a way to achieve public good provision are the laboratory experiments, a politician will be much more easily convinced by an experiment done with CEO's for example.

Caveats of field experiments

Of course, field experiments come with caveats. The most obvious are two: loss of control and decrease chances of exact replicability and thus validation of the results. For example it is not always possible to create the right counterfactual in the field. Thus that means loss of control in the sense that you cannot control the result without the treatment effect. Another problem that one might face in the field is regarding recruitment of subjects. If not done properly one might not be able to control for selection issues. Another related issue is about proper randomization. In the case of experimentation with humans, randomization guarantees control and it is absolutely crucial.

This Thesis

In this thesis I present four essays, all of which report data from a field experiment. The first paper is a short note concerning a potential bias that laboratory experiments might be victim of. It refers to the total effect that the laboratory creates. In particular it concerns with the connotations laboratory is linked: it creates an environment that subjects take a role-playing attitude that might systematically affect their behavior. In order to test such a possible effect we run an experiment (a Dictator and an Ultimatum Game) in the field and more particularly in the wagons of a train. This served the purposes since the passengers (acting as subjects) had no previous information about the conduction of the experiment; thus no expectations about participating at a study at all. In addition the wagons allowed standard practices of anonymity to be preserved. The results were not conclusive but pinpoint to such an effect. The offers in both games had a significantly wider range of usual laboratory results.

The second study is a carefully designed experiment in order to address self-selection and student bias. The literature on the issue, even though not poor has failed to carefully separate the two effects and moreover to address the possible interactions between the two. We employed a representative sample of a city's adult population and played three experimental games (namely, the Dictator Game, the Ultimatum Game and the Trust Game). We additionally classified all subjects as volunteers and no-volunteers and lastly both students and non-students comprised our sample. Thus we had the opportunity to compare the behavior of each group separately. Quite importantly we undertook an econometric analysis controlling for all the basic socio-demographics. Even if that seems a minor point, it was actually this, which allowed us to demonstrate that once such effects have been controlled for, the behavior of self-selected students is *not* that much different than that of the other groups.

The third essay is a good example of how the techniques of experimental economics can be used to answer a variety of questions. It is an experiment about distinguishing the separate effects of Alcohol intoxications and the beliefs people hold about their own intoxication level on risk-taking behavior. As we demonstrate in the article, a field design was necessary in order for such a question to be answered properly. We needed a environment where people was consuming alcohol prior to any knowledge of an experiment. This way, their estimation of their intoxication level was being done without any demand effects intervening with the task. Before actual intoxication level and self-estimation was measured, subject played a lottery that constituted the risk-taking measure of the game. Results show that underestimation rather than actual level intoxication is what affects risk taking positively.

The last essay tries to identify and test the various possible explanations of what has being labeled in the literature as altruistic giving. A representative sample of a city's adult population (the same as study two) allowed for variation in the socio-demographics, a necessary, we argue, information in order to test the various theories proposed so far. Altruistic giving was

measured by the revealed behavior in the Dictator Game. In addition, a large questionnaire was designed and administered to all participants measuring a number of attitudes, which were the possible candidates for explained observed behavior. The results clearly demonstrate that it is a number, and not only one motivation that drive altruistic giving. Socio-demographics are not very important in determining giving.

Chapter 2

Bargaining in the Trains

Abstract

The fact that subject in economic experiments (a) are recruited by an open-call, (b) take their decisions in the artificial environment of a laboratory and (c) have a significant opportunity cost in participating in a study, might create certain expectations that systematically alter their behavior. We study these effects by designing an experiment in the field were participants were the passengers of a train with no prior information about the experiment and no opportunity cost. We observe decisions in two experimental games (the Dictator Game and the Ultimatum Game). The results reveal a wider range of observed behaviors that give ground (although not conclusive) to the stated hypothesis.

1. Introduction

On January 14th 2007, the prestigious Boston Theater was absolutely packed. Everything was booked in a night when the average ticket worth 100 dollars. Quite naturally actually since Joshua Bell, one of the most famous and talented violinists of the world, was giving a concert. Two days earlier, Bell, using the same 3.5 million dollars-worth of violin performed incognito for 45 minutes at a metro station in Washington D.C. during a rush hour. He earned 32 dollars out of thousands of people that passed by. The thing that stroked him the most was the absence of applause at the end of each piece of music. However had he expected to receive the same kind of appreciation he would have committed an inference error in trying to generalize behavior from a very particular sample (music funs) to other people. The more technical term would be a sampling error because the audience in the theater was self-selected in the concert, all sharing an appreciation for classical music. Thus, their evaluation of Bell's talent (and their willingness to pay for a ticket) is not representative of a wider population.

Now imagine an economist wanting to measure risk attitudes and deciding to do so in a casino. Obviously, again, the sample might be biased in that mostly people who enjoy gambling are expected to be found in such an environment, even thought it is not that pronounced as the previous example: not only gamblers are found in a casino. Following the same argumentation, the risk attitudes that the economists will estimate will be a good predictor only for the risk attitudes of other habitués of the casino but not for the more general population. What is more important however is that the observed behavior of the participants might not even be representative of *their own* behavior. That is because even if the sample of this example were *not* gamblers, it is absolutely logical to assume that this particular night in this particular place they will behave as such. In other words even if they did not go to the casino with a "gambling" attitude, they probably became "players" the moment they stepped in the casino's fancy carpet. So measuring their risk attitudes in the casino might mis-represent their "true" every-day risk attitudes.

Something similar might be happening in the laboratories where economists conduct their experiments. Even assuming that self-selection is not an issue, students might automatically transform to "subjects" when they seat in front of the computer monitor. Thus their revealed preferences might not correspond to their true ones. This effect is distinct from self-selection bias (see Eckel and Grosmann 2000, Bellemare and Kröger 2007, and Falk et al. forthcoming), recruitment bias (Rutstrom 1998) or subject pool bias (see Brañas et al. 2012). It is closely related to what Carpenter and co-workers named social framing (Carpenter et al. 2005) and supply effects (Carpenter et al. 2010). However, these effects have been studied in isolation. The hard test would be an environement where subjects are not just students, they did not respond to open call from experiments and lastly the opportunity cost is minimized

(subjects deciding to come to a traditional laboratory experiment has in way invested their time to this, which can also be creating expectations).

In this short note, we present an experiment designed to test these effects in combination (in the spirit of Bullock 1999).[1] The experiment took place in the trains. Passengers served as subjects. They had no previous information that an experiment was going to happen, thus no previous expectations. The environment did not raise any expectations of experimentation. Passengers did not have any opportunity cost since they had to expect to reach their destinations in any case. Additionally the wagons served very well as separator in order to preserve anonymity between subjects. We were lucky enough to have almost half of the subjects being University students, thus allowing within group comparisons. In what follows we describe the procedures and the main results.

2. Experimental Design and Results

The experiment took place from 11th of May 2008 to 12Th of the same month. All sessions took place in the wagon of a train in approximately 60-minutes roots in northern Greece (details in the Appendix). A total of 143 passengers participated as subjects: 76 in the D.G. (38 Dictators) and the 67 in the U.G. (39 as proposers)[2]. Instructions were given in a written form (in a single page) and orally in small groups. Once the subject had agreed to play (response rate were about 30%), she was given 10€ in coins of 1€, a black plastic bag (at which she was instructed to put the money she was offering to player 2) and an envelope to put her part of the money. In the case of the DG, she kept the envelope and the experiment came to an end for her while the experimenter delivered the black plastic bag to the assigned receiver. In the case of the UG, the experimenter took both the black plastic bag and the envelope from the proposer and delivered the former to the receiver. In case of acceptance the receiver kept the plastic bag while the envelope was returned to the proposer. In case of rejection the experimenter kept both the envelope and the plastic bag. In both cases the proposer and the receiver was debriefed.

The results for UG and DG are depicted in Figure 1 and Figure 2 respectively. What one immediately notices is that the offers in both games have a clear focal point of 50% of the pie. In the UG, 61.54% of the subjects are offering half of the pie while in DG 34, 21%. The second that thing is that apart from the 50-50 split, the rest of the choices are distributed across the whole range of offers. Most importantly, we observe many DG and UG offers

[1] Bullock deigned a Prisoners Dilemma experiment changing three parameters simultaneously, namely the ability to communicate, to form partners by their own, and to change partners during the experiment. He observed significantly higher level of cooperation.

[2] Responders are only 22 because in one of the sessions, we were asked from the ticket collector to aboard the experiment. Since proposers had already have made their allocations, we have decided to resolve the issue by «accepting» all the offers without actually maching a real person in the role of reciever.

bigger that 50% of the pie. However, this is not likely to be driven by confusion since nobody for example offered 0 in the UG.

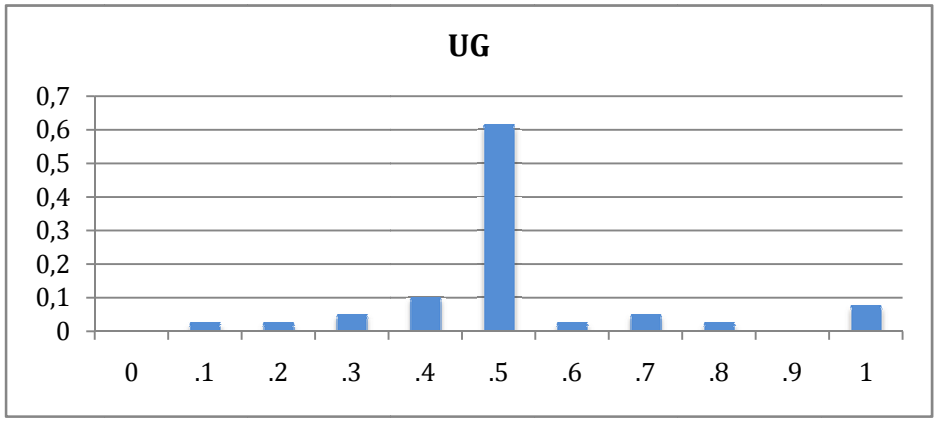

Figure 1: *Ultimatum Game Allocations*

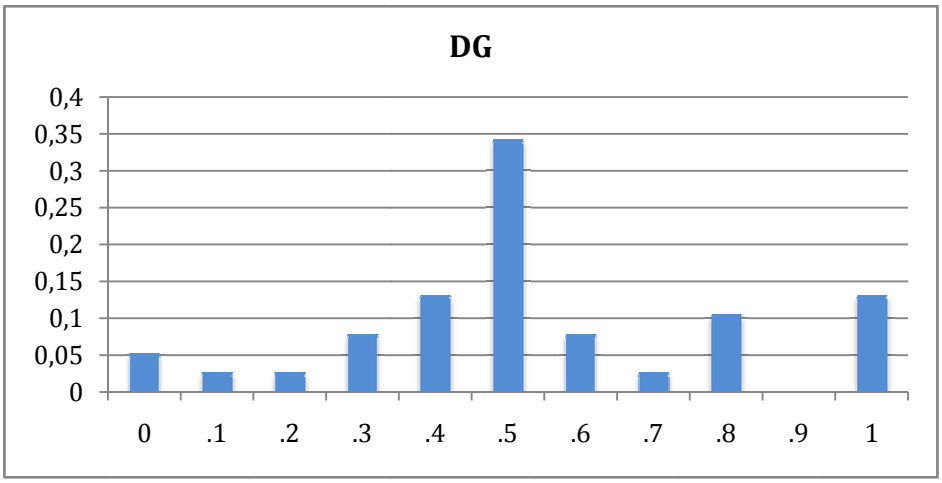

Figure 2: *Dictator Game Allocations*

In the DG, we do not observe many 0 offers that is the very usual in the laboratory experiments in many studies. Engel (2011) in a meta-analysis of 131 DG experiments summing up to 616 treatments and a total of *N*=20813, 36.11% of all subjects gave 0. Figure 3 compares the dataset of the trains with that of the Engel (2011). It is also obvious that in the trains a higher percentage gave 50% as well as the whole endowment. In total 34% of subjects in the trains opted for an offer bigger that the equal split, while almost 70% gave either 50 per cent or more. Thus we see that in the trains, in contrast with what is observed in the laboratories people do not opt for the individual maximizing option, but rather are much more generous (reaching offers of 100% of the pie that very rarely is met in the lab) and with a clear focal point of the equal split.

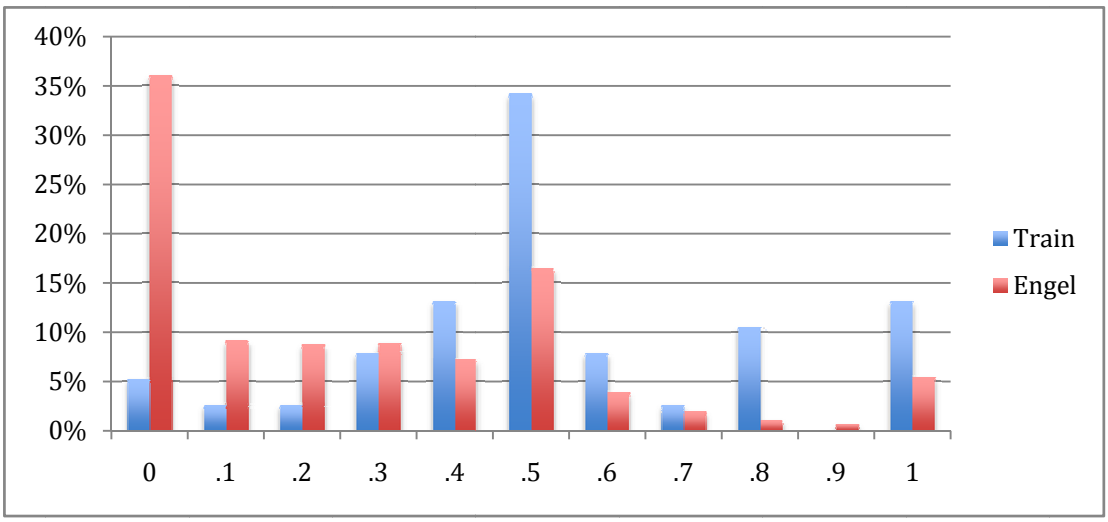

Figure 3: *Train Dataset vs. dataset from the meta-study by Engel (2011)*

In the UG, the picture even more clear: 61,53 % of the Proposers offer the 50-50 split. The norm is very clear. However what is equally interesting is that 18% of the proposers offer more than half of the pie. Turning to the UG responder's behavior we also see an interesting pattern. We observe that the probability of acceptance is not explained by the amount of offer. However an offer is more likely to be rejected if it deviates from the equal split, irrespectively on whether it deviates towards a more selfish or a less selfish distribution. Taken together (the behavior of UG proposers and responders), it reveals a very clear social norm of an equal split.

3. Conclusions

Thaler and Camerer (1995) note that Dictator giving might better be explained by what they call manners to describe the social norms than as an expression of agents' social preferences. Binmore (2010) made the same in essence point very recently. The present note, adds value to this interpretation by demonstrating that in a different environment where different norms exist, results in two popular experimental game stopped being as homogeneous and as expected as laboratory experiments demonstrate (Engel 2011 for a review). Most people in DG were very generous, while UG results reveal a prevailing norm of an equal split.

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Chapter 3

Students, Volunteers and Subjects: Experiments on Social Preferences

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 data from a survey-experiment conducted with a representative sample of a city's population (N=765). We report behavioral data from five experimental decisions in three canonical games. The dataset includes students and non-students as well as volunteers and non-volunteers. We separately examine the effects of being a student and being a volunteer on behavior, which allows a ceteris paribus comparison between self-selected students (students*volunteers) and the representative population. In spite that instances of both effects are found, our results suggest that self-selected students are an appropriate subject pool for the study of social preferences.

1. Introduction

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 behaviour. 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 et al. 2010, Camerer 2011). In other words, are results coming from experimental economics externally valid?

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 behavioural 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.[3]Second, participants are volunteers. Naturally, the behaviour of non-volunteers is not observed. There is a *self-selection bias* if volunteers share some attributes that make their behaviour 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).[4] The second comes from databases containing behavioural data drawn from more general populations. This allows economists to test whether different sub-samples (e.g. students) exhibit different behavioural patterns (Harrison et al. 2002, Fehr et al. 2003, Gächter et al. 2004, Bellemare et al. 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 groups in a variety of designs and settings.[5] 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.

[3] 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.

[4] 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.

[5] Students have been shown to behave less generously (Carpenter et al. 2005, Carpenter et al. 2008, Belot et al. 2010), less cooperatively (Gächter et al. 2004, Egas and Riedl 2008, Burks et al. 2009, Belot et al. 2010; Anderson et al. 2010) and less trustfully (Bellemare and Kröger 2007, Belot et al. 2010, Falk et al. forthcoming).

Concerning self-selection bias, research has been relatively limited since it involves obtaining behavioural 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.[6]

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' behaviour 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. 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*).

2. 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.

[6] *Within students:* Cleave et al. (2011) observe that volunteers reciprocate less in a Trust Game. Falk et al. (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.

Within each stratum we applied a proportional random method to minimize sampling errors.[7] Our sample 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).

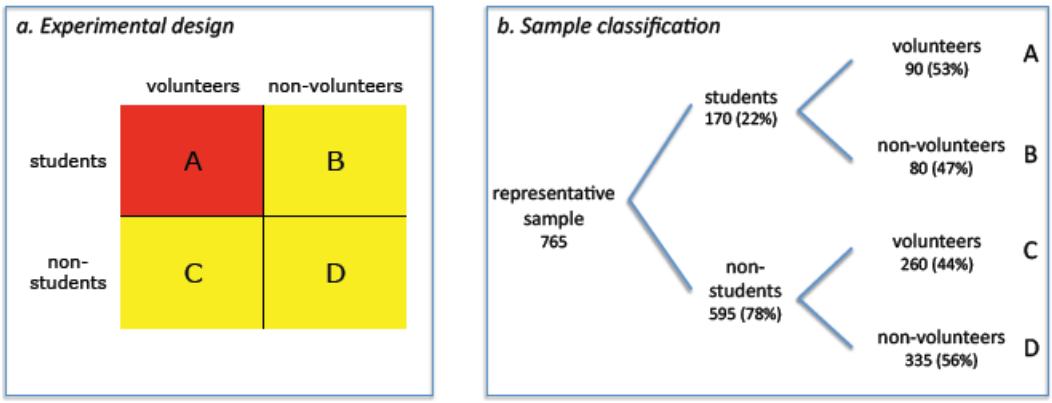

Figure 1: *Experimental design sample classification*

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

[7] 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.

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[8] 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.

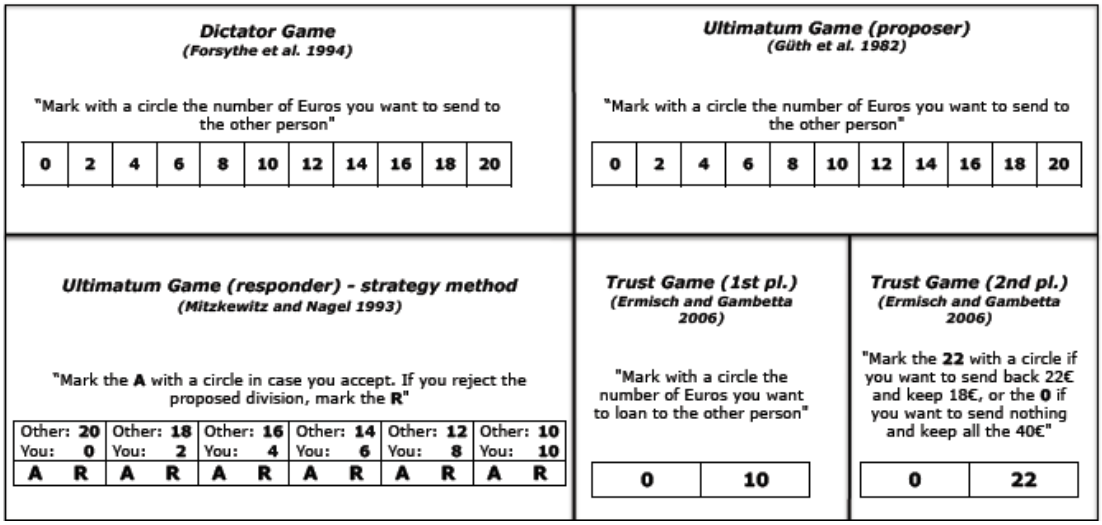

Figure 2: *Experimental decisions*

[8] 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).

Once the general instructions had been given, the interviewer read the details for each experimental decision separately. After every instruction set, 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 et al. (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?"* [9]

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 consecutive days (60%)[10], 97% of students and 83% of non-students confirmed their interest.

3. Results

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

- 22% students (*n*=170) according to the above classification.

[9] Note, however that we have intentionally removed any helping framing. Van Lange et al. (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.

[10] 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.

- 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 behavioural 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 behaviour, 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.

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-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%)[11] 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 behaviour 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

[11] These values refer to the marginal effects corresponding to the probit estimates reported in *Tables 1* and *2*.

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

	DG		UG		UG-DG		MAO		TG trustor		TG trustee	
	i		*ii*		*iii*		*iv*		*v*		*vi*	
students	-0.060*	-0.067	0.007	-0.006	0.054**	0.047	-0.039	-0.079	-0.167	-0.242	-0.083	-0.034
	(0.032)	(0.044)	(0.015)	(0.021)	(0.021)	(0.030)	(0.105)	(0.165)	(0.152)	(0.198)	(0.143)	(0.191)
volunteers	0.039	0.036	0.023	0.016	-0.010	-0.013	0.019	0.000	0.196*	0.159	0.239**	0.266**
	(0.026)	(0.024)	(0.015)	(0.016)	(0.019)	(0.019)	(0.092)	(0.112)	(0.101)	(0.103)	(0.094)	(0.117)
students x *volunteers*		0.013		0.027		0.013		0.0769		0.149		-0.096
		(0.052)		(0.027)		(0.039)		(0.201)		(0.259)		(0.268)
R^2					0.0941	0.0943	0.0223	0.0224	0.0600	0.0604	0.1012	0.1013
LR	3.80***	3.79***	1.46**	1.46**	5.81***	5.68***	56.02***	56.60***	78.49***	81.52***	98.87***	98.20***

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.

(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 behaviour (*p*s>0.49) is not surprising.

Table 2: *Between-group comparisons*

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

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.

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. 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).

4. 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[12] 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).

[12] 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).

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Chapter 4

The separate effects of self-estimated and actual alcohol intoxication on risk-taking: A field experiment

Abstract

Many risky actions are carried out under the influence of alcohol. However, the effect of alcoholic intoxication over the willingness to take risks is complex and still remains unclear. We conduct an economic field experiment in a natural, drinking and risk-taking environment to analyze how both actual and self-estimated blood alcohol concentration (BAC) levels influence subjects' choices over monetary lotteries. Our results reveal a negative impact of both actual and self-estimated BAC levels on risk-taking. However, for male and young subjects, we find a positive relationship between BAC underestimation (a pattern of estimation error which mainly occurs at high BAC levels) and the willingness to choose riskier lotteries. Our findings suggest that a risk compensation mechanism is activated only when individuals' own intoxication level is consciously self-perceived to be high. We conclude therefore that human propensity to engage in risky activities under the influence of alcohol is not due to an enhanced preference for risky choices. In addition to the suggestion in the existing literature that such propensity is due to a weakened ability to perceive risks, our results indicate that an impaired self-perception of own intoxication level may also be an important factor.

1. Introduction

The prominent role of alcohol in human life and its attendant socio-economic and health implications have attracted the research interests of many scholars. During the last five decades, many studies have been carried out in this realm. A great deal of attention has focused on the visible effect of alcoholic intoxication on risky and hazardous behaviors like criminal or aggressive activities (Ensor and Godfrey 1993; Lau et al. 1995; Richardson and Budd 2003), dangerous/risky driving patterns (Burian et al. 2003; Russ et al. 1988; Beirness 1987), risky sexual behavior (Halpern-Felsher et al. 1996; Cooper 2002) or excessive gambling (Sjoberg 1969; Cutter et al. 1973; Meier et al. 1996).

The great majority of these studies share three main characteristics. Firstly, the main focus is usually on the direct pharmacological effects of alcohol administration, leaving other important aspects related to short-term, post-consumptive behavior uninvestigated. One of such crucial parameters is the perception individuals hold about their own alcoholic intoxication levels; a psychological component that has not been studied in depth so far. A second characteristic, emanating from their focus, is the methodology used in these studies. Traditionally, research on the effects of alcohol ingestion is carried out through laboratory-based experiments (Meier et al 1996; Lane et al. 2004; Breslin et al. 1999). However, the behavioral effects of alcohol undoubtedly arise from other dimensions of its consumption other than simple pharmacological effects. For instance, the psychological and emotional state of drinkers, the social as well as the physical environment, or the laws and social norms governing a given occasion are all inseparable from alcohol-related behavioral patterns. Finally, when investigating the causal effects of alcohol on risky behavior, studies have used a wide range of risk-taking measures. Nevertheless, previous research has fallen short of distinguishing between how alcohol affects risk preferences on the one hand, and risk perceptions (Weber 1997) or abilities/skills (Byrnes 1998)on the other.

To address the above issues, we ran a field experiment in a natural drinking environment. We gauged participants' blood alcohol concentration (BAC) levels and elicited their estimations about own BAC as a measure for self-perceived intoxication level (Beirness 1987). We then analyzed separately how each measure affects subjects' risk-taking behavior in a lottery task designed specifically for this purpose, which was free of both cognitive complexity and subjective perceptions of risk.

Although there is ample epidemiological and clinical evidence linking risky behaviors to the effects of alcoholic intoxication (Cherpitel 1999; Testa and Collins 1997; Donovan and Marlatt 1982; Ferguson and Horwood 2000), the exact relationship between alcohol and risk-taking at the individual level remains unclear. Experimental results exploring such a relationship have been inconclusive. For the time being, we know that alcohol induces maladaptive risky

decision-making due to an impaired evaluation of the consequences of the existing alternatives/choices (Kyngdon and Dickerson 1999; Euser et al. 2011; Fromme et al. 1997; George et al. 2005; Lane et al. 2004).

Lane et al. (2004) found that an incorrect response to previous monetary losses led intoxicated individuals (two groups, reaching about 0.4g/L and 0.8 g/L of BAC) to choose more risky options despite being linked to long-run losses. In their experiment, the expected value (that is, $pu_x + (1-p)u_y$ where *p* is the probability that event *x* will occur and u_x and u_y are the respective payoffs for events *x* and *y*) of the risky option was always below that of the non-risky one, which led risk-taking to be maladaptive. Nevertheless, the only way for subjects to adapt their choices was by considering their experience from previous rounds since the experimenters did not explicitly provide the probabilities corresponding to the two possible outcomes within the risky option. The effect of alcohol on subjects' working memory (Euser et al. 2011) and other perceptual factors may thus be behind the higher risk taken by intoxicated subjects in such a task.

To the best of our knowledge, no alcohol-related study has tackled the question of whether intoxication promotes human preference for risky choices *per se*– that is, the taste for choices which, not being less adaptive than others, are simply riskier in terms of greater outcome variance. If this is the case, then alcohol intake would trigger risk-taking in two different ways: intoxicated individuals would *(i)* misevaluate the negative consequences involved in risky choices (Lane et al. 2004; Euser et al. 2011), and *(ii)* for options perceived as being equally adaptive, prefer riskier choices more often than sober individuals. Additionally, it can also be the case that alcohol does not alter individuals' risk preferences – as reported by Meier et al. (1996) and Breslin et al. (1999) for intoxicated subjects carrying out repeated gambling tasks with known probabilities and fluctuant adaptiveness of risk. Even more, it has been shown that intoxicated individuals, for instance, take a longer time to complete the "Stroop's color and word" cognitive test (Stroop 1935) and thus keep the number of errors low (Gustafson and Källmén 1990). Along the same line, individuals perform better on a tracking task when expecting alcohol than when expecting placebo after having received the same alcoholic dose (Finnigan 1995). These last observations indicate that a similar compensation process might lead humans to attenuate the harmful effects of alcohol in decision-making under risk when high intoxication is correctly self-estimated (as suggested by Burian et al. 2003). Such a process would develop an aversion to more volatile, riskier choices when options are perceived as identically adaptive.

We therefore hypothesize that, apart from the actual intoxication level, what may also be driving risky behavior is the drinker's self-perceived intoxication level and probably the comparative relationship between the two as well. In the DUI (Driving Under the Influence) paradigm for instance, high levels of ethanol in the driver's blood are likely to impair his

perceptual and psychomotor skills (reflexes, attention, and reaction time) (Mitchell 1985; Moskovitz and Robinson 1988). At the same time, what the driver expects about the level of his intoxication and the impairment of both his skills and judgment might also crucially affect his behavior (McMillen and Wells-Parker 1987). It is therefore possible that the level of under/overestimation of own intoxication represents a crucial parameter influencing risky decision making. Even though self-perception of alcoholic intoxication has featured in the research agenda of clinical researchers, inquiries have been mainly conducted using the placebo effect treatment (Ross and Pihl 1989; Marlatt and Rohsenow 1980). The implementation of a laboratory driving task using expectancy deception procedures by Burian et al. (2003) is a representative example of this methodology. The present study, however, is the first to explore the link between self-estimated intoxication and risk preferences, and measure how individuals' under/overestimations of their actual alcoholic intoxication levels influence behavior.

Also novel is the field methodology we employed. Indeed, the sterile environment of the laboratory does not provide for the relevant conditions and relationships found in the real world and thus excludes their scientific study (Thombs et al. 2003). It is known that the field poses several restrictions on the level of control over the phenomenon at study. However, looking at the other side of the same coin, along with new methodologies, new directions and opportunities for research arise. During the last years, within the relatively newly-born field of experimental economics, field experiments are accelerating in frequency. These experiments are aiming at the higher external validity of results and capturing critical environmental effects absent in laboratories (Harrison and List 2004). Applied to the present study field, economic experimental methodology offered important advantages over classical laboratory experiments: *(i)* experimental subjects did not self-select in the study; *(ii)* demand effects were minimized, regarding the subjects' concern of being enrolled in an experiment on the effects of alcohol or substance intake; *(iii)* alcohol consumption was done prior to the study with absolutely no involvement of the researchers; *(iv)* alcohol consumption and risk-taking took place in an environment where such behaviors are more natural; and *(v)* according to a basic principle of experimental economics (saliency of rewards, Smith 1976), the elicitation of behavioral measures was monetarily incentivized.

After controlling for other personal and environmental variables, our statistical analysis revealed a significant negative relationship between the subjects' measured BAC levels and their willingness to choose riskier options from lotteries with constant and positive expected value. We also found a very similar influence of self-estimated BAC levels over lottery choice. Hence, our findings suggest a tendency for individuals to take less risky choices along with the increase in both actual and self-perceived intoxication as part of a compensation process (Bäckman and Dixon 1992). However, when digging into the relationship between actual and self-estimated BAC levels, we found that individuals tend to overestimate own BAC at low

intoxication levels, but underestimate it at higher levels. Paradoxically, the extent of such underestimation was positively related to risky choices for male and young subjects. It is thus suggested that individuals engage in compensatory behavior insofar as their levels of intoxication are conscientiously self-perceived to be high. The finding that underestimating own intoxication level can spur risky choices in male and young sub-populations indicates that the relationship between alcohol intake and risk-taking is not clear-cut, and that self-perceived intoxication could emerge as a crucial factor to explore in future investigations.

2. Design and Procedures

2.1 Recruitment & timing

The experiment took place from May 22nd to May 24th 2008 around the amusement kiosks ((Supporting) Figure S1) at the yearly festival of Granada (Spain) called the "Feria del Corpus Christi". Potential subjects were randomly approached by the same principal interviewer (a native of Granada) and asked to participate in a study for the local university which would give them the opportunity to earn up to €60.[13] From a total of 73 participation requests, 71 individuals responded affirmatively and were finally recruited[14]; a fact that removes any kind of self-selection bias concerns. First stage instructions were given by the principal interviewer to groups of at most three individuals. However, all participants made their decisions individually, which were then revealed to only one of the three experimenters, thus ensuring independence among individual decisions.

Upon acceptance, subjects chose the lottery of their preference (the one which they would later play), which constitutes the risk measurement of the study. A short questionnaire was then administered to gather information about subjects' height, weight, age, gender, drinking habits(average number of standard drink units per drinking occasion), use of marijuana (within the last three hours), and previous experience with alcohol measurement (number of times subjects had previously used an alcoholmeter). Up to this point, subjects had no clue that the research focus of this study was alcohol intake; this prevented any experimental demand effects. Soon after completing the questionnaire and before implementing the lottery, subjects were asked whether they would like to participate in another (surprise) task which offered the possibility of earning an additional €5 if they correctly guessed their BAC levels. After accepting to participate, all 71 subjects received the additional information on this extra task and subsequently made estimations of their BAC levels. The experimental process ended with the subject's actual BAC measurement, implementation of the chosen lottery and

[13]Although we were aware of the possibility of creating a focal point (€60) by revealing subjects' potential earnings, we used such a recruitment strategy to make participation more likely.

[14] All participants gave their explicit verbal consent before proceeding. They were also informed that they were allowed to opt out of the experiment at any time. Additionally, upon payment, all subjects signed a receipt stating ex-post their written approval.

finally the payment. The timeline describing the sequence of events of the experiment is depicted in Figure 1.

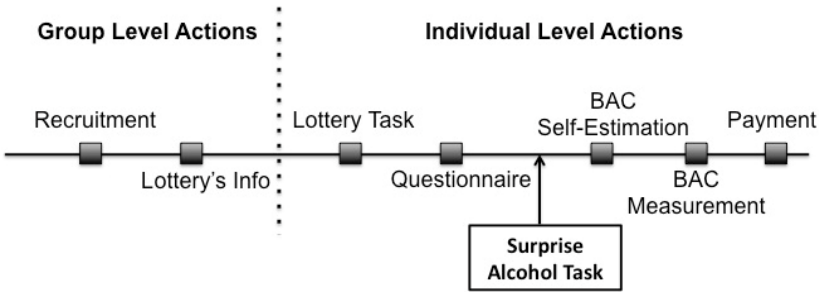

Figure 1. **Timeline of the experiment**

2.2 The field

Traditionally, local festivals in Spain are typical places for having fun, and involve, among others, folkloric activities, gambling games and alcohol consumption. Such an environment offers ideal and convivial conditions for the research purposes of our study. The "Feria del Corpus Christi" is the most popular festival in Granada, attracting all sorts of people and therefore minimizing any possible sample bias. Our final sample was comprised of individuals having consumed from zero up to large amounts of alcohol, who, moreover, had done so on their own and prior to any knowledge of participation in a study. The highest BAC levels present in our sample are hardly reached by dose-administration in laboratory settings for ethical and technical reasons. In addition, the lottery game used to measure risky behavior fitted perfectly to the general "playful" environment of the festival, diminishing in this way any experimentation flaws (e.g., demand effects).

The field allows behaviors connected with alcohol consumption and risk-taking to develop naturally in a way that the laboratory would not. To further investigate the role of the environment on behavioral expressions, we ran two separate sessions corresponding to different affect cues and behavioral dispositions. The first one (n=46), which we call the *family* session, took place on Thursday May 22nd at around 8:00-11:00 p.m. when the festival was more family oriented. The second one (n=25), denoted the *party* session, took place on Saturday May 24th after midnight at around 1:00-4:00 a.m. in an evidently party-mood atmosphere.

Finally, one disadvantage related to the nature of this field experiment was the lack of information regarding the time and the general conditions surrounding the subject's alcohol consumption. However, BAC levels obtained from breath samples is probably the most reliable and realistic measure of alcoholic intoxication in the field, and is normally used by authorities and firms to detect intoxicated drivers or employees, respectively.

2.3 Measuring risk attitudes

Participants' risk preferences were measured by subjects' unique choice over six different lotteries, which corresponded to different potential payoffs (prizes) and winning probabilities but to the same expected value. Table 1 presents the main characteristics of the six lotteries. Starting with the zero-risk lottery (100% of earning €10) on the left of the table, winning probabilities gradually decrease in a simple fashion as we move to the right. In contrast to other alcohol-risk studies (Lane et al. 2004; Kyngdon and Dickerson 1999; Euser et al. 2011; Fromme et al. 1997; George et al. 2005), in this task risk taking is not maladaptive; the expected value across all lotteries is purposely kept constant at €10 by increasing the potential prize in each lottery in proportion to the risk increase.

Our lottery-task is a simpler and more visual version of the risk-taking measure by Sabater-Grande and Georgantzís [36] (setting r=0)[15]. Risk-taking increases along with the probability assigned to the less favorable payoff, which is always zero. Thus, choosing the lot-60 corresponds to higher risk-taking behavior (in ordinal rather than in quantifiable terms) compared to choosing any other lottery. Accordingly, lot-50 is chosen by a person who exhibits lower risk-taking behavior than subjects choosing lot-60, but higher compared to persons who choose lot-40 or less. Obviously, choosing lot-10 is considered the least risk-taking behavior.

In designing the lottery-choice task, we responded to a tradeoff between richness of information of subjects' risk preferences and simplicity. Especially in a field context complexity implies noise on the data (see the relevant discussions on Andersen et al. 2010 and Dave et al. 2010). Moreover, in this particular experiment simplicity was a key feature of the design since controlling for risk perceptions was principal to the study. Our measure provides sufficient heterogeneity.

Lotteries were represented by six different boxes (Figure S2) with visible colored balls inside. Every box contained one winning, yellow ball and a different number of non-winning, pink balls. Lot-10 contained no pink balls, lot-20 contained only one, and lot-30 contained two pink balls and so on until lot-60, with five non-winning, pink balls. Each participant had to select one box. The content of the selected box was dropped into an opaque bag, from which the participant was allowed to pick only one ball that determined the lottery's final outcome.

[15] This measure is in accordance with several risk-taking theories. Nevertheless, under expected utility assumptions, our measure fails to identify the lottery choice of a risk-neutral agent.

Lottery	10	20	30	40	50	60
Probability	100%	50%	33%	25%	20%	17%
Prize	10	20	30	40	50	60
Exp. Payoff	10	10	10	10	10	10

Table 1. **Features of the risk-taking task**

This game-like task was selected due to its visual simplicity and its appropriateness to the festive environment hosting our study. Special care was taken to ensure the ease of comprehensibility of the task by presenting the winning probabilities and natural frequencies in an easily visible way. In this way, we managed to factor out any subjective effect related to individuals' risk perception. Moreover, contrary to other risk-taking measures such as driving tasks (e.g., Mitchell 1985; Moskovitz and Robinson 1988) or more complex lotteries (e.g., Meier et al. 1996; Breslin et al. 1999), subjects' cognitive abilities or skills should not influence decision making.

According to standard practices in experimental economics, the task was monetarily incentivized procuring dominance, monotonicity and saliency (Induced Value Theory, Smith 1976). Incentive compatibility ensures that subjects truthfully reveal their private values. Real incentives are particularly relevant in the case of risk-related analyses (Slovic 1969).

2.4 Measuring actual and self-estimated blood alcohol concentration

The surprise BAC self-estimation (*e*BAC henceforth) task took place upon agreement by the subjects after completion of the questionnaire and before taking the BAC measurement. BAC was measured in g/L using an ACE-AL 6000 breathalyzerafter subjects rinsed their mouth with mineral water for 30 seconds. In order to induce common information and reference points about alcoholic intoxication measurement across subjects, we informed participants *(i)* that the maximum permitted BAC when driving is 0.5 g/L under the Spanish traffic law; *(ii)* about the official correspondence between a unit of alcoholic drink (beer) and its effect on the BAC of an average weighted male or female[16]; and *(iii)* that BAC depends on individuals' weight, the time-gap between alcohol consumption and BAC measurement and the food ingested.

[16]Depending on the subject's gender, one of the following hints were given (according to the Spanish Directorate General for Traffic): a) For average weight women (60 kg), two small glasses (250ml) of beer correspond to 0.5 g. of alcohol per blood lt. b) For average weight men (70 kg), two tube-glasses (333ml) of beer correspond to 0.5 g. of alcohol per blood lt.

Monetary incentives were also introduced in this task. Subjects would win an additional €5 note if they were able to approximate their actual BAC within an allowed deviation of ±0.1 g/L. Given the existing incentive for accuracy and the information tips provided, and after controlling for subjects' experience with an alcoholmeter, subjects' significant deviations from the actual BAC should be mainly driven by their self-perceived intoxication level due to alcohol intake. Beirness (1987) has reported that individuals who self-estimate lower levels of BAC systematically "feel" themselves to be less intoxicated, which suggests a direct relationship between both measures. Moreover, self-estimated BAC, as opposed to self-reported intoxication, is quantitatively comparable across subjects.

2.5 Statistical analysis

We first implemented non-parametric tests to highlight any interesting differences among individuals and sessions. Moreover, in order to study the impact of alcohol over risk taking, we controlled for such differences through an ordered logistic regression. The fact that our dependent variable – subjects' lottery choice (10, 20, 30, 40, 50, 60) – is both discrete and ordinal justifies the adequacy of such a model. Finally, linear and locally weighted regressions were performed to identify and explain other relationships when necessary.

3. Results

The sample under examination consists of 70 subjects[17] (40 males) aged between 18 and 59. Table 2 shows descriptive statistics for the participants in each experimental session and identifies significant differences between sessions obtained through a Mann-Whitney test. Participants in the *family* session were significantly younger ($P<0.05$) and the reported BACs ($P<0.01$), *e*BACs ($P<0.05$), and drinks consumed per drinking occasion ($P<0.05$) were significantly lower. As expected, different experimental sessions were associated with different drinking patterns, with the *party* environment being linked to more severe alcoholic consumption. Spearman's rank-order correlation reveals that subjects' self-reported alcohol habits – average number of standard drinks consumed per drinking occasion – positively correlates with BAC ($\rho=0.44$, $P<0.01$) and *e*BAC ($\rho=0.37$, $P<0.01$).

[17] One subject was excluded from the sample for not satisfying independent decision-making (external influence from peers).

SESSION	Obs	Male (%)	Age (yr)	BMI (kg/m^2)	BAC (g/l)	*e*BAC (g/l)	Alcohol habits (drinks)	Earning (€)
Family	45	51.11 (±7.5)	26.73 (±1.01)	23.89 (±0.6)	0.349 (±0.068)	0.462 (±0.079)	4.02 (±0.48)	12.2 (±2.06)
Party	25	68 (±9.52)	30.88*** (±1.67)	24.04 (±0.81)	0.76*** (±0.102)	0.758** (±0.102)	5.36** (±0.57)	15.8 (±3.88)
TOTAL	70	57.14 (±0.60)	28.21 (±0.91)	24.04 (±0.48)	0.496 (±0.061)	0.568 (±0.064)	4.5 (±0.38)	13.5 (±1.90)

Table 2. Summary statistics by session. Mean values (±SEM) of principal variables in each experimental session. *Age*, *BMI*, *e*BAC and *alcohol habits* are self-reported variables. The variable *alcohol habits* indicates the number of drinks consumed per drinking occasion. **, *** indicate significantly higher rank of the variable in that session at the 0.05 and 0.01 level, respectively (Mann-Whitney test).

Measured BAC was zero for 26 (37.1%) subjects, although only 15 (21.4%) of them declared no alcoholic intake prior to the start of the experiment. Maximum values for BAC and *e*BAC were 1.8 g/l and 2.5 g/l, respectively. We found a strong positive correlation between BAC and *e*BAC for both the whole sample (ρ=0.73, P<0.01) and the subsample of subjects who declared having drunk alcohol before the experiment (ρ=0.54, P<0.01, n=55). However, in line with previous studies on self-perceived intoxication (Thombs et al. 2003; Beirness et al. 1993), there exists an increasing propensity to underestimate own BAC level along with the increase of the actual level. Figure 2 shows a locally weighted regression (red line) of *e*BAC on BAC. The dashed line displays equal values of both variables. The crossing point of the two lines is around 0.82 g/l. Hence, for BACs lower than 0.82 g/l subjects tended to overestimate their intoxication level, whereas they underestimated it for BACs above that value[18]. Similar results were obtained for the subsample of subjects (n=55) who declared having ingested alcohol prior to the experiment (blue line).

In terms of risk profiles, 70% of subjects chose lotteries with a probability of success higher than 1/4 – i.e. lotteries 10, 20, and 30. The distribution of lottery choices is illustrated in Figure 3. Table 3 shows the impact of alcohol-related variables over risk-taking. We present coefficients estimated by ordered logistic regressions with subjects' lottery choice as the dependent variable. All the models are controlled for personal and environmental characteristics, which is crucial given the field source of the data. Column 1 reports the impact of BAC over the willingness to choose riskier lotteries, which is negative and significant (P<0.05). *e*BAC was found to have a similar influence over the dependent variable (P<0.05) (column 2). The models in columns 4 and 5 depict significant interactions of BAC

[18]However, quadratic OLS regression supports the existence of a concave relationship between the two variables (P<0.05) for the whole sample but not for the aforementioned subsample (P>0.4) (available upon request). Using only their subsample of intoxicated subjects, Thombs et al. (2003) also failed to find a concave relationship.

and *e*BAC with the other explanatory variables, respectively[19]. As model 4 shows, only the interaction of BAC with gender significantly influences the dependent variable ($P<0.05$). A Wald test supported that the negative impact of BAC over lottery choice is significant for females ($P<0.01$) but not for males ($P>0.8$). The interaction of *e*BAC with gender, however, did not yield significance, whereas its interaction with age did ($P<0.01$) (column 5). The negative influence of *e*BAC over choosing riskier lotteries was found to be mainly due to younger subjects (Ps<0.05 for all ages<31, Wald test).

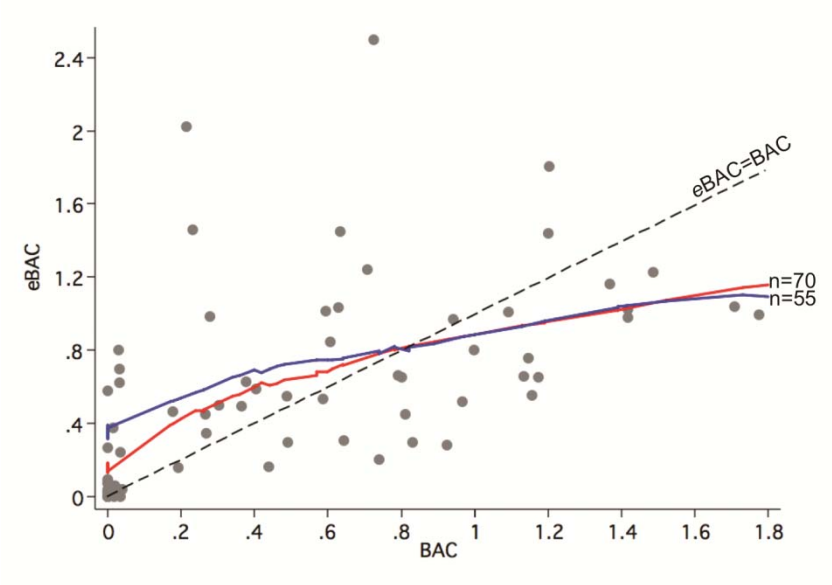

Figure 2. Lowess smoother of *e*BAC as function of BAC.Red and blue lines represent locally weighted regression of *e*BAC on BAC for the whole sample and for the subsample with *e*BAC>0, respectively. For purposes of clarity, scatter dots are illustrated allowing for a 5% error.

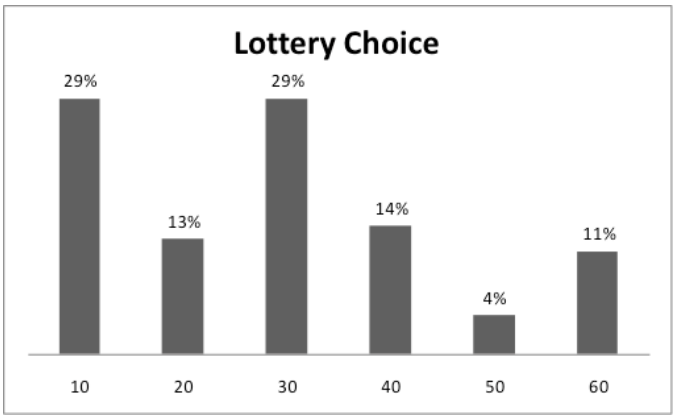

Figure 3. Distribution of lottery choices. Percentage of subjects choosing each of the six possible lotteries.

[19] The remaining possible interactions were not significant at conventional levels ($Ps>0.1$). Models with interaction terms are replicated with OLS methodology due to the complexity of nonlinear marginal effects in logistic regressions with interactions (Ai and Norton 2003). OLS regressions yield similar main results (upon request).

To analyze the influence triggered by under/overestimation of own BAC level on the dependent variable (columns 3 and 6), we reduced the sample to those subjects who declared having drunk alcohol before participating in our experiment (n=55). Note that this subsample also includes subjects (n=11) who declared a positive BAC, but whose actual BAC proved to be zero after the measurements. Moreover, we constructed an index variable called *underBAC*, which simply captures the difference between BAC and *e*BAC, such that the higher the value of *underBAC*, the larger the subject's underestimation level. This variable falls within the interval [-1.8, 0.8] with 43.64% of subjects actually underestimating their own BAC (that is, with *underBAC*>0). Although we found no significant effect of *underBAC* over the willingness to choose riskier lotteries (column 3), the interactions of *underBAC* with both gender (P<0.05) and age (P<0.01) yielded significant estimates (column 6). A positive and significant effect of underestimation over the willingness to take risks for young male subjects under 30 years old (n=23) was confirmed using the appropriate Wald tests (all Ps<0.05). On the contrary, the effect of underestimation over risk taking was negative and significant for female subjects over 33 years old (all Ps<0.05), although very few observations (n=5) satisfied this condition.

When performing an OLS regression on the continuous dependent variable *underBAC* (see Table S2) we found that for each level of BAC, younger individuals underestimate their own BAC level to a marginally significant higher extent (P=0.075) than older ones. Following the same methodology and splitting the sample by gender, we realized that the impact of youth on underestimation was significant only for male subjects (P<0.05, n=34). Therefore, young male subjects were more likely to underestimate their own intoxication levels and to consequently increase risk taking than female ones. Moreover, we found that non-heavy drinking males – as measured by the number of drinks per drinking occasion – tended to underestimate their own BAC level (P=0.055). On the other hand, no significant predictors of underestimation were found for females.

With regards to the control variables used in the previous logistic models, we can make the following comments. Firstly, unlike previous studies (e.g., Anderson and Mellor 2008), we found that the influence of age on risk-taking is significantly positive (although concave in most cases); a finding which can be attributed to the specific characteristics of our field experiment.

Ordered Logistic Regressions						
Dep. Variable:	**Main effects models**			**Interaction effects models**		
Lottery choice	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
BAC	-1.301** (0.586)			-2.388*** (0.784)		
*e*BAC		-1.212** (0.578)			-6.6*** (2.126)	
*under*BAC			0.383 (0.534)			7.709** (3.26)
BAC x male				2.579** (1.127)		
*e*BACx age					0.172*** (0.065)	
*underBAC*x male						3.055** (1.516)
*underBAC*x age						-0.3*** (0.109)
male	-0.089 (0.691)	0.052 (0.692)	0.242 (0.717)	-0.585 (0.966)	0.372 (0.72)	1.367 (0.995)
age	0.626*** (0.213)	0.598*** (0.206)	0.112** (0.052)	0.698*** (0.233)	0.533** (0.227)	0.812** (0.409)
age^2	-0.008*** (0.003)	-0.008*** (0.003)		-0.01*** (0.003)	-0.008** (0.003)	-0.012* (0.006)
BMI	0.112 (0.091)	0.132 (0.091)	0.18* (0.101)	0.135 (0.096)	0.074 (0.095)	0.219* (0.115)
alc. habits (drinks)	-0.023 (0.076)	-0.023 (0.08)	-0.131 (0.087)	-0.106 (0.085)	0.024 (0.085)	-0.217** (0.103)
marijuana	-0.044 (0.71)	-0.067 (0.733)	0.159 (0.799)	0.18 (0.73)	-0.381 (0.762)	0.035 (0.921)
party session	0.851 (0.536)	0.594 (0.523)	0.531 (0.584)	2.265** (0.923)	0.562 (0.535)	1.972* (1.093)
Party ses. x male				-2.283** (1.15)		-2.251* (1.311)
observations	70	70	55	70	70	55
LR (chi^2)	21.28***	21.25**	22.75***	28.14***	29.77***	38.05***
pseudo R^2	0.0926	0.0925	0.1261	0.1225	0.1296	0.211

Table 3: The impact of BAC and self-estimated BAC over the willingness to take risk. Standard errors in brackets. *, **, *** indicate significance at the 0.10, 0.05, and 0.01 levels, respectively. Models 3 and 6 depicting the effect of *underBAC* are reduced to the subsample of subjects who declared having ingested alcohol before the experiment. Regressions including *e*BAC and *underBAC* are additionally controlled for subject's experience with the alcoholmeter. Due to the reduced sample size, age squared and interactions between variables are excluded from those models in which their estimates are not significant in order to maximize the degrees of freedom of the models. In Table S1, regressions 2, 3, 5, 6 are repeated, excluding *e*BAC's outliers. No important differences were observed on the basic regressors.

Secondly, the interaction between gender and experimental session (*party* or *family*) was found to be significant in two out of three models: while females were less willing to take risks in the *family* session than in the *party* session, no differences in willingness to take risks across sessions were detected for males. This result may be in line with the literature asserting that females' attitude toward risk is more context-dependent than that of males (Croson and Gneezy 2009). In this vein, the affect state triggered by the party environment of the nocturnal session and the one activated during the evening session seem to generate two different contexts for risk taking (Arkes et al. 1988; Isen and Patrick 1983; Kahn and Isen 1993; Moore and Chater 2003).

Finally, we found that subjects' BMI and alcohol habits (drinks per occasion) had a weak impact on the lottery choices for some models. The former was related positively and the latter negatively to the dependent variable. No main or interaction effect of marijuana use was found. It must be said, nevertheless, that this experiment was not designed to explore relationships other than those associated with alcoholic intoxication and risk preferences. Thus, whatever insights extracted from the last findings should be taken with caution.

4. Discussion

While most studies agree that alcohol consumption is associated with risky behavioral patterns like dangerous driving (Burian et al. 2003; Russ et al. 1988; Beirness 1987), risky sexual behavior (Halpern-Felsher et al. 1996; Cooper 2002) and violence (Ensor and Godfrey 1993; Lau et al. 1995; Richardson and Budd 2003), there is no consensus on exactly how alcohol influences an individual's willingness to take risks. On the one hand, it is still unclear whether the resulting risky behaviors after alcoholic exposure are due to an enhanced preference for riskier choices in concomitance with the impaired ability to perceive risks and/or to evaluate the possible negative consequences associated with those behaviors (Lane et al. 2004; Kyngdon and Dickerson 1999; Euser et al. 2011; Fromme et al. 1997; George et al. 2005). On the other hand, we still do not know whether it is only the pharmacological effect of alcohol intake or also a psychological component which influences alcohol-related risky behaviors. So far the role of important psychological factors around the alcohol-risk relationship remains surprisingly unexplored. Concretely, how high an individual perceives his own intoxication has not been deeply investigated as a factor intimately linked to risk-taking.

In this study, we report the results of an economic field experiment designed to study the effect of alcoholic intoxication over risk-taking in three different dimensions: we separately analyze how subjects' actual BAC, self-estimated BAC, and over/underestimation of own BAC influence their willingness to choose risky lotteries with real monetary incentives. The

use of self-estimated BAC allows us to monetarily incentivize the elicitation of an individual´s self-perceived intoxication by rewarding subjects' correct guesses about their own BAC levels. In addition, self-estimated BAC offers better comparability across subjects than self-reported intoxication. In any case, previous evidence suggests a direct relationship between both measures (Beirness 1987).

Contrary to previous research based on risk-related behavioral games or gambling tasks – in which subjects' abilities play a role and often there is ambiguity about the exact risk involved across different choices – our design isolates subjects' willingness to take riskier choices as it minimizes the scope of different perceptions about the risk or negative consequences involved by choices across subjects. By implementing a simple lottery task we manage to reduce the effect of impaired cognitive abilities due to alcohol intoxication on subjects' decision making. In addition to that, we increase outcome variance along different lotteries and keep the expected value constant, positive, and easily computable across them. In this way, the differences between choices are uniquely based on the risk involved and not on different levels of long-term profitability (i.e., how comparatively adaptive the options are), learning or other required abilities. Lastly, given the field nature of our study, important environmental and individual features that may mediate or interact with the effects of alcohol over risk-taking are accounted for.

We find that both measured and self-estimated BAC levels impact negatively over the subjects' willingness to choose riskier lotteries. However, at high intoxication levels subjects tend to underestimate their own BAC, and the degree of such underestimation goes along with increasing the riskiness of choices for male and young subjects. Thus, our findings suggest that individuals take lower risks insofar as they consciously perceive their intoxication level to be high. This might work as a proximate mechanism for compensating the psychomotor impairment triggered by intoxication (Burian et al. 2003). Compensatory patterns after alcohol exposure have been reported in other behavioral tasks (Gustafson and Källmén 1990; Finnigan et al. 1995). Therefore, engagement in risky behaviors associated with alcoholic intoxication seems to be due to the impaired ability to evaluate risks and/or the negative consequences of choices when such impairment is not sufficiently compensated for as a result of an inadequately self-perceived intoxication level. Of major interest is the case of young and male individuals. Youth and maleness are considered two attributes that increase the likelihood of being involved in road accidents, and engaging in violent behavior and other risky patterns under the influence of heavy alcohol consumption (e.g., Goodman et al. 1986; Zador et al. 2000; Peck et al. 2008). Our findings suggest that the underestimation of own alcoholic intoxication level at high BAC levels could be behind the enhanced willingness to take risks in these particular population subgroups.

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Chapter 5

A survey-experiment exploring the socio-demographic and psychological determinants of giving

Abstract

The article investigates the socio-demographic and psychological determinants of what is known as altruistic giving. Using the platform of the Dictator Game and the methodology of a survey-experiment as its empirical strategy, this study presents data that allows testing the alternative explanations that over the last years have been proposed by a number of models across social l sciences to explain altruistic giving. It reports behavioral, attitudinal and personality data from over 700 dictators from a representative sample of a city's adult population. Results demonstrate a minor effect of socio-demographic characteristics on the observed behavior. Using a number of econometric specifications, results further establish that individuals' behavioral heterogeneity can be attributed to both outcome-based pro-social preferences (pure and impure altruism and inequality aversion) and intention-based preferences (reciprocity and conditional cooperation). The findings illustrate that even in the simple and controlled environment provided by the Dictator Game, it is a number rather than just one underlying psychological mechanism that can give rise to a certain behavioral phenotype. What is more, it points to interplay of different mechanisms. Such an interpretation has important implications in modeling behavior and theory development.

1. Introduction

One of the most robust conclusions coming from the research of behavioral and experimental economists is that people do not only care about their own material welfare but about other people as well, that is they have social preferences. Such a finding is by no means surprising but what is important is that it has generated a boost of theory development. The new behavioral models, which can explain social interactions more accurately than the von-Neumann-Morgersen paradigm, not only do they try to fit new utility functions to experimental data but also aim to be able to explain real life phenomena such as the provision of public goods, successful cases of resolving other social dilemmas, private and corporate philanthropy and donation of time, money and one's physical parts.[20] By now, the evidence coming from laboratory experiments is overwhelming: individuals reject unfair offers in the Ultimatum Game (UG), they trust and reciprocate in the Trust Game (TG), they contribute and punish norm violators in the Public Goods Game (PGG) and they give positive amounts in the Dictator Game (DG) (Camerer 2003 summarizes the results).

What is equally interesting however is that individuals exhibit considerable behavioral heterogeneity; Engel (2011) in a meta-analysis of DG found that 36.11% of dictators (*N*=20813) give nothing while the rest offers are well distributed in the interval (0, 100%] of the pie. Andreoni and Miller (2002) report that in a DG about 47% can be characterized as selfish (and 23% as completely selfish). Fishbacher et al. (2001) characterize 30% of the subjects in a PGG as free riders and 50% as conditional cooperators. Charness and Rabin (2002) characterize 31% of the subjects as egalitarian and 69% as surplus maximizers. Some people are driven by motives other than self-maximizing welfare, but not all of them do. So, what accounts for the observed behavior and the different behavioral patterns within individuals? Which is the underlying psychological factor that determines the behavior?

Many economists have tried to give answer to this question. Becker (1974) developed a theory of pure altruism, while Andreoni (1990) and Charness and Rabin (2002) proposed a lighter form of altruism. Fehr and Schmidt (1999) and Bolton and Ockenfels (2000) suggested that such behaviors come from individuals' dislike of unequal outcomes (especially the ones leaving them worse off). A lot of economists suggested that reciprocity is the key to understanding these behaviors (Rabin 1993; Dufwenberg and Kirchsteiger 2004; Falk and Fischbacher 2006; Segal and Sobel 2007) or similarly that individuals are conditional cooperators. (Fischbacher et al. 2001). Others proposed a theory that is based on individuals' willingness to self-signal their identity (Bénabou and Tirole 2003; Akerlof and Kranton 2000).

[20] The same task of course has been undertaken already earlier by psychologists, sociologists, political scientists and even biologists and anthropologists.

Every model has tried to find a "*simple common principle"* (Fehr and Schmidt 1999, pp. 817) that guides and can explain behavior. They all use distributional games in order to isolate the possible explanations and every one has a certain degree of success in explaining a number of situations and a proportion of the individual heterogeneity. Obviously, according to the given situation different motivations can influence behavior. However an alternative interpretation is that different psychological determinants can motivate different individuals or even that more than one psychological factor are in action simultaneously in a given situation. In the present paper we try to answer this question by employing the Dictator Game. The DG is very popular mainly because, as often cited, actions coincide with strategies (and thus motivations). Thus, it offers an ideal environment to trace back possible motivational factors. Research has been extensively on the environmental factors that affect *average* behavior, such as framing (Brañas-Garza, 2007), social proximity (Hoffmann et al. 1994 and Eckel and Grossman 1996), and institutional effects (List 2007, Bardsley, 2008) among others. However, interestingly, very little is known about the effects of socio-demographics (with the exception of age and sex, see for example Croson and Gneezy 2009) and the underlying psychological factors that determine giving. Indicative is that from a total of 131 papers (including 616 treatments) that Engel includes in his meta-analysis (Engel, 2011), less than 10% explicitly study socio-demographic effects that can account for individual heterogeneity.

In order to address these issues, we have designed a survey experiment employing a representative sample of a city's adult population (Granada, Spain)[21]. All subjects made a distribution as a dictator. In addition we included a set of attitudinal and personality measures, which will serve as the main dependent variables in explaining Dictator giving. Lastly, rich socio-demographic information was gathered. In such studies this information is particularly important in order to avoid spurious correlations. Imagine for example that one wants to test that empathic dispositions positively affect giving. Imagine also that older people tend to be more empathic. Older people are also known to be more generous in DG. Not controlling from age, one could erroneously conclude that empathic dispositions affects giving (which might be or might not be the case).

Our results demonstrate a minor effect of socio-demographics on behavior. We also find strong evidence that *i)* reciprocity and *ii)* pure and impure altruism are strong determinants of giving. The results imply that giving behavior is not just an altruistic expression of individuals. Even in such a simple decision problem such the one offered by the DG, a variety of factors influence the behavioral phenotype. The finding is in line with a study by Christina Fong (Fong 2007). Using a carefully designed survey-experiment she tried to separate two alternative

[21] Survey experiments seem to be the answer to concerns of the tradeoff between external and internal validity addressing at least the concerns coming from the narrow sample pool of university students who lack market experience and offer a very narrow sample of socio-demographic characteristics. Because of their randomized treatments, experiments have the advantage of a high degree of internal validity. Especially in Political science survey experiments are currently the state of the art (see Barabas and Jerit, 2010). Among economists the technique is also starting to gain ground.

explanations of generosity in a Dictator Game where the recipient was a charity. She found evidence for both conditional and unconditional altruism and suggested that people's behavior might be driven by a combined desired to help others and to reciprocate (she referred to it as empathetic responsiveness). Even though our results are somehow similar to that of her study we interpret them in a different manner. We suggest that it is to one unique factor that affects behavior. Conversely is a set of factors that each simultaneously affects the behavior. The different interpretation has important implications when it comes to model behavior.

The rest of the article is organized as follows: Section I offers a small review on the ways researchers tried to organize the various theories of social preferences. This exercise allows identifying the key factors that render study. Section 2 will present the empirical methodology and the attitudinal and personality measures included in the survey. Section 3 describes in some detail the database. Section 4 presents the determinants of giving and Section 5 concludes by discussing the results.

2. A meta-review on the theories of social preferences

There have been several attempts to organize the various models developed to explain social preferences. Each researcher categorized the models according to his/her personal reading of the literature. Here we offer a small review of the most recent reviews of what we will abusively call *social preferences theories*. It is worth noting that such theories exist outside of economics as well. For the purposes of this section however, we will restrict our attention mainly to reviews on economic theories as well as theories of the (economics of) philanthropy. We also mention a very comprehensive review on social psychology.[22]

All models suppose that people not only care about themselves but about others as well. In general Economists focused more in the tense between self and public interest. Social psychologists between cooperation and competitions, personality psychologists on the psychological foundations of giving while research on philanthropy, more that anything else in the institutional factors (and socio-demographic factors) that increase the probability of one donating. Philanthropy research has naturally focused only on the good side of people. Economists and Social psychologists on the other, have also highlights the "dark" side of social preferences.

[22] Reviewing the theories offered by social psychologists one quickly realize that there is a lack of communication between the various fields of social scientists. The theories developed by economists over the last years (which a great influence on many other economists) model behavior in ways very similar to that of social psychologists published many years ago. Indicative is the example of a paper by Messick and McClintock (1968) that is very close to the way economists nowadays think about social preferences. The paper has a total of 555 citation "hits" in Google Scholar and only seven are from Economic Journals (three from Economic Psychology, two from Journal of Economic and Behavioral Organization, one from Experimental Economics and one from International Journal of the Economics of Business), while non from the reviews in economics cite the paper.

Stephan Meier (2007) splits social preferences theories in three categories. In *(a)* outcome-based, *(b)* intention-based and *(c)* self-identity theories. Within the first category, which assumes that pro-social behavior is an expression of pro-social *preferences* lie theories of pure altruism, impure altruism and inequality aversion. The second category includes theories of mainly reciprocity that identify pro-social behavior as an expression of reciprocal feelings rather than preferences. Self-identity models assume that people not only care about their reputation with others but they also want to have a good self-image.

Lise Vesterlund (2006) makes a more radical categorization. On one hand theories that focus on private benefits from pro-social acts and on the other theories that focus on the public or social benefits of such actions. For example an individual modeled as one that derives only private benefits form a pro-social action, (s)he will not be deriving any utility if (s)he will not be the one making a contribution, say, to a public good. Prestige, warm glow, signaling of wealth and avoidance of guilt are examples of the underlying psychological factors driving behavior. IN the second category belong theories of pure altruism where other peoples' utility directly enters ones own.

James Andreoni (2004) suggests that behavior can be driven by five different motives: selfish, enlighten self-interest, altruism (about others or about other generations), impure altruism (like "his" warm glow), and moral codes unable to be described by traditional economic theory.

Ernst Fehr and Klaus Schmidt (2003) propose a "neater" categorization: Theories of *(a)* altruism, *(b)* Fairness and of *(c)* Reciprocity. In the first category belong theories of pure altruism like the one proposed by Becker. In the second category they belong theories of conditional altruism and inequity aversion where the individual is driven by fairness considerations. Lastly, in the reciprocity category as in Meier, people care about intentions rather than final outcomes.

Paul Van Lange (2000) reviews theories in social psychology. There the dominant paradigm is that of Social Value Orientation. So, he mentions five possible orientations: *(a)* cooperation, *(b*) equality, *(c)* generosity, *(d)* competitive, and *(e)* individualistic. *(a)*to*(d)* are other regarding orientations while *(a)* is self-regarding. Within other regarding orientations only *(a)* to *(c)* are pro-social.

Lastly René Bekkers and Pamela Wiepking (2011) provide a review within the philanthropic giving. They propose eight mechanisms that make people donate money. *(a)*awareness of need, *(b)* solicitation, *(c)* costs and benefits, *(d)* altruism, *(e)* reputation, *(f)* psychological benefits, *(g)* values and *(h)* efficacy. If one disregard the mechanisms that correspond to philanthropy in particular and cannot apply to a controlled situation of an experimental game

(which does exactly this, controlling for these factors) and the one is left with altruism, reputation, psychological benefits and values.

Based on the above reviews, we have selected to test the possible psychological determinants of DG giving. Accordingly, attitudinal and personality measures have been used to measure each of the mentioned factors.

3. Empirical Methodology

2.1. Attitudinal and personality measures[23]

For constructing the attitudinal and personality measures included in the survey, two earlier drafts have been developed that administered to two earlier samples. The first round was used in order to get a fast feedback about a large number of items that we wanted to include and to exclude those clearly useless. In addition, using this dataset, a principal component analysis on a 27-item reciprocity test developed by Perugini et al. (2003). That allowed reducing the test in a 9-item test. The second draft was administered to more than 1000 students from the majority of the departments of the University of Granada. This round served a dual purpose. First, in order to make clear that different items measuring the same attribute have a sufficient large correlation coefficient. The second reason was in order to assure a large enough variation in the subjects' answers. Taken together, the analysis of this dataset allowed the development of the final draft of the questionnaire that was administered to the representative sample of the city's population.

The relevant questions can be found in the appendix. In continuation we offer a small description of the constructed variables:

Positive reciprocity: item 12: "If somebody lends me money as a favor, I feel I should give him/her back something more than what is strictly due".
Reciprocal belief: item 6: "When I pay somebody compliments, I expect that s/he in turn will reciprocate".
Negative reciprocity: item 20: "If I see someone throwing a burning cigarette end in a forest during the summer period, it is sure that I would reprimand him/her".
Alpha part of the Fehr and Schmidt model (envy): item 3: I am not worried about how much money I have, what worries me is that there are people that have less money than I have".

[23] Personality traits are personal characteristics that lead to consistent patterns of behavior. Each one of the big Five factors for example consists of a combination of traits. They considered being stable across time. Attitudes on the other hand are relatively lasting feelings, beliefs, and behavior tendencies directed toward specific people, groups, ideas, issues, or objects and are considered to be less stable across time. In order to be predictable of the behavior measurement should take place chronically close to behavior to which it supposed to predict. The term psychological determinants refers to both attitudes and personality characteristics.

Beta part of the Fehr and Schmidt model (solidarity): item 3: I am not worried about how much money I have, what worries me is that there are people that have more money than I have".
Altruism: item 30: "I would help a person I know although I know that s/he would not do the same for me".
Social efficiency: item 9 "When my turn comes after having been waiting in a long queue in the bank, I think that I should be attended quickly in order for the others behind me not to wait as much as I did".
Honesty: item 11 "Most people are basically honest"

2.2 Experimental Procedures and design

From 23rd of November to 15th of December 2010 a representative sample of Granada (Spain) city's adult population participated in a survey-experiment conducted in randomly selected households. The survey involved answering a questionnaire and making five decisions in three different experimental games with a 10% probability of getting a real monetary payoff.

A total of 127 items, including the experimental decisions, were answered by participants during the approximately 40 minutes-long interview. The design and procedures followed are only succinctly described here as they have been already reported in Brañas-Garza et al., 2012.

Using stratified random sampling we obtained a representative sample of the city's adult population consisting of 835 individuals (over 16 years old). In particular, the city of Granada is divided into nine geographical districts, which served as strata. Within each stratum, simple random samples were selected with proportional allocation.

Data collection process was well distributed across both daytime and weekday. The sampling procedure resulted in a representative sample in terms of sex, age and geographical location (Brañas-Garza et al., 2012).

Surveys were conducted by 216 university students (grouped in 108 pairs) enrolled in an one-month course on "Field Experiments" on the fall of 2010. The great majority of them were undergraduate students from majoring in economics and management. It was required that they follow a ten-hour training on the methodology of field experiments in economics, conducting surveys, and sampling procedures. Each pair of interviewers was equipped with a bag containing the materials needed for conducting the surveys (see the "experimentalist kit" at www.ugr.es/local/pbg/City.htm). Their performance was carefully controlled using a web-based online system (updated each 8 hours).

The survey was divided in 9 thematic sections with the 8th section containing the experimental games. Table 1 offers a summary of the information gathered in each section (copies of the original survey in Spanish and the English translation are available at www.ugr.es/local/pbg/City.htm). At the beginning of section 8 and before any details were given about each decision in particular, participants received some general information about the nature of the experimental games, according to standard procedures in experimental economics. By using a decision-sheet and an envelope provided, participants' decisions were blind in the eyes of the interviewers. Also, participants were informed that their identity would never been disclosed at the other anonymous participant with whom they would be randomly matched if selected for real payment.

By the end of the experimental general instructions, the interviewer read the details for each decision separately[24]. After every instruction set, the participant was called to privately write down her decision and went ahead to the next decision. Once the five decisions were made, the participant had to introduce her decision-sheet inside the envelope and seal it. To control for possible order effects on decisions, the order both between and within games was randomized across participants, resulting in 24 different orders (holding however the two decisions of the same game always aside).

Section	Information gathered	# of items
1	Age, gender, religion, education, health, marital and labor status, income	30
2	Reciprocity, generosity, distributional preferences, social capital (I), self-esteem, trustworthiness	30
3	General and relative life satisfaction	2
4	Competitive and sanctioning behavior, social capital (II), crime victimization, personal strengths and weaknesses	13
5	Trust in known and unknown others, trust in social and public institutions	13
6	General trust, social capital (III)	4
7	Cognitive abilities, risk and time preferences (hypothetical)	21
8	Experimental games	5
9	Height, weight, digit ratio, phone number, participation in future studies	9

4. Description of the database

[24] There were decisions about three different games (Dictator, Ultimatum and Trust game), counting to six possible roles, but summing to only five different decisions to make since the receiver in the Dictator game is totally passive.

We now present an overview of the most relevant variables (excluding the basic variables that have already been explained above) included in our study. We have four groups of variables: socio-demographics, social activity and life satisfaction. Table 2 provides the descriptive statistics of these variables.

4.1 Variables

Socio-demographics

Age groups (26-40; 41-65; 65 and over; reference: younger than 25); male dummy; number of offspring; unemployment dummy; educational level of the respondent and their parents on a scale from 0 (no studies) to 8 (complete superior university-level); number of correct answers (0-5) in five mathematical (the 5-item test covered in questions 1 to 5, section 7 of the survey); low income dummy (=1, if total household monthly income is less than 1000 euros); high income dummy (=1, if total household monthly income is higher than 4000 euros); share of money that the respondent contributes to her/his household on a scale from 0 (nothing) to 4 (whole income); non believer (religion) dummy (=1, if respondent declare to be non believer). Note that the effect of the majority of these variables cannot be studied using university students.

Social activity and life satisfaction

Number of friends; Voluntary activity dummy (1 if the respondent is member of any voluntary organization); Life satisfaction ("generally speaking, how satisfied do you feel with your life?", ranging from 'completely unsatisfied' to 'completely satisfied', on a seven-point Likert scale); Effort dummy ("Do you think that success in life depends mostly on luck/effort": 1 if respondent chose effort).

Furthermore, to be answered in a seven-point Likert scale we have: Positive attitude("I have a positive attitude towards myself"); Self-confident ("When I have to deal with a problem, I am usually confident that I can find a solution"); Confidence("People who know me, trust me").

4.2 Reliability of our dictator giving data

Before proceeding to the analysis of determinants of giving it seems necessary to test whether our measure of giving outside the lab is similar to what we observe in the lab. Figure 1 compares behavior in a DG of university students who participated in a standard lab experiment with university students who participated in our survey-experiment (n=173).

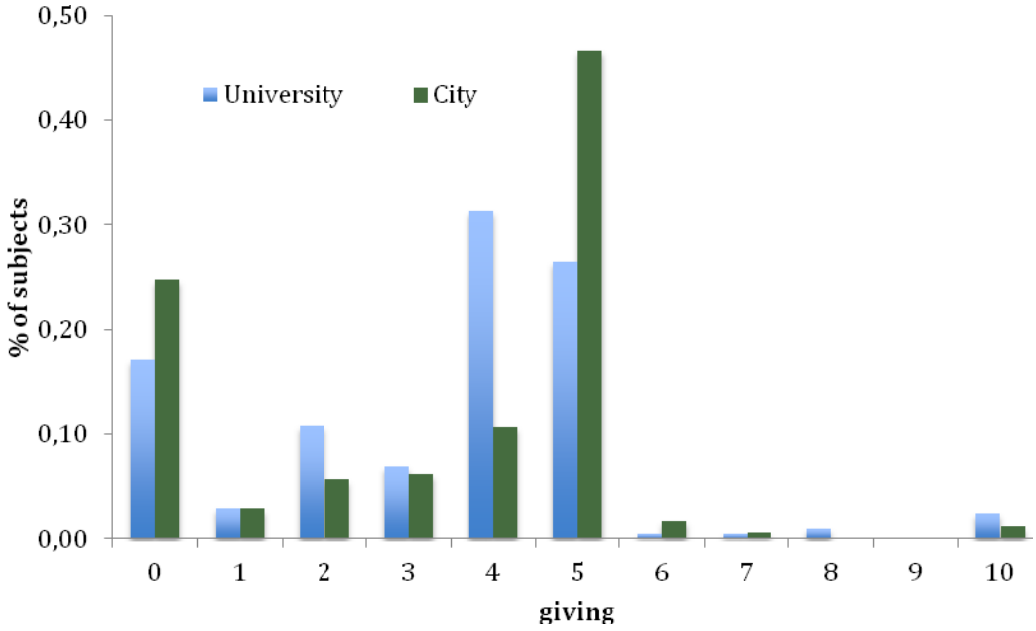
There are some differences in procedures between both experiments: *i*) In the University (hereafter U) experimental subjects were invited to express donations in real numbers with 2 decimal places –hence what its shown in Fig. 1 is a categorization. *ii*) U donations were restricted to 0 to 5€, while in the City experiment they were restricted to 0 to 10 coins of 2€ (20€). *iii*) Students from the U sample were all Economics students while the city sample is

completely open to any degree. *iv*) In both experiments we use probabilistic earnings (1/2 for U and 1/10 for the City) but, at the very end, expected returns were very similar: 0.5*5*(1-giving) ≈ 0.1*20*(1-giving).

Table 2: Descriptive statistics of the sample

Variables	Mean	Std. dv.	Maximum	Minimum
Giving DG	3.922	2.161	0	10
a) Socio-Demographics				
Age				
Less than 25	0.350	0.477	0	1
26-40	0.252	0.434	0	1
41-65	0.313	0.464	0	1
65 and over	0.085	0.279	0	1
Male	0.461	0.499	0	1
# of offspring	1.089	1.471	0	9
Unemployed	0.475	0.499	0	1
Respondent education	5.099	2.245	0	8
Math 5 items test	2.533	1.314	0	5
Father education	3.300	2.627	0	8
Mother education	2.733	2.298	0	8
Income				
>4000	0.096	0.295	0	1
<1000	0.325	0.468	0	1
% wage contributed	1.513	1.507	0	4
Non believer	0.317	0.465	0	1
b) Social activity & life				
# of friends	3.870	2.569	0	11
Voluntary activity	0.223	0.416	0	1
Life satisfaction	5.483	1.233	1	7
Effort	0.828	0.378	0	1
Positive attitude	5.548	1.408	1	7
Self-confident	5.702	1.339	1	7
Confidence	5.986	1.193	1	7
c) Other-reg. preferences				
Honesty	3.693	1.720	1	7
Positive reciprocity	3.625	2.256	1	7
Social efficiency	4.160	2.106	1	7
Reciprocal beliefs	3.571	2.101	1	7
Negative Reciprocity	5.121	2.088	1	7
Altruism	4.843	1.909	1	7
d) Fehr-Schmidt				
β inequity aversion	4.069	2.049	1	7
α inequity aversion	2.436	1.779	1	7

Figure 1:Comparison DG giving in the University and in the city



Although histograms are fairly similar there are two remarkable differences:

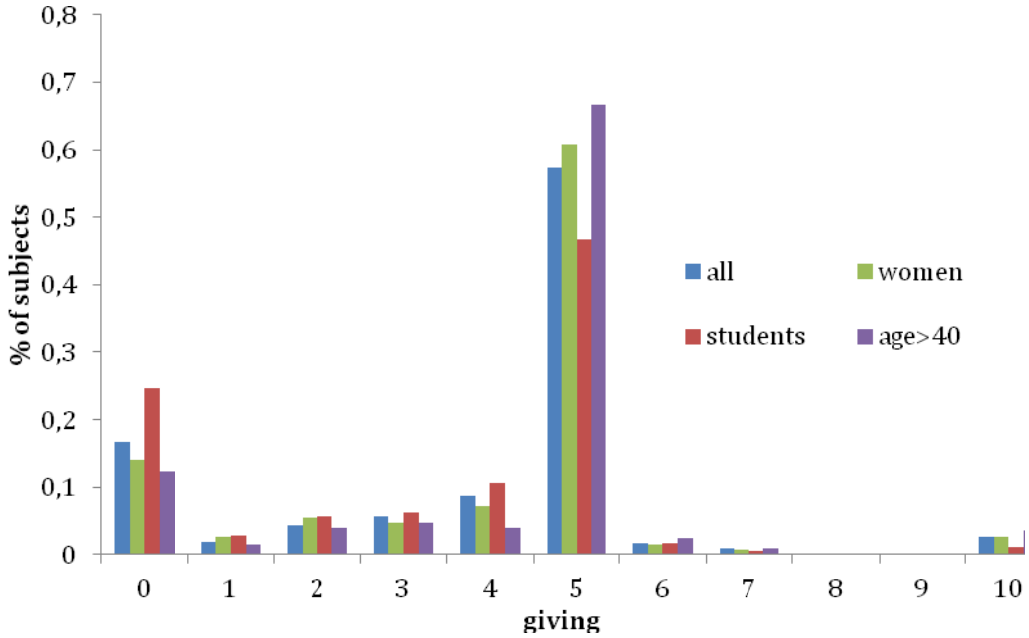
a) The category "4" (40% of the pie) is more frequent in the university environment. This is not surprising since 2€ is a clear focal point in a 5€ pie (i.e., it seems more easily justifiable for subjects to deviate selfishly from the equal split when the latter involves writing decimal numbers). In fact, if we group donations of 40% and 50% the frequencies of giving in both studies are practically identical.

b) Students in the survey experiment play Nash equilibrium more often than students in the lab.

All in all average behavior is fairly identical 3.22 en el U y 3.33 en el City (test please), hence we can conclude that behavior of the students in the lab do not differ substantially from that of other students in the field.

Figure 2 provides a general picture of the city dataset. "All" includes the whole sample (with a size of *n=817 individuals*), the "student" sub-sample (with size *173*), the "women" (with size *443*) and those older than 40 (with size *333*). We do find that students are a bit more selfish, women are more generous and people over 40 give a bit more. To test for the statistical significance of individual differences, in the next section we will conduct regression analyses, which will allow us to control for possible confounding factors.

Figure 2: Dictator giving in the city: all, students, women, and older subjects.



Using the same dataset from the city, Brañas-Garza et al. (2012) explores the effects of being student and volunteer (in experiments) on behavior in economic experiments. Overall that paper shows that experimental subjects (*studentx volunteer*) are not really different from the representative individual, although *students* are a bit more selfish in the Dictator Game.

In the next section we will explore how personal characteristics and preferences affect giving.

5. Determinants of giving

The size of our sample provides the opportunity for different approaches to estimate giving to be applied. We run standard models, like OLS or Tobit and more sophisticated approximations to data, such as Hurdled models.

We will run a series of regressions where the dependent variable is giving (an integer number between 0 and 10) and the explanatory variables are those reported in Table 2.

5.1 The effect of socio-demographics on behavior

We use five different approaches in estimating DG giving: OLS, Poisson, Negative Binomial, Ordered Logit and Censored Tobit models. Table 3 reports the estimated models. Due to missing values in some independent variables, the sample is reduced to 779 observations for the subsequent analyses. All models control for the order in which games were played. Robust standard errors are clustered by interviewer to allow for correlation of errors within the observations obtained by the same pair of interviewers.

Table 3: Estimated giving: Several approaches

Variables	OLS	Poisson	Neg. Binomial	Ordered Logit	Tobit
a) Socio-Demographics					
Age					
Less than 25	ref.	ref.	ref.	ref.	ref.
26-40	-0.043	-0.009	-0.009	-0.059	-0.093
41-65	0.152	0.038	0.039	0.314	0.170
65 and over	-0.507	-0.124	-0.123	-0.260	-0.600
Male	0.006	0.002	0.002	-0.022	-0.031
# of children	0.066	0.015	0.015	0.041	0.071
Unemployed	-0.238	-0.060	-0.059	-0.236	-0.267
Respondent education	0.017	0.005	0.005	0.023	0.014
Math 5 items test	-0.107	-0.028	-0.028	-0.088	-0.122
Father education	0.002	0.001	0.001	-0.025	0.004
Mother education	0.034	0.008	0.008	0.039	0.045
Income					
1000-4000	ref.	ref.	ref.	ref.	ref.
>4000	-0.417	-0.112	-0.112	-0.371	-0.551*
<1000	0.218	0.055	0.055	0.228	0.272
% wage contributed	-0.005	0.000	0.000	0.020	0.005
Non believer	-0.296*	-0.074	-0.074	-0.223	-0.374*
b) Social activity & life					
# of friends	-0.013	-0.003	-0.003	-0.016	-0.013
Voluntary activity	-0.129	-0.033	-0.033	-0.135	-0.166
Life satisfaction	0.022	0.006	0.006	-0.002	0.029
Effort	0.311	0.089	0.089	0.346*	0.347
Positive attitude	-0.050	-0.012	-0.012	-0.060	-0.067
Self-confident	0.140**	0.036**	0.036**	0.113	0.170**
Confidence	0.032	0.008	0.008	0.059	0.039
c) Other-reg. Preferences					
Honesty	0.116**	0.028**	0.028**	0.099**	0.142**
Positive reciprocity	0.046	0.012	0.012	0.040	0.056
Social efficiency	0.043	0.011	0.011	0.060	0.056
Reciprocity belief	-0.126***	-0.032***	-0.032***	-0.132***	-0.148***
Negative reciprocity	-0.010	-0.003	-0.003	0.002	-0.008
Altruism	0.064	0.018	0.018	0.064	0.083
d) Fehr-Schmidt					
β inequity aversion	0.112**	0.027**	0.027**	0.107**	0.132***
α inequity aversion	-0.007	-0.001	-0.001	0.006	-0.013
AIC	3367.7	3521.6	3523.4	**2160.1**	3343.8
BIC	3563.3	3717.2	3723.7	**2393.1**	3544.1
N	779	779	779	779	779

Note: * significant at 10%, ** at 5% and *** at 1%.

We first focus on the top part of the Table 3, which reports the estimated behavioral effects of the socio-demographic characteristics of the individuals. Independently of the model being implemented, we find that almost none of them influence DG giving. The two "weak"

exceptions are income and religious beliefs: richer citizens are reported to be more selfish (significant only at 10%) in the Tobit model while non-believers give less in two of the models. In the central part of the Table 3, we report the estimated behavioral effects of some aspects of the social life of the individuals. We name this block "Social activity and life satisfaction". The most salient estimate is that of *Self-confidence* which is significant in a 5% significance level and positive in four out of five models. Interestingly, it becomes not significant in the Ordered Logit model but a new variable appears, *Effort* (vs. luck), which is positive too, although marginally significant (10%). Therefore we may conclude that generosity appears as positively associated to self-confidence.

Block c) of Table 3 is devoted to "Other-regarding Preferences". The variable *Honesty* is positive and significant (5%) in all the models. This result seems intuitive: believing that people are nice, it is easier to be kind with strangers. The second robust result arises from *Reciprocal beliefs*. Not surprisingly, people who link their politeness towards others to the expectation of getting benefits in return give less money to strangers (1%). Obviously, expecting reciprocal rewards is not the kind of ingredient we should expect to find in anonymous giving. To sum up, having a positive view of other people and not having a self-centered view of social behavior seem to be essential values for generosity.

The last block of Table 3 captures the "Fehr-Schmidt" model of inequity aversion. We do not find any significant effect for envy –although the sign is the expected- but we see a notable effect of solidarity. $\beta$is positive and significant (5% or 1%) for all the models. People who care about poorer people (i.e., are averse to advantageous inequality) give more to strangers. All in all, this result seems sensible and easy to understand. Positive values regarding solidarity enhance our willingness to be generous.

The fact that socio-demographic characteristics do not affect dictator giving is still intriguing. It might be the case that some preferences are more prominent in some type of people, say age>40, and therefore preferences are hiding the real value of personal characteristics. Table 4 presents a series of new models –identical to those reported in Table 3- where all the variables included in blocks b), c) and d) are removed.

We again find absence of any systematic effect of socio-demographic characteristics on giving. Dictatorial giving seems to be completely uncorrelated to principal socio-demographic characteristics. Our interpretation is that giving is something intrinsic to individuals and linked to subjective values, which are unrelated to educational level, gender, age, etc. This lack of effect is quite surprising (see for instance gender effect in Croson and Gneezy, 2009) but it is important to remark that our study is notably different than others in that our is based not only in behavior of students (e.g., Ben-Ner et al., 2004). These studies exhibit very low variability in socio-demographics.

Another interesting result we observe in both Tables 3 and 4 is that the Ordered Logit model produces the best fit to our data (see the AIC and BIC criteria in each case). This means that an ordered discrete –instead of quantifiable and continuous- interpretation of the participants' decisions seems to be the most appropriate.

Table 4: Estimated giving: Several approaches

Variables	OLS	Poisson	Neg. Binomial	Ordered Logit	Tobit
a) Socio-Demographics					
Age					
Less than 25	ref.	ref.	ref.	ref.	ref.
26-40	-.061	.0195	.0176	.059	.042
41-65	0.392	.101	.101	.491*	.467
65 and over	0.004	.007	.004	.223	.016
Male	-0.122	-.031	-.0314	-.165	-.190
# of children	0.082	.0198	.020	.065	.090
Unemployed	-0.271	-.069	-.068	-.251	-.317
Respondent education	0.032	.008	.008	.036	.033
Math 5 items test	-0.070	-.018	-.018	-.040	-.078
Father education	0.001	.000	.000	-.019	.002
Mother education	0.031	.008	.008	.028	.042
Income					
1000-4000	ref.	ref.	ref.	ref.	ref.
>4000	-0.326	-.086	-.086	-.270	-.437
<1000	0.181	.0457	.047	.180	.238
% wage contributed	0.017	.005	.006	.038	.030
Non believer	-0.311	-.081	-.082	-.189	-.394*
AIC	3429.5	3585.8	3581.9	**2208.4**	3402.2
BIC	3555.4	3711.8	3712.5	**2371.7**	3532.8
N	785	785	785	785	785

Following this argumentation we also consider a complete different alternative. In the previous models we are assuming that people decide whether to give *0, 1, …, 10* but perhaps this is not the way in which people actually solve this problem. For instance, it might be the case that subjects first decide whether to give or not and only in a second level the exact amount of money. Obviously this poses a different psychological model. We explore this possibility in the next section.

5.2 A two-step decision process of giving

We now explore the case where subjects make a two-step decision process:

a) First, they decide whether giving money or not, that is *giving*=0 or *giving*>0.

b) Second and conditional on *giving*>0, they on the *amount of* money to give.

This type of two-step problem can be estimated using a Hurdle model. The first column in Table 5 reports the determinants of the decision of giving or not whereas the model on the right column the determinants of the amount of money conditional on giving some positive amount.

We first focus on the basic decision: to give or not (first column of Table 5). This decision might be related to many different things, for instance, to believe in the veracity of the experiment (see Frohlich et al., 2001) or the lack of information about the recipient (see Burham, 2003). This question might be completely unrelated to generosity per se.
Two socio-demographic variables are marginally significant (at 10%) but negative determinants of the decision of giving: *income>4000* and those who declare to be non-believers. Observe that these variables appeared as weak determinants of giving in previous models reported in Table 3.

Completely consistent with what we saw in Table 3, we find that those who consider that most people are basically *honest* are significantly (5%) more prone to give. In parallel, *reciprocal* people –who are motivated by future rewards- are marginally significantly less prone (10%) to give money to strangers. A new result arises from the variable *altruist*. In certain sense this question is the complementary to the former since it involves unconditional prosocial behavior, hence its positive sign (10%) comes as no surprise.

An important remark: again we do not find any effect of gender, education (and cognitive abilities), age, etc. on the decision of giving or not. Moreover, Fehr-Schmidt preferences for inequity do not play any role on the decision of giving.

The right side of Table 5 measures the size of the donation, conditional to give a positive amount. Ex ante we may expect that the variables affecting the decision of giving and those about the exact amount are not necessarily the same. This is the first result: the factors that determine the decision of giving (vs. not giving) are not the same than those that drive people to give more or less. We find two types of effects, those arising from socio-demographics and those related to pure preferences.

Table 5: A Hurdle approach to giving

Variables	Part 1. Logit	Part 2. Zero-truncated Poisson
a) Socio-Demographics		
Age		
Less than 25	ref.	ref.
26-40	-0,343	0.040
41-65	-0,026	0.053
65 and over	-0,641	-0.036
Male	-0,307	0.056**
# of children	0,119	0.004
Unemployed	0,004	-0.047
Respondent education	-0,036	0.008
Math 5 items test	0.016	-0.028**
Father education	-0,029	0.003
Mother education	0,111	-0.005
Income		
1000-4000	ref.	ref.
>4000	-0,615*	0.000
<1000	0,286	0.019
% wage contributed	0,135	-0.017
Non believer	-0,387*	-0.024
b) Social activity & life		
# of friends	0,035	-0.009*
Voluntary activity	-0,285	-0.007
Life satisfaction	-0,002	0.003
Effort	0,003	0.077**
Positive attitude	-0.102	-0.003
Self-confident	0.130	0.023**
Confidence	0.026	0.006
c) Other-reg. Preferences		
Honesty	0.157**	0.011
Positive reciprocity	0.067	0.003
Social efficiency	0.086	-0.002
Reciprocal beliefs	-0.116*	-0.018***
Negative Reciprocity	0.046	-0.008
Altruism	0.107*	-0.001
d) Fehr-Schmidt		
β inequity aversion	0.096	0.016***
α inequity aversion	-0.075	0.006
AIC	3185.9	
BIC	3577.2	
N	779	

Regarding the first block, i.e. “Socio-Demographics”, we find that, surprisingly, the fact of being *male* is positive (5%) and not negative as could be expected from previous literature. Once the decision of giving money is assumed, males are more generous than women. Males being more generous is not what most of previous studies reported (Croson and Gneezy, 2009) but the differences in the subject pool and in the statistical method (type of

model and controls included) used can influence such divergence. Perhaps the most curious result arises from the fact that those endowed with higher *mathematical abilities* are more selfish. A nice advance of the Hurdle model is that we can now say that the reason that drives individuals with higher cognitive abilities to give less is not that they are more prone to play the Nash equilibrium since they have already decided to give a positive amount of money.

The block b), i.e. "Social activity and life satisfaction" reports very salient results. It is surprising that those who report a larger *number of friends* donate significantly (although marginally, 10%) less money. This seems a bit counterintuitive. The variable *effort* (vs. luck), that in previous analyses appeared as significant only for the Ordered Logit model, is now significant and positive in this model (5%). Those who value effort as the fundamental source of success in life give more money once they have solved the decision of giving. Similarly to what we saw in Table 3 and consistently across models, we find that more *self-confident* individuals give significantly (5%) more money.

Results from the block c) "Other-regarding Preferences" are now limited to *reciprocal* subjects. These individuals give significantly (1%), and systematically across the models (Table 3) less money. In fact this type of preferences is the only variable that we find significant in both steps of the decision.

What about "Fehr-Schmidt" preferences for inequity? We saw before that no any component was useful to explain the decision of giving or not. However, *solidarity* (β) becomes crucial (1%) now, when the decision is giving more or less. People concerned about poorer individuals give more to strangers. Positive values regarding solidarity enhance our generosity.

Finally, the Hurdle model does not improve our ability to forecast individual decisions. The AIC and BIC criteria reported in Table 5 are worst than those shown in Table 3 (Ordered Logit).

5.3 Socio-demographic and psychological determinants

The nature (representative) and size (almost 800) of our sample provides a unique opportunity to check the effect of personal characteristic and preferences on giving.

All in all our models were not successful on identifying any effect of socio-demographic characteristics on giving. With very few exceptions, like the role of gender in the second step of the Hurdle model (see Table 5) we are not able to find differential effects of age, education, etc. This is a surprising results but also "good news" for Experimental Economics. A possible

explanation of this "no effect" might be that preferences are distributed among the population (for instance, young people are reciprocators) in such a way that it blurs the effect of other variables. However, estimations reported in Table 4 –where preferences were removed- suggest that this explanation is not appropriated. Nevertheless, in order to explore this issue in detail, we analyze the effect of socio-demographic characteristics on specific preferences. The set of variables which appeared as determinants of giving in the models of Table 3 will serve as dependent variables in the next analyses:

- Positive determinants: *Self-confident, Honesty* and βinequity aversion.
- Negative determinants: *Reciprocal behavior.*

We used Poisson regressions to estimate the effect of socio-demographics on preferences. Results are shown in Table 6. The first interesting finding is the notable gender bias on preferences. Males are more self-confident (5%) and more *reciprocal* (10%) than women. On the contrary, females are more inclined to show βinequity aversion (5%). Honestyis uncorrelated to gender.

Table 6: Most relevant preferences as dependent variables

Variables	Self-confident	Honesty	Reciprocal belief	βinequity aversion
a) Socio-Demographics				
Age				
Less than 25	ref.	ref.	ref.	ref.
26-40	-0.002	0.095*	-0.052	-0.033
41-65	-0.029	0.176***	-0.088	0.098
65 and over	-0.088*	0.358***	-0.061	0.284***
Male	0.038**	-0.043	0.078*	-0.092**
# of offspring	0.011	0.004	-0.009	0.014
Unemployed	-0.010	-0.044	0.048	-0.071
Respondent education	-0.002	0.025***	-0.000	-0.005
Math 5 items test	0.012	0.000	-0.025	0.005
Father education	0.004	-0.011	-0.015	-0.010
Mother education	-0.010**	0.015	0.005	-0.001
Income				
1000-4000	ref.	ref.	ref.	ref.
>4000	0.014	0.024	-0.056	0.090
<1000	-0.020	0.021	0.056	0.049
% wage contributed	0.019***	0.003	-0.009	-0.005
Non believer	-0.033*	-0.021	-0.038	-0.043
AIC	3169.3	3111.8	3395.6	3355.7
BIC	3295.7	3238.1	3521.9	3482.0
N	795	795	795	795

Note: * significant at 10%, ** at 5% and *** at 1%.

Another salient result is that people older than 65 are less self-confident (10%) but more inclined to believe that people are honest (1%). Also they exhibit higher βinequity aversion (1%). Interestingly, the individuals' *beliefs* on *reciprocal* behavior seem to be acquired before 16 years old since we do not find any differences by age groups in our sample.
The educational level of the respondent is positively related to the belief that other people are honest (1%). Finally, respondents with highly educated mothers (5%), those who contribute a smaller share of the household income (1%), and *believers* (10%) report to be less self-confident. The rest of socio-demographics are far from presenting any systematic effect on the selected variables.

5.4. Principal components analysis

A possible criticism to this study could be that the selection of some independent variables is quite ad-hoc. Given that we have more than 100 items in the survey one can think that variables others than those reported in section III might produce an even better understanding of giving behavior in the Dictator Game. In the next lines we make a new statistical exercise in which we aim to reduce the possible artificiality that the method of variable selection could have generated in previous analyses.

Using the complete set of thirty variables surveyed in section 2 of the questionnaire we run a principal component analysis. We restrict the analysis to these variables because they are measured in an identical way (a Likert scale) and cover a wide range of the personal traits contained in the survey, from reciprocity to self-esteem and from inequity aversion to social capital.

Table 7 reports a new estimation of giving using socio-demographics and other personal traits. The main difference between this model and those presented in Table 3 is that we do not use as explanatory variables a selected subsample of items of the survey but the factors obtained through principal components analysis. Principal components are, by definition, orthogonal. Hence, the findings shown in Table 7 can be driven by to collinearity between the regressors and the effects cannot be compensating each other.

The factors obtained using principal-components factoring are presented in Table A.2 (Appendix). To get a clearer pattern the factor loads have been rotated using varimax rotation. To help factor interpretation and offer a clearer picture of the relevance of each variable in the factors, we include in Table A.2 only the variables with factor loadings higher than 0.5.Let us now explain the nine factors obtained and the original variables saturated on them (saturation > 50%):

- factor 1: Self-esteem (items 21, 22, 23, 24, 25). The variables loading in this component refer to the individual conception of the self. High scores capture a positive self-concept.

- factor 2: Revenge (items 2, 4, 13). This factor captures the three questions on negative reciprocity. High scores mean negative reciprocal behavior.
- factor 3: Key person (items 28, 29). This factor captures individuals who think they play a central role when it comes to assist friends or family members.
- factor 4: Strategic hiding (items 10, 18). The most saturated variables in this component are those positively related to an individual's propensity to hide anti-social behavior with the aim of avoiding others' retaliation.
- factor 5: Pro-social (items 3, 14, 20). This factor is positively related to β solidarity, giving money to people in need, and to the willingness to punish violators of social norms.
- factor 6: Strategic showing (item 6). People who show their "nice side" only in order to receive others' reciprocal behavior are captured by this component.
- factor 7: Non self-centered fairness (items 8, 9). This factor is highly saturated by two questions, which together, can isolate whether the individuals have a broad (social) concept of justice or, rather, a self-centered one. Since item 9 loads positively and item 8 negatively, this component denotes not having a self-centered sense of fairness.
- factor 8: Negative social capital (items 5, 7). People with high scores in this factor expect opportunistic behaviors of others.
- factor 9: Bad concept of specific others (items 12, 19). Individuals with a negative perception about unemployed people and immigrants are captured by this component.

Which are the expected effects of the above components on giving? Subjects endowed with pro-social preferences (factor 5) and those whose sense of justice is not exclusively centered on themselves (factor 7) are supposed to be very concerned with other individuals' welfare and, consistently, are expected to be more generous. Our results support this view given the positive sign of these two factors. However, subjects who report to hide anti-social behavior only for avoiding retaliation by others (factor 4) were expected to give less (or, at least, giving not differentially) since there is no possibility of reciprocation in the Dictator Game. The results contradict this hypothesis, as the estimate of this factor is also positive.

Those who are vengeful (factor 2), who act pro-socially only for strategic reasons (factor 6) and those who have a bad concept of others (in general or about specific others; factors 8 and 9, respectively) are not the kind of people one expects to be altruistic with strangers. The model estimates also confirm our expectations in these cases.

The most intriguing finding is perhaps the opposite sign shown by the estimates of factors 4 and 6. In principle, it might be considered that the fact of hiding anti-social behavior to avoid reciprocation or showing pro-social behavior with the expectation of reciprocation belong to

the same construct (beliefs about others' reciprocal behavior) and, therefore, should have the same (if any) effect. However, we find a clear difference between avoiding others' negative reciprocity (factor 4) and expecting others' positive reciprocity (factor 6) in terms of their effects on giving.

The fact that questions about reciprocity (factors 2, 4, 6) result in important explanatory variables of anonymous giving seems counterintuitive. However, it might precisely happen because the Dictator Game removes the reciprocal nature intrinsic to most social interactions. Perhaps the game is dramatically changing the rules or reference points on which people who normally are reciprocal have to base their behavior. For instance, individuals who care much about reciprocity might have to base their behavior on what they guess others would do because guessing how others would respond to their own behavior makes no sense in the Dictator Game. As the behavioral rules change, behavior also changes. It is not however the aim of this paper disentangling why and through which channels reciprocity may have such an effect on anonymous giving.

The implications of the positive effect we find of factor 7 on giving are also interesting. In the Dictator Game, the only way we consider that a self-centered (vs. social) sense of fairness could relate to giving is through what the dictator thinks about the property rights over the endowment. Dictators with a self-centered concept of fairness are probably more likely to think that they deserve a bigger part of the pie because they make the effort of deciding how to split it (Oxoby and Spraggon, 2008).

The only factors, which do not significantly affect giving, are factors 1 and 3. Although self-confidence has a positive effect on giving, self-esteem has no significant effect. On the other side, the fact of considering oneself to play a central role when close people need something, although it was expected to positively affect giving, has nothing to do with generosity towards strangers. This no-effect could be related precisely to the unknown identity of the recipient, who is not of course a person close to the dictator.

Table 7.

Variables	OLS	Poisson	Neg. Binomial	Ordered Logit	Tobit
a) Socio-Demographics					
Age					
Less than 25	ref.	ref.	ref.	ref.	ref.
26-40	-0.168	-0.044	-0.044	-0.168	-0.242
41-65	0.086	0.020	0.021	0.209	0.095
65 and over	-0.636*	-0.157*	-0.157*	-0.422	-0.757
Male	0.049	0.013	0.013	0.016	0.023
# of children	0.103	0.024	0.024	0.074	0.113
Unemployed	-0.288	-0.076	-0.075	-0.270	-0.330
Respondent education	0.014	0.004	0.004	0.018	0.011
Math 5 items test	-0.108	-0.028	-0.028	-0.090	-0.124
Father education	-0.007	0.001	-0.001	-0.033	-0.007
Mother education	0.028	0.006	0.006	0.034	0.040
Income					
1000-4000	ref.	ref.	ref.	ref.	ref.
>4000	-0.420	-0.121	-0.121	-0.383	-0.580*
<1000	0.180	0.049	0.049	0.164	0.227
% wage contributed	-0.001	-0.000	0.000	0.026	0.008
Non believer	-0.326*	-0.084*	-0.085*	-0.228	-0.424**
b) Social activity & life					
# of friends	-0.028	-0.008	-0.007	-0.025	-0.032
Voluntary activity	-0.070	-0.018	-0.018	-0.080	-0.094
Life satisfaction	0.038	0.009	0.009	0.014	0.045
Effort	0.193	0.056	0.057	0.232	0.209
c) Factors					
Factor 1: Self-esteem	0.128	0.005	0.005	-0.034	0.013
Factor 2: Revenge	-0.297***	-0.076***	-0.077***	-0.313***	-0.367***
Factor 3: Key person	0.086	0.022	0.022	0.102	0.107
Factor 4: Strategic hiding	0.201**	0.054**	0.054**	0.149	0.266***
Factor 5: Pro-social	0.231**	0.059**	0.059**	0.243**	0.292***
Factor 6: Strategic showing	-0.242***	-0.062***	-0.063***	-0.258***	-0.299***
Factor 7: Non self-centered fairness	0.240***	0.062***	0.062***	0.257***	0.299***
Factor 8: Negative Social Capital	-0.129*	-0.032*	-0.033*	-0.083	-0.165*
Factor 9: Bad prejudices	-0.174**	-0.044**	-0.044**	-0.130	-0.224**
AIC	3358.5	3511.5	3513.2	2151.6	3331.1
BIC	3544.8	3697.8	3704.2	2375.1	3522.0
N	778	778	778	778	778

Table 8.

Variables	Part 1. Logit	Part 2. Zero-truncated Poisson
a) Socio-Demographics		
Age		
Less than 25	ref.	ref.
26-40	-0,441	0.024
41-65	0,054	0.040
65 and over	-0,633	-0.064
Male	-0,265	0.053*
# of children	0,130	0.010
Unemployed	-0,054	-0.052
Respondent education	-0,032	0.007
Math 5 items test	0.010	-0.026**
Father education	-0,061	0.003
Mother education	0,135**	-0.009
Income		
1000-4000	ref.	ref.
>4000	-0,648*	0.033
<1000	0,213	0.015
% wage contributed	0,141	-0.016
Non believer	-0,437*	-0.021
b) Social activity & life		
# of friends	0,018	-0.009**
Voluntary activity	-0,191	0.009
Life satisfaction	0,023	0.007
Effort	-0,089	0.061*
c) Factors		
Factor 1: Self-esteem	-0.025	0.007
Factor 2: Revenge	-0.427***	-0.015
Factor 3: Key person	0.071	0.009
Factor 4: Strategic hiding	0.343***	-0.014
Factor 5: Pro-social	0.321***	0.014
Factor 6: Strategic showing	-0.357***	-0.017
Factor 7: Non self-centered fairness	0.366***	0.012
Factor 8: Negative Social Capital	-0.117	-0.017
Factor 9: Bad prejudices	-0.277**	-0.006
AIC	3163.1	
BIC	3535.6	
N	778	

In Table 8 we estimate the effect of the latter explanatory variables but now the dependent variable is again split into two possible decisions, giving and how much (Hurdle model). It is interesting that virtually all the effects we found for factors on giving (except for factor 8, which loses its significance) are related to the decision of giving or not. Once giving has been decided, no any single factor affects significantly the amount donated.

Finally, the tiny effects of socio-demographic variables once again confirm that only intrinsic personal values are systematically relevant for giving.

6. Discussion

This paper presents a very important result to the literature: donations are basically promoted by personal traits. Specifically, being self-confident, considering that people are mostly honest and β solidarity predicts people's generosity. The second component is related to individual experiences with others (and consequently is strongly dependent of age intervals) while the other two are mostly personality characteristics. Only the gender of the responder seems to be a systematically crucial issue: while men are more prone to be self-confident, women are more prone to show high values of β. The comparison of these effects gives us a possible explanation of why these variables were not significant before. Perhaps the effects of these variables are compensating each other.

Reciprocators do not donate in this anonymous setting since they cannot be rewarded at all.

What is somehow frustrating is that we cannot say anything of how generosity towards stranger may be improved. Basically, what we find is that generosity is driven by preferences –which is not surprising- but is not related to socio-demographics. Hence, we cannot provide any kind of policy recommendation focused on target groups.

In sum, generosity toward strangers is a completely personal action. The decision of sharing a pie with a stranger is not related to socio-demographic characteristic but caused by intrinsic preferences, which are also likely to be unrelated to easily measurable personal features. Hence, altruistic behavior is a personal decision that cannot be traced.

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Resumen de los Trabajos en Castellano

Resumen del estudio número uno

El hecho de que los sujetos de experimentos económicos (a) sean reclutados mediante una llamada abierta, (b) tomen sus decisiones en el ambiente artificial del laboratorio y (c) tengan un coste de oportunidad significativo para participar en el estudio puede crear ciertas expectativas que alteran su comportamiento sistemáticamente. En este trabajo estudiamos dichos efectos por medio del diseño de un experimento de campo en el cual los participantes eran pasajeros de un tren, sin la más mínima información sobre el experimento y sin ningún coste de oportunidad. Observamos decisiones en dos juegos experimentales (el Juego del Dictador y el Juego del Ultimátum). Los resultados revelan un rangomás amplio de comportamientos observados, lo que sustenta (aunque no son conclusivos) la hipótesis planteada.

Resumen del estudio número dos

Los experimentos económicos se realizan normalmente con estudiantes universitarios que eligen participar de forma voluntaria. Tanto fuera como dentro de la disciplina existe cierta preocupación sobre cómo este "particular" grupo de sujetos puede producir resultados sistemáticamente sesgados. Centrándose en las preferencias sociales, este estudio emplea datos de una encuesta-experimento realizado sobre una muestra representativa de la población de una ciudad (N=765). Reportamos datos conductuales de cinco decisiones en tres juegos paradigmáticos. La base de datos incluye estudiantes y no estudiantes así como voluntarios y no voluntarios. De esta forma, analizamos de forma separada los efectos que el ser estudiante y voluntario tienen sobre el comportamiento, lo que nos permite una comparación *ceteris paribus* entre los estudiantes auto-seleccionados (estudiantes*voluntarios) y la población representativa. Sin perjuicio de que se encuentran vestigios de ambos efectos, nuestros resultados sugieren que los estudiantes auto-seleccionados son unos sujetos apropiados para el estudio de las preferencias sociales.

Resumen del estudio número tres

Muchos comportamientos de riesgo se llevan a cabo bajo la influencia del alcohol. Sin embargo, el efecto de la intoxicación alcohólica sobre la propensión a tomar riesgos es compleja y todavía permanece incierta. Realizamos un experimento de campo en un ambiente natural para la bebida y la toma de riesgos con la finalidad de analizar cómo la concentración de alcohol en sangre (CAS) real y auto-estimada afecta a decisiones sobre loterías con dinero real. Nuestros resultados revelan un impacto negativo del nivel de CAS, tanto real como auto-estimado, sobre la toma de riesgos. Sin embargo, para los sujetos masculinos y jóvenes, encontramos una relación positiva entre la subestimación del propio CAS (un error de estimación que se da mayormente a niveles elevados de intoxicación) y la propensión a elegir loterías más arriesgadas. Nuestros resultados sugieren que un mecanismo de compensación de riesgo se activa únicamente cuando los sujetos perciben conscientemente que su nivel de intoxicación es suficientemente elevado. Concluimos por tanto que la propensión humana a tomar riesgos bajo la influencia del alcohol no se debe a un aumento en la preferencia por decisiones arriesgadas. Añadidos a la sugerencia de la literatura existente de que tal propensión se debe a una deteriorada habilidad para percibir riesgos, nuestros resultados indican que una auto-percepción equivocada del propio nivel de intoxicación puede ser también un factor importante.

Resumen del estudio número cuatro

Este artículo investiga los determinantes socio-demográficos y psicológicos de lo que se conoce como donación altruista. Apoyados en la plataforma del Juego del Dictador y la metodología de una encuesta-experimento como estrategia empírica, este estudio presenta datos que permiten testar las explicaciones alternativas que distintos modelos dentro de las ciencias sociales han propuesto sobre la donación altruista durante los últimos años. Se presentan datos de

comportamiento, actitud y personalidad de más de 700 dictadores pertenecientes a una muestra representativa de la población adulta de toda una ciudad. Los resultados demuestran un efecto reducido de las características socio-demográficas sobre el comportamiento observado. Usando diferentes especificaciones econométricas, los resultados establecen que la heterogeneidad conductual puede ser atribuida tanto a preferencias pro-sociales basadas en el resultado (altruismo puro e impuro y aversión a la desigualdad) como a preferencias basadas en la intencionalidad (reciprocidad y cooperación condicionada). Los resultados ilustran cómo, incluso en el ambiente simple y controlado que el Juego del Dictador proporciona, es un grupo de mecanismos psicológicos los que hacen aflorar un fenotipo de comportamiento particular y no uno sólo uno de ellos. Y lo que es más, éstos apuntan a la puesta en marcha de muy diferentes mecanismos. Tal interpretación tiene implicaciones importantes para la modelización del comportamiento y para el desarrollo de teorías.

Appendices

Appendix of Chapter 2

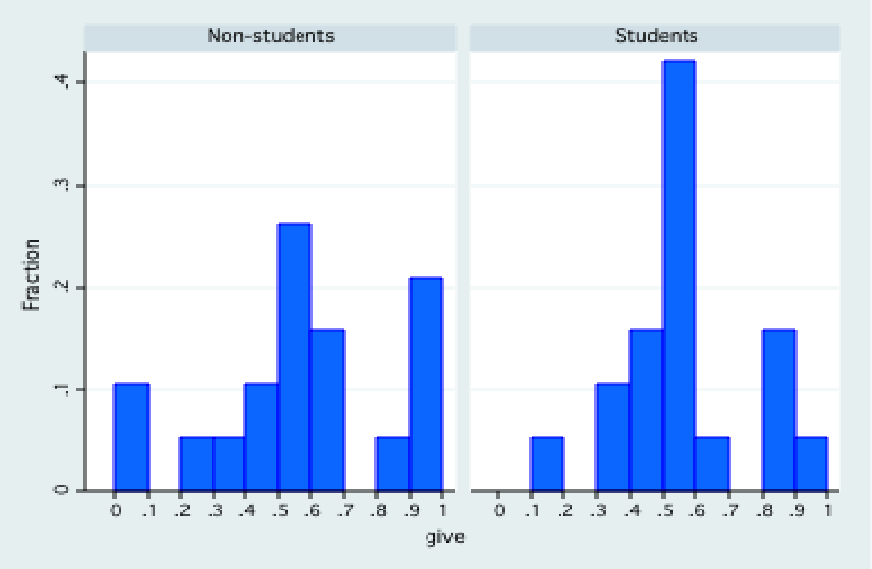


Figure S1: *comparing students and non-students allocation in DG giving*

```
probit accept what educ women age student


Probit regression                                       Number of obs   =
28
                                                        LR chi2(5)      =
5.91
                                                        Prob > chi2     =
0.3152
Log likelihood = -8.5289984                             Pseudo R2       =
0.2573

------------------------------------------------------------------------------
---------
accept |      Coef.   Std. Err.      z    P>|z|     [95% Conf.
Interval]
-------------+----------------------------------------------------------------
---------
what |  -.0778839   .1531108    -0.51   0.611    -.3779756
.2222078
educ |   .1685685   .9185303     0.18   0.854    -1.631718
1.968855
women |  -.5667373   .8507293    -0.67   0.505    -2.234136
1.100662
```

```
age |    .915062   .5163165     1.77   0.076    -.0968997    1.927024
student |   .7190293   .8527412     0.84   0.399    -.9523128
2.390371
       _cons |  -.0534101   1.612381    -0.03   0.974    -3.213619
3.106799
------------------------------------------------------------------------
---------
```

***Table S1**: probability of acceptance as a function of the offer*

```
probit accept desab educ women age student

Probit regression                                 Number of obs   =
28
                                                  LR chi2(5)      =
11.27
                                                  Prob > chi2     =
0.0463
Log likelihood = -5.8499368                       Pseudo R2       =
0.4906

------------------------------------------------------------------------
---------
accept |      Coef.   Std. Err.      z    P>|z|     [95% Conf.
Interval]
-------------+----------------------------------------------------------
---------
desab |  -.5015861   .2585808    -1.94   0.052    -1.008395
.0052229
educ |  -.1150107   1.295256    -0.09   0.929    -2.653665
2.423644
women |  -.5584955   1.064532    -0.52   0.600    -2.644941
1.52795
age |   .9855092    .768253     1.28   0.200     -.520239    2.491257
student |   .5820799   1.080614     0.54   0.590    -1.535885
2.700045
       _cons |   1.089848   2.228217     0.49   0.625    -3.277378
5.457074
------------------------------------------------------------------------
---------
```

***Table S2**: probability of acceptance as a function of the distance of the equal split*

Appendix of Chapter 3

S1. Questionnaire's contents

The questionnaire (available at www.ugr.es/local/pbg/City.htm in both Spanish – original- and English version) involved a total of 127 items, organized in eight blocks as depicted in Table S1 below.

Section	*Information gathered*	*# of items*
1	Age, gender, religion, education, health, marital and labor status, income	30
2	Reciprocity, generosity, distributional preferences, social capital (I), self-esteem, trustworthiness	30
3	General and relative life satisfaction	2
4	Competitive and sanctioning behavior, social capital (II), crime victimization, personal strengths and weaknesses	13
5	Trust in known and unknown others, trust in social and public institutions	13
6	General trust, social capital (III)	4
7	Cognitive abilities, risk and time preferences (hypothetical)	21
8	Experimental games (DG, UG & TG)	5
9	Height, weight, digit ratio, phone number, participation in future studies	9

Table S1

S2. Control variables

AGE Є [16, 91]: continuous variable

GENDER: binary variable, 1=male

EDUCATION Є [0, 17]: years of schooling. Categories: no studies (0), incomplete primary school (3), complete primary school (6), incomplete secondary school (8), complete secondary school (10), incomplete university diploma or technical degree (14), complete university diploma or technical degree(15), incomplete bachelor or postgraduate degree (15), complete bachelor or postgraduate degree (17).

HOUSEHOLD INCOME Є [0, 4500]: average household monthly income in the last year (in Euros). Categories: €0 (0), €500 (1), €1.000 (2), €1.500 (3), €2.000 (4), €2.500 (5), €3.000 (6), €3.500 (7), €4.000 (8), more than €4.000 (9).

SOCIAL CAPITAL Є [0, 3]: sum of "positive" Social Capital answers in the three questions of the General Social Survey (a, b, a for questions 1, 2 and 3 respectively):

1. Generally speaking, do you believe that:
a. Most people can be trusted
b. You must be very prudent when interacting with people

2. Do you think that most people:
a. Most people would try to take advantage of you
b. Most people would try to be fair

3. Would you say that most of the time:
a. People try to be helpful
b. People are mostly just looking out for themselves

RISK PREFERENCES Є [0, 3]: sum of "risk-loving" answers on the three following questions (b, a, Y on questions 1, 2 and 3 respectively):

1. We flip a coin. Choose one of the following options:
a. Take 1.000 Euros no matter if it is heads or tails.
b. Take 2.000 Euros if it is heads and nothing if it is tails.

2. Choose one of the following options:
a. Take a lottery ticket with 80% chance of winning 45 Euros and 20% chance of winning nothing
b. Take 30 Euros

3. Would you accept the following deal? We flip a coin. If it is heads you win 1,500 Euros and if it is tails you lose 1,000 Euros: Yes (Y), No (N)

TIME PREFERENCES Є [0, 11]: proxy for time discounting, given by the total number of impatient choices in the discounting tasks for the short-term and for the long-term with front-end delay. Each task is described below:

Short-term:
Choose one of the two options in each line:
1. Receive €5 today or receive €5 tomorrow (Td or T)
2. Receive €5 today or receive €6 tomorrow (Td or T)
3. Receive €5 today or receive €7 tomorrow (Td or T)
4. Receive €5 today or receive €8 tomorrow (Td or T)
5. Receive €5 today or receive €9 tomorrow (Td or T)
6. Receive €5 today or receive €10 tomorrow (Td or T)

Long-term:

Choose one of the two options in each line:

1. Receive €150 in a month or receive €150 in 7 months (1 or 7)
2. Receive €150 in a month or receive €170 in 7 months (1 or 7)
3. Receive €150 in a month or receive €190 in 7 months (1 or 7)
4. Receive €150 in a month or receive €210 in 7 months (1 or 7)
5. Receive €150 in a month or receive €230 in 7 months (1 or 7)
6. Receive €150 in a month or receive €250 in 7 months (1 or 7)

COGNITIVE ABILITIES Є [0, 5]: number of correct answers to the following five questions:

1. If the probability of being infected by an illness is 10%, how many persons of a group of 1000 would be infected by that kind of illness? (N if s/he cannot /do not want to answer).

2. If there are 5 persons that own the winning lottery ticket and the prize to be shared is two million Euros, how much money would each person receive?

3. Suppose that you have €100 in a savings account and the rate of interest that you earn from the savings is 2% per year. If you keep the money in the account for 5 years, how much money would you have at the end of these 5 years?:

a. More than €102
b. €102 exactly
c. Less than €102
d. S/he cannot/do not want to answer

4. Suppose that you have €100 in a savings account. The account accumulates a 10% rate of interest per year. How much money would you have in your account after two years?

5. The total cost of a bat and a ball is 1.10 Euros. The bat costs 1 Euro more than the ball. How many cents does the ball cost?

S3. Robustness analysis

S3.1. Regressions when defining students 18-24 and 18-28 years old respectively

Students: 18-24 years old

	DG		UG		UG-DG	
students	-0.050	-0.054	0.016	0.014	0.054**	0.055
	(0.034)	(0.054)	(0.015)	(0.020)	(0.022)	(0.035)
volunteers	0.041	0.040	0.023	0.022	-0.011	-0.011
	(0.027)	(0.025)	(0.015)	(0.016)	(0.019)	(0.012)
*students*x*volunteers*		0.004		0.003		0.002
		(0.061)		(0.029)		(0.044)
adj. R^2					0.0936	0.0936
LR	3.83***	3.76***	1.44*	1.40*	6.01*	5.82***

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. The first two are Tobit regressions while the third is Linear regression. *N*=765 in all regressions. Controls are: age, gender, education, household income, Social Capital, risk preferences, time preferences, and cognitive abilities. All models are also controlling for order effects. All the likelihood ratios (*LR*) shown correspond to *Chi^2* statistics, except for UG-DG column, which are based on *F.* Robust SE clustered by interviewer (108 groups) presented in brackets. *, **, *** indicate significance at the 0.10, 0.05 and 0.01 levels, respectively.

	MAO		TG trustor		TG trustee	
students	-0.174*	-0.197	-0.070	-0.208	-0.049	-0.132
	(0.105)	(0.176)	(0.152)	(0.204)	(0.142)	(0.194)
volunteers	0.023	0.014	0.199**	0.145	0.242**(*)	0.318***
	(0.093)	(0.107)	(0.101)	(0.100)	(0.094)	(0.110)
*students*x *volunteers*		0.043		0.270		-0.338
		(0.211)		(0.283)		(0.268)
pseudo R^2	0.0231	0.0231	0.0588	0.0600	0.1009	0.1028
Chi^2	54.51**	54.36**	74.50***	80.64***	98.72***	97.82***

Notes: The dependent variables are (i) the minimum acceptable offer as a fraction of the pie in UG, (ii) TG decision as a trustor; 1 if (s)he makes the loan, zero otherwise and (iii) TG decision as a trustee 1 if (s)he returns part of the loan, zero otherwise The first is an ordered Probit regression while the last two Probit regressions. *N*=765 in all regressions. Controls are: age, gender, education, household income, Social Capital, risk preferences, time preferences, and cognitive abilities. All models are also controlling for order effects. Robust SE clustered by interviewer (108 groups) presented in brackets. *, **, *** indicate significance at the 0.10, 0.05 and 0.01 levels, respectively.

Table S2

Students: 18-28 years old

	DG		UG		UG-DG	
students	-0.037	-0.042	0.014	-0.001	0.041**	0.034
	(0.030)	(0.042)	(0.015)	(0.021)	(0.020)	(0.029)
volunteers	0.040	0.037	0.023	0.017	-0.010	-0.013
	(0.027)	(0.026)	(0.015)	(0.017)	(0.019)	(0.021)
*students*x*volunteers*		0.010		0.025		0.013
		(0.048)		(0.026)		(0.037)
adj. R^2					0.0908	0.0909
LR	4.02***	4.01***	1.47**	1.45**	6.03***	5.86***

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. The first two are Tobit regressions while the third is Linear regression. *N*=765 in all regressions. Controls are: age, gender, education, household income, Social Capital, risk preferences, time preferences, and cognitive abilities. All models are also controlling for order effects. All the likelihood ratios (*LR*) shown correspond to Chi^2 statistics, except for UG-DG column, which are based on *F.* Robust SE clustered by interviewer (108 groups) presented in brackets. *, **, *** indicate significance at the 0.10, 0.05 and 0.01 levels, respectively.

	MAO		TG trustor		TG trustee	
students	-0.031	-0.024	-0.157	-0.275	-0.037	0.074
	(0.104)	(0.158)	(0.150)	(0.190)	(0.141)	(0.186)
volunteers	0.019	0.023	0.195*	0.129	0.242**	0.264**
	(0.093)	(0.114)	(0.101)	(0.103)	(0.095)	(0.120)
*students*x *volunteers*		-0.014		0.240		-0.074
		(0.196)		(0.241)		(0.254)
pseudo R^2	0.0223	0.0223	0.0599	0.0610	0.1009	0.1010
(Chi) ^{2}	55.16***	55.18**	76.16***	79.86***	101.18***	100.63***

Notes: The dependent variables are (i) the minimum acceptable offer as a fraction of the pie in UG, (ii) TG decision as a trustor; 1 if (s)he makes the loan, zero otherwise and (iii) TG decision as a trustee 1 if (s)he returns part of the loan, zero otherwise The first is an ordered Probit regression while the last two Probit regressions. *N*=765 in all regressions. Controls are: age, gender, education, household income, Social Capital, risk preferences, time preferences, and cognitive abilities. All models are also controlling for order effects. Robust SE clustered by interviewer (108 groups) presented in brackets. *, **, *** indicate significance at the 0.10, 0.05 and 0.01 levels, respectively.

Table S3

S4. Experimental Games' behavior

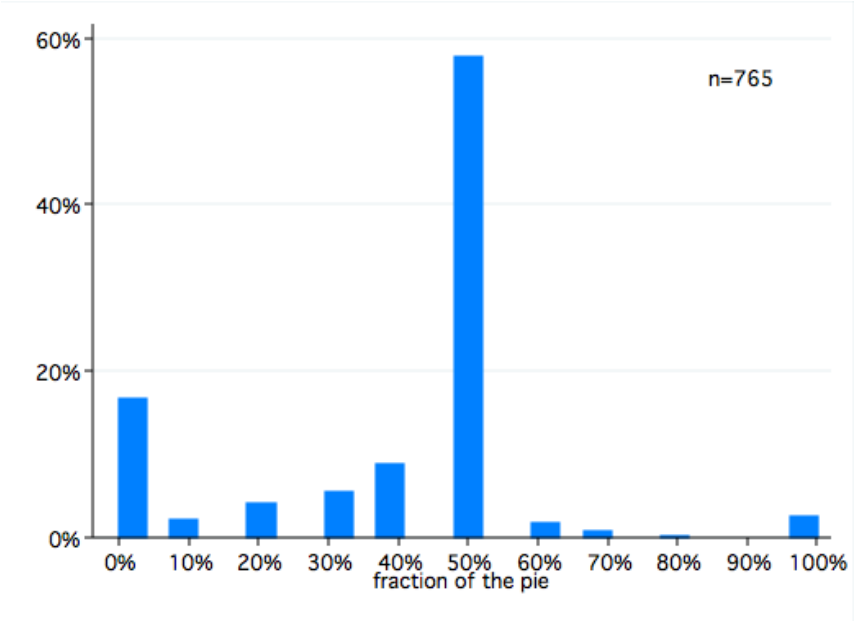


Figure S1: Dictator Game offers

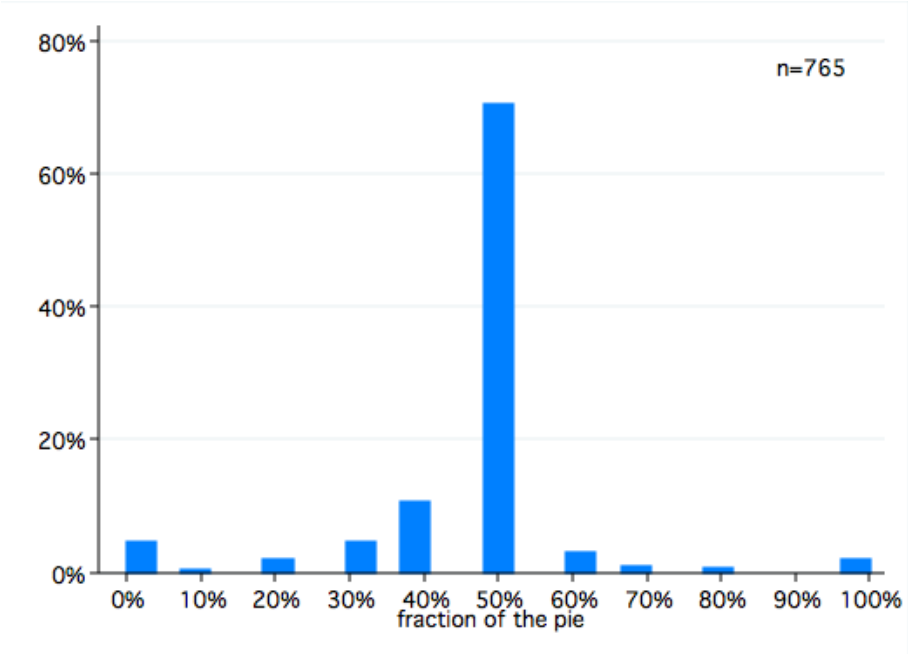


Figure S2: Ultimatum Game offers

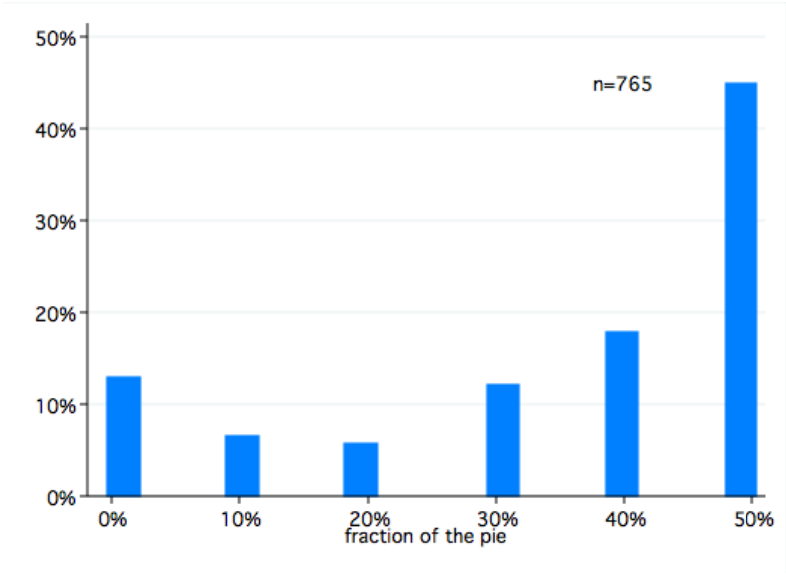


Figure S3: Ultimatum Game, minimum acceptable offers

		TG return		
		No	Yes	Total
TG pass	No	114	110	224
	Yes	106	435	541
	Total	220	545	765

Table S4: Trust Game behavior

S5. Classification of volunteers

Willingness to participate to future experiments and questionnaires:

		questionnaires		
		No	Yes	Total
experiments	No	238	177	415
	Yes	49	301	350
	Total	287	478	765

Table S5: classification of volunteers

S6. Sample's socio-demographic characteristics

Distribution of age

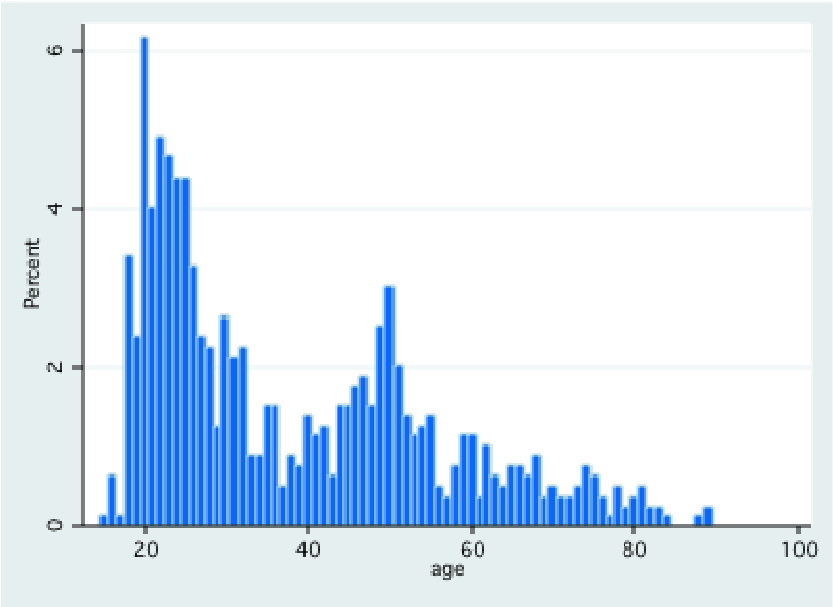


Figure S4: Histogram of age

Household income (in Euros, corrected for household size)

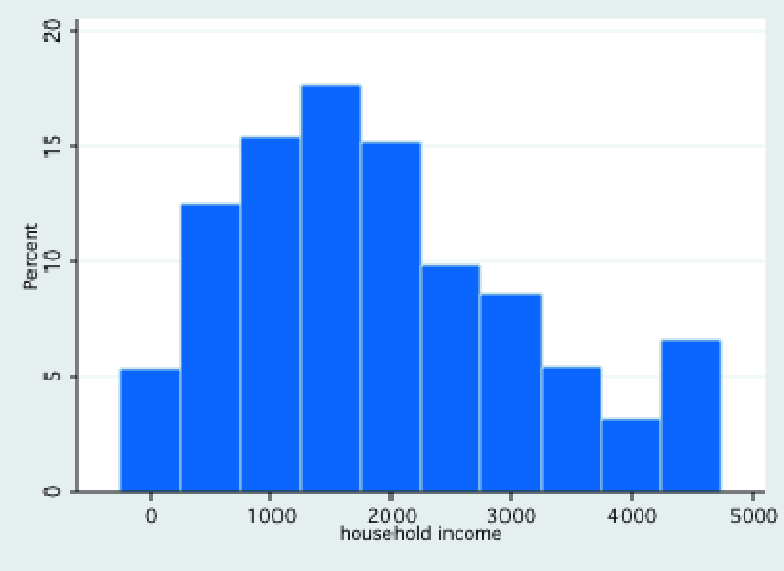


Figure S5: Histogram of household income

Education (years of schooling)

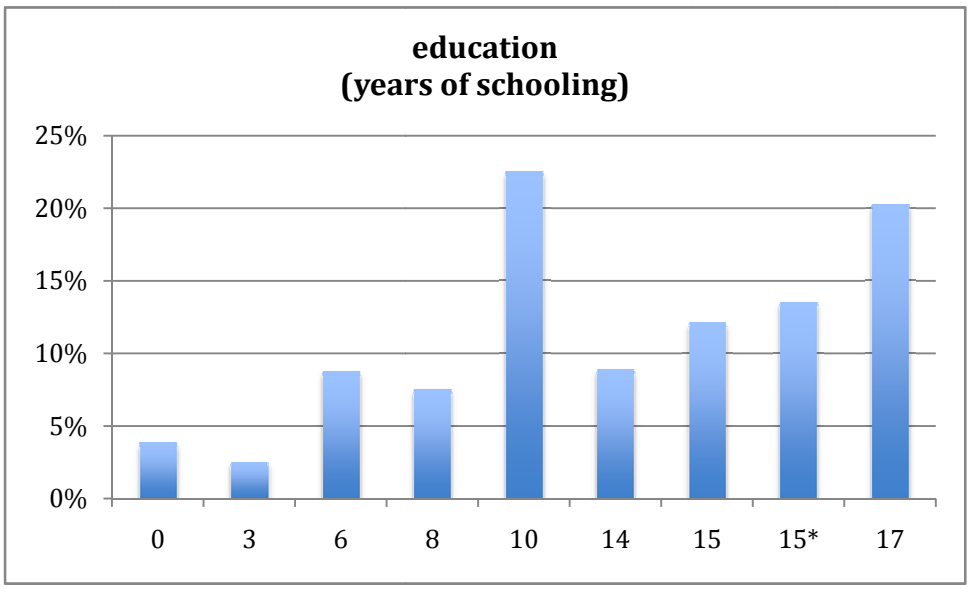


Figure S6: Histogram of education

Years of schooling	Categories
0	No studies
3	Incomplete primary school
6	Complete primary school
8	Incomplete secondary school
10	Complete secondary school
14	Incomplete university diploma or technical degree
15	Complete university diploma or technical degree
15*	Incomplete bachelor or postgraduate degree
17	Complete bachelor or postgraduate degree

Table S6

S7. Representativeness of the sample

Representativeness of the sample

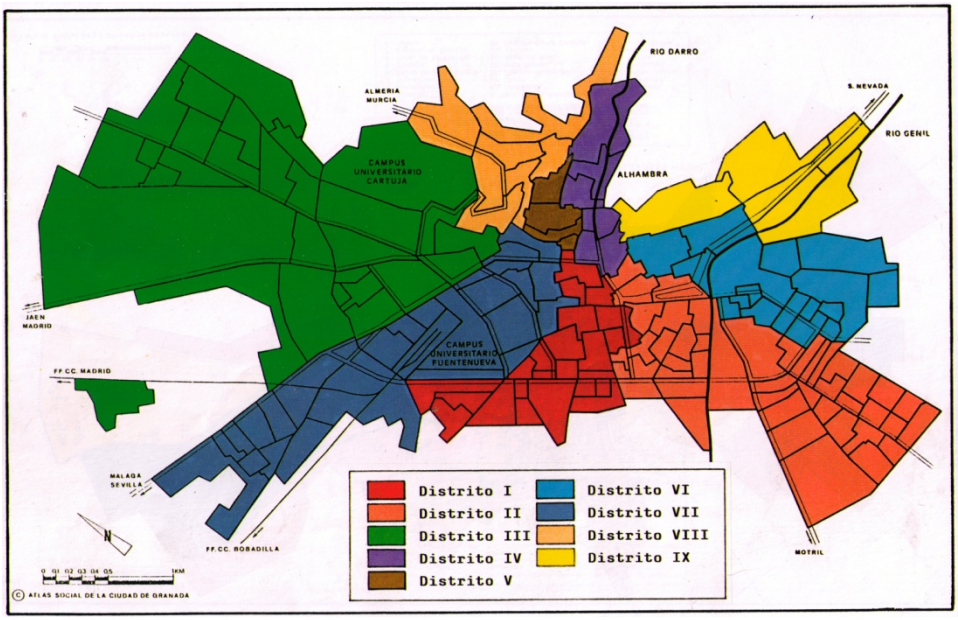
	Population (Official statistics)	Sample
Gender		
Male	46,4%	46,1%
Female	53,6%	53,9%
Age		
15-19	6%	6%
20-24	8%	24%
25-29	9%	13%
30-34	9%	9%
35-39	8%	5%
40-44	8%	6%
45-49	9%	9%
50-54	8%	9%
55-59	7%	4%
60-64	7%	4%
65-69	5%	4%
70-74	5%	3%
75-79	5%	2%
80-84	3%	2%
85 +	3%	1%

Table S7

Note: Individuals belonging to the age group of 20-24 and 25-29 are overrepresented in our sample. This difference is not without an explanation nor does it mean that we failed to find a representative sample. Granada has a very large university community hosting more than 80000 students from whom more than have are not from Granada (data available at: http://secretariageneral.ugr.es/pages/memorias/academica/20072008/cifras_comunidad/estu

diantes/datos). From those, an estimated 23500 belong to the age group 20-24 and 7000 at 25-29. Adding these to the official statistics for Granada result in increasing the corresponding percentages to 19% for group age 20-24 and 12% for 25-29.

S8. The districts of Granada



source: (Bosque et al., 1991)

Figure S7

S9. The binary Trust Game (Ermisch and Gambetta, 2006)

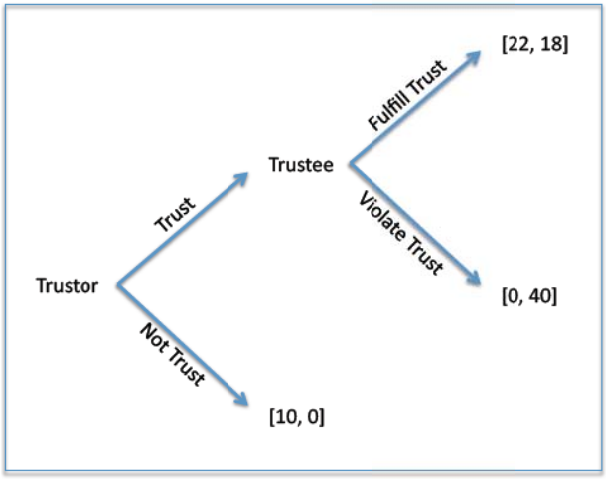


Figure S8: Strategic form of the Trust Game

S10. Interviewers bag's contents

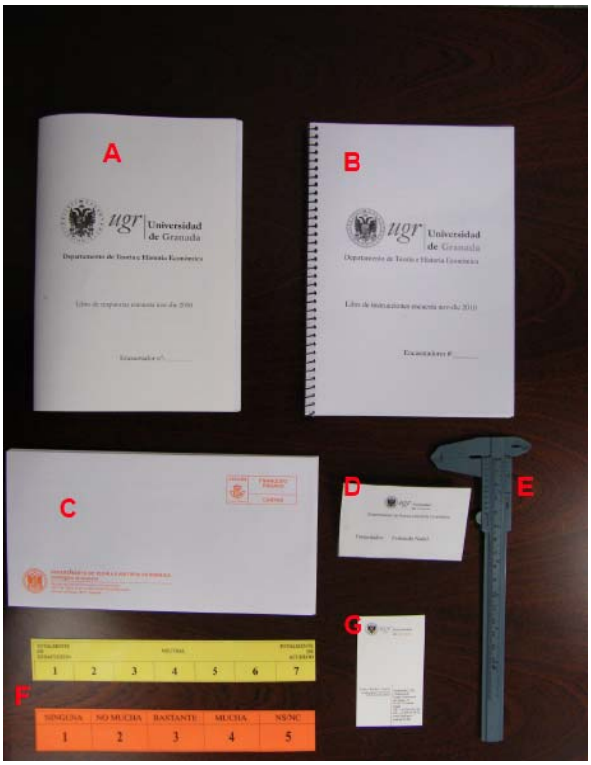


Figure S9: Experimentalist kit

A: The answer-sheet book that one of the interviewers noted down the answers

B: The book with the survey-questions

C: A university-stamped envelope that participants used to put the answer sheet of the monetary games in and seal it

D: Laminated credentials for each of our interviewers in order to include credibility
E: Caliper
F: Visual aids for the Likert-scale questions
G: A professional card of one of the principal investigators (Professor of Economics) that was shown to the participants

S11. Protocol for choosing the addresses

According to standard sampling procedures every pair of interviewers was given detailed exact information about the way they had to choose the households to be interviewed. The figure below is an example: it lists the exact addresses as well as the total number of interviews they should undergo in each of them (obviously an address can correspond to a building or a block with many apartments). In addition it provides detailed information about the way interviewers had to proceed in order to choose the apartments within each building or block. This procedure eliminates biases related to the location of houses within the blocks (for example pent-houses are more expensive). Such a randomization within the blocks is absolutely necessary.

So according to the example given below, pair 1, had to complete in total up to 5 interviews in the street Alhóndiga, at numbers 23, 19 and 13; up to 3 interviews in street Guillén de Castro at numbers 4 and 2 and so on. If in the given address, Alhóndiga 23 corresponded to a single house, obviously they had to interview this house. If however had, say 25 apartments, according to the list below they had to first try door number 12, door number 2 and so on. In case they encountered another address with 25 apartments, they had to begin by the door number 9 then proceed with door number 13 and so on (first and second line under “Blocks with 25 door numbers” respectively). Similar information was given for building with up to 50 door numbers. Each pair was given a sheet with a different randomization within buildings.

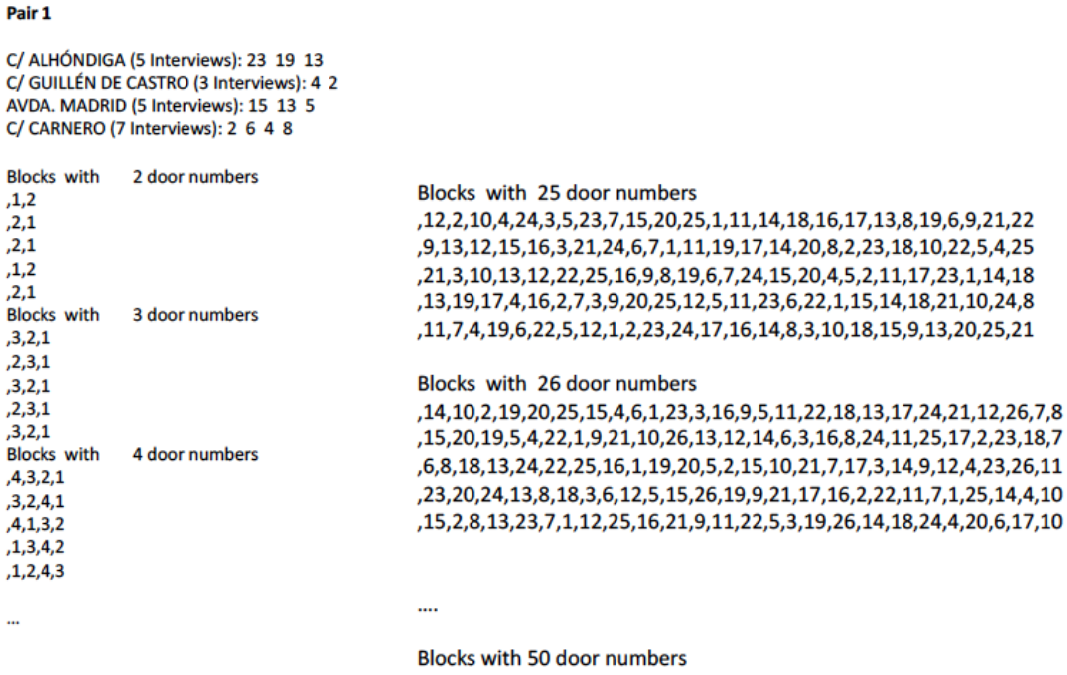


Figure S10: Example of selection protocol

S12. Controls for interviewers' performance

Students were instructed about the exact protocol they had to follow and were given three weeks to complete the surveys. They formed pairs in order to facilitate the survey implementation (one of them was always reading aloud the questions/instructions while the second was noting down the answers) and for security reasons. Their performance was controlled by follow-up calls at randomly selected participants. In addition they had to upload in a specially made webpage any new survey done and so progress was monitored by the main researchers. Every eight hours the webpage was automatically sending us a report with the progress made by each pair of interviewers. Finally, an email account was created for the special reason of responding to any questions/comments the interviewers had. This dynamic interaction facilitated the smoothness of the procedures.

S13. Games' Instructions

General instructions:

In this part, you are going to take decisions with real money. This money comes from a national research project and it is specifically for this purpose. The money you will earn depends on 5 decisions that you are going to take later. Your decisions are totally independent to each other. You have to take the decisions that you prefer in each situation, without taking into account your decisions on the other situations. You are going to be paid from only one decision.

We will make a draw in which 1 out of 10 persons will earn the real amount of money corresponding to the decision s/he has taken. Moreover, the decision that really "pays" among the 5 will be drawn randomly. For this reason, think carefully your decisions because if you are drawn, what you have declared will be what is going to be taken into account for your payment. In case you are drawn, we will make your payment within some days.

The money you earn might also depend on the decisions of other person. We explain: for the 5 decisions you are going to be paired with another person. For each decision, your pair will be different and randomly selected. This person is another interviewee but none of you can identify the other, only that it is a person also living in Granada- not even we know who s/he is. Anonymity is totally guaranteed. This is why in this part, not even we are going to know the decisions you make. For this reason, I am going to give you a sheet to write down your answers. Afterwards, you enclose your answers to an envelope, without letting us look at them. When I ask you, do not say by word of mouth your decisions; just fill the answer sheet.

Dictator Game instructions:

For this decision we give you 20€ in order for you to divide it between you and the other person. From this amount you can send to the other person the share you want, that is, you can send nothing, everything, or just a part. Obviously, the part that you do not send is for you to keep. How much money do you send to the other person? In the **BLUE** table you have to mark with a circle the number of euros you want to SEND to the other person. You can only choose even numbers: (0, 2, 4,…, 20).

Ultimatum Game (common for both proposer and responder):

In this part we give you 20€ in order for you to divide it between you and the other person. One of you is going to propose how to divide it, while the other can either accept or reject the proposed division. If s/he rejects it, none of the two will earn anything. For example: the one who decides the division sends 4€ to the other, keeping 16€ for him/herself and the other accepts it. Then the one who divides earns 16€ and the other, who accepts the division, earns 4€. Contrary, if s/he does not accept the proposal none of the two will earn anything. Understood? Decisions:

Ultimatum Game (proposer):

If you are the one who propose the division, what amount do you send to the other? The part of the 20€ you do not send is for you if the other accepts your proposed division. But keep in mind that if s/he rejects it, none of the two will earn anything. In the **RED** table you have to mark with a circle the number of euros you want to SEND to the other person. You can only choose even numbers: (0, 2, 4, …, 20).

Ultimatum Game (responder):

If you are the one who receives the money sent by the other person, you can accept or reject the division. In **YELLOW** table you have to mark the **A** with a circle in case you accept. If you reject the proposed division, mark the **R** but do not say by word of mouth. If s/he sends you:

- 0€ and keeps 20€, do you accept or reject the proposed division (A or R in the first cell of the YELLOW table). Remember that a rejection means that nobody earns anything.
- …
- 10€ and keeps 10€ (A or R in the last cell of the YELLOW table)

Trust Game (common instructions for both Trustor and Trustee):

For this part one of you- you or the other person- is going to receive 10€. The one who receives the 10€ can decide whether to keep it or make a loan the other. If s/he keeps it, the other will not earn anything. Contrary, if s/he makes the loan, the other will receive 40€ instead of 10€. The key point is that the one who receives the loan has the option of either sending back 22€ and keep

18€ or keeping all 40€ without sending anything back. That is, one of you receives 10€ and can either keep it for him/herself or make a loan to the other. If s/he makes the loan s/he can end up with either 22€ or 0€, depending on the other's decision. Understood?

Trust Game (Trustor):

If you are the one who receives the 10€, do you make the loan to the other or do you keep it for youself? Remember that if you make the loan, the other can decide to send you either 22€ back or nothing. In the **GREEN** table you have to mark with a circle the number of Euros you want to loan to the other person. That is, you should mark the **10** if you make the loan, or the **0** if you do not.

Trust Game (Trustee):

If you are the one who receives the loan and the other person decides to loan you the 10€, then you receive 40€. From these 40€ you can send back 22€ and keep 18€ or you can send back nothing and keep all 40€. In the **BLACK** table you have to 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€.

At the end of the all five decisions, the interviewer reminded the participants that:

Remember that you can be paid according to any of the decisions taken, but only one. Also, you can be selected to receive the money sent by other person in his/er **BLUE** decision.

The decision sheet that the participants had to put in the envelope once they had noted their decisions:

0	2	4	6	8	10	12	14	16	18	20

0	2	4	6	8	10	12	14	16	18	20

OTHER:20 YOU: 0		OTHER:18 YOU: 2		OTHER:16 YOU: 4		OTHER:14 YOU: 6		OTHER:12 YOU: 8		OTHER:10 YOU: 10	
A	R	A	R	A	R	A	R	A	R	A	R

0	10

0	22

References

Ermisch, John, and Diego, Gambetta. 2006. “People’s Trust: the design of a survey-based experiment.” Working Paper 2006-34. Institute for Social and Economic Research, University of Essex, Colchester.

Bosque Maurel, Joaquín, et al. 1991. “Atlas social de la ciudad de Granada.”*Caja General de Ahorros y Monte de Piedad de Granada.*

Appendix of Chapter 4

Supporting Figures

Figure S1. Map of "Feria Corpus del Christi 2008". The experiment took place around the main entrance to the university kiosk (highlighted blue area).

Figure S2. Lottery boxes

Ordered Logistic Regressions excluding Outliers				
Dep. Variable:	**Main effects**	**effects**		**Interaction**
lottery choice	**(2)**	**(3)**	**(5)**	**(6)**
BAC				
*e*BAC	-1.560** (0.684)		-5.489** (2.434)	
underBAC		0.396 (0.710)		5.498 (3.42)
BAC x male				
*e*BACx age			0.131* (0.078)	
underBACx male				6.077*** (1.848)
underBACx age				-0.325*** (0.117)
male	0.432 (0.712)	0.445 (0.771)	0.471 (0.731)	0.981 (1.010)
age	0.690*** (0.215)	0.099* (0.052)	0.589** (0.236)	1.107** (0.441)
age^2	-0.009*** (0.003)		-0.008** (0.003)	-0.016** (0.007)
BMI	0.095 (0.093)	0.152 (0.101)	0.070 (0.095)	0.277** (0.118)
alc. habits (drinks)	-0.027 (0.084)	-0.156 (0.088)	0.013 (0.087)	-0.253** (0.105)
marijuana	-0.243 (0.746)	0.075 (0.815)	-0.391 (0.761)	-0.068 (0.932)
party session	0.510 (0.533)	0.438 (0.598)	0.521 (0.761)	2.881** (1.166)
party ses. x male				-3.138** (1.386)
observations	68	53	68	53
LR (chi^2)	20.62**	19.38**	23.75***	40.59***
pseudo R^2	0.0926	0.1117	0.1066	0.2339

Table S1: The impact of BAC and self-estimated BAC over the willingness to take risk. Ordered Logistic Regressions. *e*BAC outliers are excluded. We consider outliers those observations situated three or more standard deviations from the mean. For *e*BAC (but not for BAC) two observations are classified as outliers.Standard errors in brackets. *, **, *** indicate significance at the 0.10, 0.05, and 0.01 levels, respectively. The sample in models 3 and 6 are reduced to those subjects having declared having ingested alcohol before the experiment.

Dep. Variable:	Ordinary Least Squares		
underBAC	All	Males	Females
BAC	0.640*** (0.145)	0.744*** (0.180)	0.553* (0.287)
male	0.061 (0.174)		
age	-0.022* (0.012)	-0.028** (0.014)	-0.023 (0.028)
BMI	-0.011 (0.022)	-0.009 (0.021)	-0.021 (0.076)
alc. habits	-0.026 (0.024)	-0.042* (0.021)	-0.066 (0.100)
marijuana	0.239 (0.216)	-0.128 (0.202)	-0.240 (0.709)
party session	0.121 (0.142)	0.041 (0.139)	0.139 (0.354)
alc. experience	0.003 (0.032)	0.052 (0.034)	-0.103 (0.085)
constant	0.405 (0.470)	0.606 (0.517)	0.957 (1.131)
observations	55	34	21
F	3.96***	4.85***	1.28
R^2	0.4077	0.5663	0.4081

Table S2: Understimation (*under*BAC) of one's own BAC. OLS regressions.Standard errors in brackets. *, **, *** indicate significance at the 0.10, 0.05, and 0.01 levels, respectively. The sample is reduced to those subjects who declared having ingested alcohol before the experiment. Due to the reduced sample size no interaction effects are added to the model. Age^2 is excluded since it was found to be insignificant.

Appendix of Chapter 5

Table A2: A Hurdle model where step 1 is equal split or not.

Variables	menos_5	OLS	POISSON	BINOMIAL_NEG
Socio-Econ				
Edad				
26-40	-0,161	-0,320	-0,222	-0,250
41-65	-0,633*	-0,743*	-0,477**	-0,454*
+65	0,122	-1,334***	-0,871***	-,810***
hombre	0,066	-0,069	-0,059	-0,038
nhijos	-0,074	0,167*	,099*	0,078
desemp	0,166	0,092	0,040	0,056
educ	-0,019	-0,066	-0,052	-0,059
educpadre	0,040	-0,017	-0,019	-0,020
educmadre	-0,037	0,142**	0,096**	0,112**
Renta				
>4000	0,241	-0,455	-0,354	-0,37732923
<1000	-0,296	-0,084	-0,039	-0,06986411
parterenta	-0,050	0,121	0,071	0,07284287
nocreyente	0,031	-0,467**	-0,316**	-0,338**
Social activ.				
amigos	-0,002	0,025	0,019	0,028
volun	0,139	-0,062	-0,038	-0,081
Life				
satisvida	0,059	0,001	0,008	-0,010
esfuerzo	-,442*	-0,281	-0,185	-0,186
Math				
ok5	-0,001	-0,101	-0,061	-0,070
Actitud demás				
l11SC	-0,091	0,083	0,060	0,052
l17PR	-0,036	0,064	0,033	0,045
l9quick	-0,072*	0,013	0,005	0,006
l6BR	0,149***	-0,034	-0,020	-0,021
l20puncigar	-0,019	0,001	-0,004	-0,007
l30ayuda	-,121**	0,036	0,026	0,040
Fehr				
l3IAbeta	-0,075	0,071	0,049	0,051
l16IAalpha	0,023	-0,077	-0,050	-0,058
Self Image				
l24auto	0,057	0,057	0,038	0,068
l25overc	0,005	0,141	,09043741*	0,083
l27confian	-0,069	-0,063	-0,044	-0,038
cons	1,827**	1,657*	0,421	0,374
N	740	290	290	290

Variable	Factor1	Factor2	Factor3	Factor4	Factor5	Factor6	Factor7	Factor8	Factor9
l1PR						-0.3144			
l2NR		0.7496							
l3IAbeta					0.6790				
l4NR		0.5848							
l5SC								0.6761	
l6BR						0.5924			
l7SC								0.7394	
l8slow							-0.7826		
l9quick							0.7990		
l10BR				0.7470					
l11SC		-0.4007							
l12SC									0.7105
l13NR		0.7012							
l14givin					0.5955				
l15PR				0.4775		-0.4044			
l16IAalpha						0.4393			
l17PR					0.4342			0.3101	
l18BR				0.7366					
l19inmig									0.7583
l20puncigar					0.5536	-0.3334			
l21auto	0.6873								
l22auto	0.7112								
l23auto	0.7025								
l24auto	0.7673								
l25overc	0.6071					-0.3110			
l26lider		0.3380	0.4466						
l27confian	0.3034		0.4338			-0.3310			
l28famiacu			0.7765						
l29amigacu			0.7904						
l30ayuda			0.3493			-0.3765			

1. I am ready to do a boring job to return someone's previous help. (Positive Reciprocity 1)
2. I am willing to invest time and effort to reciprocate an unfair action. (Negative Reciprocity 1)
3. I am not worried about how much money I have, what worries me is that there are people that have less money than I have. (Inequity aversion Beta part)
4. I am kind and nice if others behave well with me, otherwise it's tit-for-tat (NegativeRecip2)
5. Most people tell a lie when they can benefit by doing so. (Social Capital 1-negative)
6. When I pay somebody compliments, I expect that s/he in turn will reciprocate. (Beliefs of Recip1)
7. Some people do not cooperate because they pursue only their own short-term interest. Thus things that can be done well if people cooperate often fail because of these people (Social Capital 2-negative)
8. When my turn comes after having been waiting in a long queue in the bank, I think that I deserve being attended without rush, taking all the time I need, while the others behind me are waiting like I did.
9. When my turn comes after having been waiting in a long queue in the bank, I think that I should be attended rapidly in order for the others behind me not to wait as much as I did.
10. I avoid being impolite because I do not want others being impolite with me. (Beliefs of Recip2)
11. Most people are basically honest. (Social Capital 3)
12. There will be more people who will not work if social security system is developed further (Social Capital 4 - negative)
13. If I suffer a serious wrong, I will take my revenge as soon as possible, no matter what the costs. (Negative Reciprocity 3)
14. I usually give money to beggars in the street if they need it. (Altruism)
15. If somebody is helpful with me at work, I am pleased to help him/her. (Positive Recip. 2)
16 I am not worried about how much money I have, what worries me is that others have more money than I have. (Inequity Aversion, alpha part)
17. If somebody lends me money as a favour, I feel I should give him/er back something more than what is strictly due. (Positive Reciprocity 3)
18. I do not behave badly with others so as to avoid them behaving badly with me. (BeliefsRecip3)
19. There are too many immigrants in Spain.
20. If I see someone throwing a burning cigarette end in a forest during the summer period, it is sure that I would reprimand him/er
21. I think I am a valuable person, at least in comparison with others. (self-esteem 1)
22. I think I have many good characteristics. (self-esteem 2)
23. I am capable of doing things as well as other people do. (self-esteem 3)
24. I have a positive attitude towards myself. (self-esteem 4)

25. When I have to deal with a problem, I am usually confident that I can find a solution. (self-confidence)
26. When I work in a team, I am usually the leader.
27. People who knows me, they trust me.
28. In my family, I am the person to whom all others turn for help.
29. Among my friends, I am the person to whom everybody turns for help.
30. I would help a person I know although I know that (s)he would not do the same for me.